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Minimum Wages, Earnings, and Worker–Firm Sorting*

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Abstract

This paper studies Thailand's 2012–2013 nationwide minimum-wage reform, which raised wage floors by over 40 percent. Using matched employer–employee data, we study its effects on earnings, employment dynamics, and worker–firm sorting. We estimate a discrete type model that jointly captures heterogeneity in wages and mobility across workers and firms. Earnings rise sharply at the bottom with spillovers well above the new minimum, while employment effects are modest and concentrated among the long-term non-employed. Simulations imply sizable gains in discounted lifetime earnings, driven mainly by higher wages but amplified by mobility changes for high-turnover workers. The reform also alters career wage profiles: entry wages increase for low- and mid-wage workers, but tenure-based wage growth flattens most for mid-wage workers, generating an intertemporal trade-off between higher starting pay and slower subsequent progression. Finally, assortative matching weakens as lower-type workers move up the firm wage ladder, yet revealed-preference measures show that wage-based upgrading does not always translate into higher-valued jobs.

Keywords: Minimum wage, worker–firm sorting, job mobility, wage dynamics, matched employer–employee data, revealed preferences, lifetime earnings

JEL codes: J31, J38, J60, J64

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1 Introduction

Minimum wages are among the most widely used labor market policies intended to improve earnings and reduce inequality. A large empirical literature documents sizable short-run wage gains and, in many settings, limited disemployment. However, minimum wages are not only a static wage floor: they can reshape labor market dynamics over the longer term. A binding wage floor may raise pay at the start of a job while altering subsequent wage growth within matches, and it may reorganize worker–firm matches through changes in job-to-job mobility, job-finding from non-employment, and firm composition. These dynamic channels are central for understanding longer-run distributional consequences, but they are much less well measured than contemporaneous wage and employment effects.

The dynamic effects are theoretically ambiguous. A higher wage floor may induce firms to become more selective, strengthening positive assortative matching, or it may push workers out of low-wage firms—through contraction or exit—toward higher-wage firms, weakening sorting. Similarly, while minimum wages mechanically raise pay for workers previously below the new minimum, their implications for career wage profiles are unclear. A binding floor may change returns to tenure: firms may raise starting wages for affected workers while adjusting subsequent within-job wage growth and promotion premia. More broadly, wage growth within matches can reflect outside options and renegotiation, and the extent to which shocks are passed through into wages can vary with tenure (Bagger et al., 2014; Merkle, 2024). Because within-job wage growth and job mobility are central components of lifetime earnings (Topel, 1991; Dustmann and Meghir, 2005), identifying whether the policy shifts earnings primarily at entry or over the career is crucial.

This paper studies these issues in the context of Thailand's 2012–2013 minimum wage reform, which raised provincial minimum wages by more than 40 percent on average to a uniform 300 baht per day. Using matched employer–employee administrative data, the reform provides a unique opportunity to study dynamic responses to a large nationwide wage floor and to address three key questions. First, how did the reform affect sorting between workers and firms, and what mechanisms underlie observed changes in earnings and employment dynamics? Second, what are the long-run implications for lifetime earnings across worker heterogeneity, and what are the sources of these gains—particularly the trade-off between higher entry wages and slower subsequent wage growth? Third, when workers moved up the firm wage ladder after the reform, did they reallocate toward firms they revealed to prefer most, or did pecuniary gains diverge from preference-based measures of job quality?

To answer these questions, we estimate a wage–mobility discrete type model in the spirit of Bonhomme et al. (2019) and Lentz et al. (2023) and combine it with difference-in-differences and long-run simulations. The framework discretizes latent worker and firm heterogeneity and allows wages and transition probabilities to vary flexibly with worker type, firm type, and observables (including age, tenure, and the policy period). To avoid conflating type definitions with endogenous, policy-driven changes, we identify types from pre-reform data, while estimating model parameters on the full sample. We then use the estimated model to quantify how the reform affected earnings, employment dynamics, sorting, and revealed-preference measures of job quality.

We document four main findings. First, the reform generated large earnings gains with modest employment effects in the formal sector. Reduced-form estimates show sizable wage increases for low-wage workers, with spillovers that extend well into the wage distribution, consistent with evidence from other settings that large minimum wage changes can reshape the wage structure beyond the bottom tail (e.g., Engbom and Moser, 2022 for Brazil). Employment responses are concentrated in specific margins: we find little change for workers already employed or in short-term non-employment, but a decline in job-finding for the long-term non-employed, highlighting unemployment persistence as an overlooked margin in designs focused on contemporaneous employment. Using the Labor Force Survey as complementary evidence, we find that formal employment rises nationally during the adjustment, with regional responses that vary by exposure to the reform.

Second, lifetime simulations imply sizable gains in discounted earnings across worker types. Wage effects account for the bulk of the gains, but mobility is economically meaningful for workers with higher turnover, who disproportionately benefit from improved stability and reemployment prospects. Overall, labor-market churning declines after the reform, particularly among low- and mid-wage workers. We also document complementarity between wage and mobility channels in shaping lifetime earnings.

Third—and most centrally—the reform altered the structure of career wage profiles in a way that creates an intertemporal trade-off. Entry wages rise sharply for low- and mid-wage worker types, and returns to tenure flatten most for the middle types. This pattern is consistent with weaker outside options in the post-reform labor market and a "middle-type margin": at the bottom, wage ladders are already relatively flat, leaving limited scope for further compression without risking retention; at the top, the minimum wage is less binding. Middle types therefore exhibit the clearest shift toward higher starting pay and slower subsequent progression. This result emphasizes that minimum wages can compress inequality in the short run while simultaneously reshaping earnings growth over the career—an effect that is difficult to detect without panel matched employer–employee data and a framework that jointly models wage determination and mobility.

Fourth, the reform reshaped sorting. The policy did not merely raise wages within existing matches; it also changed the allocation of workers across firms through both job-to-job mobility and transitions into formal employment from non-employment. Lower-type workers gained access to higher-wage firms that were previously dominated by higher types, weakening positive assortative matching. We then assess whether these reallocations represent movements toward higher-valued jobs using revealed-preference rankings constructed from mobility choices (Sorkin, 2018; Taber and Vejlin, 2020; Lamadon et al., 2026). As in these frameworks, wages need not coincide with overall job value when firms differ in non-wage attributes. We find that upward moves in the firm wage ladder do not always correspond to moves toward firms that workers value most, providing suggestive evidence that pecuniary gains can diverge from preference-based job quality.

This paper is related to three branches of literature. First, it adds to the empirical minimum wage literature that has traditionally emphasized short-run wage and employment effects (Card and Krueger, 1995; Neumark and Wascher, 2008; Dustmann et al., 2021). We contribute to recent work emphasizing the importance of tracking transition paths when evaluating policies, since firms may adjust over time (Hurst et al., 2025). Using evidence from a large nationwide reform, we show that focusing only on contemporaneous wages and employment can miss important dynamic adjustment margins—changes in wage progression within jobs and reallocation across firms—that shape longer-run outcomes.[1]

Second, this paper relates to equilibrium search models with worker and firm heterogeneity that study minimum wages. Van den Berg and Ridder (1998) and Flinn (2006) show that when firms have monopsony power, minimum wages can affect wages and employment by changing which matches and firms are viable. More recent work places particular emphasis on reallocation under large hikes (Engbom and Moser, 2022) and evaluates optimal minimum wages and welfare in imperfectly competitive labor markets (Berger et al., 2025). Other work highlights additional equilibrium forces—establishment entry and exit (Karabarbounis et al., 2022) and concurrent labor supply and demand shifts (Haanwinckel, 2025)—that can shape measured effects. Our analysis is complementary: rather than specifying a full equilibrium model of firm and worker behavior, we use a flexible discrete-type framework with rich worker and firm heterogeneity in both wages and mobility to measure how a large wage-floor change reshapes reallocation across firms and, in particular, to trace its implications for wage progression within matches—an angle that has been relatively understudied.

Third, our results connect to the literature on wage dynamics, tenure returns, and job quality. Topel (1991) and Dustmann and Meghir (2005) highlight the importance of within-job wage growth and job mobility for wage progression. Bagger et al. (2014) show that wage growth within matches can arise through on-the-job search and renegotiation, while Merkle (2024) emphasizes that the pass-through of shocks into wages can vary with tenure. Our evidence provides an empirical counterpart: a large minimum wage increase can front-load earnings through higher entry wages while flattening the wage–tenure profile for mid-wage workers.

The main contribution of this paper is to show that a large minimum wage increase reshapes the labor market along dynamic margins that standard designs miss: it changes not only wage levels but also wage progression within jobs and the allocation of workers across firms. Using matched employer–employee data and a wage–mobility framework, we quantify lifetime earnings effects, characterize the entry–tenure wage trade-off, document how reallocation weakens assortative matching, and assess whether moves up the firm wage ladder align with workers' revealed preferences.

The remainder of the paper is organized as follows. Section 2 describes the institutional background and the data sources. Section 3 documents descriptive evidence of mobility heterogeneity. Section 4 presents the wage–mobility discrete type model and estimation procedure. Section 5 reports results on pre-policy characteristics of worker and firm types and sorting patterns. Section 6 presents the reduced-

[1] Related evaluations of Thailand's 2012–2013 reform using cross-sectional household data include Lathapipat et al. (2016) and Samart and Kilenthong (2024), who document wage gains with limited employment effects. Because these designs rely on repeated cross-sections, they cannot speak to within-worker earnings dynamics, mobility, or worker–firm sorting.

form policy effects, while Section 7 interprets these results through the lens of the model. Section 8 analyzes the long-term earnings decomposition and preference rankings. Section 9 concludes.

2 Institutional background and data

2.1 Institutional background

Thailand's statutory minimum wage has been set since the late 1970s by a tripartite provincial wage committee composed of employers, worker representatives, and government officials. The wage is quoted as a daily rate intended to guarantee a basic standard of living under prevailing conditions, with the Labor Protection Act of 2008 defining a normal working day as eight hours, or seven for jobs involving health and safety risks. The policy applies broadly to private-sector wage employees, with standard exclusions.[2]

In November 2011 the government announced a nationwide minimum wage of 300 baht per day, implemented in two steps: an average 40 percent increase in April 2012 (with Bangkok, surrounding provinces, and Phuket immediately reaching 300) and a nationwide harmonization to 300 in January 2013. Figure 1 plots provincial minimum wages over time.

Thailand's labor market combines both formal and informal sectors. Based on the Thai Labor Force Survey (LFS), between 2008 and 2015 the labor force comprised roughly 37 million people, about 10 percent in government/state enterprise, and among the remainder roughly one third formal and two thirds informal, while unemployment stayed close to 1 percent.[3]

Macroeconomic trends were stable around the reform period. The Consumer Price Index rose steadily. Gross Domestic Product generally increased, with temporary declines in 2009Q1 (Global Financial Crisis) and 2011Q4 (flooding), followed by a rapid rebound in the subsequent quarter (Figure 2).

The LFS is primarily cross-sectional and is used to describe aggregate labor-force composition.[4] Because it does not track workers and firms longitudinally, our main analysis relies on matched employer–employee Social Security records for formal private-sector workers.

2.2 Social Security data and baseline sample

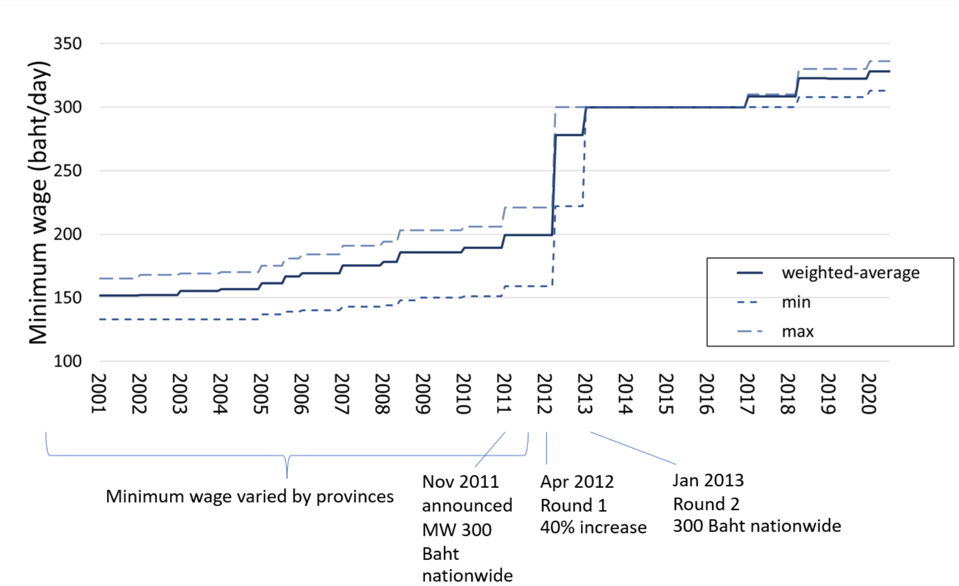
Article 33 Social Security covers private-sector employees and excludes government/state-enterprise workers and a small set of independent schemes; these exclusions largely match groups not directly subject to the minimum wage, making the data well suited for studying the reform.

[2]Exclusions include agriculture, fishery, forestry, household workers, interns, casual and seasonal workers, non-profit employees, and government/state-enterprise employees.

[3]Civil servants and state enterprise workers have their own wage and pension schemes and hence are not directly affected by the minimum wage reform.

[4]The LFS contains a small rotational panel, but it is incomplete and reset every 10 year, including 2013, the year immediately following the minimum wage reform.

Figure 1: Minimum wages across provinces in Thailand

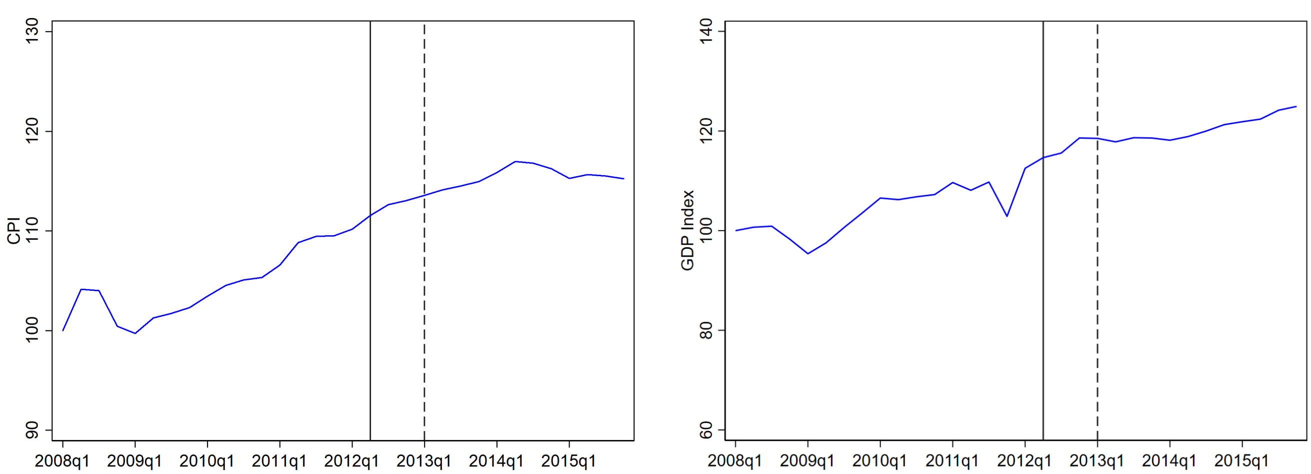


Notes: The figure shows the minimum, maximum, and employment-weighted average of provincial daily minimum wages (baht). A nationwide 300-baht floor was announced in late 2011, implemented with an increase of about 40 percent in April 2012, and extended nationwide in January 2013.

Figure 2: Macroeconomic trends in Thailand

(a) Consumer Price Index

(b) Gross Domestic Product



Notes: The left panel plots Thailand's quarterly CPI from 2008 to 2015. The right panel plots quarterly real GDP (chain volume measure, seasonally adjusted) over the same period. Both series are normalized to 100 in 2008Q1.

Over 2002–2015, Social Security coverage expanded substantially. Figure A.5 in Appendix F shows that the share of the non-government labor force contributing to Social Security rose from about 21 to 30 percent, with particularly rapid growth after 2013.

Our estimation sample uses monthly matched employer–employee records from January 2008 to March 2015 (four pre-policy years and three during/post-policy years), restricting to Article 33 workers aged 25–50 to avoid schooling and retirement transitions and requiring at least five observed wage reports per worker.[5] The resulting dataset includes 483,080 establishments and 9,297,377 workers, of whom 45 percent change employers at least once (averaging three moves). Because wages vary by establishment, our analysis is conducted at the establishment rather than the firm level. The data include establishment entry and exit, worker demographics, and monthly salaries.

There are three key measurement issues. First, wages are right-censored at the contribution cap (15,000 baht/month), around the 80th percentile pre-reform. We impute upper-tail wages following Card et al. (2013) and validate the procedure in Appendix A.[6]

Second, Social Security records only months with positive reported wages. We treat these as spells of formal employment, while non-reported months are coded as "non-employment," which in practice may include work in the informal sector. Thus, our measure of non-employment refers specifically to non-employment in the formal private sector.

Third, the data record monthly salaries but not days worked. Using the Thai Socio-Economic Panel Survey (SES) and LFS evidence, we treat 26 working days per month as typical for private-sector workers aged 25–50, so the 300-baht daily minimum corresponds to about 7,800 baht per month. Figure 3 plots the salary distribution in 2011 and 2013, with a vertical line at 7,800. The reform was highly binding (49 percent earned below 7,800 in 2011), and the mass shifts upward after implementation, yet about 22 percent still earn below 7,800 in 2013. This is consistent with legal exemptions and limited non-compliance, with exemptions likely accounting for most below-threshold observations given the low violation rates reported by Department of Labour Protection and Welfare (2013).[7] We cannot distinguish exemptions from violations in the data, so we include all such workers in the sample to evaluate the policy's overall effects.

3 Wage and mobility heterogeneity in the labor market

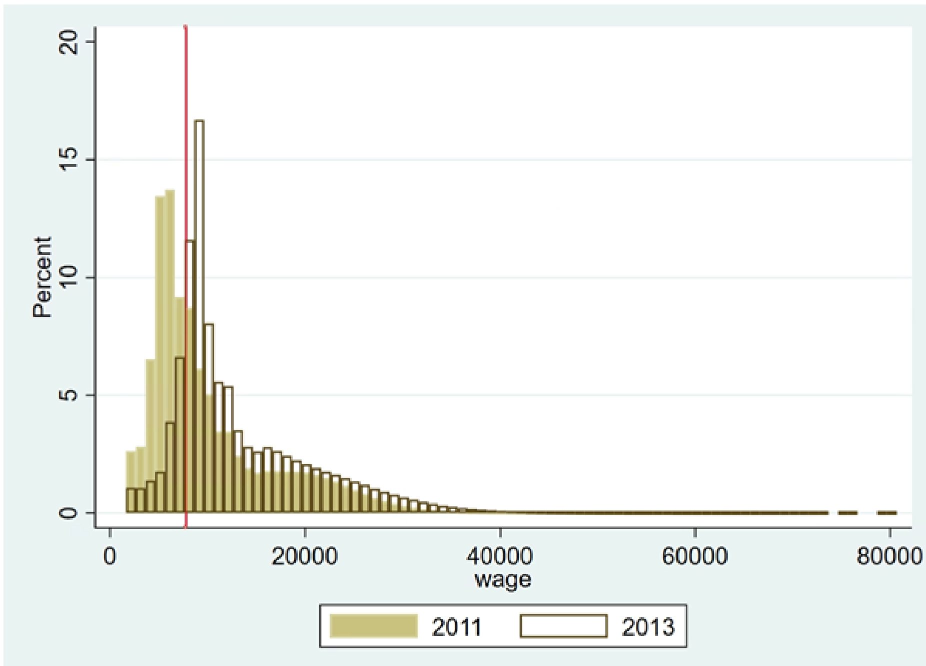
We begin by documenting heterogeneity in labor market mobility among workers with similar wages. Table 1 reports worker characteristics by average wage over the four years before the minimum wage

[5]Our sample selection criteria remove 13 percent of workers and 1 percent of total observations.

[6]Social Security also collects self-reported uncensored wages, but reporting is voluntary (about 15% missing, potentially non-random) and the series is available only for January 2011–2014. We therefore do not use it in estimation; Appendix A.1 validates our imputation by recovering artificially censored wages.

[7]The Department of Labor Protection and Welfare inspected about 10 percent of covered establishments annually and reported only 0.2–0.4 percent violations between 2013 and 2015 (Department of Labour Protection and Welfare, 2013)

Figure 3: Monthly salary distribution before and after the reform



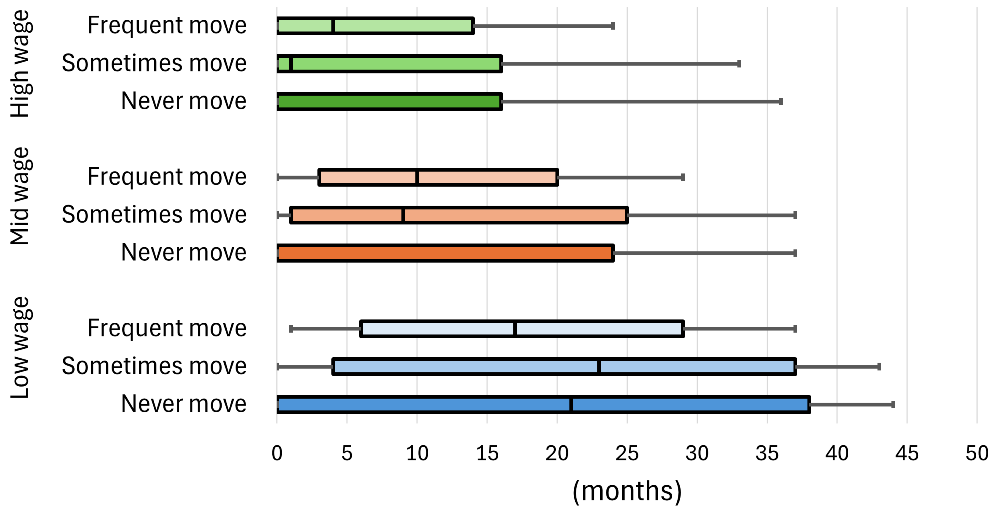
Notes: The figure shows monthly salary distributions in 2011 and 2013. The vertical line marks 7,800 baht, the implied monthly minimum from the 300-baht daily rate. The share earning below this threshold fell from 49 percent in 2011 to 22 percent in 2013.

Table 1: Worker characteristics by pre-policy wage group

	Low wage <8,000	Medium wage [8,000-12,000)	High wage 12,000+
% Never move (1 job)	0.50	0.63	0.64
% Sometime move (2-3 jobs)	0.36	0.30	0.30
% Frequent move (4+ jobs)	0.13	0.07	0.06
Average non-employment duration in months	20.29	12.49	9.34
% Male	0.50	0.52	0.55
% Movers	0.49	0.37	0.36
% Observations in sample	0.61	0.19	0.20

Notes: The table reports worker characteristics over the four years before the minimum wage reform (January 2008–March 2012). Wage groups are assigned using each worker's average monthly wage during this period: low (<8,000 baht), medium (8,000–11,999 baht), and high (≥12,000 baht).

Figure 4: Distribution of non-employment duration by pre-policy wage–mobility group



Notes: The figure shows box plots of non-employment spell duration (months) in the pre-policy period by wage–mobility category. Boxes indicate the 25th, 50th (median), and 75th percentiles; whiskers mark the 10th and 90th percentiles. Mobility categories are defined by the number of jobs held: never move (1 job), sometimes move (2–3 jobs), and frequent move (4+ jobs).

reform, grouping workers into three categories: low (roughly below the new minimum wage), medium, and high wage. The first three rows show the fractions of workers who never change jobs during the pre-policy period, those who move occasionally (two to three times), and those who move frequently (more than four times). On average, medium- and high-wage workers change jobs less frequently and experience shorter non-employment spells than low-wage workers. This pattern reflects greater job stability at higher wages.

Figure 4 provides a complementary perspective by showing box plots of non-employment duration (in months) for each wage–mover category. The boxes represent the 25th, 50th, and 75th percentiles, while the whiskers mark the 10th and 90th percentiles. Variation in non-employment duration is substantial even within wage–mover categories, particularly among low-wage workers, where the spread between the 10th and 90th percentiles is wide. Importantly, there is no simple monotonic relationship between wages and mobility: workers with similar average wages can follow very different mobility paths.

These patterns highlight two key points. First, mobility is heterogeneous and multi-dimensional, even conditional on wages. Second, this heterogeneity—potentially arising from differences across both workers and firms—matters for earnings both in the short and long run, shaping the evolution of earnings over the lifecycle. Therefore, to capture these dynamics and evaluate the minimum wage's impact, we require a framework that flexibly classifies both workers and firms by their wage and mobility patterns.

The next section develops such a framework through a wage–mobility discrete type model.

4 A wage-mobility discrete type model

4.1 The Model

The analysis is at the worker–establishment level; for convenience we refer to establishments as firms. Workers are indexed by $i \in \{1,...,I\}$ and firms by $j \in \{0,1,...,J\}$, where $j = 0$ denotes non-employment in the formal sector (which may include informal-sector employment but is referred to as non-employment throughout the paper). For each worker i, we observe a set of time-invariant characteristics z_i : gender.[8] We do not condition on fixed firm covariates because the only consistently available attribute is establishment entry date; instead, firm heterogeneity is captured through latent firm types.

Individual monthly trajectories are given by $(w_{it}, j_{it}, x_{it})_{t=1}^{T}$ where w_{it} is the worker's log nominal monthly wage (or salary), $j_{it} \equiv j(i,t) \in \{0,1,...,J\}$ is the employer ID in month t, and x_{it} is a vector of observed worker controls including age, tenure, and policy period. While T varies across workers, we use balanced-panel notation for ease of exposition.

We assume each worker i has an unobserved type $k_i \in \{1,...,K\}$ and each firm j has an unobserved type $\ell_j \in \{1,\ldots,L\}$; formal-sector non-employment is represented as type $\ell = 0$. Worker and firm type assignments are fixed over the sample window, while type-specific parameters may vary with the control vector x_{it}, which includes age, tenure, and policy period. Age is grouped into four categories (25–30, 30–35, 35–40, and 40–50). Tenure is coded as short if the worker has been with the same employer for less than one year and long otherwise; for non-employment spells, we define short tenure as less than seven months to reflect seasonal non-employment patterns in Thailand. Finally, we distinguish two policy periods: before and after the nationwide minimum wage reform. Together, these definitions yield 16 cells of x_{it}, leading to $K \times L \times 16$ parameter sets.

To streamline notation, in what follows we fix a worker i and suppress the index i when there is no ambiguity. We write (w_t, j_t, x_t) for (w_{it}, j_{it}, x_{it}), denote the worker type by $k \equiv k_i$, and let $\ell_t \equiv \ell_{j_t}$ denote the employer type in period t. Given a firm classification $F = (\ell_1,...,\ell_J)$, the structure of the likelihood function consists of three parts:

4.1.1 Initial condition

A worker enters the model with initial controls x_{i1}, capturing experience and tenure, which determine the distribution of initial matches $m(k, \ell_{i1}|x_{i1})$. Given a worker's time-invariant characteristic z_i (gender), we also specify a distribution of worker types $\pi(k|z_i)$, which maps observed characteristics into predicted latent types.

[8] Immigration status is observed but nearly all workers are native, so we do not model nativity; other standard demographics such as education and occupation are not available.

Conditional on firm type ℓ_{i1}, the initial employer j_{i1} is drawn uniformly from firms of that type,

$$\Pr\left(j_{i1} = j|\ell_{i1}\right) = \frac{1}{\#\{j : \ell_j = \ell\}} = \frac{1}{Jq(\ell_{i1}|F)}$$

where $q(\ell|F) = \#\{j : \ell_j = \ell\}/J$. Thus, within each firm type, all firms are equally likely to be selected.[9]

4.1.2 Wage distribution

Motivated by Figure 3, we model log wages as a mixture of normals through discrete match types:

$$w_{it} = \mu_{k\ell}(x) + \varepsilon_{it}, \;\; \varepsilon_{it} \sim N\left(0, \sigma^2_{k\ell}(x)\right) \tag{1}$$

allowing $\mu_{k\ell}(x)$ and $\sigma^2_{k\ell}(x)$ to vary flexibly with x (age, tenure, and policy period). For instance, mean wages of long tenure or older workers can be higher than those of the counterparts, or mean wages post the minimum wage reform can be higher than those before the policy. This structure captures the dynamics of wage variation observed empirically across time.

Several points merit comment. First, the assumption that conditional on a given match type, the current wage is independent of the past firm type is in line with the literature e.g., Abowd et al. (1999), Bonhomme et al. (2019), Di Addario et al. (2022) and Lentz et al. (2023).

Second, we treat ε_{it} as serially uncorrelated. With monthly payroll data, residual persistence reflects long runs of unchanged pay; an AR(1) is therefore a poor approximation and a high-order process would greatly complicate the likelihood. Persistence is instead captured by worker and firm types and by x_{it} (tenure, age, and policy period).[10]

Third, if wage shocks systematically affected mobility, omitting explicit serial correlation could confound wage and transition dynamics. In our framework—detailed in the next subsection—mobility depends on worker type k, firm types (current ℓ and destination ℓ') and control x. The persistent component relevant for mobility is absorbed by types and observed state. As a robustness check, we estimate a linear probability model of transition probabilities and find that including the wage shock ε_{it} does not improve explanatory power once (k, ℓ, ℓ', x) are controlled for, consistent with shocks not being a first-order driver of mobility beyond the captured state.[11]

[9]Following Lentz et al. (2023), we impose uniform sampling within firm types because we lack firm-level covariates (e.g., hiring intensity/vacancies) that would identify firm-specific initial-match probabilities.

[10]This is analogous to AKM-style wage models, which capture persistence through worker and firm effects rather than residual AR terms.

[11]We estimate a linear probability model employer-to-employer transitions on k, ℓ, ℓ', x and their interactions. The benchmark R^2 is 0.62. Adding contemporaneous wage shocks, defined as $\varepsilon_{it} = w_{it} - \mu_{k\ell}(x)$, at origin and/or destination leaves the fit essentially unchanged (R^2= 0.621).

4.1.3 Transition probabilities

Mobility to a new employer type $\ell_{t+1} = \ell'$ occurs at the end of period t, conditional on x_t. Job-to-job moves may involve switching employers within the same firm type ($\ell' = \ell$). For $\ell, \ell' \in \{1, ..., L\}$,the probability of a type-k worker moving from employer of type ℓ to ℓ' is

$$M(\ell'|k,\ell,x) = \lambda_{k\ell'}(x) P_{k\ell\ell'}(x) \tag{2}$$

where $\lambda_{k\ell'}(x)$ is the probability that a worker of type k receives a job offer from employer of type ℓ' or "chance", and $P_{k\ell\ell'}(x)$ is the probability that such a meeting results in a transition from ℓ to ℓ' or "choice". As in Lentz et al. (2023), we assume a Bradley–Terry specification for the choice probability:

$$P_{k\ell\ell'}(x) = \frac{\gamma_{k\ell'}(x)}{\gamma_{k\ell}(x) + \gamma_{k\ell'}(x)} \tag{3}$$

where $\gamma_{k\ell}(x)$ measures the perceived value of the match (k, ℓ, x). We normalize $\sum_{\ell=1}^{L} \gamma_{k\ell} = 1$ within each (k,x) cell. If a worker draws a meeting with the same firm type ($\ell' = \ell$), then, without loss of generality, we assume that the worker moves with probability 1/2 .[12]

We interpret job-to-job moves as informative about relative job value: higher $\gamma_{k\ell}(x)$ implies that, conditional on having the opportunity to switch, workers of type k are more likely to select firm type ℓ (Sorkin, 2018; Bagger and Lentz, 2019; Taber and Vejlin, 2020). We allow for heterogeneity in both offer arrival $\lambda_{k\ell'}$, and worker k's type specific ranking of $\gamma_{k\ell}(x)$. Importantly, we do not restrict the ordering of the $\gamma_{k\ell}(x)$ to coincide with mean wages, nor do we require it to be stable across policy periods.

The decomposition $M(\ell'|k,\ell,x) = \lambda_{k\ell'}(x) P_{k\ell\ell'}(x)$ can be viewed as a reduced-form counterpart of on-the-job search models with frictions: $\lambda_{k\ell'}(x)$ captures the arrival of opportunities and $P_{k\ell\ell'}(x)$ captures the acceptance decision conditional on an offer. Because job value can reflect non-wage attributes (e.g., stability or amenities), movements up the firm wage ladder need not coincide with movements up the firm value ladder implied by $\gamma_{k\ell}(x)$. Minimum-wage changes can affect mobility both by reshaping opportunity arrival patterns $\lambda_{k\ell'}(x)$ (e.g., via firm composition) and by shifting relative values $\gamma_{k\ell}(x)$ (e.g., via wage–amenity trade-offs or improving job stability). This flexible framework allows us to assess whether the ranking of firm types changes after the reform and whether workers shift toward more desirable firm types.

Transitions between formal employment and non-employment are unrestricted. From non-employment ($\ell = 0$), the probability of entering firm type $\ell' \in \{1, ..., L\}$ is

$$M(\ell'|k,0,x) = \psi_{k\ell'}(x),$$

[12] Identification of λ and γ under the Bradley–Terry specification is discussed in Section 4.4.

and from firm type $\ell \geq 1$, the probability of exiting to non-employment is

$$M(0|k,\ell,x) = \delta_{k\ell}(x).$$

By convention, we set $M(0|k,0,x) = 0$; the remaining probability is captured by the stay term $M(\neg|k,0,x) = 1 - \sum_{\ell'=1}^{L} \psi_{k\ell'}(x)$, and for $\ell \geq 1$, the probability of staying with the same employer is

$$M(\neg|k,\ell,x) = 1 - \sum_{\ell'=0}^{L} M(\ell'|k,\ell,x) = 1 - \delta_{k\ell}(x) - \sum_{\ell'=1}^{L} M(\ell'|k,\ell,x).$$

4.1.4 Likelihood

We now specify the general form of likelihood for a given firm classification. Let $\ell_{it} = \ell_{j(i,t)}$ denote the type of the firm employing worker i in period t. Define the indicator $D_{it} = 1$ if $j_{i,t+1} \neq j_{i,t}$ and D_{it}=0 otherwise. For a firm classification $F = (\ell_1, \ldots, \ell_J)$, let f, M, π and m, respectively, denote parametric versions of the wage density function; $f(w_{it}|k,\ell_{it},x_{it})$, the transition probability $M(\ell_{i,t+1}|k,\ell_{it},x_{it})$, the worker type probability $\pi(k|z_i)$, and the distribution of initial employer types $m(\ell_{i1}|k,x_{i1})$. For a parameter vector $\beta = (f, M, \pi, m)$ and classification F of firms, the likelihood for worker i conditional on (z_i, x_{i1}) is

$$\begin{aligned} L_i(k|\beta,F) = & \frac{m(\ell_{i1}|k,x_{i1})}{q(\ell_{i,1}|F)} \pi(k|z_i) \prod_{t=1}^{T} f(w_{it}|k,\ell_{it},x_{it}) \\ & \times \prod_{t=1}^{T-1} M(\neg|k,\ell_{it},x_{it})^{1-D_{it}} \left(\frac{M(\ell_{i,t+1}|k,\ell_{it},x_{it})}{q(\ell_{i,t+1}|F)} \right)^{D_{it}}, \end{aligned} \tag{4}$$

where $M(\neg|k,\ell,x) = 1 - \sum_{\ell'=0}^{L} M(\ell'|k,\ell,x)$ is the probability of staying with the same employer. We do not observe mobility after the final month and therefore do not model transitions beyond the last observed period.

4.2 The Estimation Procedure

This section describes the estimation procedure, following Bonhomme et al. (2019) and Lentz et al. (2023), which treats firm types as fixed effects and worker types as random effects; the selection of the number of worker and firm types is discussed in the next section. For a firm classification F and parameter vector $\beta = (f, M, \pi, m)$, the posterior probability that worker i is of type k is

$$p_i(k|\beta,F) \equiv \frac{L_i(\beta|k,F)}{\sum_{k'=1}^{K} L_i(\beta|k',F)}. \tag{5}$$

To jointly classify worker and firm types based on wage and mobility histories, we implement the

Classification Expectation Maximization (CEM) algorithm (Celeux and Govaert, 1992; Lentz et al., 2023), which alternates between (i) EM updates of β given F and (ii) firm reclassification updates of F given β.[13]

Let $F^{(s)}$ denote the firm-type classification at classification iteration s. Conditional on $F^{(s)}$, the EM iterations are indexed by $r = 0, 1, 2, \ldots$. In the E-step, given $(\beta^{(r)}, F^{(s)})$, we compute posterior worker-type probabilities $p_i(k|\beta^{(r)}, F^{(s)})$. In the M-step, we update β by maximizing $\sum_i \sum_k p_i(k|\beta^{(r)}, F^{(s)}) \ln L_i(\beta|k, F^{(s)})$ subject to $\sum_k \pi(k|z) = 1$ for all z and $\sum_{\ell_1} m(\ell_1|k, x_1) = 1$ for all k, x_1. We iterate the E- and M-steps until convergence to $\widehat{\beta}^{(s)}$ given $F^{(s)}$. Holding $\widehat{\beta}^{(s)}$ fixed, we then update the firm classification in the C-step to obtain $F^{(s+1)}$ by

$$F^{(s+1)} = \arg\max_F \sum_{i=1}^{I} \sum_{k=1}^{K} p_i(k|\widehat{\beta}^{(s)}, F^{(s)}) \ln L_i(\widehat{\beta}^{(s)}|k, F). \tag{6}$$

The updated classification $F^{(s+1)}$ is then used to restart the EM iterations, and the algorithm alternates between EM and C-steps until convergence of (β, F).

In practice, we implement the C-step by sequentially updating firm types one firm at a time (ordered by size), holding other firms' types fixed, and then restarting the EM routine.

As noted in Lentz et al. (2023), information-matrix standard errors are often uninformative in finite-mixture settings. In our two-sided mixture, bootstrap inference would require repeatedly re-estimating the full CEM procedure—including iterative firm reclassification—which is computationally prohibitive at our scale. We therefore focus on point estimates; given the very large sample size, sampling variation is likely to be small. To mitigate concerns about local maxima, we initialize the algorithm from 30 starting values and retain the solution with the highest log-likelihood.

4.3 Choice of period and group numbers K, L

Our sample covers seven years: four years before the reform (pre-MW) and three years after (post-MW). To study policy effects across the distribution of worker types without letting type definitions absorb endogenous post-policy outcomes, we identify worker and firm types using only pre-MW data in the E- and C-steps. We then estimate model parameters in the M-step on the full sample, allowing parameters to vary with age, tenure, and the policy period through x, while holding type definitions fixed.

To keep worker composition stable, we restrict the estimation sample to workers observed in the pre-MW period. Post-policy entrants are still assigned worker types (using the same pre-MW type taxonomy) for descriptive and validation exercises, but they do not enter the estimation objective. We cannot analogously drop post-policy entrant firms because doing so would mechanically remove associated employment spells and inflate measured non-employment. We therefore retain entrant firms and assign them to the pre-defined firm types; our sorting results are robust to their inclusion, and entrant firms represent

[13] By contrast, a sequential approach (e.g., k-means for firms followed by EM for workers) requires compressing mobility histories into fixed-dimensional summaries (such as transition matrices), which discards information. CEM instead works with the full likelihood.

a small share relative to the pre-policy firm population.

This follows the common approach in equilibrium search models with worker--firm heterogeneity that treat types as time-invariant primitives (e.g., Lise and Robin, 2017). Consistent with this assumption, firm-type assignments are highly stable ($\rho = 0.90$ between the pre-MW and full-sample models), and worker types show substantial alignment among workers observed in both periods ($\rho = 0.69$). The type distribution of post-policy worker entrants is also similar to that of pre-policy workers.

Following Lentz et al. (2023), we use k-means clustering to guide the choice of (K, L) using wage and mobility information. For workers, the Calinski–Harabasz index shows a clear elbow at $K = 6$, which we adopt. For firms, the index declines monotonically, so we set $L = 10$ as a balance between flexibility and interpretability; results are robust to nearby choices of L. Appendix B provides details.

4.4 Identification

Identification in this class of models follows from Bonhomme et al. (2019), who show that, in static matched employer-employee models, mixture models are nonparametrically identified with at least two wage observations per worker. Lentz et al. (2023) provide a complementary identification argument: their proof rests on three key conditions. First, firms must differ sufficiently in terms of observed wage and mobility patterns. Second, all transitions between types must occur with positive probability, conditional on type assignments. Third, the conditional wage distributions must be linearly independent across worker types.

Our estimation uses a monthly panel spanning seven years, with workers restricted to those having at least five wage observations in order to ensure precise type estimation. As described in Section 4.3, we select $K = 6$ worker types and $L = 10$ firm types based on pre-MW wage and mobility information. This choice guarantees sufficient heterogeneity across firms and workers to satisfy the first condition. For the second condition, we verify that all estimated transition cells are populated in the data. For the third, mean wages differ substantially across worker types, consistent with the linear independence of conditional wage distributions.

Identification of Bradley-Terry specification for $P_{k\ell\ell'}(x)$ in (3) follows directly from Lentz et al. (2023). When a worker draws a same-type job ($\ell = \ell'$), then the move occurs with probability $1/2$, hence $M(\ell|k,\ell,x) = \lambda_{k\ell}(x)/2$ trivially identifies $\lambda_{k\ell}(x)$ for all k, ℓ, x. Given $\lambda_{k\ell'}(x)$, the choice probabilities are identified by $P_{k\ell\ell'}(x) = M(\ell' \mid k, \ell, x)/\lambda_{k\ell'}(x)$. For $\ell' \neq \ell$, ratios $\gamma_{k\ell'}(x)/\gamma_{k\ell}(x)$ are identified by the odds ratios $P_{k\ell\ell'}(x)/(1 - P_{k\ell\ell'}(x))$ under the normalization $\sum_{\ell=1}^{L} \gamma_{k\ell}(x) = 1$.

5 Pre-policy labor market structure: worker and firm types

Before evaluating the effects of the minimum wage policy, we first describe the key characteristics of the worker and firm types estimated from pre-policy data. This section presents the baseline labor market's

Table 2: Average characteristics by worker type (pre-policy period)

type name (1)	k (2)	earnings (3)	women (4)	ln(wage) (5)	var (6)	EU (7)	UE (8)	EE (9)
low-pay job hopper	1	55,887	0.54	8.476	0.059	0.027	0.006	0.022
low-pay job stayer	2	82,789	0.47	8.761	0.037	0.015	0.005	0.012
mid-pay job hopper	3	99,311	0.46	8.930	0.071	0.029	0.006	0.018
mid-pay job stayer	4	109,743	0.47	9.082	0.033	0.011	0.005	0.009
high-pay job stayer	5	153,867	0.52	9.339	0.047	0.010	0.004	0.009
high-pay job climber	6	304,300	0.40	10.074	0.071	0.007	0.006	0.014

Notes: the table shows average characteristics of each worker type during the pre-policy period (2008 Q1-2012 Q1). Averages are weighted by their matching probabilities $p(k,\ell,x)$. See main text for details.

sorting, focusing on wage and mobility patterns as well as the observable characteristics associated with each worker and firm type.

5.1 Relabeling of worker and firm types

To facilitate interpretation—particularly in the context of poverty-reducing policy effects—we relabel worker types k according to their average annual earnings in the year prior to the reform. Firm types ℓ are relabeled analogously, based on their average annual earnings of workers employed at firms of type ℓ before the reform. As a robustness check, we also consider alternative orderings based on average mean wages across types, that is, the mean wage of worker type k averaged across all firms, and the mean wage of firm type ℓ averaged across all workers. The ordering of worker types is unchanged under this alternative, and the ordering of firm types is largely preserved.[14]

5.2 Worker types

Table 2 presents the average characteristics of each worker type before the policy. To aid interpretation, we assign intuitive labels to each type based on their wage levels and mobility patterns. Column (1) lists type names, while column (2) shows the corresponding type index k. Column (3) reports average annual earnings in 2011 in baht, which increase monotonically with the type index by construction (Section 5.1). Column (4) shows the share of women in each type, revealing a lower female share among higher-earning types, consistent with gender wage disparities. Columns (5)–(9) summarize average model parameters by worker type, averaging over firm types ℓ and covariates x, weighted by their matching probabilities $p(k,\ell,x)$. These include the mean log wages $\mu_{k\ell}(x)$, within-type wage variance $\sigma^2_{k\ell}(x)$,

[14] Robustness is further confirmed by a linear projection of the form

$$\mu_{k\ell}(x) = a_k + b_\ell + \bar{\mu}(x) + \tilde{\mu}_{k\ell}(x)$$

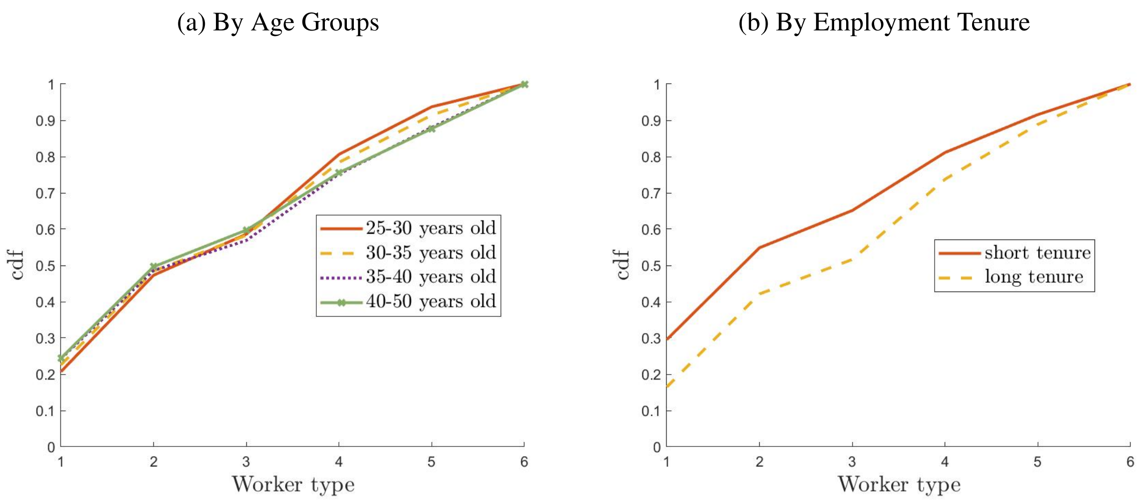
where a_k and b_l are worker and firm type dummies, $\bar{\mu}(x)$ captures covariate effects, and $\tilde{\mu}_{k\ell}(x)$ is the residual or match effect. Under this specification the ordering of worker types is unchanged, and the ordering of firm types is largely preserved.

monthly employment-to-non-employment (EU) transition rate $\delta_{k\ell}(x)$, non-employment-to-employment (UE) transition rate $\psi_{k\ell}(x)$, and job-to-job (EE) transition rate $M_{k\ell\ell'}(x)$.

To benchmark levels, full-time work at the new minimum wage corresponds to roughly 93,600 baht per year.[15] Types 1 and 2's earnings fall below this threshold. Since type 1 displays higher EU and EE mobility, while type 2 shows longer job durations, we label them low-pay job hopper and low-pay job stayer, respectively. Types 3 and 4, with earnings around the minimum wage, are mid-pay job hopper and mid-pay job stayer. Type 5 is high-pay job stayer, while type 6 is high-pay job climber, combining the highest earnings with a relatively high EE rate, consistent with climbing the wage ladder via employer-to-employer moves.

Figure 5a plots the cumulative distribution of worker types by age (constructed from the cross-

Figure 5: Cumulative distribution of worker types (pre-policy period)



Notes: The left panel shows the cumulative distribution of worker types by age group, and the right panel shows the cumulative distribution by employment tenure during the pre-policy period (2008Q1–2012Q1). Short tenure is defined as less than 12 months of employment.

sectional distribution $p(k,\ell,x)$). Differences by age are modest: workers aged 35–50 are slightly more represented among higher types than those aged 25–35. Figure 5b shows sharper contrasts by tenure: lower types are disproportionately in short-tenure jobs (under 12 months), while higher types are more often long-tenured—consistent with the higher EU and EE rates for lower types in Table 2. Because tenure is endogenous, it is important that the model allows transition probabilities to vary with tenure, which we do.

Finally, these averages provide a useful overview but mask heterogeneity across age, tenure, and firm matches. For a given worker type, wage and mobility patterns vary systematically with x and ℓ. Worker types also differ in the firm types they tend to match with—both when entering from non-employment

[15] This is based on the new minimum of 300 baht per day and evidence from the Thai SES that private-sector workers aged 25–50 typically work 26 days per month.

Table 3: Average characteristics by firm type (pre-policy period)

ℓ (1)	earnings (2)	women (3)	size (4)	age (5)	no.firms (6)	ln(wage) (7)	var (8)	EU (9)	UE (10)	EE (11)	<MW% (12)
1	34,660	0.46	4.32	3.76	33,059	7.902	0.032	0.024	0.001	0.001	0.99
2	51,641	0.42	2.72	3.37	82,114	8.355	0.024	0.022	0.002	0.001	0.98
3	57,432	0.43	24.11	3.70	13,370	8.416	0.041	0.026	0.003	0.003	0.96
4	68,145	0.51	226.17	3.78	3,515	8.530	0.060	0.026	0.008	0.015	0.90
5	83,566	0.49	5.23	3.46	107,737	8.792	0.031	0.019	0.005	0.004	0.70
6	96,252	0.47	40.26	3.89	18,968	8.888	0.039	0.017	0.004	0.009	0.56
7	100,320	0.51	365.94	4.08	3,087	8.869	0.051	0.016	0.005	0.022	0.64
8	143,403	0.43	182.83	3.82	7,852	9.246	0.052	0.012	0.006	0.019	0.31
9	147,221	0.49	8.85	3.55	68,743	9.296	0.048	0.016	0.004	0.012	0.21
10	264,234	0.51	27.11	3.61	25,925	9.904	0.062	0.008	0.005	0.013	0.04

Notes: the table shows average characteristics of each firm type during the pre-policy period (2008 Q1-2012 Q1). Averages are weighted by their matching probabilities $p(k,\ell,x)$. See main text for details.

and when making EE transitions. Mean wages are nonlinear in (k,ℓ) and are higher for older and longer-tenured workers. These patterns suggest that outcomes are shaped not only by worker type but also by interactions with firm type, which we turn to next.

5.3 Firm types

As with workers, we relabel firm types ℓ by the average annual earnings of their employees in the year prior to the minimum wage reform (see Section 5.1). Higher-indexed firm types therefore correspond to higher-paying firms. For simplicity, we refer to firms as higher- or lower-paying without assigning descriptive labels, since the focus of policy evaluation is on workers.

Table 3 summarizes the average characteristics of each firm type in the pre-policy period. Column (1) lists the firm type index, while column (2) reports average annual earnings which rise monotonically with type index by construction. Column (3) shows the share of female employees, which ranges from 0.40 to 0.51 with no clear pattern across firm types. Column (4) presents the average size, column (5) shows the average age with little variation across firm types, and column (6) presents number of firms in each type. The lowest-paying firms (types 1–3) tend to be smaller, while higher-paying firms are larger on average. Columns (7)-(11) report average model parameters for each firm type, weighted by their matching probabilities $p(k,\ell,x)$ across worker types k and covariates x. These include the mean log wage $\mu_{k\ell}(x)$, within-type wage variance $\sigma^2_{k\ell}(x)$, EU transition rate $\delta_{k\ell}(x)$, UE transition rate $\psi_{k\ell}(x)$, and EE transition rate $M_{k\ell\ell'}(x)$.

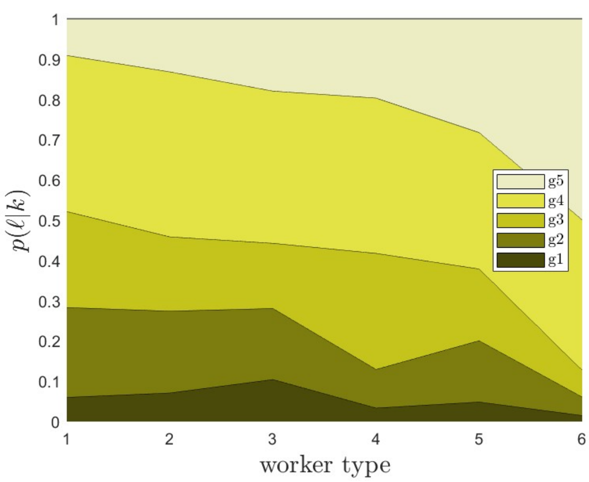
Average wages generally align with the firm-type ranking. Higher-type firms tend to offer more stable employment, as reflected in lower EU rates of their workers, and also provide greater opportunities for job-to-job transitions, as indicated by higher EE rates.

The final column reports the share of workers earning below the new minimum wage prior to the policy. Nearly all workers in low-type firms (types 1–4) earned below the threshold, mid-type firms (5–7) had between 50-70 percent affected, and high-type firms (8–10)—especially type 10—had relatively few affected workers. The variation in pre-policy exposure across firm types provides a useful indication of where employment reallocation may occur, which we examine later in the paper.

5.4 Sorting patterns

We now examine the sorting patterns between worker and firm types in the pre-policy period. Figure 6 displays the distribution of firm types conditional on worker type, $p(\ell|k)$. For legibility, we group firm

Figure 6: Firm type composition by worker type



Notes: The area chart displays the distribution of firm types conditional on worker type, $p(\ell|k)$. For presentation purposes, we aggregate over age and tenure groups and group firms into five categories labeled g_1 through g_5 where g_1 includes firm types 1-2,..., and g_5 includes firm types 9-10.

types into five categories: (1–2), (3–4), (5–6), (7–8), and (9–10). We also aggregate over age and tenure, since the sorting patterns are broadly similar across these dimensions.

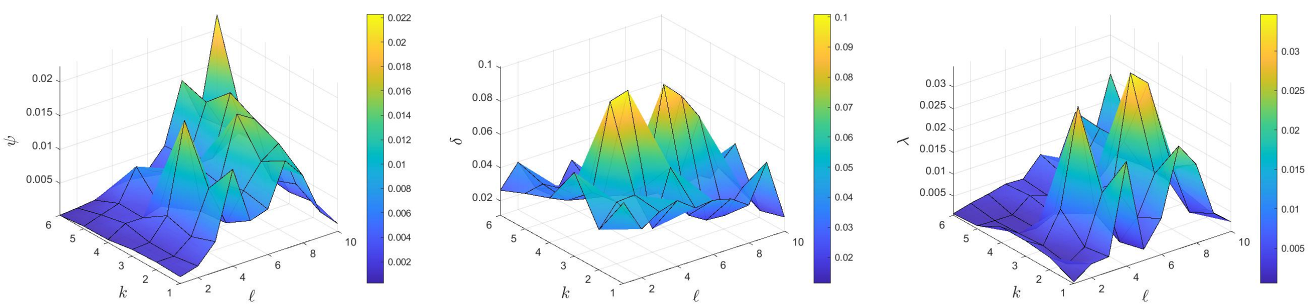
The figure reveals a clear pattern of positive assortative matching: lower-type workers are more likely to be employed at lower-type firms, while higher-type workers concentrate in higher-type firms. The pattern is robust to whether we relabel firm and worker types by their wage fixed effects or pre-policy average earnings. This together with our estimated model parameters, highlights the pre-policy structure of the labor market, where multiple mechanisms likely shape employment matches. Our framework is not an equilibrium model, but insights from the sorting literature help interpret why higher-type workers tend to sort into higher-type firms and how such dynamics may change after the reform.

One strand of theory, rooted in the frictional partnership model of Becker (1973), e.g., Shimer and Smith (2000) and Lise et al. (2016), emphasizes that sorting can emerge from differences in how workers rank firms. In these models, the relative allocation of workers across firms is shaped by ordinal rankings over matches, so differences in rankings across worker types can generate assortative matching.

A second mechanism, highlighted in Lentz (2010) and Bagger and Lentz (2019), is heterogeneity in

Figure 7: Parameter estimates by worker and firm type for young, short-tenure workers

(a) UE transition rate (b) EU transition rate (c) On-the-job offer arrival rate



Notes: The figure reports estimates for 25–30-year-old, short-tenure workers. The left panel shows UE rates, the middle panel shows EU rates and the right panel shows offer arrival rates on the job.

search intensity and expected gains. In this framework, all workers agree on the ranking of firms, but higher-type workers have stronger incentives (or lower costs) to search for better matches, leading them to target and access higher-type firms more intensively. Sorting results from the combination of common rankings and the varying intensity with which workers pursue more desirable matches.

Figure 7 illustrates this mechanism using parameter estimates for 25–30-year-old, short-tenure workers (parameters for other age-tenure groups are broadly similar). The left panel shows UE transition rates, indicating that higher-type workers are more likely to match with higher-type firms upon re-entry into employment. The middle panel presents EU separation rates and shows that high-pay workers (types 5–6) are more likely to leave lower-type firms than higher-type firms, consistent with search away from suboptimal matches. The right panel shows on-the-job offer arrival rates, highlighting that higher-type workers—particularly types 5 and 6—receive more offers from higher-type firms while employed. Together, these patterns suggest that high-type workers move more quickly toward high-type firms and receive more offers there, consistent with Lentz (2010) and Bagger and Lentz (2019).

At the same time, the final allocation across firm types also depends on choices conditional on offers, which reflect workers' relative valuations of firm types. In our model, EE mobility combines offer arrival and acceptance probabilities; thus, observed job-to-job moves reflect both the opportunities workers receive and how they rank jobs. As we show next in Section 5.5, these preferences differ across worker types, consistent with frictional matching models (Becker 1973; Shimer and Smith 2000; Lise et al. 2016). Overall, the evidence supports a view of sorting shaped jointly by search frictions/opportunity sets and preference heterogeneity.

5.5 Job Preferences

Following Sorkin (2018), Bagger and Lentz (2019) and Taber and Vejlin (2020), job-to-job mobility provides information about worker preferences: a move reflects a revealed preference for the new match over the old one. As discussed in Section 5.4, EE mobility reflects both the likelihood of receiving offers and workers' acceptance decisions conditional on offers. Recall that the probability of an EE move is

$$M(\ell'|k,\ell,x) = \lambda_{k\ell'}(x)\frac{\gamma_{k\ell'}(x)}{\gamma_{k\ell'}(x)+\gamma_{k\ell}(x)} \tag{7}$$

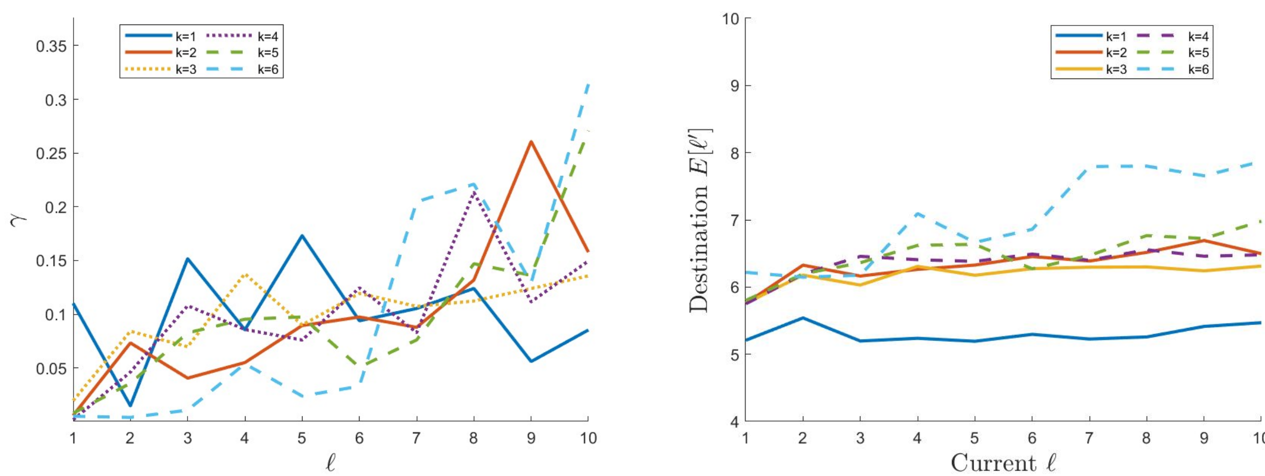
where $\lambda_{k\ell'}(x)$ is the on-the-job offer arrival rate (chance) and $\gamma_{k\ell}(x)$ measures match quality (choice). Section 5.4 shows that higher-type workers tend to receive more offers from higher-type firms; however sorting also depends on acceptance decisions conditional on offers. The ranking of $\gamma_{k\ell}(x)$ therefore provides a direct view of how preferences shape the worker–firm allocation.

Figure 8a plots an example of $\gamma_{k\ell}(x)$ for ages 25-30, short-tenure workers (see Figure A.6 in Appendix

Figure 8: Worker match preferences and expected destination firm type (pre-policy period)

(a) Match preferences $\gamma_{k\ell}(x)$: 25- 30 years old, short-tenure

(b) Expected destination firm type: EE moves



Notes: The left panel shows worker match preferences $\gamma_{k\ell}(x)$ for 25-30 years old short tenure workers in the pre-policy period. The right panel shows the expected destination firm type conditional on the origin firm type for each worker type, computed from the estimated EE transition probabilities. We average over covariates x using the observed match distribution $p_{k\ell}(x)$.

F for other age-tenure groups). The patterns are broadly similar across groups: match quality rankings vary across worker types, consistent with frictional matching models (Becker 1973; Shimer and Smith 2000; Lise et al. 2016), which show that differences in how workers rank firms can generate sorting. While $\gamma_{k\ell}(x)$ generally rises with firm type ℓ (reordered by average earnings), it is not strictly monotonic, suggesting that non-wage factors also influence preferences.

To better understand which match attributes shape preferences, Table 4 reports correlations between $\gamma_{k\ell}$ and key model parameters, averaging over worker types and covariates x. Workers generally prefer matches with higher wages ($\mu_{k\ell}$) and lower separation risk ($\delta_{k\ell}$). Job preferences and the on-the-job offer arrival rate ($\lambda_{k\ell}$) are only weakly correlated, suggesting that workers' opportunities while employed do

Table 4: Correlation between model parameters and job preferences

Correlation		Pre-MW
$\rho(\gamma_{k\ell}(x), \mu_{k\ell}(x))$	wage	0.36
$\rho(\gamma_{k\ell}(x), \delta_{k\ell}(x))$	job stability	-0.32
$\rho(\gamma_{k\ell}(x), \lambda_{k\ell}(x))$	EE offer rate	0.10
$\rho(\gamma_{k\ell}(x), \psi_{k\ell}(x))$	UE rate	0.47

Notes: Correlations are first calculated over firm types ℓ and then averaged over worker types k for given values of tenure and experience x. Uniform weights are applied when aggregating correlations across worker types and covariates x.

not strongly tilt toward their most-preferred firm types. In contrast, non-employment search ($\psi_{k\ell}$) aligns more closely with worker preferences, reflecting more directed job-finding efforts while non-employed. Overall, match value captures variation in wages, job stability, and employment opportunities.

To assess the link between EE mobility and preferences, we compute the expected destination firm type conditional on the origin firm type for each worker type, using the estimated EE transition probabilities $M(\ell'|k,\ell,x)$, and averaging over covariates x. Figure 8b shows that workers of types 1-5 tend to move toward a common intermediate firm type (around $\ell = 5-7$) with little variation across their origins, whereas the highest type 6 often moves further up the firm ladder, with expected destinations approaching the top firm types. These results illustrate how EE mobility and acceptance decisions contribute to the positive sorting documented in Section 5.4.

In sum, more valued jobs tend to be more remunerative and stable. Search while non-employed is more targeted than on-the-job search, and EE mobility generates gradual upgrading for some groups. These patterns imply that a minimum wage reform can affect sorting not only through changes in firms and opportunities but also through shifts in match values and acceptance behavior, potentially in type-specific ways. These wage and mobility objects will therefore be central for interpreting the reform's effects, which we turn to next.

6 Reduced-form policy effects

Building on the pre-policy characterization of worker and firm heterogeneity in Section 5, this section presents the reduced-form short-run effects of the minimum wage on labor market outcomes. We document changes in earnings and non-employment probabilities around the reform, adjusting for pre-policy trends.

Because the reform was large and nationwide, no worker group provides a clean untreated counterfactual. Our baseline reduced-form estimates therefore compare outcomes before and after the reform, netting out pre-reform trends. Reassuringly, the macro series in Figure 2 show no other major contemporaneous shocks around the reform period. We also complement these estimates with an exposure-based specification that exploits cross-province differences in how binding the new wage floor was; results are reported in Appendix C. Because the two hikes were only nine months apart, we treat them as a single reform episode and work with annualized outcomes.

6.1 Earnings effects by worker type

Figure 9 shows average changes in annual earnings among workers with positive formal-sector earnings by worker type following the minimum wage reform, adjusted for pre-policy trends in 2010. We define annual earnings as income earned from Q2 of year $t-1$ to Q1 of year t, so the "2013" outcome reflects Q2 2012–Q1 2013 and spans both phases of the policy. We present the average effects across age groups as patterns were broadly similar across age bins.

Ideally, we would compare treated and untreated groups using a standard difference-in-differences design. However, given the scale of the policy, there is unlikely to be a clean control group: even the highest-type workers experienced earnings increases that are difficult to attribute solely to macroeconomic trends. We therefore focus on changes relative to pre-policy trends within worker types, and we report an exposure-based specification exploiting cross-province differences in how binding the new wage floor was in Appendix C.

We see earnings gains across all worker types, with substantially larger increases among lower types. In Figure 9 low-pay job hoppers (type 1) experienced gains of roughly 40 percent. Earnings increases decline along the worker-type distribution with spillovers beyond direct targets: even high-pay job climbers (type 6) saw an increase of about 10 percent.[16] This pattern is consistent with the policy's intent to raise earnings for low-wage workers while also highlighting broader effects. The differential gains across types highlight heterogeneous policy effects, which we explore further through non-employment outcomes.

6.2 Non-employment effects by worker type

Much of the minimum wage literature finds little evidence of disemployment effects, particularly among workers who are already employed (e.g., Card and Krueger (2000); Dube et al. (2016)). Most studies focus on the separation margin and show that employed workers are not significantly more likely to lose their jobs after a minimum wage increase. Less attention has been paid to the impact on job-finding among the non-employed, especially those with longer spells of non-employment. If existing matches become less likely to separate after the reform, firms may reduce vacancy posting, potentially making it harder for non-employed workers—particularly the less attached—to find jobs.

To study this, we divide non-employment into two groups: (i) workers who have been non-employed for less than seven months (short-term non-employed), and (ii) those non-employed for more than seven

[16] Monthly earnings in the Social Security data are top-coded at 15,000 baht; we impute upper-tail wages as described in Section A. We do not use Social Security's separate self-reported uncensored earnings in the main analysis because they are available only each January in 2011–2014 and have about 15% potentially non-random missingness. As a check, however, they imply growth of roughly 6–7 percent at the 90th percentile (Appendix A.2), suggesting that spillovers among higher-paid workers are not driven solely by the imputation.

Figure 9: Changes in annual earnings by worker type, adjusted for pre-policy trends

Notes: This figure plots average changes in earnings by worker type after the minimum wage policy, adjusting for pre-policy trends using 2010 data. Worker types are labeled $k = 1,..,6$ are ordered by pre-policy earnings. Earnings for each year are measured from Q2 of year t to Q1 of year $t+1$, so the 2013 outcome reflects income after both MW hikes (April 2012 and January 2013).

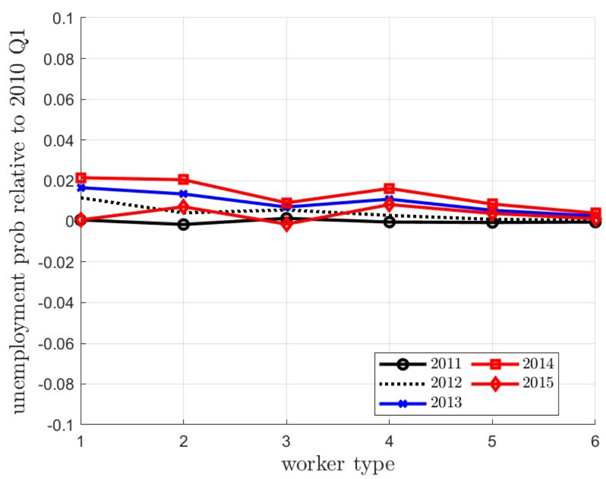
months (long-term non-employed). This distinction captures differences in labor market attachment and job-finding likelihood.

The top panel of Figure 10 shows aggregate non-employment probabilities by worker type in Q1 of each year, relative to 2010. We observe a small increase in non-employment across all types in 2013—following the minimum wage hikes—except for the highest worker type, whose non-employment remained unchanged. The bottom panels of Figure 10 disaggregate these changes by non-employment duration. Among workers with short non-employment spells (bottom left), the probability of remaining non-employed declines after the policy, particularly among the lower types. This suggests that more attached workers faced lower job separation risk post-policy, consistent with evidence that minimum wages can reduce separation rates and improve match stability (Dube et al. (2016); Gittings and Schmutte (2016)). In contrast, long-term non-employment rises after the policy, especially among lower types, suggesting that less-attached workers faced greater difficulty re-entering employment post-reform.

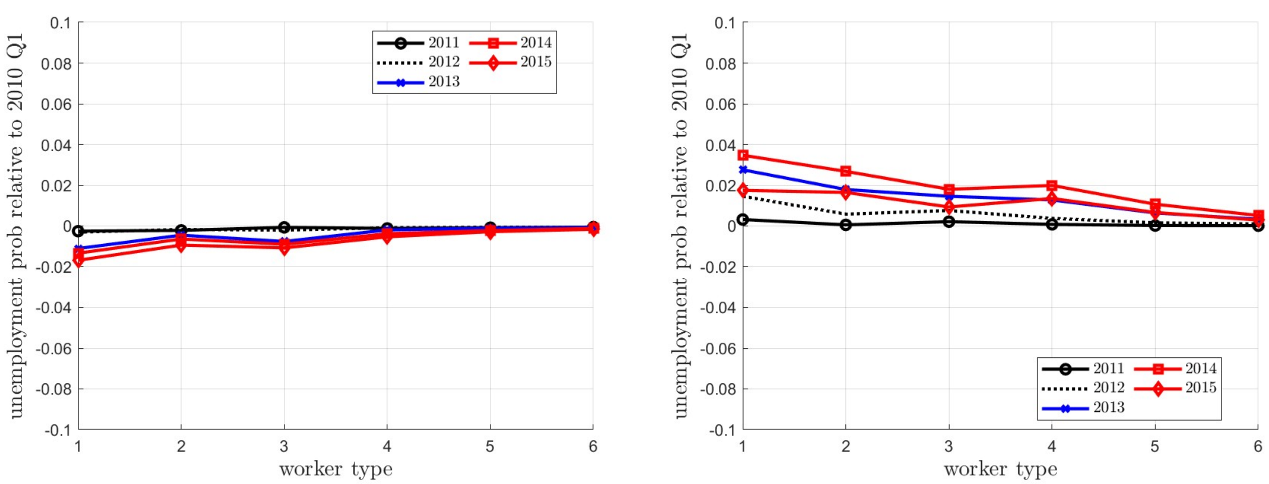
In summary, we find little evidence of adverse employment effects for currently employed or recently non-employed workers. However, the policy appears to have modestly reduced job-finding rates for the long-term non-employed. These asymmetric effects across labor market attachment margins raise questions about the policy's long-term gains as workers cycle in and out of employment. We next use the estimated model parameters to shed light on the mechanisms behind these patterns and their implications for lifetime earnings.

Figure 10: Changes in non-employment probability by worker type and non-employment duration

(a) Aggregate non-employment probability



(b) Non-employment probability of short term (left) and long term (right)



Notes: The top panel plots aggregate non-employment probabilities for each worker type in Q1 of each year, relative to 2010. The bottom panels disaggregate by non-employment duration: less than seven months (left) and more than seven months (right). All outcomes are measured in Q1, and worker types are ordered by pre-policy average earnings.

Table 5: Wage changes by worker type, tenure and age

	Pre-MW					Post-MW				
k	ave. wage	tenure L-S	age 2-1	age 3-2	age 4-3	ave. wage	tenure L-S	age 2-1	age 3-2	age 4-3
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
1	8.476	0.112	0.012	-0.020	-0.004	8.951	0.101	0.028	-0.024	-0.039
2	8.761	0.140	0.048	-0.070	-0.025	9.146	0.129	0.076	-0.060	-0.074
3	8.930	0.738	0.142	0.185	0.375	9.266	0.407	0.047	0.065	0.294
4	9.082	0.112	0.053	0.032	0.018	9.391	0.141	0.049	-0.018	-0.016
5	9.339	0.152	0.118	-0.032	-0.029	9.635	0.157	0.097	-0.020	-0.121
6	10.074	0.088	0.062	0.003	-0.120	10.248	0.095	0.015	-0.062	-0.129

Notes: The table reports average log wages and wage returns by worker type. Column (1) lists worker type, column (2) average log wages, column (3) returns to tenure defined as the wage increment from short to long tenure within a match, and columns (4)–(6) returns to age as workers move across successive groups where group 1 is 25–30; group 2 is 30–35; group 3 is 35–40; and group 4 is 40–50 years old. Results are shown separately for the pre- and post-policy periods.

7 Structural mechanisms: wages, mobility, preferences

The reduced-form results in Section 6 show substantial heterogeneity in earnings gains and non-employment responses across worker types. We now use the estimated model parameters to interpret the mechanisms behind these patterns. We first examine wage adjustment across worker and firm types, including changes in returns to age and tenure (Section 7.1). We then analyze changes in mobility and preferences (Section 7.2) and shifts in the firm distribution (Section 7.3). Finally, we examine how these changes affect worker–firm sorting (Section 7.4).

7.1 Changes in wages

We begin with mean wage adjustments. Table 5 reports changes in mean wages by worker type, age, and tenure. Column (1) lists the worker type; column (2) shows average log wages by type, aggregated over firm types, age groups, and tenure statuses during the pre-policy period; column (3) shows average wage returns to tenure, defined as the wage increment associated with moving from short to long tenure within a match; and columns (4)–(6) present average wage returns to age as workers move across successive age groups (25–30, 30–35, 35–40, and 40–50). The right panel presents the post-policy counterparts.

Several patterns emerge. First, mean wages rise sharply for low-pay workers (types 1–2), with gains tapering across the type distribution, indicating sizable spillovers beyond the policy's direct targets. Second, returns to tenure compress for lower types: types 1–3 continue to experience wage growth with tenure, but the increment is substantially smaller post-policy, with the strongest compression for type 3. This pattern is consistent with firms raising entry wages for affected workers while partially offsetting the higher starting pay through flatter within-job wage growth over tenure. By contrast, returns to tenure change little—and if anything rise slightly—for higher types (types 4–6), consistent with internal wage ladders being less directly affected when short-tenure wages are further above the wage floor. Third, age

Table 6: Changes in mobility parameters by worker type

	Pre-MW			Post-MW		
k	EU	UE	EE	EU	UE	EE
(1)	(3)	(4)	(5)	(7)	(8)	(9)
1	0.027	0.006	0.022	0.018	0.003	0.017
2	0.015	0.005	0.012	0.012	0.003	0.011
3	0.029	0.006	0.018	0.019	0.004	0.013
4	0.011	0.005	0.009	0.010	0.003	0.008
5	0.010	0.004	0.009	0.009	0.002	0.008
6	0.007	0.006	0.014	0.006	0.004	0.012

Notes: the table shows changes in average model parameters of each worker type between pre-MW and post-MW periods, weighted by their respective matching probabilities $p(k,\ell,x)$. See text for explanation.

premia are broadly stable across types before and after the reform.

Why is tenure-based wage growth compressed most strongly for type 3? A key feature of the estimates is that type 3 exhibits the steepest pre-policy wage–tenure profile. When the reform raises entry wages for this group, there is greater scope for firms to accommodate the higher starting point by compressing subsequent within-job wage growth while remaining above the new wage floor. By contrast, types 1–2 have flatter pre-policy wage growth, leaving less room for further compression, and types 4–6 are less exposed to the wage floor.

For completeness, we show analogous results by firm type in Appendix F. There, average wages increased more uniformly across firms, and returns to age and tenure changed only modestly. This highlights that wage adjustments were more pronounced by worker type than by firm type. However, these are average wages. Individual earnings trajectories depend on which firm type a worker matches with, how long they remain, and where they move next—issues we address in the next subsection on mobility.

7.2 Changes in mobility and preferences

Table 6 reports transition rates by worker type before and after the minimum wage reform. Three patterns stand out. First, EU rates fall across the board, with the largest declines for job hoppers (types 1 and 3), indicating higher job stability after the reform. Second, EE rates also decline, most sharply for type 3 (and to a lesser extent type 1), indicating less job-to-job turnover for the groups that were most mobile pre-policy. Consistent with this, post-policy offer arrival while employed becomes less tilted toward higher-paying firms for type 3, weakening outside options; this helps rationalize why tenure-based wage growth compresses most for this group (we quantify the role of mobility channels in Section 8). Third, UE rates fall for all types, suggesting slower job-finding from non-employment in the post-policy period. Together, these changes point to reduced labor-market churning. However, lower transition rates do not mean that there was no meaningful reallocation of workers across firms; we document these changes in the next subsection. By contrast, mobility parameters by firm type (see Appendix F) show smaller changes, underscoring that post-policy mobility adjustments were more pronounced on the worker side.

We next examine how the relationship between preferences and match attributes changes after the reform. Table 7 summarizes correlations in the post-policy period, averaging over worker types and the

Table 7: Correlation between model parameters and job preferences

Correlation		Pre-MW	Post-MW
$\rho(\gamma_{k\ell}(x), \mu_{k\ell}(x))$	wage	0.36	0.60
$\rho(\gamma_{k\ell}(x), \delta_{k\ell}(x))$	job stability	-0.32	-0.47
$\rho(\gamma_{k\ell}(x), \lambda_{k\ell}(x))$	EE offer	0.10	0.20
$\rho(\gamma_{k\ell}(x), \psi_{k\ell}(x))$	UE offer	0.47	0.59

Notes: Correlations are computed across firm types ℓ, then averaged over worker types k for given tenure and experience x. Uniform weights are applied when averaging across firm types ℓ and x.

covariates x. As in the pre-policy period, workers prefer matches with higher wages ($\mu_{k\ell}$) and lower separation risks ($\delta_{k\ell}$), and job-finding from non-employment ($\psi_{k\ell}$) is more tilted toward desirable matches. These correlations become stronger post-policy, suggesting that the reform amplified the alignment between wages, stability, and match values. By contrast, search while employed ($\lambda_{k\ell}$) remains weakly correlated with preferences, consistent with limited scope to direct search while employed.

Finally, we consider the stability of preferences themselves. We compute, for each worker type, the correlation between pre- and post-policy $\gamma_{k\ell}(x)$, averaging across age and tenure (see Table A.7 in Appendix F). Preferences are less stable for lower and mid types—most notably type 3—while stability increases with type, with type 6 showing the strongest pre/post correlation. This points to larger post-reform changes in the acceptance/value margin for lower-to-mid workers than for the highest types.

Taken together, the estimates indicate two mechanisms: reduced turnover and shifts in preference rankings for lower and mid types. These changes imply that the offer distributions and firm composition faced by workers may also have shifted after the reform, which we examine directly in the next subsection.

7.3 Changes in the distribution of workers and firms

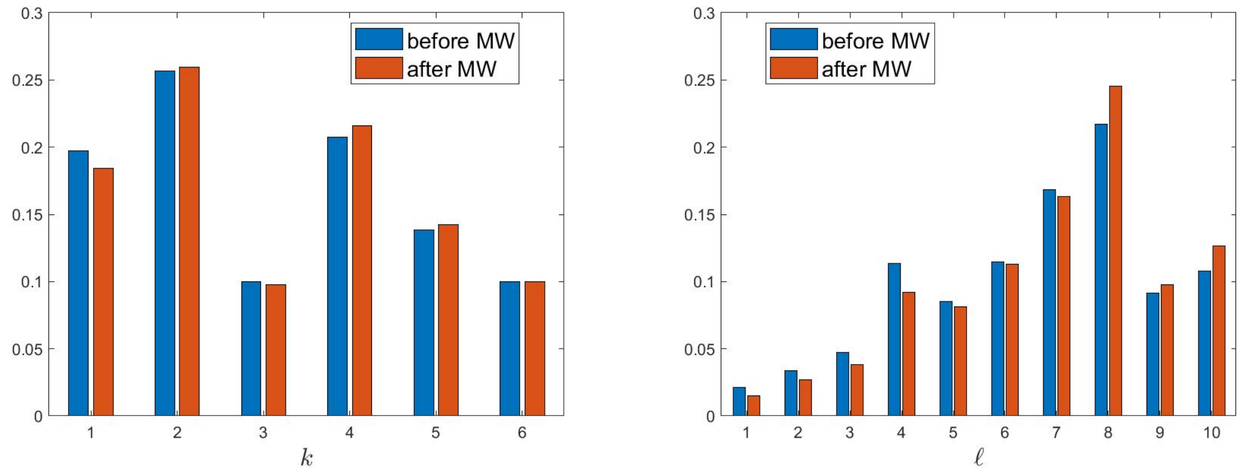
Figure 11a shows aggregate employment shares by worker type before and after the reform, restricting to workers observed pre-policy to hold composition fixed. The distribution is very stable: types 1–2 remain the most prevalent (with type 2 the largest group). Post-policy, type 1's employment share declines slightly, while types 4 (and modestly 5) gain share; other types change little. For robustness, we also compare the match distribution of workers who entered the sample after the reform. The additional 1.8 million workers are predominantly types 1–2. Most are age 25 and likely transitioning from school or training, consistent with their age profile.

Figure 11b plots the distribution of matches by firm type. Employment shares in firm types 1–5 decline, types 6–7 remain broadly stable, and types 8–10 expand. This shift aligns with pre-policy expo-

Figure 11: Aggregate employment shares by worker and firm types

(a) Aggregate employment shares by worker type

(b) Aggregate employment shares by firm type



Notes: The left panel shows the distribution of employment across worker types before and after the minimum wage reform. The right panel shows the distribution of employment across firm types before and after the minimum wage reform.

sure patterns in Table 3: low-type firms had a much larger share of workers below the new wage floor, implying a larger cost shock and stronger incentives to contract or exit, while high-type firms were less directly bound. This pattern is consistent with Dustmann et al. (2021) and Engbom and Moser (2022), who show that low-productivity firms tend to contract or exit, while high-productivity firms expand following a minimum wage reform. This suggests that workers—especially those previously concentrated in low-type firms—had to reallocate across the firm distribution, which we examine next when studying changes in sorting in Section 7.4.

7.4 Changes in sorting

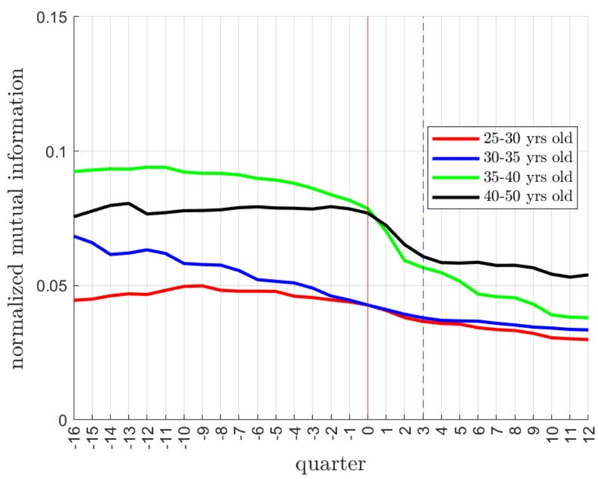
To measure sorting between worker and firm types over time, we first present mutual information (MI). A standard benchmark in the literature is the correlation between worker and firm wage components (or fixed effects). This statistic is informative about the direction and strength of linear assortative matching in wages, but it is not designed to capture nonlinear dependence in matching patterns or sorting driven by non-wage attributes. MI measures dependence between two variables X and Y without imposing a functional form. Specifically, it is the Kullback–Leibler distance between the joint distribution and the product of the marginals,

$$I(X,Y) = d_{KL}(p(X,Y)||p(X)p(Y)) = \sum_{x,y} p(x,y) \ln\left(\frac{p(x,y)}{p(x)p(y)}\right),$$

and we report the normalized version $\tilde{I}(X,Y) = I(X,Y)/\min[H(X), H(Y)]$,where $H(.)$ denotes entropy. Under independence, $\tilde{I} = 0$; under perfect dependence, $\tilde{I} = 1$.

Figure 12 plots quarterly normalized MI, with the solid vertical line marking the April 2012 hike and

Figure 12: Normalized mutual information by age group



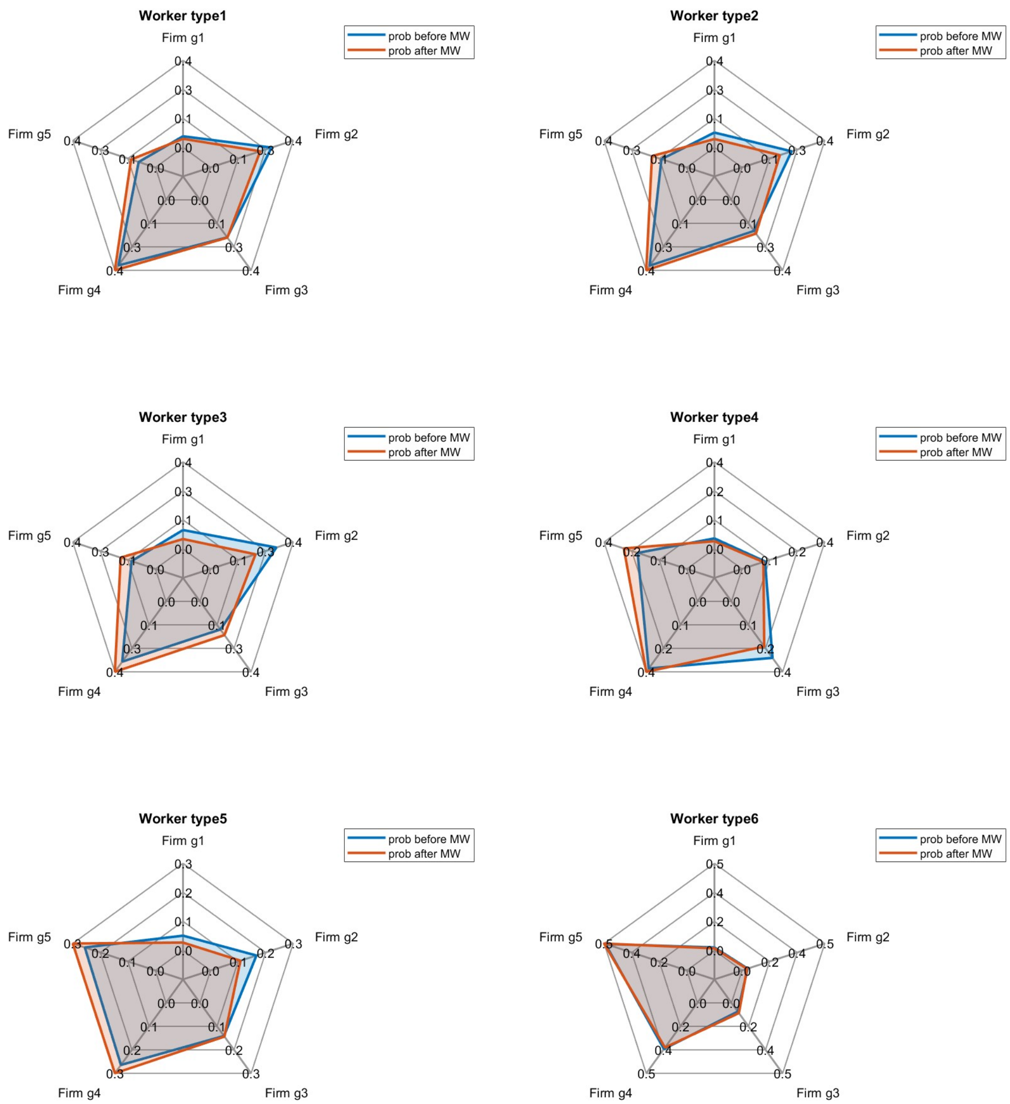
Notes: The figure plots quarterly normalized mutual information by age group. The solid red vertical line marks the first minimum wage hike in April 2012, and the dashed blue line marks the second hike in January 2013.

the dashed line the January 2013 extension. Sorting declined after the reform, with the sharpest drop for older age groups. This suggests that the policy weakened the dependence between worker and firm types, consistent with reallocation pressures discussed in the previous section.

Because MI does not indicate where workers reallocate, we complement it with the match-distribution plots in Figure 13. The figure shows the conditional probability $p(\ell|k,x)$ before and after the reform, grouping firms into five bins: group 1 (types 1–2), group 2 (3–4), ..., group 5 (9–10). The blue web shows pre-policy allocations; the orange web shows post-policy allocations. The post-policy distributions show reallocation away from the lowest firm groups toward higher firm groups for types 1–5, while type 6 remains concentrated at the top—consistent with high-type firms being less directly bound by the wage floor.

As a complementary measure of sorting, Appendix D reports the log-wage variance decomposition and the correlation between worker and firm wage components before and after the reform. Overall variance falls (0.40 to 0.33), indicating reduced wage inequality, and the worker–firm correlation declines modestly (0.31 to 0.29), pointing to a small weakening in wage-based assortative matching. The decomposition also shows a non-negligible nonlinear match component (about 5–9% of between-group variance), which declines after the reform, highlighting that changes in dependence need not be fully captured by linear wage-based correlations—motivating our use of MI alongside the correlation measure. Together, they point to weaker worker–firm dependence after the reform.

Figure 13: Distribution of worker types across firm types



Notes: The figure shows the distribution of worker types across five firm-type groups before (blue) and after (orange) the minimum wage reform. Firm types are grouped into five broader categories: group 1 (types 1–2), group 2 (3–4), group 3 (5–6), group 4 (7–8), and group 5 (9–10).

8 Long-run earnings, preferences, and informality

The reduced-form and structural evidence so far have shown sizable wage increases and upward reallocation of lower-type workers toward higher-paying firms, both of which point to higher earnings. At the same time, we find compressed returns to tenure and more persistent non-employment for some workers, which could dampen longer-run gains. This section asks three questions. First, what are the long-run implications of the reform for earnings across worker types, and which channels account for the gains? Second, do workers reallocate toward firms they value more highly according to the estimated preference parameters $\gamma_{k\ell}(x)$—that is, beyond earning more, are they also better matched? Third, how did the reform affect outcomes outside the Social Security data, particularly informality and other job characteristics such as hours and fringe benefits?

We address these questions as follows. Section 8.1 simulates lifetime earnings to decompose the sources of gains. Section 8.2 assesses whether reallocation is toward more-preferred firms. Section 8.3 uses Labor Force Survey data to study effects on informality and other job outcomes.

8.1 Long-term earnings decomposition

We use the estimated model to quantify how the minimum wage changes workers' discounted lifetime earnings and to decompose the sources of these earnings.[17] We simulate individuals of each type k starting from the estimated pre-policy initial match distribution. The simulation proceeds monthly. Tenure evolves stochastically with match allocations and durations according to the estimated transition rates; wage income is set to zero in non-employment. Because parameters are constant within each age bin, we simulate 5-year blocks within each age bin and construct a 20-year discounted lifetime measure by chaining these blocks across age bins. This approach provides a transparent mapping from the estimated age-bin model to simulated outcomes while allowing mobility responses to accumulate within each block. Discounted lifetime earnings are the present discounted value of monthly earnings using an annual discount rate of 5%. See Appendix E.1 for more details.

Policy-induced earnings gains can arise through two broad channels: wages and mobility. Because the model is nonlinear, these channels can interact. We therefore organize the decomposition as

$$\text{Full effect} = \underbrace{\text{Wage}}_{\mu,\sigma} + \underbrace{\text{Mobility}}_{\delta,\psi,\lambda,\gamma} + \underbrace{\text{Wage-Mobility Interaction}}_{\text{nonlinear complementarity/substitution}} \tag{8}$$

The full effect reflects all parameter changes from the pre- to post-policy environments. The wage channel sets wage parameters $(\mu_{k\ell}(x), \sigma_{k\ell}(x))$ to post-policy values while holding mobility parameters at pre-policy values. We further split the wage channel into an initial wage component (short-tenure wages) and a later wage component, defined as the implied change in the wage increment from short to long

[17] We focus on lifetime decomposition. One-year earnings gains are not directly comparable, because post-policy parameters are identified using multiple post years and because mobility responses accumulate over time.

tenure.[18] This distinction matters because firms may comply with a higher wage floor by raising entry wages while adjusting within-match wage progression to save labor costs.

The mobility channel sets parameters governing separations ($\delta_{k\ell}(x)$), job-finding ($\psi_{k\ell}(x)$), and job-to-job transitions—chance ($\lambda_{k\ell'}(x)$) and choice ($\gamma_{k\ell}(x)$)—to post-policy values while holding wage parameters at pre-policy values. This captures how earnings change through altered employment stability (EU/UE) and job-to-job reallocation (EE), even if wage schedules were unchanged. Within EE moves, we further distinguish "chance" and "choice". The residual between the full effect and the sum of wage and mobility effects is the wage–mobility interaction: positive values indicate complementarity (the channels reinforce each other), while negative values indicate substitution.

Table 8 reports the decomposition of NPV of discounted earnings over the 20-year horizon. We report

Table 8: Long-term earnings decomposition

Worker	Full	Wage			Mobility					Wage-Mob.
type	effect	Overall	Initial	Later	Overall	EU	UE	EE: λ	EE: γ	Interaction
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
1	69.34	52.62	50.29	2.33	11.51	4.22	4.16	-1.33	4.43	5.21
2	43.87	38.58	24.63	13.95	2.85	-0.17	5.31	-3.51	1.71	2.43
3	33.50	22.12	53.58	-31.45	7.54	4.34	7.88	-6.67	2.61	3.83
4	39.68	32.51	21.84	10.66	5.87	0.82	4.11	-1.01	2.00	1.31
5	39.77	29.67	17.42	12.26	8.31	-0.18	5.74	-0.98	3.33	1.79
6	25.68	24.55	18.62	5.93	1.42	-0.07	-0.24	0.37	1.40	-0.29

Notes: Column (2) reports the percent change in discounted lifetime earnings between the post- and pre-policy environments. Columns (3)–(5) decompose the wage channel into the overall wage effect (3), the initial-wage component (4; short-tenure wages), and the later-wage component (5; the implied change in wage growth from short to long tenure). Column (6) reports the mobility effect, with subcomponents from separations (EU, col. 7), job-finding (UE, col. 8), and employer-to-employer transitions split into chance and choice (cols. 9–10). Because mobility subcomponents are computed via separate counterfactuals, their sum need not equal column (6) due to small interaction terms within mobility. Column (11) reports the interaction between wage and mobility channels so that (2) = (3) + (6) + (11).

the same decomposition by age group in Appendix E.2; qualitative patterns are similar across age bins.

Several key findings emerge. First, the reform generates large gains in discounted lifetime earnings, with substantial heterogeneity across worker types. The full effect ranges from about 26% for type 6 to roughly 69% for type 1 (col. 2), reflecting differences in baseline wages, mobility patterns, and exposure to the minimum-wage policy across worker types.

Second, the wage channel is the dominant driver for most types, but the mobility channel is sizable for types 1, 3, and 5 (col. 6)—the more mobile types in the pre-policy period (types 1 and 3 are "job hoppers," while type 5 exhibits high pay with substantial stability). This indicates that changes in employment stability and reallocation meaningfully amplify lifetime gains beyond the direct wage schedule shifts. In contrast, for type 6 the mobility effect is close to zero, implying that most of the policy impact for this

[18] Age premia can be isolated analogously by replacing the age-bin increments in $\mu_{k\ell}(x)$ with their post-policy values while holding other parameters fixed. This counterfactual implies little change for types 1–2 but a compression of the age profile for mid/high types, especially type 3."

high-pay worker type operates through wages rather than altered mobility, consistent with the limited changes in their matching patterns documented in Section 7.4.

Third, the wage channel reveals an important distinction between initial wages and later wage growth. Most types see positive contributions from both initial wages (col. 4) and later wage growth (col. 5), but the relative importance varies. Notably, type 3 exhibits a pronounced tradeoff: a large initial wage gain coupled with a negative later component, consistent with a flattening of within-match wage growth after the reform. This pattern aligns with firms raising entry pay while compressing pay progression where feasible. At the very bottom of the wage distribution, there is limited scope to compress an already-flat ladder without risking retention; at the top, entry wages need not adjust much and internal pay-ranking constraints may limit compression. Middle types can therefore be where tenure-profile compression is most cost-saving, consistent with Table 5, the strongest compression is for type 3.

Fourth, within mobility, job-finding plays a central role, while EE contains offsetting forces. Job-finding (UE, col. 8) contributes positively for all types except type 6, and separations (EU, col. 7) improve earnings for some groups, reflecting that small monthly changes cumulate over long horizons through time spent employed. Columns (9)–(10) split EE into "chance" and "choice." A common pattern is positive choice but negative chance: conditional on receiving opportunities to move, workers reallocate toward better-paying firms, but such opportunities become less frequent (negative EE: λ). The 'chance' component is most negative for type 3; in models where outside offers discipline within-job wage growth, a decline in on-the-job offer arrival weakens outside options and can flatten tenure-based wage progression—consistent with the particularly strong post-policy compression in tenure returns for type 3 (Bagger et al., 2014; Bagger and Lentz, 2019).

Finally, the interaction between wage and mobility channels is generally small but informative about nonlinearity. Column (11) is mostly positive (or close to zero), suggesting complementarity. Intuitively, higher wages raise the payoff to being employed, so any policy-induced increase in employment stability or faster re-employment amplifies earnings gains. Conversely, as mobility shifts some workers toward better-paying firms, those additional employed months are disproportionately spent in higher-pay matches.

Taken together, these results show that the minimum wage did more than lift the pay floor. It reshaped the balance between entry wages and within-match wage growth, improved re-employment prospects for some groups, and altered worker-firm sorting patterns. Overall, it raised lifetime earnings, especially for low-type workers. These findings motivate the next subsection, which asks whether upward reallocation on the wage ladder also translated into moves toward firms that workers value more highly according to the estimated preference rankings.

8.2 Worker reallocation and job preferences

As discussed in section 5.5, workers are heterogeneous in their job preferences. To assess whether post-policy reallocations also reflect moves toward more-preferred firms, we compute a worker-type–specific weighted average rank of revealed match values:

$$\bar{\gamma}_k = \Sigma_\ell p_{k\ell}(x) \times \mathscr{R}(\gamma_{k\ell}(x)) \tag{9}$$

where $\mathscr{R}(.)$ denotes the rank of firm types by $\gamma_{k\ell}(x)$ within worker type k (1 = least preferred and 10 = most preferred). Ranks are computed separately for each worker type based on their pre-policy revealed-preference ordering of firms. Our benchmark results use pre-policy ranks to isolate reallocation across a fixed preference ladder. This avoids conflating worker movements with any policy-induced reshuffling of firms' relative desirability.

Table 9 reports, by worker type, the pre-policy average rank (Column (2)) and the distribution of

Table 9: Worker allocation across firm types before and after the minimum wage reform

		Percent by firm rank				Percent by firm rank			
Worker type	Avg. Rank	Top 3	Middle 4	Bottom 3	Δ Avg Rank	Δ Top 3	Δ Middle 4	Δ Bottom 3	Δ Alter. Avg Rank
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
1	6.09	26.67	60.30	13.03	-0.10	-3.66	2.74	0.92	-0.02
2	6.13	34.68	45.26	20.07	0.22	2.58	0.20	-2.77	0.04
3	6.72	46.86	37.01	16.13	-0.14	-4.26	4.91	-0.65	-0.02
4	6.92	48.18	38.10	13.72	0.07	0.61	0.38	-0.98	0.02
5	7.10	50.40	38.78	10.82	0.08	2.83	-3.69	0.86	0.03
6	8.45	75.77	21.41	2.82	0.09	2.10	-1.91	-0.19	0.04

Notes: Columns (2)–(5) report pre-policy values: the weighted average firm rank and the shares of workers in the top-3, middle-4, and bottom-3 firm types. Ranks are defined separately for each worker type, where 1 = least preferred and 10 = most preferred. Columns (6)–(9) show changes (post–pre) in these metrics, computed using post-policy match probabilities but the pre-policy ranking. Column (10) reports the change in the weighted average of $\gamma_{k\ell}(x)$ as a robustness measure.

matches across the top-3, middle-4, and bottom-3 ranks (Columns (3)–(5)). Columns (6)–(9) report changes (post minus pre) in these metrics, where the change in average rank is computed using post-policy match probabilities $p_{k\ell}^{\text{post}}(x)$, but the pre-policy ranking $\mathscr{R}^{\text{pre}}(\gamma_{k\ell}(x))$. Column (10) provides a robustness check based on the weighted average of $\gamma_{k\ell}(x)$ rather than its rank. In both Columns (6) and (10), a positive change indicates reallocation toward more-preferred firms, holding the preference ladder fixed.

The results reveal heterogeneous responses. Worker types 1 and 3 show modest declines in average rank, indicating a slight movement away from more-preferred firms. By contrast, types 2, 4, 5, and 6 all improve, with type 2 showing the largest gains. As shown in Column (10), results are qualitatively similar when we use the cardinal values of γ: types 1 and 3 worsen, type 2 improves substantially, and types 4–6 gain modestly. As a check, we also recompute ranks using the post-policy ordering of $\mathscr{R}^{post}(\gamma_{k\ell}^{post}(x))$. However, this alternative captures both reallocation and any policy-induced reshuffling in firms' relative desirability, so it is not directly comparable to our benchmark fixed-ladder measure. Under this metric, average ranks improve for nearly all worker types (though unevenly), consistent with higher-paying firm types becoming more attractive overall after the reform.

These preference-based results highlight that upward movement on the wage ladder need not coincide with movement toward more-preferred firms. In particular, even though the acceptance/choice margin of EE mobility contributes positively to earnings in the decomposition (Table 8), types 1 and 3 reallocate slightly toward lower-ranked firms on their pre-policy preference ladder. Wage gains therefore do not automatically imply better matches in preference terms, which matters for broader welfare conclusions.

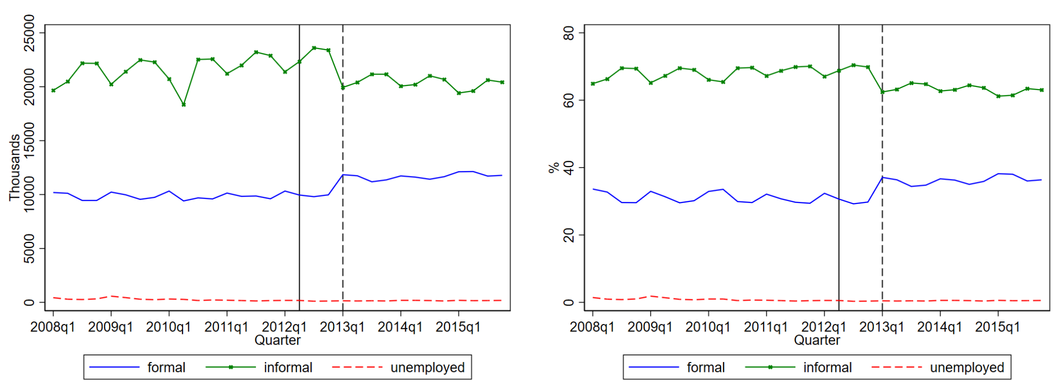
8.3 Informality and other outcomes

To complement the Social Security analysis of the formal sector, we use the Thai Labor Force Survey (LFS), a nationally representative repeated cross-section that covers both formal and informal workers and reports education, occupation, and—only for salaried workers—wages. We classify informal workers as unpaid family workers, domestic workers, and the self-employed (Samutpradit, 2024).[19]

Figure 14 plots the number and share of workers aged 25–50 in formal employment, informal employment, and unemployment between 2008 and 2015. Formal employment rises over the period (from 33 to 39 percent), with a noticeable increase after the minimum wage reform, while informal employment declines and non-employment remains roughly stable. The shift is most pronounced among younger and less-educated workers. Nevertheless, non-college workers remain concentrated in informal employment, whereas college graduates remain predominantly in formal salaried jobs. Among formal salaried workers, the share earning below the new minimum wage fell from 64% to 31% after the reform, consistent with our Social Security results.

Figure 14: Formal and Informal Employment in Thailand

(a) Employment levels (b) Employment shares



Notes: The left panel plots the number of workers aged 25–50 in formal employment, informal employment, and unemployment (in thousands). The right panel plots the corresponding shares. Source: Thai Labor Force Survey (LFS).

Although the share of formal employment rises nationally, regions may have adjusted differently

[19] The Thai LFS only began directly asking about Social Security status in 2016. Prior to this, formal/informal status is inferred using occupation, sector, and employer characteristics.

because the nationwide minimum wage was more binding in some provinces than others. To quantify differential changes in formality, we exploit cross-province variation in exposure. Following Dustmann et al. (2021), we define a province-level exposure measure:

$$GAP_{p,2011} = \frac{\Sigma_{i \in p} \max\left\{0, MW - w_{i,2011}\right\}}{\Sigma_{i \in p} w_{i,2011}} \tag{10}$$

where p indexes provinces, MW is the new minimum wage, and $w_{i,2011}$ denotes wages observed in the LFS in 2011 among formal salaried workers (the group for which wages are reported). A GAP of zero implies that no worker in province p earns below the new minimum, while higher values reflect both a larger share of sub-minimum-wage workers and larger shortfalls relative to the new threshold.

We compute $GAP_{p,2011}$ for 76 provinces using the 2011 LFS. The average GAP is 0.15, with Bangkok and other metropolitan areas exhibiting lower values and northeastern provinces exhibiting higher ones. We then estimate exposure-based regressions of the form:

$$y_{ipt} = \alpha_p + \theta_t + \delta\left(GAP_{p,2011} \times POST_t\right) + \beta_p t + X'_{ipt}\beta_x + \varepsilon_{ipt} \tag{11}$$

where y_{ipt} is an indicator for being formally employed, informally employed, or unemployed, α_p is province fixed effect, θ_t is year fixed effect, $POST_t$ is an indicator for the post-policy period (2013-2015), t is a linear time index (allowing for province-specific trends β_p), and X_{ipt} includes controls for age, education and experience. The coefficient δ captures differential changes after the reform in more exposed provinces relative to less exposed provinces.

Table 10: Effect of minimum wage exposure (GAP) on employment status by education group

	All	Primary	High school	College
	(1)	(2)	(3)	(4)
prob(being formal)	-0.229**	-0.488**	-0.1	-0.083
	(0.07)	(0.105)	(0.12)	(0.094)
prob(being informal)	0.152**	0.378**	-0.038	-0.016
	(0.074)	(0.111)	(0.045)	(0.055)
prob(non-employed)	0.158**	0.107	0.085	0.158
	(0.07)	(0.078)	(0.11)	(0.118)

Notes: Each row reports the coefficient on $GAP_{p,2011} \times POST_t$ from regressions of employment status on provincial exposure to the 2012–2013 minimum wage reform. All regressions control for age, education, and experience and include province and time fixed effects as well as province-specific linear time trends. Standard errors are clustered at the province level. ** indicates 5 percent significance and * indicates 10 percent significance.

Table 10 reports estimates for formal, informal, and unemployed status in the aggregate and by education group. Provinces that were more exposed to the reform exhibit a smaller post-reform increase in formal employment and a corresponding relative shift toward informal employment, with effects concentrated among workers with only primary schooling. At the aggregate level, moving from the 10th to the 90th percentile of exposure implies about a 3.7 percentage-point smaller post-policy increase in formal employment in more exposed provinces.[20] This pattern is consistent with the aggregate trends in Figure 14 which shows that formality rises nationwide after the reform, however the exposure design indicates that the increase was less pronounced in more exposed provinces. The unemployment estimate is positive in the pooled sample but is imprecisely estimated within education groups. Overall, the LFS evidence points to modest compositional adjustment at the margin of formality in more exposed provinces, concentrated among the least educated.

This exposure-gradient is consistent with the broader literature. Ulyssea et al. (2025) note that while reduced-form findings on informality are mixed, quantitative equilibrium models often imply that a binding wage floor can slow formal-sector expansion and shift some employment toward informality; see also Magruder (2013) for related evidence from Indonesia.

Finally, we examine other outcomes such as hours worked and fringe benefits and find no statistically significant effects. Taken together with the Social Security results, these patterns reinforce the main conclusion: the 2012–2013 reform substantially raised formal-sector earnings while generating, at most, modest compositional shifts at the margin of formality.

9 Conclusion

This paper evaluates Thailand's large 2012–2013 minimum wage reform using matched employer–employee data, complemented by evidence from the Labor Force Survey. The reform, which raised the national wage floor by about 40 percent, provides a unique opportunity to examine how a large, nationwide cost shock reshapes wages, mobility, and worker–firm allocation.

We estimate a wage–mobility discrete type model in the spirit of Bonhomme et al. (2019) and Lentz et al. (2023), which classifies workers and firms by their wage and mobility patterns. The model is estimated on pre-policy data, ensuring that type definitions reflect the structure of the labor market before the reform.

Three key findings emerge. First, the reform raised earnings substantially across the distribution, with the largest gains for low-wage workers but sizable spillovers to higher types. These effects are confirmed both in reduced-form comparisons and in spatial regressions exploiting provincial variation in exposure. On employment, we find little evidence of disemployment for the employed or recently non-employed. The main adjustment occurs at the margin of long-term non-employment, where job-finding slows; in simulations, these losses are more than offset by higher wages and improved stability over the life cycle.

[20] The GAP values at the 10th (less exposure) and 90th percentiles (more exposure) are .06 and .223. Comparing provinces at the 90th versus 10th percentile of $GAP_{p,2011}$ implies a post-policy difference of $\hat{\delta} \times \left(GAP^{90} - GAP^{10}\right)$ in the outcome. For formal employment (All), this equals $-0.229 \times (0.223 - 0.06) = -0.037$, i.e. a 3.7 percentage-point smaller post-policy increase in the probability of being formally employed in more exposed provinces.

Second, the reform changed the composition of wage growth. Entry wages increased substantially for exposed groups, while within-job wage growth over tenure flattened—most notably for the middle type with the steepest pre-policy tenure profile—consistent with firms partly accommodating higher starting pay by compressing subsequent wage progression. Lifetime earnings decompositions show that wage effects account for most of the gains, but mobility also matters for the more mobile types through lower separation risk and improved re-entry, even as job-to-job opportunities become less frequent and overall labor-market churning declines.

Third, the reform reshaped worker–firm sorting. Employment shifted up the firm ladder, with lower- and mid-type workers reallocating toward higher-paying firm types. Overall dependence between worker and firm types declines after the reform, consistent with weaker positive assortative matching. Importantly, preference-based measures indicate that upward moves on the wage ladder do not uniformly translate into moves toward more-preferred firms, highlighting that wage gains and match quality need not move together.

Finally, Labor Force Survey evidence points to modest compositional adjustments at the margin of formality: in more exposed provinces, formal employment growth is less pronounced and is partly offset by higher informality among less-educated workers, with limited changes in unemployment.

Overall, Thailand's reform substantially raised earnings—especially at the bottom—with limited adverse effects on aggregate employment, but it also altered wage progression, mobility, and the structure of matching. These results underscore that large minimum wage hikes operate not only through wage floors within jobs, but also through mobility and reallocation across firms. For policy design, this suggests that distributional gains can be large, while the longer-run implications for wage growth and match allocation are central for evaluating the full impact.

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APPENDIX

A Wage imputation

The Social Security taxable salary has been capped at 15,000 baht since the start of the system. In our sample of 503,694,802 employment observations, about 20 percent are right-censored at the cap, with censoring rising from roughly 10 percent in 2009 to 30 percent in 2015.

Although Social Security also collects a separate dataset of self-reported uncensored wages, reporting is voluntary, with about 15 percent of values missing in a potentially non-random way, and the data are available only for January of each year. For these reasons, we do not use the uncensored distribution in estimation but instead rely on imputation following Card et al. (2013). We fit a total of 840 Tobit models, separately by month-year, gender, and five-year age groups (25–29, 30–34, 35–39, 40–44, and 45–50). Each regression includes worker, firm, and job characteristics.

Worker characteristics include the age of first registration with Social Security (a proxy for education), mean log wages in the past 12 months, the fraction of months censored, and non-employment months in the past year. For workers observed only once, we include a dummy and impute past wages and censoring fractions using age–gender cell means. Firm characteristics include firm size (and its square), mean log co-worker wages, gender composition, fraction of censored co-worker wages, and median wages. For single-worker firms, we use age–gender cohort means instead of co-worker averages.

Imputed wages are constructed from the Tobit-predicted mean $X'\beta$ and variance σ^2. Let $y \sim N\left(X'\beta, \sigma^2\right)$ denote the uncensored log wage. For censored cases $y \geq c$, the uncensored draw is

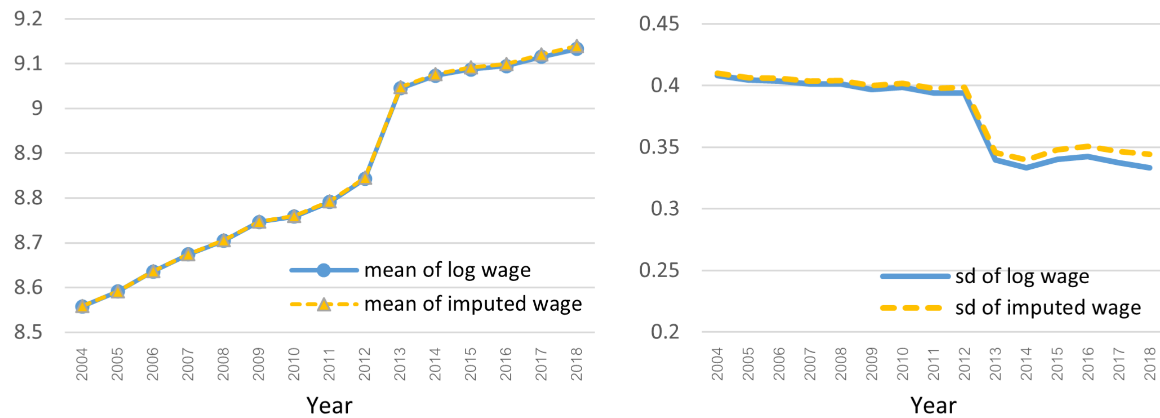
$$y^u = X'\beta + \sigma\Phi^{-1}\left[k + \mu\left(1-k\right)\right],$$

where $k = \mu\Phi\left[\frac{c-X'\beta}{\sigma}\right]$ is a standard normal CDF and $\mu \sim U\left[0,1\right]$ is a draw from a standard uniform distribution, and σ is estimated from the Tobit model.

A.1 Imputation validation

We validate the imputation by artificially censoring known wages. Using men aged 25–29, whose actual censoring rates range from 0.8 to 5 percent, we impose an artificial cap at 14,000 baht (below the true 15,000 cap). We then impute wages using the model and compare them to actual uncensored values. Figure A.1 shows that the means and standard deviations of imputed wages closely match the true distribution, confirming the reliability of the approach.

Figure A.1: Imputation validation exercise



Notes: The left panel compares the mean of log wages in the data with the imputed values; the right panel compares the corresponding standard deviations.

A.2 Evidence of spillovers in the upper tail

One concern is whether monthly earnings growth observed at the top of the distribution could be a mechanical artifact of the imputation procedure. While the Social Security self-reported monthly earnings distributions contain potentially non-random missing values and bunching at round numbers, we can still use them to provide supporting evidence.

Table A.1 compares percent monthly earnings growth in the uncensored versus imputed distributions at each percentile in 2011. For the uncensored cross-sections, we track average monthly earnings growth from January 2012 to January 2014. For the imputed distribution, we compute the same growth by percentile. While the imputed distribution has a slightly longer right tail, the extent of spillovers in the upper tail is similar in both datasets.

This exercise suggests that the positive spillovers observed for higher-type workers—such as the 10 percent earnings gains for type 6—are not mechanically generated by the imputation procedure, but reflect genuine growth in the upper tail.

B Determining the number of worker and firm types

To determine the number of worker (K) and firm types (L), we follow Lentz et al. (2023) and apply K-means clustering combined with the Calinski–Harabasz (CH) index, defined as the ratio of between-cluster to within-cluster sum of squares. Intuitively, higher CH values indicate more distinct and compact clusters. Our goal is to select K and L that balance explanatory power with interpretability, using variables aligned with the likelihood in Eq(4).

Table A.1: Percent change in monthly wages in the uncensored and imputed distributions

	SSO uncensored wage		Imputed wage	
2011 level	2011 percentile	avg. growth	2011 percentile	avg. growth
(1)	(2)	(3)	(4)	(5)
16,000	87.24	7.93%	84.40	10.56%
18,000	89.13	7.45%	86.07	9.08%
20,000	90.78	7.53%	87.96	8.00%
22,000	91.90	6.82%	89.93	7.36%
24,000	92.77	7.54%	91.85	7.05%
25,000	93.22	4.06%	92.76	7.00%
26,000	93.67	3.21%	93.62	7.00%
28000	94.30	0.75%	95.16	7.13%
30000	96.62	7.13%	96.39	7.41%

Notes: Column (1) reports monthly wage (salary) levels in 2011. Columns (2) and (4) report the corresponding percentiles in the uncensored and imputed distributions, respectively (percentiles are defined with respect to the 2011 distribution). Columns (3) and (5) report average percentage monthly wage growth between January 2012 (pre-policy) and January 2014 (post-policy) in the uncensored and imputed distributions, respectively. The uncensored data are available only for January 2011–2014.

B.1 Worker types

For workers, we include each individual's mean and standard deviation of wages during the pre-policy period (January 2008–March 2012). To capture additional wage variation and mobility, we also use wage distribution percentiles (p20, p50, p80), the number of jobs held, and the duration of non-employment spells. All variables are normalized to [0,1] to ensure comparability across features.

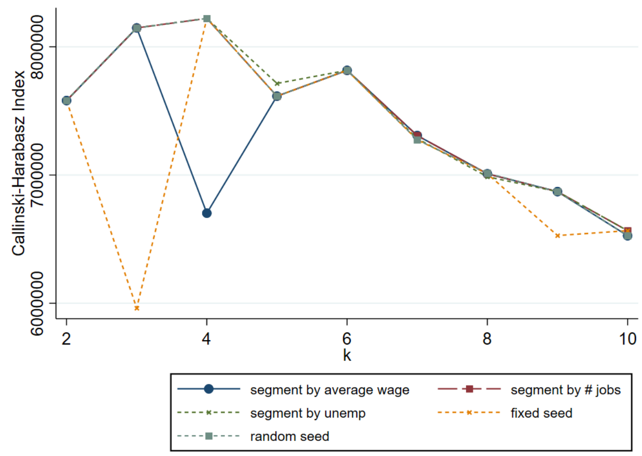
To evaluate clustering quality, we compute the CH index for varying numbers of clusters under different centroid initializations: sorted by average wage, number of jobs, non-employment duration, and random assignment. Figure A.2 plots the resulting CH indices. For $K = 2-4$, the CH index is highly sensitive to initialization, with some runs producing very high values and others very low ones, indicating a lack of robustness. Starting at $K = 5$, the indices stabilize across different initializations, and at $K = 6$ the CH index remains relatively high and robust across segmentations. Beyond $K = 6$, the CH index declines monotonically as additional clusters add little structure while reducing interpretability.

Based on this combination of fit, stability, and interpretability, we select $K = 6$ as the preferred number of worker types.

B.2 Firm types

For firm types, we include the within-firm mean and standard deviation of wages, wage distribution percentiles (p20, p50, p80), firm size, and the percentage of inflow and outflow of workers between January 2008–March 2012 (pre-policy period). Because wage distribution measures for small firms can be redundant, we estimate two versions of the model: Model 1 (with wage distribution percentiles) and

Figure A.2: CH index of worker k-means clustering by number of clusters and initialization

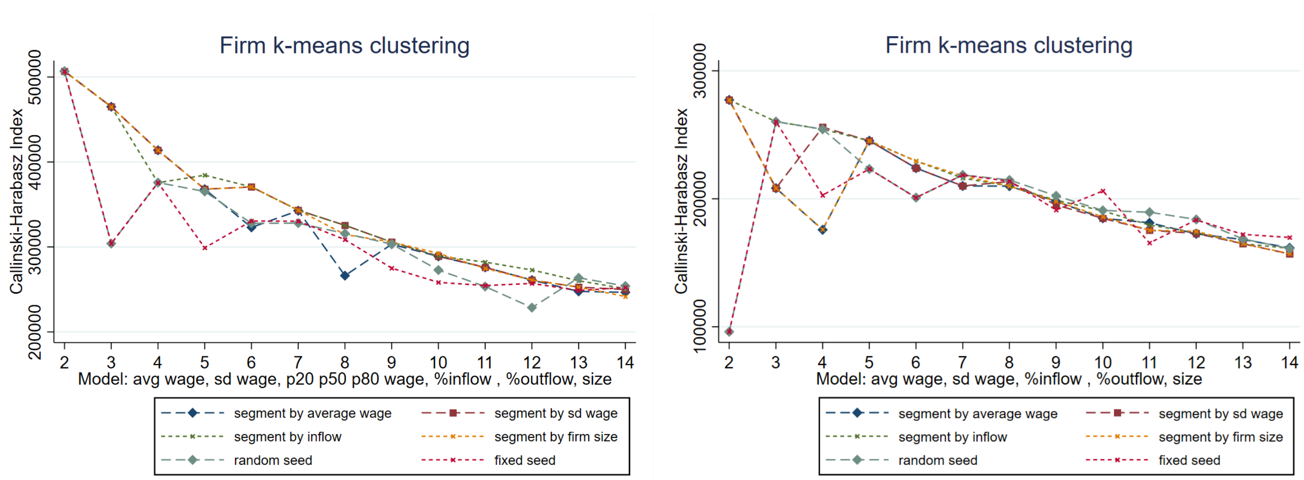


Notes: The figure plots the Calinski–Harabasz (CH) index for different numbers of worker clusters (K) using alternative initializations.

Model 2 (excluding them). All variables are normalized to [0,1] to ensure comparability across features.

Figure A.3 plots the Calinski–Harabasz (CH) index for different numbers of clusters and initialization

Figure A.3: CH index of firm k-means clustering by number of clusters and initialization



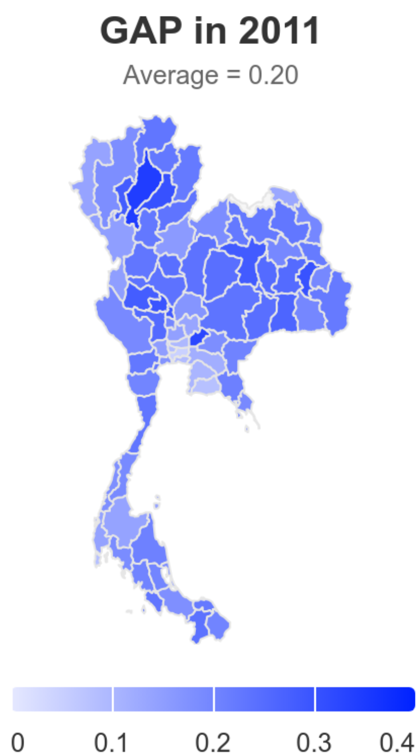
Notes: The figure plots the Calinski–Harabasz (CH) index for two models of firm features: Model 1 (left panel) includes wage distribution percentiles, Model 2 excludes them.

methods. In both models, the CH index declines steadily as the number of clusters increases, and no sharp "elbow" emerges. This suggests that additional clusters provide diminishing improvements in fit. We therefore select $L = 10$ firm types, which balances capturing heterogeneity in firms' wage–mobility profiles with maintaining interpretability. Results are robust to nearby choices of L.

C Spatial variation in exposure: wage, earnings and employment

To supplement the evidence on earnings and non-employment in Section 6, we exploit cross-province variation in how binding the new wage floor was prior to the reform. Following Dustmann et al. (2021), we measure provincial exposure $GAP_{p,2011}$ defined in Eq(11) in Section 8.3. In words, $GAP_{p,2011}$ summarizes the proportional shortfall of pre-policy wages below the new minimum within province p. In this appendix we apply the same measure using the Social Security wage distribution in 2011. The average GAP from Social Security data is 0.20, with Bangkok and other metropolitan areas exhibiting lower values and northeastern provinces exhibiting higher ones. Figure A.4 shows the geographic variation in $GAP_{p,2011}$.

Figure A.4: Provincial exposure to the minimum wage reform (GAP measure)



Notes: The figure maps provincial values of GAP, defined as the average shortfall between actual wages and the 2012 minimum wage threshold, based on 2011 wage distributions. Higher values indicate greater provincial exposure to the reform.

We estimate province-level exposure regressions of the form

$$Y_{pt} = \alpha_p + \theta_t + \delta_Y(GAP_{p,2011}POST_t) + \beta_p t + \varepsilon_{pt}$$

where Y_{pt} is the outcome of interest in province p year t, α_p is province fixed effect, θ_t is time fixed effect,

$POST_t$ indicates post-policy years, and $\beta_p t$ allows for province-specific linear trends (included in some specifications). The coefficient δ_Y captures differential post-reform changes in more exposed provinces relative to less exposed provinces in the outcome Y.

Table A.2 reports estimates for employment, earnings, and wages in logs with and without province-specific trends. The estimated effects on wages and earnings are positive and precisely estimated: a one–percentage point increase in GAP is associated with about a 0.4–0.5 percent larger increase in average wages and earnings in the post-policy period.

Table A.2: Impact of minimum wage exposure (GAP) on employment, earnings, and wages

Specification:	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variables:	Employment	Employment	Earnings	Earnings	Wage	Wage
GAPxPost	-0.038	-0.345***	0.491***	0.458***	0.491***	0.444***
	(0.07)	(0.06)	(0.03)	(0.03)	(0.03)	(0.03)
Year fixed effects	yes	yes	yes	yes	yes	yes
Province fixed effects	yes	yes	yes	yes	yes	yes
Province linear trend	no	yes	no	yes	no	yes
R^2	0.999	0.565	0.996	0.962	0.996	0.97
N	539	539	539	539	539	539

Notes: Standard errors in parentheses, clustered at the province level. * and ** denote statistical significance at the 5% and 1% levels, respectively. Outcomes are in logs. GAP is calculated using 2011 data and captures pre-policy provincial exposure to the minimum wage.

Employment estimates are sensitive to the inclusion of province-specific trends. Without trends, the estimate is statistically indistinguishable from zero; with trends, the estimate is negative. Interpreting magnitudes, comparing provinces at the 90th versus 10th percentile of $GAP_{p,2011}$ implies a differential post-policy change of $\hat{\delta}_{post}\left(GAP^{90} - GAP^{10}\right) = -0.058$ log points.[21] It is worth noting that this is a relative comparison: employment can rise overall while growing more slowly (or declining) in more exposed provinces than in less exposed provinces. These results suggest that employment effects are smaller and less precisely estimated than wage and earnings effects.[22]

Overall, provinces more exposed to the reform experienced larger wage and earnings gains, while employment estimates are smaller and more sensitive to province-specific trend controls. Following Haanwinckel (2023), we view regional exposure designs as potentially sensitive to measurement error and functional-form assumptions, and therefore interpret these results as complementary descriptive evidence. A related concern is that exposure may be endogenous to internal migration or other location-specific adjustments. Addressing such dynamics would require modeling location choice or an alternative design, which we leave for future work.

[21] Using $GAP^{90} = 0.284$ and $GAP^{10} = 0.116$, we have $\left(GAP^{90} - GAP^{10}\right) = 0.168$. With province-specific trends, the point estimate implies $-0.345 \times 0.168 = -0.058$.

[22] With province-specific linear trends, and with employment and wages in logs, the exposure design implies an employment elasticity with respect to own wage equal to, $\hat{\delta}_{Emp}/\hat{\delta}_{Wage} = -0.345/0.444 = -0.78$ where $\hat{\delta}_{Emp}$ and $\hat{\delta}_{Wage}$ denote the coefficients $\hat{\delta}_Y$ form the employment and wage regressions. The standard error (0.15) is computed using the delta method.

D Wage inequality and variance decomposition

This appendix reports the variance decomposition underlying the discussion in Section 7.1. We decompose the unconditional log-wage variance into within- and between-group components as follows:

$$\begin{aligned} \mathrm{V}(w_{it}) &= \mathrm{E}[V(w_{it}|k_i,\ell_{it},x_{it})] + \mathrm{V}[\mathrm{E}(w_{it}|k_i,\ell_{it},x_{it})] \\ &= \mathrm{E}[\sigma^2(k_i,\ell_{it},x_{it})] + \mathrm{V}[\mu(k_i,\ell_{it},x_{it})], \end{aligned}$$

where the first term captures within-type wage dispersion and the second reflects variation in conditional means $\mu(k_i,\ell_{it},x_{it})$. We further decompose the latter into worker, firm, match, and covariate effects:

$$\begin{aligned} \mathrm{V}[\mu(k_i,\ell_{it},x_{it})] = {} & \mathrm{V}[\overline{\mu}(x_{it})] + \mathrm{V}[a(k_i)] + \mathrm{V}[b(\ell_{it})] \\ & + \mathrm{V}[\widetilde{\mu}(k_i,\ell_{it},x_{it})] + 2\,\mathrm{Cov}[\overline{\mu}(x_{it}),a(k_i)] \\ & + 2\,\mathrm{Cov}[\overline{\mu}(x_{it}),b(\ell_{it})] + 2\,\mathrm{Cov}[a(k_i),b(\ell_{it})], \end{aligned}$$

where $a(k)$ and $b(\ell)$ are worker- and firm-type effects, $\tilde{\mu}(k,\ell,x)$ is the residual (match) component, and $\bar{\mu}(x)$ captures covariates.

Table A.3 reports the decomposition for the pre- and post-policy periods. Overall log-wage variance

Table A.3: Unconditional variance decomposition

		Before MW	After MW
Variance w		0.40	0.33
Percent contribution:			
Within-var	$E\sigma^2$	13.44	28.63
Between-var	Var(μ)	86.56	71.37
Person effect	Va	27.92	22.64
Firm effect	Vb	28.37	26.48
Cross effect	2Cov(a,b)	17.56	14.20
Match effect	V$\tilde{\mu}$	8.76	5.46
	V$\overline{\mu}$	2.11	1.52
Observed het.	2Cov$(a,\overline{\mu})$	1.83	1.05
	2Cov$(b,\overline{\mu})$	0.01	0.02
Sorting	Corr(a,b)	0.31	0.29

Notes: The table reports variance decompositions of log wages for the pre- and post-policy periods based on the equations in Appendix D. The overall variance is split into within-type dispersion and between-type components, the latter further decomposed into worker, firm, and match effects. See Section 7.1 for interpretation of these results.

fell from 0.40 to 0.33, indicating that the minimum wage reduced wage inequality. The share of variance explained by conditional means $\mu_{k\ell}(x)$ declined from 86.6 percent before the reform to 71.4 percent after, a reduction partly driven by our use of pre-policy data to estimate types. When types are re-estimated using the full sample rather than pre-policy data alone, the share rises to 80.1 percent, closer to pre-policy levels. Interestingly, the relative contributions of worker, firm, and match effects remain stable across these two specifications. Non-linear match effects accounted for about 8.8 percent of between-group variance before the policy and 5.5 percent after; worker effects declined from 28.0 to 22.6 percent, and firm effects from 28.0 to 26.5 percent. The covariance between worker and firm effects—a standard measure of sorting—fell from 17.6 to 14.2 percent, with the worker–firm correlation decreasing from 0.31 to 0.29. This suggests that sorting became modestly less positive after the reform.

E Simulation

E.1 Implementation

This appendix provides details of the simulation procedure used to compute lifetime earnings under the minimum wage policy and to decompose the contributions of different channels.

We simulate monthly career paths using the estimated model. The covariate x corresponds to an age bin and tenure bin. Age bins are 25–30, 30–35, 35–40, and 40–50. Tenure bins distinguish short- versus long-tenure spells. To construct long-run earnings over a 20-year horizon, we implement the simulation using five-year blocks within each age bin (holding the age bin fixed within a block), and then chain the blocks across age bins. Because parameters are constant within each age bin, this approach provides a transparent mapping from the estimated age-bin model to simulated outcomes while allowing mobility responses to accumulate within each block.

- For each worker type k and each age $a \in \{25\text{–}30, 30\text{–}35, 35\text{–}40, 40\text{–}50\}$, we simulate a five-year monthly panel with the age bin held fixed at a.
- We do this separately for short- and long-tenure initial conditions (as defined by x). When aggregating within an age bin, we weight short- and long-tenure simulations using empirical tenure shares for that age bin, since each simulation uses the same number of workers and firms. Each block simulates 4,800,000 workers and 200,000 firms, with the workers-per-firm distribution matched to the data.
- We construct a 20-year discounted lifetime earnings measure by chaining the four five-year blocks sequentially across age bins (25–30, 30–35, 35–40, 40–50) and discounting at five percent per year.

Initial matching: Worker and firm types are assigned in proportion to $p(k)$ and $q(\ell)$. Initial matches are drawn according to $m(\ell|k,x)$.

Transitions: Transitions are governed by $\delta_{k\ell}(x)$ (employment–to–non-employment), $\psi_{k\ell}(x)$ (non-employment–to–employment), and $M(\ell'|k.\ell,x)$ (job-to-job).

Wages and employment status: At each month, workers are either employed at a firm of type ℓ or in non-employment. Earnings are set to zero in non-employment. Conditional on (k,ℓ,x) monthly wages are drawn from the estimated wage distribution with mean $\mu_{k\ell}(x)$ and dispersion $\sigma_{k\ell}(x)$.

Counterfactual setup: We run counterfactual simulations to isolate the contribution of each channel:

- Overall effect (full): all parameters set to post-policy values.
- Wage effect: wage parameters $\mu_{k\ell}(x), \sigma_{k\ell}(x)$ set to post-policy values, mobility parameters fixed at pre-policy.
- Mobility effect: mobility parameters set to post-policy values, wage parameters fixed at pre-policy.
- Initial wage effect: short-tenure wages are shifted to post-policy values, while the profile of wage growth from short to long tenure remains fixed at pre-policy levels, and all mobility parameters remain pre-policy.
- Later wage effect: computed as the difference between the overall wage effect and the initial wage effect.
- EU effect: only separation parameters $\delta_{k\ell}(x)$ set to post-policy values (holding other mobility and wage parameters at pre-policy values).
- UE effect: only job-finding parameters $\psi_{k\ell}(x)$ set to post-policy values.
- EE effect: only EE transition components set to post-policy values. Within EE:
  - Chance: on-the-job arrival component $\lambda_{k\ell}(x)$ set to post-policy values holding $\gamma_{k\ell}(x)$ fixed.
  - Choice: preferences $\gamma_{k\ell}(x)$ set to post-policy values holding $\lambda_{k\ell}(x)$ fixed.

E.2 Simulation results by age group

This section reports simulation results by age group. Table A.4 replicates the decomposition in Table 8 separately for each five-year age-bin block: ages 25–30, 30–35, 35–40, and 40–50. Each table reports percentage changes in discounted earnings over the five-year horizon, using the same counterfactual definitions and column structure as in the main decomposition.

Table A.4: Long-term earnings decomposition by age group

(a) 25-30 years old

Worker type (1)	Full effect (2)	Wage Overall (3)	Initial (4)	Later (5)	Mobility Overall (6)	EU (7)	UE (8)	EE: λ (9)	EE: γ (10)	Wage-Mob. Interaction (11)
1	63.33	48.95	44.75	4.20	9.74	4.37	3.85	4.25	-2.74	4.64
2	33.82	36.82	21.96	14.86	-3.05	-2.34	3.94	-0.43	-3.67	0.05
3	71.64	48.61	50.06	-1.45	11.29	8.78	6.94	1.31	-4.92	11.74
4	45.20	38.03	27.87	10.15	4.90	1.40	2.02	1.88	-0.52	2.27
5	48.83	40.12	26.18	13.95	6.06	-0.90	3.86	2.89	-0.09	2.64
6	32.09	31.34	30.55	0.79	0.72	0.19	-0.79	1.13	0.10	0.03

(b) 30-35 years old

Worker type (1)	Full effect (2)	Wage Overall (3)	Initial (4)	Later (5)	Mobility Overall (6)	EU (7)	UE (8)	EE: λ (9)	EE: γ (10)	Wage-Mob. Interaction (11)
1	75.29	54.15	53.44	0.71	16.00	5.20	4.84	7.38	-1.44	5.14
2	40.48	32.05	17.05	15.00	7.11	1.35	7.32	3.86	-5.13	1.32
3	44.44	31.48	48.04	-16.57	10.36	4.57	7.08	4.44	-5.45	2.60
4	44.85	35.85	23.87	11.98	7.04	0.80	4.35	3.09	-1.20	1.96
5	38.03	28.78	18.30	10.48	7.89	0.02	5.33	3.24	-1.03	1.36
6	25.93	27.08	16.62	10.46	-0.67	0.05	-0.93	0.38	0.03	-0.47

(c) 35-40 years old

Worker type (1)	Full effect (2)	Wage Overall (3)	Initial (4)	Later (5)	Mobility Overall (6)	EU (7)	UE (8)	EE: λ (9)	EE: γ (10)	Wage-Mob. Interaction (11)
1	77.06	57.30	54.30	3.00	11.29	3.42	3.93	3.12	0.08	8.47
2	73.70	57.08	39.42	17.66	6.76	1.43	4.76	3.08	-2.09	9.86
3	12.22	5.84	48.78	-42.95	4.84	2.38	8.75	3.02	-9.47	1.55
4	34.74	28.68	17.83	10.85	5.57	0.43	5.26	1.68	-1.57	0.48
5	40.63	26.17	14.16	12.01	12.45	0.49	8.19	4.97	-1.72	2.01
6	17.87	19.59	11.70	7.89	-0.97	-0.03	-0.70	-0.60	0.38	-0.75

(d) 40-50 years old

Worker type (1)	Full effect (2)	Wage Overall (3)	Initial (4)	Later (5)	Mobility Overall (6)	EU (7)	UE (8)	EE: λ (9)	EE: γ (10)	Wage-Mob. Interaction (11)
1	63.79	52.41	52.35	0.06	8.16	3.24	3.84	1.68	0.15	3.22
2	40.43	33.98	29.03	4.96	4.30	0.29	5.36	1.18	-1.64	2.14
3	16.05	9.85	64.36	-54.50	5.16	2.72	8.48	1.94	-6.55	1.03
4	28.81	22.91	13.50	9.41	6.19	0.35	5.91	1.10	-0.95	-0.30
5	25.56	17.04	4.66	12.37	7.79	-0.08	6.63	2.28	-1.54	0.73
6	22.09	11.11	4.95	6.16	10.91	-0.97	3.10	7.17	1.65	0.08

Notes: Column (2) reports the percent change in discounted lifetime earnings between the post- and pre-policy environments. Columns (3)–(5) decompose the wage channel into the overall wage effect (3), the initial-wage component (4; short-tenure wages), and the later-wage component (5; the implied change in wage growth from short to long tenure). Column (6) reports the mobility effect, with subcomponents from separations (EU, col. 7), job-finding (UE, col. 8), and employer-to-employer transitions split into chance and choice (cols. 9–10). Because mobility subcomponents are computed via separate counterfactuals, their sum need not equal column (6) due to small interaction terms within mobility. Column (11) reports the interaction between wage and mobility channels so that (2) = (3) + (6) + (11).

F Additional tables and figures

Table A.5: Changes in wages by firm type, age and tenure

	Pre-MW					Post-MW				
ℓ	ave. wage	tenure L-S	age 2-1	age 3-2	age 4-3	ave. wage	tenure L-S	age 2-1	age 3-2	age 4-3
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
1	7.902	0.044	0.013	0.004	0.006	8.248	-0.031	0.026	-0.004	-0.004
2	8.355	0.022	0.017	0.010	0.012	8.678	-0.028	0.025	0.001	0.000
3	8.416	0.142	0.044	0.021	0.032	8.844	0.120	0.046	0.020	0.013
4	8.530	0.260	0.074	0.030	0.026	8.954	0.232	0.056	0.038	0.013
5	8.792	0.119	0.042	0.019	0.039	9.102	0.059	0.031	0.004	0.002
6	8.888	0.214	0.062	0.036	0.049	9.193	0.169	0.043	0.028	0.020
7	8.869	0.310	0.104	0.075	0.087	9.225	0.243	0.065	0.046	0.047
8	9.246	0.293	0.169	0.141	0.104	9.524	0.283	0.105	0.087	0.077
9	9.296	0.247	0.143	0.096	0.075	9.546	0.209	0.101	0.055	0.036
10	9.904	0.201	0.181	0.094	0.001	10.075	0.231	0.126	0.040	-0.026

Notes: The table reports average log wages and wage returns by firm type. Column (1) lists firm type, column (2) average log wages, column (3) returns to tenure defined as the wage increment from short to long tenure within a match, and columns (4)–(6) returns to age as workers move across successive groups (25–30, 30–35, 35–40, and 40–50 years old). Results are shown separately for the pre- and post-policy periods.

Table A.6: Changes in average model parameters by firm type

	Pre-MW			Post-MW		
ℓ	EU	UE	EE	EU	UE	EE
(1)	(2)	(3)	(4)	(5)	(6)	(7)
1	0.024	0.001	0.001	0.018	0.000	0.001
2	0.022	0.002	0.001	0.016	0.001	0.001
3	0.026	0.003	0.003	0.020	0.001	0.002
4	0.026	0.008	0.015	0.019	0.004	0.007
5	0.019	0.005	0.004	0.015	0.002	0.004
6	0.017	0.004	0.009	0.014	0.003	0.008
7	0.016	0.005	0.022	0.011	0.003	0.017
8	0.012	0.006	0.019	0.009	0.004	0.015
9	0.016	0.004	0.012	0.013	0.003	0.011
10	0.008	0.005	0.013	0.007	0.003	0.012

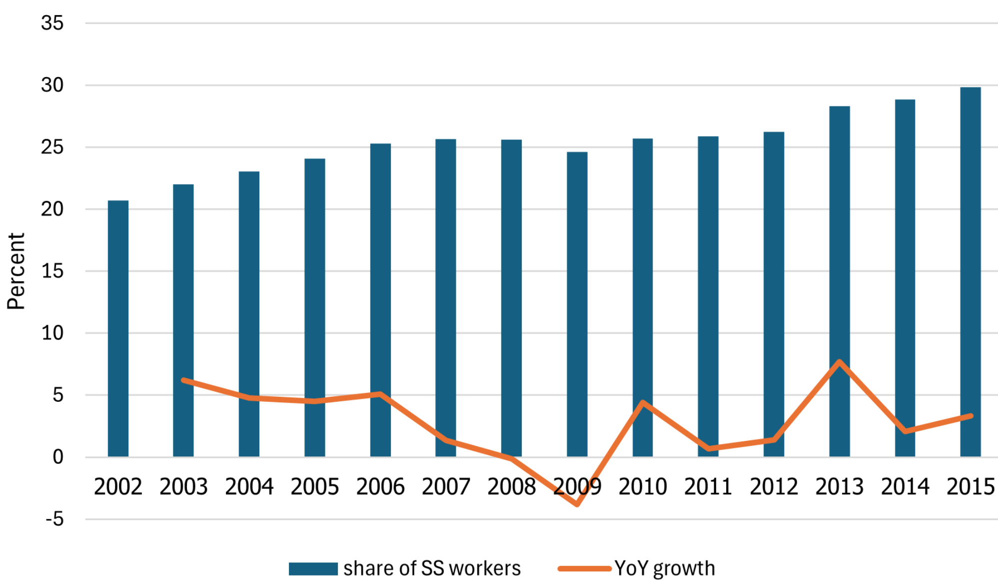
Notes: the table shows changes in average model parameters of each firm type between pre-policy and post-policy periods, weighted by their respective matching probabilities $p(k,\ell,x)$. See text for explanation.

Table A.7: Correlations between pre- and post-policy job preferences $\gamma_{k\ell}$

Worker type	Correlation
1	0.51
2	0.69
3	0.35
4	0.60
5	0.71
6	0.88

Notes: Correlations are computed between $\gamma_{k\ell}(x)$ pre- and post-minimum wage reform for each state x. Uniform weights are applied when averaging correlations across states x for each worker type.

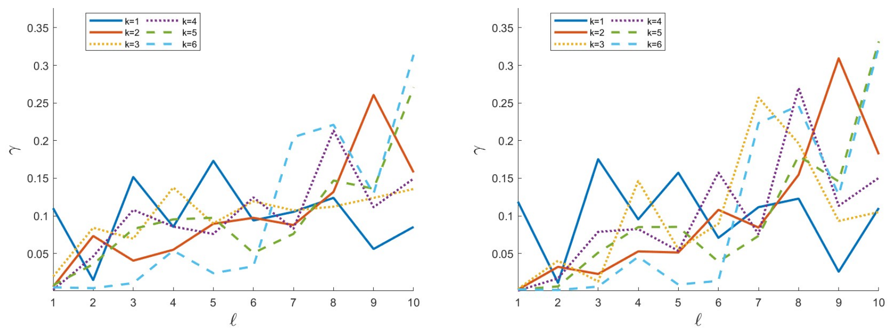
Figure A.5: Share of workers contributing to Social Security



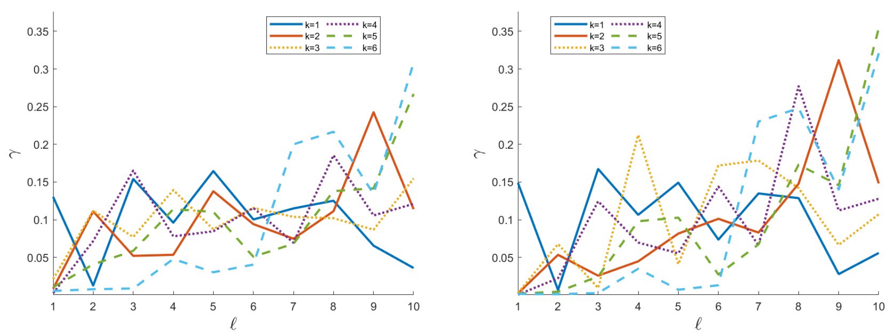
Notes: The figure plots the share of the labor force (excluding government and state enterprise workers) contributing to Social Security. Coverage expanded in 2002 to all establishment sizes, and the nation-wide minimum wage reform was introduced in 2012–2013.

Figure A.6: Worker match preferences $\gamma_{k\ell}(x)$ (pre-policy period)

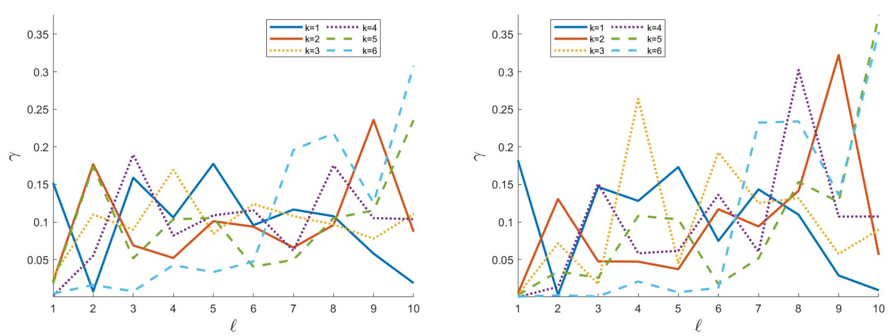
(a) 25- 30 yrs old: short tenure (left) long tenure (right)



(b) 30-35 yrs old: short tenure (left) long tenure (right)



(c) 35-40 yrs old: short tenure (left) long tenure (right)



(d) >40 yrs old: short tenure (left) long tenure (right)