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and Wage Growth**

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ABSTRACT

Job-to-Job Transitions, Sorting, and Wage Growth*

We measure the contribution of match quality to the wage growth experienced by job movers. We reject the exogenous mobility assumption needed to estimate a standard fixed-effects wage regression in the Danish matched employer-employee data. We exploit the sub-sample of workers hired from unemployment, for whom the exogenous mobility assumption is not rejected, to estimate firm fixed effects. We then decompose the variance of wage growth of *all* job movers. We find that 66% of the variance of wage growth experienced by job movers can be attributed to variance in match quality. Expected match quality growth is higher for higher-skilled occupations.

JEL Classification: J62, J63, C23

Keywords: job mobility, fixed-effect wage models, panel data models, assortative matching

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1 Introduction

Why do workers change jobs? If a worker moving to a new firm is paid more, is it because that firm is more productive and pays all workers more, or is it because of a better match between that particular worker's skills and that particular firm's needs? In this paper, we explore the wage dynamics of workers moving between jobs, and assess the relative contribution of firm-specific versus match-specific wage premia in triggering worker mobility. Job-to-job mobility is an important driver of wage growth. We find that job-to-job mobility is associated with a average wage gain of two percent, around twenty times higher than the average wage gain experienced by workers who remain in their current job in a typical year.¹ We show that the variance in the wage growth of job-to-job movers is mainly explained by variation in the quality of the worker-firm match. This result suggests that job mobility plays an important role in correcting the initial misallocation of workers across firms.

The most recent empirical literature on sorting has used data on worker skills and firm productivity to show that better workers tend to work at better firms (de Melo, 2015; Håkanson et al., 2015; Bagger and Lentz, 2016; Law et al., 2016). This empirical literature is in line with the theoretical implications of both classic and more recent on-the-job search models (Postel-Vinay and Robin, 2002; Lise and Robin, 2016; Bagger and Lentz, 2016; Kantenga and Law, 2016). An influential older literature using wage data alone, however, has consistently found either negative or no assortative matching between workers and firms. This older literature compares worker and firm fixed effects estimated from the additive two-way fixed-effects wage model proposed in the seminal paper by Abowd et al. (1999).²

Why do these two empirical literatures come to different conclusions? There are a number of reasons why an OLS estimation of a fixed-effects wage model may lead to questionable results. Among these, possibly the most critical is the assumption of exogenous mobility which is required for consistency of the estimators.³ For this

¹Topel and Ward (1992) and Eckstein et al. (2011), among others, document the importance of job-to-job transition in explaining wage growth.

²For example, see Abowd et al. (1999), Abowd et al. (2002), Goux and Maurin (1999), and Gruetter and Lalive (2009).

³Other problems are that firms that pay more and thus have higher fixed-effects may not be more productive or better in any meaningful way, so there is a fine line between positive and negative assortative matching. Moreover, if wages are not linear in the worker or firm fixed effects, the correlation is biased downwards (Eeckhout and Kircher, 2011; Law et al., 2016; Bagger and Lentz, 2016).

assumption to be valid, after conditioning upon the time invariant unobservable worker and firm effects, when a worker moves to a new firm she must draw at random from the existing firms in the economy. That is, a worker cannot observe the wage she will receive at her new firm until she has accepted the offer. This assumption of exogenous mobility clashes with the search theory mentioned above. Despite this well-known unattractive feature, due to its simplicity and tractability labor economists continue to extensively apply two-way fixed-effect wage regression models to study diverse topics including the assortativity of workers and firms (Abowd et al. (1999), Combes et al. (2008)), the determinants of executive compensation (Graham et al. (2011)), the difference between native and immigrant wages (Aydemir and Skuterud (2008)), the rise in wage inequality (Card et al. (2013)), and the gender wage gap (Card et al. (2016)).

We argue that, whatever theory says, the exogenous mobility assumption does not hold in the Danish data. To this end, we run several tests of exogenous mobility, both novel and taken from the existing literature. In particular, we develop symmetry tests that build on the test proposed by Card et al. (2013) and Card et al. (2016).⁴ Specifically, we show that wage gains for workers transitioning to better firms are on average larger than the wage losses for workers moving to worse firms. We argue that this is because workers moving to worse firms must be compensated. The quality of a firm is proxied by either the average wage of co-workers, the residual wage of co-workers, the poaching index as introduced by Bagger and Lentz (2016), or accounting measures such as profit, shareholder equity, and value added per worker. This conclusion contrasts with the results reported in Card et al. (2013) and Card et al. (2016) for German and Portuguese worker-firm data.

We propose a three-way fixed effect wage growth model, in which a match effect is included explicitly and allowed to vary freely. We present a novel estimation strategy to decompose the variance in wage growth. We start by observing that the mobility pattern of workers who experience spells of unemployment between jobs differs significantly from the mobility pattern of job-to-job movers. Moreover, when using the sub-sample of workers transitioning from unemployment to work, the previously discussed symmetry tests *do not* reject the assumption of exogenous mobility. We rationalize this result by arguing that, because the reservation wage of unemployed workers is low, the range

⁴Card et al. (2013) and Card et al. (2016) propose to categorize jobs based on the mean wage of co-workers while we introduce alternative job classifications as well.

of acceptable jobs is much larger for unemployed than it is for employed workers. Consequently, the unemployed accept nearly any job offer, even if the offered job has low match quality. Said differently, unemployed workers don't typically sort themselves into good matches.

Assuming the exogenous mobility of unemployment-to-job workers, we estimate a two-way fixed effect wage regression model on only this sub-sample of workers.⁵ We retrieve the estimated firm fixed effects from this estimation, and use them to decompose the variance in wage growth experienced by job-to-job movers into time-varying observables and firm and match-specific fixed effects. The results indicate that 66% of the variance of wage growth of job-to-job movers stems from variance in match quality, while only 9% comes from the change in wage premium paid by the firm, i.e. the firm fixed effect in the wage equation.

Finally, we find evidence supporting the compensation hypothesis, that a worker only moves to a firm that pays a relatively low wage premium if she is compensated by a relatively high match quality. First, we find that the change in the firm fixed effect of movers is strongly negatively correlated with change in their match quality. Second, we observe that the quality of the match improves on average for workers who move into firms with lower fixed effects, even though they experience an overall drop in wages. In other words, the wage drop triggered by a move into a low wage-premium firm are partially offset by an improvement in match quality. We also analyze wage growth across occupations. We find that higher-skilled workers tend to sort themselves into better matches when they move, while lower-skilled workers experience very little wage growth from a move on average.

Our results suggest that sorting plays a major role in the labor market. Within this context, our paper is related to the recent empirical literature that assesses the importance of sorting in explaining wage dispersion.⁶ Because we include match effects in a wage growth equation, we extend the analyses of [Sørensen and Vejlin \(2011\)](#) and [Sørensen and Vejlin \(2013\)](#). While [Sørensen and Vejlin \(2013\)](#) proposes a decomposition of wage growth into a worker and a firm fixed effects, we argue that including a match effect is crucial for understanding the wage growth of job movers, and therefore for understanding their mobility decision. [Sørensen and Vejlin \(2011\)](#) propose a wage regression model that

⁵More precisely, we only use unemployment-to-job spells. We do not use any job spell which was transitioned into directly from another job spell.

⁶See, [Lise and Robin \(2016\)](#), [Bagger and Lentz \(2016\)](#), and [Sørensen and Vejlin \(2013\)](#) among others.

includes a match effect but they impose the strong assumption that this match effect is orthogonal to the worker and firm fixed effects. Our paper is also closely related to the one by [Gruetter and Lalive \(2009\)](#). In this paper, the authors also identify a different mobility pattern for job-to-job movers compared to job-unemployment-job movers, and they estimate a two-way fixed effect wage model separately for these two types of workers. We argue that, because job-to-job workers move endogenously across firms, this type of estimation leads to biased firm fixed effects that cannot be used to decompose either wage dispersion or wage growth. We propose an alternative estimation strategy to explain the wage growth of job movers and therefore, to go deeper into the analysis of worker mobility patterns.

The paper proceeds as follows. The next section presents the results of various empirical tests on the Danish register data, which invalidate the exogenous mobility assumption required for identification of two-way fixed-effect wage models. Section 3 presents a wage growth decomposition and proposes an estimation strategy to quantify the contribution of firm and match effects to the wage growth experienced by job-to-job movers. Section 4 concludes.

2 Endogenous Mobility

In this section, we provide evidence that workers take the quality of their match with a firm into account when they transition between jobs. This endogenous mobility of workers prevents the consistent estimation of two-way fixed effect models as presented in [Abowd et al. \(1999\)](#) (hereafter AKM). Therefore we start by presenting this wage regression model before moving on to present heuristic tests of exogenous mobility that build on this specification.

2.1 Basic Statistical Model

The AKM wage equation decomposes log wage w_{it} of individual i in time t into additively separable worker and firm components:

$$w_{it} = \alpha_i + \psi_{J(i,t)} + x'_{it}\beta + r_{it} \quad (1)$$

where x_{it} captures the time-varying effect of observed characteristics, and α_i and $\psi_{J(i,t)}$ represent the time-invariant and unobservable determinants of wages that are specific to the worker and the firm, respectively. Following [Card et al. \(2013\)](#), x_{it} includes year dummies, quadratic and cubic terms in age, and their interactions with education dummies.

Suppose we have an annual unbalanced panel of N workers distributed across J firms. Let w denote the column vector of N^* annual log wage outcomes for workers, D and F the $[N^* \times N]$ and $[N^* \times J]$ matrices of worker and firm indicators, X the $[N^* \times K]$ matrix of time-varying observables, and r the column vector of N^* error terms. The matrix formulation that defines the AKM wage regression model is:

$$w = D\alpha + F\psi + X\beta + r$$

In order to estimate this regression model using ordinary least squares, we require the following necessary condition for consistency:⁷

$$\mathbb{E}[r|D, F, X] = 0 \tag{2}$$

The literature has named Condition (2) exogenous mobility. The rationale for this name is that, conditional on fixed effects, workers are not allowed to sort toward firms at which they get particularly high wages. Movement of workers across firms is thus required to be conditionally exogenous.

Threats to the validity of the exogenous mobility assumption might arise through three channels. To explore these channels, we decompose the error term r_{it} as follows: $r_{it} = \eta_{it} + \varsigma_{J(i,t),t} + \xi_{iJ(i,t)} + \epsilon_{it}$. The final component is simply white noise, which can be thought of either as measurement error, or idiosyncratic bonuses which workers receive and which are uncorrelated over time. The first two components, η_{it} and $\varsigma_{J(i,t),t}$, indicate that worker and firm quality can change over time. Simple time trends can be easily controlled for in the wage equation. The identification threat is instead if the changes are idiosyncratic, persistent, and correlated with mobility. With a period of analysis long enough, however, even persistent idiosyncratic shocks will average out, and our empirical

⁷[Abowd et al. \(2016\)](#) define and test for an additional sufficient condition, which we will discuss further below.

exercises below are based on eleven years of data. This paper focuses on the other threat to the exogenous mobility assumption, the permanent match specific components of wages, $\xi_{iJ(i,t)}$. This component is related to the productivity of a particular worker at a particular firm. Exogenous mobility requires that match quality of observed wages in the data must be conditionally zero, at least on average. Without strong assumptions on technology or information frictions, reasonable models of labor market sorting will instead predict that firms are more likely to hire workers particularly well-suited to their own particular needs (and through bargaining, this match quality should show up in wages). To determine the empirical relevance of this threat to the exogenous mobility assumption, we will employ several tests below on the wages of Danish workers. Thus we turn to a description of our data.

2.2 Matched Employer-Employee Data from the Danish Registry

Before presenting our tests of exogenous mobility, we describe our sample. We use linked worker-firm data from Statistics Denmark for the period 2000-2010. The data contain the employment history of every working-age citizen residing in Denmark at the end of the year. We use person-level administrative registers that provide information on each person's age, education, gender, civil status, labor market status, labor market experience, and, if employed, sector, tenure, occupation, and estimated hourly wage for the job held in November.⁸ We restrict our sample to all workers, with non-missing and non-zero wage, between 25 and 60 years old. We discard observations of workers who are either employed in the public sector or self-employed, and we only include the observations that relate to the workers' main job. We also delete observations of workers with undisclosed establishment or firm identification numbers.⁹ We deflate hourly wages by the 2000 CPI index, and we drop all observations that belong to the bottom or top percentile of the yearly wage distributions.

We merge in firm-level register data from the General Enterprise Statistics database. This database gathers both general firm statistics available at the establishment level (e.g., employment level, industry, and geographical location) and accounting data available

⁸We mainly use the Danish Integrated Database for Labor Market Research, i.e., the IDA register.

⁹This correction decreases the sample size by around 7% before 2008 while it has no effect after 2008. Indeed, from 2008 onward, the IDAN register records the firm identification number, partially solving the issue of fictitious establishments.

at the firm level (e.g., profit, shareholder equity, and value added). The general firm statistics cover all firms whose activity exceeds a triviality threshold.¹⁰ In comparison, the accounting data, which are survey-based, only account for a selection of firms.¹¹ We only select firms that report accounting data, a selection that decreases our sample size by about 25%. Because some of our analyses rely on accounting data, we aggregate establishment-level data at the firm level to bring the level of analysis to the firm rather than the establishment.¹² Finally, our base sample contains 10,071,509 yearly worker-firm observations, 1,640,590 workers, and 175,950 firms. The diverse tests of exogenous mobility performed in the following sections rely on sub-samples of this base sample, and each sub-sample will be further explained in the corresponding section.

2.3 The Symmetry Test: Our baseline

Our baseline test was developed in [Card et al. \(2013\)](#) and [Card et al. \(2016\)](#) to validate the assumption of exogenous mobility. To implement the test, jobs are first categorized into types. Because this test aims to validate fixed-effect wage models, the job type does not employ AKM firm fixed effects themselves, but rather an ex ante proxy for the firm wage premium: the mean wage of co-workers. Specifically, origin and destination jobs are classified based on the quartile of the mean wage of co-workers in that year. With four types of firm to transition from, and four types of firm to transition to, we obtain sixteen job transition categories.¹³

Suppose that exogenous mobility is violated by workers moving conditional on match quality. We would expect wage changes to be positive after a move, independent of the category of the job transition (up to unobserved compensating differentials). In particular, we would expect wages to increase for a worker leaving a generally high-wage job for a generally low-wage job, on the principle that the low-wage job must be a good

¹⁰The triviality threshold has two criteria: i) a labor cost that exceeds half a full-time employment; ii) earnings that exceed a sector-dependent volume. Because the triviality threshold is quite low, almost all active firms are included in the dataset.

¹¹Firms are selected based on their size: firms employing more than 50 workers are surveyed each year, firms with 20-49 employees are surveyed for three years followed by three years of exemption, firms with 10-19 employees are surveyed for two years followed by eight years of exemption, and firms with 5-9 employees are surveyed once every 10 years.

¹²Because only 3.5 to 4.6 percent of all the listed firms have more than one establishment, results do not differ significantly when running at the establishment level analyses that don't rely on accounting data.

¹³Test results in this section are all robust to a change in the number of categories to three or five.

match for this worker. This match effect in wages then compensates the worker for the low type of the firm and triggers the job transitions. We refer to this hypothesis as the “compensation hypothesis”. The aim of the test is therefore to compare the change in wages for workers moving from low-wage jobs to high-wage jobs with the change in wages experienced by workers moving in the opposite direction.

More formally, let ψ_k be the expected wage-level in a job in Quartile k .¹⁴ Using the fixed effect wage equation (1), we can write the expected wage change induced by a worker moving from a job in Quartile 1 to a job in Quartile 4:¹⁵

$$\mathbb{E}[w_{i,t+1} - w_{i,t}|Q_1 \rightarrow Q_4] = \psi_4 - \psi_1 + \mathbb{E}[\xi_{iJ(i,t+1)} - \xi_{iJ(i,t)}|Q_1 \rightarrow Q_4] + \Delta X_{i,t+1}\beta \quad (3)$$

Change in white noise ϵ is absent because it is zero in expectation. Up to the time-varying observables X_{it} , the expected change in wage experienced by job movers is the change in the wage premium for the job quartiles ψ_k , plus the expected change in the match quality ξ . If the exogenous mobility condition (2) holds, then by the law of iterated expectations the expected change in the match quality is zero. In this case, up to time-varying observables, the expected wage gains experienced by movers from Quartile 1 to Quartile 4 should be exactly the inverse of expected wage losses of workers moving from Quartile 4 to Quartile 1. It is this empirical claim which the symmetry test is designed to evaluate.

Figure 1 shows the average residual log wage changes experienced by job movers.¹⁶ When analyzing the dynamics of wage residuals, wage gains experienced by upwards movers exceed wage losses experienced by downwards movers.¹⁷ This violation of exogenous mobility is what one might predict if workers move conditional on match quality. In Panel (b), we observe that the wage change plots of movers who transition

¹⁴The ranking is defined at the job level rather than at the firm level, i.e. quartiles refer to the yearly distribution of mean co-worker wages across worker observations.

¹⁵As discussed above in Section 2.1, we abstract here from idiosyncratic time-varying worker effects η and time-varying firm effects ς .

¹⁶Residuals wages in this paper are always derived from a regression of log wages on year, education, sex, marital status, industry, occupation, and location, as well as quadratic and cubic age and experience trends. In addition we include as regressors interactions between year and education dummies, and interactions between education dummies and age and experience trends.

¹⁷Wage drops can occur both voluntarily due to unobserved compensating differentials, and also due to workers who have involuntarily lost their jobs. By augmenting population register data from Statistics Denmark with survey data, [Taber and Vejlin \(2016\)](#) estimate that 20.5% of job-to-job transitions are involuntary.

in opposite directions lie above the red dotted line that represents the case of perfect symmetry in wage changes.¹⁸ This result is at odds with the findings of [Card et al. \(2013\)](#) and [Card et al. \(2016\)](#) in other countries. When transiting to a firm that is one quartile higher, workers experience an average wage gain of 4.8%, while workers transiting in the opposite direction face an average wage drop of 0.9%, only 18% of the wage gain. When workers move two quartiles up, their average wage increases by 7.2%, whereas when moving down, they experience an average wage drop of 4.2%, or 59% of the wage gain. Finally, when moving up and down three quartiles, the average wage gain is 9.5%, while the average wage drop is 8.1%, 86% of the wage gain. This asymmetry in wage dynamics, especially for job-to-job transitions between relatively similar firms, suggests that moves might be triggered by match effects, and therefore that the exogenous mobility assumption does not hold.

2.4 Alternative Symmetry Tests

2.4.1 Mean Residual Wage of Co-Workers

One drawback of the symmetry test is that results might depend on the choice of the proxy for the firm wage premium that is used to categorize jobs into quartiles. Indeed, if we use Equation (1) to write the job fixed effect ψ at a particular firm $J(i, t)$, we get:

$$\psi_{J(i,t)} = \bar{w}_{-it} - \bar{\theta}_{iJ(i,t)} \quad (4)$$

Here \bar{w}_{-it} represents the average wages of i 's coworkers, and $\bar{\theta}_{it} = \frac{1}{N_{J(i,t)}} \sum_{k=-i} (\alpha_k + x_{kt}\beta + \xi_{kJ(i,t)})$ and $N_{J(i,t)}$ is the number of co-workers in firm $J(i, t)$. Through the lens of our wage model (1), a job ranking based on the mean wage of co-workers is only identical to the job ranking based on the firm fixed effect if both the distribution of workers types is identical across firms, and also match quality of all workers is zero.¹⁹ A violation of either of these conditions will lead to an incorrect job ranking through the lens of the

¹⁸As in [Card et al. \(2013\)](#) and [Card et al. \(2016\)](#), the hypothesis of symmetry is formally rejected.

¹⁹If the second condition is violated, the first condition will be violated too, but the reverse is not true. Indeed, as explained by [Card et al. \(2016\)](#), sorting might arise even in absence of endogenous mobility, if job-to-job mobility is related to time-invariant characteristics of workers, for example their search behavior on the job.

model.²⁰

We partially correct for this problem by regressing coworker wages on a large set of observables and using wage residuals in order to classify jobs.²¹ We then categorize jobs based on the mean residual wage of co-workers. Because $\bar{\theta}_{iJ(i,t)}$ includes unobservable worker characteristics and unobservable match effects, however, this method is only a partial correction. Because we control for some variables included in $\bar{\theta}_{iJ(i,t)}$, this measure brings the firm ranking closer to the firm fixed effect ranking of the model. Figure 2 shows how the job ranking based on the mean residual wage of co-workers differs from the classification based on the mean wage of co-workers. Depending on the quartile, between 27% and 66% of jobs are re-classified to a higher or lower quartile when using the mean residual wage of co-workers to define quartiles. Moreover, we find that 59% of job transitions are reclassified either for the origin or destination job.²² The magnitude of this re-classification suggests that the distribution of workers and match effects differs significantly across firms.

We redo the symmetry test using a ranking of jobs based on the mean *residual* wage of coworkers. Figure 4 shows that the wage gains associated with upward transitions are significantly larger than the wage drops associated with downward transitions when we classify jobs based on the mean residual wage of co-workers. When transiting to a firm that is one quartile higher, workers experience an average wage gain of 5.5%, while workers transiting in the opposite direction face an average wage drop of 2.6% (48% of the wage gain). When workers move two quartiles up, their average wage increases by 9.5%, whereas when moving down, they experience an average wage drop of 7.1% (75% of the wage gain). Finally, when moving up and down three quartiles, the average wage gain is 13.8% while the average wage drop is 12.4% (90% of the wage gain).

²⁰On the other hand, if mobility is exogenous, (3) shows that the wage gains and losses should be symmetric whatever classification methodology we use. The main reason to worry about firm classification is that the symmetry test loses the power to reject exogenous mobility if the classification of jobs is completely random.

²¹Specifically, the model includes dummies for year, education, occupation, industry, region, gender, and marital status, as well as quadratic and cubic terms in age and experience fully interacted with education dummies, and year dummies interacted with education dummies.

²²For more details, see Appendix A.

2.4.2 Using Accounting Data and Poaching Index

An alternative to using wage data to classify firms is to make use of the firm dimension of the matched employer-employee database. From the General Enterprise Statistics dataset, we draw information on each firm's profit per employee, value added per employee, and shareholder equity per employee. We use each of these three proxies of firm productivity (or firm value), and we classify jobs into quartiles based on the distribution of each of the three measures. Results are shown in Figure 5, Panels (a), (b) and (c). Workers moving to better firms benefit from a positive wage growth of about 2-5%, while workers transitioning to lower-quartile firms also benefit from a positive wage growth, though moderate, of about 0-3%.

Our last symmetry test relies on the use of the poaching index, introduced by [Bagger and Lentz \(2016\)](#), which ranks firms based on their productivity level. As more productive firms are more able to compete with other firms for workers, and therefore to poach employed workers, compared to less productive firms, the percentage of workers previously employed in each firm's pool of new hires, i.e., the poaching index, is expected to monotonically increase with the firm's productivity. We restrict our dataset on firms that have more than 13 new hires over the period, from which at least one is hired from unemployment. Shown in Figure 5, Panel (d), the results indicate that wages increase slightly, independently of the type of transition that workers experience.

These results suggest that more productive firms do not necessarily pay higher wage premia, a possible explanation of this being the presence of match effects that offset the firm fixed effects. These alternative firm classifications therefore reinforce our prediction that workers sort themselves into better matches, even when the improvement in the quality of the match is obtained by transitioning to less productive firms, as measured by profit per employee, value added per employee, shareholder equity per employee, and the poaching index.

2.5 More suggestive tests of exogenous mobility

In this section we report the results of several alternative tests of exogenous mobility. These tests should be considered suggestive, as unlike the symmetry test above they do not directly test the necessary conditions for exogenous mobility, and the first test depends on a strong assumption.

2.5.1 Hausman test

Consider a model similar to (1) but with a worker-firm fixed effect:

$$\ln w_{it} = M_{i,J(i,t)} + X_{it}\beta + \varepsilon_{it} \quad (5)$$

If our fixed effect wage equation (1) is correctly specified, then in an analogue to the fixed-effects/random-effects comparison, the OLS estimator of β from (1) is more efficient than the OLS estimator of β in the saturated model (5), since the saturated model has many more parameters to estimate.²³ The saturated model is also consistent, as any match effect potentially correlated with the observables X_{it} is included in the worker-firm fixed effect.

In order to use the classic Hausman specification test to evaluate the OLS estimator of β in (1) for consistency, we need one additional strong assumption. We must assume that the OLS estimator for β in (1) is efficient. This assumption is critical to constructing the variance estimator in the Hausman test statistic. If this assumption is violated, then the variance estimator itself will not be consistent, and the results of the Hausman test will not be meaningful. We have no way to verify that the OLS estimator of (1) is efficient. Indeed the number of fixed effects we are estimating might lead one to suspect that the estimator is not efficient. For this reason we consider results of this test only suggestive.

With that caveat, we proceed to implement the test. Let $\hat{\beta}_{AKM}$ be the (1) estimator, and $\hat{\beta}_{SM}$ be the estimator from the saturated model (5). The Hausman test statistic H_β is:

$$H_\beta = \left(\hat{\beta}_{SM} - \hat{\beta}_{AKM} \right)' \left(\text{Var}(\hat{\beta}_{SM}) - \text{Var}(\hat{\beta}_{AKM}) \right)^{-1} \left(\hat{\beta}_{SM} - \hat{\beta}_{AKM} \right) \quad (6)$$

Under the null hypothesis that the (1) OLS estimator of β is consistent, the test statistic is distributed chi-squared, with degrees of freedom equal to the rank of the sandwiched variance difference in the above formula. With a test statistic of 8,993 and 54 degrees of freedom, we strongly reject the null hypothesis. That is, we reject the consistency of the OLS estimator of β in (1). If the estimator of β is inconsistent, the estimators of worker and firm fixed effects will generically be inconsistent as well, assuming that the time-varying observables are correlated with the fixed effects. This

²³It is a slight abuse of terminology to call this model saturated, as we still assume that the match effect is constant over time. A truly saturated model would include a fixed effect for each worker-year pair.

is a consequence of the Frisch-Waugh-Lovell Theorem. We interpret this outcome as suggestive evidence against the consistency of worker and firm fixed effects in OLS estimates of (1).

2.6 Abowd-McKinney-Schmutte tests

Finally, we perform on the Danish register data the two tests of exogenous mobility based on wage residuals proposed by [Abowd et al. \(2016\)](#). These tests investigate the validity of the following condition:

$$\mathbb{E}[r|X] = 0 \text{ and } Pr[D, F|X, r] = Pr[D, F|X] \quad (7)$$

Condition 7 is stronger than the exogenous mobility condition. This condition requires that the conditional distribution of D and F on ξ and X is identical to the unconditional distribution of D and F . Said differently, the requirement is that D and F are independent of r conditional on X . In this case, knowledge of any set of residuals from an OLS is in no way informative about any fixed effect in the model. If Condition 7 holds, then exogenous mobility (2) holds as well, but not vice versa. That is, Condition 7 is sufficient for identification of AKM fixed effects, but not necessary.

Both of the tests proposed by [Abowd et al. \(2016\)](#) are chi-squared independence tests of structural residuals and fixed effect estimates from the OLS estimation of (1). Considering only workers who transition to new firms between period t and $t+1$, the “match effects test” looks at the relationship between the worker’s residuals at time t and the fixed effect of the worker’s firm at time $t+1$. The “productive workforce” test probes the independence between mean residuals of workers at a particular firm in period t and the mean worker fixed effect at that firm in period $t+1$.²⁴ For both tests, we first estimate (1) using OLS, recovering estimates $\hat{\theta}_i$ for each worker, $\hat{\psi}_j$ for each firm, and residuals \hat{r}_{it} for each period and worker. We consider only workers who move between firms, and for each mover, we calculate the mean residual wage in the years before moving, \bar{r}_i . Then, we discretize \bar{r}_i , estimated worker fixed effects, and estimated firm fixed effects into deciles. The “match effects test” runs as follows: if the exogenous mobility condition holds, then the joint probability of observing a worker fixed effect, an origin firm fixed effect, a

²⁴Since our implementation of these tests does not differ from that in [Abowd et al. \(2016\)](#), we only summarize these tests briefly. Readers interested in a more detailed discussion should see [Abowd et al. \(2016\)](#).

destination firm fixed effect, and a mean residual decile is the same as the unconditional probability of observing a mean residual decile multiplied by the joint probability of observing a worker fixed effect, an origin firm fixed effect, and a destination firm fixed effect. We compare the empirical sample analogues of these probabilities and, using the chi-squared property of the distribution of their difference, we test the null that the exogenous mobility holds. The results show a chi squared test statistics of 32,430 with 8991 degrees of freedom, indicating that the “match effects test” strongly reject Condition 7. We conduct the “productive workforce” test by empirically assessing whether the mean wage residual in each firm is predictive of the distribution of worker fixed effects one period later, conditional on the firm fixed effect. With a chi-squared statistic of 3,076 and 810 degrees of freedom, the “productive workforce” test also strongly rejects Condition 7, a result that is similar to the one obtained by [Abowd et al. \(2016\)](#) on the US Longitudinal Employer-Household Dynamics data.

3 Quantifying the Match Effect on Wage Growth

Every one of the tests that we presented in the previous section rejects in the Danish data the assumption of exogenous mobility. In particular, our direct test of exogenous mobility, the symmetry test, rejects under a wide variety of specifications. Consequently, we argue that any estimation of fixed-effect wage model (1) on Danish data is inconsistent. Our results call for the inclusion in the wage equation of a match effect which is not required to be conditionally mean zero. In this section, we propose an estimation strategy that allows us to decompose the wage growth of job movers into into observables, firm, and match effects, without assuming that the match effect is conditionally mean zero, i.e. that mobility is exogenous.

3.1 Wage Growth of Job Movers

We modify the fixed-effect wage model (1) to explicitly include a match effect:

$$w_{it} = \alpha_i + \psi_{J(i,t)} + x'_{it}\beta + \xi_{iJ(i,t)} + \epsilon_{it} \quad (8)$$

where $\xi_{iJ(i,t)}$ is the match effect on wages for workers i employed in firm $J(i,t)$ at time t . In principle our model allows any relationship between firms and wages, including

the non-monotonic relationship between firm type and wages documented by [Eeckhout and Kircher \(2011\)](#). We expect $\xi_{iJ(i,t)}$ to be an inverse U-shaped function of firm type around the worker’s optimal firm type. In fact, Equation (8) encompasses as special case [Eeckhout and Kircher \(2011\)](#)’s wage equation.²⁵

To assess the importance of sorting in worker mobility, we focus on the first difference of Equation (8):

$$\Delta w_{it} = \Delta \psi_{J(i,t)} + \Delta X'_{i,t} \beta + \Delta \xi_{iJ(i,t)} + \Delta \epsilon_{i,t} \quad (9)$$

The worker’s own fixed-effect drops out, and we are left with differences in the firm fixed-effect ψ , time-varying observables X , the match fixed-effect ξ , and the white noise ϵ .

3.2 Empirical Strategy and Results

Without imposing the strong assumption of orthogonality of the match effect,²⁶ we cannot directly estimate Equation (8) via OLS. We therefore propose the following two-step estimation strategy. First, because the endogenous feature of worker mobility might lead to inconsistent estimates of firm fixed effects, we start by identifying a group of employment spells for which the assumption of exogenous mobility is not rejected. We use the set of employment spells of workers are hired directly from unemployment.²⁷ We estimate the fixed-effect wage model (1) on only this sub-sample of spells and save the estimated firm fixed effects. Second, using these estimated firm fixed effects, we decompose the observed wage growth of all movers according to (9).

3.2.1 Unemployment-to-Job Workers

To conduct the symmetry test of exogenous mobility, we restrict our analysis to unemployment-to-work job spells of workers for whom we observe at least two moves from unemployment. These moves from unemployment do not have to be successive. That is, a worker may have held other jobs. We do not use these other job spells in our analysis. We rely on the fact that the reservation wage of unemployed workers is

²⁵In [Eeckhout and Kircher \(2011\)](#), the wage equation is a function of the match and the worker fixed effects.

²⁶See [Sørensen and Vejlin \(2011\)](#) who runs an OLS estimation on a three-way fixed-effect wage model.

²⁷We take a broad view of unemployment, so we use here any transfer from not working to working, including workers entering the labor force, or reentering after a period out of the labor force.

much lower than the reservation wage of employed workers, potentially low enough that workers will accept any job offer. If this is the case, the exogenous mobility assumption will be satisfied. Below we present some suggestive evidence supporting this claim.

First, we show that the wage growth of workers hired from unemployment is markedly different than wage growth of workers hired from another job. Figure 6, Panel (a) shows the cumulative distribution function of the residual wage changes experienced by unemployment-to-job workers. That is, residual wage changes between job spells where workers are moving out of unemployment. Panel (a) also shows residual wage changes of job-to-job workers, that is workers moving between jobs without transitioning through unemployment. Wage changes for unemployment-to-job workers are roughly uniform, with the median wage change close to zero. In comparison, wage growth is normally distributed in the sample of job-to-job workers with the median well above zero at around five percent.

Next, we test for the exogenous mobility of unemployment-to-job workers by running the symmetry tests presented in the previous sections. The only difference is that a move is not necessarily an adjacent year, since unemployment-to-job spells do not generically follow immediately after one another. Of course, we continue to use residual wages, which flexibly remove calendar year intercepts, age intercepts, experience, and other time-varying variables. We always use the residual wages from the first year of unemployment-to-job spells.²⁸ The results are shown in Figure 6, Panel (b) for the classification of firms based on the mean wage of co-workers, and Panel (c) for the mean residual wage of co-workers. Figure 7 presents results for classification of firms using the other measures of firm productivity described in Section 2.3.

The wage growth patterns of unemployment-to-job workers contrast with those of job-to-job workers presented in the previous section. For workers hired from unemployment, the wage gain of workers transitioning to better firms approximately equal to the wage drop experienced by workers transitioning to worse firms. Depending on the measure of firm productivity, the wage change plots of movers who transition in opposite directions around the red dotted line that represents the case of perfect symmetry in wage changes. This evidence is inconsistent with the compensation hypothesis that suggests that workers accept jobs at low productivity firms only if they benefit from a positive match effect that

²⁸We have also used the last year of the previous spell and the first year of the next spell. The results do not change significantly. We weakly prefer to use first year back from unemployment rather than last year because it is more comparable to compare wages of workers just back from unemployment.

offsets the negative change in firm fixed effects. Said differently, we find no evidence that unemployment-to-job workers sort themselves into better matches and therefore, that sorting triggers their mobility.

While the symmetry test is our only direct test of the necessary conditions for the exogenous mobility condition, we also perform the other suggestive tests from the literature on unemployment-to-job spells. These tests continue to reject exogenous mobility, even on our preferred sample of unemployment-to-job spells. The Hausman test, however, gives us an inconclusive test statistic of -2,556 with 54 degrees of freedom. The Hausman statistic is asymptotically chi-squared, so it should never be negative, a result that would suggest that the OLS estimator of β in the saturated model is not efficient. Therefore, because the Hausman test is only meaningful under the strong assumption of efficiency, and the Abowd-Schmutte tests are testing a condition much stronger than necessary for consistency in OLS, these results do not inform on whether the exogeneity condition holds on the sample of unemployment-to-job workers.

3.2.2 Decomposing Wage Growth

What drives the wage dynamics of movers? To answer this question we decompose the variance of the wages of job-to-job workers after a move. If the match effect of unemployment-to-job workers is truly conditionally mean zero, a two-way fixed-effect wage model can be consistently estimated on the sample of workers who are hired from unemployment. On only this sub-sample, we estimate the fixed-effect wage equation (9) using OLS. We find a negative correlation of -0.08 between worker fixed effects and firm fixed effects. After estimating the wage equation, we recover the worker fixed effects for only workers hired from unemployment, but we also recover firm fixed effects which apply to all workers.

Using our first-stage estimated firm fixed effects, we decompose wage growth according to (9). In this difference equation, the worker's own fixed effect drops out. Remaining effects are simply the change in the firm fixed effect which we recovered in the first stage, changes in observables, changes in the match effect, and change in white noise. We use the following decomposition of component y 's contribution $D(y)$ to the variance of wages:²⁹

²⁹See [Gibbons et al. \(2012\)](#) for a nice overview of the literature on wage variance decomposition.

$$D(y) = \frac{Cov(y, \Delta \ln w)}{Var(\Delta \ln w)} = \frac{Var(y) + \sum_{z \neq y} Cov(y, z)}{Var(\Delta \ln w)} \quad (10)$$

One additional step is necessary to calculate this wage decomposition. By assumption, white noise $\Delta\epsilon$ is uncorrelated with the other regressors, but we still need the variance of $\Delta\epsilon$. From our estimation we only recover the variance of the composite error $\Delta r = \Delta\xi + \Delta\epsilon$. In order to estimate the variance of $\Delta\epsilon$, we use the variance of log wage changes net of changes in time-varying observables for workers who remain at the same firm:

$$\widehat{V(\Delta\epsilon_t)} = V(\Delta w_t - \Delta X_t \hat{\beta})$$

Here we use $\hat{\beta}$, our estimate of β from the fixed effect OLS regression using only workers moving from unemployment to a job spell. This relationship comes directly from (9). With this estimate of the variance of $\Delta\epsilon_t$ in hand, we can decompose the variance of log wages into the variance of changes in firm fixed effects, the variance of changes in observables, the variance of changes in the match effect, and the variance of changes in white noise. Said another way, we obtain the changes in the match effect by netting out the variances of changes in the firm effects, observables, and white noise.

Before we present the results, we must first discuss a caveat about our estimation methodology. We can only estimate firm fixed effects for firms visited by workers moving from unemployment to employment. Thus, relative to the full sample OLS estimation, we lose a significant number of firms, especially smaller firms. Specifically, our sample of only workers moving from unemployment contains 868,027 workers, 86,533 firms, and 3,503,431 observations. This is roughly half the number of workers and firms in our full sample, and around one third of the observations.

The variance decomposition is reported in Table 1. We find that neither time-varying observables nor firm fixed effects can explain much of the variation in wages. Time-varying observables explain less than 1% of the variance of changes in wages, and changes in firm fixed effects explain only 9% of the variance. Instead the primary driver of the wage dynamics of movers is the change in the match effect, which explains 66% of the variance in wage changes. This is followed by change in white noise which explains 25% of the variance of wage changes. This result is not driven by a small variance in firm fixed effects. The standard deviation of the firm fixed effect across all movers in the first year of their new job is 0.13, or 13% of wages. The driver of the small variation in the *change*

in the firm fixed effect is that workers tend to move between firms with fixed effects of a similar level. Among movers, we find that only 7% move to a firm which has a fixed-effect differing from their previous firm by more than 0.01, or 1% of wages.

We argued above that the match effect is not conditionally mean zero due to the compensation hypothesis, which predicts that a worker moving from a firm with a high wage premium to a firm with a low wage premium should be compensated with higher match effect growth than workers moving in the opposite direction. In the correlation matrix presented in Table 2, we report that the correlation between the change in the match effect and the change in the firm fixed effect is -0.522.³⁰ This negative correlation is consistent with the compensation hypothesis.

In Table 3, we show how our estimates depend on the change in the firm effect. First, when workers move to a firm with a higher fixed effect, wages go up, but wages react only marginally to moves in the opposite direction. In other words, the symmetry test fails, and we continue to reject the exogenous mobility condition necessary to estimate the wage equation using OLS on the full sample. Secondly, workers moving to a lower fixed effect firm on average get a match effect gain of 0.098, or 9.8% of wages, while those moving to a higher fixed effect firm experience a average match effect loss of 0.074, or 7.4% of wages. The difference in absolute values is significant at all conventional levels.

While the compensation hypothesis correctly predicts that we will observe a growth in the match effect for workers moving to lower fixed effect firms, the hypothesis is silent about the direction of the match effect for workers moving to higher fixed effect firms. Looking outside of the compensation hypothesis, the inverse relationship we observe between the match effect and the firm fixed effect may be picking up bargaining as in classic on-the-job search models (Postel-Vinay and Robin, 2002). All else equal, a firm might be able to pay a worker coming from a low-wage firm less.

3.2.3 Heterogeneity across Occupations

In this section we investigate how the importance of changes in wage components differs for workers of different occupations. Figure 8 decomposes the expectation of wage changes $\mathbb{E}[\Delta \ln w]$ into the expectation of changes in the firm fixed effect, the observables, and

³⁰More precisely, we find that the composite error has a negative correlation with the firm fixed effect. Since the correlation between the firm fixed effect and white noise is zero by assumption, this is also the correlation between the firm fixed effect and the match effect.

the match effect. We find that workers at a basic skill level tend to experience very little change in wages after a move.³¹ Intermediate and highest level workers tend to experience significant wage growth after a move, and nearly all of the wage growth is due to the match effect. This evidence suggests that higher skill level workers may be less substitutable, and that these workers must change jobs several times to find an optimal match.

4 Conclusion

In this paper, we provide evidence that job-to-job movers transition endogenously across firms. The exogenous mobility assumption necessary to identify the classic two-way fixed-effect wage equation is rejected in Danish data. Our results underline the necessity of including an explicit match effect in the wage regression model. Unlike job-to-job movers, workers who are hired from unemployment do not appear to sort into firms with high match quality. Consequently, we exploit the exogenous mobility of these workers to estimate the firm fixed effects and we use these estimated firm fixed effect to quantify the contribution of the match effect in explaining the wage dynamics experienced by job-to-job movers. We find that 66% of the variance of changes in wages after a move can be attributed to changes in match quality. We present evidence supportive of the compensation hypothesis, that workers who move from higher to lower paying firms are compensated by a improvement in their match quality. Finally we find that sorting is particularly important for understanding the mobility decisions of higher skilled workers. Overall, our analysis suggests that job-to-job mobility is an important tool for correcting the initial misallocation of workers across firms arising from the frictions in the labor market.

³¹The skill level classification in our data is based on the International Standard Classification of Occupations. Basic skill level roughly corresponds to unskilled blue collar workers. Intermediate level is roughly skilled workers or tradesman, and the highest level is managers and professionals. Basic level roughly corresponds to high school education, intermediate a short tertiary education, and highest level a college degree.

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A Misclassification of Job Quartiles

Using Equation (4), we can express the expected firm fixed effect conditional on the firm's quartile based on the mean wage of co-workers:

$$E(\psi_J | J \in Q_q^{\bar{w}}) = \bar{w}_q - E(\bar{\theta}_J | J \in Q_q^{\bar{w}}) \quad (11)$$

Because of the variation in $\bar{\theta}_J$ across firms, not all moves from low to high quartile of the mean co-worker wage distribution are moves from low to high fixed-effect firms. The same is true for downwards job-to-job transitions. We argue that this mis-classification has important consequences for the symmetry in the wage patterns for upward and downwards job-to-job transitions. As explained in Section 2.3, we expect a worker who voluntarily transitions from a high to a low fixed-effect firm to be compensated by a large match effect. On the contrary, an upward transition might not necessarily be associated with a large match effect given that the worker already benefits for the positive change in the firm fixed effects. Therefore, if some transitions between firms with similar fixed-effects and small match effects are misclassified as transitions from high to low quartile jobs, the average match effects would be downward biased. In particular, we hypothesize that:

$$E(\xi_{iJ(i,t+1)} - \xi_{iJ(i,t)} | \text{move from } Q_1^{\bar{w}} \text{ to } Q_4^{\bar{w}}) \approx E(\xi_{iJ(i,t+1)} - \xi_{iJ(i,t)} | \text{move from } Q_1^{\bar{w}^{res}} \text{ to } Q_4^{\bar{w}^{res}}) \quad (12)$$

$$E(\xi_{iJ(i,t+1)} - \xi_{iJ(i,t)} | \text{move from } Q_4^{\bar{w}} \text{ to } Q_1^{\bar{w}}) < E(\xi_{iJ(i,t+1)} - \xi_{iJ(i,t)} | \text{move from } Q_4^{\bar{w}^{res}} \text{ to } Q_1^{\bar{w}^{res}}) \quad (13)$$

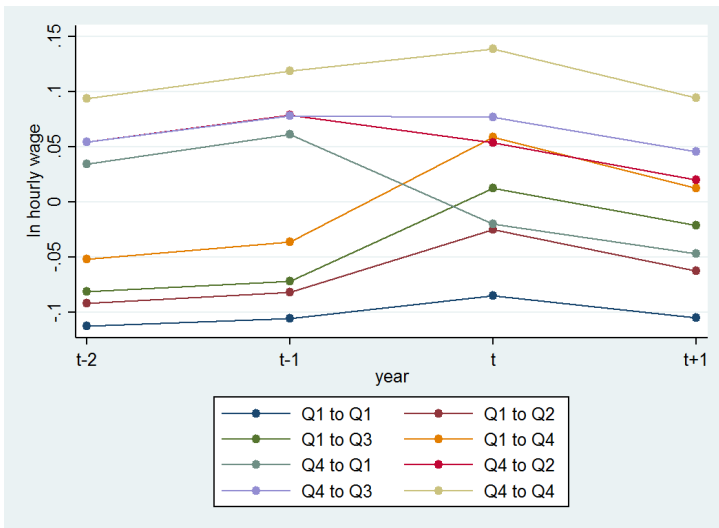
where $Q_q^{\bar{w}^{res}}$ denotes the quartile q when jobs are classified based on the mean residual wage of coworkers and $q = (1, 2, 3, 4)$.

Figure 3, Panel (a) shows the percentage of job-to-job transitions that are misclassified. Across all 4*4 mover groups, only 41% of job-to-job transitions are identically classified when using the mean residual wage of co-workers to define quartiles. 34% of all transitions are re-classified as being better moves (smaller downwards mobility or stronger upwards mobility) when using the mean residual wage of co-workers to define quartiles, whereas 25% are re-classified as being worse transitions (stronger downwards mobility or smaller upwards mobility). Panel (b) shows that the classification bias is larger for downwards job-to-job transitions compared to upwards transitions. Indeed, 50% of the

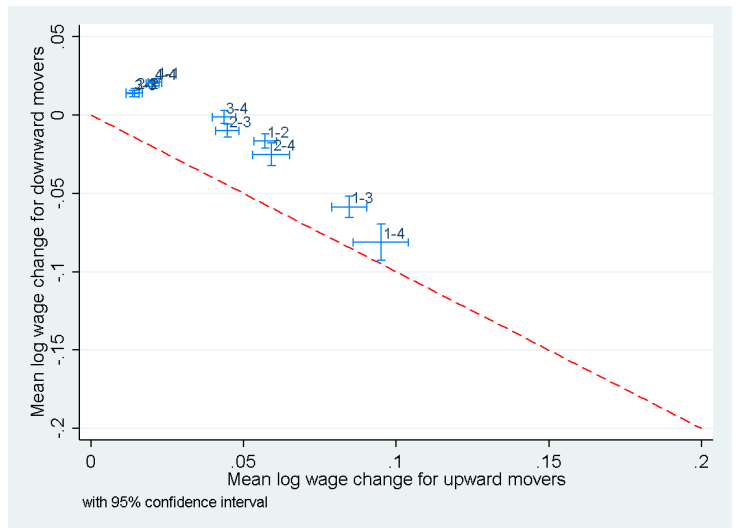
downwards transitions are re-classified as being an upward transition, a transition to a similar job quartile, or a smaller drop in quartile. This bias is only partially averaged out by an opposite pattern of upwards transitions. We explain this asymmetry in the bias for upwards and downwards transitions by pointing at the workers job-to-job transition strategy: while workers seek to avoid a drop in the firm fixed effect that directly reduces their wages, they do not necessarily refrain from transiting to lower average wage firms if these firms provide a large match effect. As a result, downwards transition groups recorded by classifying jobs based on the mean wage of co-workers include a large share of better transitions for which we expect the match effect to be relatively small. This mis-classification potentially reduces the estimated match effect, and would lead to the wage gains and losses experienced by upwards and downwards job movers to wrongly appear symmetric.

B Figures

Figure 1: Residual wage Gains vs. Wage Losses for Job Movers



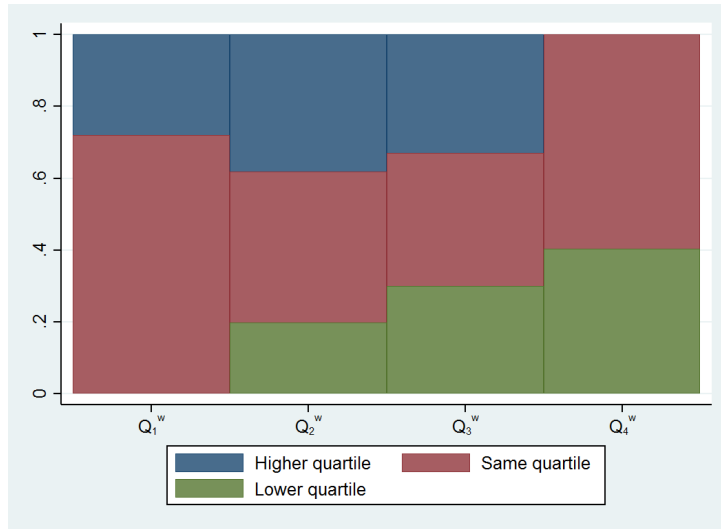
(a)



(b)

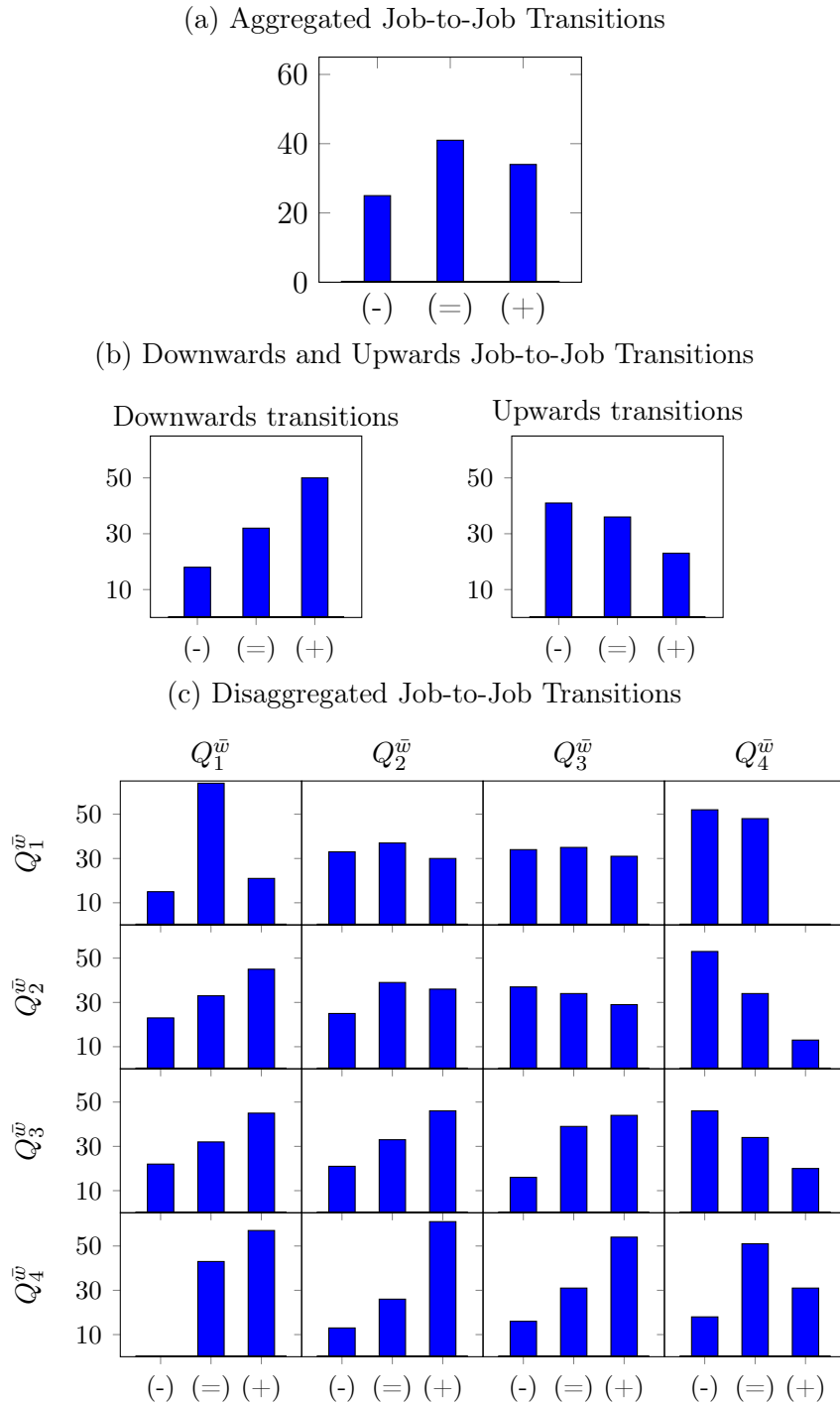
Note: Jobs are categorized based on the mean wage of co-workers. In both panels, wages are regression adjusted. Panel (a) shows the wage dynamics experienced by job movers, shown over 4 years. The indicated year zero is the workers' first year at the new job. Panel (b) shows the mean wage changes experienced in the move year by movers who transition between symmetric quartiles. Wage changes for workers moving to a higher quartile are on the x-axis, and for those moving to a lower quartile are on the y-axis. The dotted red line represent the case of perfect symmetry between wage gains and wage losses experienced by job movers.

Figure 2: Misclassification of Jobs



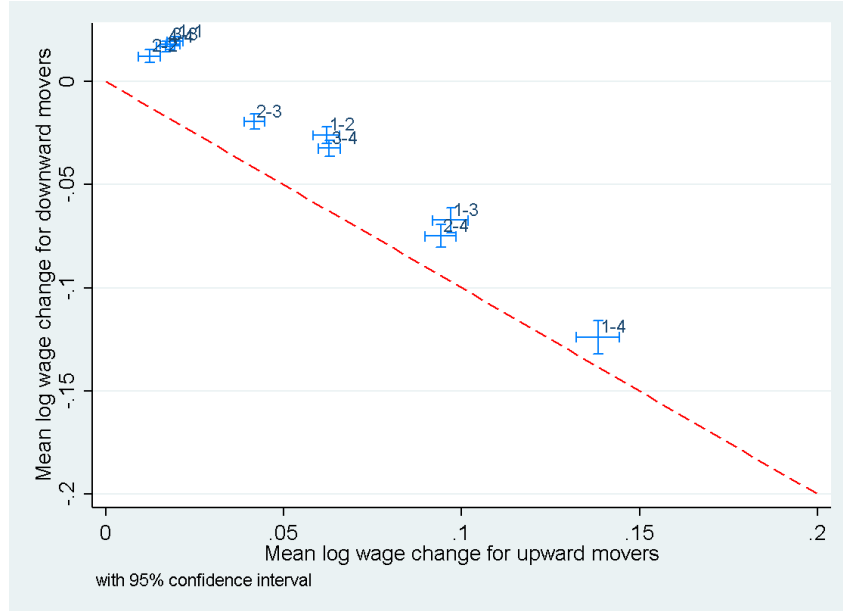
Note: The figure shows the percentage of jobs that are classified into a lower, identical or higher quartile when using the job classification based on the mean residual wage of co-workers, for each quartile of the distribution of mean wage of co-workers $Q_q^{\bar{w}}$ where $q = (1, 2, 3, 4)$.

Figure 3: Misclassification of Job-to-Job Transitions



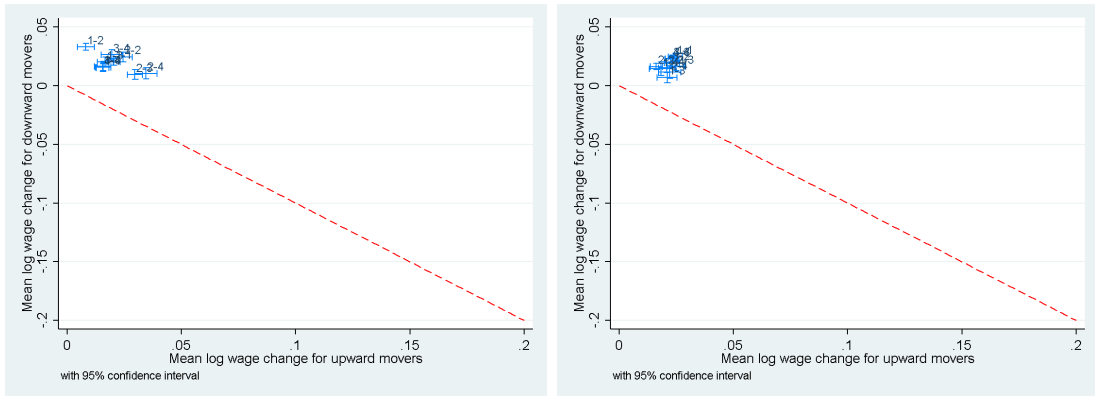
Notes: Panels (a) and (b) compare two classifications of job-to-job transitions. Following the first methodology, each job-to-job transition is classified based on the quartiles of the mean wage of co-workers for both the origin and destination job, while following the second methodology, each job-to-job transition is classified based on the quartiles of the mean residual wage of co-workers for both the origin and destination job. Panel (a) shows the percentage of all/upwards/downwards job-to-job transitions recorded using the first methodology that show: (-) a lower increase or bigger drop in quartile when using the second methodology, (=) an identical classification when using the second methodology, (+) a larger increase or smaller drop in quartile when using the second methodology. Panel (b) disaggregates the analysis for each job-to-job transition recorded using the first methodology (Q_q^w -rows- to Q_q^w -columns-, where $q=(1,2,3,4)$).

Figure 4: Residual wage Gains vs. Wage Losses for Job Movers



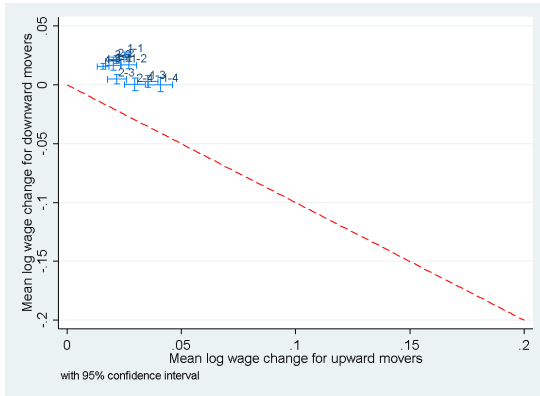
Note: Jobs are categorized based on the mean residual log wage of co-workers (the wage model includes dummies in occupation, industry, geographical location, gender, education, year, marital status, as well as tenure, a quadratic term in tenure, experience, a quadratic and cubic terms in experience, all fully interacted with education, and the interaction term between education and year dummy). Wage changes for job changers are also regression-adjusted. This graph shows mean log wage changes experienced in the move year by movers who transition between symmetric quartiles. Wage changes for workers moving to a higher quartile are on the x-axis, and for those moving to a lower quartile are on the y-axis. The dotted red line represent the case of perfect symmetry between wage gains and wage losses experienced by job movers.

Figure 5: Wage Gains vs. Wage Losses for Movers

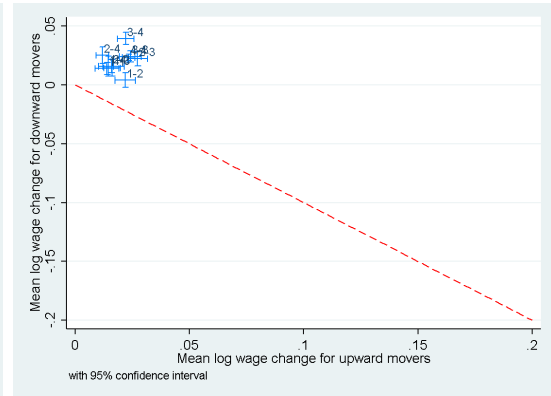


(a) Firm classification based on the firms' economic profits per employee

(b) Firm classification based on the firms' shareholder equity per employee



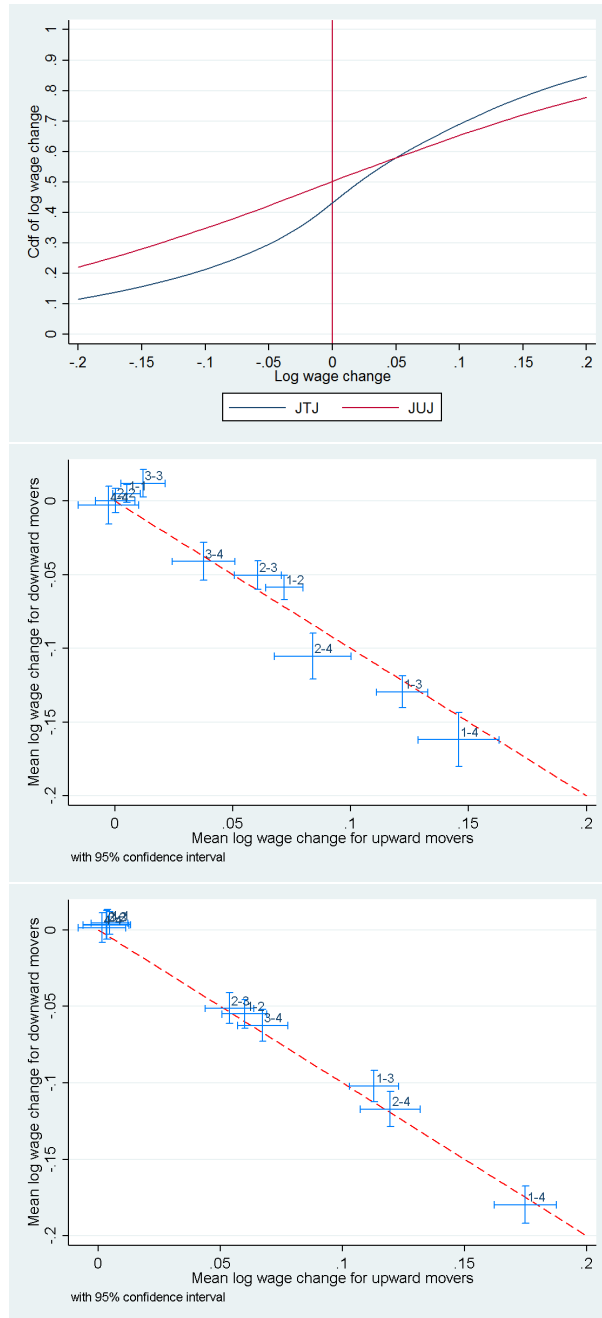
(c) Firm classification based on the firms' value added per employee



(d) Firm classification based on the firms' poaching index

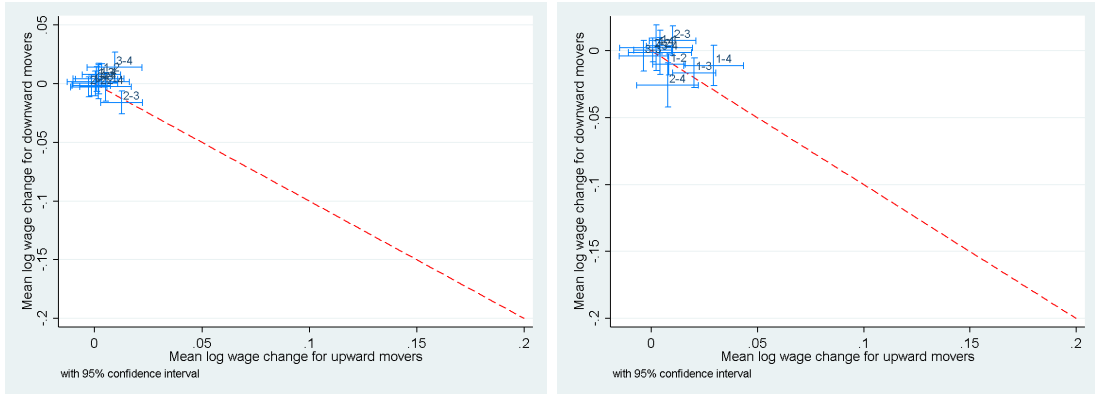
Note: Jobs are categorized based on (a) economic profit per employee, (b) firm equity per employee, (c) value added per employee, and (d) the poaching index. In all panels, wages are regression-adjusted residual log wages. All panels show mean log wage changes experienced in the move year by movers who transition between symmetric quartiles. Wage changes for workers moving to a higher quartile are on the x-axis, and for those moving to a lower quartile are on the y-axis. The dotted red line represent the case of perfect symmetry between wage gains and wage losses experienced by job movers.

Figure 6: Wage Gains vs. Wage Losses for Unemployment-to-Job Movers



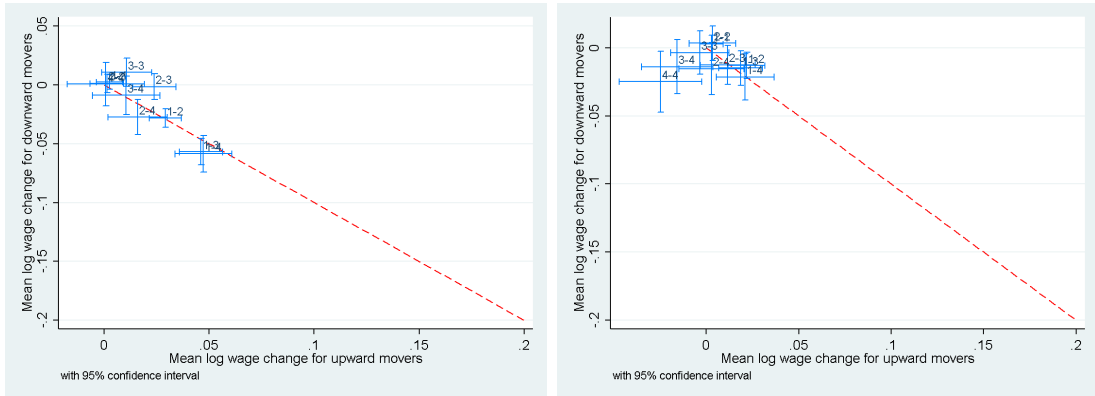
Note: Panel (a) shows the cdf on wage change for unemployment-to-job movers versus job-to-job movers. Panels (b) and (c) show the wage dynamics experienced by unemployment-to-job movers who transition symmetrically (from Q_q^w to $Q_{q'}^w$ vs. from $Q_{q'}^w$ to Q_q^w , where $q=(1,2,3,4)$). These two panels use the sub-sample of workers who experience at least two unemployment-to-job spells. The dotted red line represent the case of perfect symmetry between wage gains and wage losses experienced by job movers. Wage changes for job changers are regression-adjusted using the coefficients from a model of wage change for job stayers who remain in the same job (the wage model includes dummies in experience and education, and a quadratic term in experience interacted with education). In Panel (b), jobs are categorized based on the mean wage of co-workers. In Panel (c), jobs are categorized based on the mean residual wage of co-workers.

Figure 7: Wage Gains vs. Wage Losses for Unemployment-to-Job Movers



(a) Firm classification based on the firms' economic profits per employee

(b) Firm classification based on the firms' shareholder equity per employee

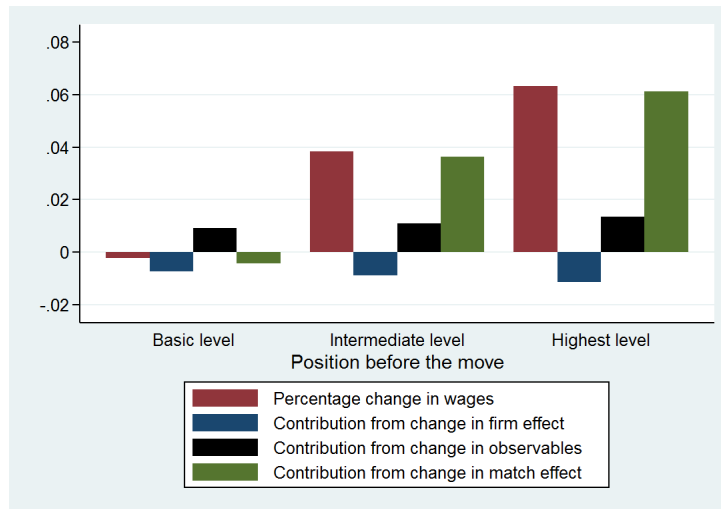


(c) Firm classification based on the firms' value added per employee

(d) Firm classification based on the firms' poaching index

Note: Sub-sample of workers who have at least two unemployment-to-job spells. Jobs are categorized based on (a) economic profit per employee, (b) equity per employee, (c) value-added per employee, (d) poaching index. In all panels, wage changes for job changers are regression-adjusted using the coefficients from a model of wage change for job stayers who remain in the same job (the wage model includes dummies in experience and education, and a quadratic term in experience interacted with education). All panels show the wage dynamics experienced by movers who transition symmetrically (from $Q_q^{\bar{w}}$ to $Q_{q'}^{\bar{w}}$ vs. from $Q_{q'}^{\bar{w}}$ to $Q_q^{\bar{w}}$, where $q=(1,2,3,4)$). The dotted red line represent the case of perfect symmetry between wage gains and wage losses experienced by job movers.

Figure 8: Expected Wage Change, Firm Effect and Match Effect by Occupation Level



Note: The change in wages (red bar) is decomposed into the contribution from the change in firm effect (blue bar), the change in match effect (green bar), and the change in observable characteristics (year dummies, quadratic and cubic terms in age, and their interactions with education). Job-to-job transitions are categorized based on the occupation level in the origin job (basic, intermediate, highest level of wage earner).

C Tables

	Mean	SD	Covariance comp.	Variance decomp.
Δ log wage	0.019	0.207	0.043	1
Δ firm fe	-0.008	0.151	0.004	0.088
Δ observables	0.010	0.023	0.000	0.003
Δ Match effect	0.020	0.219	0.028	0.658
Δ White noise	-0.004	0.104	0.011	0.251

Table 1: Job-to-Job movers: Variance decomposition

	Δ log wage	Δ firm fe	Δ observables	Δ composite err
Δ log wage	1	0.120	0.026	0.779
Δ firm fe	0.120	1	0.015	-0.522
Δ observables	0.026	0.015	1	-0.083
Δ composite err	0.779	-0.522	-0.083	1

Table 2: Job-to-Job movers: Correlation matrix of differences in wage components

	Firm FE decrease		Firm FE increase	
	Mean	SD	Mean	SD
Δ log wage	-0.000	0.203	0.041	0.210
Δ firm fe	-0.108	0.107	0.104	0.107
Δ observables	0.010	0.023	0.010	0.024
Δ composite err	0.098	0.199	-0.074	0.204

Table 3: Job-to-Job movers: stats conditional on direction of firm fixed effect