WORKING PAPER 2025:17

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ISSN 1651-1166

Outside options and the sharing of match-specific rents*

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May 22, 2025

Abstract: We show that workers use outside options to extract match-specific rents. Exploiting unique data on worker skills, we construct a measure of match quality based on the relationship between workers' multidimensional skills and job-specific skill requirements. Focusing on job-to-job movers, we first demonstrate that match quality associated with the previous job has a stronger impact on current wages than current match quality. This finding strongly suggests that job-to-job movers use previous match quality as an outside option in negotiations with their current employer. Our second key finding relates to wages within ongoing matches. We show that an improvement of local labor market conditions increases the wage return to match quality. This finding is robust to an unusually detailed set of controls, including job-year fixed effects that account for heterogeneity in wage cyclicality across jobs. Outside offers, which are more frequent in tighter labor markets, thus allow incumbent workers to extract a larger share of match-specific surplus. Hence, rent-sharing is pro-cyclical, counter to standard wage-sharing assumptions used in most empirical reduced-form specifications and in canonical search models.

Keywords: Match quality; Wage dispersion; Rent-sharing; On-the-job search; Business cycles

JEL Codes: J31; J24; J41; J63

*We thank Michael Boehm, Anders Forslund, Maarten Goos, Espen Moen and seminar participants at the Danish Central Bank, EALE Padua 2021, IFAU, IFN, BI Norwegian Business School, and SOLE Minneapolis 2022 for useful comments and discussions.

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1 Introduction

Equally skilled workers earn different wages across jobs due to imperfect competition and labor market frictions. Recent research has documented that both external factors, such as outside employment opportunities, and internal factors, such as match quality, affect workers' wages; see, e.g., Caldwell, Haegele, and Heining (2025), Caldwell and Danieli (2024), Fredriksson, Hensvik, and Skans (2018) and Guvenen et al. (2020). However, it is still an open question how to best model the relationship between these factors and workers' wages; see, e.g., Hall and Krueger (2012) and Jäger et al. (2020). In canonical wage bargaining models (Pissarides, 2000), wages are set as weighted averages between match productivity and aggregate conditions with fixed weights, implying that within-firm wage dispersion is unaffected by aggregate market conditions. An alternative set of models in the sequential auction tradition (e.g., Bernhardt and Scoones, 1993, and Postel-Vinay and Robin, 2002) emphasizes that workers should be able to use outside offers to bid up their current wage. Such models imply, for example, that well-matched workers extract a greater share of their match surplus when the state of the labor market improves, which creates pro-cyclical returns to match quality. The issue of how and when counteroffers are used has also attracted interest from policymakers who are concerned that bargaining based on previous wages generates path dependence and perpetuates wage disparities.[1]

In this paper, we provide new evidence on how aggregate and idiosyncratic outside options affect the relationship between match-specific productivity and wages. To accomplish this objective, we construct a measure of job-level match quality using Swedish enlistment data on eight distinct skills following Fredriksson, Hensvik, and Skans (2018), and estimate the interplay between match-quality, outside options, and wages. Our stylized theoretical framework where workers use counteroffers during bargaining predicts that voluntary *job movers* should earn higher wages if their previous match quality was higher. Moreover, match quality will have a larger impact on wages for *job stayers* when the aggregate arrival rate of offers is higher. These two predictions reflect the two possible outcomes of wage re-negotiations in response to counteroffers: if an offer has higher match quality, the worker moves—receiving

[1] As an example, the recent EU Directive 2023/970 aims to strengthen workers' agency by preventing employer-initiated bargaining relative to previous wages through Article 5: "An employer shall not ask applicants about their pay history during their current or previous employment relationships."

a new wage which is increasing in previous match quality; if the worker's current match is better than the offer, the employer may respond by raising wages.

Our measure of match quality relates each worker's skill vector to skill demand in their job (occupation-by-establishment). We show that all elements of the skill vector are relevant for wages and sorting patterns throughout the workers' careers, even though skills are measured at age 18-19. Skill demand in each job is proxied by the skill-composition of other incumbent workers in the job. To isolate idiosyncratic match quality, we analyze our measure conditional on job-specific fixed effects and the market returns to skills. To proxy for the arrival rate of counteroffers among remaining workers, we mainly use variation in the local unemployment rate. For workers who change jobs, we use match quality in the previous job as a measure of outside options.

As a prelude, we show that better matched workers have higher wages and lower separation rates, conditional on job-year fixed effects and skills. Consistent with the notion that employed workers get more offers in years when local labor market conditions are more favorable, we show that the local unemployment rate is negatively correlated with wages and job-to-job mobility, conditional on job fixed effects, aggregate time effects, and skills.

For job-to-job movers, the previous match is a precise measure of outside options, which can be used as a counteroffer when bargaining for wages in a new job, regardless of the cycle. Our first key contribution therefore relates the wages in new jobs to previous, and current, match quality for job-to-job movers. Controlling for the skill composition in both the previous and the current job, we find that match quality in the previous job is more important for the current wage than current match quality.[2] Strikingly, previous match quality has a statistically significant impact on the current wage even when controlling for the previous wage, suggesting that workers may use the potential for further wage increases in the (old) job to bid up wages in the new job. We do not expect workers who enter a new job after an interim spell of unemployment to be able to use their previous job match as an outside option when bargaining for their new wage. Consistent with this prediction, we do not find any impact of previous match quality on current wages for this group of workers; by contrast, current match quality has a large and statistically significant wage impact. Taken together, these results show that previous match quality only affects the wage for workers who could

[2]Interpreted through the lens of our model, our estimates suggest that the "pure" bargaining power parameter is around 0.4.

use their previous job as an outside option in the wage bargain.

The use of counteroffers can affect how wages of remaining workers evolve across the cycle due to the cyclicality of job-to-job transition opportunities. Our second key contribution is to test this hypothesis by estimating how the wage return to match quality among job stayers vary with local labor market conditions. We identify the interaction between match quality and local unemployment in tightly specified wage equations. The preferred model allows the wage impact of skills to vary over the cycle and includes job-year fixed effects that non-parametrically account for heterogeneity in wage cyclicality across jobs. The wage return to match quality for job stayers is 50 percent higher in the lowest ventile of the local unemployment distribution compared to the highest ventile. The results are not sensitive to functional form; they are robust to alternative measures of local labor market conditions—including shift-share instruments and occupation-specific employment—and do not depend of how we define the local labor market. The effects are significant across the occupational distribution, with a somewhat larger effects for managers and professionals, consistent with survey evidence on counteroffer bargaining in Caldwell, Haegele, and Heining (2025). Interpreted through the lens of our theoretical model, the results suggest that incumbent workers use counteroffers to extract a larger share of the match-specific surplus when the market tightens.

Overall, our results imply that internal (match quality) and external (outside options) factors are not additively separable as in canonical bargaining models. Instead, the results suggest that workers leverage alternative offers to extract rents from the quality of their previous job when moving, and by increasing their share of the match surplus when market conditions improve and counteroffers become more frequent.

We contribute to several strands of literature. First, there is survey evidence describing how wages are set, which aligns well with our first set of findings regarding counteroffers for movers. Barron, Berger, and Black (2006) find that 44 percent of firms would consider making a counteroffer. Hall and Krueger (2012) note that around 40 percent of workers bargained for wages in a new job while maintaining the option to go back to their previous job. More recently, Caldwell, Haegele, and Heining (2025) showed that the majority of workers and firms participate in individual wage bargaining in response to counteroffers.

Second, there is a small literature that have examined how the quality of previous jobs affects current wages among job movers. Di Addario et al. (2023) augment the canonical AKM model of Abowd, Kramarz, and Margolis (1999) by also including *past* firm effects as a measure of outside options for job switchers using Italian data. Overall, they find that such previous firm-effects have a small impact on wages. This result, which stands in contrast to survey evidence on the use of counteroffers in bargaining (e.g., Caldwell, Haegele, and Heining, 2025), masks interesting heterogeneity related to institutional rigidities: the effects are non-trivial in market segments where individual bargaining is prevalent. Our analysis of job switchers differs from Di Addario et al. (2023), since we leverage individual-level variation in match quality—thus capturing the idiosyncratic outside option of each job-to-job mover. Our results show clear evidence of a significant wage impact of past match quality.[3] Incorporating past firm effects as controls do not alter our conclusions, suggesting that workers are able to leverage their match-specific conditions (within firms) when bargaining for new wages after a job transition.

Third, an extensive set of reduced-form studies (surveyed by Jäger et al., 2020) has analyzed how match-specific factors and outside options, respectively, affect wages. Fredriksson, Hensvik, and Skans (2018) study match quality and wages at the job-level whereas Guvenen et al. (2020) and Lise and Postel-Vinay (2020) use occupations. Studies of outside options analyze unemployment (Blanchflower and Oswald, 1994), outside firms (Lamadon, Mogstad, and Setzler, 2022), employment patterns of similar workers (Caldwell and Danieli, 2024), networks (Caldwell and Harmon, 2019), dual jobs (Lachowska et al., 2022), and benefits (Jäger et al., 2020).[4] In contrast, we examine the *interaction* between match productivity and outside options, finding larger returns to match quality under more favorable market conditions.

Finally, there is a literature relying on structural approaches to compare wage-setting mechanisms in settings with two-sided heterogeneity. Postel-Vinay and Robin (2002) discuss how counteroffers and poaching affect the wage distribution within and across job spells in a model with one-dimensional worker and firm heterogeneity. Mortensen (2003) found support for wage bargaining over wage-posting in many market segments. Cahuc, Postel-Vinay, and Robin (2006) embed outside offers and traditional bargaining, finding more support for the

[3]It should be noted that our target metrics are also slightly different; Di Addario et al. (2023) focus on the contribution of past and current firm effect to the overall variance of starting wages (which also depends on the wage dispersion across origin firms) whereas we focus on the relative impact of past and current match quality measured on an identical scale.

[4]See Carlsson, Skans, and Skans (2019) (unemployment), Carlsson, Messina, and Skans (2016) (outside firms), and Fredriksson and Söderström (2020) (benefits) for work on Swedish data.

former. Hagedorn and Manovskii (2013) show that unemployment at the time of hire has no impact on current wages conditional on previous tightness (which they interpret as a proxy for match quality). Bagger and Lentz (2019) and Yamaguchi (2010) use models featuring endogenous search and human capital accumulation to decompose wage dispersion. In contrast to these papers, we build our analysis on a "direct" measure of multi-dimensional match quality. The measure is direct in the sense that it is not inferred from wages, tenure, or market tightness, which allows ut to provide less model-dependent evidence on how market conditions interact with match quality when wages are renegotiated. Our results consistently show that tighter markets are associated with a stronger relationship between idiosyncratic factors and wages.

The paper is structured as follows: Section 2 presents the theoretical framework and derives the key predictions tested in the empirical section. Section 3 details how we take the predictions of the model to data. Section 4 describes the data and the measurements. Section 5 reports the results: we begin with a validation of our measures (see Section 5.1); Section 5.2 examines the prediction related to match quality in the previous job and wages of job movers; Section 5.3 tests the prediction regarding outside offers and rent sharing with remaining workers; Section 5.4, finally, analyzes heterogeneity across occupations. Section 6 concludes.

2 Theoretical framework

We outline a stylized theoretical framework that highlights how and why counteroffers provide an important link between outside options and returns to individual match productivity. The labor market is segmented by skill levels. To simplify, the value of unemployment, b, is homogeneous across workers within each skill segment. For ease of exposition, we focus on a single segment.[5] Productivity (p) differs across matches (worker-job pairs) since workers' have different skill bundles and different jobs require different skills. Workers search for jobs randomly and match quality is revealed after workers and firm have met.

Consider a two-period set-up. In period 1, unemployed workers meet a firm with probability λ; they draw idiosyncratic match quality p from a (differentiable) distribution $F(p)$, decide on whether to match, and bargain over the wage, $w = w(p, b)$. In period 2, employed

[5] This is consistent with our empirical analysis, which holds the main effects of skills constant.

workers meet with other firms (again at rate λ), and draw match quality, p'. Since unemployed and employed workers draw matches at the same rate, the reservation wage (and reservation match quality) in period 1 is b.

Workers move if $p' > p$. When $b < p' \leq p$, offers are instead used to bid up the wage in the current match *if* the worker can use the alternative offer in negotiations with the current employer. There are three scenarios when meeting a firm in period 2:

1. If $p' \leq b$, nothing happens and the worker retains $w(p, b)$;
2. If $b < p' \leq p$, the worker (potentially) renegotiates the wage to $w(p, p')$; and
3. If $p' > p$, the worker moves and earns $w(p', p)$.

Following Cahuc, Postel-Vinay, and Robin (2006), we let wages be affected by Nash bargaining and by counteroffers. Without counteroffers, workers obtain a share η of the (flow) surplus. If employers can respond to counteroffers, wages are instead defined by:

1. $w(p, b) - b = \eta(p - b)$ for $p' \leq b$ (and entrants from unemployment);
2. $w(p, p') - p' = \eta(p - p')$ for $p' \in (b, p]$; and
3. $w(p', p) - p = \eta(p' - p)$ for $p' > p$.

With a slight change of notation, define p_1 and p_2 as the first and second most productive relationship the worker has found during his current employment spell. p_1 is thus match quality with the current employer and p_2 the best outside offer. Then, the wage-setting rule is:

$$w = w(p_1, p_2, b) = \eta p_1 + (1 - \eta) \max(b, p_2) \tag{1}$$

where $p_2 = 0$ if the worker has received no alternative wage offer. This rule is more general than it may appear. It is derived by Cai (2020) as the result of a strategic alternating bargaining game (of the Rubinstein (1982) type) when the risk of an exogenous break-up of the bargain is ignorable.

In our empirical work, we estimate separate wage regressions for stayers and movers. Consider, first, the wage for a stayer in the second period:

$$\begin{aligned} \mathbb{E}[w \mid p' \leq p, p, \lambda] &= w(p, b) + \theta \left(\mathbb{E}[w(p, p') \mid p' \leq p, p] - w(p, b) \right) \\ &= \eta p + (1 - \eta) b + (1 - \eta) \theta \left(\mathbb{E}(p'_s) - b \right) \end{aligned} \tag{2}$$

where θ denotes the probability of receiving an outside offer that is higher than b conditional on remaining in the initial job:

$$\theta = \frac{\lambda(F(p) - F(b))}{1 - \lambda + \lambda F(p)} \tag{3}$$

and the second line uses (1), as well as the notation $\mathbb{E}(p'_s) = \mathbb{E}[p' \mid b \leq p' \leq p]$. θ is thus the probability that the wage is adjusted for a stayer, while $\mathbb{E}(p'_s)$ denotes "offer quality"—the average productivity of the outside offers that can be used in renegotiations.

Differentiating with respect to p, we obtain:

$$\frac{\partial \mathbb{E}[w \mid p' \leq p, p, \lambda]}{\partial p} = \eta + (1 - \eta)\left[\frac{\partial \theta}{\partial p}\left(\mathbb{E}(p'_s) - b\right) + \theta \frac{\partial \mathbb{E}(p'_s)}{\partial p}\right] \geq \eta \tag{4}$$

Since θ and $\mathbb{E}(p'_s)$ are increasing in p, it follows that workers receive higher rents when outside offers can be used in bargaining than in the canonical wage bargaining set up (where the effect would be given by η).

An improvement in outside opportunity—as measured by the arrival rate λ—changes the wage impact of match quality by:

$$\frac{\partial^2 \mathbb{E}[w \mid p' \leq p, p, \lambda]}{\partial p \partial \lambda} = (1 - \eta)\left[\frac{\partial \theta}{\partial \lambda}\frac{\partial \mathbb{E}(p'_s)}{\partial p} + \frac{\partial^2 \theta}{\partial p \partial \lambda}\left(\mathbb{E}(p'_s) - b\right)\right] \tag{5}$$

The first term is obviously positive: the probability of a wage adjustment rises with λ and the average quality of such outside offers is higher for well-matched workers. However, the cross-derivative in the second term is in principle ambiguous in sign—essentially because probabilities are bounded. In Ek et al. (2023) we examine under what conditions the derivative in (5) is positive. This depends on λ, b, and the match-quality distribution $F(p)$. Using simulation, we find, for example, that equation (5) is always positive under log-normality of $F(p)$, no matter the values of λ and b. In general, an increase in λ raises the wage impact of match quality in all realistic cases.[6] Rents among stayers are thus increasing in outside

[6]One can show analytically that $\omega\theta \leq 0.5$, where ω is defined by $\mathbb{E}(p'_s) = \omega p + (1 - \omega)b$, implies that equation (5) is positive; here, ω reflects the shape of the density of the match quality distribution on the (p, b) range. A decreasing (increasing) density has $\omega \leq 0.5$ ($\omega \geq 0.5$). A uniform distribution has $\omega = 0.5$, and thus $\omega\theta \leq 0.5$ always. Moreover, $\lambda(1 - F(b)) \leq 0.5$, implies $\theta \leq 0.5$, and then the sign of equation (5) is positive, no matter

options.

This prediction contrasts with implications of a standard Nash-bargaining sharing framework (e.g., Pissarides, 2000) where $w = \eta p + (1-\eta)b(\lambda)$. In this case, the only relevant outside option is the aggregate state $(b(\lambda))$; and the wage effects of idiosyncratic productivity would remain constant if λ changes. In our empirical work we examine whether the wage impact of idiosyncratic productivity is pro-cyclical among workers who remain in their jobs.

The second part of the model pertains to workers who change jobs in between two years. Consider the wage for a worker who moved to a new job because $p' > p$. Then, bargaining revolves around the quality of the old match, regardless of the arrival rates of outside offers:

$$\mathbb{E}\left[w \mid p' > p,\ p,\ p'\right] = \eta p' + (1-\eta)p \tag{6}$$

This wage equation has an extremely simple form. The key prediction is that previous match productivity matters for wages also in the new job. This is, again, different from the standard sharing framework where the outside option *at the market-level* would replace the previous match productivity, p, in equation (6).

Equation (6) is only relevant for workers that got an alternative job before separating from their initial match. If the worker separates *before* finding alternative employment, the wage equation is instead $w(p', b)$: that is, previous match quality, p, is not the relevant outside option for worker with an intermittent spell of unemployment. In our empirical part we thus estimate the wage-impact of previous match quality for job-to-job (or EE) movers and for (EUE) movers with an interim unemployment spell, where previous match quality only matters for EE movers.

To summarize, our empirical work tests three predictions, in the following order:

Prediction 1 (EE movers)

Match quality in the previous job has a positive impact on wages in the current job for workers who have made an employment-to-employment move.

the shape of the match quality distribution.

Prediction 2 (EUE movers)

Match quality in the previous job has no impact on wages in the current job for workers who have moved to a new job with an intermittent spell of unemployment.

Prediction 3 (Stayers)

For stayers, the wage return to match quality in the current job is increasing in the probability of obtaining an outside offer.

3 Taking the predictions to data

Much of the literature on match productivity and wages have inferred match quality indirectly from tenure, wages or market conditions, interpreted through the lens of structural models. The aim of this paper is to present evidence on how match quality and aggregate market conditions interact to determine the wages of stayers and movers while imposing a minimum of structural assumptions. For these purposes, we need a measure of match quality that does not rely on wages, market conditions, or the decision to remain with the current employer.

We draw on the skill-weights approach (Lazear, 2009) to derive our measure of match quality. Intuitively, we think of idiosyncratic match quality as the intersection of a multidimensional skill vector and job-specific skill demands. This approach requires data on a vector of skills that are differentially relevant across jobs, and a proxy measure of job-level skill requirements. We follow Fredriksson, Hensvik, and Skans (2018) and use the skill sets of other tenured workers in the same job to measure skill requirements. The strategy leverages that workers who remain within a job should have a skill set that is well aligned with the skill demands of the job.[7]

Formally, for each worker i, in job j, the average skill level in dimension k among *other*, tenured, incumbents in j is denoted by $\tilde{s}_{j,k}^{-i}$ where $-i$ represents all individuals except i. We hold $\tilde{s}_{j,k}^{-i}$ constant across time within jobs to ensure that the skill-requirement is not endogenous to the cycle. For each skill-dimensions k, we compute the absolute differences between

[7] A possible alternative is to instead use job-specific skill-prices but the obvious drawback is a lack of precision when estimating these prices. In the robustness section, we show that this strategy yields similar results to our baseline approach.

individual skills $s_{i,k}$ and $\tilde{s}_{j,k}^{-i}$ and sum across the K skill dimensions:

$$D_{i,j} = \sum_{k=1}^{K} \left| s_{i,k} - \tilde{s}_{-i,j,k} \right| \tag{7}$$

to get a measure of the distance between the skill set of worker i, and the average skill composition of others in the job. Our measure of match quality (M) is the negative of the standardized distance D:

$$M_{i,j} = - \left(\frac{D_{i,j} - \text{mean}\,(D_{i,j})}{\text{sd}(D_{i,j})} \right) \tag{8}$$

The standardization implies that a unit change in match quality is equivalent to a one standard deviation reduction in the distance to the average skills of the coworkers.

To illustrate the properties of the measure, it is useful to consider a generic wage equation describing the wages $w_{i,j}$ of worker i in job j:

$$w_{i,j} = \alpha_j + \boldsymbol{\beta'} \boldsymbol{s}_i + \mu M_{i,j} \tag{9}$$

where α_j captures job-specific fixed effects and $\boldsymbol{s}_i$ is a vector comprising all skills. A marginal increase in $s_{i,k}$ will have an effect through the market (via $\beta_k > 0$) and an effect from job-match quality (through $\mu > 0$). If the worker is "underskilled" at job j in dimension k (i.e., if $s_{i,k} < \tilde{s}_{j,k}^{-i}$), the overall wage return equals $\beta_k + \mu$; if the worker is "overskilled", on the other hand, we have $\beta_k - \mu$. The model thus allows the overall wage return to additional skills to *kink* at the point of optimal match quality (i.e., where $s_{i,k} = \tilde{s}_{j,k}^{-i}$).

In our regressions, we isolate the wage impact of match quality (i.e., $\mu > 0$) by always controlling for worker skills. We also include job fixed effects to avoid confounding from fixed job amenities.

To measure match quality, we use data on 8 skills from Swedish military enlistments to compute match quality. In Section 5.1, we corroborate this measure by showing that it has a positive impact on wages and that it is negatively related to separations.

For the arrival rate of offers, we use standard measures of local labor market conditions (unemployment in the baseline). For job movers, we use match quality in the previous job as our measure of the outside option. Notice that our approach differs from Postel-Vinay and

Robin (2002) and Di Addario et al. (2023) that instead focuses on *one-dimensional* worker and firm heterogeneity. In our setting, job movers with similarly (market) valued skills—leaving the same firm—bargain from different positions depending of how well their specific skills matched the skill requirements in the past job.

4 Institutions, data, and measurement

4.1 Institutions and data sources

Our analysis uses data from Sweden, where collective bargaining covers the majority of workers; see Olsson and Skans (2024) for a detailed description of the bargaining process. Bargaining takes place in two stages. In the first stage, industry-level agreements establish the framework for subsequent local negotiations in the second stage. Importantly, the wage-setting protocol includes an element of individual bargaining related to the job performance of the worker in most cases. Most industry-level agreements stipulate a required rate of wage increases at the firm or individual level, but these agreements do not constrain firms from increasing wages even more. In general, because most collective agreements specify rates of wage increases rather than binding minimum wage levels, starting wages tend to be less institutionally constrained than wage increases. Because Swedish collective agreements require individual level assessments during wage setting, vacancies are never posted with explicit wages.

Our baseline strategy include data from the public sector for completeness. Contrary to many other European countries, hiring and wage setting in Swedish public sector organizations do not involve civil servant tests, national wage scales, or similar constraints. Furthermore, collective agreements in the public sector tend to be at least as flexible as in the private sector and recruitment constraints and practices are similar to large private organizations, i.e., line managers recruit and decide on individual-level wages with the assistance of HR departments.

Our data are drawn from population-wide registers with linked information on individuals, workplaces, and firms, as well as basic individual characteristics such as age, education, and municipality. Employment status in November is derived from tax returns, with data spanning from 1985. Employment status is used to infer labor market experience, measured as years observed in employment and capped at 12 years.

The Wage Structure Statistics (WSS), covering 1997-2013, record hours-adjusted wages and three-digit occupations (SSYK-96, corresponding to ISCO-88) annually for half of the private sector and all public sector employees. Data cover all the workers in sampled firms. The sampling probability increases with firm size, and all firms with 500 or more employees are included in the sample. The data include sampling weights and we show robustness tests where we apply these weights (results do not change).

From the Swedish War Archives, we add detailed measures of cognitive and non-cognitive ability. These were collected during the Swedish military draft procedure and are available for nearly all males in the 1951-1976 cohorts, who were enlisted during 1969-1994 at age 18 or 19.

Finally, we collect aggregate data on unemployment from Statistics Sweden and the Swedish Public Employment Service.

4.2 Variables and definitions

4.2.1 The definition of a job

We focus on match quality at the worker-job level. A *job* is an occupation at a workplace.[8] This allows skill requirements to vary between different occupations in a given workplace, as well as between different workplaces for a given occupation.[9]

4.2.2 Local unemployment

Unemployment is measured at the workplace municipality level.[10] It is computed from individual-level information on non-employed job seekers (including participants in active labor market programs) registered at the public employment service. We define *unemployment* as the annual incidence of registered job-seekers among all residents aged 20-64 (i.e., not just labor force participants). Our results are robust to using other measures (see Section 5.3.1).

[8]Data cover 113 occupations. Our regression sample covers 30,000 workplaces and 70,000 jobs.

[9]Sorting across both margins appear empirically relevant; see, e.g., Fredriksson, Hensvik, and Skans (2018) and Choné, Kramarz, and Skans (2023).

[10]Sweden has 10 million residents, 290 municipalities and 21 counties.

4.2.3 Multidimensional skills

Mood, Jonsson, and Bihagen (2012) and Lindqvist and Vestman (2011) provide extensive details on the test scores. Four cognitive skills are based on written tests. These capture *inductive reasoning*, *verbal comprehension*, *spatial ability* and *technical understanding*. The remaining four non-cognitive abilities are assessed during a 25-minute interview with a trained psychologist. These capture *social maturity*, *psychological energy*, *intensity*, and *emotional stability* according to Mood, Jonsson, and Bihagen (2012). We standardize the data to mean zero and standard deviation one for each skill and cohort.

4.3 Sample and descriptive statistics

Our sample consists of workers who were $i)$ employed in year t, $ii)$ sampled in the WSS, $iii)$ Swedish residents during $[t-1, t+1]$ and $iv)$ in our skills data. To define the study population, we use data for 1997-2012 (which allows for a 1 year follow-up). We only use jobs with at least five tenured coworkers to get a reasonably precise measure of average skills within jobs. To construct our subsample of job movers, we select observations for individuals that were observed in a different firm in the previous year and for which we can observe match quality in both periods. Job stayers are instead those in the main sample that had at least one year of tenure.

Columns (1) and (2) in Table 1 report means and standard deviations in the mover and stayer samples, respectively. For comparison, column (3) describes all male workers in the WSS. Job movers are slightly younger and higher educated than job stayers as well as males in general. They are also more likely to be working as professionals and technicians/associated professionals as opposed to in lower- and middle-skilled occupations. The differences between job stayers and males in general (which are mainly driven by firm size) arise because we require jobs to have at least five male workers with skills within each job. The larger firm size makes both job stability and wages somewhat higher than in the overall sample. The distribution of occupations among stayers is similar to the overall population.

For our purposes, a key aspect of the skill scores is that they are relevant predictors of labor market outcomes across the workers' careers. In Figure 1, we show how the wage returns to each component evolves with age. The returns to a standard deviation increase in skills vary between 7 and 14 percent. Initially, the returns grow with age, which is usually

Table 1: Characteristics of the main samples and all workers sampled in the WSS

	Sample		
	Job movers	Job stayers	All males in WSS
	(1)	(2)	(3)
Subsequent job separation	0.172	0.085	0.137
log wage	10.25 (0.39)	10.19 (0.36)	10.13 (0.36)
Age	39.00 (7.741)	42.01 (8.20)	42.87 (11.72)
Years of schooling	13.37 (2.54)	12.43 (2.40)	12.37 (2.45)
Establishment size	427 (83)	548 (1050)	376 (881)
Years of workplace tenure			
—1 to 3	1	0.182	0.345
—4 to 6	0	0.212	0.188
—7 to 9	0	0.152	0.121
—10 or more	0	0.454	0.346
Occupation category (one digit)			
—Legislators, senior officials, and managers	0.061	0.068	0.084
—Professionals	0.384	0.222	0.214
—Technicians and associate professionals	0.245	0.213	0.204
—Clerks	0.032	0.05	0.062
—Service workers and shop sales workers	0.043	0.051	0.084
—Skilled agricultural and fishery workers	0.001	0.005	0.007
—Craft and related trades workers	0.077	0.13	0.119
—Plant machine operators and assemblers	0.135	0.223	0.172
—Elementary occupations	0.021	0.038	0.054
N	58,553	3,938,251	13,352,143

Notes: Columns (1) and (2) report the mean and (when relevant) the standard deviation of key variables in the main job mover and job stayer samples, respectively. For comparison, column (3) reports descriptive statistics for all male workers for which we observe wages and occupation in the Wage Structure Statistics (WSS).

interpreted as reflecting employer learning, at least to some extent. The returns flatten out at around age 40, with some decline after age 55 for non-cognitive skills. Panel (b) of the same figure instead shows that each skill predicts the intensity of skills (along the same dimension) among coworkers in the same job. These patterns are essentially flat, except for a small increase in the beginning of the career, which is consistent with learning about match quality among market entrants (Fredriksson, Hensvik, and Skans, 2018).

If workers leave jobs where they are poorly matched relative to the job requirements, we expect match quality to improve with tenure. Figure 2 shows that this expectation is borne out by the data. In particular, it shows how match quality along a specific dimension, $MQ_{i,j,k} = -\left|s_{i,k} - \tilde{s}_{-i,j,k}\right|$ is related to tenure, holding job fixed effects and skill levels constant. Because worker skills as well as skill requirements are fixed over time, match quality only changes over the tenure profile because of selective separations. Workers with a different skill profile than their coworkers are more likely to separate from their jobs, implying that match quality improves with tenure.

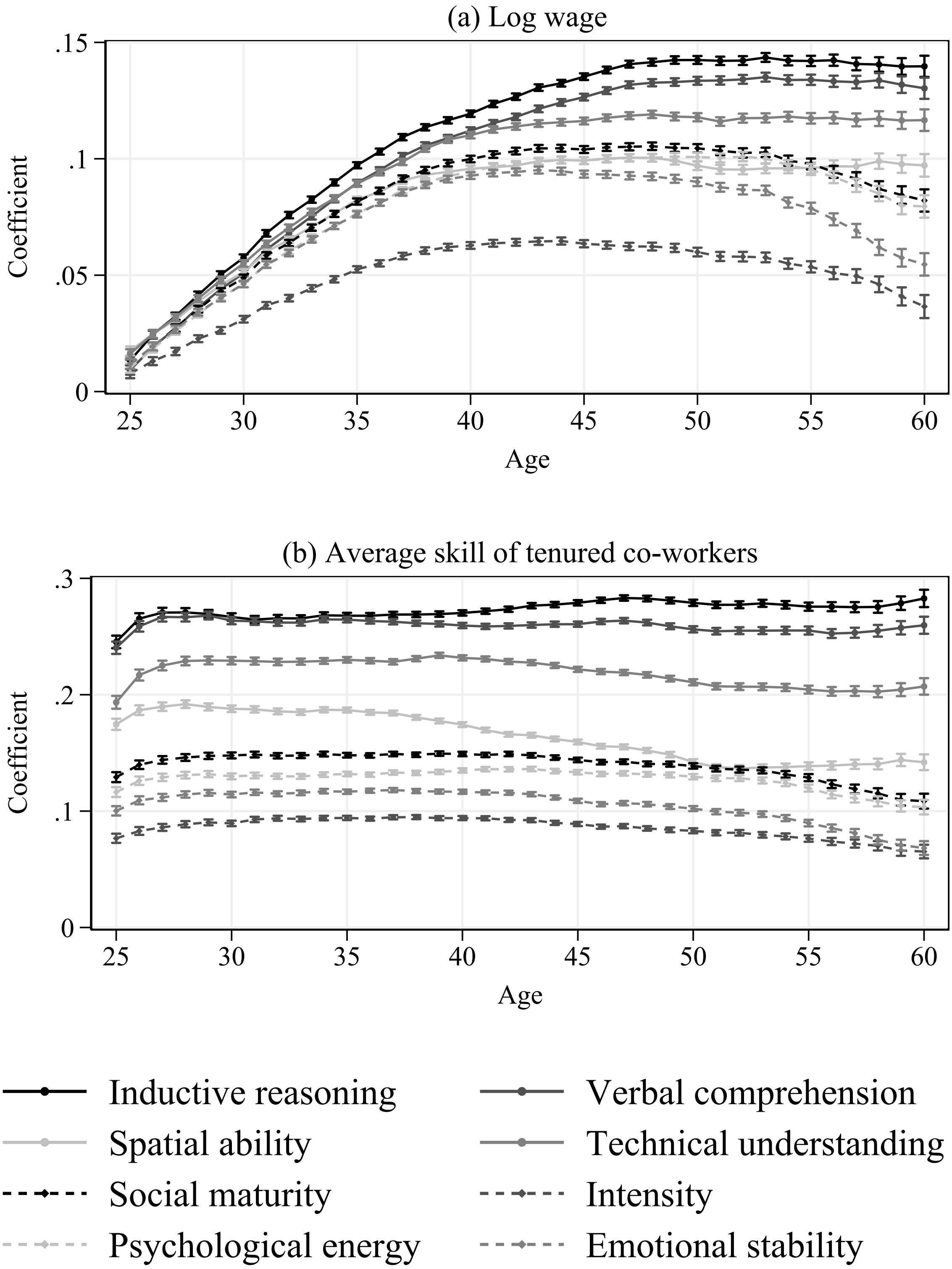
5 Empirical analysis

5.1 The average wage impact of match quality and unemployment

Before proceeding to the main analysis, we show how match quality and local unemployment affect wages and separations on average. The results are presented in Figure 3. The models control for skills and job fixed effects and observed human capital. In practice, we use a second-order polynomial in each of the eight skills, age, education and experience fixed effects. We further include year-specific job effects when analyzing wages (making local unemployment redundant) and time-constant job effects (also capturing fixed location characteristics) when analyzing local unemployment.

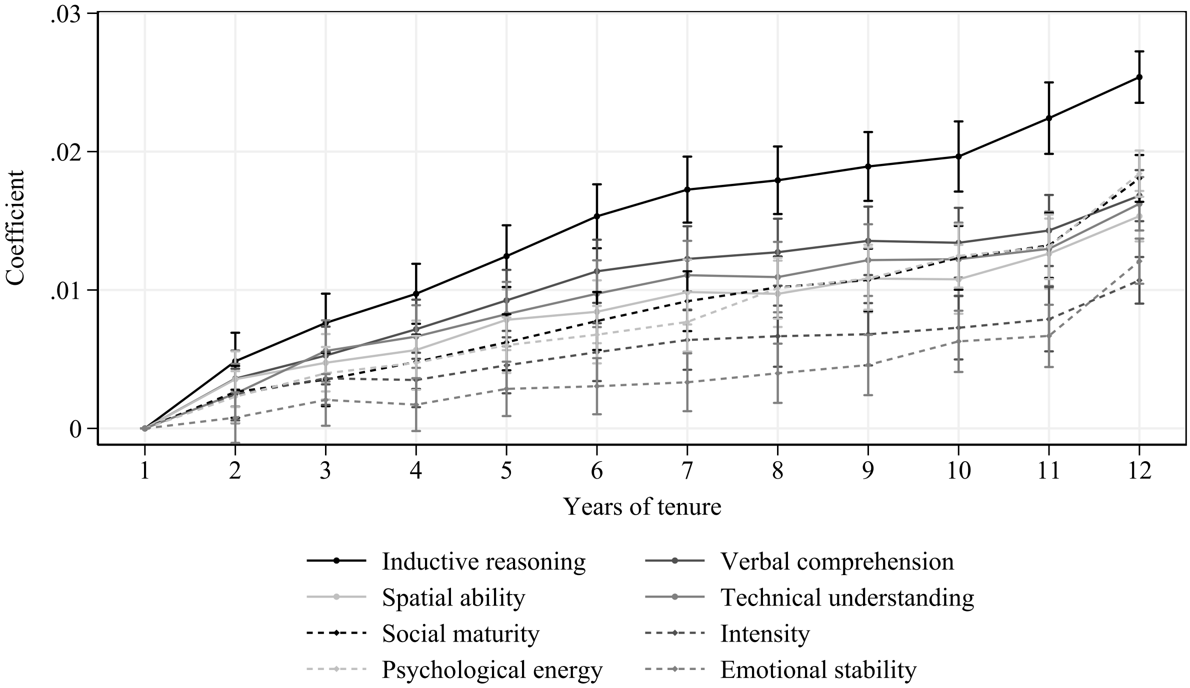
Panel A shows a positive monotonic relationship between match quality and wages. Moving from the 10th to the 90th percentile of the match-quality distribution is associated with approximately two percent higher wages. Panel B shows that separation rates are lower among well-matched workers. The separation probability, which is 10 percent on average, falls by 0.5 percentage points across the 90-10 percentile range of measured match quality. Considering that our fixed-effects specification isolates differences in wages and separation rates

Figure 1: Wage returns to skills and skill sorting, by age and skill dimension



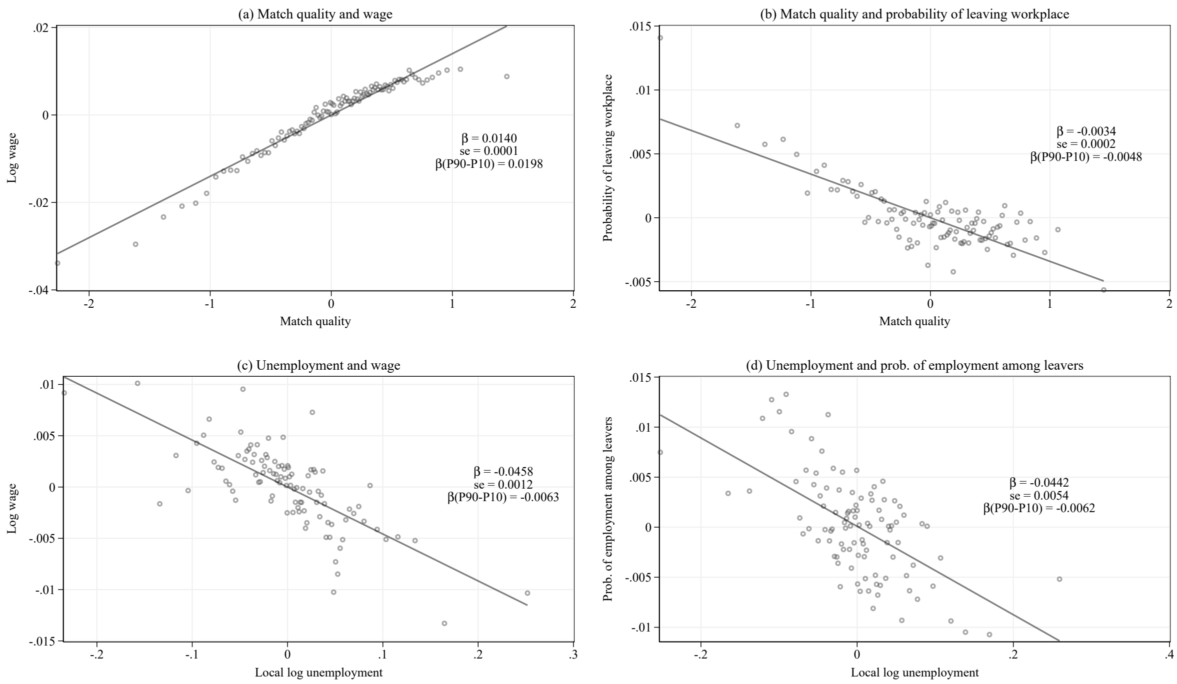
Notes: Panel (a) reports estimated coefficients and 95-percent confidence intervals obtained by regressing log wages on skills separately by age and all the eight skill metrics. Each separate regression also includes year fixed effects. Panel (b) reports the results from analogous regressions using the average skills (along the same dimension) of tenured co-workers as the outcome.

Figure 2: Match quality by skill dimension and tenure



Notes: The figure reports the coefficients obtained by regressing the negative of the absolute difference between the skills of individual i and average skills of his tenured co-workers in job j on tenure (winsorized at 12 years) separately for all eight skill dimensions. The reference category is new hires with tenure = 1. The regressions additionally control for job-by-year fixed effects, education and experience fixed effects, age (linearly) and, for each of the eight skills, a second-order polynomial for the skill level of individual i.

Figure 3: How wages and separations relate to match quality and unemployment



Notes: Panels (a) and (b) relate log wages and the probability of separating from the current workplace between t and $t+1$, respectively, to match quality. Panel (c) plots log wages against unemployment, while panel (d) relates the probability of being employed in $t+1$, for workers that left the workplace between t and $t+1$, to unemployment in $t+1$. We have residualized all variables with respect to a 2nd order polynomial in each of the eight included skills, age, as well as education and experience fixed effects (FEs). In addition, panels (a) and (b) control for job-by-year FEs; panels (c) and (d) hold job and year FEs constant.

across coworkers within very precise job (by year) categories, we interpret these magnitudes as both reasonable and economically meaningful.

Our baseline empirical approach uses local (municipal) unemployment as a proxy for the aggregate arrival rate of job offers. In Panels C and D, we document two well-known patterns related to local business cycles: both wages and job-to-job mobility rates are procyclical. Conditional on skills, match quality, and job fixed effects, both wages and job-to-job mobility vary by 0.6 percent across the local unemployment distribution (the 90-10 percentile). These patterns are consistent with the notion that employed workers receive more outside offers in years when the local unemployment rate is low.

5.2 Match quality in the previous job and the wages of job movers

The first key prediction from our theoretical framework is that workers who voluntarily move to a new job can use their previous job as an outside option to negotiate higher wages in the new job. Consequently, the wage in the new job should be a function of match quality in the previous job, as outlined in Prediction 1 of Section 2.

The framework in Section 2 incorporates both negotiations involving outside offers and "pure" rent sharing, whereby firms may offer a larger portion of the current surplus than is strictly necessary to outbid the previous employer. Consequently, the quality of the current (new) match should also influence starting wages. The difference between the effects of previous and current match quality, both measured in the same way, will indicate the relative importance of counteroffers versus "pure" sharing motives.

Previous match quality should only be relevant if it is credible that the worker can return to their old job if negotiations with the new employer fail. Therefore, we expect previous match quality to matter only for job-to-job movers, not for workers who change jobs after an intervening period of unemployment, as outlined in Prediction 2 of Section 2. Current match quality, however, should influence starting wages even if the worker was laid off from the previous job.

To test these predictions, we identify workers who switched to a new firm between $t-1$ and t. We require that they have no previous experience from the new workplace, and that we observe match quality and wages in both periods. The majority of these workers (54,314 individuals) switched jobs without experiencing unemployment, and thus made an Employment-to-Employment (EE) move, whereas the remainder experienced an interim spell of unemployment. We refer to the 4,239 workers who collected any unemployment-related income during t as EUE movers. Using these data, we estimate wage regressions with controls for current and previous match quality interacted with indicators for the type of transition (direct or through unemployment). We thus estimate the wage regression:

$$\begin{aligned} w_{i,j',j,t} =& \rho_t + \omega I_{i,t}^{EE} + \sum_{r \in \{EE,EUE\}} I_{i,t}^{r} \bigg(\kappa^r M_{i,j'} + \nu^r M_{i,j} \bigg) \\ &+ g^m(\boldsymbol{s}_i) + g^m_{j'}(\tilde{\boldsymbol{s}}_{j'}^{-i}) + g^m_{j}(\tilde{\boldsymbol{s}}_{j}^{-i}) + \boldsymbol{\psi}'\boldsymbol{x}_{it} + \varepsilon_{i,j',j,t} \end{aligned} \tag{10}$$

where i is an index for each worker and j' and j index the current and previous job, respectively. We let ρ_t indicate time dummies whereas $I_{i,t}^r$ denotes an indicator for belonging to transition type $r \in \{EE, EUE\}$. The parameter ω thus represents the average wage premium for job-to-job movers relative to those with an intermediate unemployment spell, while κ^r (ν^r) captures the importance of current (previous) match quality, indicated by $M_{i,j}$ ($M_{i,j'}$), separately for the two transition types. We let $h(\boldsymbol{s}_i)$ represent second-order polynomials in each of the eight skill measures. In addition, for each of these eight skill dimensions, we include a second-order polynomial in the average skill level of the *coworkers* in the current (i.e., new) job ($g^m_{j'}(\tilde{\boldsymbol{s}}_{j'}^{-i})$) and (separately) in the previous job ($g^m_{j}(\tilde{\boldsymbol{s}}_{j}^{-i})$). This is a parsimonious way of controlling for the features of the current and the previous jobs. In the sequel, we show that the main results are robust to using unrestricted fixed effects for the old and the new firm instead . The control set $\boldsymbol{x}_{i,t}$ includes a linear control for age, as well as fixed effects for experience and education, we restrict these background controls to have the same impact across the two transition types.

In addition to using log wages as the outcome variable—a standard practice in reduced-form empirical work—we also estimate equation (10) using wage levels, which aligns better with the standard theoretical formulation. To ensure comparability of the wage measure over time and to facilitate interpretation, we standardize wage levels (mean = 0, standard deviation = 1) separately by year.

The results from our preferred model are presented in Columns (1) and (3) of Table 2. The two columns use log wages and standardized wages, respectively. Columns (2) and (4) add controls for the wage in the previous job. Our preferred model indicates a positive and statistically significant effect of both current and previous match quality for the EE group. The estimates suggest that the impact of previous match quality on the current wage is indeed stronger than the impact of current match quality, which is consistent with counteroffers from the previous employer playing an important role in wage setting, in line with Prediction 1 in Section 2. Through the lens of the model, the positive estimate on current match quality reflects the "pure" sharing parameter η with an implied estimate of $\eta = 0.37$.[11] The estimates for standardized wages suggest a marginally different sharing parameter: $\eta = 0.41$.

[11] This is obtained as $\eta = 0.0101/(0.0101 + 0.0172)$, where the normalization takes into account that we only observe a proxy for match quality rather than actual match productivity.

Table 2: Wage regressions for job movers

	Log wage		Standardized wage	
	(1)	(2)	(3)	(4)
EE × prev. match quality	0.0172	0.00401	0.0537	0.0141
	(0.00202)	(0.00131)	(0.00639)	(0.00424)
EE × new match quality	0.0101	-0.000115	0.0379	0.00558
	(0.00207)	(0.00134)	(0.00658)	(0.00431)
EUE × prev. match quality	0.00405	-0.00457	0.0215	0.00104
	(0.00487)	(0.00386)	(0.0116)	(0.00753)
EUE × new match quality	0.0195	0.00865	0.0633	0.0232
	(0.00490)	(0.00385)	(0.0117)	(0.00745)
EE × previous wage		0.774		0.752
		(0.00430)		(0.0190)
EUE × previous wage		0.516		0.324
		(0.0120)		(0.0218)
EE mover	0.129	0.122	0.260	0.346
	(0.00333)	(0.00419)	(0.00791)	(0.0137)
N	58,553	58,553	58,553	58,553
R^2	0.616	0.848	0.367	0.741

Notes: "EE movers" are workers who changed firms between $t-1$ and t without an intermittent spell of unemployment; "EUE movers" are workers who changed firms between $t-1$ and t with an intermittent spell of unemployment. A worker is defined as having experienced unemployment if he or she received some unemployment-related payments during t. We also require that we observe wages and match quality in the current and previous job. All regressions include controls for education and experience fixed effects, a linear control for age, and a second-order polynomial for each of the eight skills. In addition, we control for a second-order polynomial for the average in each skills among tenured workers in the previous as well as current job. The sample includes 54,350 EE movers, and 4,243 EUE movers. Robust standard errors in parentheses.

The impact of previous match quality may, in principle, be mediated by the previous wage. To assess the role of previous match quality conditional on previous wages, Columns (2) and (4) present results from an augmented version of equation (10) that adds a control for the wage in the previous job. Including previous wages in the specifications naturally reduces the estimated impact of match quality, as wages are a function of match productivity. Nevertheless, previous match quality still has a significant impact on wages for EE movers. A natural interpretation is that match quality, conditional on wages, reflects the potential for further increasing the wage within the (old) job.

The third and fourth rows of the table show estimates for workers with an interim unemployment spell (EUE movers). For this group, previous match quality appears irrelevant for current wages, in line with Prediction 2 in Section 2—the point estimates are statistically insignificant and very close to zero, in particular when using log wages as the outcome (see Column 1). By contrast, current match quality has a positive and statistically significant impact on current wages for EUE movers across all columns, suggesting that current rents are shared through η for this group as well. The fact that the point estimates for current match quality are larger than for previous match quality in the EUE group stands in sharp contrast to the reverse patterns we find for the EE group. As the EUE sample is smaller, and we are testing (and not rejecting) the prediction of a null effect from previous match quality, it is particularly reassuring that we have sufficient statistical power to estimate a highly significant effect for *current* match quality in all columns.

Finally, it is worth noting that the estimates for previous wages in Columns (2) and (4) are considerably smaller for EUE movers (0.516 for log wages) than for EE movers (0.774 for log wages). This is also in line with the view that the previous job does not represent an outside option for those with intermittent unemployment spells. The final row of the table shows that job-to-job movers have substantially higher wages in their new job compared to movers with interim unemployment.

5.2.1 Robustness

Table 3 provides a number of robustness checks for the results on job movers. First, we weight the original regression using the product of the inverse sampling weight from the previous and current year (winsorized at the 99th percentile). Second, we introduce fixed effects for both the previous and new firm to account for one-dimensional bargaining effects discussed in Di Addario et al. (2023). Third, we take this one step further and introduce previous and new *job* fixed effects. We then rely on variation stemming from multiple workers leaving the same job with different destinations and multiple workers entering the same job from multiple sources. In the model with job fixed effects, around one third of our sample is in effect lost due to lack of variation.

As before, we estimate regressions using log wages—see columns (1)-(3)—and standardized wages—see columns (4)-(6) of Table 3. The results are well in line with those in Table 2: For EE movers, both new and previous match quality is important for wages, and the estimate for previous match quality is larger than that for current match quality throughout the specifications. For EUE movers, previous match quality plays a minor role for wages, while the estimate for current match quality is sizable and larger than the estimate for EE movers.

Table A.2 in the appendix reports estimates from regressions estimated separately for EE-movers and EUE-movers. The impact of current match quality is a bit weaker for EUE-movers when the wage regressions are estimated separately than in the pooled wage regression. However, given the small sample of EUE movers, we place greater emphasis on the pooled regression results.

5.3 Outside offers and the sharing of rents with remaining workers

Since the rate of outside offers is pro-cyclical, the use of outside counteroffers in wage bargaining can have important consequences for the cyclical variation of wages among workers who remain in their jobs. This section therefore examines this process through the lens of Prediction 3 in Section 2, which posits that the returns to match quality among workers who remain on the job should be positively related to the arrival rate of outside offers. To test this, we examine how the wage returns to measured match quality vary with local unemployment and other proxies for the arrival rate of outside offers. Consistent with the theoretical framework, this part of our analysis includes only workplace stayers (i.e., those with at least one year of tenure).

We regress wages (w) on match quality (M), local unemployment (u), and their interaction, controlling for job fixed effects and the direct impact of skills. More specifically, we estimate different versions of the following model:

$$w_{i,j,l,t} = \alpha_{j,t} + \phi u_{l,t} + \delta M_{i,j} + \mu \left(u_{l,t} \times M_{i,j}\right) + g(\boldsymbol{s}_i) + u_{l,t} \times h(\boldsymbol{s}_i) + \boldsymbol{\gamma}' \boldsymbol{x}_{i,t} + \varepsilon_{i,j,l,t} \quad (11)$$

where indexes i, j, l, and t denote individual, job, local labor market (municipality), and year, respectively. We include job-by-year fixed effects ($\alpha_{j,t}$) in the tightest specification, which

Table 3: Robustness checks for job movers

	Log wage			Standardized wage		
	(1)	(2)	(3)	(4)	(5)	(6)
EE $\times$ prev. match quality	0.0149	0.0111	0.0102	0.0556	0.0384	0.0262
	(0.00313)	(0.00180)	(0.00252)	(0.0113)	(0.00598)	(0.00967)
EE $\times$ new match quality	0.0116	0.00574	0.00508	0.0428	0.0251	0.0192
	(0.00316)	(0.00196)	(0.00260)	(0.0105)	(0.00687)	(0.00961)
EUE $\times$ prev. match quality	-0.00912	0.00371	0.00345	-0.00005	0.0153	0.00964
	(0.00764)	(0.00470)	(0.00775)	(0.0186)	(0.0122)	(0.0210)
EUE $\times$ new match quality	0.0394	0.0111	0.00913	0.111	0.0471	0.0296
	(0.00722)	(0.00484)	(0.00742)	(0.0173)	(0.0124)	(0.0197)
EE mover	0.119	0.0676	0.0597	0.230	0.123	0.123
	(0.00514)	(0.00351)	(0.00498)	(0.0116)	(0.00889)	(0.0133)
Survey weights	✓			✓		
Prev. and new firm FE		✓			✓	
Prev. and new job FE			✓			✓
N	58,553	55,964	38,543	58,553	55,964	38,543
R^2	0.638	0.778	0.890	0.404	0.574	0.740

Notes: The table reports the results from wage regressions for workers that moved between workplaces and firms between $t-1$ and t. "EE movers" are workers who changed firms between $t-1$ and t without an intermittent spell of unemployment; "EUE movers" are workers who changed firms between $t-1$ and t with an intermittent spell of unemployment. A worker is defined as having experienced unemployment if he or she received some unemployment-related payments during t. All regressions include controls for education and experience fixed effects, a linear control for age, a second-order polynomial for each of the eight skills and skills squared, and a second-order polynomial for the average of all skills among tenured workers in the previous as well as new job. "survey weights" refer to weighting the regressions by the product of the inverse probability weights from the previous and current year (winsorized at the 99th percentile). "firm fixed effects" and "job fixed effects" refer to incorporating both origin and destination firm and job fixed effects, respectively. Whenever job fixed effects are incorporated, the average skill controls are rendered superfluous. The sample is conditional on observing wages and match quality in the last job and current job. It includes 54,350 EE movers, and 4,243 EUE movers. Columns (1) and (4) report robust standard errors. (2) and (5) report standard errors clustered at the current firm level. (3) and (6) report standard errors clustered at the current firm level

.

implies that we identify the return to match quality solely from variation across co-workers within the same job and year. The terms $g(\boldsymbol{s}_i)$ and $h(\boldsymbol{s}_i)$ represent second-order polynomials in each of the eight skill measures. We control flexibly for skills to account for changes in market-wide returns to skills (through $g(\boldsymbol{s}_i)$) *and* business cycle variation in skill returns through the interaction $u_{l,t} \times h(\boldsymbol{s}_i)$). Finally, $\boldsymbol{x}_{i,t}$ includes a linear control for age, as well as fixed effects for experience and education.

Prediction 3, refers to the interaction parameter μ. Since unemployment is negatively related to the aggregate offer rate, a negative estimate suggests that workers receive a larger share of the surplus from match quality when the arrival rate of job offers is high (i.e., when unemployment is low). To facilitate interpretation, we demean unemployment, allowing the estimates for δ and ϕ to be interpreted as the average effects of match quality and unemployment, respectively.[12] We estimate the model separately for log wages and (standardized) wage levels.

Table 4 reports the main results based on equation (11). The first three columns use log wages as the outcome, while the subsequent three columns use standardized wage levels instead. Columns (1) and (4) include separate job and year fixed effects, allowing us to also identify the main effect of unemployment. Consistent with previous findings, wages are, on average, negatively related to unemployment and positively related to match quality. Columns (2) and (5) incorporate job-year interactions, which remove the impact of local unemployment. Columns (3) and (6) further include interactions between match quality and municipality, as well as match quality and year.

[12]Note that ϕ is not identified when job-by-year fixed effects are included in the regressions.

Table 4: Wage regressions for job stayers

	Log wage			Standardized wage		
	(1)	(2)	(3)	(4)	(5)	(6)
Match quality × ln(unemployment)	-0.00534	-0.00264	-0.00391	-0.00999	-0.0216	-0.0331
	(0.000487)	(0.000495)	(0.00112)	(0.00193)	(0.00194)	(0.00377)
Match quality (M)	0.0133	0.0137		0.0471	0.0441	
	(0.000136)	(0.000135)		(0.000544)	(0.000531)	
ln(unemployment)	-0.0455			-0.0580		
	(0.00123)			(0.00447)		
Year fixed effects	✓			✓		
Job fixed effects	✓			✓		
Job × year fixed effects		✓	✓		✓	✓
M × municipality interactions			✓			✓
M × year interactions			✓			✓
N	3,938,251	3,938,251	3,938,251	3,938,251	3,938,251	3,938,251
R^2	0.806	0.834	0.834	0.604	0.661	0.661

Notes: The table reports versions of the regression model in equation (11). In addition to the control variables listed in the table, all regressions control for education and experience fixed effects as well as age (linearly) and, for each of the eight skills, a second-order polynomial for the skill level of individual i. All models also include interactions between each of the individual skill polynomials and unemployment. Standard errors are heteroskedasticity-robust.

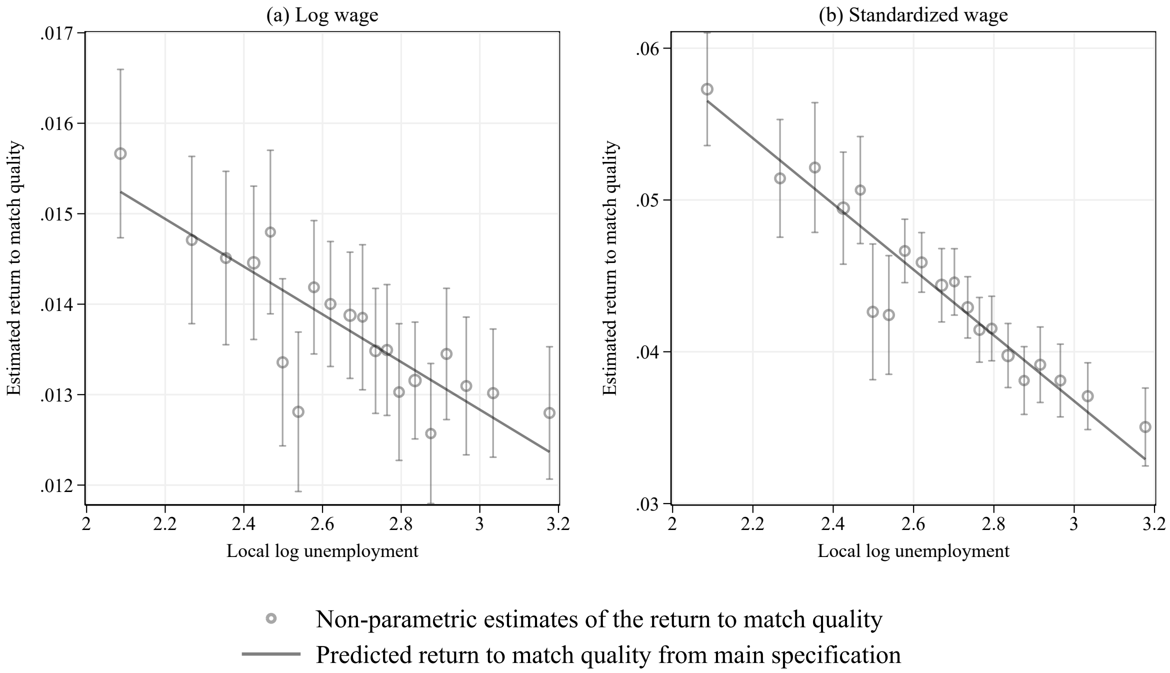
The main takeaway from the table is that the interaction terms between match quality and local unemployment are consistently negative and precisely estimated. This implies that as local unemployment falls, the returns to a good match grows. Our preferred specification, used in Columns (2) and (5), includes job-year fixed effects. This specification fully accounts for all job-specific factors that may cause some jobs to be more sensitive to the business cycle than others. Notice that all our models also include flexible interactions between skills and unemployment; thus we account for any confounding impact due to cyclical variation in the returns to skills.

A possible remaining concern is that the interaction between match quality and unemployment picks up national trends, or time-invariant differences across municipalities, in the returns to match quality that are not caused by unemployment. To address such issues, we interact match quality with year fixed effects, and with municipality fixed effects. Results from this specification are presented in Columns (3) and (6). The added interactions imply that the model only relies on variation within municipalities over time when identifying μ. Reassuringly, the estimates on the interaction terms are fully robust to this amendment. If anything, the coefficients on the interaction terms are larger in absolute value.

Qualitatively, it does not matter whether we use log wages or standardized wages, which is reassuring as it suggests that our conclusions are independent of the choice of functional form. To facilitate comparison across outcome measures, it is useful to relate the interaction estimates to the average impact of match quality. Generally, the interaction terms are more important in relative terms when using standardized wages in columns (4)-(6) than when using log wages in columns (1)-(3).

Overall, the magnitudes of the interaction estimates are non-trivial. For ease of interpretation, the solid line in Figure 4 (panel a), plots the return to match quality along the distribution of local unemployment using the specification in column (2). The estimates imply that the return to match quality rises by 0.275% if unemployment falls from the top to the bottom end of the distribution. This increase corresponds to 20 percent ($\approx 0.275/1.37$) of the average return to a standard deviation increase in match quality. Figure 4 (panel b) relies on the standardized wages instead, using the specification in column (5). In this case, the implied range of estimates across the unemployment distribution is equivalent to 2.3 percentage points, which corresponds to 52 percent of the average estimated returns ($\approx 2.3/4.41$).

Figure 4: The return to match quality along the unemployment distribution



Notes: Non-parametric estimates are obtained by grouping data into 20 bins based on the ventiles of the distribution of $u_{j,t}$. The return to match quality is then estimated using a version of equation (11) which interacts match quality with the indicators for each bin. Panel (a) shows the estimates for log wages while (b) presents the same information for wage levels standardized (mean = 0, standard deviation = 1) separately by year. The 95-percent confidence intervals are based on robust standard errors. The solid lines show the predicted return to match quality based on the estimates in columns (2) and (5) from Table 4.

Figure 4 also provides non-parametric estimates of the return to match quality.[13] The non-parametric estimates closely align with the predicted returns from the main specification.

5.3.1 Robustness

To assess the robustness of our results, we produce a battery of checks: we use alternative definitions of local labor markets, employ different proxies for the arrival rate of offers, such as local employment instrumented by a shift-share instrument, occupation-specific local employment, and labor market tightness. We also examine the relationship between the return to match quality and average unemployment during workers' tenure spells, assign equal weight to each municipality regardless of size, weight the sampled parts of the data by the inverse sampling weight, incorporate individual $\times$ job fixed effects in our main specification to control for time-invariant match-specific unobservables, and vary the calculation of standard errors. Our results remain robust across all these alternative specifications. The results are summarized in Figure 5 and details are presented in table format in the Appendix.

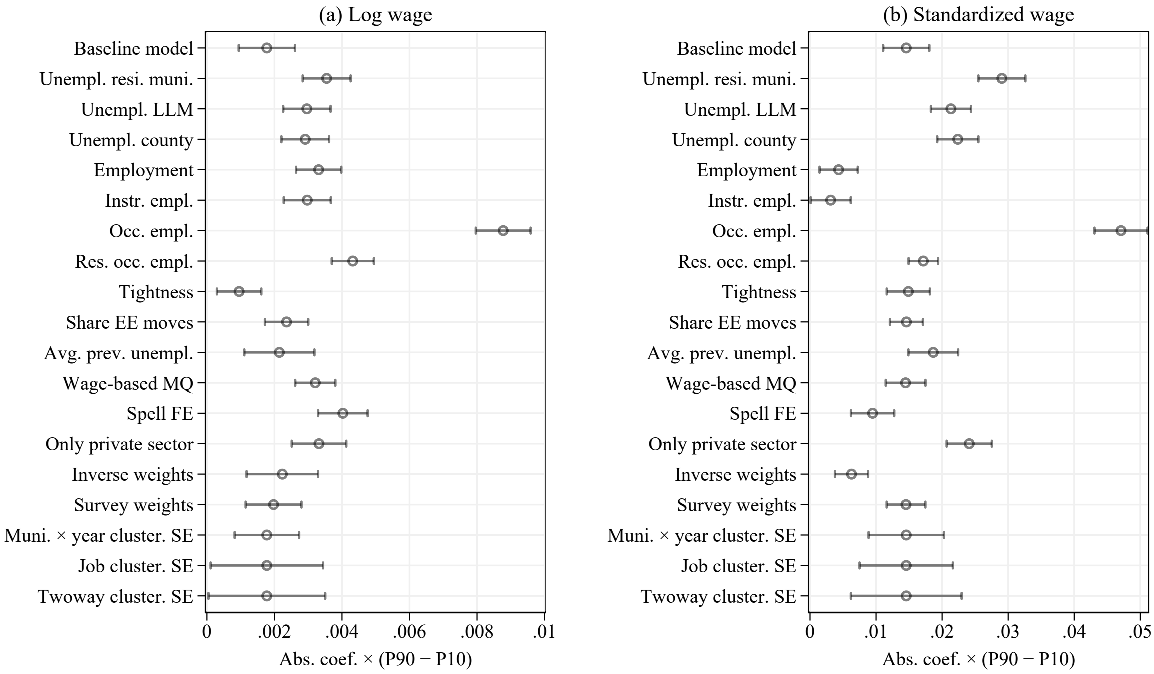
Alternative definitions of local labor markets. The main analysis utilizes unemployment in the municipality where the job is located. Measuring unemployment at a more aggregate level (commuting zone/local labor market or county) does not alter our conclusions; if anything, the heterogeneity in returns to match quality with respect to unemployment becomes larger. Moreover, using the *residential* (instead of workplace) municipality of each worker, which may vary within each job $\times$ year cell, yields very similar results.[14]

Actual and instrumented municipality employment. Substituting unemployment for the logarithm of the number of employed workers in the workplace municipality provide very similar results as the main specification. As a plausibly exogenous measure of local labor market conditions, we also instrument log employment by a shift-share/Bartik (1991) style instrument. The instrument is based on the national deviation in industry s and year t from the industry average logarithm of employment. The employment in each municipality and year is then predicted based on the local industry composition in 1997 using:

[13]These non-parametric estimates are obtained by grouping the data into 20 bins based on the ventiles of the unemployment distribution. The return to match quality is then estimated using a modified version of equation (11), where match quality is interacted with dummies for each unemployment ventile instead of interacting it directly with unemployment.

[14]The main effect of residential municipality unemployment on wages (not reported) is also both statistically and economically significant.

Figure 5: Robustness checks for job stayers



Notes: The figure reports the absolute value of the predicted difference in the effect of match quality between the 90th and 10th percentile of the outside option distribution. See the detailed description of all models in connection to Table A.1 in Appendix. All regressions include the same controls as the model reported in columns (2) and (5) in Table 4. See the description of the robustness checks in the main text.

$$\ln(\text{employment})^{Instr.}_{l,t} = \sum_{s} \text{Share}_{l,s,1997} \times \left(\ln(\text{employment})_{s,t} - \overline{\ln(\text{employment})}_{s}\right) \quad (12)$$

where $\text{Share}_{l,s,1997}$ is the fraction of all workers in municipality l in 1997 that were employed in industry s. We use the highest level of aggregation in the Swedish Standard Industrial Classification, which includes 21 different industry categories.

Regressing standardized local employment, $\ln(\text{employment})_{l,t} - \overline{\ln(\text{employment})}_{l}$, onto the instrumented standardized log employment using 4913 observations at the municipality-year level returns a coefficient of 0.9 with a t-value of 64, indicating that the first stage equation is sufficiently precise. We study the direct impact of the shift-share instrument by inserting it into equation (11) instead of unemployment. Qualitatively, the results are robust to using log employment as the proxy for the arrival rate of offers.

Occupation-specific local employment. When using employment instead of unemployment it is also possible to compute the measure separately for each occupation. Occupation-specific local employment is plausibly a more relevant measure of available jobs and the frequency of outside offers. We compute such a measure using 3-digit occupations to be consistent with how we define *jobs*. We use the logarithm of the number of employees in each occupation $\times$ municipality $\times$ year cell. We also residualize this measure by running a regression at the same level of observation which incorporates municipality $\times$ year and occupation $\times$ year fixed effects, which implies that the variation in the residuals will stem from the deviation from the national occupation-specific employment and local overall employment in a given year. Both variants are reported in Figure 5, and in both cases the return to match quality is increasing in occupation-specific employment. Moreover, there is more heterogeneity in the return to match quality across the occupation-municipality log employment distribution compared to local unemployment.

Vacancies relative to unemployment. From a theoretical viewpoint, labor market *tightness* may be a more appealing measure than unemployment. From an empirical perspective it is more complicated because vacancy counts are notoriously poorly measured and trends in vacancy posting behavior may distort the measurement of available jobs in the local economy. To compute tightness, we use the inflow of posted vacancies at the Swedish Public Employment Service job portal *Platsbanken* divided the number of unemployed job seekers.

We standardize the measure to mean zero, standard deviation one. The estimates concur with our baseline findings: the return to match quality increases in tightness.

Employment-to-employment movers. Our final alternative proxy of the arrival rate of outside offers draws on Moscarini and Postel-Vinay (2017) by using E-to-E movers. We compute the share of workers that separate from their initial workplace between $t-1$ and t that are categorized as employed in t. The share is calculated at the workplace municipality in $t-1$ and year level, and workers are allowed to transition between municipalities.[15] Figure 5 shows that the results only change marginally with this alternative proxy.

Unemployment during the tenure spell. A reasonable conjecture is that the full history of market conditions during a tenure spell should affect the returns to match quality. To assess this conjecture, we have calculated average unemployment during each worker's tenure spell, winsorized at 15 years. Unfortunately, municipality-level data on unemployment is only available from 1997, and so we're forced to exclude around half of all observations, especially those with long tenure and the early years of our sample.[16] Reassuringly, our conclusions are basically unaffected by using average unemployment during the tenure spell.

An alternative match quality proxy. To see whether our results are sensitive to how we define match quality, we have constructed an alternative proxy based on the wage returns to abilities within each job compared to the market returns for the same abilities. The idea is that the job-specific returns are informative about the importance of each ability in each job. One disadvantage of this method is the noisiness of the estimates for small job cells. To construct the measure, we first estimate a regression of the following form, for each job j:

$$w_{i,j,t} = \alpha_{j,t} + \boldsymbol{\beta'}_j^{\text{Job}} \boldsymbol{s}_i + \varepsilon_{i,j,t} \tag{13}$$

where $\alpha_{j,t}$ is a set of job-times year fixed effects, the vector $\boldsymbol{s}_i$ comprises the eight skill measures, and the vector $\boldsymbol{\beta'}_j^{\text{Job}}$ contains the returns to each skill for job j. We then estimate the same regression for all workers simultaneously, treating the whole market as a single job, to obtain market-wide returns, $\boldsymbol{\beta'}^{\text{Market}}$.

[15] This is equivalent to the measure used in panel (d) in Figure 3.

[16] To be able to compare these estimates to the baseline estimates, we have also re-estimated our main specification for this subset of observations. The interaction estimates for the log wage and the standardized wage equal -0.00177 and -0.0256, respectively; these estimates are similar to those obtained in our baseline specification (-0.00264 and -0.0216, respectively).

Our wage-based match quality metric, $M_{i,j}^{\text{wage}}$, is calculated as follows. To remove outliers, we exclude observations below the 1st and above the 99th percentile of the $M_{i,j}^{\text{wage}}$ distribution. We standardize the measure to mean zero and standard deviation one to make it comparable to our baseline match quality proxy:

$$M_{i,j}^{\text{wage}} = \frac{\left(\hat{\boldsymbol{\beta}'}_j^{\text{Job}} - \hat{\boldsymbol{\beta}'}^{\text{Market}}\right)\boldsymbol{s}_i - \text{mean}\left(\left(\hat{\boldsymbol{\beta}'}_j^{\text{Job}} - \hat{\boldsymbol{\beta}'}^{\text{Market}}\right)\boldsymbol{s}_i\right)}{\text{sd}\left(\left(\hat{\boldsymbol{\beta}'}_j^{\text{Job}} - \hat{\boldsymbol{\beta}'}^{\text{Market}}\right)\boldsymbol{s}_i\right)} \tag{14}$$

The results using the alternative measure of match quality is presented in Figure 5. They are very similar to those of our main specification, which mimics the conclusions in Fredriksson, Hensvik, and Skans (2018) using a similar robustness test in the context of their study.

Spell fixed effects. We add individual $\times$ job fixed effects on top of the job $\times$ year fixed effects to remove any constant wage differences between individuals and jobs. We thus rely on within spell variation in wages and local unemployment across time to identify the interaction between match quality and unemployment. The resulting interaction estimates for log wages are somewhat larger, while the estimate for standardized wages are somewhat smaller relative to the preferred specifications.[17]

Private sector. We have also examined whether the results are different in the private sector compared with the overall labor market. Qualitatively the results are robust, but our results suggest that wage renegotiation using outside offers is more prevalent in the private sector compared to the public sector.

Alternative weights and standard errors. To verify that our results are not driven solely by large municipalities (e.g., Stockholm), and hold also in smaller settings, we have weighted the regressions with the inverse of the number of observations in each municipality $\times$ year cell, thus putting equal weight on each cell. The interaction effect remains statistically significant and similar in size. To verify that the results are not affected by under-sampling of small firms, we have also re-estimated the models using the sampling weights in the spirit of how Statistics Sweden computes their aggregate statistics. The results are robust to using these weights. The main analyses employ heteroskedasticity-robust standard errors since the variable of interest varies at the individual level. When we cluster the standard errors at the

[17] In unreported regressions, we have also explored variations of this model such as (global) individual fixed effects and job $\times$ year fixed effects as well as only individual $\times$ job fixed effects, and the results are robust.

municipality-year level, the job level, or use two-way clustering based on both, the estimates of interest remain significant at the 5-percent level as seen at the bottom of Figure 5.

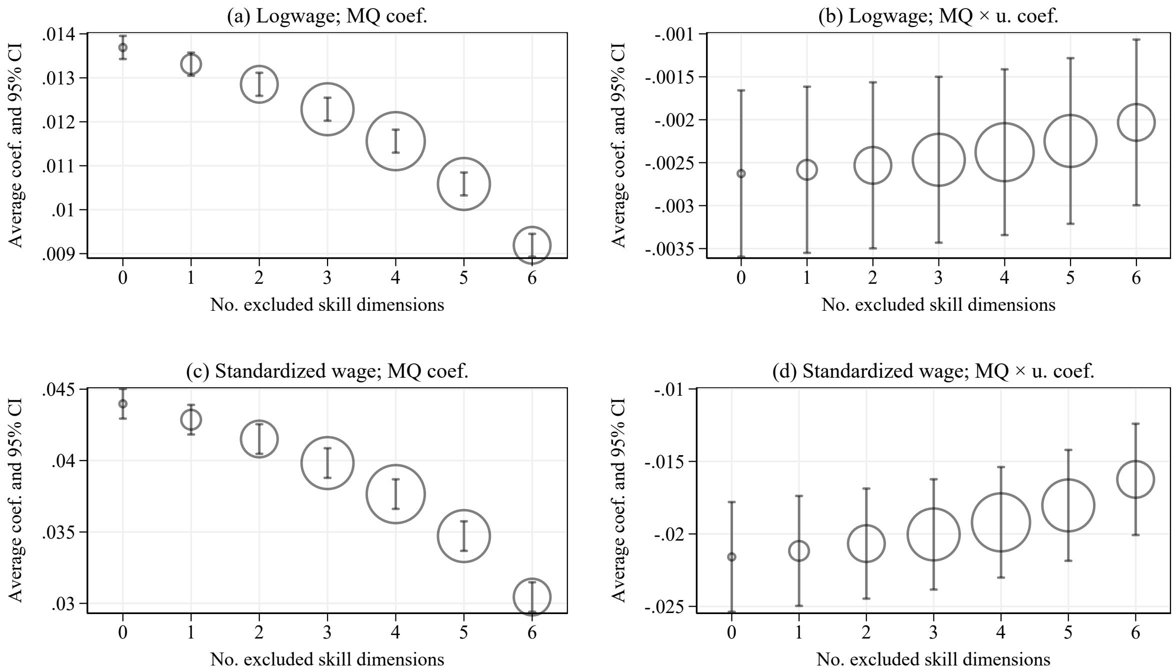
Measurement errors. Finally, we make an effort to assess how measurement errors in match quality would affect our conclusions. Although our vector of skills is unusually rich compared to most data sources, it is obvious that workers can have skills in other (unmeasured) dimensions that may matter as well. To assess how sensitive our estimates are to lack of data on other skills, we have explored how our key estimates change if we *reduce* the dimensionality of our skill vector. The idea is that when we reduce the number of skill dimensions, we introduce additional errors. We provide separate estimates where we gradually reduce the dimensions by sequentially removing (and thus ignoring) 1-6 of the observed skill dimensions. The results, presented in Figure 6, show that the qualitative conclusions are robust, and that estimates are increasingly attenuated when we reduce the amount of information contained in the skill vector.

5.4 Heterogeneity across occupational categories

Rent sharing may vary across different segments of the labor market, as the extent of individualistic wage bargaining and bargaining power can differ by segment. To examine how the effects of interest vary across occupational categories, we estimate versions of equations (10) and (11) where the coefficients on match quality, and the interaction of match quality with local unemployment, are allowed to vary across different occupational categories. We consider six occupational groupings: (i) managers and professionals, (ii) technicians, (iii) services/administration, (iv) crafts, (v) machine operators, and (vi) other (elementary occupation as well as agriculture, forestry, and fishing). We use the standardized wage as the outcome.

The left-hand panel of Figure 7 pertains to stayers. It shows estimates from our baseline approach corresponding to Column (5) of Table 4; more precisely, it plots estimates of the interaction between match quality and local unemployment across occupational categories. The estimates indicate that effects are present across the occupational distribution. The effects are somewhat larger for managers and professionals (this is mostly driven by managers), which may be groups that are best able to use outside offers to renegotiate their wage with their current employers. Due to lack of power, it is in general difficult to pin down statistically significant differences across occupations. While we can reject the hypothesis that the

Figure 6: Wage regressions for job stayers using match quality constructed from skill subsets



Notes: The figure plots the average coefficients and 95-percent confidence intervals of the main effect of match quality and the interaction effect between match quality and unemployment on wages from regressions using alternative match quality metrics based on subsets of the eight skill measures. The horizontal axis shows the number of skill measures excluded when constructing the match quality metric. We estimate regressions for all combinations of skill subsets, conditional on the number of excluded skills. The size of each marker is governed by the number of estimated models that the average is based on. In all other regards, the estimated models are identical to those reported in columns (2) and (5) in Table 4.

coefficient for managers and professionals is different from the one for crafts, other contrasts are not significantly different from one another.[18]

The right-hand panel of Figure 7 pertains to EE-movers. In particular, it plots the estimated coefficient on match quality in the previous job—the relevant outside option for this group of workers—from a regression corresponding to Column (3) of Table 2across occupational categories. Again, the estimates suggest that effects are present across the occupational distribution, while being slightly larger for managers and professionals.

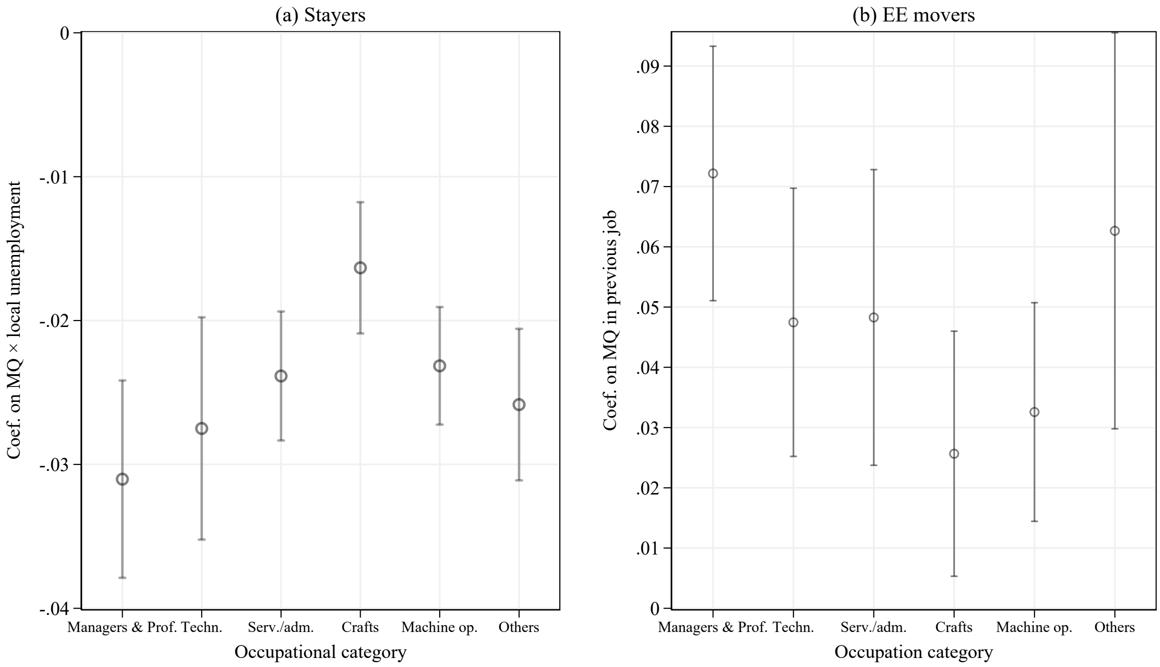
Overall, Figure 7 suggests that all occupational groups can use outside options to bid up current wages. The impact of outside options is somewhat stronger among managers and professionals than among other groups. At a general level, these patterns thus align well with the findings of Caldwell, Haegele, and Heining (2025), who demonstrate that firms are more inclined to negotiate with managers than with other workers in their German survey data on firm bargaining strategies, and with Di Addario et al. (2023), who find that the previous firm has a bigger impact on current wages in occupational layers where individual bargaining is a more prevalent wage-setting mode using Italian register data.

6 Conclusions

We offer two pieces of evidence demonstrating that workers use outside offers to extract rents from match productivity. Using a measure of match quality based on the relationship between workers' multidimensional abilities and the skill requirements of their jobs, we show that: (i) wages of job movers are positively related to match quality in the previous job, even when controlling for the previous wage, while wages of workers who are hired after an unemployment spell are unrelated to the match quality in the last job; (ii) wages within ongoing matches are more closely aligned with match quality following an improvement of local labor market conditions. These findings run counter to the implications of the standard wage bargaining framework as well as the typical reduced-form rent-sharing set-up, both of which assume that the impacts of idiosyncratic rents and outside options are additively separable in the wage equation.

[18] A concern with these estimates may be that local unemployment is differentially informative about outside offers across occupational categories. Therefore, we have also used occupation-specific employment at the local level as an alternative proxy for outside offers. The estimates from this alternative approach reveal a similar pattern as in panel (a) of Figure 7

Figure 7: Wages and outside options by occupational category



Notes: The left-hand panel shows the estimate on the interaction between match quality and log local unemployment across occupational categories for stayers; the regression specification corresponds to equation (11) with job-by-year fixed effects. The right-hand panel shows the estimate on previous match quality across occupational categories for job-to-job movers. The regression specification corresponds to (10) when including previous and new match quality. Both variables, as well as the indicator for being an EE rather than an ENE mover, are interacted with occupational category. Bars are 95-percent confidence intervals based on robust standard errors.

Our results are not sensitive to functional form; they are robust to alternative measures of local labor market conditions, and do not depend of how we define the local labor market. The effects are significant across the occupational distribution, with a tendency for larger effects towards the higher end of the occupational distribution.

Our findings have clear implications for the interpretation of within-firm wage inequality. When firms are willing to make counteroffers, the wage returns to tenure can be unrelated to the accumulation of firm-specific productivity. More broadly, wages vary among equally productive workers performing the same tasks, weakening the link between within-firm wage ranks and individual productivity.

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Appendix A Additional robustness checks

Table A.1: Robustness checks for job stayers

	Log wage		Standardized wage	
	Coef. (SE)	Coef.×(P90 - P10) [P90 - P10]	Coef. (SE)	Coef.×(P90 - P10) [P90 - P10]
	(1)	(2)	(3)	(4)
Alternative local labor markets				
Residential municipality	-0.00476	-0.00355	-0.03899	-0.02905
	(0.00049)	[0.74497]	(0.00243)	[0.74497]
Workplace local labor market	-0.00458	-0.00296	-0.03304	-0.02134
	(0.00055)	[0.64581]	(0.00239)	[0.64581]
Workplace county	-0.00474	-0.00291	-0.03641	-0.02238
	(0.00059)	[0.61468]	(0.00258)	[0.61468]
Alternative proxies of expected outside option				
$\ln(\text{employment})_{l,t}$-$\overline{\ln(\text{employment})}_l$	0.01968	0.00331	0.02580	0.00434
	(0.00203)	[0.16817]	(0.00879)	[0.16817]
Instrumented $\ln(\text{employment})_{l,t}$	0.02299	0.00299	0.02487	0.00324
	(0.00275)	[0.13012]	(0.01201)	[0.13012]
Occupation-specific ln(employment)	0.00195	0.00878	0.01046	0.04709
	(0.00009)	[4.50102]	(0.00046)	[4.50102]
Residualized occ. ln(employment)	0.00168	0.00432	0.00666	0.01716
	(0.00012)	[2.57626]	(0.00044)	[2.57626]
# New $\text{vacancies}_{l,t}$/# $\text{Unemployed}_{l,t}$	0.00047	0.00095	0.00729	0.01489
	(0.00016)	[2.04304]	(0.00082)	[2.04304]
Share of separations to employment	0.01844	0.00236	0.11412	0.01460
	(0.00256)	[0.12793]	(0.00995)	[0.12793]
Mean local unemployment	-0.00331	-0.00214	-0.02881	-0.01866
	(0.00082)	[0.64756]	(0.00297)	[0.64756]
Alternative specifications				
Wage-based match quality	-0.00477	-0.00321	-0.02150	-0.01447
	(0.00045)	[0.67315]	(0.00228)	[0.67315]
Individual × job and job × year FE	-0.00598	-0.00403	-0.01408	-0.00948
	(0.00056)	[0.67315]	(0.00248)	[0.67315]
Only private sector	-0.00493	-0.00332	-0.03582	-0.02411
	(0.00061)	[0.67315]	(0.00259)	[0.67315]
Weighted regression using inverse	-0.00332	-0.00223	-0.00934	-0.00629
of N at municipality × year level	(0.00080)	[0.67315]	(0.00190)	[0.67315]
Survey weights	-0.00293	-0.00197	-0.02160	-0.01454
	(0.00063)	[0.67315]	(0.00222)	[0.67315]
Standard errors				
Clustering at municipality × year level	-0.00264	-0.00177	-0.02165	-0.01457
	(0.00072)	[0.67315]	(0.00432)	[0.67315]
Clustering at job level	-0.00264	-0.00177	-0.02165	-0.01457
	(0.00126)	[0.67315]	(0.00536)	[0.67315]
Twoway clustering at municipality × year and job	-0.00264	-0.00177	-0.02165	-0.01457
	(0.00131)	[0.67315]	(0.00635)	[0.67315]

Notes: Columns (1) and (3) report the estimates and standard errors (in parenthesis) for the interaction between match quality and proxy of workers' expected outside options. Columns (2) and (4) report the predicted difference in the effect of match quality between the 90th and 10th percentile of the outside option proxy distribution, and the difference between the 90th and 10th percentile (in brackets). See also the description of Table 4. All regressions include the same controls as the model reported in column (2) in that table. See the description of the robustness checks in the main text.

Table A.2: Separate regressions for EE and EUE job movers

	Log wage				Standardized wage			
	EE		EUE		EE		EUE	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
New match quality	0.0186	0.0101	0.00847	0.00606	0.0645	0.0374	0.0232	0.0175
	(0.00174)	(0.00210)	(0.00410)	(0.00452)	(0.00568)	(0.00671)	(0.00800)	(0.00863)
Previous match quality		0.0159		0.00579		0.0506		0.0138
		(0.00204)		(0.00446)		(0.00648)		(0.00867)
N	54,314	54,314	4,239	4,239	54,314	54,314	4,239	4,239
R^2	0.604	0.605	0.603	0.603	0.356	0.356	0.397	0.398

Notes: The table reports the results from separate wage regressions for "EE movers"—workers who changed firms between $t-1$ and t without an intermittent spell of unemployment—and "EUE movers"—workers who changed firms between $t-1$ and t with an intermittent spell of unemployment. A worker is defined as having experienced unemployment if he or she received some unemployment-related payments during t. All regressions include controls for education and experience fixed effects, a linear control for age, a second-order polynomial for each of the eight skills and skills squared, and a second-order polynomial for the average of all skills among tenured workers in the previous as well as new job. The sample is conditional on observing wages and match quality in the last job and current job. Robust standard errors in parenthesis.