The Peer Effect on Future Wages in the Workplace*

Long Hong
Arizona State University

Salvatore Lattanzio
Bank of Italy and Dondena

November 29, 2024

Abstract

This paper examines workplace peer effects in two directions, leveraging employer-employee data for Italy. First, using a novel estimation approach and addressing endogenous worker-peer sorting, we estimate that a 10 percent increase in peer quality raises one's wage by 1.8 percent in the next year. The effect declines to 0.7 percent after five years. Second, in an event study around mobility episodes, we quantify wage changes associated with the entry and leave of high-quality and low-quality workers. Hiring high-quality workers positively affects peer wages, as does separating from low-quality workers. Movers experience immediate gains upon moving to high-quality peer groups.

Keywords: Peer effects, panel data, high-dimensional fixed effects, wage growth, linked employer-employee data

JEL codes: J24, J31, J41, C18, C23, M52

*We are indebted to the Editor and three anonymous referees, Jaime Arellano-Bover, Thomas Cornelissen, Bernardo Fanfani, Eric French, Lukas Freund, Zeina Hasna, John Kennan, Rasmus Lentz, Matthew Lindquist, Kai Liu, Hamish Low, Corina Mommaerts, Matthias Parey, Michel Serafinelli, Jeff Smith, Mikkel Sølvsten, Chris Taber, Eliana Viviano, Weilong Zhang and audiences at the University of Wisconsin-Madison, University of Cambridge, University of Reading, Bank of Italy, Annual Meeting of the Italian Association of Labor Economists (AIEL), European Winter Meeting of the Econometric Society, Annual Meeting of the Society of Labor Economists (SOLE), Ph.D. Workshop in Economics at Collegio Carlo Alberto and Brucchi Luchino Labor Economics Workshop for valuable comments. We thank Fondazione Rodolfo Debenedetti and Bocconi University for their generous help in accessing the data. The views expressed in this article are those of the authors and are not the responsibility of the Bank of Italy or the Eurosystem.
Contacts: Hong: long.hong@asu.edu; Lattanzio: salvatore.lattanzio@bancaditalia.it.

1 Introduction

The literature has well documented that wages typically increase over the life cycle. However, significant heterogeneity exists in wage growth among workers. In line with the canonical models in Becker (1964) and Ben-Porath (1967), wage growth reflects workers' accumulation of knowledge and skills on the job. As interaction is essential in the workplace, it is natural that on-the-job learning is primarily the result of interaction with coworkers. Despite the importance, little is known about the link between coworkers and wage growth from both empirical and theoretical perspectives. If two workers have the same ability, does the worker with better coworkers have higher wages in the future? How persistent are such peer effects? How does the move of a high-quality worker in a firm contribute to the wage growth of his or her new coworkers, and how are past ones affected? This paper seeks to shed light on these questions.

There is a growing literature that investigates the relationship between coworkers and wages.[1] Earlier empirical evidence mainly focuses on the effect of coworkers on contemporaneous wage levels in a specific workplace (e.g., Mas and Moretti, 2009) or based on laboratory experiments (e.g., Falk and Ichino, 2006). For example, Mas and Moretti (2009) provide persuasive evidence in a supermarket chain that a cashier's productivity increases when they work alongside more productive coworkers. Nevertheless, it is unclear to what extent these findings, based on a specific firm or laboratory experiment, apply to more general settings and labor markets. Increased access to administrative data allowed researchers to investigate peer effects in one or more local labor markets. Battisti (2017), Cornelissen et al. (2017), and Cardoso et al. (2018) use employer-employee matched administrative datasets to provide estimates of the peer effect in the workplace. Their studies focus on the overall impact of coworkers on contemporaneous wage levels, and consistently find a positive effect, though the magnitudes vary across different labor markets. However, if the peer effect is persistent over time, it would also be appropriate to consider dynamic effects: that is, the impact of coworkers could materialize on current *and* future wages. Despite the potential relevance, only a handful of papers have examined the link between coworkers and wage growth. Two related papers, Jarosch et al. (2021) and Herkenhoff et al. (2024) employ different structural models, and both find substantial knowledge spillovers from coworkers that facilitate wage growth, using data from Germany and the United States, respectively.

We aim to explore the causal effect of coworkers on wages in several directions. To this end, we use a matched employer-employee administrative dataset – the Veneto Worker History panel –, which covers the universe of private-sector workers and firms in Veneto, one of the largest Italian regions, from 1975 to 2001. The availability of the full employment information for all firms in the dataset is crucial for our purposes as it allows us to track workers and coworkers over a

[1] Throughout the paper we use the terms peer and coworkers interchangeably.

long period of time. The dataset further records broad occupations (blue-collar, white-collar, and managers), which we use as proxies for peer groups, which we define as workers in the same firm and occupation in a given year.[2]

With this data, we first explore the overall impact of peer quality on one's future wages. By incorporating a coworker component, measured by the average of leave-out coworkers' individual fixed effects, in the canonical AKM model (Abowd et al., 1999), our econometric strategy helps circumvent the common reflection problem (Manski, 1993) and accounts for workers' endogenous sorting into peer groups and firms. We estimate the model using a newly developed estimation approach via the method of moments, leveraging sparse matrices and the conjugate gradient method, to overcome the computational challenge induced by the high-dimensional fixed effects and matrix inversion. When estimating peer effects, we exploit two sources of identifying variation: changes in peer quality for workers who switch peer groups and changes in peer quality for workers who remain with their peer group as other workers join or leave it. We also prove the consistency of our estimator under a set of standard assumptions.

Our analysis reveals that peer effects are an important and persistent driver of future wages. Our baseline results show that a 10 percent increase in coworker quality, as measured by the average AKM worker effect of one's peers, implies an increase of contemporaneous weekly wages by 2.6 percent and next year's wages by 1.8 percent. The effect decays over time, but it is also substantial in the future, as we find that better coworkers increase one worker's wage by 0.7 percent after five years. The effect is stronger for workers moving to a new job, and for young and low-tenure workers. We do not find differences between workers employed by peer groups of different sizes.

In the second part of the paper, we delve deeper into the mechanisms that identify the peer effect and study mobility of workers across firms and how it affects coworkers' wages in the origin and destination firms. Specifically, we exploit an event study analysis around the mobility of workers and investigate how the *entry* of a high-quality worker, who could potentially transmit knowledge to her peers, changes the trajectory of peer average wages in the destination firm. As the choice to hire a worker is not random, comparing outcomes in firms that hire to those that do not hire would likely bias our estimates upward: hiring choices are correlated with firm performance and, therefore, average wages. We overcome this problem by selecting a sample of firms that hire a worker in a given year and analyzing coworkers' wages in firms hiring a high- or low-quality worker relative to firms hiring similar-quality workers: we define high, similar, and low-quality workers by comparing the estimated AKM worker fixed effect of the mover, from the first part of our analysis, to that of coworkers in the firm. Besides focusing only on firms hiring or separating from

[2]This definition of peer groups has limitations, as broad occupations may comprise more than one work team in a firm. However, the structure of the Italian labor market helps us circumvent this problem, as the average firm size in Veneto (and Italy, in general) is small and allows us to work with peer groups of comparable size to that reported in the literature (e.g., Cornelissen et al., 2017; Cardoso et al., 2018).

a worker, we perform ex-ante propensity score matching before mobility and assess the absence of observable differences between treated and control firms. Moreover, we use data for a subset of firms to show that mobility decisions in the matched sample are not correlated with leads and lags of sales and value added per worker, reassuring on the validity of our empirical strategy. These analyses reveal that hiring a high-quality worker is associated with an increase of peer wages of 3.1 percent relative to firms hiring similar-quality workers three years after mobility. The effect is stronger for coworkers belonging to the same occupation of the new hire, as one would expect that more interaction occurs in the workplace among them. The effect is not statistically significant, instead, for firms hiring low-quality workers.[3]

We also examine how wages of coworkers in the origin firm are affected by the departure of high- and low-quality workers. We find comparable results to those outlined above, as the departure of a high-quality worker leads to a drop in wages of about 1.6 percent for coworkers in the origin firm relative to firms separating from a similar-quality worker. In contrast, separating from low-quality workers benefits coworkers in the firm, whose wages increase by 2.8 percent on average in the three years after the move.

Finally, we shift the focus to movers and examine how wages of workers who move into different peer groups evolve over time, by comparing the wage trajectories of workers moving into high- and low-quality peer groups to that of matched workers moving into similar-quality peer groups. We find that workers gain from moving into high-quality peer groups. On average, their wage increases by 3.9 percent relative to workers moving into similar peer groups. We find, instead, a null effect for workers moving into a low-quality peer group. Taken together, these findings highlight the importance and persistence of coworkers – and high-quality ones, especially – in shaping wage growth.

Our paper makes three main contributions to the literature. First, we advance the understanding of peer effects in the workplace by examining not only their contemporaneous impacts but also their dynamic evolution over time. While previous studies typically focus on contemporaneous peer effects within single workplaces (e.g., Mas and Moretti, 2009; Papay et al., 2020; Brune et al., 2020; Sandvik et al., 2020) or within broader labor markets (e.g., Lengermann, 2002; Battisti, 2017; Cornelissen et al., 2017; Cardoso et al., 2018), our study broadens this scope by analyzing the dynamic effects of coworkers over time. The dynamic perspective allows us to offer new insights that a contemporaneous focus may overlook. Specifically, our study builds on and extends the findings of Cornelissen et al. (2017) and Battisti (2017), who document positive peer effects on wages within the Munich and Veneto labor markets, respectively (we use the same data source as Battisti, 2017). We go further by providing evidence on how peer influence affects both current and

[3] Herkenhoff et al. (2024) and Jarosch et al. (2021), using two different approaches, also find similar results, where workers catch up to more knowledgeable coworkers but are not dragged down by less knowledgeable ones.

future wages, shedding new light on the persistent impact of peer effects.[4] To this end, our paper also contributes to two small but growing strands of literature on workplace learning sources. On one hand, we add to the literature examining learning from coworkers as a driver of wage growth, such as Jarosch et al. (2021), Herkenhoff et al. (2024), and Nix (2020).[5] While these studies employ empirical specifications similar to ours, measuring worker quality through proxies like observed wages or educational attainment, we follow Arcidiacono et al. (2012) and instead use the average long-term productivity of coworkers (proxied by individual fixed effects) as our measure of peer quality. This approach not only allows us to circumvent the reflection problem but also, through an extensive set of fixed effects, addresses endogeneity concerns surrounding peer quality. On the other hand, we contribute to the literature exploring the firm-specific environment as a determinant of wage growth (e.g., Arellano-Bover and Saltiel, 2021; Gregory, 2019). However, much remains unknown about the inner workings of this "black box" of firm-specific factors. Our study offers novel insights by identifying and examining the role of coworkers in this context, providing new evidence on how peer interactions impact wage trajectories.

Second, we offer novel insights into the underlying mechanisms of peer effects by distinguishing between job stayers, who experience changes in peer composition as coworkers join or leave the firm, and job switchers, who alter their peer group by moving to different firms. Leveraging the richness of our data, we investigate the impact of worker mobility across firms on the wage growth of both movers and their peers. Previous research has highlighted the importance of hiring "good" workers for firm performance (e.g., Serafinelli, 2019), yet little is known about the trickle-down effects on *coworkers*. We bridge this gap and offer new evidence by providing comprehensive analyses on how hiring high-quality workers or moving into high-quality peers influences wage outcomes for both movers and peers.

Finally, we develop a novel estimation method for the coworker effect in the "augmented" AKM model via the method of moments, which improves the widely used iterative approach pioneered by Arcidiacono et al. (2012). Specifically, we do not impose a restriction on the parameter space of peer effect to be smaller than 0.4, which is used as a sufficient condition for the iterative convergence in Arcidiacono et al. (2012).

[4]Cornelissen et al. (2017) provide evidence on the dynamics of peer effects as an additional result, estimating the effect of lagged peer quality on worker wages while fixing current peer composition. They find that lagged peer quality only marginally impacts wages in high-skill occupations. Our approach departs from theirs by fixing current peer composition and examining its influence on workers' future wages. Our findings indicate larger effects, which we discuss further, highlighting both contemporaneous and dynamic impacts.

[5]The key distinction between our approach and the empirics in Jarosch et al. (2021) and Herkenhoff et al. (2024) lies in the measure of coworker quality: they use leave-one-out average wages, while we use leave-one-out worker fixed effects, which capture innate ability and time-invariant traits. Their findings indicate persistent and increasing peer effects over time, whereas we find diminishing peer effects, consistent with Nix (2020), who use the fraction of college-educated coworkers as a measure of quality. Despite these differences, all studies highlight the significant role of coworker learning in shaping future wages.

The rest of the paper is organized as follows. Section 2 describes the data and provides descriptive statistics. Section 3 describes the estimation strategy in the AKM framework and Section 4 reports the results. Section 5 describes the event study analysis around mobility episodes and presents the results. Finally, Section 6 concludes.

2 Data and Descriptive Statistics

Data We use social security administrative data that contain the entire working population and private firms in the region of Veneto in Northern Italy– the Veneto Worker History (VWH) dataset – from 1975 to 2001, used also in Battisti (2017), among others. We can observe every coworker of each worker over their working life. The database contains three types of administrative datasets: (1) a worker-level demographic register, (2) a firm-level record, and (3) an annual firm-worker social security contribution register. A brief description of each follows.

1. The worker register tracks over 3 million workers from 1975 to 2001. It records the entire working history of a worker in the private sector, as long as he/she worked one day in Veneto. It contains basic demographic information, including birth year and place, gender, nationality.

2. The firm register contains all private firms that employ each individual in the worker register.[6] It includes a firm's detailed information such as national tax code, address, start and closure dates, sector (based on Ateco 91 codes, broadly corresponding to NACE Rev. 1). This register also includes information on firms outside of Veneto if the worker has been employed in one such firm.

3. The last register links the firm and worker registers. A private firm has to report the payment to its workers and the corresponding labor contract to the National Institute of Social Security (INPS). Therefore, the register contains accurate information on annual earnings (equal to full net earnings, plus all kinds of pecuniary compensation, grossed up with labor income taxes and social security contributions on the employee, without top-coding), weeks worked, occupation (white-collar, blue-collar, manager, apprentice), type of contract (fixed-term or open-ended) and type of working schedule (full-time or part-time). Annual earnings have been inflation-adjusted to the price level of the year 2003.

[6]There are two important related points. First, the public sector is not included in this database. Second, the firm is not at the establishment level. It might be ideal to use establishment-level data for our analysis, but using firm-level data would not make a difference for two reasons. First, most firms, especially in our sample period where the franchise is not typical, are single-establishment firms. Second, the firm size is typically small, with a median size of six workers, and firms with fewer than 25 employees take up around 90 percent of the total number of firms.

Sample selection We use all workers and firms within the Veneto region only. We focus on the period from 1982 to 2001 because the information on working weeks before 1982 is not accurate (Battisti, 2017). Besides, we have a few minimal restrictions, mainly following the standard practice in the literature. First, if a worker holds multiple jobs in a year we keep only the main one, defined as the job with the highest annual earnings or the highest number of weeks worked (breaking randomly the few remaining ties, which account for less than 1 percent of the data), and we restrict the working ages from 16 to 65. Also, we exclude part-time jobs and apprentices because their wages cannot be compared to regular full-time employment (as we have no information on working hours). Since we are interested in coworkers, we drop single-worker firms. Following the practice of Cornelissen et al. (2017) and Caldwell and Harmon (2019), we also restrict, for computational reasons, the firm size to be smaller than 5000 (less than two percent of the sample observations). Lastly, due to the identification requirement in the AKM analysis below, we need to restrict the sample to the largest connected set – i.e., the largest groups of firms connected by worker mobility (Abowd et al., 1999) – which takes up around 97 percent of the sample. In the resulting data, workers are observed for 7 years on average.

Peer group definition We define the peer group as all the workers employed in the same firm with the same occupation in a given year, where the occupation is given by broad professional levels (blue-collar, white-collar and executive).[7]

Descriptive statistics Table 1 presents descriptive statistics of the sample used in the analysis. We have 17.7 million person-year observations, 2.5 million workers and 168 thousand firms. Full-time workers earn annually on average 33.4 thousand euros (in 2003 prices) and the mean weekly wage is 744 Euros. The average number of weeks worked is 42 (and the median is 52, indicating that the median worker is a full-time and full-year one). Average age is 34.5 and average tenure is 2.5 years. As expected, firms are small, reflecting the structure of the Italian labor market, with a mean firm size of 17 employees and a median of 6. Similarly, the peer group size – that is, workers in the same occupation and firm – is on average 12 and 4 at the median. The mean number of movers per firm is 4, indicating that, on average, 4 workers move to other firms or to non-employment. Overall, 61 percent of workers change job at least once throughout the whole period of analysis. The share of women is 36 percent, reflecting the relative low female labor force participation. The majority of workers are employed in blue-collar occupations (70 percent) and are on open-ended contracts (97 percent). More than half of the workers are employed in manufacturing (53 percent).

[7]Compared to Cornelissen et al. (2017), who use a similar peer group definition, we have a lower detail of occupational categories, as we do not have detailed occupation codes. However, given the small average size of firms in Veneto – especially in the period of time we focus on – we end up with peer groups that are comparable in size to those reported in Cornelissen et al. (2017): the average peer group size in our data is 12, whereas in their paper is 9.3.

Table 1: Summary statistics

	(1) Mean	(2) S.D.	(3) Median
Annual earnings	33350.06	40250.33	31730
Weekly wage	744.38	1652.81	652
Weeks worked	42.41	15.24	52
Age	34.54	10.69	32
Tenure	2.45	2.58	2
Firm size	17	75	6
Movers per firm	4	26	1
Peer group size	12	54	4
Mover	0.61	0.49	
Woman	0.36	0.48	
Blue-collar	0.70	0.46	
Open-ended contract	0.97	0.16	
Manufacturing	0.53	0.50	
Person-year observations		17,723,260	
Number of workers		2,531,411	
Number of firms		168,613	

Notes. The table reports means, standard deviations and medians of each variable in columns (1) to (3), based on the largest connected set of workers and firms from the Veneto Worker History Panel. See text for details about data and sample restrictions.

There exists considerable heterogeneity in wage profiles for workers employed in peer groups of different quality. To see this point, we run a canonical two-way fixed effects AKM regression (Abowd et al., 1999), i.e.,

$$w_{it} = \alpha_i + \psi_j + \mathbf{x}_{it}'\gamma + \varepsilon_{it}, \tag{1}$$

where w_{it} are log weekly wages of individual i at time t; α_i are worker fixed effects; ψ_j are firm fixed effects; $\mathbf{x}_{it}$ contains age squared, tenure, tenure squared, and a dummy for tenure larger than ten years; ε_{it} is an error term.[8] Using the estimates of α_i from equation (1), we compute for each worker the leave-one out average peer quality as the average worker effect of his or her coworkers

[8] Various papers have tested the appropriateness of the AKM model in the Italian administrative data (Casarico and Lattanzio, 2024) and in the Veneto Worker Histories (Fanfani, 2022; Devicienti et al., 2019). We further corroborate this evidence in Appendix A, where we report the canonical tests of conditional random mobility, following Card et al. (2013).

in a given firm and year.[9] We then show descriptively how wage growth varies for workers in firms with better or worse peers, by grouping workers into different quantiles of the peer quality distribution. The results are reported in Figure 1 for white-collar and blue-collar workers in panels A and B, respectively. The figure depicts the growth in log weekly wages (i.e., the difference relative to the entry log wage) for workers entering the firm at age 25 by tenure with the firm and by peer quality, grouped in six discrete groups based on percentiles of its distribution: below 10, 10-25, 25-50, 50-75, 75-90 and above 90. The figure shows that there is wide heterogeneity in wage growth for workers joining a firm in different parts of the peer quality distribution, for both blue- and white-collar employees. Six years after joining the firm, white-collar workers in the top decile experience 0.16 ($= 0.36 - 0.20$) log points larger wage growth than a worker in the bottom decile of peer quality, whereas for blue-collar workers the additional wage growth equals 0.12 ($= 0.19 - 0.07$) log points. The difference in wage growth between workers with better and worse peers signals the contribution of the workplace environment to wage growth. However, we can say little about the direct effect of peers on wage growth as this descriptive analysis does not rule out sorting on productivity between workers and firms. This evidence likely suggests that part of the differential in wage growth is attributable to peers and part to firm-time and occupation-time specific shocks. In the next section, we adopt a more formal empirical strategy to measure the causal effect of peers on wages.

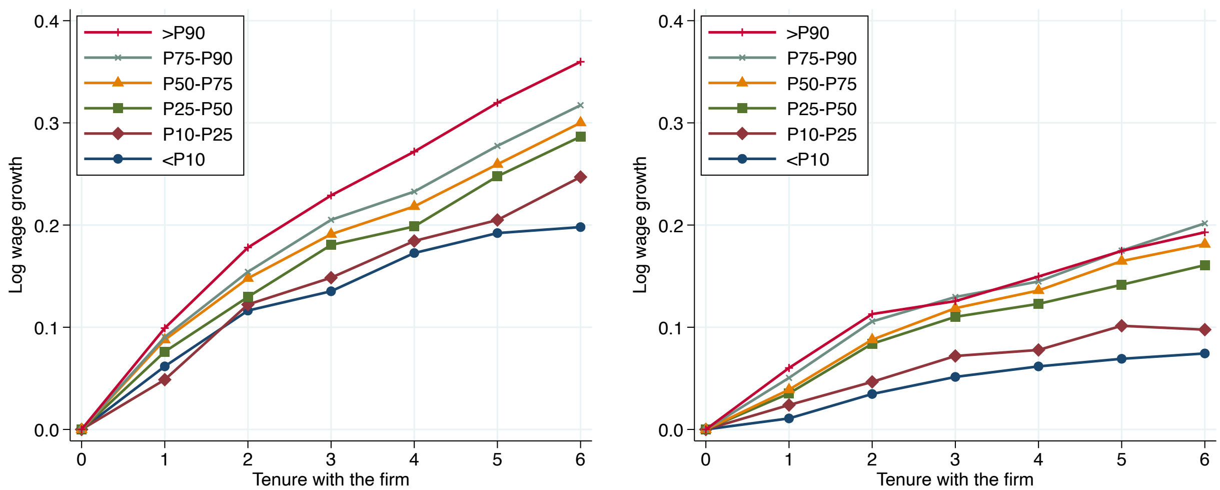
3 An AKM Approach to Identify Peer Effects

In this section, we explore the overall effect of coworkers on future wages. Specifically, we build our empirical strategy on the canonical AKM model (Abowd et al., 1999), by incorporating the average peer quality and additional fixed effects to better deal with the sorting of workers across firms and occupations. Moreover, we discuss in detail the identification of the model and our estimation method.

[9] In other terms, we compute for each worker i the following quantity:

$$\overline{\alpha}_{-i,j,t} = \frac{1}{|N_{-i,j,t}|} \sum_{k \in N_{-i,j,t}} \alpha_k.$$

$|.|$ defines the modulus of the coworker vector $N_{-i,j,t}$; hence, $|N_{-i,j,t}|$ represents the number of coworkers.



(A) White-collar, hired at age 25

(B) Blue-collar, hired at age 25

Figure 1: Log wage growth for workers entering the labor market in different quantiles of the peer quality distribution, by years of tenure and occupation

Notes. The figure reports the difference between average log weekly wages in tenure years 1 to 6 and baseline log weekly wage in tenure year 0 for workers entering the labor market in different quantiles of peer quality, defined as the average firm-level leave-one out worker effect estimated from equation (1). The sample includes workers hired at age 25. Panels A and B show results for white- and blue-collar workers, respectively.

3.1 Empirical Strategy

Our regression specification builds on Cornelissen et al. (2017), Battisti (2017) and Nix (2020), that include the coworker component into the canonical AKM model, as expressed in equation (2):

$$w_{i,t+h} = \alpha_i + \beta\,\bar{\alpha}_{-i,t} + \mathbf{x}'_{it}\gamma + \psi_{jt} + \eta_{ot} + \theta_{oj} + \varepsilon_{it}, \tag{2}$$

where

$$\bar{\alpha}_{-i,t} = \frac{1}{|M_{-i,t}|}\sum_{k \in M_{-i,t}} \alpha_k, \;\text{ and }\; M_{-i,t} = \{k : oj(k,t) = oj(i,t),\ k \neq i\}. \tag{3}$$

In equation (2), $w_{i,t+h}$ is the log weekly wage at time $t+h$, where $h \geq 0$. α_i is the worker fixed effect, which measures the portable component of earnings and is a proxy for quality or innate ability of a worker. $\alpha_{-i,t}$ is the average coworker's quality at time t. Specifically, we define the peer group as all the workers in the same occupation o within the same firm j as described in equation (3), where $M_{-i,t}$ is the set of all workers in the same peer group, *excluding* worker i. $\mathbf{x}_{i,t}$ is a set of individual time-varying characteristics, including quadratic polynimials in age (excluding the linear term, as suggested by Card et al., 2018) and tenure, and a dummy for tenure larger than ten years. ψ_{jt}, η_{ot}, θ_{oj} are firm-year, occupation-year, firm-occupation fixed effects. β is our parameter of interest. It describes how peer quality affects contemporaneous and future wages.

3.2 Identification Challenges

We face three challenges in the identification of peer effects: (*i*) the reflection problem; (*ii*) the non-random sorting of workers across peer groups; (*iii*) the presence of unobserved correlated shocks. The reflection problem was first introduced by Manski (1993) who referred to it when discussing the problem of identifying the peer effect from contemporaneous peer effort or productivity (e.g., wages). For example, in a firm, the effort of peers influences a worker's effort, which in turn affects his or her peers. In the presence of such "reflection," it is difficult to identify the peer effect. As explained by Cornelissen et al. (2017), using long-term predetermined characteristics of peers solves the reflection problem as it avoids contemporaneous productivity measures interacting with each other. For this reason, we measure peer quality with the leave-out average AKM worker fixed effect, which we interpret as a proxy of peers' long-term productivity.[10]

We address the endogenous sorting of workers across peer groups and the presence of unobserved shocks by controlling for a rich set of fixed effects. Peer quality may be correlated with workers' wages if high-quality workers sort into high-quality peers. We therefore control for worker fixed effects α_i in equation (2), so to estimate the impact of within-individual changes in peer quality on wages. Moreover, peer quality can be correlated with worker's wages in the presence of sorting between high-quality workers into high-quality firms or occupations. For this reason, we include firm-time fixed effects ψ_{jt} that control for firm-level shocks, occupation-time fixed effects η_{ot} that control for different time trends in occupation-specific pay, and occupation-firm fixed effects θ_{oj} that control for the possibility that firms pay higher wages to specific occupations.

Even in the presence of the rich set of fixed effects discussed above, the estimate of β can still be biased if there exist unobserved background characteristics that vary at the occupation-firm-time level (i.e., at the peer group-time level) that are correlated with changes in peer quality observed between consecutive periods. To see this point, observe that there are two sources of variations for the identification of β. For job switchers, peer quality changes when they move to another firm. For job stayers, peer quality changes when other workers join or leave the peer group (either from other firms or from other occupations within the same firm).Both these variations entail changes in the peer group that allow identification of β. If such changes are correlated with time-varying occupation-firm-specific shocks, then the estimates of β would be biased. For example, the firm may decide to invest in automation which complements white-collar workers

[10] Jarosch et al. (2021) use wages to measure peer quality and to provide reduced form evidence before focusing on a detailed structural model of knowledge flows within the workplace. Since we are interested in estimating a causal parameter, we do not follow their approach and measure, instead, peer quality with the average leave-out worker effect to circumvent the reflection problem.

and substitutes for blue-collar workers. Assuming that skills and occupations are correlated, and therefore white-collar workers are more skilled, this would raise peer quality and firm output (and therefore wages) simultaneously, leading to an upward bias in the estimate of β. The opposite would be true if a firm decides to divest in some occupation-specific technology which would decrease peer quality, and firm wages simultaneously, biasing downwards the estimate of β. One way to deal with time-varying occupation-firm shocks would be to include an occupation-firm-time effect and therefore exploit within peer group variation to estimate β. However, this would limit the identification to changes in peer group sizes of the job stayers only.For this reason, we prefer our baseline specification, which exploits variation coming from both stayers and movers.

The discussion so far implicitly assumes that peer quality is an observed quantity, which in fact is not. The following section discusses how we estimate peer effects when both peer quality and other fixed effects are unobserved.

3.3 Estimation

There are at least two main difficulties in the estimation of β in equation (2). First, the worker fixed effect is unobserved and needs to be estimated, but, at the same time, the average coworker quality is a function of the worker fixed effects. Second, the high dimensionality of the fixed effects makes it hard to solve the resulting system of equations. We employ a standard method of moments strategy, leveraging sparse matrices and the conjugate gradient method to overcome the computational burden of the estimation.

To facilitate the analysis, we rewrite equation (2) in matrix form:

$$w = A\alpha + F\psi + O\eta + V\theta + X\gamma + \beta_0 \cdot \tilde{C}\alpha + \varepsilon, \tag{4}$$

where $w \in \mathbb{R}^n$ is the log weekly wage and n is the number of observations. A, F, O, V are the matrices containing all the corresponding dummies of the fixed effects $\alpha, \psi, \eta, \theta$, respectively. X is a matrix containing all the observables. $\tilde{C}$ is a coworker averaging matrix (see Appendix B.1 for details on its construction), where $\tilde{C}$ has the same dimension $n \times \ell$ of A, with ℓ being the number of workers, and $\tilde{C}\alpha = \bar{\alpha}_{-i,t}$. $\beta_0 \in B$ is our parameter of interest, where B is a compact parameter space. Rearranging terms of equation (4), we have

$$w = \begin{bmatrix} A & F & O & V & X \end{bmatrix} \begin{bmatrix} \alpha & \psi & \eta & \theta & \gamma \end{bmatrix}' + \beta_0 \cdot \begin{bmatrix} \tilde{C} & \mathbf{0} \end{bmatrix} \begin{bmatrix} \alpha & \psi & \eta & \theta & \gamma \end{bmatrix}' + \varepsilon. \tag{5}$$

Defining $Z = \begin{bmatrix} A & F & O & V & X \end{bmatrix}$, $C = \begin{bmatrix} \tilde{C} & \mathbf{0} \end{bmatrix}$, where $\mathbf{0}$ is a matrix that has the same dimension $n \times r$ of $\begin{bmatrix} F & O & V & X \end{bmatrix}$, with r being the total number of coefficients except worker fixed effects in equation (2), and $\delta = \begin{bmatrix} \alpha & \psi & \eta & \theta & \gamma \end{bmatrix}'$, equation (5) can be rewritten in a compact matrix form below:

$$w = Z\delta + \beta_0 \cdot C\delta + \varepsilon, \tag{6}$$

The peer effect β is estimated by solving a non-linear least squares problem in the following objective function Q_n:

$$\widehat{\beta} = \arg\min_{\beta \in B} Q_n(\beta) = \arg\min_{\beta \in B} \left\{ \min_{\delta \in \mathbb{R}^2} \|w - Z\delta - \beta \cdot C\delta\|^2 / n \right\}. \tag{7}$$

We derive the moment condition for β by taking the first order conditions of β and δ in equation (7) and get:

$$S_n(\beta) = w'MC \left(R'R\right)^{-1} R'w/n = 0, \tag{8}$$

where $R = Z + C\beta$ and $M = I_n - R\left(R'R\right)^{-1} R'$. We propose the following theorem for the consistency of $\widehat{\beta}$.

Theorem 1 *Under assumptions A1-A3, the estimate $\widehat{\beta}$ in equation (7) is a consistent estimate of β_0, where β_0 is the unique minimizer of the population analog to Q_n, with $\mathbb{E}[S_n(\beta_0)] = 0$.*

A1. Exogeneity. $\mathbb{E}[\varepsilon | Z, C] = 0$*;*

A2. Homoskedasticity. $\mathbb{E}[\varepsilon\varepsilon' | Z, C] = \sigma^2 I_n$*, where* $\sigma^2 > 0$ *is unknown;*[11]

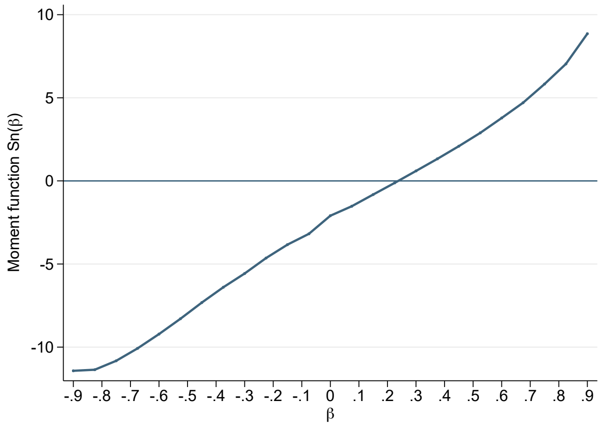
A3. Full rank. The design matrix $X + C\beta$ has full rank k for any $\beta \in B$.

Proof. See Appendix B.2.

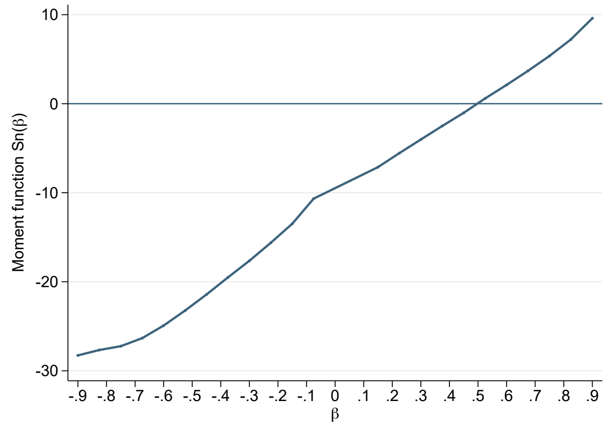
Theorem 1 ensures that the solution to the moment condition (8) is consistent. However, the computation of the moment is still demanding. We exploit the feature that these matrices contain a large fraction of zeros by applying the sparse matrix operation to mitigate the computational burden. Moreover, the matrix inversion could be slow and infeasible due to the high dimensionality of the matrices. We solve this issue by employing the conjugate gradient method.

Our method is nevertheless a parallel yet more general development of Arcidiacono et al. (2012), who use an iterative method.Specifically, a sufficient condition for the iterative method to converge

[11] Although the homoskedasticity assumption is almost universal in the peer effect literature (see Arcidiacono et al., 2012), one could relax it to allow heteroskedasticity. Hong and Sølvsten (2021) propose a new estimator that shows non-negligible bias in estimating classroom peer effects. We have applied their method to the local labor market of Padua (due to computational barriers, as their method is computationally intensive, we cannot use our entire sample). We find that the heteroskedasticity estimator is somewhat larger than our estimate. We, therefore, maintain the homoskedasticity assumption in order to focus on the larger Veneto region rather than smaller local labor markets. Results are available upon request.



(A) Empirical moment function



(B) Moment function in simulated data

Figure 2: The behavior of the moment function $S_n(\beta)$

Notes. The figure shows the value of the moment function $S_n(\beta)$ derived in equation (8) as we change β using our main analysis sample described above. Figure (A) is calculated using our main sample, and Figure (B) is computed using one of the simulated data.

to a fixed point is that the true underlying parameter of interest, β_0, is smaller than 0.4 (Theorem 2 on page 430, Arcidiacono et al., 2012). In our method, we have minimal requirements for the parameter space B. We follow the linear-in-mean peer effect literature and set our parameter space to be between -1 and 1, and practically, we use $B = [-0.9, 0.9]$.[12] As shown in Figure 2A, the moment function (8) estimated using our main sample is very smooth and monotonically increases in β and it crosses zero – i.e., $S(\beta) = 0$ – at $\beta = \widehat{\beta}$. The figure also shows that β is well-identified and uniquely estimated over the entire parameter space.

To further explore the behavior of our estimator when $\beta_0 > 0.4$, we conduct a simulation exercise. Specifically, we use the estimated parameters, $\widehat{\delta}$, from the previous exercise for equation (6). We set the “true" underlying peer effect $\beta_0 = 0.5$ and simulate the new outcome variable w by drawing homoskedastic errors from a normal distribution $N(0, var(\widehat{\varepsilon}))$, where $\widehat{\varepsilon}$ is also estimated from the previous exercise. We use the same estimation method mentioned above to estimate β using the simulated w in 1,000 repetitions. Our estimator has an average bias of 0.003, and the nominal 95 percent confidence interval coverage rate is 93 percent. Figure 2B shows the moment function (8) using one of the simulated data. It behaves similarly to Figure 2A and crosses zero close to the true $\beta_0 = 0.5$.

[12]Note that when $\beta = 1$, the design matrix $X + C\beta$ does not have a full rank, violating Assumption 3 of Theorem 1.

4 Results on the Peer Effect on Future Wages

4.1 Baseline Results

Summary statistics after estimation Table 2 reports summary statistics from the estimation of equation (2) for $h = 0$. The standard deviation of log weekly wages is 0.44. As typical in the literature that studies decompositions of the variance of wages, most of the variability in wages is accounted by variability in worker fixed effects (see Card et al., 2018, for a review). The standard deviation of the average peer fixed effect is 0.18 and, more importantly, the correlation between worker fixed effect and average peer fixed effect is 0.55, highlighting a positive assortative matching between workers and coworkers, documented as well, for example, in Lopes de Melo (2018). The table also reports the standard deviation of the change in peer effects between consecutive years. The identification of β in equation (2) rests on changes in peer quality between subsequent years. Hence, one needs sufficient variation in peer quality to identify β. The standard deviation of the change in the average peer fixed effect equals 0.09.

We also distinguish between movers and stayers: for the former, changes in peer quality happen because they move into a new peer group; for the latter, changes in peer quality happen if peers join or leave the current peer group. Not surprisingly, there is larger variation in the change in peer quality for movers than for stayers (0.17 and 0.07, respectively), as for the latter peer quality may not change at all between consecutive years. We corroborate this finding by plotting the density of the change in peer quality for movers and stayers in Figure E.1, which shows the existence of a mass around 0 for stayers (i.e., when the peer group does not change) and more variability for movers. Even for stayers, the standard deviation of peer quality changes amounts to approximately 37 percent of the overall variability in peer quality, indicating substantial variation in the data to identify the peer effect.

Main estimates of the peer effect Figure 3 shows our baseline results. Each dot in the graph represents the estimate β in equation (2) using current and future wages as the dependent variable in each year ahead (h), where $h \geq 0$.[13] The shaded area is a 95 percent confidence interval, retrieved from bootstrapped standard errors. The figure shows that the peer effect is large not only for the contemporaneous wage but also for the wages in the following years. A 10 percent increase in peer quality increases the contemporaneous wage by 2.58 percent. This result is within the range of estimates reported in the literature adopting a similar identification strategy. In particular, a one standard deviation increase in peer quality increases contemporaneous wages by 4.6 percent (0.258*0.178). In Cornelissen et al. (2017) by 0.3 percent, in Cardoso et al. (2018) by 5.7 percent

[13]Different future wages are used as outcomes in separate estimations. In the cases when $h > 0$, workers who do not have wages in year $t + h$ are excluded. Therefore, each coefficient is estimated on different samples.

Table 2: Standard deviation of wages and fixed effects and correlation between fixed effects

Statistic	Value
Standard deviation log weekly wages	0.436
Standard deviation worker fixed effect	0.269
Standard deviation peer fixed effect	0.178
Standard deviation occupation-time fixed effect	0.065
Standard deviation firm-occupation fixed effect	0.103
Standard deviation firm-time fixed effect	0.137
Standard deviation change of peer fixed effect between t and $t-1$	0.090
Standard deviation change of peer fixed effect between t and $t-1$ for movers	0.173
Standard deviation change of peer fixed effect between t and $t-1$ for stayers	0.066
Correlation worker fixed effect/peer fixed effect	0.551

Notes. The table reports summary statistics from the estimation of equation (2) for $h = 0$, based on the largest connected set of workers and firms from the Veneto Worker History Panel. See text for details about data and sample restrictions.

and in Battisti (2017) by 7.8 percent. Differences are likely due to the different labor markets being analyzed. Differences with Battisti (2017), who uses the same data source as ours, may be also a consequence of different sample selection – in particular, we use a narrower peer definition by occupations and use weekly instead of monthly earnings as the wage measure – and model specification – we include a richer set of fixed effects (i.e., firm-year, occupation-year, and firm-occupation fixed effects).

Next year's wage increases by 1.78 percent in response to a 10 percent increase in peer quality (see Table E.1 for detailed estimates and sample sizes). These are sizeable effects because the return to one year's experience during the same period was around 1.36 percent as shown in Table E.2. The effect gradually fades out to around 0.73 percent after five years. It is consistent with other papers that coworkers in the past three years play the most important role in wage growth (e.g., Caldwell and Harmon, 2019). The peer effect estimates are similar when we restrict to large local labor markets such as Padua and Venice, as Figure E.2 shows.

The peer effect can be a result of different factors. As highlighted in Cornelissen et al. (2017) peers may boost productivity and wages by a mechanism of either peer pressure, according to which a worker increases her own effort in response to increased effort by her coworkers, or production complementarity, for which a worker's productivity can be improved with the help of a good coworker (see, e.g., Moretti, 2004a, on production complementarities within geographic locations or Battisti et al., 2024, on their role in attenuating worker responses to idiosyncratic shocks). At the same time, workplace interaction is crucial for human capital accumulation, as workers transmit knowledge among each other, which makes them more productive on the job (Jarosch et al., 2021).

One concern about our results is the effective ability of workers to bargain over their wages,

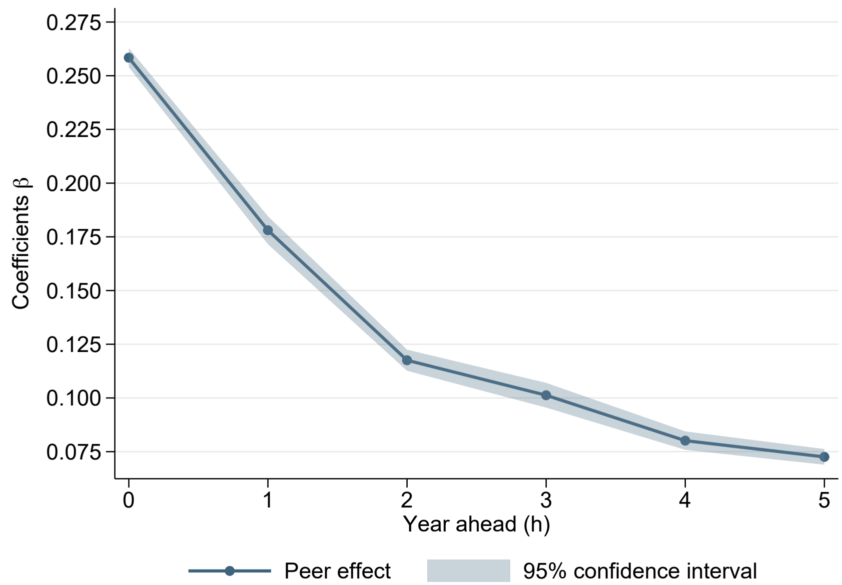


Figure 3: Peer effect on future wages (β)

Notes. The figure plots the estimates of β from equation (2) using the future wages as the dependent variable in each year ahead h, where $h = 0, 1, \ldots, 5$. Shaded areas are 95 percent confidence intervals, obtained from bootstrapped standard errors with clustering at the firm level. Detailed estimates and sample sizes are reported in Table E.1.

which requires some degree of wage flexibility. Italy is characterized by a two-tier wage-setting scheme, with collective contracts defining wage floors at the sector level. At the firm level, workers can bargain, individually or through employees' organizations, top-up agreements that increase their compensation. Moreover, part of the pay that we observe is composed of bonuses and premiums that employees receive on top of the basic compensation established by either the collective or firm-level contract. These wage premia are sizeable: Bartolucci et al. (2018) document that in the Veneto sample, wage premia are about 24 percent above the basic pay at the median. Hence, wage setting in the Italian context should be flexible enough to incorporate peer effects.

5 Mobility, Workers' Quality and Wage Growth

Thus far, we have focused on the impact of past coworkers on future wages and have found a positive and long-lasting effect. In this section, we switch our focus and analyze the peer effect in a different, albeit connected, perspective. In particular, we ask how important is worker mobility in shaping peers and movers' wages. In doing so, we distinguish between workers of different quality (or skill level) and separately analyze how the entry or leave of high- and low-quality workers affect coworkers in the origin or destination firms and how moving to peer groups with different average quality impacts movers' wages.

We move from the observation that, as mentioned before, the identification of β is achieved through the following channels:

1. for job stayers, the peer quality changes when a worker *enters* the peer group or when a worker *leaves* the peer group;

2. for job switchers, the peer quality changes as they move to another firm.

We separately study these channels, following the wage trajectories of workers and coworkers around mobility episodes in our data. Here, given the empirical strategy outlined below, we focus only on mobility across firms and not across occupations within firms. We study channel 1 by setting up a coworker-level event study around the mobility of a worker and analyze the evolution of wages of coworkers of the mover in the destination and origin firms. We distinguish three types of movers: high-quality, low-quality, and similar-quality, where the quality of the mover is based on a comparison of her worker fixed effect with the average peer fixed effect of coworkers from equation (2). In particular, we classify firms as hiring a high-quality or low-quality worker if her worker fixed effect is at least 10 percent higher or lower, respectively, than the average peer fixed effect of coworkers in the destination firm. We classify, instead, firms as hiring a similar-quality worker if her worker fixed effect is between -10 and $+10$ percent of the average peer fixed effect in the destination firm. When we focus on coworkers in the origin firm, we use the same classification, based on the comparison between leavers and coworkers in the origin firm.

We study channel 2 in a similar fashion. We follow the wage trajectories of workers moving into peer groups of different quality, where, again, we define a peer group as high, average, or low-quality if the mean peer fixed effect is 10 percent higher, between -10 percent and $+10$ percent, or 10 percent lower than the worker fixed effect of the mover.

For both channels 1 and 2 we provide evidence in Appendix D for when we define worker quality as a continuous variable to ease concern that the definitions of high-, similar-, and low-quality workers are arbitrary. Additionally, Appendix D provides robustness checks showing no correlation between firm performance and the worker mobility episodes that we study here.

5.1 Empirical Strategy

We now turn to a more formal illustration of our empirical strategy.

5.1.1 Coworker-level Event Study

Empirical design Figure 4 illustrates the definition of treatment and control groups in the event study. Primarily, we define the event as a worker who moves to a firm and stays there for three years. We choose three years as it takes time for the mover to have an effect on her coworkers' wages,

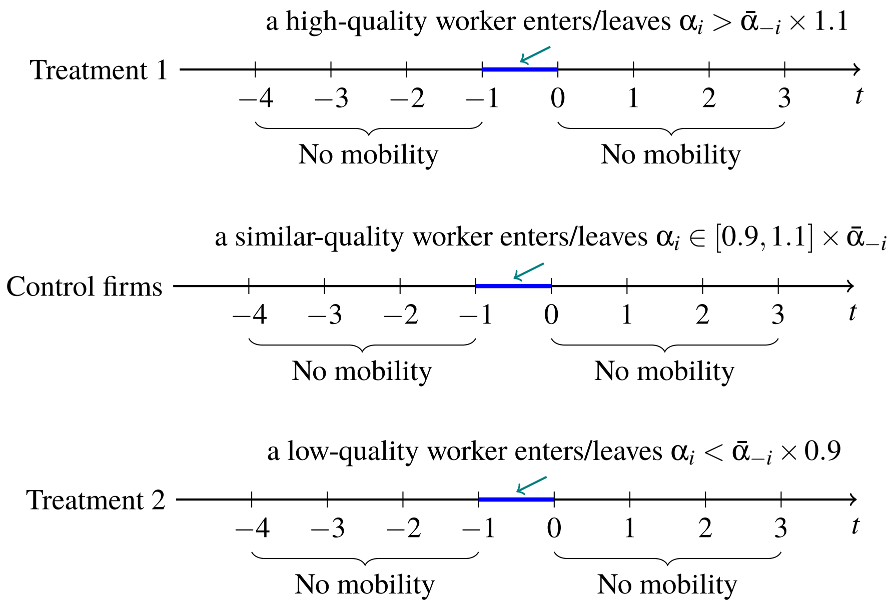


Figure 4: Treatment and control groups in the event study analysis, hire and leaver design

Notes. The diagram shows the empirical design behind the coworker-level event study estimated in equation (9). *Treatment 1* refers to mobility of high-quality workers, whose worker effect is more than 10 percent larger than the average peer effect in the origin or destination firm. *Treatment 2* refers to mobility of low-quality workers, whose worker effect is more than 10 percent smaller than the average peer effect in the origin or destination firm. *Control firms* refer to mobility of similar-quality workers, whose worker effect is between -10 and $+10$ percent of the average peer effect in the origin or destination firm.

especially because knowledge may need some time to be transmitted. When the event year has multiple workers entering, we exclude the firm from the analysis. We then use as outcome wages of coworkers of the mover (thus excluding the mover from the sample) in either the destination or origin firm, when we study the impact of the move on the "new" or "old" coworkers, respectively: we define these two analyses the "hire" design and the "leave" design.

We restrict the sample firms to be observed for eight consecutive years, in which there is no worker mobility in the four years before and three years after the event. While we risk losing generalization by such a substantial restriction, we believe it is essential for our analysis. First, as shown in Section 4, the coworker's effect substantially decreases after three years. Second, we need the pre-event period to examine the parallel trend assumption from a methodological perspective. Finally, since learning and knowledge spillover take time to be reflected in wages, we need a few years after the event to show the wage trajectory after the new worker enters.

In order to measure the impact of mobility on coworkers' wages, we cannot simply compare outcomes in firms that choose to hire a worker to those in firms that choose not to hire any worker, as the worker flows are very likely endogenous to firm performance and, ultimately, workers' compensation. We overcome this issue by selecting only firms that hire a given worker in a specific year and comparing firms hiring high-quality or low-quality workers to firms hiring similar-quality workers. Similarly, when we look at the impact of mobility on coworkers' wages in the origin firm we select only firms that separate from a worker in a given year. In the first case, we define the treatment groups as the coworkers in the firms that hire a high- or low-quality worker in period $t = 0$, where high-quality and low-quality workers are defined as workers with worker fixed effect that is 10 percent higher or lower than the average peer effect at the firm, respectively. We denote the group that hires a high-quality worker as treatment 1 in Figure 4 and the group that hires a low-quality worker as treatment 2. We define the control group as the coworkers in the firms that hire a similar-quality worker, whose ability is similar (within 10 percent difference) to the workers in the firm.

Similarly, in the "leave" design, when we analyze the effect on "old" coworkers, we maintain the same definitions, but the reference for defining high-, similar- and low-quality workers are peers in the origin firm.

Propensity score matching A critical issue that prevents us from identifying the effect is that worker mobility is not random. For example, the decision to hire a high-quality worker might be endogenous to firm performance, which also affects a worker's wage growth. For example, a firm may decide to hire a high-quality worker because of complementarity with some technology the firm decides to invest in. Such an investment could raise the productivity of the firm and eventually compensate all employees with wage raises. While there is no perfect remedy for this, we construct comparable firms between the treatment and control groups through ex-ante propensity score matching. The implicit assumption is that similar firms have similar hiring strategies, leading to a quasi-random hiring on average, such that the only difference between firms hiring a high- or low-quality worker rather than a similar-quality worker is precisely the worker quality.[14]

We estimate the propensity score using a wide range of firm-level variables and some industrial and geographic variables. We match on the following set of covariates at time $t = -3$: the AKM firm-time effects and AKM average worker effects estimated from section 3, the average age of employees, the share of female workers, the share of blue-collar workers, firm size, firm age, sales, value added,[15] industry dummies, and province dummies. We also match average weekly wages at time $t = \{-4, -3, -2\}$.We use the single nearest neighbor matching without replacement to match the treatment groups 1 and 2 with the control group, separately. Tables E.3 and E.4 in the Appendix report the means and the p-values of the differences in the covariates we have used for matching for the hire and leaver design, respectively, for both comparisons of high-quality vs similar-quality

[14]Our analysis will still be biased if the decision to hire is based on firms' unobservables which we cannot control for. We try to minimize the risk of the presence of such bias by including firm fixed effects in the regression analysis.

[15]Sales and value added variables are merged from the external balance-sheet firm-level database, AIDA. However, there is a good portion of firms that are not covered in AIDA. To utilize the information from balance-sheet data, we impute the missing value and create a dummy to indicate the missing observations.

(columns 1-4) and low-quality vs similar-quality (columns 5-8). Both tables highlight the presence of significant differences in observables between treated and control firms before matching, which, apart from very few exceptions, become minimal and statistically insignificant in the matched samples. Moreover, we check the common support assumption by plotting the density of the propensity score in Figure E.3 and E.4 in the Appendix for the analysis on the entry and on the leave of a worker. In both cases, and for both treatments 1 and 2, there is a wide overlap in the propensity score densities.

Overall, when studying the entry of a worker in a firm, our sample consists of 2,164 firms hiring a high-quality worker, matched with the same number of firms from the control group. 1,848 firms have hired a low-quality worker, and they are matched with the same number of firms from the control group. The two matched samples consist of 285,350 and 238,046 person-year observations, respectively. When we focus on the leave of a worker from a firm, we have 2,905 firms where a high-quality worker leaves and 1,885 where a low-quality worker leaves, both matched with the same number of firms in the control group. The two matched samples consist of 393,061 and 235,440 person-year observations, respectively. Table E.6 compares descriptive statistics for matched treated and control movers in the hire and leave designs in columns (1) and (2), respectively, to those of all other movers in column (4). The mobility episodes we are studying are not too dissimilar from the average mobility episodes in the data. Movers in both designs have similar annual earnings to those of other movers, though they earn slightly less per week and have a higher labour supply. They have similar age (around 30-31 years), but are more attached to their firms (their tenure is almost 1 year larger than the average mover). They are less likely to be female and more likely to be blue-collar. Overall, the differences are economically small. Hence, although our sample selection is quite demanding, the episodes we are studying are generalizable to a broader population of movers.

Event study On the matched sample of firms, we use the following event study specification to analyze the impact of a high- or low-quality worker's entry or leave on past and current coworkers' wages.

$$w_{-i,j,t} = \eta_t + \psi_j + \sum_{k\neq -1} \beta_k (Treat_j \times \mathbf{1}\{t=k\}) + \varepsilon_{-i,j,t}, \tag{9}$$

where $w_{-i,j,t}$ is the log weekly wage of coworkers, excluding the mover i, in period $t \in \{-4,-3,...,3\}$ and firm j, where j is the firm the worker joins when in the hire design and it is the firm the worker leaves in the leave design. η_t and ψ_j are year and firm fixed effects, respectively. $\varepsilon_{-i,j,t}$ is an error term. $Treat_j$ is a dummy variable for treated firms. The coefficients of interest are the β_k's, which measure the differential impact of hiring a high- or low-quality worker relative to hiring a similar-quality worker on wages in each period k.

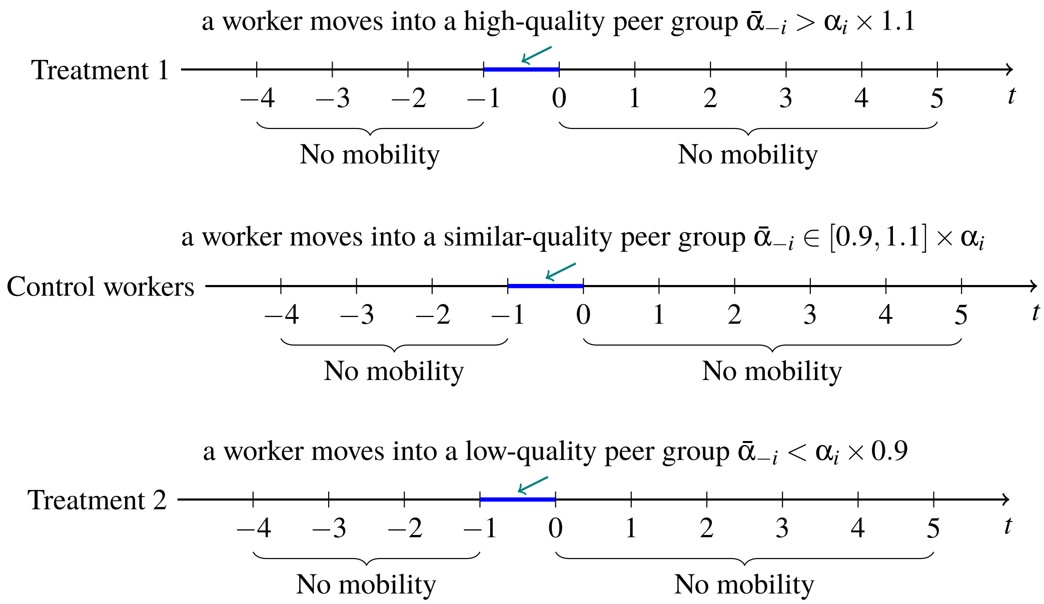


Figure 5: Treatment and control groups in the event study analysis, mover design

Notes. The diagram shows the empirical design behind the worker-level event study estimated in equation (10). *Treatment 1* refers to mobility into high-quality peer groups, whose average peer effect is more than 10 percent larger than the worker effect of the mover. *Treatment 2* refers to mobility into low-quality peer groups, whose average peer effect is more than 10 percent smaller than the worker effect of the mover. *Control workers* refer to mobility of workers into similar-quality peer groups, whose average peer effect is between -10 and $+10$ percent of the worker effect of the mover.

5.1.2 Worker-level Event Study

Empirical design We study the impact on movers' wages of joining peer groups of different quality. In particular, we use an analogous empirical strategy to that outlined before: We follow movers between 4 years before the move and 5 years after. We focus on workers moving once and not changing employer in the 4 years before the move and in the 5 years after, as described by the scheme in Figure 5. We match on workers' characteristics measured 3 years before the move, such that, conditional on observables, the only difference between movers is the peer group they end up joining. As before, we compare movers into high-quality (treatment 1) and low-quality (treatment 2) peer groups with movers into similar-quality peer groups (control), where the comparison of own quality and peer quality is based on the worker and average peer fixed effect estimated in section 4.

Propensity score matching We perform ex-ante propensity score matching on the following set of worker and firm observables at time $t = -3$: the number of weeks worked, age, gender, tenure, occupation (blue-collar or white-collar), the decile of the AKM worker fixed effect, the AKM firm-time fixed effect, log firm size, sector dummies, and province dummies. Besides, we match workers on log weekly wages at time $t = \{-4, -3, -2\}$. As before, we use the single nearest neighbor matching without replacement. Table E.5 reports the mean difference and the p-values of the differences between treated and control workers, separately for treated workers that move into high-quality peer groups (columns 1-4) and treated workers that move into low-quality peer groups (columns 5-8) for both unmatched and matched samples. The differences between treated and control workers in the unmatched samples become statistically insignificant when we perform matching. Furthermore, we keep only workers in the common support of the propensity score, which, as shown in Figure E.5, displays a wide overlap for both groups of workers.

After matching, we are left with a sample of 31,102 workers (15,551 in the treatment and 15,551 in the control group) when studying movers into high-quality peer groups, for a total of 310,220 person-year observations. When we study movers into low-quality peer groups we have a sample of 25,556 workers (12,778 in the treatment and 12,778 in the control group), for a total of 255,560 person-year observations. Column (3) of Table E.6 reports descriptive statistics for matched treated and control workers in this empirical design, while column (4) reports them for the average mover, who is not included in any of our designs. Matched movers earn more per year than the average mover, mainly because they have a higher labor market attachment (they work 49 weeks compared to 40 for other movers). They are slightly older, have longer tenure, are less likely to be female, and are more likely to work in blue-collar occupations and in manufacturing. In this case, the differences are more pronounced than in the hire and leave designs. Therefore, we trade off some generalizability of the results to the wider population of movers for a better identification strategy.

Event study On the matched samples, we estimate the following event study regression:

$$w_{i,t} = \eta_t + \alpha_i + \sum_{k \neq -1} \gamma_k (Treat_i \times \mathbf{1}\{t = k\}) + \varepsilon_{i,t}, \tag{10}$$

where $w_{i,t}$ is the log weekly wage of worker i in period $t \in \{-4, -3, ..., 5\}$. η_t and α_i are year and individual fixed effects, respectively.[16] $\varepsilon_{i,t}$ is an error term. $Treat_i$ is a dummy variable for treated workers (either movers into high-quality peers or movers into low-quality peers). The coefficients of interest are the γ_k's, which measure the differential impact of moving into a high- or low-quality peer group relative to a similar-quality peer group on wages in each period k.

[16]We do not include the destination firm fixed effects as in that case identification would only come from firms having more than one mobility event in the time period under analysis, hampering the external validity of our findings. Note, however, that we do match on ex-ante firm characteristics, such as the AKM firm-time effects, log firm size and the sector, which should ease concern that we are capturing wage changes related to firm characteristics besides the quality of the peer group.

5.2 Results

5.2.1 Coworker-level Event Study

When a high- or low-quality worker *enters* Figure 6 reports the event study coefficients β_k for each $k \in \{-4, ..., +3\}$, for both treatment groups (coworkers in firms hiring a high-quality worker and coworkers in firms hiring a low-quality worker), relative to the control group (coworkers in firms hiring a similar-quality worker). The parallel trend assumption holds as the effect before the event is small and statistically insignificant.

The post-event effects are quite different for the two treatments. Compared to firms hiring a similar-quality worker, peers in firms that hire a high-quality worker experience a positive and significant increase in wages. One year after the high-quality worker's entry, coworkers' wages are 1.9 percent higher than in control firms. The effect persists in the following years and reaches 3.1 percent after three years. In contrast, there is no effect in period 0. In other terms, the high-quality worker's entry does not affect the coworkers' wage on impact, but it takes some time for the peer effect to diffuse and be reflected in wages. On the other hand, when a firm hires a low-quality worker, the effect on her coworkers' future wages is slightly negative but statistically insignificant. The knowledge spillover may play a role in explaining our findings. A high-quality worker, when joining a new firm, would be able to transmit knowledge to her coworkers, and therefore eventually drive up their wages in the following years. On the contrary, when a low-quality worker enters, the amount of knowledge she can transmit is much more limited and, therefore, it is less relevant for coworkers' wages. This result is in line with the reduced form evidence provided in Jarosch et al. (2021), who show that the peer effect is stronger when coming from more knowledgeable (i.e., more productive) workers. As discussed already, learning from high-quality coworkers is not necessarily the only mechanism at play. The entry of a high-quality worker could affect coworkers' wages through alternative channels, e.g., peer pressure and production complementarity or better network (e.g., Moretti, 2004b; Mas and Moretti, 2009; Caldwell and Harmon, 2019).

To gather further insights into the mechanisms that determine our findings, we explore heterogeneous effects across different peer groups. Figure 7A shows the effect of a high-quality worker's entry on her peer and non-peer group, i.e., coworkers in the same and different occupation, respectively. For the peer group, the effect is almost identical to the one in Figure 6: on average, the effect in the post-event window is 1.8 percent. On the other hand, there is no significant effect for the non-peer group (the static difference-in-differences point estimate is 0.4 percent with a standard error of 0.7).

Figure 7B shows the same heterogeneous effects for coworkers of low-quality movers. We find a small and statistically insignificant negative effect for coworkers in the same peer group and a positive (but still imprecise) effect for non-peers. The estimates are hardly significant, but the

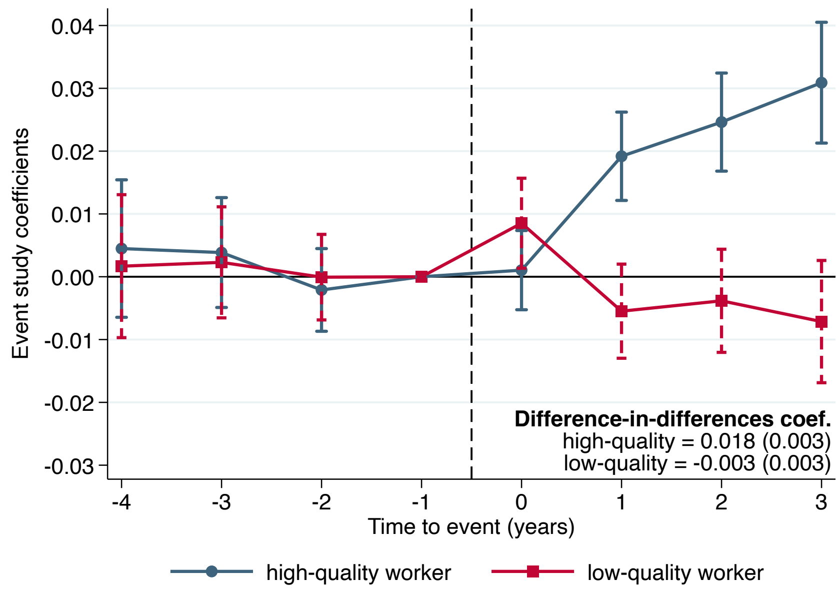


Figure 6: The effect of a high-/low- quality worker's entry on coworkers' future wages

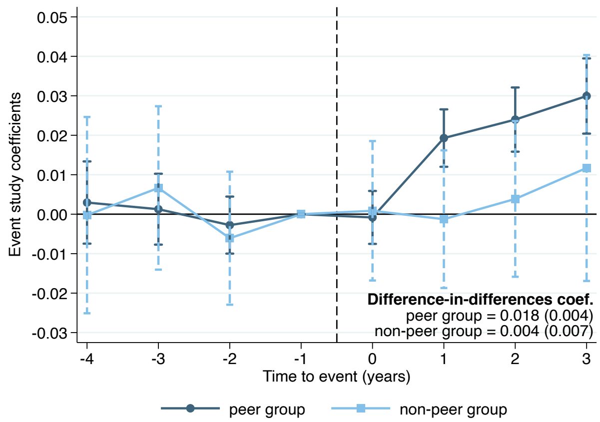
Notes. The figure reports estimates of β_k from equation (9), separately for firms hiring high-quality and low-quality workers relative to firms hiring similar-quality workers. The dependent variable is the log weekly wage of coworkers in the destination firm. Vertical bars are 95 percent confidence intervals, obtained from cluster-robust standard errors at the firm level.

positive coefficient on non-peers could signal some within-firm organizational changes that follow the low-quality hire and allow the promotion of better-skilled workers to higher-paying occupations.

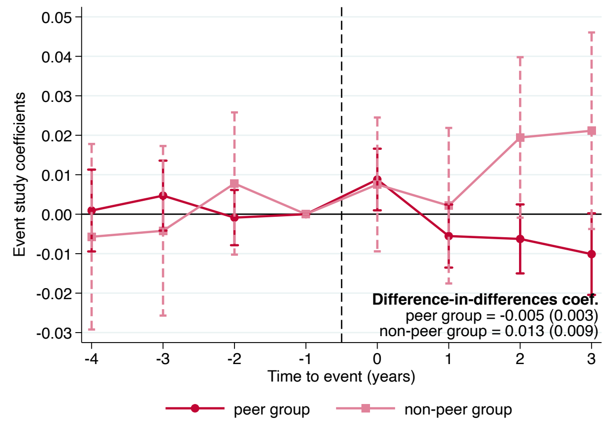
We separately study the effect for high-wage and low-wage workers in Figure 7C and 7D. We define high-wage workers as those having a wage higher than the median of the sector (1-digit Ateco 91) they belong to. Figure 7C reports the estimates from the entry of a high-quality worker and shows that the benefit of working with such workers are equally shared between high-wage and low-wage workers. In contrast, Figure 7D seems to suggest different wage trajectories for high-wage and low-wage workers as they collaborate with the new low-quality hire. While high-wage workers do not experience any wage change, low-wage workers experience some wage loss, although the estimates are statistically insignificant.

When a high- or low-quality worker *leaves* We estimate equation (9) on the matched sample of coworkers in firms where high-quality and low-quality workers leave compared to coworkers in firms where a similar-quality worker leaves. The outcome variable is, in this case, the coworkers' wage in the origin firm. Figure 8 reports the event study coefficients β_k for each $k \in \{-4, ..., +3\}$.

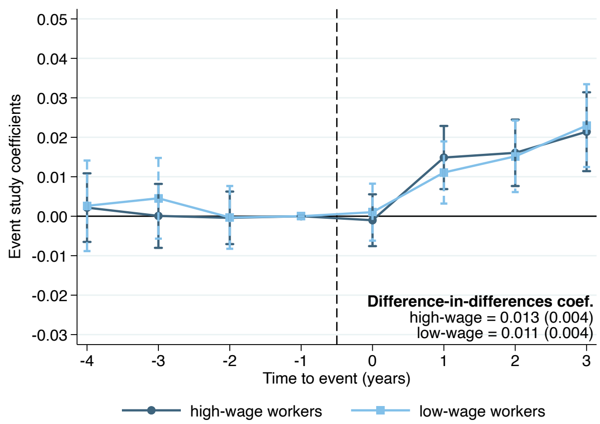
Our findings are somewhat symmetric with respect to those we find for the entry of workers. When a low-quality worker leaves, coworkers' wages increase by 2.8 percent on average, whereas the departure of a high-quality worker depresses coworkers' wages by -1.6 percent. When a firm



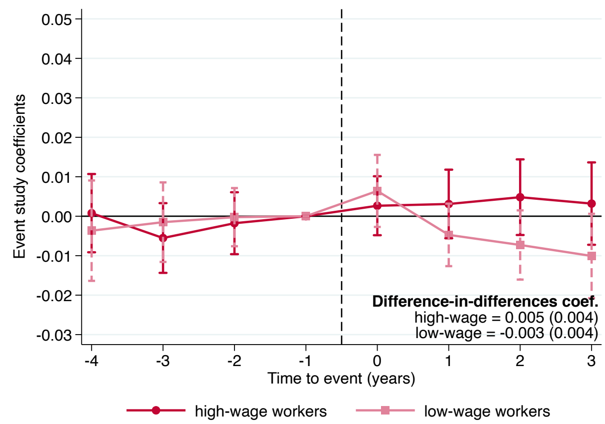
(A) High-quality entry, peers vs non-peers



(B) Low-quality entry, peers vs non-peers



(C) High-quality entry, high- vs low-wage



(D) Low-quality entry, high- vs low-wage

Figure 7: Heterogeneous effects for peers and non-peers (top panels) and high-wage and low-wage coworkers (bottom panels), hire design

Notes. The figure reports estimates of β_k from equation (9) for different groups of workers. Panel A and B compare high- and low-quality hires, respectively, to similar-quality hires, distinguishing the effect for workers belonging to the same or different peer group (i.e., to the same occupation). Panel C and D compare high- and low-quality hires, respectively, to similar-quality hires, distinguishing the effect for high- and low-wage workers (i.e., above or below the median wage in the 1-digit sector they belong to). The dependent variable is the log weekly wage of coworkers in the destination firm. Vertical bars are 95 percent confidence intervals, obtained from cluster-robust standard errors at the firm level.

separates from a low-quality worker, the average peer quality in a firm increases: this likely makes knowledge spillover easier in the firm, which eventually increases future wages. On the other hand, when a high-quality worker leaves, there are potentially two (opposite) effects. First, the leave of a high-quality worker reduces overall peer quality, thus decreasing the intensity of knowledge spillover. Second, the high-quality worker's human capital "left" into the firm may still have a persistent effect over the next few years, which could help boost wage growth. Overall, results

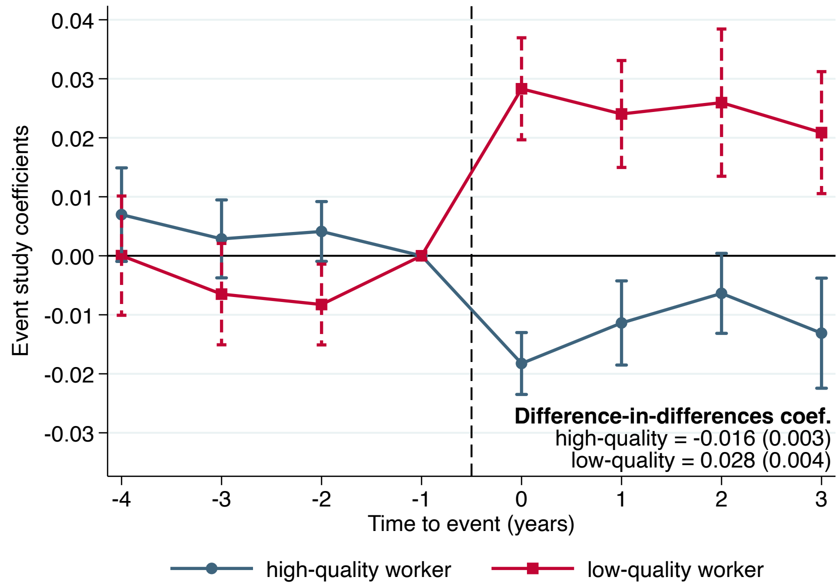


Figure 8: The effect of a high-/low-quality worker's leave on coworkers' future wages

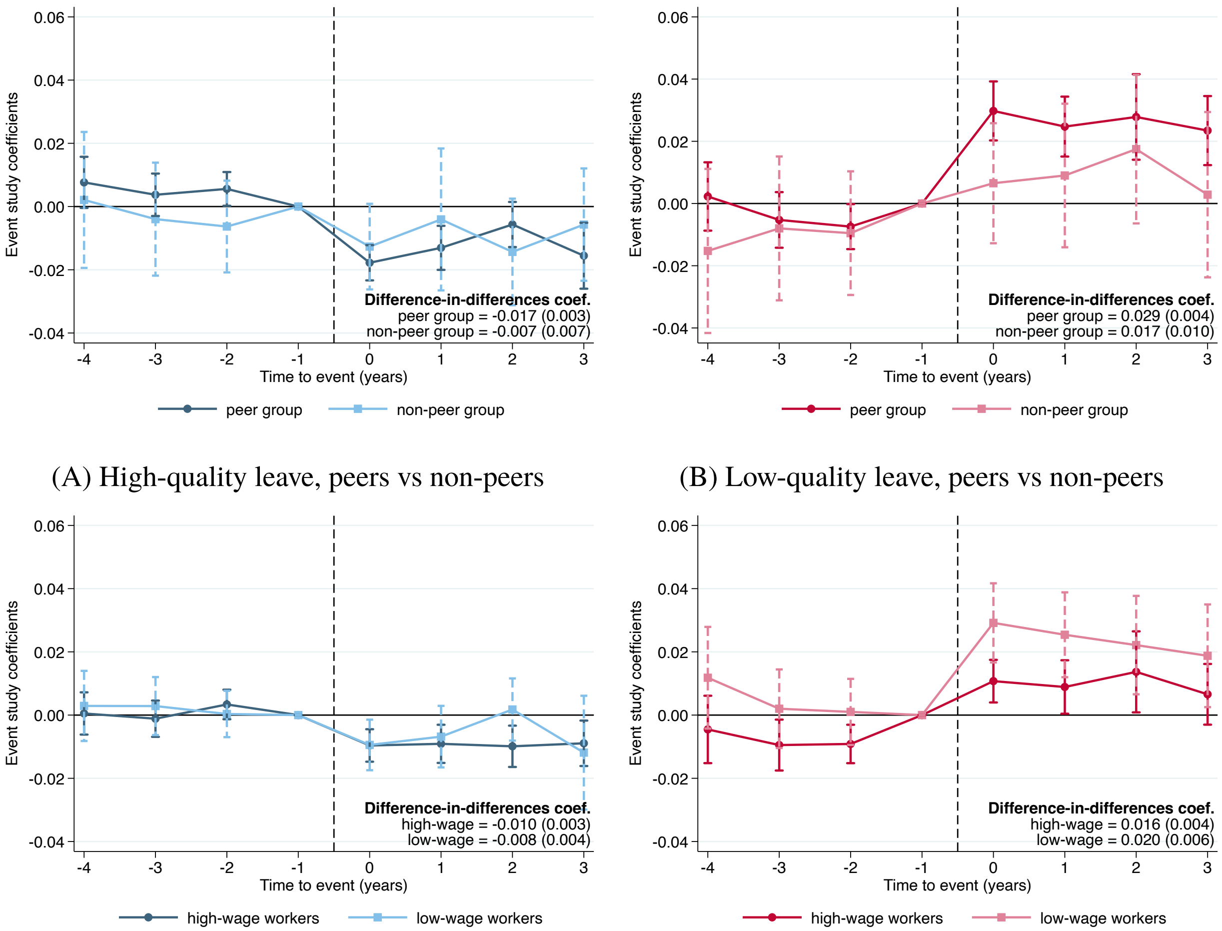
Notes. The figure reports estimates of β_k from equation (9), separately for firms separating from high-quality and low-quality workers relative to firms separating from similiar-quality workers. The dependent variable is the log weekly wage of coworkers in the origin firm. Vertical bars are 95 percent confidence intervals, obtained from cluster-robust standard errors at the firm level.

suggest that the first channel exceeds the second, indicating an overall negative effect.[17]

We again explore heterogeneous effects distinguishing peers and non-peers in Figure 9A and 9B, respectively for high-quality and low-quality leaves. While the departure of a high-quality worker has similar effects on peers and non-peers, when a low-quality worker leaves the firm, peers seem to benefit more than non-peers, although there is some overlap in confidence intervals.

When we distinguish between high-wage and low-wage workers (defined as before, based on the median weekly wage in the sector), we do not find significant differences between the two groups of workers when a high-quality worker leaves the firm in Figure 9C. In contrast, low-wage coworkers gain more from the departure of low-quality workers than high-wage coworkers, especially in the first two years following mobility.

[17]Moreover, a high-quality worker may have a hold-up power with the firm, which eases rent-sharing for coworkers. Losing such a worker would therefore represent a wage loss for incumbents (Bloesch et al., 2021). In addition, this result aligns well with Jäger and Heining (2022), whio investigate how exogenous worker exits affect incumbent workers and new hires. They find that coworkers in the same occupation as the deceased see positive wage effects, while coworkers in other occupations experience wage decreases when a high-skilled or specialized worker dies.



(A) High-quality leave, peers vs non-peers

(B) Low-quality leave, peers vs non-peers

(C) High-quality leave, high- vs low-wage

(D) Low-quality leave, high- vs low-wage

Figure 9: Heterogeneous effects for peers and non-peers (top panels) and high-wage and low-wage coworkers (bottom panels), leave design

Notes. The figure reports estimates of β_k from equation (9) for different groups of workers. Panel A and B compare separations from high- and low-quality workers, respectively, to similar-quality separations, distinguishing the effect for workers belonging to the same or different peer group (i.e., to the same occupation). Panel C and D compare high- and low-quality separations, respectively, to similar-quality separations, distinguishing the effect for high- and low-wage workers (i.e., above or below the median wage in the 2-digit sector they belong to). The dependent variable is the log weekly wage of coworkers in the origin firm. Vertical bars are 95 percent confidence intervals, obtained from cluster-robust standard errors at the firm level.

5.2.2 Worker-level Event Study

We report in Figure 10 the estimates from equation (10), comparing the wage trajectories of movers into high- and low-quality peer groups relative to movers into similar-quality peer groups for event period $k \in \{-4, ..., +5\}$. The event study analysis suggests that, before the move, there are no significant differences in wage trajectories of high- and low-quality peers relative to similar-quality peers. In the post-event window, estimates indicate that moving into high-quality peer groups

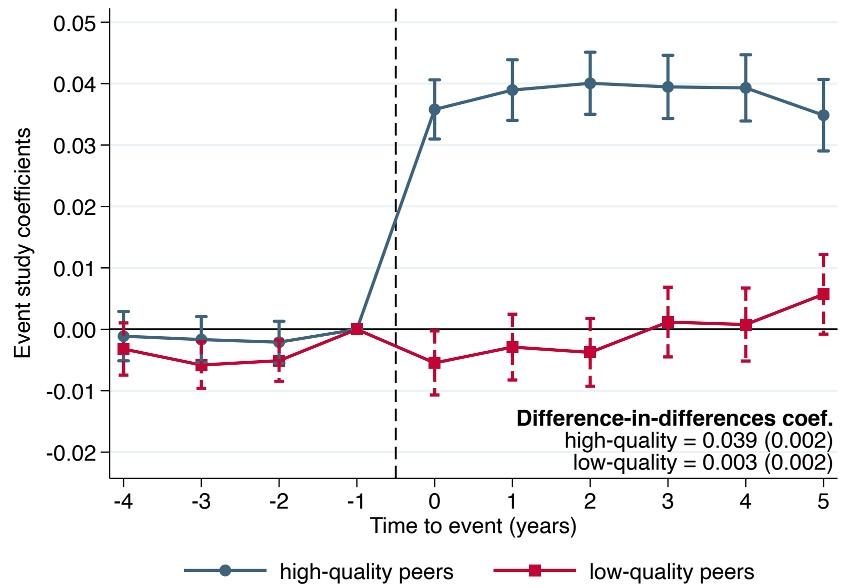


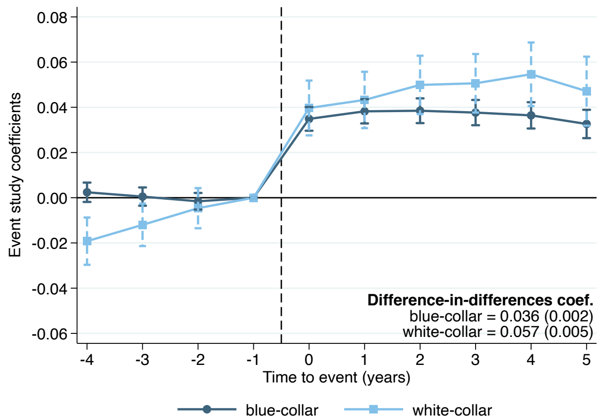
Figure 10: The effect of moving into a high-/low-quality peer group on movers' future wages

Notes. The figure reports estimates of γ_k from equation (10), separately for workers moving into high-quality and low-quality peer groups relative to those moving into similar-quality peer groups. The dependent variable is the log weekly wage of the mover. Vertical bars are 95 percent confidence intervals, obtained from cluster-robust standard errors at the individual level.

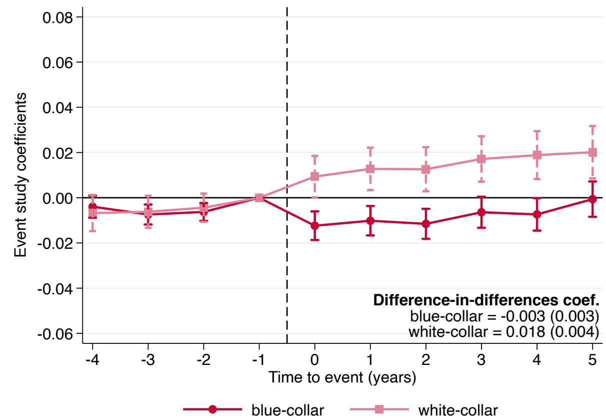
represents an important and substantial driver of wage growth. On average, the weekly wages of such movers increase by 3.9 percent. The effect materializes on impact and then remains approximately constant throughout the five years after mobility. In contrast, moving into low-quality peer groups does not affect wages. This finding highlights the importance, in terms of wage progression, of working in a high-quality workplace.

We explore heterogeneous effects by workers' occupation in the year before the move. That is, we distinguish among blue-collar and white-collar movers in Figure 11A and 11B for movers into high-quality and low-quality peers, respectively. The positive effect of high-quality peers is larger for white-collar workers compared to blue-collar ones: the average difference-in-differences coefficient in the post-event window is 5.7 percent for the former vs. 3.6 percent for the latter, though we observe an increasing trend in wages before the move for white-collar workers (but a clear trend break, nonetheless). Interestingly, white-collar workers gain also when moving into low-quality peer groups (Figure 11B), as they earn 1.8 percent higher wages on average after mobility relative to white-collar movers into similar-quality peers.Overall, the net gain for white-collar workers from moving into high-quality peers relative to low-quality peers is 3.9 percent (5.7 – 1.8).

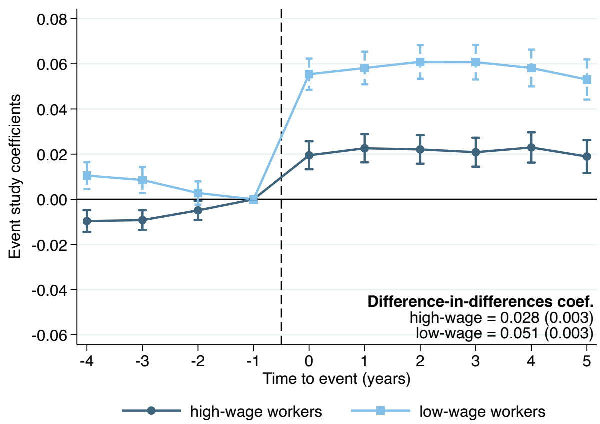
Figure 11C and 11D report heterogeneous effects for high-wage and low-wage movers (defined, as before, with respect to the median wage in the sector). High-quality peers benefit particularly



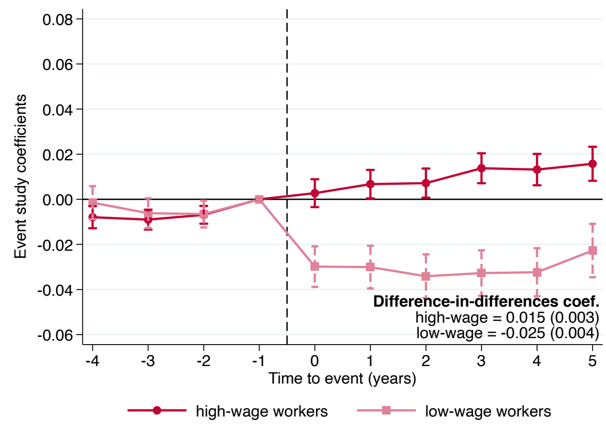
(A) High-quality peers, blue- vs white-c. mover



(B) Low-quality peers, blue- vs white-c. mover



(C) High-quality peers, high- vs low-wage mover



(D) Low-quality peers, high- vs low-wage mover

Figure 11: Heterogeneous effects for blue- and white-collar (top panels) and high-wage and low-wage movers (bottom panels), worker-level design

Notes. The figure reports estimates of γ_k from equation (10) for different groups of workers. Panel A and B show the effects for movers into high- and low-quality peers, respectively, relative to similar-quality peers, distinguishing movers that in the year before the move are employed as blue- or white-collar workers. show the effects for movers into high- and low-quality peers, respectively, relative to similar-quality peers, distinguishing high- and low-wage movers (i.e., above or below the median wage in the 1-digit sector they belong to). The dependent variable is the log weekly wage of movers. Vertical bars are 95 percent confidence intervals, obtained from cluster-robust standard errors at the individual level.

low-wage workers, who experience instead a wage penalty when moving into low-quality peers.

Overall, this analysis helps us understand the mechanisms that identify the peer effect in section 4. We conclude that changes in peer quality for job stayers determined by the hire of a high-quality worker or the separation from a low-quality worker are the most important ones in determining wage growth for job stayers, with the positive effects being generally stronger for workers in the same peer group. For job switchers, moving into high-quality peer groups contribute to raising wages on impact, but moving to low-quality peers has a small and statistically insignificant negative effect, on average.

5.2.3 Discussion

How do the estimates of the event study analyses compare with the effects reported in Figure 3? The estimates from the two analyses cannot be quantitatively compared, but they have the same qualitative implications. Figure 3 reports the effects of a 10 percent increase in peer quality on workers' wages. The coworker-level event study analysis measure the wage changes stemming from hiring or separating from high- and low-quality workers with respect to similar-quality workers. The worker-level event study measures the effects on movers' wages of joining high- or low-quality peer groups with respect to similar-quality peer groups. The effect size we measure in the event study is therefore only approximately comparable to that in Figure 3, as high-quality (low-quality) mobility is defined as having a worker fixed effect that is *at least* 10 percent higher (lower) than the average of peers in the firm. With this caveat, we can still discuss whether the estimates for the two different analyses are broadly aligned. Specifically, when focusing on the hire of high-quality workers, the leave of low-quality workers and on movers into high-quality peer groups, we get average effects of 1.8, 2.4 and 3.9 percent, respectively. The contemporaneous effect from Figure 3 is 2.6 percent,[18] which lies within the boundaries of the event study estimates. When focusing on the hire of low-quality workers, the leave of high-quality workers and on movers into low-quality peer groups, we generally find either small negative or null effects. Therefore, we conclude that the two analyses provide broadly similar peer effects when focusing on *increases* in peer quality. The event study analysis further highlights that the peer effects are not necessarily symmetric, implying that working with high- or low-quality peers has different impacts on workers' current and future wages.

6 Conclusion

This paper explores a critical driver of future wages: peer quality in the workplace. We find that the quality of coworkers plays an important role in increasing future wages. By incorporating a coworker component in the canonical AKM model, we show that a 10 percent increase in peer quality raises the next year's wage by 1.8 percent, which is larger than the return to one year's experience during the same period. The peer effect gradually decreases in magnitude over time, but we find that after five years a 10 percent increase in past peer quality still determines 0.7 percent higher wages. When exploring heterogeneous effects, we find that the peer effect is larger for

[18]The comparison with the contemporaneous peer effect is the most appropriate as the event studies restrict to movers who are observed in the destination firm for the whole post-mobility window.

movers, workers with low tenure, young workers, and in small peer groups, although differences between peer groups of different sizes are small in magnitude.

Furthermore, the peer effect is estimated using the variations from both job stayers and job switchers. For job stayers, peer quality changes when a worker enters or leaves the firm. For job switchers, peer quality changes when they move into a new peer group. We separately analyze each of these channels by setting up a novel event study analysis around worker mobility, combined with propensity score matching in the pre-mobility period. We find that, in firms hiring a high-quality worker, coworkers' wages increase by 1.8 percent relative to coworkers' wages in firms hiring a similar-quality worker. We do not find, instead, significant effects in firms hiring a low-quality worker. The opposite effect is found in firms separating from a worker: coworkers' wages increase in firms separating from low-quality workers and moderately decrease in firms separating from high-quality workers. We also explore the wage trajectories of workers who move into peer groups of different quality and find that moving into high-quality peers is an important driver of wage growth, with wages being on average 3.9 percent higher than those of workers moving into similar-quality peer groups. In contrast, the wages of workers moving into low-quality peer groups remain unchanged. Overall, our findings suggest that hiring high-quality workers, separating from low-quality workers, and moving into high-quality peer groups generate higher wages than counterfactual scenarios where peer quality does not change.

Future research should focus on opening the black box of the mechanisms behind the contribution of peers to workers' future wages. Increased availability of administrative data, coupled with either structural or reduced-form models, or laboratory experiments, will help reach a more complete answer to this question.

References

Abowd, John M., Francis Kramarz, and David N. Margolis, "High Wage Workers and High Wage Firms," *Econometrica*, 1999, *67* (2), 251–333.

Arcidiacono, Peter, Gigi Foster, Natalie Goodpaster, and Josh Kinsler, "Estimating spillovers using panel data, with an application to the classroom," *Quantitative Economics*, 2012, *3* (3), 421–470.

Arellano-Bover, Jaime and Fernando Saltiel, "Differences in On-the-Job Learning Across Firms," *Working Paper*, 2021.

Bartolucci, Cristian, Francesco Devicienti, and Ignacio Monzón, "Identifying Sorting in Practice," *American Economic Journal: Applied Economics*, 2018, *10* (4), 408–38.

Battisti, Michele, "High wage workers and high wage peers," *Labour Economics*, 2017, *46* (February 2015), 47–63.

_ , Ryan Michaels, and Choonsung Park, "Labor Supply within the Firm," *Journal of Labor Economics*, 2024, *2* (0), 000–000.

Becker, Gary, *Human Capital*, Chicago: Univ. Chicago Press, 1964.

Ben-Porath, Yoram, "The Production of Human Capital and the Life Cycle of Earnings," *Journal of Political Economy*, 1967, *75* (4), 352–365.

Bloesch, Justin, Birthe Larsen, and Bledi Taska, "Which Workers Earn More at Productive Firms? Position Specific Skills and Individual Hold-up Power," Technical Report, mimeo 2021.

Brune, Lasse, Eric Chyn, and Jason Kerwin, "Peers and Motivation at Work," *Journal of Human Resources*, 2020, pp. 0919–10416R2.

Caldwell, Sydnee and Nikolaj Harmon, "Outside Options, Bargaining, and Wages: Evidence from Coworker Networks," *Working Paper*, 2019.

Card, D., J. Heining, and P. Kline, "Workplace Heterogeneity and the Rise of West German Wage Inequality," *Quarterly Journal of Economics*, 2013, *128* (3), 967–1015.

Card, David, Ana Rute Cardoso, Joerg Heining, and Patrick Kline, "Firms and Labor Market Inequality: Evidence and Some Theory," *Journal of Labor Economics*, 2018, *36* (S1), S13–S70.

Cardoso, Ana Rute, Paulo Guimaraes, Pedro Portugal, and Hugo Reis, "The Returns to Schooling Unveiled," IZA Discussion Papers 11419, Institute of Labor Economics (IZA) March 2018.

Casarico, Alessandra and Salvatore Lattanzio, "What Firms Do: Gender Inequality in Linked Employer-Employee Data," *Journal of Labor Economics*, 2024, *42* (2), 325–355.

Cornelissen, Thomas, Christian Dustmann, and Uta Schönberg, "Peer effects in the workplace," *American Economic Review*, 2017, *107* (2), 425–456.

de Melo, Rafael Lopes, "Firm Wage Differentials and Labor Market Sorting: Reconciling Theory and Evidence," *Journal of Political Economy*, 2018, *126* (1), 313 – 346.

Devicienti, Francesco, Bernardo Fanfani, and Agata Maida, "Collective Bargaining and the Evolution of Wage Inequality in Italy," *British Journal of Industrial Relations*, 2019, *57* (2), 377–407.

Falk, Armin and Andrea Ichino, "Clean Evidence on Peer Effects," *Journal of Labor Economics*, 2006, *24* (1), 39–57.

Fanfani, Bernardo, “Tastes for discrimination in monopsonistic labour markets,” *Labour Economics*, 2022, p. 102107.

Gregory, Victoria, “Firms as Learning Environments: Implications for Earnings Dynamics and Job Search,” *Working Paper*, 2019.

Herkenhoff, Kyle, Jeremy Lise, Guido Menzio, and Gordon M. Phillips, “Production and Learning in Teams,” *Econometrica*, 2024, *92* (2), 467–504.

Hong, Long and Mikkel Sølvsten, “Estimation and Inference in a Peer Effects Model with Heteroscedasticity,” *Mimeo*, 2021.

Jäger, Simon and Jörg Heining, “How substitutable are workers? evidence from worker deaths,” Technical Report, National Bureau of Economic Research 2022.

Jarosch, Gregor, Ezra Oberfield, and Esteban Rossi-Hansberg, “Learning From Coworkers,” *Econometrica*, 2021, *89* (2), 647–676.

Lengermann, Paul A., “Is it Who You Are, Where You Work, or With Whom You Work? Reassessing the Relationship Between Skill Segregation and Wage Inequality,” Longitudinal Employer-Household Dynamics Technical Papers 2002-10, Center for Economic Studies, U.S. Census Bureau 2002.

Manski, Charles F., “Identification of Endogenous Social Effects: The Reflection Problem,” *The Review of Economic Studies*, 1993, *60* (3), 531–542.

Mas, Alexandre and Enrico Moretti, “Peers at work,” *American Economic Review*, 2009, *99* (1), 112–145.

Moretti, Enrico, “Estimating the social return to higher education: evidence from longitudinal and repeated cross-sectional data,” *Journal of Econometrics*, 2004, *121* (1), 175–212. Higher education (Annals issue).

_ , “Workers’ Education, Spillovers, and Productivity: Evidence from Plant-Level Production Functions,” *American Economic Review*, June 2004, *94* (3), 656–690.

Nix, Emily, “Learning Spillovers in the Firm,” *Working Paper*, 2020.

Papay, John P., Eric S. Taylor, John H. Tyler, and Mary E. Laski, “Learning Job Skills from Colleagues at Work: Evidence from a Field Experiment Using Teacher Performance Data,” *American Economic Journal: Economic Policy*, 2020, *12* (1), 359–88.

Sandvik, Jason J, Richard E Saouma, Nathan T Seegert, and Christopher T Stanton, "Workplace Knowledge Flows," *The Quarterly Journal of Economics*, 04 2020, *135* (3), 1635–1680.

Serafinelli, Michel, ""Good" firms, worker flows, and local productivity," *Journal of Labor Economics*, 2019, *37* (3), 747–792.

Online Appendix to

The Peer Effect on Future Wages in the Workplace

Long Hong, *Arizona State University*
Salvatore Lattanzio, *Bank of Italy and Dondena*

A Non-Parametric Tests of Conditional Random Mobility

The OLS estimation of model (1) is consistent under the assumption of conditional random mobility, where:

$$E(D\varepsilon) = E(F\varepsilon) = E(X\varepsilon) = 0,$$

with D as the matrix of individual dummies, F for firm dummies, and X as the matrix of regressors. Here, ε is the matrix of errors, with observations stacked across individuals and time. We focus here on the restrictions on the matrix of firm dummies.

Following Card et al. (2013), we decompose the error term ε_{it} into three components:

$$\varepsilon_{it} = \phi_{jt} + m_{ij} + u_{it}, \tag{A.1}$$

where ϕ_{jt} represents time-varying firm-specific shocks, m_{ij} represents match effects, and u_{it} is an idiosyncratic individual shock. We conduct event-study analyses around job changes to ensure mobility is uncorrelated with firm-level shocks, match effects, and individual shocks.

(a) Firm-Level Shocks If employees tend to leave firms experiencing negative shocks for firms with positive ones, ϕ_{jt} in equation A.1 would correlate with the probability of employment at firm j in time t. This would imply that workers might see a decline in earnings before moving, followed by a pay increase. Figure A.1 investigates this by examining log daily earnings of job movers across quartiles of firm fixed effects, showing no notable earnings changes before or after moves for both men and women, besides an upward trend reflecting real wage growth. This suggests that firm-level shocks do not seem to influence mobility decisions. Note also that in equation (2) we flexibly control for firm-time specific shocks, thus effectively absorbing any firm-time factor that could impact workers' earnings.

(b) Match Effects If moves are motivated by a better match with the destination firm, then m_{ij} would correlate with employment probabilities, leading to earnings gains regardless of firm pay levels. We examine this by comparing earnings changes for moves across firm quartiles. Figure

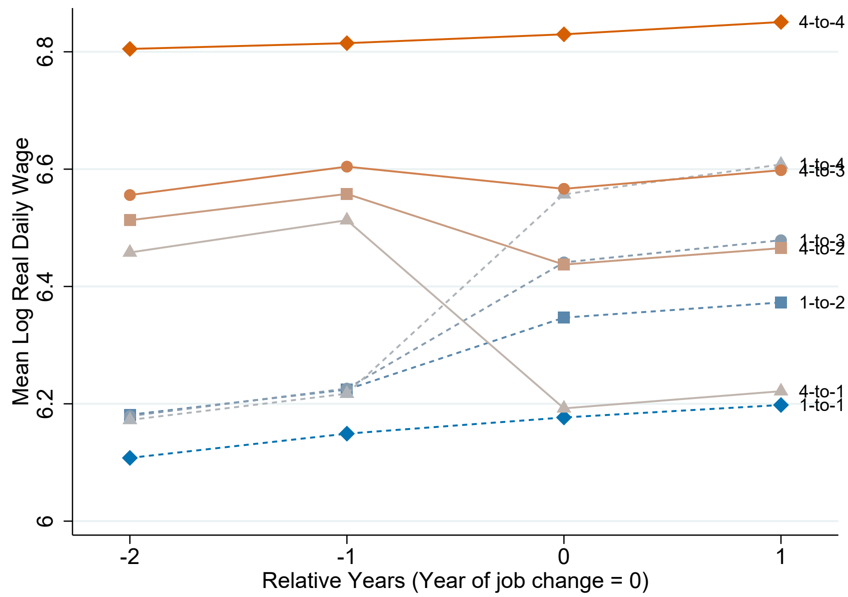


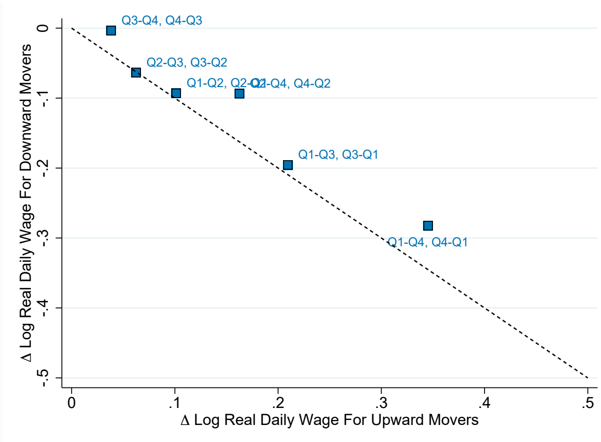
Figure A.1: Mean daily earnings of movers across quartiles of firm fixed effects

Notes. The figure shows mean daily earnings of workers who change firms, and were employed in the preceding firm for two or more years, and in the new firm for two or more years. Each firm is classified into quartiles based on the estimated AKM firm fixed effects.

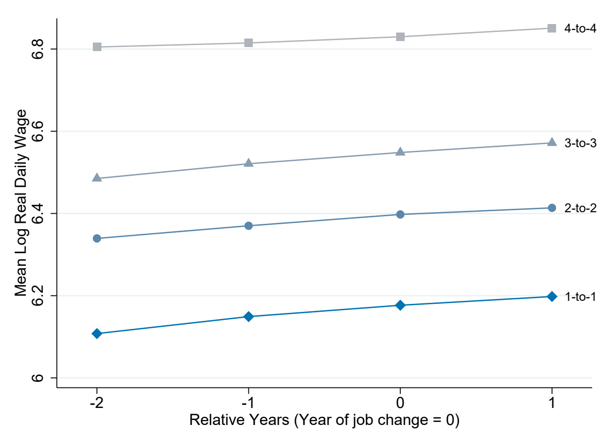
A.2, panel A, shows symmetric earnings changes for upward and downward moves, with points clustering around a slope of -1, indicating that each move yields a similar change of opposite magnitude.[19] This symmetry suggests that mobility is not driven by match effects. Additionally, panel B shows flat earnings for movers within the same quartile. If match effects were important, we would have expected earnings gains also in these types of moves. Lastly, residuals from equation (1) are assessed by deciles of worker and firm effects in Figure A.4, where small average residuals affirm the model's specification.

(c) Individual Shocks Individual shocks may correlate u_{it} with firm employment, potentially due to a temporary productivity rise or skills accumulation prompting moves to higher-paying firms, or due to earnings declines leading workers to move for stability. Figure A.1, however, shows no significant differences in earnings trends before moves across quartiles of firm fixed effects—besides an upward trend both in the origin and destination jobs reflecting wage growth—indicating that individual transitory shocks are not a primary driver of mobility.

[19] The earnings changes are netted of the change for within-quartile moves.



(A) Change in earnings of symmetric job moves across quartiles of firm fixed effects



(B) Mean daily earnings of movers within same quartiles of firm fixed effects

Figure A.2: Change in daily earnings of symmetric job moves and mean daily earnings in same-quartile moves

Notes. Panel (A) shows mean daily wage changes over a 4-year interval for job switchers who move across the firm fixed effects quartile groups indicated. The dashed line is a 45-degree line, indicating symmetric changes for upward and downward movers. Panel (B) shows mean daily earnings of workers who change firms, and were employed in the preceding firm for two or more years, and in the new firm for two or more years. Each firm is classified into quartiles based on the estimated AKM firm fixed effects. Only moves within the same quartile of origin and destination firms are reported.

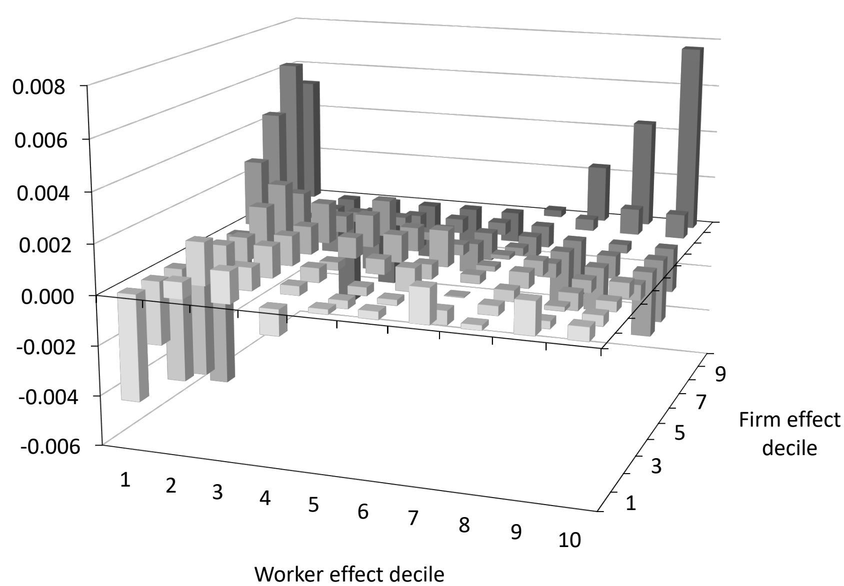


Figure A.3: Mean AKM residuals across deciles of person and firm effects

Notes. The figure shows mean AKM residuals (from equation 1) across decile bins of worker and firm effects.

B Estimation

This section provides more details on the estimation method in Section 3.3. Specifically, Section B.1 shows the detailed construction steps for our key coworker-averaging matrix C. Section B.2 shows the proof of Theorem 1.

B.1 Construction of the C Matrix

As a simple example on how we construct C, suppose we have the following data, where there are only five workers and two peer groups. The first column and second column of the data indicate the worker ID and peer group ID, respectively.

$$\text{data} = \begin{pmatrix} 1 & 1 \\ 2 & 1 \\ 3 & 2 \\ 4 & 2 \\ 5 & 2 \end{pmatrix}$$

We first construct an averaging matrix $\tilde{C}$ below to detect who is each worker's peer and what weight they are assigned when calculating the average peer quality. One might read $\tilde{C}$ as follows. The first row of $\tilde{C}$ says: 1 is not a coworker of himself, 2 is her coworker, and 3, 4, 5 are not her coworkers. The third row says, 1 and 2 are not 3's coworkers, 3 is not a coworker of herself, but 4 and 5 are her coworkers. Both of them weight half when calculating the average coworker quality.

$$\tilde{C} = \begin{pmatrix} 0 & 1 & 0 & 0 & 0 \\ 1 & 0 & 0 & 0 & 0 \\ 0 & 0 & 0 & 0.5 & 0.5 \\ 0 & 0 & 0.5 & 0 & 0.5 \\ 0 & 0 & 0.5 & 0.5 & 0 \end{pmatrix}$$

To make sure C and X have the same dimension, we add an auxiliary matrix 0 to $\tilde{C}$ as a final component of C. That is, $C = [\tilde{C}, 0]$.

B.2 Proof of Consistency

Proof of Theorem 1. The essential property that would ensure consistency of the non-linear least squares estimator is that β_0 is the unique minimizer of the population analog to Q_n, i.e., that

$$\beta_0 = \arg\min_{\beta\in B} \mathbb{E}[Q_n(\beta) \mid Z, C] \tag{B.1}$$

Noting that one could write the objective $Q_n(\beta)$ as a function of linear project matrices

$$Q_n(\beta) = ||M(\beta)w||^2/n$$

where $M(\beta) = I_n - P(\beta)$ and $P(\beta)$ is the linear projection onto the span of $Z + C\beta$, i.e.,

$$P(\beta) = (Z + C\beta)\left((Z + C\beta)'(Z + C\beta)\right)^{-1}(Z + C\beta)'.$$

To determine when equation (B.1) holds, one can calculate that

$$nQ_n(\beta) = ||M(\beta)(Z\delta + C\delta\beta_0)||^2 + ||M(\beta)\varepsilon||^2 + 2\varepsilon' M(\beta)(Z\delta + C\delta\beta_0).$$

When taking the conditional expectation of $Q_n(\beta)$ of the above equation, the following happens.

- The exogeneity condition (A1) leads the third term to have zero expectation
- The homoskedasticity condition (A2) leads the second term to have expectation equal to

  $$\mathbb{E}\left[||M(\beta)\varepsilon||^2\right] = \text{trace}(M(\beta))\sigma^2 = (n-k)\sigma^2$$

  for any value of β.

Finally, since $M(\beta)(Z\delta + C\delta\beta) = 0$, by taking expectation of $Q_n(\beta)$, we obtain that

$$\mathbb{E}[Q_n(\beta) \mid Z, C] = (\beta - \beta_0)^2 ||M(\beta)C\delta||^2/n + \frac{n-k}{n}\sigma^2. \tag{B.2}$$

Clearly, β_0 is a minimizer of equation (B.2). It is also unique if $||M(\beta)C\delta||^2/n \neq 0$ holds, which is ensured by the full-rank condition (A3). Therefore, the consistency of $\widehat{\beta}$ follows under the additional "regularity" condition that $\max_{\beta\in B} |Q_n(\beta) - Q(\beta)| \xrightarrow{p} 0$.

To prove $\mathbb{E}[S_n(\beta_0)] = 0$, we could use the same arguments above. Under homoskedasticity and exogeneity,

$$\begin{aligned}
&\mathbb{E}\left[\varepsilon' M(\beta_0) C \left((Z+C\beta_0)'(Z+C\beta_0)\right)^{-1} (Z+C\beta_0)'\varepsilon\right] \\
&= \operatorname{trace}\left(M(\beta_0) C \left((Z+C\beta_0)'(Z+C\beta_0)\right)^{-1} (Z+C\beta_0)'\right) \sigma^2 = 0,
\end{aligned}$$

where the last equality follows from $M(\beta)(Z+C\beta) = 0$ and the property of trace where $\operatorname{trace}(AB) = \operatorname{trace}(BA)$. ■

C Heterogeneous Peer Effects

We explore heterogeneous peer effects across different groups of workers and firms.

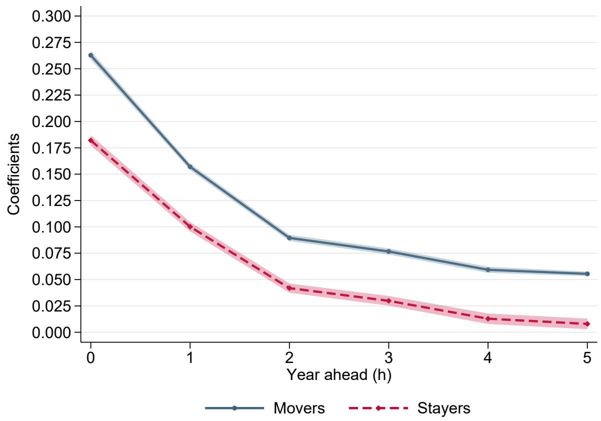
Figure C.1A separates movers and stayers, where the former are workers that change employer in at least one year $t+h, h \geq 0$, whereas the latter are workers employed in the same firm throughout the whole time window. The figure shows that peer effects are more important for movers: after five years, the peer effect for them is 0.6 percent, whereas the effect for stayers is 0.1.[20] The difference between movers and stayers can reflect differences in the ability to learn from peers for these two types of workers or the endogeneity of mobility to learning chances in the incumbent and poaching firm: the latter may offer better learning and, therefore, wage prospects that the incumbent firm cannot offer (e.g., Gregory, 2019). At the same time, we have highlighted that changes in peer quality are much more common for movers than for stayers (Figure E.1): hence, movers may have better chances of acquiring knowledge as they move into new peer groups.

Figure C.1B shows that there are no evident differences among peer groups of different sizes. We divide peer groups among those with less than 10 workers, between 10 and 50, and more than 50. Peer effects are slightly larger in small peer groups, where interaction with coworkers is likely easier, but differences are small in magnitude between groups, indicating that peer effects are an important channel of wage growth irrespective of peer group size.

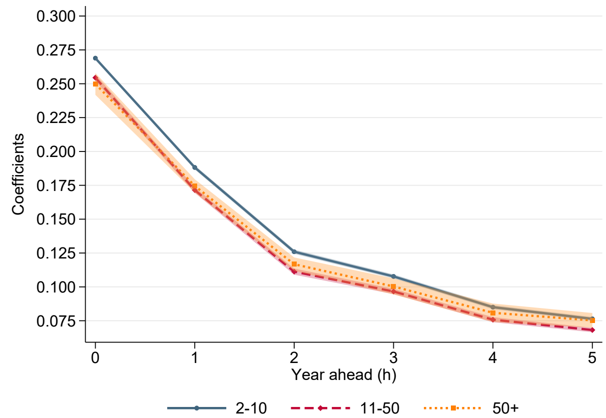
Figure C.1C shows how the effects differ across different tenure years. Specifically, we assign workers at $h=0$ to three discrete categories of tenure with the firm – 0-1 years, 2-3 years, and 4 or more years – and explore how peer effects change for each group of workers. The results illustrate a clear pattern that peers matter the most for low tenure workers, while the effect decreases as one experiences more years in the same firm. The finding is consistent with a learning process: there is more room for a new hire (a worker with low tenure) to learn from peers in a firm.

A similar pattern arises when we explore the heterogeneous impacts across different age groups, as shown in Figure C.1D. The effects are larger for young workers (below age 30), but the decay over time is similar across age brackets.

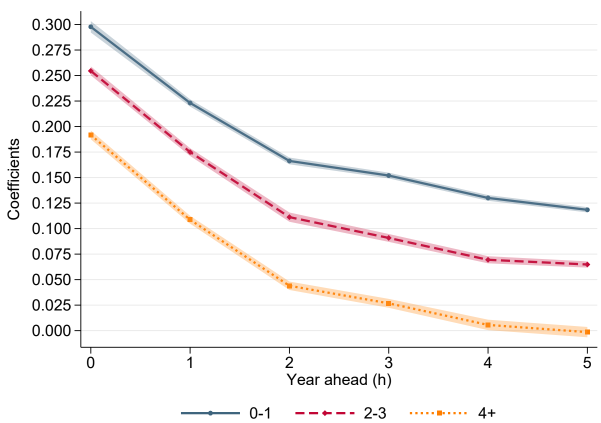
[20]The coefficients are smaller than those in the main analysis because the sample used in this case contains only continually observed workers.



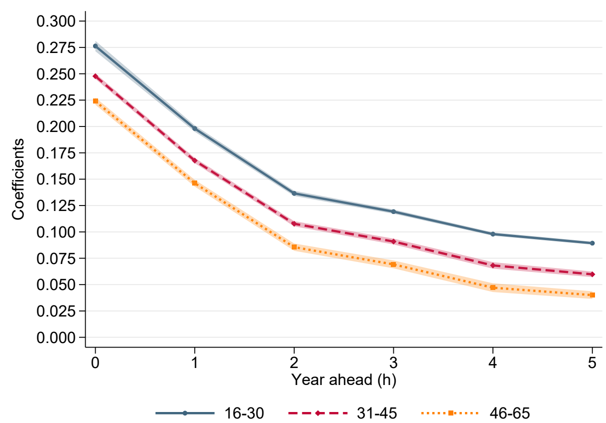
(A) Movers and stayers



(B) Peer group sizes



(C) Tenure brackets



(D) Age brackets

Figure C.1: Heterogeneous peer effects across worker subgroups

Notes. The figure reports the estimates of β from equation (2) using current and future wages as the dependent variable in each year ahead h, where $h \geq 0$, focusing on different groups of workers. Panel A shows estimates for movers and stayers, panel B for workers in peer groups of different size (2-10, 11-50 and more than 50 employees), panel C for different tenure brackets (0-1, 2-3, 4 or more years) and panel D for different age brackets (16-30, 31-45, more than 45 years old). Detailed estimates and sample sizes are reported in Table E.1.

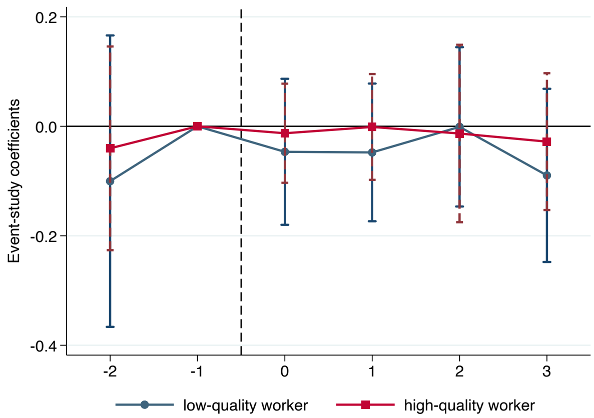
D Robustness Checks for the Event Study Analysis

Firm-level shocks One concern with our mobility design is that the entry or leave of a worker may be correlated with firm-level shocks, even after matching on a rich set of observables. On the one hand, the entry of a high-quality peer may be correlated with expectations of future sale growth. On the other hand, the departure of a high-quality peer may be correlated with expectations of negative shocks to firm sales. We attempt to address this concern by, first, comparing coworkers' wages in firms that hire (or separate from) workers of high- or low-quality relative to firms that hire (or separate from) workers of average quality and, second, by matching firms on observables before mobility happens. We also inspect the evolution of firms' sales around mobility episodes. Due to data limitations, we only have information on sales (and value added, as further robustness) for a subset of our data over the period 1996-2001.[21] We re-estimate equation (9) in the matched sample using log sales (value added) per worker as a dependent variable and we weight regressions by firm size. Given the sample restrictions and the data limitations, we cannot have as many pre-periods as in the main analysis, hence we limit the period before mobility to two years. Figures D.1 and D.2 report the event study estimates for the hire and leave designs, respectively. Both figures show results for log sales per worker in panel A and log value added per worker in panel B and indicate that, in the subsample for which data is available, there is no significant correlation between the quality of workers who move between firms and firm sales or value added. Moreover, before the move, we do not observe different patterns in sales and value added evolution in different groups. This evidence, albeit descriptive and limited to a subset of data, indicates that, after matching, the different groups of firms are comparable in terms of their sale and value added growth. At the same time, this analysis does not really say whether workers' mobility is determined by *expectations* of sale growth, but only that it is not correlated with *realizations* of firm growth.

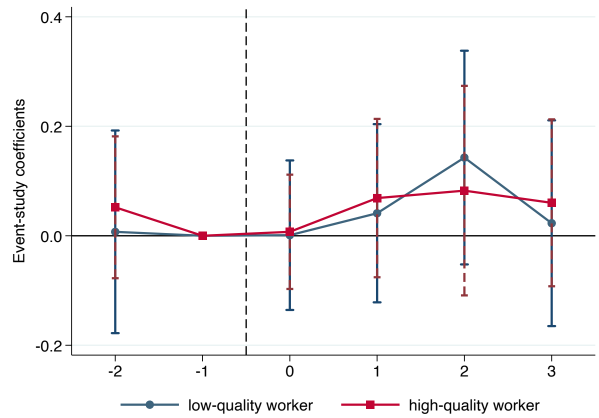
Continuous treatment We replicate our analysis using a continuous treatment, rather than dividing firms into those hiring (separating from) high-, low- or similar-quality workers and movers into those joining high-, low- or similar-quality peer groups. Specifically, in the coworker-level event study, we replace the dummy $Treat_j$ in equation (9) with the AKM worker effect of the worker joining or leaving the firm, i.e., we estimate:

$$w_{-i,j,t} = \eta_t + \psi_j + \sum_{k \neq -1} \widetilde{\beta}_k (\widehat{\alpha}_i \times \mathbf{1}\{t = k\}) + \sum_{k \neq -1} \theta_k (X_j^{pre} \times \mathbf{1}\{t = k\}) + \varepsilon_{-i,j,t}, \tag{D.1}$$

[21]Specifically, in the matched sample, we have information on 325 firms in the hire design (13,029 person-year observations) and 338 firms (14,639 person-year observations) in the leave design.



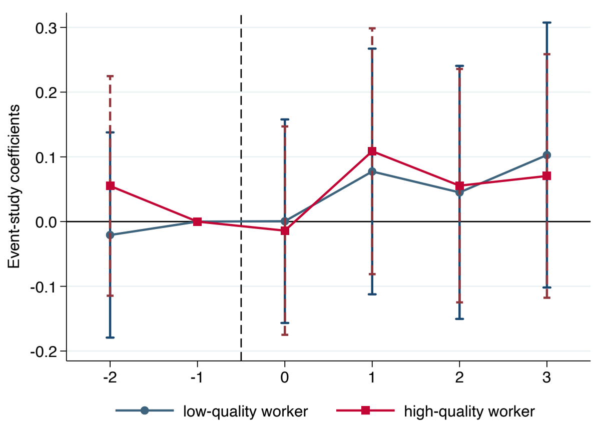
(A) Sales per worker



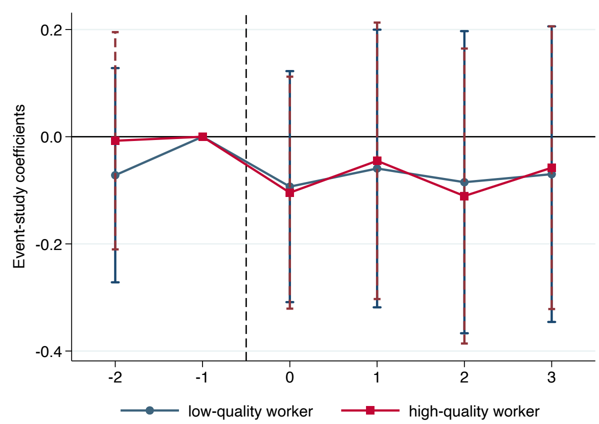
(B) Value added per worker

Figure D.1: Log value added per worker and log sales per worker around mobility, hire design

Notes. The figure reports estimates of β_k from equation (9), separately for firms hiring high-quality and low-quality workers relative to firms hiring similar-quality workers. The dependent variable is log sales per worker in panel A and log value added per worker in panel B. Regressions are weighted by firm size. Vertical bars are 95 percent confidence intervals, obtained from cluster-robust standard errors at the firm level.



(A) Sales per worker



(B) Value added per worker

Figure D.2: Log value added per worker and log sales per worker around mobility, leave design

Notes. The figure reports estimates of β_k from equation (9), separately for firms separating from high-quality and low-quality workers relative to firms separating from similar-quality workers. The dependent variable is log sales per worker in panel A and log value added per worker in panel B. Regressions are weighted by firm size. Vertical bars are 95 percent confidence intervals, obtained from cluster-robust standard errors at the firm level.

where $\widehat{\alpha}_i$ is the pre-estimated worker effect of the joiner or leaver and the other variables are defined as before. We include controls for pre-mobility firm characteristics in X_j^{pre}, interacted with time event dummies: average weekly wages in all pre-periods, the AKM firm effect, the average worker effect, the average age of employees, the share of female and blue-collar employees, firm size and age measured at time -3. The $\widetilde{\beta}_k$'s measure in this case the dynamic effects on coworkers' wages of a one percent increase in the joiner/leaver quality.

In the worker-level event study, we replace $Treat_i$ in (10) with the average AKM peer effect of coworkers in the destination firm. In other terms, we estimate:

$$w_{i,t} = \eta_t + \alpha_i + \sum_{k \neq -1} \widetilde{\gamma}_k(\overline{\widehat{\alpha}}_{-i} \times \mathbf{1}\{t = k\}) + \sum_{k \neq -1} \kappa_k(X_i^{pre} \times \mathbf{1}\{t = k\}) + \varepsilon_{i,t}, \tag{D.2}$$

where $\overline{\widehat{\alpha}}_{-i}$ is the average pre-estimated worker effect of coworkers in the destination firms and the other variables are defined as before. We include controls for pre-mobility worker characteristics in X_i^{pre}, interacted with time event dummies: weekly wage, number of weeks worked, decile of worker fixed effect distribution, age, gender, tenure, occupation, AKM firm effect and firm size, all measured at time -3. The $\widetilde{\gamma}_k$'s measure the dynamic effects on the mover's wage of a one percent increase in the peer quality of the destination firm.

Results are reported in Figure D.3, which shows a very similar pattern to that reported in the main analysis. A 10 percent increase in the quality of a new hire increases coworkers' wage by 0.9 percent on average in the period after mobility, whereas a 10 percent increase in the quality of separating workers does not affect coworkers' wages in the origin firm, although the dynamic effect shows some negative adjustment in the year following mobility. For a mover, a 10 percent increase in the quality of peers at the destination firm increases wages by 2 percent on average. These magnitudes are also broadly in line with those in Figure 3, as highlighted in Section 5.2.3. Overall, these results confirm the findings from the event study with discrete treatment groups.

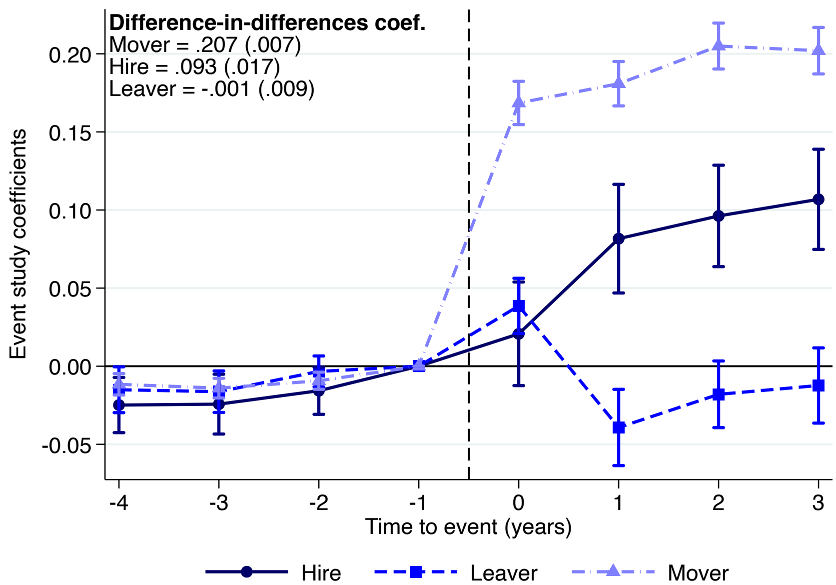


Figure D.3: Event study, continuous treatment

Notes. The figure reports estimates of $\widetilde{\beta}_k$, i.e., the dynamic effects of a 1 percent increase in the quality of a new hire or a separation on coworkers' wages in the origin (*Hire*) and destination firms (*Leave*) from equation (D.1), and the estimates of $\widetilde{\gamma}_k$, i.e., the dynamic effects of a 1 percent increase in the quality of the peer group a mover joins (*Mover*), from equation (D.2). Vertical bars are 95 percent confidence intervals, obtained from cluster-robust standard errors at the firm (for $\widetilde{\beta}_k$) and individual (for $\widetilde{\gamma}_k$) level.

E Additional Figures and Tables

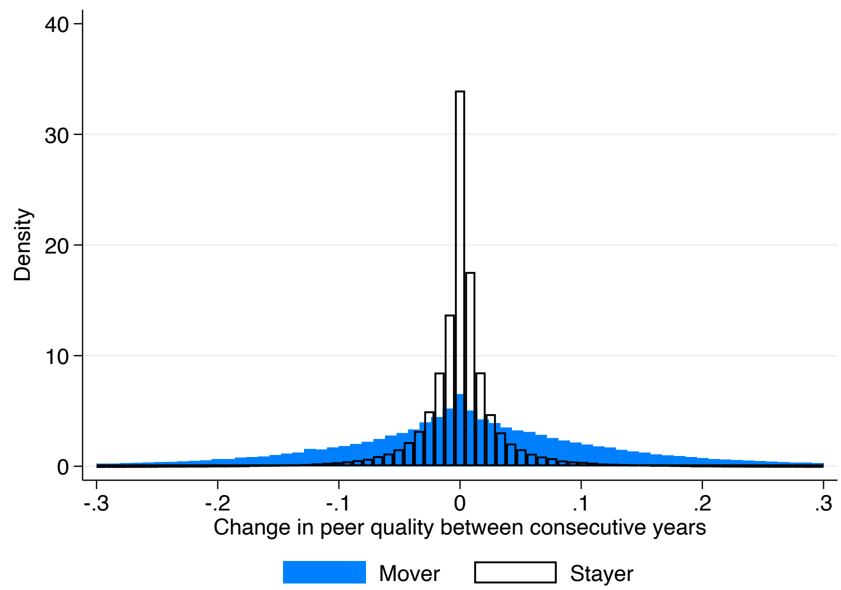
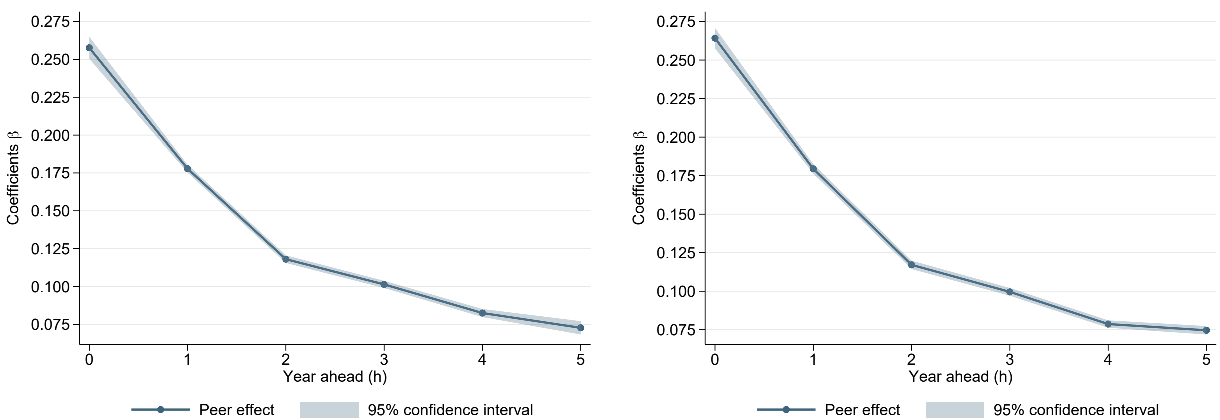


Figure E.1: Density of the change in peer fixed effect between consecutive years for movers and stayers.

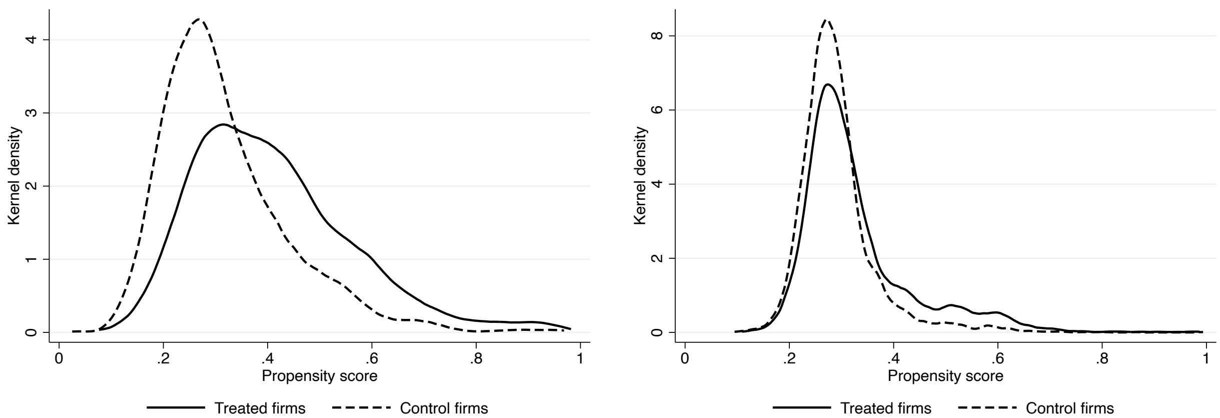
Notes. The figure shows the density of changes in peer quality between consecutive years for movers and stayers. The peer quality is measured as the leave-out average of AKM worker effects at the peer group level, i.e., $\bar{\alpha}_{-i,t} = \frac{1}{|M_{-it}|}\sum_{k \in M_{-it}} \alpha_k$, where $|M_{-it}|$ is the number of peers of worker i.



(A) Padua (B) Venice

Figure E.2: Peer effect estimates in Padua and Venice

Notes. The figures report the peer effect stimates using equation (2) for the two largest local labor markets in Veneto - Padua and Venice.

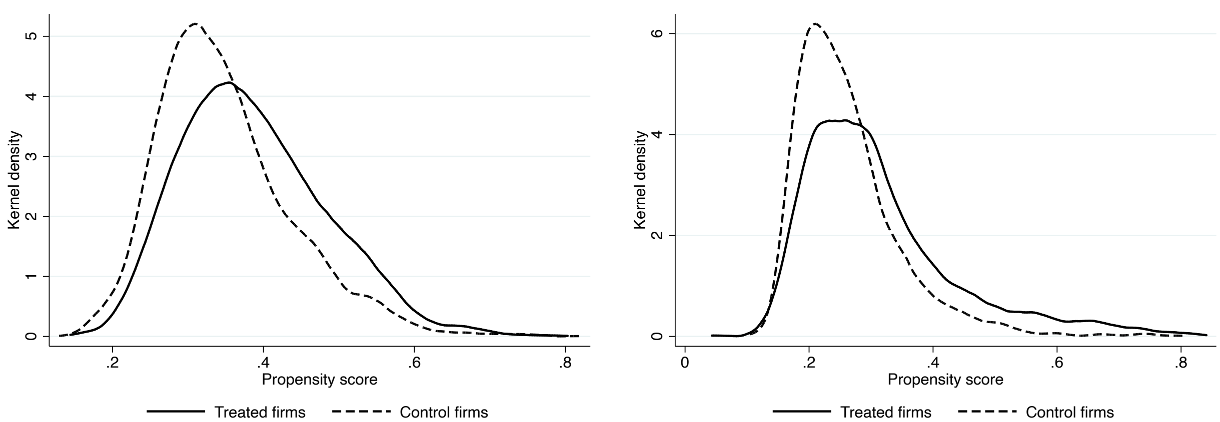


(A) Treatment 1 vs Control group

(B) Treatment 2 vs Control group

Figure E.3: Propensity score density, hire design

Notes. The figure reports the propensity score density for firms hiring high-quality (treatment 1) and similar-quality (control) workers in panel A and for firms hiring low-quality (treatment 1) and similar-quality (control) workers in panel B.

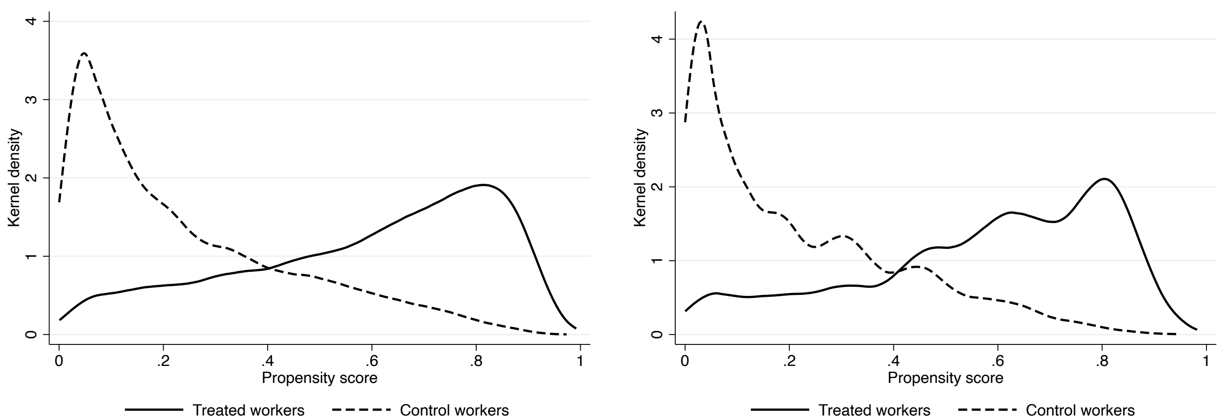


(A) Treatment 1 vs Control group

(B) Treatment 2 vs Control group

Figure E.4: Propensity score density, leave design

Notes. The figure reports the propensity score density for firms separating from high-quality (treatment 1) and similar-quality (control) workers in panel A and for firms hiring low-quality (treatment 1) and similar-quality (control) workers in panel B.



(A) Treatment 1 vs Control group

(B) Treatment 2 vs Control group

Figure E.5: Propensity score density, mover design

Notes. The figure reports the propensity score density for workers moving into high-quality (treatment 1) and similar-quality (control) peer groups in panel A and for workers moving into low-quality (treatment 1) and similar-quality (control) peer groups in panel B.

Table E.1: Estimates of the peer effect on current and future wages

Years ahead		Baseline	Mover/Stayer		Peer size			Tenure brackets			Age brackets		
			Mover	Stayer	$0-10$	$11-50$	50+	$0-1$	$2-3$	4+	$16-30$	$31-45$	$46-65$
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$h=0$	$\hat{\beta}$	0.258	0.263	0.182	0.269	0.259	0.250	0.298	0.255	0.192	0.276	0.248	0.224
	s.e.	(0.002)	(0.002)	(0.003)	(0.001)	(0.001)	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)	(0.001)	(0.002)
	# obs	17,723,260	4,587,715	6,096,654	4,921,186	5,434,321	7,367,753	6,832,798	3,865,810	7,024,652	7,678,846	6,733,816	3,310,598
$h=1$	$\hat{\beta}$	0.178	0.157	0.100	0.188	0.174	0.172	0.223	0.175	0.109	0.198	0.168	0.146
	s.e.	(0.003)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)
	# obs	14,386,952	4,643,066	4,851,831	4,159,930	4,376,922	5,850,100	5,227,468	3,214,061	5,945,423	6,176,693	5,582,025	2,628,234
$h=2$	$\hat{\beta}$	0.118	0.090	0.042	0.126	0.114	0.113	0.166	0.111	0.044	0.137	0.108	0.086
	s.e.	(0.003)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)
	# obs	12,268,216	4,610,848	3,884,112	3,675,595	3,703,138	4,889,483	4,398,535	2,780,110	5,089,571	5,339,434	4,810,235	2,118,547
$h=3$	$\hat{\beta}$	0.101	0.077	0.030	0.108	0.098	0.098	0.152	0.091	0.027	0.119	0.091	0.069
	s.e.	(0.003)	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)
	# obs	10,613,066	4,460,695	3,115,653	3,292,768	3,183,362	4,136,936	3,811,229	2,431,132	4,370,705	4,714,626	4,199,253	1,699,187
$h=4$	$\hat{\beta}$	0.080	0.059	0.013	0.085	0.076	0.078	0.130	0.069	0.006	0.098	0.068	0.047
	s.e.	(0.002)	(0.001)	(0.003)	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)	(0.003)	(0.001)	(0.002)	(0.002)
	# obs	9,201,160	4,229,238	2,490,626	2,952,472	2,751,998	3,496,690	3,336,339	2,130,425	3,734,396	4,180,065	3,679,285	1,341,810
$h=5$	$\hat{\beta}$	0.073	0.055	0.008	0.076	0.068	0.072	0.118	0.065	-0.001	0.089	0.060	0.040
	s.e.	(0.002)	(0.001)	(0.003)	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)	(0.003)	(0.001)	(0.001)	(0.002)
	# obs	7,959,214	3,938,659	1,977,090	2,633,008	2,373,113	2,953,093	2,929,847	1,880,429	3,148,938	3,694,233	3,227,907	1,037,074

Notes. The table reports estimates of equation (2) for time horizons $h = 0,...,5$. Column (1) reports estimates for the full sample. Column (2) and (3) report estimates for workers changing employer at least once between $h = 0$ and $h = 5$ (*Mover*) and for those staying in the same firm (*Stayer*). Note the number of observations of Mover and Stayer does not add up to the one in Column (1). It is because we exclude the workers who are only observed for one period, who moved out of the labor market, and who newly entered. Columns (4) to (6) report estimates for workers in different peer group size brackets: 2-10, 11-50, and more than 50. Columns (7) to (9) report estimates for different tenure brackets: 0-1, 2-3, and more than 3 years of tenure with the firm. Columns (10) to (12) report results for different age groups: younger than 30, between 31 and 45, and older than 45. Robust standard errors, clustered at the firm level, are reported in parentheses.

Table E.2: Return to experience in Veneto from 1982 to 2001

	log(wages)
Experience	0.020
Experience^2	-0.001
$\mathbb{E}[\frac{\partial w_{it}}{\partial e_{it}}] = \gamma_1 + 2\gamma_2 \mathbb{E}[e_{it}] = 1.36\%$	

Notes. The table uses workers who are *not* left-censored so that we could calculate their actual working experience. We then use the following modified Mincer regression

$$w_{it} = \alpha_i + \gamma_1 e_{it} + \gamma_2 e_{it}^2 + \psi_{jt} + \phi_{oj} + \theta_{ot} + \varepsilon_{it},$$

where e_{it} is the years of experience and the average experience in the sample is around 3.2 years. The two coefficients in the table refer to $\widehat{\gamma}_1$ and $\widehat{\gamma}_2$ in the regression.

Table E.3: Balance test of covariates, before and after matching, hiring design

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	High-quality mover				Low-quality mover			
	Unmatched		Matched		Unmatched		Matched	
	Diff.	P-value	Diff.	P-value	Diff.	P-value	Diff.	P-value
Mean wage -4	-21.574	0.000***	-5.932	0.141	7.136	0.124	1.291	0.785
Mean wage -3	-21.511	0.000***	-5.905	0.156	7.951	0.093*	4.570	0.342
Mean wage -2	-21.050	0.000***	-5.580	0.165	10.742	0.026**	4.523	0.356
AKM worker effect	-0.088	0.000***	-0.006	0.178	-0.000	0.955	0.006	0.273
AKM firm effect	-0.001	0.721	-0.006	0.155	-0.025	0.000***	-0.001	0.791
Employees' mean age	3.512	0.000***	-0.177	0.399	0.659	0.000***	-0.165	0.456
Share female	0.006	0.534	0.003	0.811	0.028	0.007***	-0.011	0.383
Share blue-collar	0.014	0.145	-0.007	0.513	-0.049	0.000***	0.011	0.400
Firm size	0.293	0.136	-0.069	0.770	0.236	0.233	0.106	0.642
Firm age	0.492	0.000***	-0.107	0.512	0.280	0.047**	-0.021	0.905
Value added	332.034	0.645	-425.186	0.631	-1.2e+03	0.138	-75.134	0.938
Revenues	94.932	0.895	-420.232	0.634	517.826	0.501	-11.555	0.990
Manufacturing	-0.104	0.000***	0.012	0.425	-0.041	0.002***	0.018	0.263
Construction	0.055	0.000***	-0.006	0.548	0.002	0.823	0.008	0.382
Wholesale	-0.020	0.030**	0.001	0.904	-0.038	0.000***	-0.001	0.927
Accommodation	0.018	0.000***	-0.004	0.519	0.008	0.074*	-0.004	0.462
Transports	0.001	0.771	-0.003	0.574	-0.007	0.125	-0.003	0.630
Finance	-0.012	0.000***	0.001	0.733	0.001	0.809	-0.003	0.633
Services	-0.010	0.058*	-0.002	0.783	-0.010	0.100*	-0.005	0.459
Health	0.038	0.000***	0.010	0.062*	0.047	0.000***	-0.009	0.171
Domestic	0.003	0.289	-0.000	0.897	0.005	0.128	0.001	0.784
Other	0.031	0.000***	-0.010	0.237	0.032	0.000***	-0.003	0.751
Belluno	0.019	0.001***	0.002	0.746	0.001	0.882	-0.004	0.590
Padua	-0.023	0.016**	0.005	0.692	-0.001	0.934	0.006	0.617
Rovigo	-0.002	0.722	0.000	0.940	-0.006	0.275	0.005	0.389
Treviso	-0.003	0.766	0.004	0.723	-0.010	0.335	0.006	0.609
Venice	-0.004	0.686	-0.012	0.254	0.004	0.657	-0.015	0.212
Vicenza	-0.008	0.433	0.003	0.820	-0.003	0.784	-0.002	0.903
Verona	0.020	0.036**	-0.002	0.875	0.015	0.152	0.002	0.861
N. treated	2517		2164		2015		1848	
N. control	4636		2164		4636		1848	

Notes. The table reports a balance test of covariates used for matching in the analysis of the effect of a new hire on coworkers' wages. Columns (1) to (4) report the average difference and the p-value of the difference for each variable in the unmatched (columns 1-2) and matched (column 3-4) samples comparing firms hiring a high-quality worker to those hiring a similar-quality worker. Columns (5) to (8) report the average difference and the p-value of the difference for each variable in the unmatched (columns 5-6) and matched (column 7-8) samples comparing firms hiring a low-quality worker to those hiring a similar-quality worker. Heteroskedasticity robust p-values are obtained from univariate regressions of each covariate on a dummy for firms hiring high-quality workers (in columns 2 and 4) or low-quality workers (in columns 6 and 8). Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table E.4: Balance test of covariates, before and after matching, leave design

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	High-quality mover				Low-quality mover			
	Unmatched		Matched		Unmatched		Matched	
	Diff.	P-value	Diff.	P-value	Diff.	P-value	Diff.	P-value
Mean wage -4	-2.060	0.509	-4.895	0.163	-1.729	0.676	-0.362	0.940
Mean wage -3	-1.298	0.682	-3.758	0.292	-0.208	0.960	-1.934	0.688
Mean wage -2	0.039	0.990	-4.018	0.256	4.700	0.285	-1.859	0.693
AKM worker effect	-0.036	0.000***	-0.004	0.327	-0.036	0.000***	-0.002	0.708
AKM firm effect	0.000	0.940	-0.004	0.264	-0.008	0.061*	-0.002	0.735
Employees' mean age	2.109	0.000***	-0.040	0.811	1.972	0.000***	0.202	0.352
Share female	-0.006	0.470	0.009	0.365	0.048	0.000***	-0.017	0.183
Share blue-collar	0.005	0.539	-0.002	0.840	-0.093	0.000***	0.015	0.260
Firm size	0.642	0.000***	-0.046	0.828	-0.149	0.519	-0.385	0.087*
Firm age	0.124	0.309	-0.072	0.616	0.006	0.965	-0.037	0.836
Value added	39.360	0.952	782.142	0.303	709.148	0.354	209.679	0.825
Revenues	-290.164	0.657	176.462	0.816	363.910	0.627	177.693	0.851
Manufacturing	-0.057	0.000***	-0.002	0.895	-0.105	0.000***	0.014	0.392
Construction	0.045	0.000***	0.003	0.682	0.013	0.091*	0.014	0.139
Wholesale	-0.022	0.007***	-0.001	0.915	-0.027	0.004***	-0.006	0.596
Accommodation	0.017	0.000***	-0.002	0.701	0.003	0.528	0.001	0.860
Transports	0.003	0.462	-0.003	0.557	-0.001	0.796	0.002	0.762
Finance	-0.008	0.016**	0.004	0.250	0.003	0.502	-0.007	0.237
Services	-0.025	0.000***	-0.001	0.880	0.004	0.504	-0.007	0.399
Health	0.017	0.000***	0.005	0.231	0.055	0.000***	-0.003	0.676
Domestic	0.006	0.037**	-0.001	0.846	0.009	0.014**	-0.002	0.634
Other	0.023	0.000***	-0.003	0.628	0.046	0.000***	-0.006	0.535
Belluno	0.013	0.009***	-0.003	0.644	0.010	0.079*	-0.002	0.827
Padua	-0.023	0.008***	-0.001	0.892	-0.018	0.070*	-0.002	0.868
Rovigo	0.006	0.200	0.000	0.949	0.004	0.439	0.005	0.411
Treviso	-0.019	0.029**	0.011	0.250	-0.004	0.708	0.008	0.526
Venice	0.020	0.015**	0.001	0.915	0.002	0.808	-0.001	0.964
Vicenza	-0.003	0.785	-0.008	0.447	-0.008	0.429	0.010	0.467
Verona	0.006	0.489	-0.000	0.972	0.014	0.148	-0.019	0.139
N. treated	3065		2905		2046		1885	
N. control	5374		2905		5374		1885	

Notes. The table reports a balance test of covariates used for matching in the analysis of the effect of a separation on coworkers' wages. Columns (1) to (4) report the average difference and the p-value of the difference for each variable in the unmatched (columns 1-2) and matched (column 3-4) samples comparing firms separating from a high-quality worker to those separating from a similar-quality worker. Columns (5) to (8) report the average difference and the p-value of the difference for each variable in the unmatched (columns 5-6) and matched (column 7-8) samples comparing firms separating from a low-quality worker to those separating from a similar-quality worker. Heteroskedasticity robust p-values are obtained from univariate regressions of each covariate on a dummy for firms separating from high-quality workers (in columns 2 and 4) or low-quality workers (in columns 6 and 8). Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table E.5: Balance test of covariates, before and after matching, mover design

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	High-quality peers				Low-quality peers			
	Unmatched		Matched		Unmatched		Matched	
	Diff.	P-value	Diff.	P-value	Diff.	P-value	Diff.	P-value
Wage -4	-0.070	0.000***	0.007	0.513	0.201	0.000***	-0.009	0.658
Wage -3	-0.076	0.000***	0.008	0.490	0.216	0.000***	-0.011	0.578
Wage -2	-0.081	0.000***	0.007	0.567	0.229	0.000***	-0.009	0.681
Weeks worked	-0.420	0.004***	0.105	0.321	0.196	0.017**	0.085	0.618
AKM worker effect decile	-2.682	0.000***	-0.033	0.712	2.881	0.000***	0.094	0.279
Age	6.504	0.000***	-0.303	0.425	0.360	0.407	-0.523	0.068*
Female	0.091	0.000***	0.002	0.957	-0.125	0.000***	-0.023	0.182
Tenure	-0.114	0.038**	0.091	0.074*	-0.012	0.760	0.070	0.328
Blue-collar	0.049	0.001***	0.008	0.658	-0.261	0.000***	0.031	0.209
AKM firm effect	-0.005	0.341	0.002	0.680	-0.003	0.477	0.007	0.231
Log firm size	0.260	0.002***	0.002	0.973	0.319	0.000***	0.033	0.759
Manufacturing	0.015	0.358	0.001	0.964	-0.027	0.052*	-0.019	0.490
Construction	0.004	0.361	0.001	0.867	0.005	0.238	-0.002	0.800
Wholesale	-0.007	0.528	-0.003	0.790	-0.004	0.539	0.012	0.410
Accommodation	-0.000	0.683	0.001	0.259	0.001	0.216	-0.001	0.443
Transports	-0.002	0.706	0.001	0.818	-0.005	0.175	-0.002	0.455
Finance	-0.018	0.235	0.004	0.649	0.010	0.197	0.018	0.161
Services	-0.002	0.605	0.000	0.836	0.002	0.533	-0.002	0.784
Health	0.003	0.267	-0.001	0.409	0.008	0.232	-0.002	0.680
Domestic	0.003	0.148	-0.001	0.286	0.003	0.408	-0.001	0.839
Other	0.003	0.411	-0.003	0.745	0.009	0.147	-0.001	0.945
Belluno	0.001	0.667	0.002	0.512	0.005	0.085*	0.001	0.668
Padua	-0.009	0.429	-0.001	0.963	-0.011	0.078*	0.011	0.571
Rovigo	-0.003	0.408	0.000	0.979	-0.009	0.031**	-0.003	0.494
Treviso	0.000	0.995	0.001	0.936	0.004	0.501	-0.006	0.641
Venice	-0.008	0.510	0.001	0.956	-0.005	0.595	-0.000	0.986
Vicenza	0.018	0.073*	-0.004	0.756	0.019	0.078*	-0.001	0.974
Verona	0.001	0.830	0.001	0.879	-0.002	0.704	-0.003	0.757
N. treated	26194		15511		22547		12778	
N. control	46007		15511		46007		12778	

Notes. The table reports a balance test of covariates used for matching in the analysis of the effect on worker's wages of moving into peer groups of different quality. Columns (1) to (4) report the average difference and the p-value of the difference for each variable in the unmatched (columns 1-2) and matched (column 3-4) samples comparing workers moving into high-quality peers to those moving into similar-quality peers. Columns (5) to (8) report the average difference and the p-value of the difference for each variable in the unmatched (columns 5-6) and matched (column 7-8) samples comparing workers moving into low-quality peers to those moving into similar-quality peers. Heteroskedasticity robust p-values are obtained from univariate regressions of each covariate on a dummy for workers moving into high-quality peers (in columns 2 and 4) or low-quality peers (in columns 6 and 8). Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table E.6: Summary statistics of movers in different samples

	(1) Hire	(2) Leave	(3) Worker	(4) Other
Annual earnings	29134.05	29147.11	34501.23	29876.82
	(14591.10)	(11993.73)	(15182.85)	(23340.24)
Weekly wage	617.19	617.73	691.49	683.59
	(241.94)	(182.10)	(274.16)	(1849.68)
Weeks worked	45.66	45.81	48.66	40.02
	(12.55)	(12.44)	(8.92)	(15.76)
Age	31.35	29.99	31.96	30.88
	(9.57)	(8.97)	(8.46)	(9.33)
Tenure	2.74	2.90	3.48	2.04
	(2.25)	(2.21)	(2.48)	(2.33)
Woman	0.29	0.30	0.28	0.35
	(0.45)	(0.46)	(0.45)	(0.48)
Blue-collar	0.80	0.77	0.78	0.73
	(0.40)	(0.42)	(0.42)	(0.45)
Open-ended contract	1.00	1.00	1.00	0.99
	(0.06)	(0.03)	(0.02)	(0.10)
Manufacturing	0.54	0.49	0.66	0.54
	(0.50)	(0.50)	(0.47)	(0.50)
Number of workers	10,524	12,626	82,402	1,585,305

Notes. The table reports means and standard deviations (in parentheses) of selected mover characteristics three years before the move. Columns (1) to (4) report statistics for movers in the hire design, in the leave design, in the worker-level design, and in all other moves not included in the previous ones, respectively.