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

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Abstract

In this paper, we measure the relative contribution of firm effects versus match quality to the wage growth experienced by job changers. We provide evidence that standard two-way fixed-effect wage models can only explain a small share of the observed wage dynamics of job changers. We propose a novel strategy to estimate an additive model of wage changes that includes a worker-firm match effect. Using estimates from Danish linked worker-firm data, we find that 44% of the wage growth experienced by job-to-job movers is attributable to an improvement in the quality of the worker-firm match, and 66% of the variance of wage growth is explained by the variance of the change in match effects. These results suggest that job mobility plays an important role in correcting the initial misallocation of workers across firms.

Keywords: Job mobility; Fixed-effect wage models; Panel data models; Assortative matching

JEL classification: J62; J63; C23

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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. In Danish administrative data, job mobility is associated with an average yearly wage gain of two percent, around seven times higher than the average wage gain experienced by workers who remain in their current job in a typical year.[1]

The additive two-way fixed-effect wage model proposed by Abowd et al. (1999) has been used extensively to analyze the contributions of worker and firm fixed effects to wages, as well as to measure the extent of sorting between workers and firms. Some recent topics labor economists have studied by employing the two-way fixed-effect wage model include the assortativity of workers and firms (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). The two-way fixed-effect model is attractive because it is both easy to understand and to implement. As a rule, these papers find that the worker fixed effect is significantly more important than the firm effect in explaining variation in wage levels.[2]

In this paper we investigate the applicability of the two-way fixed-effect wage model to studying wage growth, rather than wages in level. When analyzing wage growth, permanent worker effects are irrelevant since they are differenced out. While firm effects could potentially explain wage growth as implied by the two-way fixed-effect wage model, we find that their contribution to the wage dynamics of workers moving between jobs is small. In our data, the variance of changes in firm fixed effects estimated from a standard two-way fixed-effect wage model explains only 17% of the variance in wage growth. Our findings are consistent with the literature which has found that most workers transition

[1] Topel and Ward (1992) and Eckstein et al. (2011), among others, document the importance of job-to-job transition in explaining wage growth. Topel and Ward (1992) estimate that around 30% of individual wage growth over the life-cycle can be explained by worker mobility across firms.

[2] For example, Card et al. (2013) find for German workers that the variance of worker fixed effects explains 50% - 60% of the overall variance in wages, while the variance in firm fixed effects explains only around 20% of the overall variance in wages.

between similar firms (Schmutte, 2015; Card et al., 2016).

Another implication of the two-way fixed-effect wage model for wage growth is the symmetry in wage gains and wage drops experienced by workers moving to better and worse firms. If the change in wages experienced by job changers were driven by firm effects, the wage gains experienced by workers transitioning to better firms would be of the same magnitude as the wage losses experienced by workers moving to worse firms. We develop symmetry tests that build on the ones proposed by Card et al. (2013) and Card et al. (2016). We find that, in the Danish data, wage dynamics feature clear asymmetric patterns. We relate this asymmetry in wage gains and wage drops experienced by workers transitioning to better or worse firms to the "compensation hypothesis", namely that workers moving to worse firms must be at least partly compensated by an improvement in match quality. Our results are robust to various proxies for firm type: average wage of co-workers, average residual wage of co-workers, poaching index as introduced by Bagger and Lentz (2018), or accounting measures such as profit, shareholder equity, and value added per worker.

The standard two-way fixed-effect wage model is thus unable to account for most of the wage growth experienced by workers moving between jobs. We argue that the underlying reason for this inappropriateness is related to the presence of match-specific effects on wages that trigger worker mobility. Hence, worker mobility is endogenous, a feature that invalidates the exogenous mobility assumption needed to consistently estimate two-way fixed-effect wage models. While other studies in the literature have found a meaningful contribution of match effects to wages (Woodcock (2008); Gruetter and Lalive (2009); Abowd and Schmutte (2015)), the major contribution of our paper is a novel estimation strategy that allows us to quantify the contribution of the match effect to wage growth. This estimation strategy hinges on a simple additive three-way fixed-effect wage model that appear better suited for understanding wage dynamics, while at the same time retaining the attractive simplicity and implementability of the two-way fixed-effect model. The third fixed effect is worker-firm match specific and is allowed to vary freely.[3]

Following a two-step estimation strategy, we find that 44% of the wage growth

[3]This additional match effect allows us to address the critique of the two-way fixed-effect model formulated by Eeckhout and Kircher (2011): because wages are non-linear in firm types, it is impossible to identify the sign of assortative matching using wage data alone (Law et al., 2016; Bagger and Lentz, 2018). Indeed, in our setting, a worker transitioning to a better firm might experience a wage drop if the positive effect on wages of moving to a better firm is offset by a negative change in match quality.

experienced by job-to-job movers is attributable to an increase in the match effect. We also carry out a variance decomposition exercise and we show that 66% of the variance of wage growth of job-to-job movers stems from the variance in match quality, while only 10% comes from the change in firm fixed effects.[4] This result clearly indicates that job mobility plays an important role in correcting the initial misallocation of workers across firms. We also find that the relative contribution of firm-specific versus match-specific wage premia varies over age and working position. Young workers appear to transition more to better matches, compared to workers above 45, a result that highlights the importance of job mobility at early stages of the workers' careers. Last, we observe that these improvements in match quality experienced by young workers who transition across firms are particularly large for managers and middle managers.

Our proposed two-step estimation strategy proceeds as follows. We start by observing that the mobility pattern of workers who experience spells of unemployment between jobs differs markedly 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 show symmetric wage growth patterns between workers transitioning to better and worse firms. These wage dynamics are consistent with the implications of the two-way fixed-effect wage model, and thus, are not consistent with the compensation hypothesis. We rationalize this result by arguing that, because the reservation wage of unemployed workers is low, the range of acceptable jobs is much larger for unemployed than it is for employed workers. Consequently, the unemployed might accept job offers that feature low match quality. Said differently, unemployed workers do not typically sort themselves into good matches. Using the exogenous mobility assumption on unemployment-to-job workers, we first 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. Next, we use these estimated firm fixed effects to decompose the wage growth experienced by job-to-job movers into the change in time-varying observables, the change in (estimated) firm-specific fixed effects, and the change in match-

[4]If we were instead to use the firm fixed effects estimated from the job-to-job mover sample (which we argue are inconsistent due to the compensation hypothesis), we would find that changes in firm fixed effects account for 21%, and changes in the match effect accounts for only 56% of the variance of wage growth. This strategy would have significantly overestimated the contribution of firm wage premia to wage dynamics, and underestimated the contribution of mismatch correction.

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

specific fixed effects.

Finally, we test the "compensation hypothesis" using our estimates. We find evidence 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. The change in firm fixed effects of movers is strongly negatively correlated with the change in their match quality. Second, we observe that the quality of the match improves on average for workers who transition to 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 is partially offset by an improvement in match quality.

Our results suggest that match effects play 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] Our focus however differs as our aim is to investigate the wage dynamics of job-to-job movers. By including match effects in a wage growth equation, we extend the analyses of Sørensen and Vejlin (2011), Sørensen and Vejlin (2013), and Woodcock (2015). While Sørensen and Vejlin (2011) propose 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 (2013) and Woodcock (2015) propose a wage regression model that includes a match effect but they impose the strong assumption that this match effect is orthogonal to the worker and firm fixed effects.[7] 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 next section briefly reviews the standard two-way fixed-effect wage model. We then discuss our data in Section 3. Section 4 tests the implications of the standard two-way fixed-effect wage model on the Danish register data. Section 5 presents a wage

[6]See Lise and Robin (2016), Bagger and Lentz (2018), and Sørensen and Vejlin (2013) among others.

[7]They also estimate a hybrid mixed effect model with worker, firm, and match fixed effects, which requires a weaker identification assumption than a standard random effect model.

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 6 concludes.

2 Background: Two-Way Fixed-Effect Wage Model

Because it will help us to fix notation and frame our discussion below, in this section we briefly review the wage equation of Abowd et al. (1999) (AKM) which decomposes log wage w_{it} of worker i in time t into additively separable worker and firm components:

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

where $x_{i,t}$ captures the time-varying effect of observed characteristics, 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, and $r_{i,t}$ is the error term. Following Card et al. (2013), $x_{i,t}$ 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:

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

The literature has named Equation (2) the exogenous mobility condition. 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.

3 Data

3.1 Matched Employer-Employee Data from the Danish Registry

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, and provide information on each worker's labor market status, labor market experience, sector, tenure, occupation, and estimated hourly wage for the job held in November.[8] Hourly wages are deflated using the 2000 Consumer Price Index. We merge in person-level administrative registers to obtain workers' demographic information: age, gender, education, marital status, and area of residence. We create an education measure that categorizes education levels into four groups: secondary and high school education, vocational and short cycle tertiary education, medium cycle tertiary education, and long cycle tertiary education. Over the period 2000-2010, our raw sample contains 40,732,616 observations. The sample selection is summarized in Table 1, Panel A. We delete all public sector observations (9,431,920 observations deleted) as well as observations pertaining to self-employment or secondary employment (11,666,103 observations deleted). We also discard observations with estimates of hourly wage considered to be unreliable for analysis (2,881,422 observations deleted).[9] We restrict our sample to all workers between 25 and 60 years old (2,212,532 observations deleted). We drop all observations with zero or missing hourly wage (57,615 observations deleted) or undisclosed establishment or firm identification numbers (828,006 observations deleted). We drop all observations that belong to the bottom or top percentile of the yearly wage distributions (270,161 observations deleted). Finally, we keep full-time workers only (3,048,939 observations deleted). Our full sample, containing 10,335,918 observations, brings together 1,800,844

[8]We use the Danish Integrated Database for Labor Market Research, i.e, the IDA register. Our sample ends in 2010, as this is the last year in which our wage variable (timelon) is available. We restrict our sample to 11 years not only because computation on a larger sample would be unwieldy for the large fixed effects models such as the AKM model we describe below, but also because we merge our data with accounting data that became available in 1999-2000. In Appendix A, we discuss how the IDA register relates to a more recent (beginning in 2008) alternative database, the E-Indkomst Register, which contains more comprehensive information about worker job spells and hours.

[9]We use the standard quality threshold of the wage quality variable tlonkval<50.

workers and 206,966 firms.

3.2 Firm-Level Accounting Data

Because some of our analyses rely on firm accounting data, we also 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 at the firm level (e.g., operating profits, 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] As shown in Table 1, Panel B, around three quarters of the full sample can be linked to accounting data. Nevertheless, all analyses that do not involve accounting data are run on the full sample (or the sub-samples described in Sections 3.3 and 3.5). Variables of profits per worker, value added per worker, and shareholder equity per worker are constructed by dividing operating profits, value added, and shareholder equity by full-time equivalent workers. We deflate these variables using the 2000 CPI.

Because the accounting data are available at the firm level, we aggregate establishment-level data at the firm level to bring the level of analysis to the firm rather than the establishment.[12].

3.3 Samples for Wage Level Analysis

Starting from the full sample, we create two sub-samples. The Job-to-Job (JtJ) sample includes only the employment spells of workers who have been hired directly from employment. Specifically, starting with the full sample, we only keep observations pertaining to the entire employment spells of workers who were employed in another firm

[10]The 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.

[11]Firms 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.

[12]Because 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 do not rely on accounting data.

in the November month preceding the start of their job and who did not experience any unemployment spell in the period between the preceding November month and the start of their job. The JtJ sample contains 7,237,995 worker-year observations, 1,336,249 workers, and 165,746 firms. In comparison, the Unemployment-to-Job (UJ) sample includes only the entire employment spells of workers who have been hired directly from unemployment or out of the labor force. Specifically, we restrict the sample to observations pertaining to employment spells of workers who were either unemployed or out-of the labor force in the November month preceding the start of their job, or unemployed at some point in time between the last November month and the start of their job. The UJ sample contains 2,364,318 worker-year observations, 692,530 workers, and 128,457 firms.[13] The JtJ sample, and the UJ sample are described in Table 2. Notice that, even though our samples cover the 2000-2010 period, we use pre-sample data from 1981 onwards to categorize employment spells that started before 2000 into either the JtJ or UJ sample.

3.4 Diagnostic Tests

Before analyzing wage growth, we run diagnostic tests on wages in level to insure the comparability with previous works of our sample selection and results on wage decomposition. We first estimate a two-way fixed effect wage equation as described by Equation (1). Our baseline set of time-varying observables includes year dummies, a quadratic and cubic term in age, and the interaction of these variables with our education measure. We apply this AKM wage decomposition to the full sample, the JtJ sample, and the UJ sample. As explained by Abowd et al. (1999), inter-firm mobility is critical to the estimation of the wage model. Table 3 shows that 59% of all workers were employed in at least two firms over the period 2000-2010 and that 40% were employed in a single but connected firm, i.e. a firm that hired a worker who was previously working in another firm or that was left by a worker who moved to another firm. All in all, 99% of the full sample is used for the estimation of firm fixed effects. In comparison, 98% and 89% of

[13] Notice that, even though the discrepancy is small, the JtJ and the UJ samples do not sum up to the full sample. The reason is that the JtJ sub-sample does not include workers who were reported as being neither unemployed nor out of the labor force in the November month previous to their transition to their new firm while at the same time not appearing as employed in the origin firm in that November month. This might happen for several reasons: if a worker was on leave from the origin firm in the November month previous to the transition and did not receive any wage, if we observe a missing wage in the origin firm in the November month previous to the transition, or in case of an undisclosed identification number for the origin firm in the November month previous to the transition.

the JtJ and UJ samples, respectively, are used for the estimation.

We measure the relative contribution of worker and firm effects to the variance of wages. As shown in Table 4, most of the wage variance is explained by worker effects. Indeed, in the full sample, the contribution of worker heterogeneity to wage dispersion is 78%, while firm effects contribute up to 12%. The relative contribution of worker and firm effects is robust to the choice of time-varying observables included in the two-way fixed effect model. Indeed, the share of wage variance explained by worker and firm effects is similar to the ones that we obtain when the set of time-varying observables includes year dummies, experience and a quadratic term in experience (see Appendix Table 2).

Comparing the JtJ sample to the UJ sample, the relative contribution of worker effects to the variance of wages is considerably lower for the set of workers who were hired from unemployment, while time-varying observables and, to a certain extent, firm effects explain more of the dispersion in wages for this sub-sample of workers. These discrepancies suggest that wage determination might differ according to the workers' labor market status at the time of hiring.

Moreover, as shown in Table 5, the ranking of firms differs significantly according to the sample used to estimate firm fixed effects. While the correlation between the firm fixed effects obtained from the full sample and the ones obtained from the JtJ sample is relatively high (roughly 0.80 if employment-weighted, 0.63 if non-weighted) due to the large overlap between the samples, the correlation between the firm fixed effects obtained from the full sample and the ones obtained from the JUJ sample is much lower (about 0.51 if employment-weighted, 0.35 if non-weighted). Comparing the mutually-exclusive JtJ and UJ samples, we obtain correlations of 0.34 (employment-weighted) and 0.12 (non-weighted) between the firm fixed effect estimates.

3.5 Identifying Worker Mobility

To focus on the wage dynamics experienced by workers who transition between firms, we also construct a sample of job transitions. Starting from the full sample described in Section 3.3, we keep only employment spells of workers who had at least two employers over the 2000-2010 period. Next, we keep the first wage observation of each employment spell,[14] and we measure wage change as the log-difference between the real hourly wage

[14]Focusing on the first observation per match allows us to use comparable hourly wage estimates to construct our wage change measure. Indeed, were starting salary measures noisy (Lund and Vejlin,

received at the destination firm and the real hourly wage received at the origin firm. A job transition is identified by a non-missing value of wage change. Our full mobility sample consists of 1,099,145 job transitions. These job transitions include both job-to-job transitions as well as transitions via unemployment.

To compare various types of inter-firm mobility, we construct two additional sub-samples. First, starting from the full mobility sample, we retain only direct job-to-job transitions. Specifically, we keep job transitions of workers who are observed to be employed in a firm in some November month and in another firm in the next November month, and who did not experience any period of unemployment in between. For this sample, we also measure wage change as the log-difference between the first real hourly wage of the employment spell at the destination firm and the first real hourly wage of the employment spell at the origin firm. Our resulting JtJ mobility sample contains 526,756 workers and 93,621 firms. Second, starting from the full mobility sample, we keep only job transitions that involved a period of unemployment. Specifically, we keep job transitions of workers who were either unemployed in the November month between two employment spells in different firms, and workers who were employed in a firm in some November month, employed in another firm in the next November month, and who experienced a period of unemployment in between. For this sample also, we measure wage change as the log-difference between the first real hourly wage of the employment spell at the destination firm and the first real hourly wage of the employment spell at the origin firm. A job transition is identified by a non-missing value of wage change. Our resulting Job-Unemployment-Job (JUJ) mobility sample contains 181,699 workers and 64,802 firms. The JtJ mobility sample and the JUJ mobility sample are therefore two mutually exclusive sub-samples of the full mobility sample. The three mobility samples are described in Table 6.

2015), the noise would affect similarly the origin and destination firm wages, leaving our measure of wage change unaffected. Alternatively, we could have used the t-2 and t+1 wages to construct our wage change measure, but such a strategy would reduce the sample size significantly as we would need to focus on workers who stayed at least two years in the origin firm and two years in the destination firm. This decrease in sample size would affect our JUJ mobility sample dramatically. Our results are robust to measuring wage changes using wages received in the November month before and after the transition.

4 Testing the Implications of Two-Way Fixed-Effect Wage Models for Wage Growth

4.1 Testable Implications for the Wage Growth of Job Changers

4.1.1 Importance of Observables and Firm Effects

Because the worker fixed effect is constant over time, the AKM wage decomposition implies that the wage growth experienced by job movers is explained by the change in time-varying observable characteristics and by the change in firm fixed effect, as shown by differencing Equation (1):

$$w_{i,t} - w_{i,t-1} = \psi_{J(i,t)} - \psi_{J(i,t-1)} + x'_{i,t}\beta - x'_{i,t-1}\beta + r_{i,t} - r_{i,t-1} \tag{3}$$

which, in expectations, becomes:

$$\mathbb{E}[\Delta w_{i,t}] = \mathbb{E}[\Delta \psi_{J(i,t)}] + \mathbb{E}[\Delta x'_{i,t}\beta] \tag{4}$$

Because the error term is mean zero, the change in the error term is zero in expectation.

When applying the AKM decomposition to explain wage dynamics, we predict that the wage gain that job changers experience on average is attributable to either an improvement of observable characteristics and/or to workers transitioning to higher-paying firms, i.e. firms with higher firm fixed effects.

4.1.2 Symmetry in Wage Changes Associated with Upwards and Downwards Transitions

The AKM wage decomposition has testable implications regarding the symmetry in wage changes experienced by workers moving between high-paying and low-paying firms. This observation was first made by Card et al. (2013) and Card et al. (2016). To illustrate these implications of symmetry, first suppose we can categorize firms into high- and low-paying quartiles. We discuss the numerous ways to empirically rank firms in Section 4.2.3. For now, let ψ_k be the expected wage level associated with a Quartile-k job. Using the two-way fixed-effect wage model, Equation (3), we can write down the wage change expected by a worker i moving from a Quartile-1 job to a Quartile-4 job, and by a worker

k moving in opposite direction:

$$\mathbb{E}\left[\Delta w_{i,t}|Q_1 \to Q_4\right] = \psi_4 - \psi_1 + \mathbb{E}\left[\Delta x'_{i,t}\beta|Q_1 \to Q_4\right] \tag{5}$$

$$\mathbb{E}\left[\Delta w_{k,t}|Q_4 \to Q_1\right] = \psi_1 - \psi_4 + \mathbb{E}\left[\Delta x'_{k,t}\beta|Q_4 \to Q_1\right] \tag{6}$$

Changes in error term r are absent because they are equal to zero in expectation. The AKM wage decomposition therefore implies that, up to the time-varying observables x_{it}, the expected wage gains experienced by workers moving upwards, e.g. from a Quartile-1 job to a Quartile-4 job, should be exactly the same as the wage losses expected by workers making the opposite transition, e.g. from a Quartile-4 job to a Quartile-1 job. Said differently, the AKM wage decomposition implies that, after controlling for time-varying observables, the average wage gain experienced by upwards movers is symmetric with the average wage drop experienced by downwards movers. Also, the AKM wage decomposition predicts that job changers transitioning between firms that belong to identical quartiles do not experience any change in their residual wage.

4.2 Empirical Analysis of Wage Growth

4.2.1 Measuring the Relative Importance of Observables and Firm Effects

We apply the wage change decomposition embodied by Equation (4) to the Danish employer-employee data described in Section 3.5. We first analyze all job transitions included in the full mobility sample, i.e. both direct job-to-job transitions and job transitions involving a spell of unemployment. Our baseline set of time-varying observables includes year dummies, a quadratic and cubic term in age, and the interaction of these three variables with our education measure. Regarding the firm fixed effect, we propose three set of estimates: i) the firm fixed effects obtained by estimating a two-way fixed-effect model on the full sample (entire employment spells of all workers); ii) the firm fixed effects obtained by estimating a two-way fixed-effect model on the JtJ sample (entire employment spells of workers who were hired directly from employment); iii) the firm fixed effects obtained by estimating a two-way fixed-effect model on the UJ sample (entire employment spells of workers who were hired from unemployment). Indeed, as shown in Table 5, correlations between firm fixed-effects estimated on the UJ sample and those estimated on other samples are small, a result that suggests our wage change decomposition results might be affected by the choice of firm effect estimates.

Table 7 reports the empirical wage change decompositions. Focusing first on the full mobility sample, we find that, independently of the set of estimated firm fixed effects, firm effects contribute little to the wage gain experienced by job changers. The share of wage growth attributable to workers moving to better firms is close to zero. In comparison, changes in time-varying observables explain between 75% and 78% of the wage gain experienced by job changers. Consequently, the residuals are substantial, ranging between 20% and 32%.

The failure of firm effects to explain wage growth can be drawn closer to the results presented by Card et al. (2013) and Card et al. (2016) who show that most worker mobility occurs between firms that belong to the same quartile. In Germany over the period 2002-2009, 57% of all male job transitions took place between firms belonging to the same quartile, while transitions to higher-quartile firms represented only 23% of all male job transitions. In Portugal over the same period, the figures are 48% and 30% , respectively. By designing a probability matrix characterizing transitions between origin and destination firm deciles, Schmutte (2015) reaches a similar conclusion since the highest frequencies are mostly located on the diagonal of the probability matrix.

To compare the different types of worker mobility, we apply the same wage change decomposition to our sample of direct job-to-job transitions (JtJ mobility sample) and of job transitions involving a spell of unemployment (JUJ mobility sample). First, the change in wages is positive for all job changers, though job-to-job changers benefit much more from firm mobility than workers who move via unemployment: around 8% wage growth for the former vs. 2.2%-3% for the latter. Second, the wage growth experienced by job changers cannot be explained by workers moving to better firms, as changes in firm effects contributes little to the wage growth they experience. Third, there is a clear distinction between the two panels in the size of the unexplained fraction of wage growth. While 38% to 51% of the wage growth experienced by job-to-job movers remains unexplained by firm effects and time-varying observables, the residuals are negative for job changers who experienced a spell of unemployment. These negative residuals obtained on the JUJ sample may be explained by the negative impact of unemployment on earnings due to skill erosion, worker discouragement, and employer discrimination (Krueger et al., 2014), which are not included in the set of control variables because they are mostly unobservable.[15]

In Table 8, we report the empirical variance decomposition of wage changes. Consistent with the mean decomposition exercise, these results also highlight the difference in wage dynamics between job-to-job movers and unemployment-to-job workers. When using the firm fixed effects estimated on the full sample, we observe that the variance of firm fixed effects explains 21% of the variance of wage changes for job-to-job movers, while the residual contributes to 75% of this variance. For unemployment-to-job workers, the relative contribution to the variance in wage changes is 28% for firm fixed effects, and 70% fot the residual. The findings also confirm that the variance decomposition results are sensitive to the choice of sample on which to perform the two-way fixed-effect wage decomposition.

4.2.2 Testing the Symmetry in Wage Changes Associated with Upwards and Downwards Transitions

Our first task is to rank firms and categorize them into types. In this section, we base our methodology on Card et al. (2013) and Card et al. (2016) who originally developed the symmetry test to validate fixed-effect wage models, and we conduct robustness checks in Section 4.2.3. We use mean co-workers wages as a proxy for firm wage premia. Hence, we classify origin and destination jobs based on the quartile of the mean wage of co-workers in that year. The baseline wage measure that we use is the residual wage after controlling for year dummies, a quadratic and cubic term in age, and their interactions with our education measure. Moreover, the 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 residual wages across worker observations. With four types of job to transition from, and four types of job to transition to, we obtain sixteen job transition categories.[16]

Because Equations (5) and (6) contain the change in time-varying observables, we first control for our baseline set of observables before to measure the wage change experienced by job changers. The expected wage gains and drops experienced by all workers moving between firms that belong to identical or different quartiles are shown in Figure 1, Panel (a), and reported in Table 9. The red dashed line represents the case of perfect symmetry

[15] On the effects of unemployment on earnings, see also Jacobson et al. (1993) who analyze the earning losses of displaced workers using U.S. administrative data.

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

between wage gains and drops of job changers moving in opposite directions. The dynamics of wage residuals show that wage gains experienced by upward movers largely exceed wage losses experienced by downward movers. When transiting to a firm that is one quartile higher, workers experience an average wage gain of 9.2%, while workers transiting in the opposite direction face an average wage drop of 2.0%, only 22% of the wage gain. When workers move two quartiles up, their average wage increases by 14.2%, whereas when moving down, they experience an average wage drop of 8.5%. Finally, when moving up and down three quartiles, the average wage gain is 21.3%, while the average wage drop is 17.2%. Moreover, job changers transitioning between firms that belong to the same quartile experience on average a 3.7% wage increase. The asymmetry in wage gains and drops is even more striking when we focus on job-to-job movers (Panel (b)). We observe that the wage change plots of movers who transition in opposite directions lie further away from the red dotted line that represents the case of perfect symmetry in wage changes.[17]

In contrast, when restricting our analysis to the sub-sample of job changers who experienced a spell of unemployment, in Figure 1, Panel (c), and Table 9, we find that the wage increase expected from moving to a higher-quartile firm is roughly of the same magnitude as the wage decrease expected from moving to a lower-quartile firm. When transiting to a firm that is one quartile higher, workers experience an average wage gain of 6.7%, while workers transiting in the opposite direction face an average wage drop of 7.1%. When workers move two quartiles up, their average wage increases by 13.5%, whereas when moving down, they experience an average wage drop of 14.8%. Finally, when moving up and down three quartiles, the average wage gain is 19.3%, while the average wage drop is 23.8%. Moreover, job changers transitioning between firms that belong to the same quartile experience on average a 0.3% wage increase.

4.2.3 Alternative Symmetry Tests

We first show results obtained using alternative measures of wage change. However, the main challenge in performing the symmetry test is in choosing the proxy for the firm

[17] We do not expect wage changes to be positive in both directions, even on the sub-sample of JtJ movers. Indeed, wage drops can occur both voluntarily, due to unobserved compensating differentials, and involuntarily, with workers who have lost their jobs without experiencing any unemployment spell. 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.

fixed effect. Therefore in this Section, we also confirm the robustness of the conclusions drawn in Section 4.2.2 to alternative choices of firm ranking. In particular, we propose to categorize firms based on a new measure of co-workers wages, accounting data (operating profits per employee, shareholder equity per employee, added value per employee), the poaching index proposed by Bagger and Lentz (2018), and the AKM firm fixed effects obtained from different samples.

Alternative Measures of Wage Change

To ease the comparison with Card et al. (2013) and Card et al. (2016), Figure 2 displays the results that we obtain using alternative measures of wage changes on the sample of job-to-job movers (JtJ mobility sample). In Panel (a), we show the change in raw wages experienced by job changers, while in Panel (b), wages are regression-adjusted using an alternative set of controls (education and year dummies, experience and experience squared) prior to measure wage changes. In Panel (c), we restrict the sample to job transitions that occur within a year: workers are observed for the first time in a firm in a given year, and observed for the first time in another firm in the next year, without any period of unemployment in between. Wages of job changers are regression-adjusted using our baseline set of controls (year dummies, a quadratic and cubic term in age, and their interactions with our education measure) prior to measure wage changes. In Panel (d), we analyze the same observations as in Panel (c), but we follow the methodology proposed by Card et al. (2016) and adjust wage changes using coefficients obtained from a model estimated on job stayers.

In line with Card et al. (2013) and Card et al. (2016), when using raw wage changes, job changers moving to higher-quartile firms experience wage gains that are much larger than the wage drops experienced by job changers moving in opposite directions. A comparison with Figure 1, Panel (b), indicates that, as expected, the distance from symmetry line is partly explained by the fact that changes in observable characteristics trigger a larger wage increase for workers transitioning to better firms. Interestingly, when analyzing regression-adjusted wage changes, we still obtain large asymmetries in wage gains and drops for workers moving in opposite quartiles, a result that is at odds with Card et al. (2016). As discussed in Appendix B, we argue that this discrepancy might be due to differences in the implementation of the adjustment methodology.[18]

[18] In Appendix B, we also detail how our results compare to the methodology based on adjusted wage

Figure 3 shows the results obtained when using these alternative measures of wage change on the sample of job changers hired from unemployment. We again find that the wage growth patterns of JUJ movers contrast with those of JtJ movers. Independently of the measure used, for workers hired from unemployment, the wage gain of workers transitioning to better firms is similar to the wage drop experienced by workers transitioning to worse firms.

Alternative Measures of Mean Co-Worker Wages

In this section we discuss several further ways to rank firms. One possibility is to categorize jobs based on the mean raw wage of co-workers instead of using the mean residual wage of co-workers. Figure 4 shows patterns of wage dynamics that are very similar to the ones obtained when applying our baseline method to categorize firms, shown in Figure 1.

The goal of our firm rankings are to approximate the theoretical firm fixed effect $\psi_{J(i,t)}$. Equation (1) relates the true fixed effect ψ at a particular job $J(i,t)$ to mean co-worker wages as follows:

$$\bar{w}_{-it} = \psi_{J(i,t)} + \bar{\theta}_{iJ(i,t)} \tag{7}$$

Here $\bar{w}_{-it}$ represents the average wages of worker i's coworkers, $\bar{\theta}_{iJ(i,t)} = \frac{1}{N_{J(i,t)}} \sum_{k=-i}(\alpha_k + x_{kt}\beta)$. Without strong monotonicity assumptions, our ranking of firms based on co-worker wages may not be the same as a ranking based on true firm job effects. We can partially alleviate this problem by controlling for additional time-varying characteristics (experience and a quadratic term in experience, interacted with our education measure) as well as time-invariant characteristics included in worker effects α (gender, occupation, marital status, area of residence). However, because $\bar{\theta}_{iJ(i,t)}$ also includes unobservable worker characteristics, this method is only a partial correction.

As shown in Figure 5, our results are robust to using this extended set of controls to measure mean residual co-worker wages. Indeed, as discussed above, we believe there are theoretical reasons to prefer this method of ranking jobs to the baseline. We keep the baseline as is, however, for comparability with Card et al. (2013) and Card et al. (2016).

Alternative Firm Classifications Using Accounting Data and Poaching

changes as implemented in Card et al. (2013).

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An alternative to using wage data to classify firms is to make use of the firm dimension of the matched employer-employee database. As explained in Section 3.2, we draw information on each firm's profit per employee, value added per employee, and shareholder equity per employee from the General Enterprise Statistics dataset. We use each of these three proxies of firm productivity and classify firms into quartiles based on the distribution of each of the measures. Results for job-to-job movers are shown in Figure 6, Panels (a), (b), and (c). We find that workers moving to higher- and lower-quartile firms both experience wage growth of around 5%, although growth is slightly higher for workers moving to better firms.

Another symmetry test relies on the use of the poaching index, introduced by Bagger and Lentz (2018). The idea is that more productive firms are able to poach workers away from less productive firms. The poaching index of a firm is defined as the fraction of the new hires at that firm who are directly poached from other firms, over the period 2000-2010. We expect this measure to increase with a firm's productivity.[19] We restrict our dataset to firms that count more than 13 new hires over the period, from which at least one is hired from unemployment. Shown in Figure 6, Panel (d), the results indicate that job-to-job movers see their wage increase after the job transition, independently of the type of transition that they experience.

The proximity of the wage change plots in Figure 6 also reveals a weak relationship between wages and the level of productivity of firms. This weak relationship is confirmed by comparing the magnitudes of the wage growth experienced by JtJ workers moving across firms that belong to different quartiles based on accounting data to the ones observed when using the mean residual wage of co-workers to classify firms (see Figure 1, Panel (b)). It emerges that firms that appear more productive based on accounting data do not necessarily pay higher wage premia. A possible explanation is the presence

[19] We denote by N_J^{JtJ} the number of firm J's hires that were hired from employment, over the period 2000-2010, and N_J^{UJ} the number of firm J's hires that were hired from unemployment, over the same period. Formally, firm J's poaching index, π_J, is measured as follows:

$$\pi_J = \frac{N_J^{JtJ}}{N_J^{JtJ} + N_J^{UJ}} \tag{8}$$

Using a on-the-job search model with endogenous search intensity and wage setting through bargaining, with heterogeneous employers competing for heterogeneous workers, Bagger and Lentz (2018) show that the poaching index is monotonically increasing in the firm's productivity index.

of match effects that partly offset the firm fixed effects. We will discuss this point in Section 5.1.

In Figure 7, we report the wage dynamics experienced by job changers who transited via unemployment. While the patterns clearly differ from the ones obtained on the JtJ mobility sample, they are noisy and we do not see any clear asymmetry between wage gains and wage drops.

Alternative Firm Classifications Using AKM Firm Fixed Effects

Even though the symmetry test was initially designed to validate fixed-effect wage models and therefore used an ex-ante proxy for the firm wage premium rather than employing the AKM firm fixed effects themselves, we also perform the symmetry test classifying firms based on the AKM firm effect estimates. As shown in Figure 8 (for job-to-job movers), in Figure 9 (for workers transitioning via unemployment) and in Table 10, independently of the sample used to fit the two-way fixed-effect wage decomposition and estimate firm fixed effects (full sample, the JtJ sample, and the UJ sample), the conclusions remain unchanged: job-to-job movers who transition to higher-quartile firms experience a wage gain that is much larger than the wage drop experienced by workers moving in opposite direction, while unemployment-to-job workers who transition to higher-quartile firms experience a wage gain that is of the same magnitude as the wage drop experienced by workers moving in opposite direction.

4.2.4 Formal Symmetry Tests

In this section, we formally test the symmetry of log wage changes for workers moving between firm categories. That is, we test that the absolute value of mean log wage changes for workers moving from Quartile q to Quartile q' are equal to the absolute value of mean log wage changes for workers moving from Quartile q' to Quartile q. We perform unpaired t-tests for equality. In particular, for each pair of firm quartiles, we regress wage changes for job changers transitioning between those quartile pairs on a constant and a dummy for moving from the lower to the higher quartile. If wage changes when moving downwards are smaller in absolute value than wage changes experienced when moving upwards, we expect this dummy to have a positive sign. If this dummy is statistically significant, we reject the null hypothesis of symmetry.[20]

In Table 11 we report the t-statistic for different cuts of our data, ranking firms into quartiles using our baseline methodology. The top panel contains results for the full sample of workers moving between firm categories. The middle panel focuses only on workers who moved directly from one job to another, and the bottom panel contains only workers who experienced a spell of unemployment before finding a new job. Each row represents a different firm quartile pair. This table uses our baseline categorization of firms using residual wages of coworkers. The second column contains mean log wages for workers moving to a higher firm quartile added to mean log wages for workers moving to a lower firm quartile. The next column contains our sample size for each row. The final three columns are t-statistics: first for standard errors with no clustering, then standard errors clustered by year, and finally standard errors clustered by age. We present three different standard error calculations for transparency, but we believe that the results clustered by year are most appropriate, as the business cycle strongly affects the labor market. For example, we have an average log wage change of 0.052 for job changers in 2008, while this figure drops to 0.015 in 2010. We also have nearly twice as many JUJ observations in 2010 (32,607) as we have in 2008 (17,522).

The results for all movers, and especially the results for job-to-job movers show clear asymmetric patterns. Wages changes experienced by workers moving to higher-quartile firms are larger than wage drops for workers moving to lower-quartile firms. T-statistics are large enough that we reject the symmetry hypothesis for any standard critical value. Results for workers who experience a spell of unemployment are markedly different. First of all, t-statistics are much smaller in absolute value. For both our clustering strategies, only three out of six firm quartile categories are statistically significant at the one-percent critical threshold (2.32). While there is more rejection if we do not cluster our standard errors, we find that, even in this case, t-statistics are an order of magnitude smaller than those applying to job-to-job movers. Part of this difference is due to a smaller sample size. Around three times as many movers move directly to a new job as go through a spell of unemployment. However, even if we divide the t-statistics obtained on the sample of job-to-job movers by $\sqrt{3}$, we still strongly reject the symmetry hypothesis for all clustering strategies and quartile pairs. Another difference is that workers transitioning

[20] We use Stata's "regress" function rather than the "ttest" function, because "ttest" does not allow us to cluster standard errors.

via unemployment mostly experience wage changes that are larger in absolute value when moving to lower-quartile firms than when moving to higher-quartile firms, although not always significantly so. Since we argue that these deviations from symmetry are mostly noise, we do not further interpret their directions.[21]

Summarizing the first section of our paper, the symmetry hypothesis is strongly rejected for both the full mobility sample and the JtJ sample, under a wide variety of specifications. Moreover, observables and firm effects appear to explain only a fraction of the wage growth of job-to-job movers. Consequently, we argue that the two-way fixed-effect wage model, Equation (1), does not provide a useful setting for analyzing the wage growth experienced by job-to-job movers, while it provides a reasonable characterization of wage growth for workers transitioning via unemployment. Our findings call for the inclusion of a match effect in the wage model applied to job-to-job movers. In the next section, we propose an estimation strategy allowing us to decompose the wage growth of job-to-job movers into changes in observables, changes in firm effects, and changes in match effects, without assuming that the match effect is conditionally mean zero, i.e. that mobility is exogenous.

5 Quantifying the Match Effect on Wage Growth

5.1 Endogenous Mobility of Job-to-Job Movers

As we touched upon in Section 2, the main threat to validity of two-way fixed-effect wage models relates to the sorting of workers to firms based on the permanent match-specific component included in the error term. Formally, endogenous mobility calls for the inclusion of a match effect ξ that extends the wage model to a three-way fixed-effect wage model:[22]

$$w_{i,t} = \alpha_i + \psi_{J(i,t)} + \xi_{iJ(i,t)} + x'_{i,t}\beta + \epsilon_{i,t} \tag{9}$$

[21] Results we present in this section are based on the baseline Figure 1 symmetry test. We get less statistical significance for JUJ movers under alternative firm classification strategies. In particular, we include our preferred firm classification strategy – mean coworker wages adjusted with additional controls – in Table 12. With this classification strategy, none of the JUJ category pairs are statistically significant at the 1% level when clustering by year.

[22] As discussed above in Section 2, we abstract here from idiosyncratic time-varying worker effects η and time-varying firm effects ς.

where $\xi_{iJ(i,t)}$ is the contribution to the wage of the productive characteristics of the match between worker i and firm $J(i,t)$ employing worker i in time t. Differencing Equation (9) we obtain:

$$\Delta w_{i,t} = \Delta\psi_{J(i,t)} + \Delta\xi_{iJ(i,t)} + \Delta x'_{i,t}\beta + \Delta\epsilon_{i,t} \tag{10}$$

and taking expectations leads to:

$$\mathbb{E}[\Delta w_{i,t}] = \mathbb{E}[\Delta\psi_{J(i,t)}] + \mathbb{E}[\Delta\xi_{i,J(i,t)}] + \mathbb{E}[\Delta x'_{i,t}\beta] + \mathbb{E}[\Delta\epsilon_{i,t}] \tag{11}$$

where the change in the error term is zero in expectation.

Before discussing the implications of this three-way fixed-effect wage model, it is important to understand how we can interpret these fixed effects. Up to the time-varying observables, we interpret Equation (9) as a decomposition of wages into a worker effect, α_i, a firm effect, $\psi_{J(i,t)}$, and a match effect $\xi_{i,J(i,t)}$. Consistently with Eeckhout and Kircher (2011), this decomposition allows for a non-monotonicity of wages in firm type. Indeed, this framework includes the case in which workers are not necessarily better off matching with a higher-type firm. If a low-type worker, transitioning to a high-type firm, must compensate the new firm for not matching with a higher-type worker, the job transition might involve a wage cut. In this case, Equation (9) will capture a positive change in firm effect, as the worker transitions to a more productive firm, and a negative change in match effect. The resulting change in wages will be negative if the deterioration in match quality offsets the positive change in firm productivity. We can also draw a parallel between the matching wage model, Equation (11), and the optimal allocation concept introduced by Eeckhout and Kircher (2011). As long as a worker did not reach her optimal firm, a transition to a more productive firm will come together with an improvement in the quality of the match. Both $\Delta\psi_{J(i,t)}$ and $\Delta\xi_{i,J(i,t)}$ are positive, and the wage growth is positive. However, past the worker's optimal firm, a transition to a more productive firm will come together with a deterioration in the quality of the match. While $\Delta\psi_{J(i,t)}$ is positive, $\Delta\xi_{i,J(i,t)}$ is negative, partly or over-compensating the positive change in firm productivity.

In Section 4.2.1, we documented the failure of observables and fixed effects alone to account for the wage growth experienced by job-to-job changers. Indeed, for this category of job changers, the change in observables and firm effects contributed between 49% and 62% of the average wage growth, depending on the choice of firm effect estimates (Table

7). These results brought us to question the validity of two-way fixed-effect wage models to explain the wage dynamics of job-to-job movers. In comparison, the wage model extended to allow for worker-firm match effects enables us to interpret the residuals presented in Table 7.

Moreover, with the addition of a worker-firm match effect which is allowed to be different from zero in expectation, the symmetry between wage gains and drops experienced by job-to-job changers moving in opposite directions is broken. To illustrate the link between endogenous mobility and wage growth patterns, we consider Equation (11) for both job changers moving from a Quartile-1 job to a Quartile-4 job and job changers moving in opposite direction:

$$\mathbb{E}\left[\Delta w_{i,t}|Q_1 \to Q_4\right] = \psi_4 - \psi_1 + \mathbb{E}\left[\Delta\xi_{i,J(i,t)}|Q_1 \to Q_4\right] + \mathbb{E}\left[\Delta x'_{i,t}\beta|Q_4 \to Q_1\right] \tag{12}$$

$$\mathbb{E}\left[\Delta w_{k,t}|Q_4 \to Q_1\right] = \psi_1 - \psi_4 + \mathbb{E}\left[\Delta\xi_{k,J(k,t)}|Q_4 \to Q_1\right] + \mathbb{E}\left[\Delta x'_{k,t}\beta|Q_4 \to Q_1\right] \tag{13}$$

If the exogenous mobility condition, Equation (2), holds, then by the law of iterated expectations, the expected change in the match quality is zero, as captured by Equations (5) and (6). Now, suppose that exogenous mobility is violated by workers moving conditional on match quality. If all moves were voluntary, 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, e.g., moving from a Quartile-4 job to a Quartile-1 job, on the principle that the low-wage job must be a good match for this worker, e.g., $\Delta\xi_{k,J(k,t)}|Q_4 \to Q_1 > 0$. This match effect in wages then compensates the worker for the low firm type and triggers the job transition. We refer to this hypothesis as the "compensation hypothesis". By compensating the negative change in firm effects, the improvement in the match quality either moderates the wage drop or turns the wage drop into a wage gain, hence breaking the symmetry in wage change for upwards and downwards job changers.

Comparing these theoretical implications of endogenous mobility to the empirical wage dynamics shown in Figure 1 (and in all robustness figures), we conclude that the wage model extended to allow for worker-firm match effects appears better suited to explain the wage dynamics of job-to-job movers. Indeed, independently of the firm/job categorization method, the symmetry test fails when performed on the group of job-to-job movers, as these wage changers experience moderate wage drops when moving to lower-type firms compared to the wage gain experienced by upwards movers. This asymmetry in wage dynamics, especially for job-to-job transitions between relatively similar firms, suggests that workers sort themselves into better matches, even when the improvement in the quality of the match is obtained by transitioning to lower-quartile firms, and therefore that the exogenous mobility assumption does not hold.

5.2 Empirical Estimation Strategy

Without imposing the strong assumption of orthogonality of the match effect,[23] we cannot directly estimate Equation (9) with Ordinary Least Squares. We therefore propose the following two-step estimation strategy. First, because the endogenous 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 strongly rejected, namely the employment spells of workers who are hired directly from unemployment. We estimate the two-way fixed-effect wage model, Equation (1), on only this sub-sample of spells, and save the estimated firm fixed effects. Second, we decompose the observed wage growth of job-to-job movers using these estimated firm fixed effects.

5.2.1 Unemployment-to-Job Movers

First, we show that the wage growth of workers hired from unemployment is markedly different from the wage growth of workers hired from another job. Figure 10 compares the distribution of both raw and residual wage percentage changes experienced by job changers transitioning via unemployment to the one obtained on the sample of job-to-job movers. Wage changes for JUJ workers are roughly uniform, with the median raw wage percentage change close to zero and the median residual wage change at zero. In comparison, wage growth is normally distributed in the sample of job-to-job workers with the median well above zero, at around seven percent for the median raw wage change, and four percent for the median residual wage change.

Moreover, all the symmetry tests performed in Section 4 clearly point towards the

[23] See Sørensen and Vejlin (2013) and Woodcock (2015) who run an OLS estimation on a three-way fixed-effect wage model.

difference in wage dynamics between job-to-job changers and job changers who experience a spell of unemployment. When job changers transition via unemployment, the gain in wages that they experience when moving from low-quartile jobs to high-quartile jobs is roughly of the same magnitude as the drop in wages that they experience when moving in the opposite direction. Hence, the wage change plots of movers who transition in opposite directions lie around the red dotted line that represents the case of perfect symmetry in wage changes (Figure 1 and robustness Figures 3, 4, 5, 7, and 9). 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 offsets the negative change in firm fixed effects. Said differently, we find little evidence that unemployment-to-job workers sort themselves into better matches and therefore, that sorting triggers their mobility.

Because the wage dynamics observed for this group of job changers fit reasonably well the implications of the AKM wage decomposition, the exogenous mobility assumption seems a more reasonable assumption for the group of workers hired from unemployment.[24] Indeed, we rely on the fact that the reservation wage of unemployed workers is lower than the reservation wage of employed workers, potentially low enough that workers might accept job offers involving wage cuts. Unemployment therefore enlarges the range of acceptable job offers.[25] Consequently, it appears that two-way fixed-effect wage models seem well suited to the analysis of the wage growth they experience.

5.2.2 Decomposing the Wage Growth of Job Changers

If the match effect of unemployment-to-job workers is truly conditionally mean zero, the AKM two-way fixed-effect wage equation can be consistently estimated on the sample of workers who are hired from unemployment. On only this sub-sample of job changers,

[24]Exogenous mobility does not require that worker moves between firm types are random, only that worker moves conditional on firm type and time-varying observables are random. Indeed, there is a strong positive correlation (0.453) between the types of the source and destination firms of job-to-job movers as categorized by the symmetry tests. This correlation is much weaker (0.237) for workers that face a spell of unemployment before finding a new job. We take this as additional heuristic evidence that unemployed workers are more likely to take any available job, and therefore more likely to satisfy the exogenous mobility assumption.

[25]See for example Kantenga and Law (2016) who compare the sets of firms to which employed and unemployed workers are willing to move. While an unemployed worker accepts a job offer as soon as the surplus at the new firm is positive, an employed worker would move to a new firm only if the surplus at the new firm exceeds the current surplus.

we estimate the fixed-effect wage equation (1) using OLS. After estimating the wage equation, we recover the estimated coefficients $\hat{\beta}$, as well as the firm fixed effects $\hat{\psi}$, which we interpret as the firm-specific wage premium paid to *all* workers. Therefore, we implicitly assume that the firm effect is identical for workers coming from unemployment and workers coming from other firms, and the firm fixed effects estimated on the sample of workers who are hired from unemployment apply to the all workers. We allow the match effect to depend on the worker and firm effects, a specification that is consistent with existing models.[26]

One additional step is necessary in order to perform the mean and variance decompositions. We want to separate the change in white noise $\Delta\epsilon$ from the change in match effect $\Delta\xi$. However, from our estimation so far, we can only recover the mean and variance of the composite error $\Delta r = \Delta\xi + \Delta\epsilon$. We propose to proxy the mean and variance of $\Delta\epsilon$ using the sample of job stayers. Specifically, since neither the firm effect nor the match effect change for this group of workers, Equation (10) reduces to:

$$\Delta w_{i,t} = \Delta x'_{i,t}\beta + \Delta\epsilon_{i,t} \tag{14}$$

Using $\hat{\beta}$, our consistent estimate of β from the fixed effect OLS regression fitted to only out-of-unemployment workers, we approximate the mean and variance of $\Delta\epsilon$:

$$\widehat{\mathbb{E}\left[\Delta\epsilon_{i,t}\right]} = \mathbb{E}\left[\Delta w_{i,t} - \Delta x'_{i,t}\hat{\beta}\right] \tag{15}$$

$$\widehat{Var\left[\Delta\epsilon_{i,t}\right]} = Var\left[\Delta w_{i,t} - \Delta x'_{i,t}\hat{\beta}\right] \tag{16}$$

Through the lens of our model, the mean and variance of the change in white noise are proxied by the mean and variance of the log wage changes experienced by job stayers net of changes in time-varying observables. By netting out our approximation of the mean change in white noise, we obtain the mean change in match effect (see Equation 17). We then decompose the mean log wage growth of job-to-job movers according to Equation (10).

Next, we rearrange Equation (10). Using our first-stage estimated firm fixed effects, $\hat{\psi}$, estimated coefficients $\hat{\beta}$, and proxy for the mean change in white noise, $\widehat{\mathbb{E}\left[\Delta\epsilon_{i,t}\right]}$, we

[26]See for example Postel-Vinay and Robin (2002) who develop a model in which the wage offered by a firm depends on the firm's type, captured in our model by the firm fixed effect, and on the labor market history of the worker in relation to the firm's type, captured in our model by the match effect.

retrieve the expected change in match effect for job-to-job movers:

$$\mathbb{E}\left[\Delta\xi_{i,J(i,t)}\right] = \mathbb{E}\left[\Delta w_{i,t}\right] - \mathbb{E}\left[\Delta\hat{\psi}_{J(i,t)}\right] - \mathbb{E}\left[\Delta x'_{i,t}\hat{\beta}\right] - \widehat{\mathbb{E}\left[\Delta\epsilon_{i,t}\right]} \tag{17}$$

Because the difference in log wages is observed, the firm fixed effects and effects of observables are estimated using the JUJ mobility sample, and the change in white noise is proxied using the sample of job stayers, the change in match effect is therefore the residual.[27]

Moreover, with our proxy for the variance of $\Delta\epsilon_{i,t}$ in hand, we can decompose the variance of the change in log wages experienced by JtJ movers 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.[28] We assess the relative contribution $D(\Delta y)$ of each component Δy (change in the firm fixed effect, change in observables, change in the match effect, change in white noise) to the variance of log wage change:[29]

$$D(\Delta y) = \frac{Cov(\Delta y, \Delta w)}{Var(\Delta w)} = \frac{Var(\Delta y) + \sum_{z \neq y} Cov(\Delta y, \Delta z)}{Var(\Delta w)} \tag{18}$$

5.2.3 Mean and Variance Decomposition

In Table 13, we report the mean and variance decomposition results obtained by applying Equations (17) and (18) to the sample of job-to-job movers. We now attribute some of the unexplained share of wage growth discussed in Section 4.2.1 to changes in match effects. We find that the positive change in match effects explains 44% of the 8%-wage growth experienced by job-to-job movers, the remaining part being mostly explained by improving observable characteristics. That is, job-to-job movers appear to sort into better matches, and the related improvement in the quality of the match explains almost half

[27] A caveat about our estimation methodology is therefore that we can only estimate firm fixed effects for firms employing workers moving from unemployment to employment. As shown in Tables 1 and 2, relative to the full sample that contains 206,966 firms, we lose around 38% of firms, especially smaller firms, as the JUJ sample contains 128,457 firms.

[28] We are implicitly assuming that the variance of changes in white noise for job stayers is the same as the variance of changes in white noise for job changers. To the extent that these two populations are not identical, this assumption may be somewhat strong. At most, errors here will change the percentage of the composite error variance we assign to the match effect and white noise.

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

of the wage growth that they experience.

The variance decomposition highlights a similar pattern. The primary driver of dispersion in wage dynamics is the change in the match effect, which explains 66% of the variance in wage changes experienced by job-to-job movers. This is followed by change in white noise which contributes to 20% of the variance of wage changes. We find that neither the change in time-varying observables nor the change in firm fixed effects can explain much of the variation in wage changes: observables explain around 5% of the variance of wage changes, while firm fixed effects explain only 10% of the variance. This result is not driven by a small dispersion in the change in firm fixed effects. The standard deviation of $\Delta\psi$ is 0.17, while the standard deviation of wage changes is 0.22.

To highlight the importance of the choice of firm fixed effects, we report in Table 14 the variance decomposition results we would obtain if we were to use the firm fixed effect estimated on the JtJ sample. Changes in firm fixed effects would account for 21% of the variance of the wage growth experienced by job-to-job movers, and the change in match effect would account for only 56% of this variance. Hence, we would have underestimated the contribution of mismatch correction.

5.2.4 Compensation Hypothesis

We argue in Section 5.1 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 15, we report a correlation between the change in the match effect and the change in the firm fixed effect equal to -0.56, a negative correlation that is consistent with the compensation hypothesis.[30]

In Table 15, we also show how our estimates depend on the sign of the firm effect differential. While workers moving to firms with higher fixed effects observe their wage increase by around 10%, workers moving to lower fixed effect firms also experience a wage increase, though of lower magnitude, around 6%. Indeed, for this group of workers, the positive change in match effect (0.21) overcompensates the negative change in firm

[30] More 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.

fixed effect (-0.12), further supporting the compensation hypothesis that predicts an improvement in the quality of the match for workers moving to lower fixed effect firms. However, the compensation hypothesis is silent about the direction of the match effect for workers moving to higher fixed effect firms. Table 15 indicates that upwards job changers experience a slight deterioration in the quality of the match (-0.06). Looking outside of the compensation hypothesis, the inverse relationship between match effect differential and firm effect differential 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 less a worker coming from a low-wage firm.

5.2.5 Heterogeneous Match Effect across Job Changers

In this section we investigate how the relative importance of the drivers of wage growth differs across age and working position. First, Figure 11 decomposes the expectation of wage changes $\mathbb{E}[\Delta w]$ into the expectation of changes in firm fixed effect, observables, and match effect, for different age groups. Consistent with the hump-shaped wage profile commonly presented in the literature on life-cycle earnings (e.g. Mincer, 1974; Becker, 1994; Lagakos et al., 2018), we observe that young workers tend to experience higher wage growth when transiting to new firms compared to senior workers. Relatedly, young workers appear to transition to better matches, more so than workers above 45. We also observe that workers do not transition to higher fixed effect firms, independently of age. This result therefore highlights the importance of job-to-job transitions for correcting misallocations of workers at early stages of their careers. As workers sort themselves into better and better matches, the marginal improvement in match quality decreases. Hence, job-to-job transitions feature decreasing match returns over the life cycle.

Next, in Figure 12, we break down this wage growth decomposition across working position. We find that the contribution of the change in match effect to wage growth is rather constant over the life cycle for blue-collar workers, and corresponds to 4-5% wage growth. In contrast, the contribution of the change in match effect to wage growth sharply declines over the life cycle for middle managers and even more so for managers. Indeed, while an increase in match effect explains half of the 16% wage growth experienced by transitioning managers below 35, we observe no change in match effect for transitioning managers above 45. This evidence suggests that the search for an optimal match is a crucial driver of job transitions for young middle managers and managers.

6 Conclusion

In this paper, we argue that if we aim to understand wage dynamics in an empirical fixed-effect wage regression model, it is important to include a freely varying match effect. We show that standard two-way fixed-effect wage models, which require match effects to be conditionally zero in expectation, are unable to account for the wage dynamics of workers transitioning between firms. We propose an estimation strategy based on evidence that workers who are hired from unemployment do not appear to sort into jobs with high match quality. Consequently, we exploit the exogenous mobility of these workers to estimate the firm fixed effects, and 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 log wages after a move can be attributed to changes in match quality. We also 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. 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.

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Table 1: Sample Selection: Full Sample

Selection	Observation losses	Remaining sample
Raw data		40,732,616
Private sector	9,431,920	31,300,696
No self-employment		
& Primary employment	11,666,103	19,634,593
High-quality wage observations	2,881,422	16,753,171
Age 25-60	2,212,532	14,540,639
Non-missing hourly wage info.	57,615	14,483,024
Non-missing workplace info.	828,006	13,655,018
No outliers	270,161	13,384,857
Full-time workers	3,048,939	10,335,918
		N
Full sample		
Number of observations		10,335,918
Workers		1,800,844
Firms		206,966
Full sample with accounting data		
Number of observations		7,950,618
Workers		1,428,203
Firms		155,666

Note: In the upper part of the Table, we list the sample selection applied to obtain the full sample. In the lower part, we present the share of the full sample that contains firm accounting information.

Table 2: Sample Selection: JtJ and UJ Samples

	Observation losses compared to full sample	N
JtJ sample		
Number of observations	2,454,625	7,237,995
Workers		1,336,249
Firms		165,746
UJ sample		
Number of observations	7,971,600	2,364,318
Workers		692,530
Firms		128,457

Note: In this Table, we compare the size of two sub-samples: the UJ sample and the JtJ sample. The JtJ sample is a sub-sample of the full sample in which we keep only employment spells of workers hired directly from employment. The UJ sample is a sub-sample of the full sample in which we keep only employment spells of workers hired directly from unemployment.

Table 3: Worker Mobility

Number of employers	Full sample N	Full sample %	JtJ Sample N	JtJ Sample %	UJ sample N	UJ sample %
1	4,280,071	41%	3,329,364	46%	1,825,700	77%
1(a)	4,122,013	40%	3,157,164	44%	1,561,703	66%
2	3,388,398	33%	2,390,848	33%	426,151	18%
3	1,702,159	17%	1,037,758	14%	92,894	4%
4	691,807	7%	359,448	5%	16,464	1%
5	215,531	2%	98,511	1%	2,680	0%
6	49,151	0%	19,031	0%	338	0%
7	7,846	0%	2,823	0%	83	0%
8	906	0%	202	0%	8	0%
9	27	0%	10	00%	0	0%
10	11	0%	0	0%	0	0%
11	11	0%	0	0%	0	0%
Total	10,179,634	100%	7,237,995	100%	2,364,318	100%
Av. number of years in sample	7.72		7.54		5.55	

Note: The JtJ sample is a sub-sample of the full sample in which we keep only employment spells of workers hired directly from employment. The UJ sample is a sub-sample of the full sample in which we keep only employment spells of workers hired directly from unemployment. Line 1(a) refers to the subset of workers with one employer only over the period 2000-2010 who were employed in a connected firm, i.e. a firm that employed at least one worker who changed firms over the period.

Table 4: Decomposition of the Variance of Wages

	Std. dev.	Cov(w,y)	D(y)
Full sample			
Log real hourly wage w	0.316	0.100	1.000
Time-varying observables $X\beta$	0.584	-0.001	-0.007
Worker effect α	0.650	0.078	0.778
Firm effect ψ	0.106	0.012	0.117
Composite error r	0.106	0.011	0.112
JtJ sample			
Log real hourly wage w	0.315	0.100	1.000
Time-varying observables $X\beta$	0.601	-0.001	-0.012
Worker effect α	0.668	0.081	0.812
Firm effect ψ	0.104	0.010	0.099
Composite error r	0.101	0.010	0.101
UJ sample			
Log real hourly wage w	0.278	0.077	1.000
Time-varying observables $X\beta$	0.602	0.009	0.120
Worker effect α	0.645	0.050	0.647
Firm effect ψ	0.148	0.011	0.145
Composite error r	0.083	0.007	0.089

Note: The JtJ sample is a sub-sample of the full sample in which we keep only employment spells of workers hired directly from employment. The UJ sample is a sub-sample of the full sample in which we keep only employment spells of workers hired directly from unemployment. The time-varying observables include year dummies, a quadratic and cubic term in age, and an interaction of these three variables with education dummies. y represents each component of the AKM wage decomposition: time-varying observables $X\beta$, worker effect α, firm effect ψ, and composite error r. $D(y) = \frac{Cov(y,w)}{Var(w)}$ represents the relative contribution of each component y to the variance of log real hourly wages w.

Table 5: Correlation between Firm Fixed Effects

	Firm effect ψ from full sample	Firm effect ψ from JtJ sample	Firm effect ψ from UJ sample
Firm effect ψ from full sample	1.000^a 1.000^b		
Firm effect ψ from JtJ sample	0.797^a 0.633^b	1.000^a 1.000^b	
Firm effect ψ from UJ sample	0.513^a 0.354^b	0.339^a 0.122^b	1.000^a 1.000^b

Note: The Table reports the correlation between firm fixed effects obtained by estimating a two-way fixed effect log wage equation on the full sample, the JtJ sample, and the UJ sample. The JtJ sample is a sub-sample of the full sample in which we keep only employment spells of workers hired directly from employment. The UJ sample is a sub-sample of the full sample in which we keep only employment spells of workers hired directly from unemployment. (a) refers to the specification in which firms are weighted by employment. (b) refers to the specification in which firms are not weighted by employment (i.e., we use one observation per firm).

Table 6: Mobility Samples Selection

	Number of remaining observations		
Full sample	10,335,918		
Selection:			
At least 2 employers	6,055,847		
First wage observation per match	2,710,927		
	Full mobility sample	**JtJ mobility sample**	**JUJ mobility sample**
Number of job transitions	1,099,145	727,916	203,504
Number of workers	639,344	526,756	181,699
Number of firms	116,389	93,621	64,802

Note: The full mobility sample includes all job transitions. The JtJ mobility sample is a sub-sample of the full mobility sample in which we only keep direct job-to-job transitions. The JUJ mobility sample is a sub-sample of the full mobility sample in which we only keep job transitions that involved a period of unemployment.

Table 7: Wage Change: Mean Decomposition

	Firm effect ψ from full sample		Firm effect ψ from JtJ sample		Firm effect ψ from UJ sample	
	Mean	$\frac{\text{Mean}(\Delta y)}{\text{Mean}(\Delta w)}$	Mean	$\frac{\text{Mean}(\Delta y)}{\text{Mean}(\Delta w)}$	Mean	$\frac{\text{Mean}(\Delta y)}{\text{Mean}(\Delta w)}$
Full mobility sample						
Δw	0.0587	1.0000	0.0590	1.0000	0.0606	1.0000
$\Delta X\beta$	0.0437	0.7451	0.0460	0.7786	0.0465	0.7675
$\Delta\psi$	0.0009	0.0159	0.0013	0.0213	-0.0053	-0.0882
Residual	0.0140	0.2390	0.0118	0.2000	0.0194	0.3206
JtJ mobility sample						
Δw	0.0804	1.0000	0.0792	1.0000	0.0806	1.0000
$\Delta X\beta$	0.0407	0.5057	0.0430	0.5427	0.0433	0.5379
$\Delta\psi$	0.0076	0.0949	0.0062	0.0786	-0.0040	-0.0493
Residual	0.0321	0.3993	0.0300	0.3787	0.0412	0.5115
JUJ mobility sample						
Δw	0.0228	1.0000	0.0218	1.0000	0.0294	1.0000
$\Delta X\beta$	0.0423	1.8586	0.0449	2.0638	0.0452	1.5399
$\Delta\psi$	-0.0136	-0.5973	-0.0086	-0.3963	-0.0067	-0.2272
Residual	-0.0060	-0.2613	-0.0145	-0.6675	-0.0092	-0.3127

Note: The JtJ sample is a sub-sample of the full sample in which we keep only employment spells of workers hired directly from employment. The UJ sample is a sub-sample of the full sample in which we keep only employment spells of workers hired directly from unemployment. The full mobility sample includes all job transitions. The JtJ mobility sample is a sub-sample of the full mobility sample in which we only keep direct job-to-job transitions. The JUJ mobility sample is a sub-sample of the full mobility sample in which we only keep job transitions that involved a period of unemployment. The time-varying observables include year dummies, a quadratic and cubic term in age, and an interaction of these three variables with education dummies. y represents the components of the change in log real hourly wages w: change in time-varying observables $X\beta$, change in firm effect ψ, and residual.

Table 8: Wage Change: Variance Decomposition

	Firm effect ψ from full sample			Firm effect ψ from JtJ sample			Firm effect ψ from UJ sample		
	Std. dev.	Cov.	D(Δy)	Std. dev.	Cov.	D(Δy)	Std. dev.	Cov.	D(Δy)
Full mobility sample									
Δw	0.2578	0.0665	1.0000	0.2524	0.0637	1.0000	0.2463	0.0607	1.0000
$\Delta X\beta$	0.0532	0.0028	0.0418	0.0540	0.0029	0.0448	0.0504	0.0026	0.0421
$\Delta\psi$	0.1269	0.0157	0.2359	0.1265	0.0126	0.1982	0.1810	0.0101	0.1670
Residual	0.2201	0.0480	0.7224	0.2273	0.0482	0.7570	0.2660	0.0480	0.7909
JtJ mobility sample									
Δw	0.2313	0.0535	1.0000	0.2297	0.0528	1.0000	0.2184	0.0477	1.0000
$\Delta X\beta$	0.0493	0.0024	0.0452	0.0503	0.0025	0.0470	0.0471	0.0023	0.0475
$\Delta\psi$	0.1097	0.0111	0.2056	0.1148	0.0110	0.2085	0.1690	0.0045	0.0952
Residual	0.2028	0.0401	0.7493	0.2036	0.0393	0.7445	0.2552	0.0409	0.8573
JUJ mobility sample									
Δw	0.3089	0.0954	1.0000	0.3009	0.0906	1.0000	0.3017	0.0910	1.0000
$\Delta X\beta$	0.0557	0.0025	0.0265	0.0566	0.0026	0.0285	0.0515	0.0020	0.0224
$\Delta\psi$	0.1612	0.0265	0.2776	0.1505	0.0115	0.1275	0.2132	0.0307	0.3370
Residual	0.2576	0.0664	0.6958	0.2972	0.0764	0.8439	0.2705	0.0583	0.6406

Note: The JtJ sample is a sub-sample of the full sample in which we keep only employment spells of workers hired directly from employment. The UJ sample is a sub-sample of the full sample in which we keep only employment spells of workers hired directly from unemployment. The full mobility sample includes all job transitions. The JtJ mobility sample is a sub-sample of the full mobility sample in which we only keep direct job-to-job transitions. The JUJ mobility sample is a sub-sample of the full mobility sample in which we only keep job transitions that involved a period of unemployment. The time-varying observables include year dummies, a quadratic and cubic term in age, and an interaction of these three variables with education dummies. y represents the components of the change in log real hourly wages w: change in time-varying observables $X\beta$, change in firm effect ψ, and residual. Cov.=$Cov(\Delta y, \Delta w)$ is the covariance between the change in log real hourly wages w and the change in each component y. D(Δy) = $\frac{Cov(\Delta y, \Delta w)}{Var(\Delta w)}$ represents the relative contribution of each component y to the variance of the change in log real hourly wages w.

Table 9: Wage Dynamics by Transition Type

	Full mobility sample					JtJ mobility sample					JUJ mobility sample				
			Wage change (%)					Wage change (%)					Wage change (%)		
Mobility	N	%	res.	raw	adj.	N	%	res.	raw	adj.	N	%	res.	raw	adj.
1 to 1	92,884	9.9	4.2	6.9	7.1	58,279	9.2	4.7	7.0	5.4	18,270	11.2	2.5	5.4	3.6
1 to 2	68,407	7.3	10.4	13.1	12.9	47,070	7.4	10.7	13.2	10.5	12,663	7.8	8.8	11.8	9.3
1 to 3	30,760	3.3	15.7	19.0	20.2	17,697	2.8	15.7	18.6	17.9	7,942	4.9	15.7	18.7	16.2
1 to 4	19,413	2.1	21.3	24.9	25.5	11,070	1.7	21.0	24.2	22.5	4,496	2.8	19.3	22.7	20.5
2 to 1	51,368	5.5	-3.0	-0.0	0.9	29,388	4.6	-0.6	2.1	0.6	13,621	8.3	-7.0	-4.0	-4.4
2 to 2	92,846	9.9	2.6	5.2	3.6	68,139	10.7	3.3	5.6	2.6	14,076	8.6	-0.1	2.8	1.2
2 to 3	55,536	5.9	8.2	11.5	10.3	36,858	5.8	8.8	11.8	9.6	10,653	6.5	6.2	9.2	6.9
2 to 4	33,578	3.6	12.9	16.4	14.7	21,861	3.4	13.6	16.9	13.4	6,316	3.9	10.6	13.7	9.8
3 to 1	28,314	3.0	-9.7	-6.5	-2.4	13,972	2.2	-5.6	-2.6	-4.2	9,326	5.7	-14.6	-11.9	-14.1
3 to 2	48,702	5.2	-2.2	0.9	2.0	30,196	4.7	-0.2	2.8	1.4	11,184	6.9	-7.0	-4.2	-7.1
3 to 3	97,963	10.4	3.1	6.1	4.2	75,486	11.9	3.7	6.5	3.3	11,335	6.9	0.4	3.2	0.7
3 to 4	71,032	7.6	8.8	12.0	8.8	52,657	8.3	9.5	12.5	8.2	8,466	5.2	4.3	7.3	4.1
4 to 1	18,373	2.0	-17.2	-14.0	-8.6	9,052	1.4	-10.6	-7.7	-9.2	5,939	3.6	-23.8	-21.0	-20.2
4 to 2	30,163	3.2	-7.4	-4.1	-3.5	17,582	2.8	-4.0	-0.9	-2.7	7,333	4.5	-15.3	-12.4	-11.8
4 to 3	59,165	6.3	-1.1	2.2	2.1	40,311	6.3	1.2	4.3	1.8	9,340	5.7	-7.7	-4.7	-5.7
4 to 4	142,291	15.1	4.5	7.8	5.2	106,248	16.7	5.6	8.7	4.5	12,286	7.5	-2.3	1.0	-2.6

Note: The full mobility sample includes all job transitions. The JtJ mobility sample is a sub-sample of the full mobility sample in which we only keep direct job-to-job transitions. The JUJ mobility sample is a sub-sample of the full mobility sample in which we only keep job transitions that involved a period of unemployment. For each mobility sample and for each transition type, we report the number of observations, the mean change in log residual wages (controls include year dummies, a quadratic and cubic term in age, and their interactions with our education measure), and the mean log raw wage change. In the last column of each mobility sample, we also report the mean regression-adjusted log wage change of workers who transition within a year. To adjust wage changes, we use the coefficient estimates from a model fit to job stayers (model includes age and education dummies and a quadratic term in age interacted with education). The number of observations only refers to residual and raw wage changes. Our sample drops when adjusting wage changes. The quartiles are obtained by categorizing jobs based on the mean residual wage of co-workers (controls include year dummies, a quadratic and cubic term in age, and their interactions with our education measure).

Table 10: Wage Dynamics by Transition Type: Alternative Firm Classifications

	(a) Firm classification based on AKM from full sample				(b) Firm classification based on AKM from JtJ sample				(c) Firm classification based on AKM from UJ sample			
	JtJ mobility sample		JUJ mobility sample		JtJ mobility sample		JUJ mobility sample		JtJ mobility sample		JUJ mobility sample	
	N	*res.*	N	*res.*	N	*res.*	N	*res.*	N	*res.*	N	*res.*
1 to 1	72,461	4.8	24,294	2.2	77,406	4.5	17,395	2.4	29,943	5.3	15,817	0.2
1 to 2	54,991	12.2	13,954	11.9	57,445	11.4	11,240	5.9	21,901	7.0	10,308	9.6
1 to 3	21,881	18.1	9,394	18.1	23,822	16.6	7,994	10.9	18,590	8.9	8,406	15.5
1 to 4	18,635	27.7	8,766	28.8	21,181	25.5	7,537	16.0	17,987	12.2	8,392	24.6
2 to 1	37,196	-3.0	16,922	-0.0	36,110	-2.9	13,062	-5.5	37,977	6.9	9,949	-9.9
2 to 2	64,589	4.4	14,570	5.2	77,070	3.8	12,222	-0.5	26,166	5.0	9,061	-0.5
2 to 3	47,989	8.3	10,452	11.5	43,919	8.4	9,246	3.3	27,130	6.3	7,824	6.0
2 to 4	27,142	16.8	8,947	16.4	27,522	15.8	7,664	7.5	19,784	10.6	8,031	15.6
3 to 1	18,392	-10.3	12,763	-6.5	20,295	-9.2	9,917	-10.5	17,311	1.4	9,118	-17.0
3 to 2	31,766	0.9	11,603	0.9	31,553	1.2	9,534	-5.3	31,837	2.7	8,955	-6.0
3 to 3	75,831	4.3	11,938	6.1	59,483	4.6	9,840	0.5	41,679	5.9	9,409	0.4
3 to 4	53,035	11.1	10,655	12.0	49,137	10.9	8,464	4.4	52,252	6.3	10,865	9.5
4 to 1	15,218	-20.6	12,127	-14.0	17,328	-18.2	9,426	-17.1	15,833	-1.6	9,082	-25.3
4 to 2	21,226	-6.4	10,485	-4.1	21,574	-5.3	8,826	-11.5	19,229	1.0	8,764	-15.1
4 to 3	41,648	-0.1	11,492	2.2	39,996	0.3	9,176	-6.4	47,023	3.3	10,829	-7.6
4 to 4	123,938	4.5	14,600	7.8	101,443	4.4	10,840	-1.6	62,093	3.8	15,234	1.8

Note: The JtJ mobility sample is a sub-sample of the full mobility sample in which we only keep direct job-to-job transitions. The JUJ mobility sample is a sub-sample of the full mobility sample in which we only keep job transitions that involved a period of unemployment. For each mobility sample and for each transition type, we report the number of observations and *res.*, the mean change in log residual wages (controls include year dummies, a quadratic and cubic term in age, and their interactions with our education measure). The quartiles are obtained by categorizing jobs based on the firm fixed effects estimates obtained by fitting a two-way fixed-effect model on: (a) the full sample; (b) the JtJ sample; (c) the UJ sample. The JtJ sample is a sub-sample of the full sample in which we keep only employment spells of workers hired directly from employment. The UJ sample is a sub-sample of the full sample in which we keep only employment spells of workers hired directly from unemployment.

Table 11: Formal Symmetry Test: T-Statistics, Baseline Firm Classification

Mobility	Δw up $+\Delta w$ down	N	no cl.	year cl.	age cl.
Full mobility sample					
1<>2	0.073	119,775	56.16	9.48	15.90
1<>3	0.061	59,074	27.33	7.10	7.57
1<>4	0.041	37,786	12.71	4.50	3.48
2<>3	0.060	104,238	40.81	7.78	7.76
2<>4	0.055	63,741	25.67	5.06	5.10
3<>4	0.077	130,197	57.48	8.75	7.76
JtJ mobility sample					
1<>2	0.101	76,458	68.02	13.31	26.36
1<>3	0.100	31,669	34.89	10.47	13.87
1<>4	0.105	20,122	25.62	11.80	10.06
2<>3	0.086	67,054	50.74	12.95	11.89
2<>4	0.096	39,443	38.01	10.60	9.77
3<>4	0.107	92,968	73.46	12.10	11.60
JUJ mobility sample					
1<>2	0.018	26,284	5.61	1.46	2.26
1<>3	0.010	17,268	2.39	0.78	0.91
1<>4	-0.045	10,435	-7.03	-3.73	-3.26
2<>3	-0.008	21,837	-2.19	-0.45	-0.73
2<>4	-0.046	13,649	-9.00	-2.74	-3.27
3<>4	-0.033	17,806	-7.55	-2.42	-2.32

Note: The full mobility sample includes all job transitions. The JtJ mobility sample is a sub-sample of the full mobility sample in which we only keep direct job-to-job transitions. The JUJ mobility sample is a sub-sample of the full mobility sample in which we only keep job transitions that involved a period of unemployment. For each mobility sample and for each transition type, we report the sum of the mean change in log residual wages experienced by upward movers and the mean change in log residual wages experienced by downward movers, as well as the number of observations and three t-statistics (no clustering, clustering by year, clustering by age). The mean log residual wages are obtained by controling for year dummies, a quadratic and cubic term in age, and their interactions with our education measure. The quartiles are obtained by categorizing jobs based on the mean residual wage of co-workers (controls include year dummies, a quadratic and cubic term in age, and their interactions with our education measure).

Table 12: Formal Symmetry Test: T-Statistics, Preferred Firm Classification (Residual+)

Mobility	Δw up $+\Delta w$ down	N	no cl.	year cl.	age cl.
Full mobility sample					
1<>2	0.076	105,053	53.57	16.91	12.09
1<>3	0.062	76,935	33.49	10.31	6.94
1<>4	0.049	61,800	20.58	4.88	4.49
2<>3	0.068	113,801	51.42	9.83	8.07
2<>4	0.061	79,248	32.89	6.21	5.53
3<>4	0.077	131,701	56.86	9.15	7.87
JtJ mobility sample					
1<>2	0.101	65,648	63.47	17.46	15.65
1<>3	0.097	44,197	42.63	15.12	12.03
1<>4	0.102	34,213	34.41	11.22	10.70
2<>3	0.091	80,694	63.29	11.30	11.28
2<>4	0.096	50,635	44.91	12.89	9.47
3<>4	0.107	90,998	70.36	13.15	11.62
JUJ mobility sample					
1<>2	0.017	22,905	4.69	1.38	1.85
1<>3	-0.004	20,031	-1.01	-0.26	-0.36
1<>4	-0.029	16,376	-5.99	-2.04	-2.21
2<>3	-0.009	18,652	-2.15	-0.62	-0.74
2<>4	-0.039	15,418	-8.21	-1.94	-2.76
3<>4	-0.028	19,814	-6.71	-1.73	-2.09

Note: The full mobility sample includes all job transitions. The JtJ mobility sample is a sub-sample of the full mobility sample in which we only keep direct job-to-job transitions. The JUJ mobility sample is a sub-sample of the full mobility sample in which we only keep job transitions that involved a period of unemployment. For each mobility sample and for each transition type, we report the sum of the mean change in log residual wages experienced by upward movers and the mean change in log residual wages experienced by downward movers, as well as the number of observations and three t-statistics (no clustering, clustering by year, clustering by age). The mean log residual wages are obtained by controling for year dummies, a quadratic and cubic term in age, and their interactions with our education measure. The quartiles are obtained by categorizing jobs based on the mean residual(+) wage of co-workers, i.e. using an alternative set of controls (year dummies, a quadratic and cubic term in age, their interactions with our education measure, the interaction of a quadratic and cubic term in experience with education, gender, occupation, marital status, and area of residence).

Table 13: Extended Wage Change Decomposition: FFE obtained from UJ Sample

	Mean	$\frac{\text{Mean}(\Delta y)}{\text{Mean}(\Delta w)}$	Std. dev.	Cov.	D(Δy)
Full mobility sample					
Δw	0.0606	1.0000	0.2463	0.0606	1.0000
$\Delta X\beta$	0.0465	0.7675	0.0504	0.0026	0.0421
$\Delta\psi$	-0.0053	-0.0882	0.1810	0.0101	0.1670
$\Delta\xi$	0.0135	0.2223	0.2474	0.0384	0.6338
$\Delta\epsilon$	0.0060	0.0983	0.0976	0.0095	0.1571
JtJ mobility sample					
Δw	0.0806	1.0000	0.2184	0.0477	1.0000
$\Delta X\beta$	0.0433	0.5379	0.0471	0.0023	0.0475
$\Delta\psi$	-0.0040	-0.0493	0.1690	0.0045	0.0952
$\Delta\xi$	0.0354	0.4388	0.2356	0.0313	0.6563
$\Delta\epsilon$	0.0059	0.0727	0.0979	0.0096	0.2010
JUJ mobility sample					
Δw	0.0294	1.0000	0.3017	0.0910	1.0000
$\Delta X\beta$	0.0452	1.5399	0.0516	0.0020	0.0224
$\Delta\psi$	-0.0067	-0.2272	0.2132	0.0307	0.3370
$\Delta\xi$	-0.0172	-0.5849	0.2536	0.0495	0.5432
$\Delta\epsilon$	0.0080	0.2722	0.0942	0.0089	0.0974

Note: The JtJ mobility sample is a sub-sample of the full mobility sample in which we only keep direct job-to-job transitions. The JUJ mobility sample is a sub-sample of the full mobility sample in which we only keep job transitions that involved a period of unemployment. The time-varying observables include year dummies, a quadratic and cubic term in age, and an interaction of these three variables with education dummies. The firm fixed effect estimates are obtained by fitting a two-way fixed-effect wage model on the UJ sample (sub-sample of the full sample in which we keep only employment spells of workers hired directly from unemployment). y represents the components of the change in log real hourly wages w: change in time-varying observables $X\beta$, change in firm effect ψ, change in match effect ξ, and white noise ϵ. Cov.=$Cov(\Delta y, \Delta w)$ is the covariance between the change in log real hourly wages w and the change in each component y. D(Δy) = $\frac{Cov(\Delta y, \Delta w)}{Var(\Delta w)}$ represents the relative contribution of each component y to the variance of log real hourly wages w. ϵ is our empirical analogue to white noise – wage changes net of changes in time varying observables for workers remaining at a firm.

Table 14: Extended Wage Change Decomposition: FFE obtained from JtJ Sample

	Mean	$\frac{\text{Mean}(\Delta y)}{\text{Mean}(\Delta w)}$	Std. dev.	Cov.	D(Δy)
Full mobility sample					
Δw	0.0590	1.0000	0.2524	0.0637	1.0000
$\Delta X\beta$	0.0460	0.7786	0.0540	0.0029	0.0448
$\Delta\psi$	0.0013	0.0213	0.1265	0.0126	0.1983
$\Delta\xi$	0.0056	0.0943	0.2046	0.0385	0.6034
$\Delta\epsilon$	0.0062	0.1058	0.0989	0.0098	0.1535
JtJ mobility sample					
Δw	0.0792	1.0000	0.2297	0.0528	1.0000
$\Delta X\beta$	0.0430	0.5427	0.0503	0.0025	0.0470
$\Delta\psi$	0.0062	0.0786	0.1148	0.0110	0.2085
$\Delta\xi$	0.0238	0.3002	0.1778	0.0294	0.5578
$\Delta\epsilon$	0.0062	0.0992	0.0993	0.0099	0.1867
JUJ mobility sample					
Δw	0.0218	1.0000	0.3009	0.0906	1.0000
$\Delta X\beta$	0.0449	2.0638	0.0566	0.0026	0.0285
$\Delta\psi$	-0.0086	-0.3963	0.1505	0.0115	0.1275
$\Delta\xi$	-0.0224	-1.0299	0.2820	0.0676	0.7464
$\Delta\epsilon$	0.0079	0.3624	0.0940	0.0088	0.0975

Note: The JtJ mobility sample is a sub-sample of the full mobility sample in which we only keep direct job-to-job transitions. The JUJ mobility sample is a sub-sample of the full mobility sample in which we only keep job transitions that involved a period of unemployment. The time-varying observables include year dummies, a quadratic and cubic term in age, and an interaction of these three variables with education dummies. The firm fixed effect estimates are obtained by fitting a two-way fixed-effect wage model on the JtJ sample (sub-sample of the full sample in which we keep only employment spells of workers hired directly from employment). y represents the components of the change in log real hourly wages w: change in time-varying observables $X\beta$, change in firm effect ψ, change in match effect ξ, and white noise ϵ. Cov.=$Cov(\Delta y, \Delta w)$ is the covariance between the change in log real hourly wages w and the change in each component y. D(Δy) = $\frac{Cov(\Delta y, \Delta w)}{Var(\Delta w)}$ represents the relative contribution of each component y to the variance of log real hourly wages w. ϵ is our empirical analogue to white noise – wage changes net of changes in time varying observables for workers remaining at a firm.

Table 15: Job-to-Job Movers: Statistics Conditional on Direction of Firm Fixed Effect

	Correlation matrix				Conditional Moments			
					Δ Firm FE < 0		Δ Firm FE > 0	
	Δw	$\Delta X\beta$	$\Delta\psi$	Δr	Mean	Std. dev.	Mean	Std. dev.
Δw	1.0000				0.0621	0.2160	0.1005	0.2193
$\Delta X\beta$	0.2203	1.0000			0.0430	0.0464	0.0478	0.0478
$\Delta\psi$	0.1231	0.0163	1.0000		-0.1172	0.1183	0.1180	0.1246
Δr	0.7339	-0.0066	-0.5597	1.0000	0.2135	0.2135	-0.0612	0.2145

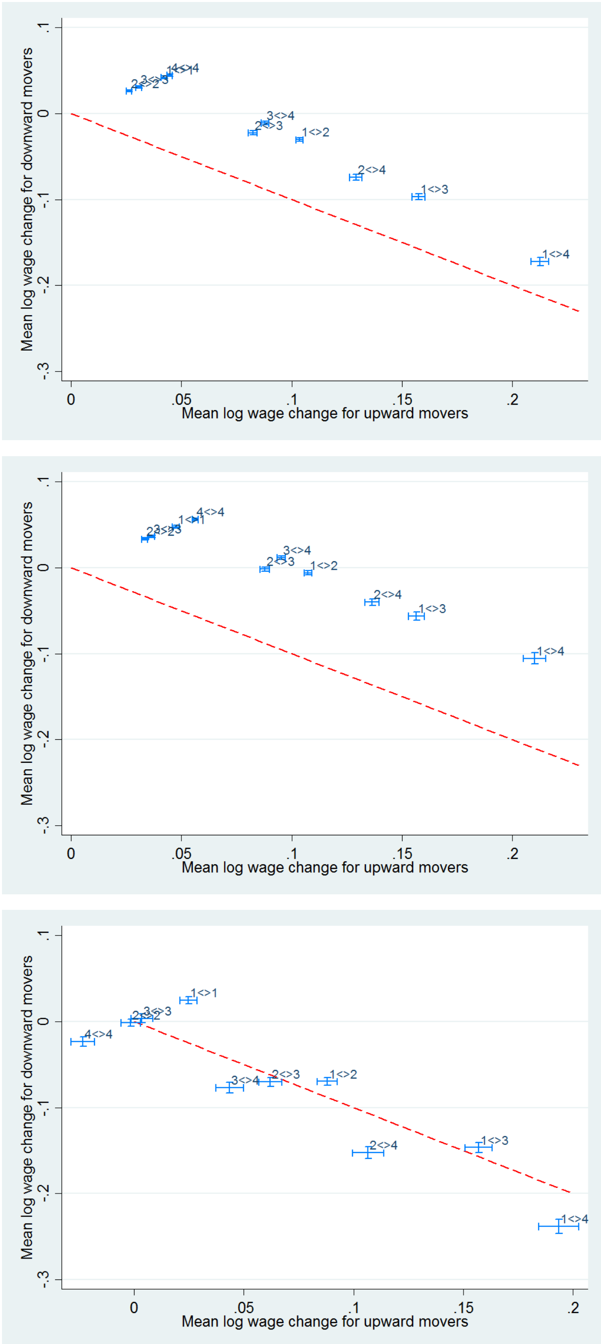
Note: This Table refers to job-to-job movers. The JtJ mobility sample is a sub-sample of the full mobility sample in which we only keep direct job-to-job transitions. The time-varying observables include year dummies, a quadratic and cubic term in age, and an interaction of these three variables with education dummies. The firm fixed effect estimates are obtained by fitting a two-way fixed-effect wage model on the UJ sample (sub-sample of the full sample in which we keep only employment spells of workers hired directly from unemployment).

Figure 1: Wage Gains vs. Wage Losses for Job Changers

(a) Full mobility sample

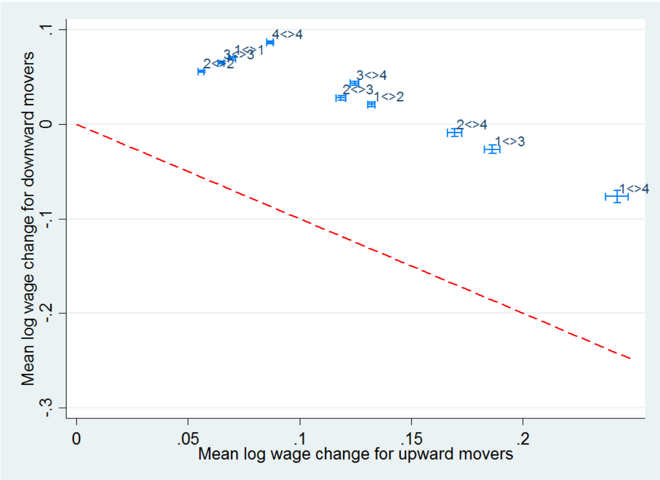
(b) JtJ mobility sample

(c) JUJ mobility sample

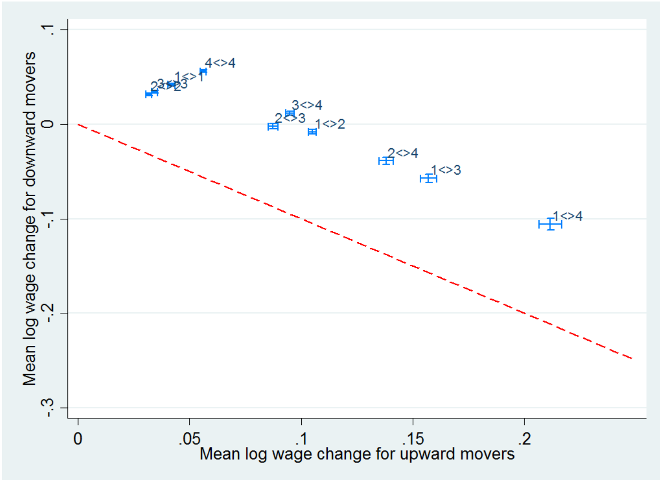


Note: The figures show the mean wage changes experienced by movers who transition between symmetric quartiles. Panel (a) shows the wage dynamics of all job-to-job changers, while Panel (b) and (c) report the wage dynamics of JtJ movers and JUJ movers, respectively. In all panels, wages of job changers are regression-adjusted before to measure wage change (controls include year dummies, a quadratic and cubic term in age, and their interactions with our education measure), and jobs are categorized based on the mean residual wage of co-workers (controls include year dummies, a quadratic and cubic term in age, and their interactions with our education measure). Wage changes experienced by workers moving to a higher quartile are on the x-axis, and wage changes experienced by those moving to a lower quartile are on the y-axis. The dotted red line represents the case of perfect symmetry between wage gains and wage losses experienced by job movers (slope = -1).

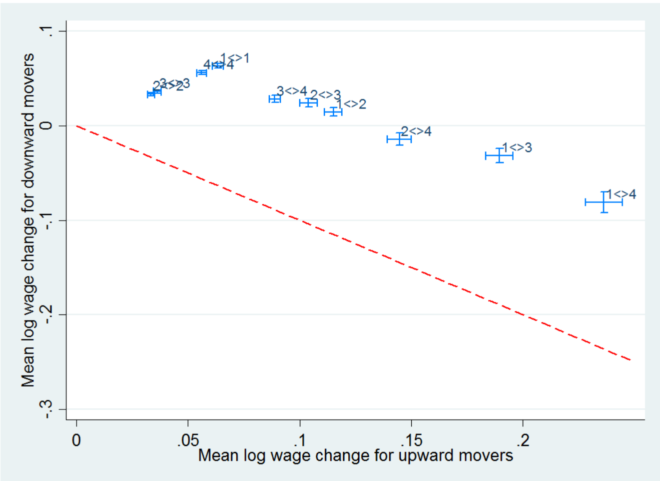
Figure 2: Wage Gains vs. Wage Losses for JtJ Movers, Alternative Wage Change Measures



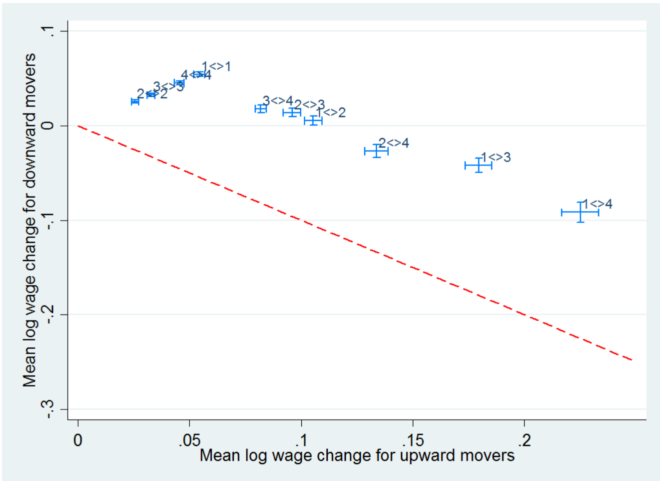
(a) Raw wage change



(b) Residual(+) wage change



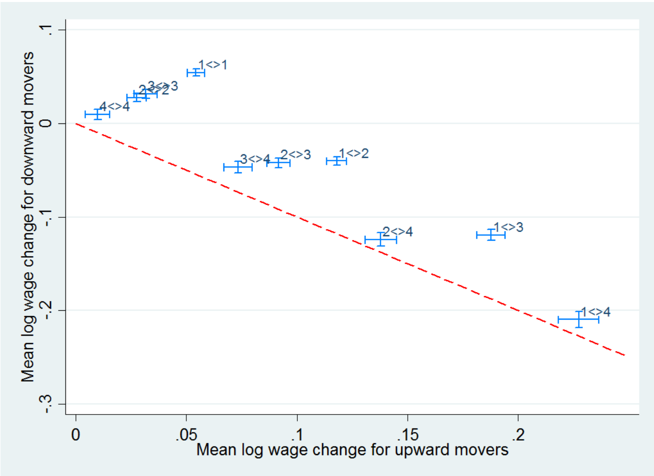
(c) Two-year residual wage change



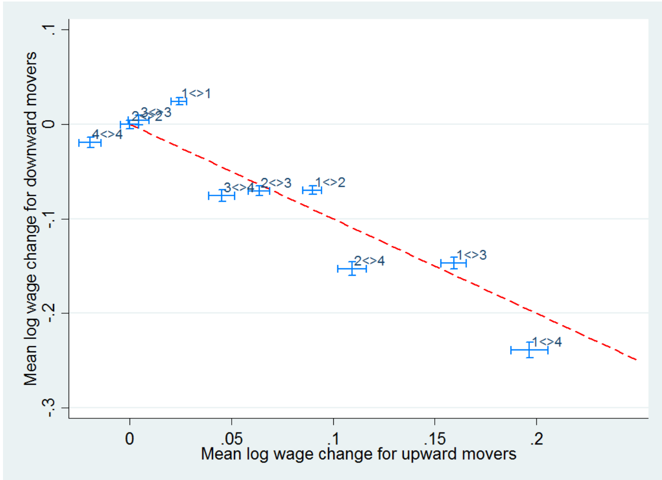
(d) Adjusted wage change

Note: All figures show the mean wage changes experienced by JtJ movers who transition between symmetric quartiles. Panel (a) reports the average raw wage change for different transition groups, while in Panel (b), wages of job changers are regression-adjusted using an alternative set of controls (education and year dummies, experience and experience squared) before to measure wage change. In Panel (c), wages of job changers are regression-adjusted using our baseline set of controls before to measure wage change but we focus on job transitions that occur within a year. In Panel (d), we also focus on job transitions that occur within a year but wages are regression-adjusted using the coefficient estimates from a model fit to job stayers. In all panels, jobs are categorized based on the mean residual wage of co-workers (controls include year dummies, a quadratic and cubic term in age, and their interactions with our education measure). Wage changes experienced by workers moving to a higher quartile are on the x-axis, and wage changes experienced by those moving to a lower quartile are on the y-axis. The dotted red line represents the case of perfect symmetry between wage gains and wage losses experienced by job movers (slope = -1).

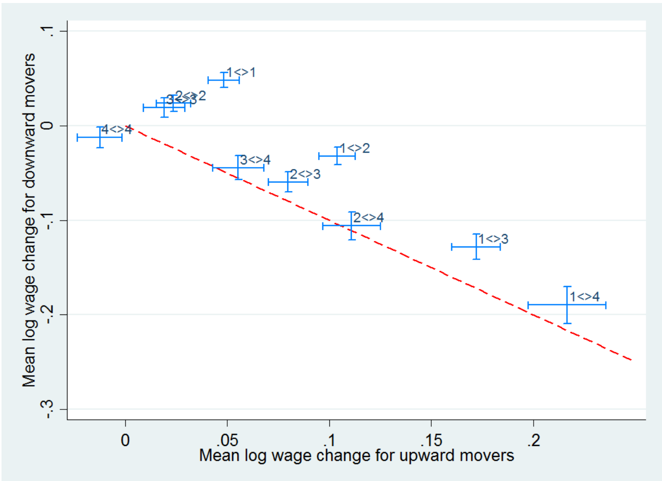
Figure 3: Wage Gains vs. Wage Losses for JUJ Movers, Alternative Wage Change Measures



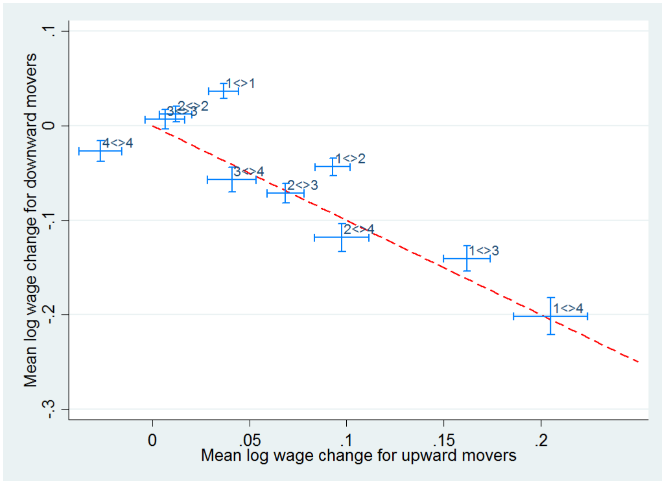
(a) Raw wage change



(b) Residual(+) wage change



(c) Two-year residual wage change



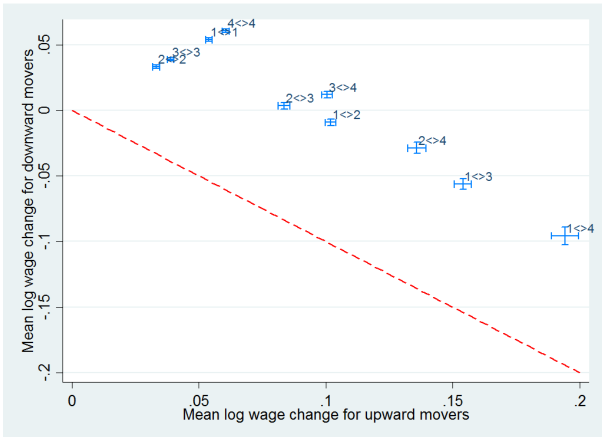
(d) Adjusted wage change

Note: All figures show the mean wage changes experienced by JUJ movers who transition between symmetric quartiles. Panel (a) reports the average raw wage change for different transition groups, while in Panel (b), wages of job changers are regression-adjusted using an alternative set of controls (education and year dummies, experience and experience squared) before to measure wage change. In Panel (c), wages of job changers are regression-adjusted using our baseline set of controls before to measure wage change but we focus on job transitions that occur within a year. In Panel (d), we also focus on job transitions that occur within a year but wages are regression-adjusted using the coefficient estimates from a model fit to job stayers. In all panels, jobs are categorized based on the mean residual wage of co-workers (controls include year dummies, a quadratic and cubic term in age, and their interactions with our education measure). Wage changes experienced by workers moving to a higher quartile are on the x-axis, and wage changes experienced by those moving to a lower quartile are on the y-axis. The dotted red line represents the case of perfect symmetry between wage gains and wage losses experienced by job movers (slope = -1).

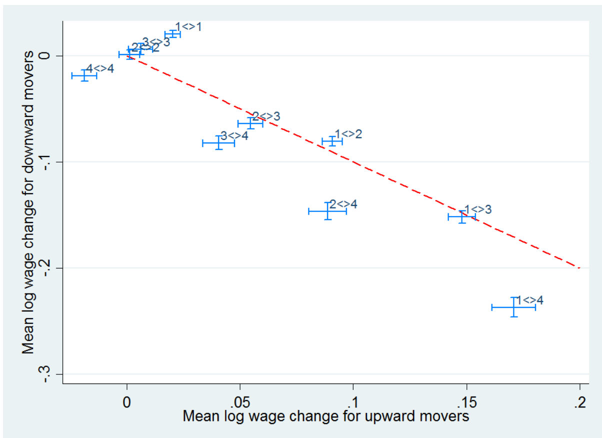
Figure 4: Wage Gains vs. Wage Losses for Job Changers, Firm Classification Based on Mean Raw Wage of Co-Workers

(a) Full mobility sample

(b) JtJ mobility sample



(c) JUJ mobility sample

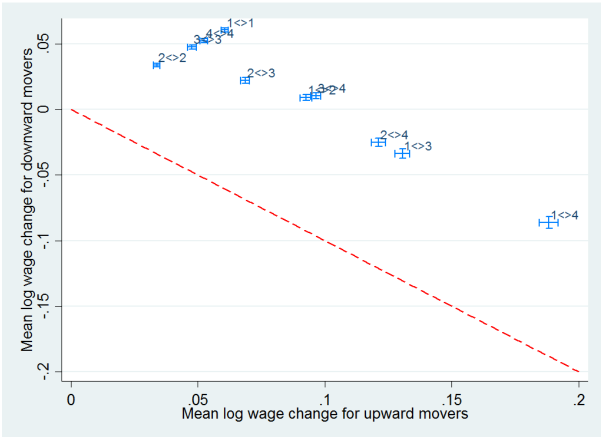


Note: The figures show the mean wage changes experienced by movers who transition between symmetric quartiles. Panel (a) shows the wage dynamics of all job-to-job changers, while Panel (b) and (c) report the wage dynamics of JtJ movers and JUJ movers, respectively. In all panels, wages of job changers are regression-adjusted before to measure wage change (controls include year dummies, a quadratic and cubic term in age, and their interactions with our education measure), and jobs are categorized based on the mean raw wage of co-workers. Wage changes experienced by workers moving to a higher quartile are on the x-axis, and wage changes experienced by those moving to a lower quartile are on the y-axis. The dotted red line represents the case of perfect symmetry between wage gains and wage losses experienced by job movers (slope = -1).

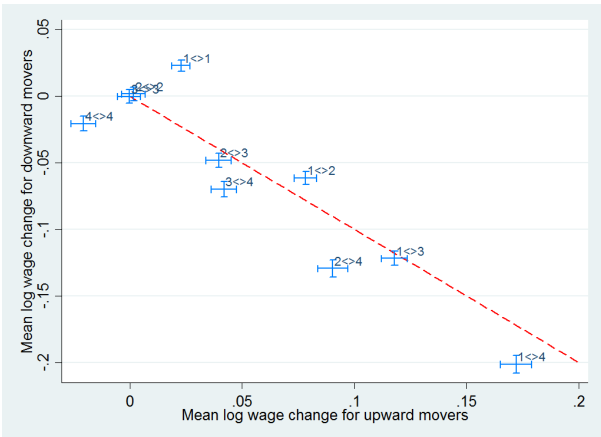
Figure 5: Wage Gains vs. Wage Losses for Job Changers, Firm Classification Based on Mean Residual(+) Wage of Co-Workers

(a) Full mobility sample

(b) JtJ mobility sample



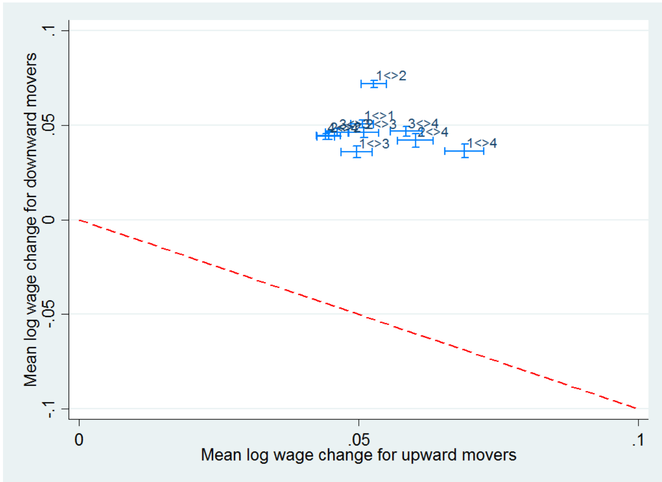
(c) JUJ mobility sample



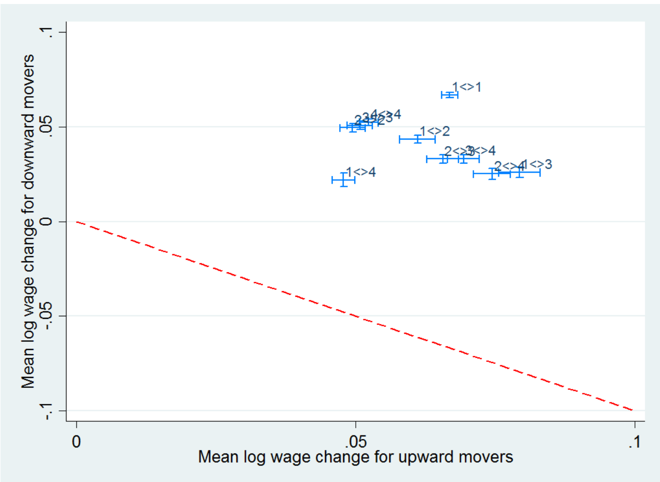
Note: The figures show the mean wage changes experienced by movers who transition between symmetric quartiles. Panel (a) shows the wage dynamics of all job-to-job changers, while Panel (b) and (c) report the wage dynamics of JtJ movers and JUJ movers, respectively. In all panels, wages of job changers are regression-adjusted before to measure wage change (controls include year dummies, a quadratic and cubic term in age, and their interactions with our education measure), and jobs are categorized based on the mean residual wage of co-workers using an alternative set of controls: year dummies, a quadratic and cubic term in age, their interactions with our education measure, the interaction of a quadratic and cubic term in experience with education, gender, occupation, marital status, and area of residence. Wage changes experienced by workers moving to a higher quartile are on the x-axis, and wage changes experienced by those moving to a lower quartile are on the y-axis. The dotted red line represents the case of perfect symmetry between wage gains and wage losses experienced by job movers (slope = -1).

Figure 6: Wage Gains vs. Wage Losses for JtJ Movers, Firm Classification Based on Accounting Data

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



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

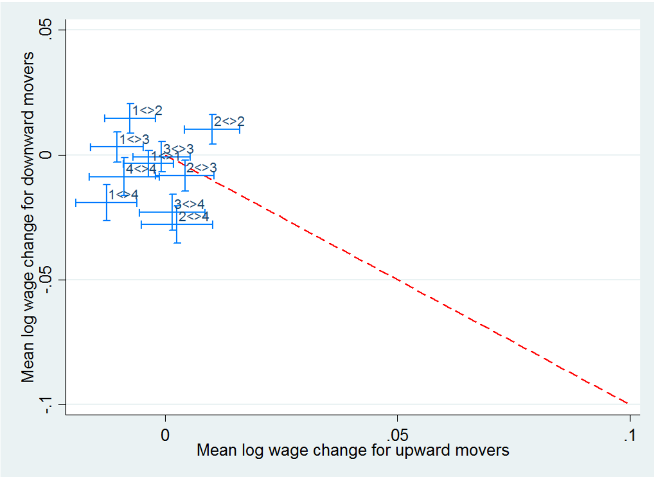


(c) Firm classification based on the firms' value added per employee

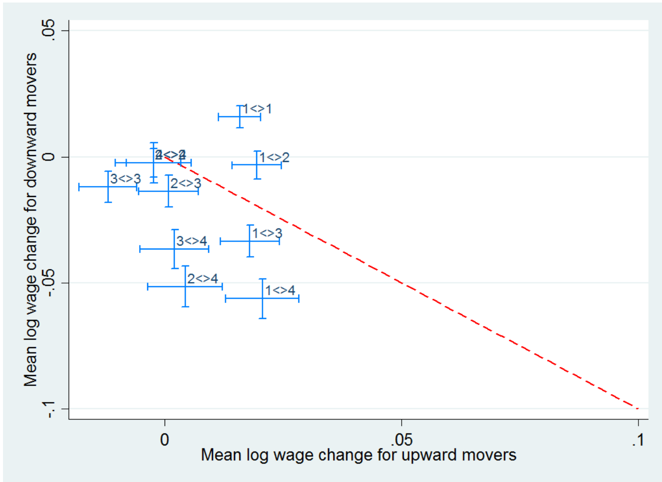
(d) Firm classification based on the firms' poaching index

Note: The figures show the mean wage changes experienced by JtJ movers who transition between symmetric quartiles. In all panels, wages of job changers are regression-adjusted before to measure wage change (controls include year dummies, a quadratic and cubic term in age, and their interactions with our education measure). Firms are categorized based on (a) economic profit per employee, (b) firm equity per employee, (c) value added per employee, and (d) the poaching index. 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 represents the case of perfect symmetry between wage gains and wage losses experienced by job movers (slope = -1).

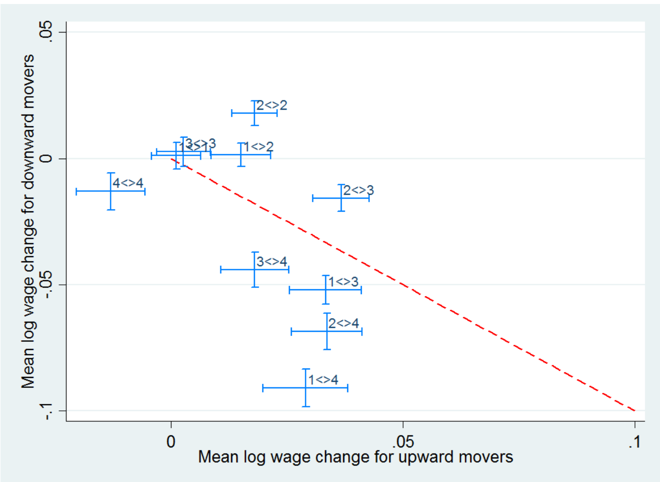
Figure 7: Wage Gains vs. Wage Losses for JUJ Movers, Firm Classification Based on Accounting Data



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



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



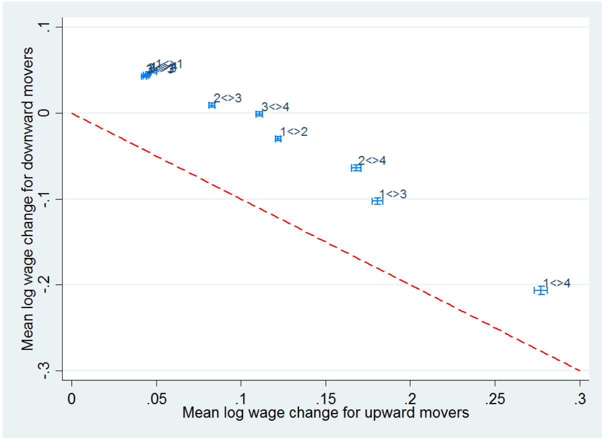
(c) Firm classification based on the firms' value added per employee

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

Note: The figures show the mean wage changes experienced by JUJ movers who transition between symmetric quartiles. In all panels, wages of job changers are regression-adjusted before to measure wage change (controls include year dummies, a quadratic and cubic term in age, and their interactions with our education measure). Firms are categorized based on (a) economic profit per employee, (b) firm equity per employee, (c) value added per employee, and (d) the poaching index. 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 represents the case of perfect symmetry between wage gains and wage losses experienced by job movers (slope = -1).

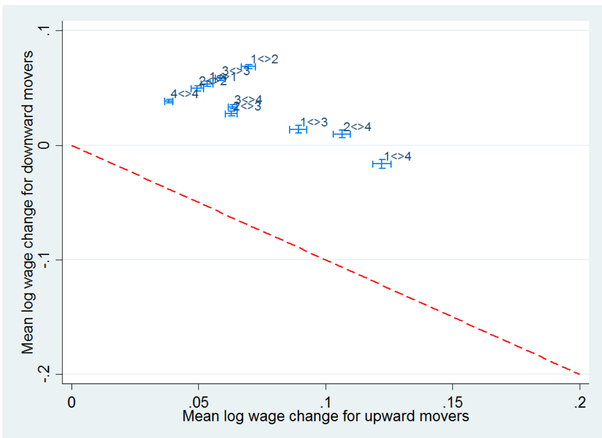
Figure 8: Wage Gains vs. Wage Losses for JtJ Changers, Firm Classification Based on AKM Firm Effects

(a) AKM firm fixed effects estimated on the full sample



(b) AKM firm fixed effects estimated on the JtJ sample

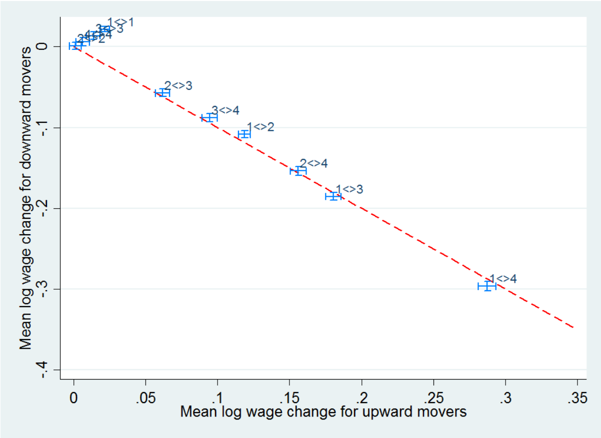
(c) AKM firm fixed effects estimated on the UJ sample



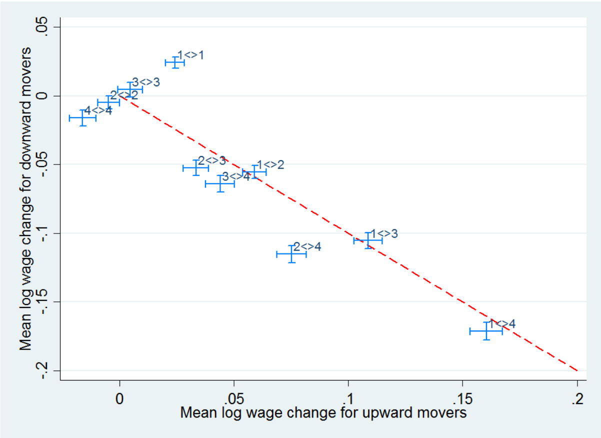
Note: The figures show the mean wage changes experienced by JtJ movers who transition between symmetric quartiles. In all panels, wages of job changers are regression-adjusted before to measure wage change (controls include year dummies, a quadratic and cubic term in age, and their interactions with our education measure). Firms are categorized based on their firm fixed effect obtained by estimating a two-way fixed-effect wage regression on (a) the full sample, (b) the JtJ sample, and (c) the UJ sample. Wage changes experienced by workers moving to a higher quartile are on the x-axis, and wage changes experienced by those moving to a lower quartile are on the y-axis. The dotted red line represents the case of perfect symmetry between wage gains and wage losses experienced by job movers (slope = -1).

Figure 9: Wage Gains vs. Wage Losses for JUJ Changers, Firm Classification Based on AKM Firm Effects

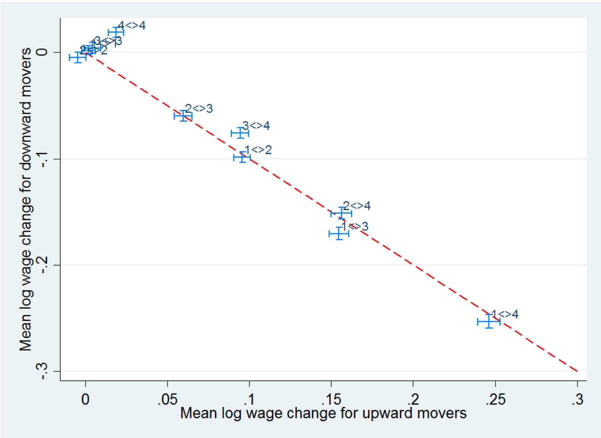
(a) AKM firm fixed effects estimated on the full sample



(b) AKM firm fixed effects estimated on the JtJ sample



(c) AKM firm fixed effects estimated on the UJ sample



Note: The figures show the mean wage changes experienced by JUJ movers who transition between symmetric quartiles. In all panels, wages of job changers are regression-adjusted before to measure wage change (controls include year dummies, a quadratic and cubic term in age, and their interactions with our education measure). Firms are categorized based on their firm fixed effect obtained by estimating a two-way fixed-effect wage regression on (a) the full sample, (b) the JtJ sample, and (c) the UJ sample. Wage changes experienced by workers moving to a higher quartile are on the x-axis, and wage changes experienced by those moving to a lower quartile are on the y-axis. The dotted red line represents the case of perfect symmetry between wage gains and wage losses experienced by job movers (slope = -1).

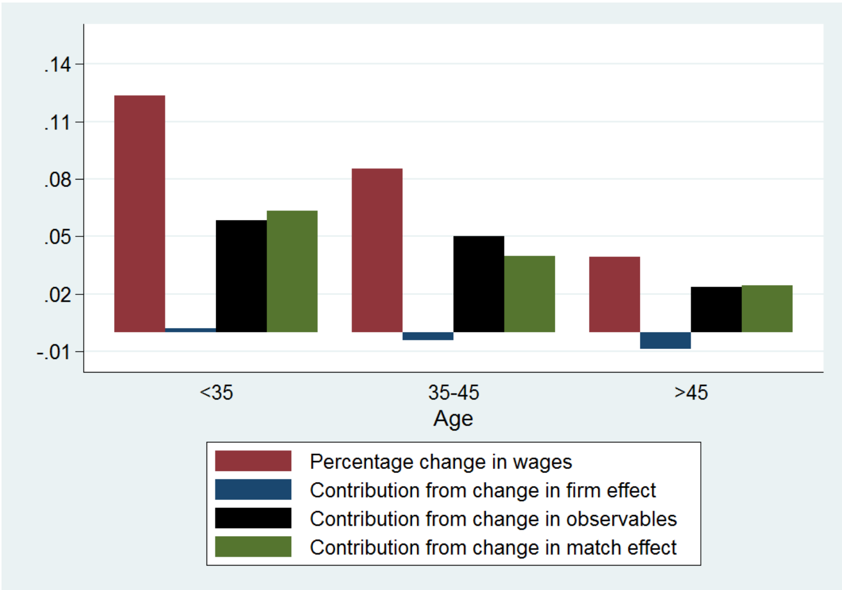
Figure 10: Cumulative Distribution Functions of Wage Growth

(a) Percentage change in raw wages

(b) Percentage change in residual wages

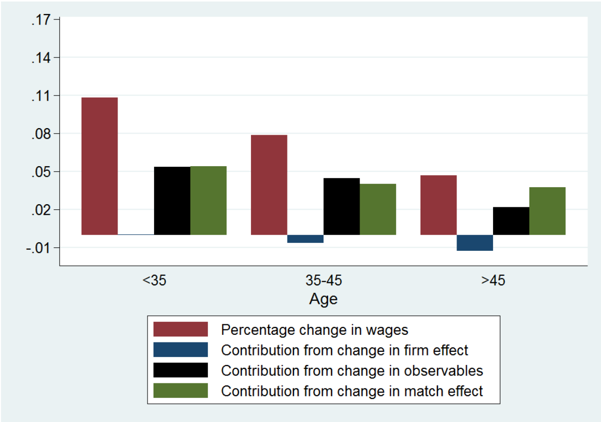
Note: Panel (a) shows the distribution of percentage change in raw wages, while Panel (b) shows the distribution of percentage change in residual wages (controls include year dummies, a quadratic and cubic term in age, and their interactions with our education measure). The panels report the distributions for both job-to-job movers and job changers who transitioned via unemployment.

Figure 11: Wage Decomposition by Age, Job-to-Job Movers

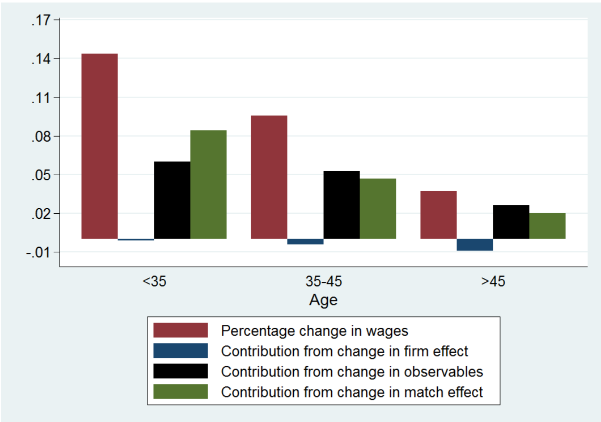


Note: The change in wages (red bar) is decomposed into the contribution from the change in firm effect (blue bar), and the change in observable characteristics (black bar), and the change in match effect (green bar). Job-to-job transitions are categorized based on the worker's age at the time of the transition. The firm fixed effect estimates are obtained by fitting a two-way fixed-effect wage model on the UJ sample (sub-sample of the full sample in which we keep only employment spells of workers hired directly from unemployment).

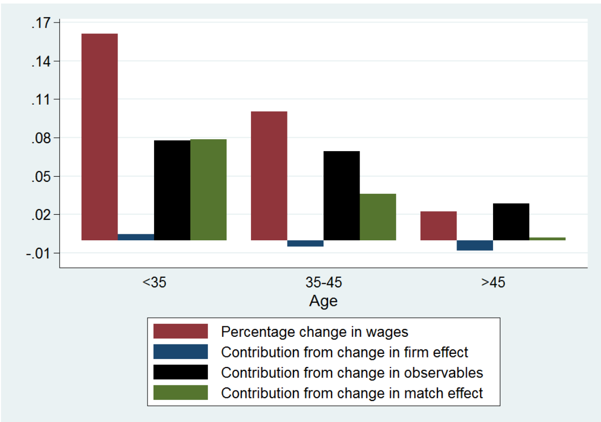
Figure 12: Wage Decomposition by Age and Working Position, Job-to-Job Movers



(a) Blue-Collars



(b) Middle managers



(c) Managers

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. Job-to-job transitions are categorized based on the worker's age at the time of the transition and on the working position in the origin job (blue-collars, middle managers, managers). The firm fixed effect estimates are obtained by fitting a two-way fixed-effect wage model on the UJ sample (sub-sample of the full sample in which we keep only employment spells of workers hired directly from unemployment).

Appendix A Results using the E-Indkomst Register

Beginning in 2007, Danish firms were asked to begin entering workers' pay and hours in an electronic system known as the E-indkomst register. The E-indkomst register contains information on all income for all residents of Denmark, including wages, pension income, welfare income, unemployment insurance, etc. Most importantly for us, the E-indkomst register includes data reported by firms on each worker's hours. Previously, firms reported worker income to the government, but not hours. Thus wages constructed using the IDA register must impute hours for workers based on pension contributions, as pension contributions increase in steps with hours worked. It is common practice to use only full-time workers in the IDA register, as we do in our analysis above, because it is likely that the hours imputation is more accurate for full-time workers. (CITE RUNE)

In this section, we first compare the IDA wages to similarly constructed E-indkomst wages for the three years in which our sample period overlaps, 2008-2010. We show that the correlation between log wages in the two registers for the same workers is high, but not exactly one. We then replace IDA wages in the period 2008-2010 with E-indkomst wages and rerun some of our analysis. We find no qualitative differences in our results.

A.1 Comparing IDA wages and E-Indkomst Wages

As described in our main text, IDA wages are the annual income from the primary job a worker held in November, divided by the imputed hours at that job. We use only full time workers, and drop one percent wage outliers in the left and right tails. For comparison we construct E-indkomst wages similarly. We define a worker's primary job in November as the job at which he worked the most hours. If there is a tie, then we choose the job with the highest November income to be the primary job. Next we sum monthly income and hours at this particular job to get annual income and hours. One intriguing aspect of the E-indkomst data is that it is unusual for workers to work 100% full time (160.33 hours) for a long series of consecutive months. Thus, we define a worker as full time in a year if she worked at least 90% full time at her November job's firm every month of the year.

We find 26,768 such year worker observations. This is a sizeable number of workers, but well short of every full-time worker in Denmark. Our criteria for full-time work in particular is somewhat restrictive. For example, if a worker worked 20% overtime in one month to meet a production deadline, and then 80% undertime the next month to compensate, we would drop that worker from our E-indkomst sample, although this worker may be counted as full time in IDA data. Even so, we can at least be sure that all the workers in the comparison sample were full-time workers, and wages were constructed as similarly as possible to the construction of the IDA wages we use in our baseline estimates.

Table 1, Panel A contains information on wages calculated for the same full-time workers in the two registers. Wages in IDA across the board are around 15% higher than those in the E-indkomst register. It is likely that some income counted in IDA as coming from the firm was counted in E-indkomst as coming from another source. Reading the documentation for the two data sets, we could not figure out why they should have different levels.[31] On the other hand, the log wage standard deviations are similar, implying that, in the two data sets, wages fluctuate in the cross-section by about the same percentage. The correlation between the two variables is also reassuringly high, at 96% (Table 1, Panel B).

We can also regress E-indkomst log wages on IDA log wages. Results from this regression are reported in Table 1, Panel C. The slope of the relationship between log wages for full-time workers is close to one, and the R^2 is 0.924.

Because of data disclosure rules, we are unable to provide a scatter plot of the wages against each other, as this would disclose individual wage observations. Instead, Appendix Figure 1 contains a two-dimensional kernel density plot. Contours bound areas with more observations. As implied by our summary table above, there is a nearly linear relationship with slope approximately one between log wages. The density is slightly below the 45 degree line, because wages in IDA are as a rule around 15% higher than wages in the E-indkomst register.

Overall, wages in the two data sets for full-time workers appear to be similar, except in levels. Wages in IDA are around 15% higher than wages in the E-indkomst register. The cause of the discrepancy in levels is an interesting topic for researchers using the Danish register database, but since it is not the topic of our paper we leave it for future research.

[31] Specifically, we used the narrow measure of income (ajo_smalt_loenbeloeb) to construct our E-indkomst wage measures, as is recommended by DST to compare with the IDA wage measure timelon. This measure does not include pension contributions. For more information, see documentation here https://www.dst.dk/da/Statistik/dokumentation/Times/ida-databasen/ida-ansaettelser/timelon

A.2 Our results with E-indkomst wages when available

In the last section, we showed that E-indkomst wage data for full-time workers does not differ significantly from wage data in the IDA register. The wages in the two data sets are not, however, exactly the same. In this section, we present symmetry tests from the body of our paper using wage data from 2000-2010, but with E-indkomst wages replacing IDA wages when available in 2008-2010. These tests are contained in Appendix Figure 2. For all movers and job-to-job movers, wages drop less when moving to lower wage firms than they rise when moving to higher wage firms. For unemployment-to-job workers, there is no statistical difference in wage changes when moving to higher or lower wage firms. These results are qualitatively the same as in our baseline.

Appendix B Symmetry Tests and Wage Change Adjustments

In this section, we propose some explanations for the discrepancy found between our findings and those of earlier studies. First, when analyzing the wage gains and drops experienced by job changers transitioning across firms that belong to different quartiles, Card et al. (2016) propose to use a residual wage change, controlling for observable characteristics and using the coefficient estimates obtained by fitting a wage model to the sample of job stayers. The observable characteristics used in the wage model are age, education, and a quadratic term in age interacted with education. This method generates a measure of wage change that is controlled for the wage change that job stayers sharing identical age and education characteristics would experience. We argue that, if the wage growth of job stayers is affected by age and education, both included in the wage model, their wage growth is also largely explained by the tenure effect that is captured by the constant term. If we deduct the constant term together with the estimated impact of age and education (using the estimates from the sample of job stayers) from the observed wage change of job changers, we would subtract from the wage change experienced by job changers the average tenure effect experienced by job stayers. This would artificially decrease the wage change of job movers, and bring the wage gains and drops experienced by upwards and downwards movers closer to symmetry. Since we do not know how the method was exactly performed, this remains a possible explanation. In fact, using wage change measured over 3 years and deducting the constant term when adjusting wage changes, we find that the wage change plots are closer (but below) the symmetry line.

Moreover, Card et al. (2013) write, referring to the symmetry test performed on German data: "Another remarkable feature of [the symmetry test] is the approximate symmetry of the wage losses and gains for those who move between quartile 1 and quartile 4 establishments. This symmetry suggests that a simple model with additive worker and establishment effects may provide a reasonable characterization of the mean wages resulting from different pairings of workers to establishments." Also the authors reject that the raw wage gains of upwards movers moving to better firms are of the same magnitude as the raw wage losses of downwards movers, they find symmetry when adjusting wage changes by the change experienced by job changers who remain in the same quartile. First, we argue that the AKM wage decomposition implies that, up to the time-varying characteristics, workers moving across firms that belong to the same quartile should experience no change in their wage. Therefore, such an adjustment goes against the AKM wage model. Second, we argue that such an adjustment is in fact equivalent to adjusting for the match effect itself. Indeed, if we get asymmetry in wage gains and drops for workers transitioning across similar quartiles because of positive changes in match effects, adjusting for the wage change they experience is equivalent to adjusting for the positive match effect that brings asymmetry.

Appendix C Abowd-McKinney-Schmutte Tests

We perform on the Danish register data the two tests of exogenous mobility based on wage residuals proposed by Abowd et al. (2018) (hereafter AMS). The AMS tests investigate the validity of the following condition:

$$\mathbb{E}[r|X] = 0 \text{ and } Pr[D, F|X, r] = Pr[D, F|X] \tag{19}$$

Condition (19) is stronger than the exogenous mobility condition. This condition requires that the conditional distribution of D and F on r and X is identical to the conditional distribution of D and F on X. 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 (19) holds, then exogenous mobility (2) holds as well, but not vice versa. That is, Condition

(19) is sufficient for identification of AKM fixed effects, but not necessary.[32]

Both of the tests proposed by Abowd et al. (2018) are chi-squared independence tests of structural residuals and fixed effect estimates from the OLS estimation of (1). Since our implementation of these tests follows Abowd et al. (2018) exactly, we only briefly summarize one of the two tests in order to give the reader an idea of how they work. Readers interested in a more detailed discussion should refer directly to Abowd et al. (2018).

The "match effects test" looks at the relationship between a mover's wage residual at her source firm and the fixed effect of her destination firm. If Condition (19) holds, we should not be able to predict anything about the destination firm effect using her source firm wage residual. To implement the "match effects test", 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 match and period. For each match we calculate the mean residual wage $\overline{r}_{ij}$ to approximate the match effect.[33] Then, we discretize mean residual wages, estimated worker fixed effects, and estimated firm fixed effects into deciles for all workers.

With these deciles, we can now construct the test statistic. First we put movers into bins of worker effect decile, source firm effect decile, and destination firm effect decile. If Condition (19) holds, we should be able to predict the fraction of each of these bins in a source match effect decile by multiplying by the unconditional probability of being in a source match effect decile. That is, if 12% of all movers move from a match with match effect in the first decile, then 12% of the movers in *each of our bins* should come from a match with match effect in the first decile. Our test statistic is the sum across bins of the squared difference between actual and predicted frequency divided by the predicted frequency. Under the null hypothesis of independence, this test statistic is distributed

[32]For example, suppose that some lower wage jobs are governed by wage ladders standardized by unions. At all higher wage jobs, however, wages are freely set by the firm. Because of this, matches with high worker effects and low firm effects results in wage residuals with less variance than matches with high worker effects and high firm effects. This would violate Condition (19), since knowledge of the variance of wage residuals would convey information about worker and firm effects. As long as the conditional wage residuals are mean zero, however, the weaker exogenous mobility condition (2) would still hold. Indeed, any difference in higher order conditional wage residual moments– variance, skewness, eccentricity, and so on – will result in a violation of Condition (19). The requirement that all conditional wage residual moments are the same across worker and firm effect pairs is significantly stronger than the requirements that only one moment (the conditional mean) is the same.

[33]The mean residual wage is the mean across all periods in which a worker was working at a firm. We do this match by match rather than period by period to minimize the effect of random white noise within a match.

chi-squared, with 7,184 degrees of freedom.[34] We calculate the 95% critical value to be $7184 + 1.96 * (2 * 7184) = 7303$, while our test statistic is 743,448. The second AMS test, the so called "productive workforce test", has a similar implementation, but tests rather whether the mean wage residual of workers at a firm in one period can predict the worker effects at that firm in the next period. For this test we derive a test statistic of 33,415, which is distributed chi-squared with 810 degrees of freedom under the null. The 95% critical value in this case is 889. Thus both tests strongly reject Condition (19).

We also perform the AMS tests on unemployment-to-job spells. These tests continue to strongly reject Condition 19 on our preferred sample of unemployment-to-job spells. In particular, the first AMS test performed on unemployment to job spells results in a test statistic of 276,400, which under the null is chi-squared distributed with 5,803 degrees of freedom. The second AMS test results in a test statistic of 3,641,300, which under the null is chi-squared with 880 degrees of freedom. Since these tests are designed to evaluate a condition stronger than necessary for consistency in OLS, our results should not lead us to immediately reject that the sample of unemployment-to-job workers satisfy exogenous mobility.

[34] Theoretically we should have $(10^3 - 1) \times (10 - 1) = 8991$ degrees of freedom, since this is our number of bins (minus one) multiplied by the number of independent groups (minus one). In practice, however, some bins have zero frequency among movers.

Appendix Table 1: Summary Statistics, IDA and BFL Registers

Panel A: Moments		
	mean	std. dev.
E-ind log wage	5.290	0.333
IDA log wage	5.444	0.335

Panel B: Correlation	
	E-indkomst log wage
IDA log wage	0.961

Panel C: Regression results	
	E-indkomst log wage
IDA log wage	0.967
	(0.002)
Constant	0.027
	(0.009)
N	26,758
R^2	0.924

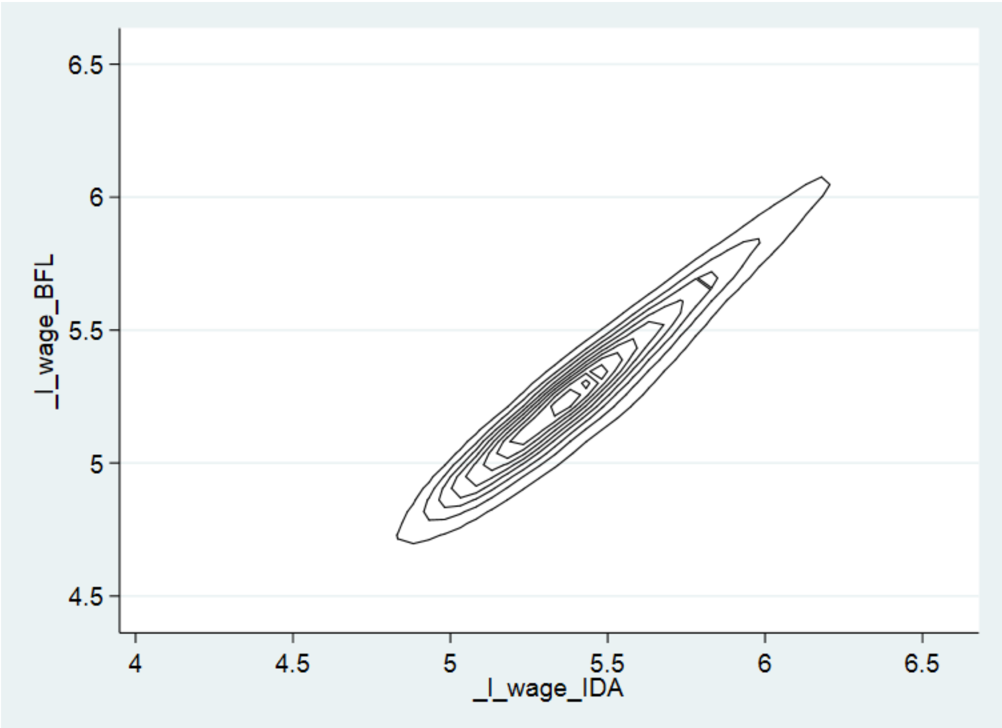
Note: Log wages of full-time workers, wage data from the IDA register and E-indkomst wage data from the BFL register.

Appendix Table 2: Decomposition of the Variance of Wages

	Std. dev.	Cov(w,y)	D(y)
Full sample			
Log real hourly wage $\ln w$	0.316	0.100	1.000
Time-varying observables $X\beta$	0.060	0.001	0.005
Worker effect α	0.280	0.076	0.760
Firm effect ψ	0.107	0.012	0.119
Composite error r	0.108	0.012	0.116
JtJ Sample			
Log real hourly wage $\ln w$	0.316	0.100	1.000
Time-varying observables $X\beta$	0.074	0.000	0.004
Worker effect α	0.291	0.079	0.792
Firm effect ψ	0.105	0.010	0.100
Composite error r	0.102	0.010	0.105
UJ Sample			
Log real hourly wage $\ln w$	0.278	0.077	1.000
Time-varying observables $X\beta$	0.060	0.001	0.015
Worker effect α	0.266	0.058	0.750
Firm effect ψ	0.149	0.011	0.145
Composite error r	0.084	0.007	0.091

Note: The JtJ sample is a sub-sample of the full sample in which we keep only employment spells of workers hired directly from employment. The UJ sample is a sub-sample of the full sample in which we keep only employment spells of workers hired directly from unemployment. The time-varying observables include year dummies, experience, and a quadratic term in experience. y represents each component of the AKM wage decomposition: time-varying observables $X\beta$, worker effect α, firm effect ϕ, and composite error r. $D(y) = \frac{Cov(y, \ln w)}{Var(\ln w)}$ represents the relative contribution of each component y to the variance of log real hourly wages.

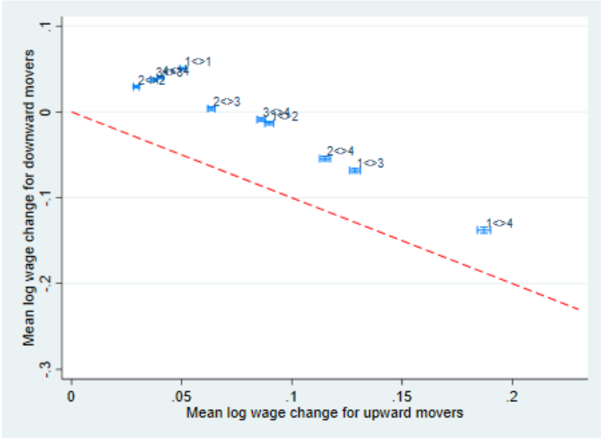
Appendix Figure 1: Kernel Density Plot



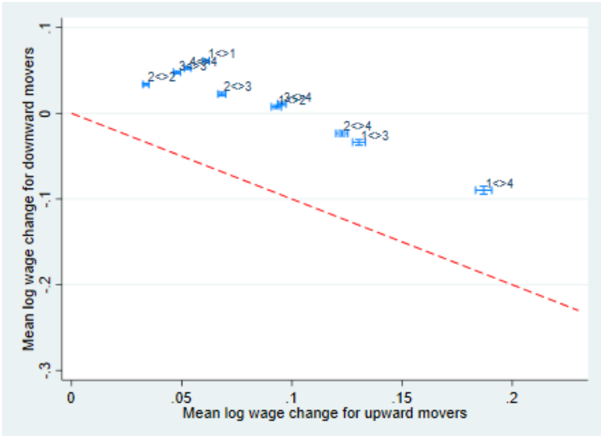
Note: Two-dimensional kernel density plot of log wages for full-time workers in IDA and E-indkomst (BFL register).

Appendix Figure 2: Wage Gains vs. Wage Losses for Job Changers, E-indkomst Wages

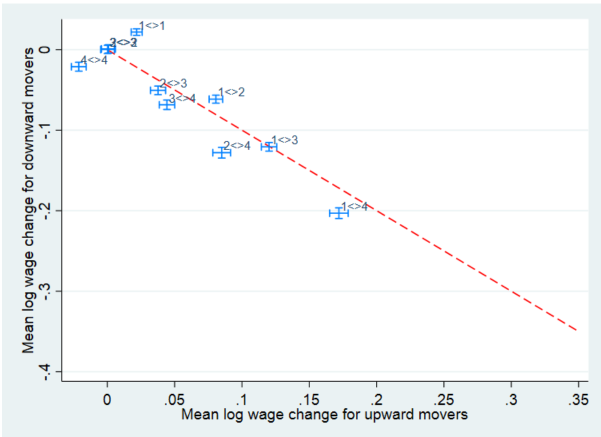
(a) Full mobility sample



(b) JtJ mobility sample



(c) JUJ mobility sample



Note: E-indkomst wage data (BFL register), 2008-2010. Jobs are categorized based on the mean residual wage of co-workers (controls include year dummies, a quadratic and cubic term in age, and their interactions with our education measure). In all panels, wages of job changers are regression-adjusted (controls include year dummies, a quadratic and cubic term in age, and their interactions with our education measure) before to measure wage change. The figures show the mean wage changes experienced by movers who transition between symmetric quartiles. Wage changes experienced by workers moving to a higher quartile are on the x-axis, and wage changes experienced by 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 (slope = -1). Panel (a) shows the wage dynamics of all job-to-job changers, while Panel (b) and (c) report the wage dynamics of JtJ movers and JUJ movers, respectively.