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Firms and the Wage Penalty of Temporary Work

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Abstract

Using matched employer–employee data from the Netherlands, we decompose the wage penalty of temporary work. Temporary workers earn 28 log points less per hour than permanent workers. Worker fixed effects explain half this gap, while firm pay premia account for 28%. Most of the firm contribution reflects temporary workers sorting into low-paying firms rather than within-firm pay differences. As temporary employment differs substantially across demographic groups, we extend our framework to gender and native–migrant wage gaps, separating contract from firm sorting. Differential exposure to temporary work explains part of the native–migrant firm pay-premia gap but matters little for the gender wage gap.

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1 Introduction

European labor markets are widely characterized by a dual structure in which permanent contracts with high dismissal costs coexist alongside temporary contracts without employment protection. Workers on temporary contracts (henceforth, temporary workers) typically earn substantially less than those on permanent contracts (henceforth, permanent workers) (e.g., Drenik et al., 2023; Bergeaud et al., 2024). A natural explanation is that temporary and permanent workers differ in their productivity. Yet a growing literature, building on Abowd et al. (1999), shows that firms' hiring and pay policies can significantly amplify wage differences across workers, which raises the question: how much of the temporary work wage penalty is attributable to workers, and how much is driven by firms?

Employers may contribute to this penalty through two channels. The first is an across-firm channel, *sorting*, which operates if permanent workers are disproportionately employed at high-paying firms. The second is a within-firm channel, *pay-setting*, which operates if temporary workers receive lower pay than permanent workers at the same firm. Recent evidence suggests firms share fewer rents with fixed-term and temporary agency staff (Daruich et al., 2023; Drenik et al., 2023). Less is known about the relative importance of pay-setting versus sorting in the temporary work pay gap, despite its importance for policy: within-firm pay differences can be targeted through equal-pay mandates, while pay differences due to sorting across firms would require intervention in employer hiring practices.

Using Dutch matched employer–employee data from 2013 to 2019, we quantify the role of workers and firms in shaping the wage gap between temporary and permanent workers directly hired by firms. To do so, we extend the Abowd et al. (1999) framework (henceforth, AKM), allowing firm pay premia to vary by employment contract. We model hourly wages as a function of a worker effect, time-varying observables, a firm-contract effect, and a residual. We then use the estimated worker and firm-contract effects to quantify the workers' and firms' contribution to the temporary work wage gap. We further decompose the firm-premia gap into *sorting*, capturing differences in the firms where temporary and permanent workers are employed, and *pay-setting*, capturing differences in their pay premia within firms.

On average, temporary workers earn less than permanent workers, with an unconditional wage gap of 28 log points. Around half of this gap reflects differences in worker effects, which we interpret as time-invariant productivity differences across contract types. But firms also matter: on average, temporary workers earn firm premia that are 7.9 log points lower than those earned by permanent workers, accounting for approximately 28 percent of the overall wage penalty. Decomposing the firm-premia gap, we find that sorting explains about 71 percent and pay-setting the remaining 29 percent, roughly 20 and 8 percent of the overall gap, respectively. Thus, firms contribute to the temporary-work wage penalty primarily because temporary workers sort into lower-paying firms, rather than being paid less within the same firm. This result is not driven by sorting into low-paying sectors: even within the same sector, temporary workers are less likely to be employed at high-paying firms.

One potential explanation for the sorting effect is that it reflects differences in worker productivity across contract types: high-paying firms may disproportionately hire high-productivity workers, who are themselves more likely to hold permanent contracts. To test this, we follow Gerard et al. (2021) and characterize each firm's skill distribution using estimated worker effects as a proxy for skill. Within each skill group, we then assign the contract employment shares observed in the firm's local labour market, thereby mimicking 'contract-blind' hiring, and repeat the decomposition. We find that assortative matching explains about one-third of the sorting effect, with the remainder reflecting differences in firms' hiring across contract types beyond workforce composition.

Finally, we use our framework to show that employment contracts constitute a distinct margin of worker–firm allocation that can matter for conventional decompositions of wage gaps. Women and migrants are considerably more likely than men and natives to hold temporary contracts, we therefore decompose the firm pay premium component of the gender and migrant–native wage gaps into differential exposure to temporary work and differential sorting across firms conditional on contract type. Standard group-specific AKM applications typically cannot separately identify these margins (e.g., Card et al., 2016; Gerard et al., 2021; Dostie et al., 2023), as they typically abstract from contract type and therefore absorb contract-driven wage differences into conventional firm sorting and pay-setting effects. For instance, when temporary jobs are concentrated at lower-paying employers, a group's greater exposure to temporary work potentially appears as standard firm sorting.

We find that the gender wage gap is driven almost entirely by firm sorting, consistent with evidence that women sort into lower-paying firms than men independent of contract type (Card et al., 2016). For the migrant-native gap, by contrast, a substantially larger share of the firm-premium gap is attributable to differential exposure to temporary work. Thus, while abstracting from contract type has little bearing on the gender pay gap, ignoring it may conflate firm sorting and pay-setting estimates for migrant-native wage inequality.

Our findings carry direct policy implications. Because the firm contribution to the temporary work penalty is driven primarily by sorting rather than within-firm pay-setting, equal-pay mandates such as the EU Fixed-term Work Directive can address only a minor share of the penalty. Moreover, dual labor market reforms that facilitate transitions into permanent employment offer a viable margin for reducing native-migrant wage disparities, but will have negligible effect on the gender gap, which is driven by female underrepresentation at high-paying firms.

Related literature This paper makes two contributions. First, we add to the literature on wage penalties from non-standard employment arrangements (e.g., Blanchard and

Landier, 2002; Booth et al., 2002; Goldschmidt and Schmieder, 2017; Biesenbeek and Volkerink, 2023; Bergeaud et al., 2024; Carreño Bustos et al., 2024). Building on Daruich et al. (2023) and Drenik et al. (2023), who show that firms share lower rents with fixed-term workers and those hired through temporary work agencies, we decompose the firm contribution to the temporary work wage penalty into sorting across employers and differential pay-setting within employers. We show that, while within-firm gaps are present, sorting accounts for the larger share of the temporary work wage penalty.

Second, we build upon group-specific AKM models (e.g., Abowd et al., 1999; Card et al., 2016; Bruns, 2019; Gerard et al., 2021; Dostie et al., 2023; Arellano-Bover and San, 2026; Guo et al., 2025; Bødker et al., 2024) by allowing firm fixed effects to vary by employment contract type. Unlike time-invariant demographic traits like gender or race, contract type is job-specific and changes over time as workers transition across contracts, both within and across firms. This distinction allows us to leverage rich within-worker contract transitions to cleanly identify contract-specific pay-setting separately from employer sorting. Standard group-specific AKM models that abstract from contract type potentially absorb temporary work penalties into traditional sorting and pay-setting channels. We show that this distinction matters little for the gender firm-premia gap, which is driven primarily by firm sorting, but matters substantially for the migrant–native gap, where migrants' greater exposure to temporary employment accounts for a sizable share of the firm-premia gap.

2 Econometric Framework

2.1 A Job Ladder Model of Wages

Building upon the AKM tradition, we specify a wage determination model that allows firm effects to differ between workers hired directly by the firm under permanent and temporary contracts.[1] Formally, we estimate:

$$\ln w_{it} = \alpha_i + \psi_{J_{it}}^{c_{it}} + X_{it}\gamma + \epsilon_{it}, \tag{1}$$

where $\ln w_{it}$ is the log hourly wage of worker i at time t, α_i represents a worker fixed effect, c_{it} indexes the worker's contract type—temporary or permanent—and J_{it} denotes the firm J where individual i is employed at time t. The firm-contract term ψ_J^c captures the wage premium that firm J pays to workers with contract c. We assume these are constant over 2013–2019, consistent with evidence that firm effects are highly persistent (Lachowska et al., 2023). X_{it} are time-varying controls, including age, job characteristics (full-time-equivalent and hours flexibility), tenure, and time effects. The error term ϵ_{it} captures all remaining wage determinants for worker i at time t. Since Equation 1 includes both person and year fixed effects, age effects cannot be separately identified. We therefore impose a non-linear age profile that becomes flat at age 40 (Card et al., 2018).

Standard group-specific AKM models (e.g., by gender or race) require estimating separate regressions for each group. The reason is that group membership in those settings is time-invariant, so worker mobility generates identifying variation only for the firm effects associated with that specific group. Therefore, firm effects are identified relative to group-specific reference firms and cannot be compared across groups without imposing a common normalization. By contrast, contract type varies within workers over time. This within-worker mobility allows us to estimate a single model with contract-specific firm premia that are directly comparable across contracts, without requiring a common normalization.

Identifying Assumption We estimate Equation 1 by OLS. Identification of ψ_J^c relies on workers' transitions across firm-contract pairs. For the estimates to be unbiased, the fol-

[1] Our framework follows Drenik et al. (2023) and Bergeaud et al. (2024), who allow firm effects to differ between regular workers and those employed through temporary work agencies. We instead focus on in-house workers and distinguish them by contract type.

lowing orthogonality condition must hold:

$$\mathbb{E}(\epsilon_{it}|i, c_{it}, J_{it}, X_{it}) = 0.$$

This condition imposes two requirements: (i) the mapping from worker and firm heterogeneity into expected log wages is additively separable, and (ii) worker mobility across firm-contract pairs is not driven by time-varying residual wage components, commonly referred to as the "exogenous mobility" assumption. This assumption rules out sorting driven by transitory shocks or idiosyncratic match-specific effects. Section 4.2 provides evidence supporting both exogenous mobility and the additive structure in Equation 1.

2.2 The Temporary Work Wage Gap

We now interpret wage differences between workers on temporary contracts and those on permanent contracts through the lens of the wage model described in Equation 1.

Overall Wage Gap The overall wage gap between temporary and permanent workers is defined as:

$$\Delta w \equiv \mathbb{E}[\ln w_{it} \mid c_{it} = T] - \mathbb{E}[\ln w_{it} \mid c_{it} = P] \tag{2}$$

where T and P index temporary and permanent workers, respectively. We quantify the gap by estimating:

$$\ln w_{it} \;=\; \beta \;\cdot\; \mathbb{1}(c_{it} = T) \;+\; \epsilon_{it} \tag{3}$$

where $\ln w_{it}$ is the log hourly wage of worker i at time t and $\mathbb{1}(c_{it} = T)$ equals one if worker i at time t holds a temporary contract and 0 otherwise. The coefficient β captures

Δw. Based on Equation 1, Δw can be expressed as:

$$\begin{aligned}\Delta w \;=\; & \mathbb{E}[\alpha_i \mid c_{it} = T] \;-\; \mathbb{E}[\alpha_i \mid c_{it} = P] \\ & + \gamma \cdot \big(\mathbb{E}[X_{it} \mid c_{it} = T] \;-\; \mathbb{E}[X_{it} \mid c_{it} = P]\big) \\ & + \; \mathbb{E}[\psi^T_{J_{it}} \mid c_{it} = T] - \mathbb{E}[\psi^P_{J_{it}} \mid c_{it} = P]. \end{aligned} \tag{4}$$

In Equation 4, the first term isolates differences in mean worker effects, measuring the average productivity gap between temporary and permanent workers at a standardized age. The second term accounts for differences in time-varying observables. The third term captures the average difference in firm-contract pay premia received by the two groups.

Firm Pay Premia Gap Let $\Delta\psi$ denote the difference in the average firm pay premia paid to temporary workers and permanent workers:

$$\Delta\psi \equiv \mathbb{E}[\psi^T_{J_{it}} \mid c_{it} = T] - \mathbb{E}[\psi^P_{J_{it}} \mid c_{it} = P] \tag{5}$$

By adding and subtracting the term $\mathbb{E}[\psi^P_{J_{it}} \mid c_{it} = T]$, we can decompose $\Delta\psi$ into two components:

$$\Delta\psi \;=\; \underbrace{\mathbb{E}[\psi^P_{J_{it}} \mid c_{it} = T] - \mathbb{E}[\psi^P_{J_{it}} \mid c_{it} = P]}_{\text{sorting}} \;+\; \underbrace{\mathbb{E}[\psi^T_{J_{it}} - \psi^P_{J_{it}} \mid c_{it} = T]}_{\text{pay-setting}} \tag{6}$$

In Equation 6, the *sorting* term measures differences in the average value of $\psi^P_{J_{it}}$ across contract types, and is negative if permanent workers are more likely to be employed at high-premium firms. The *pay-setting* term isolates the average within-firm pay premia difference between temporary and permanent workers. The term is negative when temporary workers receive lower firm premia than their permanent co-workers at the same employer.

3 Background and Data

3.1 Institutional Setting

Employment in the Netherlands is organized around two contract types: permanent, open-ended contracts, and temporary contracts of limited duration. Since the liberalization of temporary employment in the 1990s, its incidence has risen steadily, exceeding 20% of employment by 2019, above OECD and EU averages (Figure A.1). The key distinction between contracts is employment protection. Permanent contracts entail substantial dismissal costs, whereas temporary contracts can expire at little or no cost. Compared with other OECD countries, the Netherlands combines strict protection for permanent contracts with relatively weak protection for temporary workers (Figure A.2). In particular, dismissing permanent workers requires approval on specified grounds, including economic reasons, poor performance, prolonged illness, or misconduct.

Firms face no restrictions on the share of temporary workers they may employ, but workers can receive at most three consecutive temporary contracts with the same employer over a three-year window, after which the contract must become permanent or end.[2] A break of at least three months resets the count, allowing the employer to restart the sequence.[3] Dutch labor law otherwise requires equal health-insurance and pension contributions for both contract types, conditional on tenure and wages. This leaves firms with considerable discretion over wages, bounded only by the statutory minimum wage — varying by age, with workers under 21 entitled to a fraction of the adult minimum wage — and by collective bargaining agreements (CBAs). CBAs apply to signatory firms and are often extended by government decree to entire sectors. Consequently, coverage is high: 76% of employees on average during our sample period (OECD, 2007). These agreements typically specify wage increases rather than levels. Importantly for our analysis, both institutional constraints apply equally to temporary and permanent contracts.

[2] Two years between 2015 and 2019.
[3] Six months between 2015 and 2019.

3.2 Data and Descriptives

Dataset We use administrative data from Statistics Netherlands, covering the universe of workers and firms in the Netherlands over the period 2013–2019. Our starting point is a matched employer–employee dataset containing information on contract type (permanent or temporary), monthly earnings and hours worked (regular and overtime), whether hours are fixed or flexible, job start and end dates, and firm sector. From these data, we construct a quarterly panel by summing up hours and earnings for a worker within a firm. The panel includes information on the hourly wage, computed as total gross earnings divided by total hours worked, as well as contract type and other job characteristics. When multiple jobs are observed within the same quarter, we retain the job that accounts for the largest share of earnings. We deflate wages using the Consumer Price Index (2015 = 100). Finally, we merge the job-level data with demographic information from population registries, including age, gender, and migration background.

Sample Selection We focus on workers hired directly by firms on either temporary or permanent contracts and exclude temporary agency workers, whose user firm is unobserved in our data. This distinction is important for our decomposition of the firm-premia gap because treating the staffing agency as the employer would distort both the sorting and pay-setting components. Workers placed at different user firms would be pooled under a common agency identifier, preventing us from identifying sorting across the firms where they actually work. Similarly, wage differences between placed temporary workers and the agency's own permanent employees would be incorrectly attributed to within-firm pay-setting. Restricting the sample to direct hires therefore ensures that the firm identifier corresponds to the employer within which temporary and permanent workers are compared. This restriction retains over 70% of worker-year observations with a temporary contract. Finally, we exclude workers who are ever observed in the top or bottom 0.5 percent of the wage distribution.

Firm-contract effects in Equation 1 are identifiable only within "connected sets" of firm-contract pairs that are linked by worker mobility. Accordingly, we restrict our attention to the largest connected set of firm–contract pairs and the workers employed in it. Finally, firms that employ workers exclusively under a single contract type limit our ability to assess the within-firm contribution, since wages under the alternative contract are never observed. To implement the decompositions in Equations 4 and 6, we therefore further limit the sample to the largest connected set of firms that employ at least one worker under each contract type during the sample period, which we refer to as the "dual-contract set."

Summary Statistics Table 1 reports summary statistics separately by contract, for the overall sample, the largest connected set, and the dual-contract set. The latter cover 97% and 91% of the overall sample, respectively, and remain highly representative of the overall sample. Restricting the analysis to either connected set has little bearing on the mean values of worker characteristics, as shown in Table 1.

Temporary workers account for 26% of observations. They are younger, more likely to be women and migrants, and have lower wages and job tenure than permanent workers. They also tend to work at larger, lower-paying firms. Table A.1 shows that temporary employment varies considerably across sectors, from over 50% in accommodation and food services to around 11% in mining, but is present in all major sectors. Sectors with high temporary employment shares account for relatively little total employment, suggesting that sectoral composition alone is unlikely to explain aggregate differences. We also show that our results are not driven by any particular sector.

4 Results

4.1 The Temporary Work Pay Gap

We quantify the temporary work pay gap by estimating the regression in Equation 3 on the dual-contract set. Table 2 reports OLS estimates of β.[4] Column 1 shows an unconditional gap of 28 log points per hour. Adding year and employer fixed effects in Column 2 reduces the gap by about 40% to 17 log points. Columns 3–5 assess the role of worker heterogeneity in the remaining within-firm gap by sequentially adding demographic controls (gender and migrant background), time-varying controls, including the full-time equivalent and hours flexibility, cubic polynomials in age and job tenure, and finally worker fixed effects. With worker effects included, the within-firm gap falls to 2.1 log points, suggesting that time-invariant worker heterogeneity explains much of the within-firm gap and leaves a limited role for differential pay-setting. The estimate is similar to Bergeaud et al. (2024) for temporary agency workers in France. We next quantify the worker and firm contributions and decompose the latter into sorting and pay-setting.

4.2 Unpacking the Temporary Work Pay Gap

To decompose the temporary work pay gap, we first estimate the modified AKM model in Equation 1 and then use the recovered parameters to implement the decompositions in Equations 4 and 6.

4.2.1 AKM Model Estimation

Table A.3 summarizes the results from estimating Equation 1. In general, the two-way fixed effects model fits the data well, with an adjusted R^2 of 0.94. The standard deviation of worker effects is more than twice that of firm–contract effects, implying that wage dispersion is driven primarily by worker characteristics rewarded similarly across

[4] Results are identical using the largest connected set (Table A.2).

firms. Consistent with this pattern, a variance decomposition shows that fixed worker characteristics account for about 60% of total wage variation in the full sample, while firm–contract effects explain an additional 11%. Although plug-in AKM variance estimators can suffer from limited-mobility bias (Andrews et al., 2008; Bonhomme et al., 2019), this is not a concern in our analysis because we decompose means rather than variances. Appendix B nonetheless shows that our findings are identical across three bias-correction methods. Next, we present supporting evidence for two key identifying assumptions: additive separability and exogenous mobility.

Additive Separability Equation 1 imposes additive separability between worker effects and firm-contract effects, implying a firm pays an equal wage premium to all temporary workers and an equal premium to all permanent workers, regardless of their skill. We validate this assumption in two ways. First, adding interactions between worker and firm–contract effects yields only a small increase in the adjusted-R^2, from 0.943 to 0.960, whereas a violation of additive separability would yield a much larger increase in explanatory power. This suggests that incorporating unobserved worker–firm–contract complementarities explain little additional wage variation. Second, following Card et al. (2013), we examine mean residuals across deciles of estimated worker and firm–contract effects. Figure A.3 confirms that mean residuals are uniformly near zero across all decile combinations, providing further support for additive separability.

Exogenous Mobility OLS estimates of the firm-contract wage premia are biased if worker mobility across firms and contract types (within or across firms) is correlated with time-varying residual components of wages. To assess the plausibility of this assumption, we follow Card et al. (2013) and present event studies of wage changes around job transitions across firm-contract pairs. We classify origin and destination pairs by quartiles of coworker wages (Figure A.4) or estimated firm-contract premia (Figure A.5) and trace the evolution of mean wages around the time of a job transition. Since our framework allows for contract-specific premiums, we conduct this check separately for each possible contract transition: permanent-to-permanent, temporary-to-permanent, temporary-to-temporary, and permanent-to-temporary.

Two patterns emerge. First, irrespective of contract transition, wage changes exhibit the approximate symmetry predicted by an additive model under exogenous mobility: gains from moving to higher-premium pairs mirror losses from moving to lower-premium pairs. Second, wage profiles are essentially flat before and after a move within each transition group, indicating that time-varying unobservables do not systematically drive wages or mobility decisions.

In our modified AKM framework, exogenous mobility must also hold *within* the firm: the conversion of a worker from a temporary to a permanent contract at the same employer, or the reverse, should not be correlated with time-varying residual wage components. Figure A.6 shows that wage changes around within-firm conversions are also approximately symmetric, though smaller in magnitude than those around across-firm moves. Moreover, wages are flat before and after the conversion, as in Daruich et al. (2023), who also study contract conversions. Though, flat wage profiles are to be expected in our setting: the expiry date of a temporary contract is fixed when the contract is signed, so the timing of conversion is largely predetermined and cannot respond to a worker's recent productivity or wage realizations. Taken together, the event-study evidence supports the wage model in Equation 1 and the exogenous mobility assumption required for consistent identification of the firm-contract effects.

4.3 Decomposition Results

We use the parameter estimates from Equation 1 to decompose the temporary work pay gap into worker and firm effects, and to quantify the relative contributions of sorting (across-firm) and pay-setting (within-firm) effects, based on Equations 4 and 6. Table 3 reports the results.

Row 1 presents the results for our baseline sample, the dual-contract set. As documented earlier, Column 1 shows that temporary workers earn, on average, 28 log points less per hour than permanent workers. Columns 2 and 3 decompose this gap into differences in mean worker effects and differences in firm-contract effects, respectively.[5] Worker effects account for roughly half of the gap, while firm pay premia account for an additional 28 percent: temporary workers receive firm premia about 7.9 log points lower than permanent workers.[6] Columns 4 and 5 decompose the firm pay premium gap into sorting and pay-setting, following Equation 6. Sorting is substantially more important: it accounts for approximately 71 percent of the gap, compared with 29 percent for pay-setting. Thus, most of the firm contribution to the temporary work pay penalty reflects the underrepresentation of workers with temporary contracts in high-paying firms, rather than differential pay within the same firm. This pattern is not driven by any particular sector.[7]

Row 2 shows that these results are robust to an alternative decomposition, in which the sorting effect is computed comparing the average temporary-contract pay premia across jobs held by temporary and permanent workers, while the pay-setting effect is calculated using the distribution of jobs held by permanent workers.[8]

We also perform the decomposition separately by gender, estimating Equation 1 for women and men, which yields gender-specific firm–contract effects. The resulting decompositions, reported in rows 3 and 4 of Table 3, are very similar across genders: in both cases, differential sorting accounts for slightly more than two-thirds of the firm pay-premium gap.

[5]For brevity, we omit differences in mean time-varying observables.

[6]This finding is consistent with Goldschmidt and Schmieder (2017), who document a firm wage premium gap of roughly 12 log points between outsourced and non-outsourced workers.

[7]Results are robust to sequentially excluding major sectors (Figures A.7 and A.8).

[8]The alternative specification is implemented using the modified version of Equation 6:

$$\Delta\psi \ = \ \mathbb{E}[\psi^T_{J_{it}} \mid c_{it} = T] - \mathbb{E}[\psi^T_{J_{it}} \mid c_{it} = P] \ + \ \mathbb{E}[\psi^T_{J_{it}} - \psi^P_{J_{it}} \mid c_{it} = P].$$

Institutional Constraints An important consideration in our context is the minimum wage, which during our sample period was approximately 47% of the median full-time wage for workers over 21 (OECD, 2015). Binding minimum-wage constraints could limit firms' ability to offer lower wages, increasing $\psi^T_{J_{it}}$ relative to $\psi^P_{J_{it}}$ and biasing the pay-setting estimate downward. Re-estimating our modified AKM model on a sample restricted to workers aged 30 and older, whose wages are less likely to be affected by the minimum wage, yields a pay-setting contribution only slightly larger than baseline (Row 5 of Table 3), suggesting the minimum wage has little effect on our results.

Comparability Between Temporary and Permanent Jobs If firm-specific pay premia accrue primarily in stable, long-term employment relationships (Kline et al., 2019), for instance through firm-specific human capital accumulation, the estimated pay-setting component may partly reflect tenure differences across contract types. Although our baseline specification flexibly controls for employer tenure, we further address this concern by restricting the sample to workers with less than three years of tenure, the legal maximum duration of temporary contracts in the Netherlands. The resulting decomposition is very similar to the baseline (Table 3, Row 6).

Relatedly, temporary contracts may be disproportionately concentrated in early-career jobs, particularly as workers transition out of education. Even with age controls, differences in pay premia could reflect lifecycle effects if temporary employment captures low-wage initial matches among workers with limited experience. Estimates for workers aged 30 and older closely mirror the baseline (Table 3, Row 5), indicating that lifecycle differences play little role in our estimated firm contribution.

Another concern is that temporary and permanent workers may sort into different occupations, so wage differences could partly reflect occupational composition. While we cannot observe occupations in the data, education is available for approximately 60% of the largest connected set.[9] Following Card et al. (2016), we re-estimate Equation 1 replacing time fixed effects with education-by-time fixed effects, distinguishing low, medium (applied or vocational degree), and high education (university degree). To the extent that education captures occupational differences, this specification mitigates concerns about unobserved occupation effects. Estimates remain unchanged (Table 3, Row 7), with firm pay-premia differences still driven primarily by sorting rather than pay-setting.

Finally, differences in firm pay premia between temporary and permanent workers may reflect differences in hours worked, if firms offering contracts characterized by higher hourly wages and longer working hours disproportionately rely on permanent contracts. To address this, we re-estimate the decomposition on a restricted sample of full-time male workers.[10] The resulting decomposition, reported in the last row of Table 3, remains qualitatively unchanged: sorting continues to account for the majority of the firm pay gap, though its contribution falls from 71% to 62%, suggesting hours worked play some role but do not drive our results.

5 Understanding the Sorting Effect

Since sorting, rather than pay-setting, drives most of the firm component of the temporary work penalty, we now examine whether sorting reflects temporary workers clustering in low-paying sectors or positive assortative matching between workers' skill and firms.

5.1 Sorting Within and Across Sectors

Table A.1 shows substantial sectoral variation in temporary employment. The sorting effect may therefore reflect either the concentration of temporary workers in low-paying sectors (*across-sector* sorting) or underrepresentation of temporary workers at high-paying

[9] Although slightly younger, the subsample's demographic and job characteristics closely mirror the main sample.

[10] We focus on men because in the Netherlands women work part-time at far higher rates: 60% versus 20% (OECD, 2019a).

firms within sectors (*within-sector* sorting). To separate the two, we decompose the sorting term in Equation 6 into within- and across-sector components. Letting s denote a sector:

$$\begin{aligned} \text{Sorting} = & \underbrace{\sum_s \left[\mathbb{E}\left(\psi^P_{J_{it}} \mid T, s \right) - \mathbb{E}\left(\psi^P_{J_{it}} \mid P, s \right) \right] \Pr(s \mid T)}_{\text{within-sector}} \\ & + \underbrace{\sum_s \mathbb{E}\left(\psi^P_{J_{it}} \mid P, s \right) \left[\Pr(s \mid T) - \Pr(s \mid P) \right]}_{\text{across-sector}}. \end{aligned} \tag{7}$$

The first term captures *within-sector* sorting and is negative whenever temporary workers sort into lower-paying firms within the same sector. The second captures *across-sector* sorting, reflecting the differential distribution of temporary and permanent workers across sectors. Panel A of Table 4 shows the sorting effect reflects primarily temporary workers' underrepresentation at high-paying firms within sectors, not their concentration in low-paying sectors: within-sector sorting accounts for 49–59% of the total effect.

5.2 Assortative Matching

If high-skill workers are more likely to work at high-paying firms *and* hold permanent contracts, the sorting we measure may reflect positive assortative matching based on skill. To quantify this channel, we follow Gerard et al. (2021). We classify workers into quartiles of the pooled worker-effect distribution, as a proxy for skill, and compute counterfactual employment shares $\tilde{\pi}^T_j$ and $\tilde{\pi}^P_j$. The counterfactuals preserve each firm's skill composition, while matching contract shares of the local labor market within each skill quartile, thereby mimicking 'contract-blind' hiring.

Using these counterfactual shares, we decompose sorting into a skill-biased component and a residual component (see Appendix C.2):

$$\text{Sorting} = \underbrace{\sum_j \psi^P_j \left(\tilde{\pi}^T_j - \tilde{\pi}^P_j \right)}_{\text{skill-biased sorting}} + \underbrace{\sum_j \psi^P_j \left[(\pi^T_j - \pi^P_j) - (\tilde{\pi}^T_j - \tilde{\pi}^P_j) \right]}_{\text{residual sorting}}. \tag{8}$$

where π_j^c is firm j's observed employment share under contract c. The first term isolates the sorting arising solely from skill differences between temporary and permanent workers, capturing what would occur if every firm offered temporary and permanent contracts in the same proportions as its local labor market within each skill group. The second term captures residual sorting, the difference between actual and skill-based sorting. It is positive when higher-premium firms employ fewer temporary workers than their skill composition and local labor market would predict.

Table 4, Panel B, shows that skill-biased sorting explains 31–34% of the sorting effect, leaving a substantial share unexplained by worker composition alone, consistent with systematic differences in how firms hire workers across contract types beyond what their workforce composition would predict.

6 Contract and Firm Sorting in Other Wage Gaps

Table 1 shows that women and migrants— individuals with at least one foreign born parent— are significantly more likely to hold a temporary contract than men and native workers, respectively. We ask whether these differences in temporary employment contribute to the gender and native–migrant wage gaps.

Using the modified AKM model in Equation 1, the wage gap between groups g and g' is:

$$\Delta w \;=\; \Delta\mathbb{E}[\alpha_i \mid g] \;+\; \gamma \cdot \Delta\mathbb{E}[X_{it} \mid g] \;+\; \Delta\mathbb{E}[\psi^c_{J_{it}} \mid g], \tag{9}$$

where $\Delta\mathbb{E}[\cdot \mid g] \equiv \mathbb{E}[\cdot \mid g] - \mathbb{E}[\cdot \mid g']$. Let $\Delta\psi \equiv \Delta\mathbb{E}[\psi^c_{J_{it}} \mid g]$ denote the firm-contract premium gap (the third term in Equation A.8). We decompose $\Delta\psi$ into a *contract* and a *firm sorting* component, under the assumption that the firm-contract premium paid by a given firm is the same for both groups (see Appendix D for details):[11]

[11] This assumption aligns with evidence that within-firm differences in firm pay premia explain little of the gender and immigrant-native wage gaps (Card et al., 2016; Dostie et al., 2023).

$$\Delta\psi = \underbrace{\overbrace{\sum_c (\pi_c^g - \pi_c^{g'})\,\mathbb{E}[\psi^P_{J_{it}} \mid c, g]}^{\text{Across-firm}} + \overbrace{(\pi_T^g - \pi_T^{g'})\,\mathbb{E}[\psi^T_{J_{it}} - \psi^P_{J_{it}} \mid T, g]}^{\text{Within-firm}}}_{\text{Contract sorting}}$$

$$+ \underbrace{\overbrace{\sum_c \pi_c^{g'}\left[\mathbb{E}[\psi^P_{J_{it}} \mid c, g] - \mathbb{E}[\psi^P_{J_{it}} \mid c, g']\right]}^{\text{Across-firm}} + \overbrace{\pi_T^{g'}\left[\mathbb{E}[\psi^T_{J_{it}} - \psi^P_{J_{it}} \mid T, g] - \mathbb{E}[\psi^T_{J_{it}} - \psi^P_{J_{it}} \mid T, g']\right]}^{\text{Within-firm}}}_{\text{Firm sorting}}. \tag{10}$$

Contract sorting captures the portion of $\Delta\psi$ arising because g and g' differ in their likelihood of holding a temporary contract, and combines two effects. The *across-firm effect* reflects the sorting of temporary and permanent workers into different firms. Since temporary contracts are concentrated at low-paying firms, the group with higher temporary employment is more exposed to low-paying firms. The *within-firm effect* reflects differences in the pay premia received by temporary and permanent workers within firms.

Firm sorting captures differential matching to firms, conditional on contract type. The *across-firm effect* captures whether groups sort into firms with different pay premia, measured by differences in the average permanent-worker premia. The *within-firm effect* reflects whether groups sort into firms with different temporary–permanent pay premia gaps. For example, groups may work at firms offering similar permanent-worker premia but different temporary-worker premia.

Equation 10 reveals an underlying margin behind firms' role in wage inequality across demographic groups that standard group-specific AKM applications cannot separately identify. Standard group-specific AKM models attribute differences in firm pay premia either to sorting across firms or pay-setting (different groups paid differently at the same firm). However, these models abstract from contract types. Consequently, any wage penalty from greater exposure to temporary work is potentially absorbed into one of these two channels. When temporary contracts are concentrated at lower-paying employers, a group's higher likelihood of temporary employment shows up as standard sorting, even though the underlying driver is contract type. Similarly, when firms instead differ in their temporary–permanent pay-setting gaps, differential contract exposure appears as group pay-setting differences. For example, women may appear to be paid less than men at the same firm, even when the employer treats all workers identically conditional on contract type. Our decomposition separates contract-driven differences from firm sorting conditional on contract type, allowing us to assess their relative importance.

We use this decomposition to study the gender and native–migrant wage gaps. Table 5 reports the results. Women earn, on average, 22 log points less than men, of which 4.6 log points, or 21% of the overall gap, reflect differences in firm-contract premia. The premium gap is explained primarily by firm sorting—women and men are employed at different firms conditional on contract type— rather than by women being more likely to be employed under a temporary contract. Within firm sorting, the across-firm channel dominates: women are disproportionately employed at lower-premium firms relative to men, consistent with Card et al. (2016). For the migrant-native wage gap, contract sorting matters more than firm sorting. Of the 11 log point gap, 9% reflects differences in firm-contract premia, of which 70.3% is accounted for by contract sorting. Contract sorting operates mainly through the across-firm channel, as migrants' higher share of temporary employment increases their exposure to the lower-paying firms where temporary work is concentrated.

Taken together, our results suggest that contract type plays only a marginal role in explaining the gender wage gap, but ignoring differential contract-type exposure may conflate firm sorting and pay-setting estimates when analyzing the native-immigrant wage gap.

7 Conclusion

Using matched employer–employee data for the Netherlands, we decompose the wage penalty of temporary work. Extending the Abowd et al. (1999) framework to allow firm effects to vary by contract type, we find that worker fixed effects explain half of the 28-log-point wage gap and firm pay premia 28%. The firm contribution operates mainly through sorting: temporary workers earn less because they work at lower-paying firms, rather than receiving lower pay within firms. Extending the framework to gender and native–migrant wage gaps, we find that contract exposure explains part of the native–migrant firm pay-premia gap but little of the gender gap. Thus, equal-pay mandates within firms may have limited effects, while contract-focused reforms may affect demographic groups differently.

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Table 1: Descriptive Statistics: Temporary vs. Permanent Workers

	Overall Sample			Connected Set					
				All Firms			Dual-contract Firms		
	Permanent (1)	Temporary (2)	Difference (3)	Permanent (4)	Temporary (5)	Difference (6)	Permanent (7)	Temporary (8)	Difference (9)
Worker characteristics									
Age (years)	43.18	35.52	7.66***	43.14	35.46	7.68***	43.19	35.35	7.83***
Female (%)	35.62	42.89	−7.27***	35.33	42.81	−7.49***	35.15	42.84	−7.70***
Migrant background (%)	19.24	28.51	−9.26***	19.33	28.36	−9.03***	19.40	27.48	−8.08***
Job characteristics									
Hourly wage (€)	22.17	16.37	5.81***	22.25	16.40	5.85***	22.34	16.57	5.77***
Hours/week (total)	21.49	19.30	2.20***	21.59	19.40	2.19***	21.69	19.69	2.00***
Tenure (years)	9.68	2.30	7.38***	9.68	2.28	7.40***	9.74	2.22	7.52***
Firm characteristics									
Firm size	16.00	18.95	−2.95***	19.48	20.37	−0.89***	23.13	25.48	−2.35***
Avg. hourly wage (€)	18.42	16.88	1.54***	18.20	16.90	1.30***	18.12	17.61	0.51***
Share of temporary workers (%)	19.22	61.85	−42.63***	21.64	61.38	−39.74***	26.67	50.25	−23.58***
Observations	61,687,543	22,004,248	–	60,047,656	21,429,540	–	57,230,580	19,336,796	–

Notes: The table reports average characteristics of workers with temporary and permanent contracts. The sample consists of private-sector workers aged 21 to 65 with regular employment contracts, excluding individuals ever observed in the top or bottom 0.5% of the wage distribution. Columns 1 and 2 report statistics for the full sample. Columns 4 and 5 restrict the sample to the largest connected set, defined as the largest group of firm–contract pairs linked by worker mobility. Columns 7 and 8 further restrict this sample to firms in the largest connected set that employ workers under both contract types. Columns 3, 6, and 9 report differences in means (Permanent minus Temporary). Wages are measured in real (2015 = 100) euros per hour. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 2: The Temporary Work Pay Gap

Dependent variable: Log Hourly Wage

	(1)	(2)	(3)	(4)	(5)
Temporary	-0.284^{***}	-0.169^{***}	-0.128^{***}	-0.069^{***}	-0.021^{***}
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0001)
Time FE	✓	✓	✓	✓	✓
Demographic controls			✓	✓	
Time-varying controls				✓	✓
Firm FE		✓	✓	✓	✓
Worker FE					✓
Adj-R^2	0.089	0.522	0.314	0.611	0.945
Observations	76.57*m*	76.57*m*	76.57*m*	76.57*m*	76.57*m*

Notes: The table reports OLS estimates of β in Equation 3, with standard errors clustered at the worker level reported in parentheses. *Demographic controls* include indicator variables for gender and migrant status, where migrant status equals one if at least one parent of the respondent is not native Dutch. *Time-varying worker controls* include a cubic polynomials in age and employer tenure, as well as the full-time employment equivalent and a dummy for if hours worked are flexible or fixed. The sample is the dual-contract sample as described in Section 3. ***, ** and * represent statistical significance at 1%, 5% and 10% levels, respectively.

Table 3: Decomposition of the Temporary Work Pay Gap

	Δw	$\Delta\alpha$	$\Delta\psi$	$\Delta\psi$ decomposition (%)	
				Sorting	Pay-setting
Baseline	-0.284	-0.146	-0.079	71.3	28.7
Alternative decomposition	-0.284	-0.146	-0.079	67.7	32.3
Males	-0.304	-0.167	-0.081	72.4	27.6
Females	-0.218	-0.084	-0.070	68.5	31.5
30+ years old	-0.225	-0.159	-0.074	70.1	29.9
Employer tenure ≤ 3	-0.249	-0.136	-0.064	69.9	30.1
Education	-0.277	-0.130	-0.072	71.2	28.8
Full-time work	-0.255	-0.159	-0.054	62.4	37.6

Notes: The table reports the results from the decomposition of the mean temporary work pay gap based on Equations 4 and 6. Column 1 presents the average temporary work log wage gap. Columns 2 and 3 decompose this gap into differences in mean person effects and mean firm effects, respectively. Columns 4 and 5 report the decomposition of the firm pay premia gap into a sorting component and a pay-setting component. Row 1 and 2 use the dual-contract sample. Row 3 and 4 restrict the sample to females and males, respectively. Rows 5, 6, 7, and 8 restrict the sample to workers older than 30 years, workers who have been employed with a given employer for less than three years, workers with education data (we also added education$\times$ times fixed effects into the AKM model), and workers employed full-time, respectively.

Table 4: Decomposition of the Sorting Channel

	Total sorting	Component 1	Component 2
Panel A: Sector Sorting		Within-sector	Across-sector
Major sectors	-0.057	-0.033	-0.023
		(59.2%)	(40.8%)
2-digit sectors	-0.057	-0.028	-0.029
		(49.3%)	(50.7%)
Panel B: Skill-Biased Sorting		Skill-biased	Residual
NUTS2	-0.057	-0.017	-0.039
		(30.6%)	(69.4%)
NUTS3	-0.057	-0.019	-0.037
		(33.8%)	(66.2%)

Notes: The table reports two decompositions of the total sorting term, -0.057 log points in Table 3. Panel A decomposes total sorting into within- and across-sector components, following Equation 7, at two levels of sector granularity. Panel B decomposes total sorting into skill-biased and residual components, following Equation 8, under two alternative definitions of the skill distribution (skill quartiles interacted with NUTS2/NUTS3 region). Below each estimate we report the percentage of total sorting attributable to that component.

Table 5: Gender and Migrant Wage Gaps: Contract vs. Firm Sorting

	Δw	$\Delta\alpha$	$\Delta\psi$	$\Delta\psi$ decomposition (%)			
				Contract sorting		Firm sorting	
				across-firms	within-firms	across-firms	within-firms
Female vs. Male	-0.220	-0.180	-0.046	7.7	3.0	88.5	0.8
Migrant vs. Natives	-0.110	-0.102	-0.010	50.7	19.6	12.0	17.7

Notes: The table decomposes the firm-contract premium gap, $\Delta\psi$, following Equation 10. Column 1 presents the average log wage gap for each group comparison. Columns 2 and 3 show the mean differences in person effects and firm-contract premia, respectively. Columns 4 through 7 report the decomposition of the firm-contract premium gap into a contract sorting component — split into an across-firm channel (Column 4) and a within-firm channel (Column 5) — and a firm sorting component — split into an across-firm channel (Column 6) and a within-firm channel (Column 7).

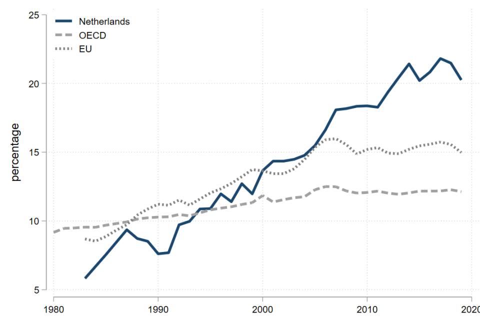
Firms and the Wage Penalty of Temporary Work

Ana Figueiredo Agnieszka Markiewicz Elliott Weder

Online Appendix

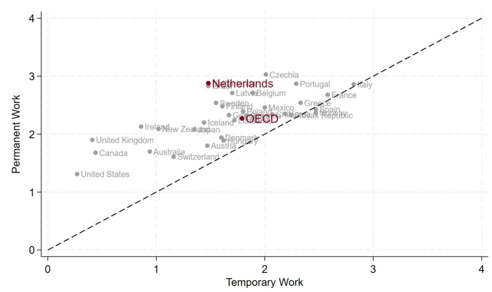
A Additional Tables and Figures

Figure A.1: Temporary Employment



Notes: The figure plots workers with a temporary contract as a share of the total labor force. Temporary contracts are defined as contracts that terminate upon meeting an objective criterion, such as reaching a specified end date or the completion of a task. Source: OECD (2021).

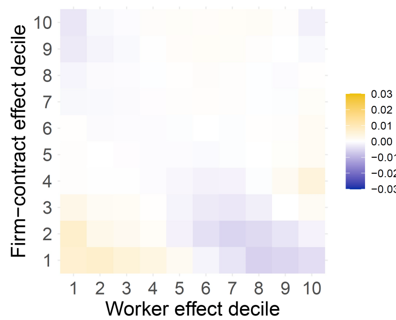
Figure A.2: Employment Protection Indicator



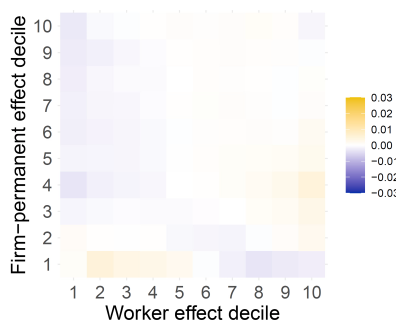
Notes: The figure plots the OECD emplpoyment protection indicator for permanent and temporary contracts. Higher values indicate higher protection. Source: OECD (2019b)

Figure A.3: Mean Residuals by Decile of Work and Firm-Contract Fixed Effects

(a) Full Sample



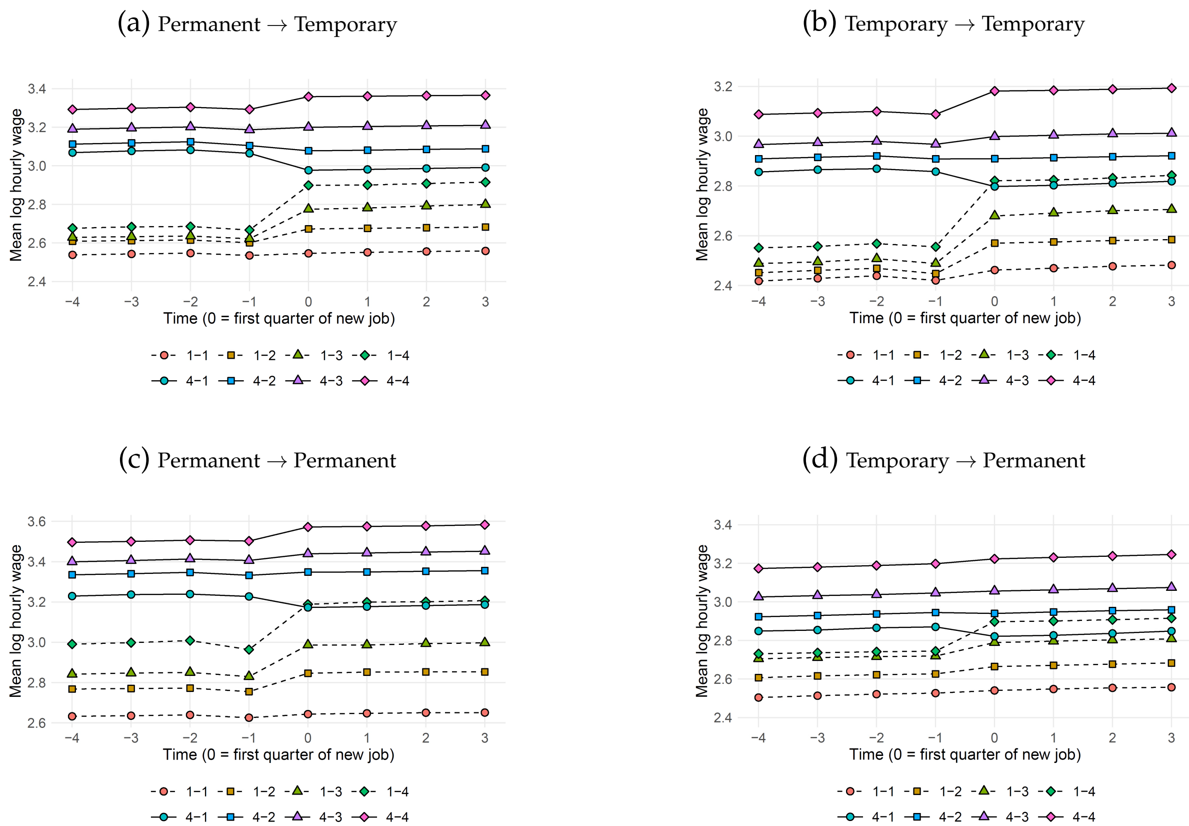
(b) Permanent Workers



(c) Temporary Workers

Notes: Panels (a), (b) and (c) report mean residuals from the wage model in Equation 1 for all workers sample, permanent workers, and temporary workers, respectively. Each of the 100 squares per panel is classified by the decile of estimated firm-contract effects and worker effects. Sample defined in Section 3.

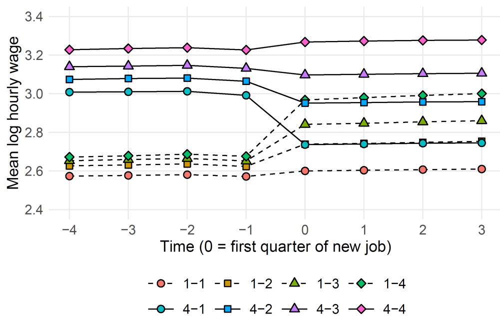
Figure A.4: Wage Changes for Job Movers, by Quartile of Origin and Destination Coworker Wages



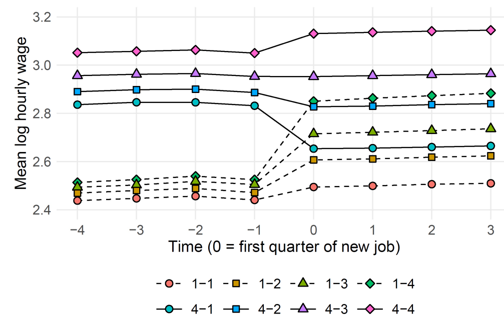
Notes: Panels (a)-(d) show event-study plots of wage changes around job transitions across firm-contract pairs, classified into quartiles based on the average wage of co-workers. Each point is the average wage for workers employed for four consecutive quarters before and after the move. Panels correspond to different contract transitions—permanent-to-temporary, temporary-to-temporary, permanent-to-permanent, and temporary-to-permanent—and focus on moves across firms in the lowest and highest firm–contract premia quartiles. Sample is defined in Section 3.

Figure A.5: Wage Changes for Job Movers, by Quartile of Origin and Destination Firm-Contract Effects

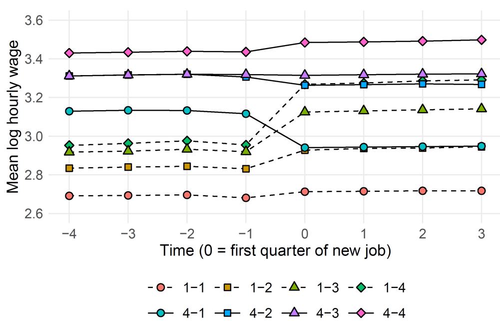
(a) Permanent → Temporary



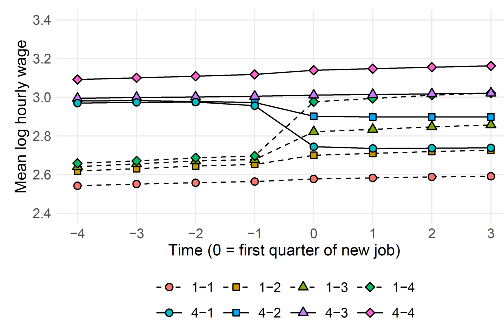
(b) Temporary → Temporary



(c) Permanent → Permanent



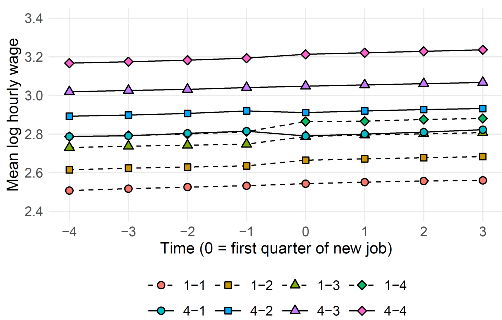
(d) Temporary → Permanent



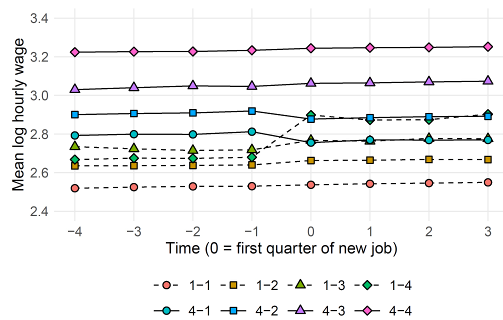
Notes: Panels (a)-(d) show event-study plots of wage changes around job transitions across firm-contract pairs, classified into quartiles based on estimated firm–contract fixed effects from Equation 1. Each point is the average wage for workers employed for four consecutive quarters before and after the move. Panels correspond to different contract transitions—permanent-to-temporary, temporary-to-temporary, permanent-to-permanent, and temporary-to-permanent—and focus on moves across firms in the lowest and highest firm–contract premia quartiles. Sample is defined in Section 3.

Figure A.6: Wage Changes for Contract Movers Within Firms, by Quartile of Origin and Destination Firm-Contract Effects

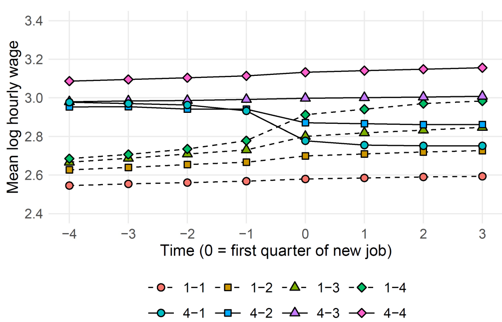
(a) Temporary → Permanent



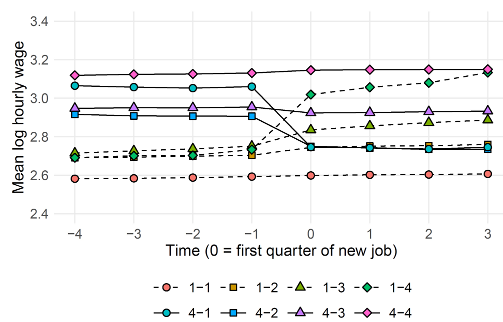
(b) Permanent → Temporary



(c) Temporary → Permanent

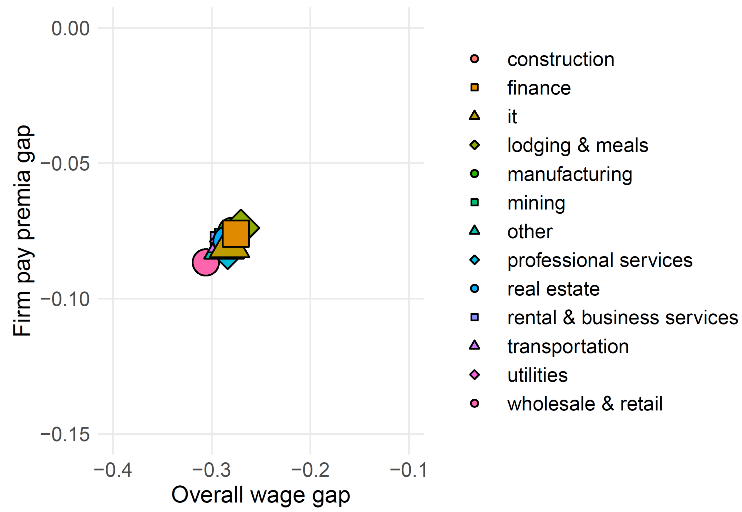


(d) Permanent → Temporary



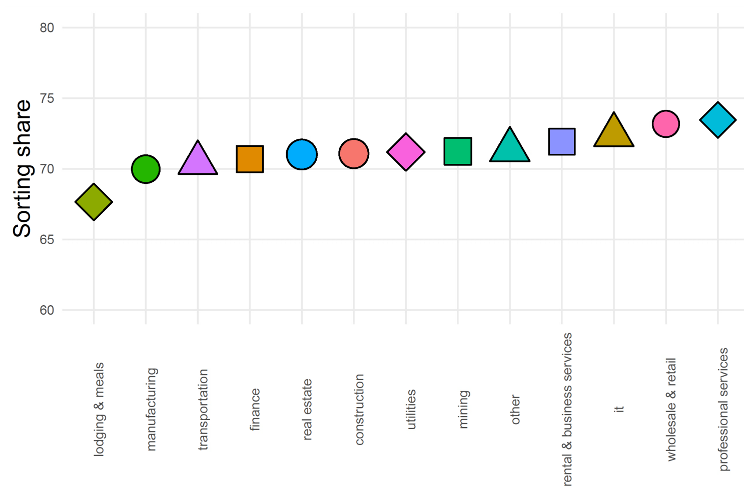
Notes: Panels (a)-(d) show event-study plots of within-firm wage changes around job transitions across firm-contract pairs, classified into quartiles based on estimated co-worker wages (panels (a) and (b)) and firm–contract fixed effects from Equation 1 (panels (c) and (d)). Each point is the average wage for workers employed for four consecutive quarters before and after the move. Sample is defined in Section 3.

Figure A.7: Temporary Work Pay Gap and Firm Pay Premia Gap



Notes: The figure plots the wage gap (Δw) and firm premia pay gap ($\Delta \psi$) when a single sector is left out of the decomposition in Equation 4, namely the one in the label. Sample defined in Section 3.

Figure A.8: Sorting Contribution to Firm Pay Premia Gap



Notes: The figure shows the sorting share of the firm pay premia gap when a single sector is left out of the decomposition in Equation 6, namely the one in the label. Sample defined in Section 3.

Table A.1: Share of workers, temporary workers, and temporary share within each major sector

	Share (%)		
	All workers (1)	Temporary workers (2)	Temporary share (3)
Construction	7.42%	5.17%	18.34%
Finance	4.36%	2.58%	15.59%
IT	5.70%	5.31%	24.52%
Lodging & meals	6.43%	12.39%	50.70%
Manufacturing	16.24%	10.65%	17.24%
Mining	0.24%	0.10%	11.25%
Other services	5.23%	7.09%	35.61%
Professional services	10.37%	11.13%	28.22%
Real estate	1.49%	1.25%	21.98%
Rental & business services	6.57%	8.12%	32.51%
Transportation	9.14%	8.72%	25.07%
Utilities	1.35%	0.75%	14.60%
Wholesale & retail	25.45%	26.74%	27.63%

Notes: The table reports the share of all observations per sector (Column 1), the share of all temporary workers observations (Column 2), and the temporary share within each sector (Column 3). Sample is the largest connected set as described in Section 3.

Table A.2: The Temporary Work Pay Gap: Largest Connected Set

Dependent variable: Log Hourly Wage

	(1)	(2)	(3)	(4)	(5)
Temporary	-0.290^{***}	-0.169^{***}	-0.131^{***}	-0.069^{***}	-0.023^{***}
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0001)
Time FE	✓	✓	✓	✓	✓
Demographic controls			✓	✓	
Time-varying controls			✓	✓	✓
Firm FE		✓	✓	✓	✓
Worker FE					✓
Adj-R^2	0.093	0.533	0.319	0.621	0.945
Observations	81.48*m*	81.48*m*	81.48*m*	81.48*m*	81.48*m*

Notes: The table reports OLS estimates of β in Equation 3, with standard errors clustered at the worker level reported in parentheses. *Demographic controls* include indicator variables for gender and migrant status, where migrant status equals one if at least one parent of the respondent is not native Dutch. *Time-varying worker controls* include a cubic polynomials in age and employer tenure as well as the full-time employment equivalent. The sample is the largest connected set as described in Section 3. ***, ** and * represent statistical significance at 1%, 5% and 10% levels, respectively.

Table A.3: AKM Estimation Results

	Full Sample (1)	Permanent workers (2)	Temporary workers (3)
Mean log hourly wage	2.937	3.013	2.724
Sd. log hourly wage	0.417	0.410	0.362
Std. worker effects	0.324	0.334	0.265
Std. firm-contract effects	0.141	0.137	0.135
Number of observations	81,477,191	-	-
Number of worker effects	4,732,203	-	-
Number of firm-contract effects	498,137	-	-
Number of movers	4,261,804	-	-
Adj-R^2	0.943	-	-

Notes: The table summarizes the results from estimating the two-way fixed effects model for the log hourly wage described in Equation 1 using person-quarter observation in the largest connected sect described in Section 3.

B Limited Mobility Bias

As it is well-known in AKM estimations, a small number of firm-to-firm switches—from which the fixed effects are identified—leads to bias in variance decompositions (Andrews et al., 2008; Bonhomme et al., 2023). The bias may also appear in our setting if there are few firm-contract-to-firm-contract movers. Still, the OLS estimates for α_i and $\psi^c_{J_{it}}$ are unbiased under our exogenous mobility and additive separability assumptions. Therefore, we do not expect our results to be driven by limited mobility bias, because our decomposition only relies on the average and not the variance of fixed effects. We conduct three robustness exercises to show that it is not the case.

First, beginning with the largest connected set in the baseline sample, we further restrict each firm-contract pair to be observed at least ten times and to participate in at least five firm-contract-to-firm-contract moves. Over 80% of observations and workers are retained in this highly-connected sample; however, only around 25% of firms are. So, the mobility restriction strengthens the identification of the firm-contract fixed effects at the expense of being able to estimate fewer firm-contract fixed effects. The 'Highly-connected firms' row in Table A.4 indicates our decomposition result is unchanged to the above sample restrictions.

Second, we employ the 'jack-knife' correction from Dhaene and Jochmans (2015). The correction reduces the bias present in the baseline fixed effects by cleansing the estimates with the average bias from estimates on smaller samples. To employ the correction, we split workers in the baseline sample into two groups—S_0 and S_1—within firms. We next estimate Equation 1 separately on each sample, and construct the jack-knife estimator $\tilde{\psi}^c_{J_{it}}$ as:

$$\tilde{\psi}^c_{J_{it}} = 2 \times \psi^c_{J_{it}} - 0.5 \times (\psi^{c,S_0}_{J_{it}} + \psi^{c,S_1}_{J_{it}}), \tag{A.1}$$

in which $\psi^c_{J_{it}}$ is the firm-contract estimate from the baseline estimation and $\psi^{c,S_n}_{J_{it}}$ denotes the estimate obtained from sample S_n. In the above equation, $\psi^c_{J_{it}}$ is multiplied by 2 because the limited mobility bias is expected to be twice as large in the smaller sets S_n than in the baseline. The 'Jack-knife' row in Table A.4 shows our results are unaltered.

As a last check for limited mobility bias, we estimate a hybrid model based on Bonhomme et al. (2019). Instead of estimating a wage premium for every firm-contract pair, we cluster firms by their empirical wage distribution and estimate a wage premium for each cluster-contract pair. Following Bonhomme et al. (2019), we cluster firms via a k-means clustering algorithm with $K = 10$ clusters. All firms within a cluster-contract pair are assumed to extend the same wage premia to their workers employed on that contract type. Identification of the cluster-contract premia relies on mobility across cluster-contract pairs, rather than across firm-contract pairs. Since $K = 10$ is considerably less than the number of firms in the sample, over 99% of observations from the overall sample are retained in this new largest connected set. Formally, after clustering firms, we estimate:

$$w_{it} = \alpha_i + \psi_k^c + \boldsymbol{X}_{it}\boldsymbol{\gamma} + u_{it}, \tag{A.2}$$

in which k denotes the cluster that the firm j is assigned to. The 'Hybrid' row in Table A.4 shows our baseline results remain unchanged. We conclude that our firm contribution and sorting-pay-setting split results are not driven by limited mobility bias, as we expected ex ante.

Table A.4: Decomposition of the Temporary Work Pay Gap, Limited Mobility Bias

	Δw	$\Delta \alpha$	$\Delta \psi$	$\Delta \psi$ decomposition (%)	
				Sorting	Pay-setting
Baseline	-0.284	-0.146	-0.079	71.3	28.7
Highly-connected	-0.296	-0.158	-0.080	72.7	27.3
Jack-knife	-0.286	-0.149	-0.080	70.7	29.3
Hybrid	-0.286	-0.150	-0.079	73.0	27.0

Notes: The table reports the results from the decomposition of the mean temporary work pay gap based on equations 4 and 6. Column 1 presents the average temporary work pay gaps. Columns 2 and 3 decompose this gap into differences in mean person effects and mean establishment effects, respectively. Columns 4 and 5 further decompose the firm effect in Column 3 into a sorting component and a pay-setting component. See the text for the description of each row in the table.

C Decomposition Derivations

This appendix presents the derivations for the sector sorting decomposition (Equation 7, Section 5) and the skill-based sorting decomposition (Equation 8, Section 5.2). In both, we start from the sorting term in Equation 6, which we denote Δ_{across}:

$$\Delta_{across} = \mathbb{E}[\psi^P_{J_{it}} \mid T] - \mathbb{E}[\psi^P_{J_{it}} \mid P]. \tag{A.3}$$

C.1 Within and Across Sector Sorting

Let s denote a sector. By the law of total expectation, each conditional mean in Equation A.3 can be written as a probability-weighted average over sectors s. Thus, we can rewrite Equation A.3 as

$$\Delta_{across} = \sum_s \mathbb{E}[\psi^P_{J_{it}} \mid T, s] \Pr(s \mid T) - \sum_s \mathbb{E}[\psi^P_{J_{it}} \mid P, s] \Pr(s \mid P). \tag{A.4}$$

Adding and subtracting $\sum_s \mathbb{E}[\psi^P_{J_{it}} \mid P, s] \Pr(s \mid T)$, the sector-specific permanent-worker premium, reweighted by the sectoral distribution of *temporary* workers, and rearranging yields

$$\Delta_{across} = \underbrace{\sum_s \left[\mathbb{E}(\psi^P_{J_{it}} \mid T, s) - \mathbb{E}(\psi^P_{J_{it}} \mid P, s)\right] \Pr(s \mid T)}_{\text{Within-sector sorting}} + \underbrace{\sum_s \mathbb{E}(\psi^P_{J_{it}} \mid P, s) \left[\Pr(s \mid T) - \Pr(s \mid P)\right]}_{\text{Across-sector sorting}}. \tag{A.5}$$

The within-sector term above fixes the sectoral distribution at temporary workers' actual sector mix, $\Pr(s \mid T)$, and asks whether temporary workers sort into lower-premium firms *within* the same sector. The across-sector term fixes the pay level at permanent workers' sector-specific premium, $\mathbb{E}(\psi^P_{J_{it}} \mid P, s)$, and asks whether temporary workers are concentrated in sectors that pay less overall, irrespective of firm-level sorting.

C.2 Skill-Biased Sorting

We can rewrite Equation A.3 as a sum over firms j:

$$\Delta_{across} = \sum_j \psi^P_j \left(\pi^T_j - \pi^P_j\right), \tag{A.6}$$

where π^c_j is the observed share of contract-c employment at firm j.

Let $\tilde{\pi}^c_j$ denote the counterfactual share of contract-c employment at firm j, this is the share firm j would have if contracts were assigned "blindly" with respect to type within each skill-region-time cell. Adding and subtracting the counterfactual shares $\tilde{\pi}^T_j$ and $\tilde{\pi}^P_j$, that is, the shares firm J would have if contracts were assigned "blindly" with respect to type within each skill-region-time cell, and then rearranging yields

$$\Delta_{across} = \underbrace{\sum_j \psi^P_j \left(\tilde{\pi}^T_j - \tilde{\pi}^P_j\right)}_{\text{skill-biased sorting}} + \underbrace{\sum_j \psi^P_j \left[(\pi^T_j - \pi^P_j) - (\tilde{\pi}^T_j - \tilde{\pi}^P_j)\right]}_{\text{residual sorting}}. \tag{A.7}$$

The skill-biased term captures how much sorting we would observe if each firm assigned contracts according to the overall temporary/permanent shares observed among workers of a given skill level in a given local labor market, that is, if a firm simply mirrored the contract-type distribution for its workers' skill group and region, rather than exercising any discretion of its own. The residual term captures sorting attributable to deviations from this skill-based benchmark, potentially reflecting firm-level hiring or screening policies not captured by worker skill.

D Extending the Decomposition to Other Wage Gaps

This appendix derives the four-term decomposition of the firm-contract premium gap between two groups g and g', presented in Equation 10 (Section 6). Following the modified AKM model in Equation 1, the wage gap between two groups g and g' can be written as

$$\Delta w \;=\; \Delta\mathbb{E}[\alpha_i \mid g] \;+\; \gamma \cdot \Delta\mathbb{E}[X_{it} \mid g] \;+\; \Delta\mathbb{E}[\psi^c_{J_{it}} \mid g], \tag{A.8}$$

where $\Delta\mathbb{E}[\cdot \mid g] \equiv \mathbb{E}[\cdot \mid g] - \mathbb{E}[\cdot \mid g']$. Let $\Delta\psi$ be the firm-contract premium gap (the third term in the equation above) by $\Delta\psi \equiv \Delta\mathbb{E}[\psi^c_{J_{it}} \mid g]$. By the law of total expectation, each term in $\Delta\psi$ can be written as a sum over contract types c, weighted by each group's contract-type shares $\pi^g_c \equiv \Pr(c \mid g)$:

$$\Delta\psi = \sum_c \pi^g_c\, \mathbb{E}[\psi^c_{J_{it}} \mid c, g] \;-\; \sum_c \pi^{g'}_c\, \mathbb{E}[\psi^c_{J_{it}} \mid c, g']. \tag{A.9}$$

Adding and subtracting $\sum_c \pi^{g'}_c\, \mathbb{E}[\psi^c_{J_{it}} \mid c, g]$, and then rearranging yields:

$$\Delta\psi = \underbrace{\sum_c (\pi^g_c - \pi^{g'}_c)\, \mathbb{E}[\psi^c_{J_{it}} \mid c, g]}_{\text{Contract sorting}} \;+\; \underbrace{\sum_c \pi^{g'}_c \left[\mathbb{E}[\psi^c_{J_{it}} \mid c, g] - \mathbb{E}[\psi^c_{J_{it}} \mid c, g']\right]}_{\text{Firm sorting}}. \tag{A.10}$$

The first term, contract sorting, isolates how much of $\Delta\psi$ arises because g and g' are employed under different contracts. The second term, firm sorting, isolates how much of $\Delta\psi$ is driven by g and g' working at different firms. Both terms, however, remain uninformative about *why* sorting across different contracts or different firms translates into pay differences between the two groups. For instance, differences in exposure to temporary versus permanent work may matter either because: (i) temporary and permanent workers are paid differently at the same firm, or (ii) temporary workers are employed at different types of firms than permanent workers. To separate them, we use $\psi^c_{J_{it}} = (\psi^c_{J_{it}} - \psi^P_{J_{it}}) + \psi^P_{J_{it}}$, and substitute into the contract sorting and firm sorting components of Equation A.10:

$$\text{Contract sorting} = \sum_c (\pi^g_c - \pi^{g'}_c)\, \mathbb{E}[\psi^P_{J_{it}} \mid c, g] + \sum_c (\pi^g_c - \pi^{g'}_c)\, \mathbb{E}[\psi^c_{J_{it}} - \psi^P_{J_{it}} \mid c, g], \tag{A.11}$$

$$\text{Firm sorting} = \sum_c \pi_c^{g'} \left[\mathbb{E}[\psi^P_{J_{it}} \mid c, g] - \mathbb{E}[\psi^P_{J_{it}} \mid c, g']\right] + \sum_c \pi_c^{g'} \left[\mathbb{E}[\psi^c_{J_{it}} - \psi^P_{J_{it}} \mid c, g] - \mathbb{E}[\psi^c_{J_{it}} - \psi^P_{J_{it}} \mid c, g']\right]. \tag{A.12}$$

Collecting terms yields the full decomposition:[12]

$$\begin{aligned}\Delta\psi = &\underbrace{\sum_c (\pi_c^g - \pi_c^{g'})\, \mathbb{E}[\psi^P_{J_{it}} \mid c, g] + (\pi_T^g - \pi_T^{g'})\, \mathbb{E}[\psi^T_{J_{it}} - \psi^P_{J_{it}} \mid T, g]}_{\text{Contract sorting}} \\ &+ \underbrace{\sum_c \pi_c^{g'} \left[\mathbb{E}[\psi^P_{J_{it}} \mid c, g] - \mathbb{E}[\psi^P_{J_{it}} \mid c, g']\right] + \pi_T^{g'} \left[\mathbb{E}[\psi^T_{J_{it}} - \psi^P_{J_{it}} \mid T, g] - \mathbb{E}[\psi^T_{J_{it}} - \psi^P_{J_{it}} \mid T, g']\right]}_{\text{Firm sorting}}.\end{aligned} \tag{A.13}$$

[12]Since $c \in \{T, P\}$ and $\psi^c_{J_{it}} - \psi^P_{J_{it}} \equiv 0$ identically when $c = P$, any term built from $\psi^c_{J_{it}} - \psi^P_{J_{it}}$ collapses to its $c = T$ component alone: this applies to the first term of Contract sorting and to the second term of Firm sorting in Equation A.12.