Outside Options, Bargaining, and Wages: Evidence from Coworker Networks*

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Abstract

This paper analyzes the link between wages and outside employment opportunities. To overcome the fact that factors that affect a worker's outside options may also impact her productivity at her current job, we develop a strategy that isolates changes in a worker's information about her outside options. This strategy relies on the fact that individuals often learn about jobs through social networks, including former coworkers. We implement this strategy using employer-employee data from Denmark that contain monthly information on wages and detailed measures of worker skills. We find that increases in labor demand at former coworkers' current firms lead to job-to-job mobility and wage growth. Consistent with theory, larger changes are necessary to induce a job-to-job transition than to induce a wage gain. Specification tests leveraging alternative sources of variation suggest these responses are indeed due to information rather than unobserved demand shocks. Impacts on earnings are concentrated among workers in the top half of the skill distribution. Finally, we use our reduced-form estimates to identify a structural model that allows us to estimate bargaining parameters and investigate the relevance of wage posting and bargaining across different skill groups.

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1 Introduction

There is growing evidence that imperfect competition and frictions in the labor market have a significant impact on the wage distribution (Card et al., 2013; Barth et al., 2016; Card et al., 2016*c*). In such a labor market, workers' wages depend not only on their productivity, but on the characteristics of the firm they work at and on the characteristics of the firms they could have worked at.[1] However, to date, there is little empirical evidence on the link between workers' outside options and their wages. If two workers at a firm are equally productive, does the worker with better opportunities at other firms (or better information about these opportunities) earn more? Can workers renegotiate their wage with their current firm if they receive an outside offer?

The link between an individual's outside options and her wages is important both for distinguishing between different models of wage-setting and for understanding how recent developments in the labor market, including the use of no-poach agreements and the rise in labor market concentration, will impact wages. However, examining this link empirically is challenging both because outside options are not observed in standard datasets and because most factors that shift workers' outside options also shift their productivity in their current job. This is a problem because changes in productivity at the incumbent firm should impact wages, even if the labor market is perfectly competitive.

This paper overcomes these challenges by combining a novel identification strategy that exploits changes in workers' *information* about their outside opportunities with rich administrative data that contain high-frequency (monthly) wage data and detailed measures of workers' skills. The empirical strategy is motivated by a large literature, pioneered by Granovetter, that documents that workers learn about job opportunities through their social networks (Granovetter, 1973; Ioannides and Datcher Loury, 2004; Topa, 2011).[2] We create measures of a worker's information about outside opportunities by weighting firm-specific changes in labor demand by each worker's unique coworker network. These networks consist of the set of individuals a worker has worked with in the recent past, but is no longer working with. They allow us to identify which new positions an individual is likely to hear about. Because networks vary across workers within the same occupation, and even within the same firm-and-occupation group, we are able to exploit differences in information between workers in the same narrow skill group.

[1] This is explicit in models where wages are determined by bargaining between an individual and a firm or a union and a firm (Pissarides, 2000; Acemoglu, 2001; Farber, 1986). It is implicit in models with posting; in these models, the wage a firm chooses to post depends on the wages chosen by other firms (Burdett and Mortensen, 1998; Manning, 2003).

[2] Similar facts were presented in prior work by Myers and Shultz (1951), Rees (1966), and Rees and Shultz (1970).

The data come from a new monthly linked employer-employee database covering the universe of employees at Danish companies. While wages in Denmark were historically set by union bargaining, firms today have considerable latitude to negotiate wages with individual employees (Dahl et al., 2013). Our data cover the period post-decentralization.[3] The data contain information on individuals' monthly earnings and hours worked, and on their six-digit industry and occupation.

We start by deriving our measure of outside options from a search model where firms renegotiate wages with workers that receive outside offers. The model allows us to illustrate the two key predictions of this class of models. First, workers who receive outside offers from more productive firms leave. Second, workers who receive outside offers from less productive firms that dominate their current position renegotiate. We modify the model to allow workers to learn about job opportunities both through public sources and through their, individual-specific, social networks. This allows us to derive a measure of outside options that we can take to the data.

We then test the key predictions of this theory by regressing indicators for mobility and measures of wage growth on our individual- and time-specific measures of outside options. Our baseline measure weights the number of new positions at each firm by an individual's exposure to that firm through their coworker network. The identifying assumption is that, conditional on the included covariates, unobserved determinants of individual mobility or wage growth are uncorrelated with time-varying labor demand at an individual's former coworkers' current firms. In order to focus on variation in outside options over time for a given worker, we include worker fixed effects in all of our specifications. We also control, non-parametrically, for month- and (four-digit) industry-specific demand shocks. The primary threat to validity, which we address through a series of distinct tests, is that the coworker networks proxy for specific types of skills, and that there are unobserved month-specific changes in demand for these skills, that are correlated with unobserved determinants of job-to-job mobility and wage growth.

We present non-parametric evidence that confirms both predictions: (1) changes in workers' information about their outside opportunities lead to mobility and wage growth, and (2) larger changes are necessary to induce a job-to-job transition than to induce a wage change. Virtually all of the increased mobility is the result of moves to firms where the worker has a former coworker. This is consistent with the idea that workers learned about the opportunity through their former colleagues. We find an additional ten new positions at an individual's former coworkers' current firms results in a fifteen percent higher probability the worker makes a job-to-job transition that

[3]Our data cover the period 2008-2016. Most wage decentralization occurred in the 1990s.

month.[4] The same change translates to approximately $50 more earnings over the course of the year. However, most individuals do not renegotiate: the impact on *whether* an individual sees an earnings gain is less than a percentage point. If all of the gains were associated with gains for workers who were driven to renegotiate (see a positive earnings change), the average full-time worker would see an 11% increase in base pay.

Several distinct pieces of evidence suggest that our results are not driven by unobserved changes in demand for workers' skills. First, we show that the estimates are stable when adding more detailed non-parametric controls for changes in demand for different occupation or skill groups. These controls are based on different combinations of our industry, occupation, and education fixed effects. We also show that the results are robust to adopting a within-firm identification strategy that exploits variation in coworker networks that emerges from differences in tenure at the current firm and at past firms. The evidence is most consistent with *worker*-initiated renegotiation, not firm-initiated raises. If the earnings changes were the result of firms learning about the market price of their workers' skills, we would expect to see all workers within the same firm and occupation see equal wage growth.

We decompose our measure of outside options into portions coming from different subsets of an individual's former coworkers. We find that the changes in earnings are driven by changes in labor demand at the firms of closely-connected former coworkers, consistent with our information-transmission story. In particular, coworkers who are still working in the same (of five) administrative region matter more, as do coworkers the individual worked with more recently. We also construct similar measures of outside options based on an individual's future coworker network. If our results were driven by unobserved demand shocks, we would expect these measures to have a significant impact on both mobility and wage growth. We would also expect that adding these measures as controls to our baseline regression would reduce our estimates. We do not find support for either of these predictions.

Wage effects are largest for higher skilled workers and for workers with more specialized skills. We divide workers into eight broad occupation groups and re-estimate the effects within each group.[5] We find that the impact on workers with specialized skills (professionals) is double that of workers in the middle skill group (technicians), and nearly five times that of workers in the least skilled

[4] Because our data are monthly, the base rate is low: roughly one percent of workers make a job-to-job transition each month.

[5] The groups are: (1) managers, (2) professionals, (3) technicians, and associate professionals (4) clerical support workers, (5) service and sales workers, (6) craft and related trade workers, (7) plant and machine operators, and (8) assembly workers. These groups are based on the broad International Standard Classification of Occupation (ISCO) codes.

group. Because workers with specialized skills also have higher baseline earnings, these impacts on translates into substantially larger effects on the level of earnings. However, there are impacts on mobility and earnings for all but the least skilled workers. Within each skill group, women benefit less than men.

In addition, both job-mover and stayers appear to benefit. We find that individuals who stay at their current firm obtain roughly 20% the earnings gain of job movers. Posting models—including monopsony models—would predict a ratio of zero: wages do not adjust unless the individual switches firms. Spot market models where wages freely fluctuate in response to changes in demand for a worker's skill would predict a ratio of one. We are able to reject both of these extremes.

Our reduced form results only indicate that some firms and workers engage in wage renegotiation; they do not indicate that all firms renegotiate. Some firms may be able to commit not to renegotiate wages with employees who receive outside offers (Postel-Vinay and Robin, 2004; Doniger, 2015). To assess the extent to which firms negotiate with different groups of workers, we use our reduced form estimates to identify a structural search model incorporating on-the-job search, information transmission through networks, and a mass of posting firms. The model is based on Flinn and Mullins (2017); our estimates contribute to a small literature on the empirical relevance of wage-posting and bargaining (Hall and Krueger, 2012; Doniger, 2015).

We estimate this model, separately for different skill groups, using simulated method of moments. Our estimates indicate that wage renegotiation is more common among high skilled workers. Using these parameters, we estimate that a 50% reduction in the arrival rate for employed workers would lead to a significant reduction in wage growth. For high skilled workers a larger portion of this is due to decreased on-the-job bargaining; for lower skilled workers this is mostly due to decreased mobility. Overall, the results indicate that changes in the labor market that hamper workers' ability to obtain or use outside offers may have meaningful impacts on wage growth.

1.1 Related Literature

This paper contributes to several distinct literatures. In particular, outside options are a key ingredient in macroeconomic search and bargaining models, which assume that individual workers negotiate–and potentially renegotiate–their wage with their employers (Pissarides, 2000; Postel-Vinay and Robin, 2002; Cahuc et al., 2006). In some of these models, the worker's outside option is the value of non-employment. In models where employed workers can renegotiate wages with their current firm, the outside option is typically the best outside offer the worker has received. Bargaining on the basis of outside offers rationalizes many macroeconomic phenomena including wage dispersion (Hornstein et al., 2011), mismatch (Hagedorn et al., 2017; Lise and Robin, 2017), and wage cyclicality for job switchers (Gertler et al., 2016). A large literature uses structural models of renegotiation on the basis of outside offers to measure the determinants of wage growth (see, e.g., Bagger et al., 2014*b*; Lise et al., 2016; Jarosch, 2015).[6]

Only a handful of papers have directly examined the role of workers' outside options in wage-setting and none, to our knowledge, have used individual-level variation. Beaudry et al. (2012) use cross-city variation in the growth of different industries to show that there are sectoral linkages in wages, consistent with bargaining models (see also Bidner and Sand, 2016; Fortin and Lemieux, 2015). Our approach is more similar to that used by Hagedorn and Manovskii (2013). They use a proxy for the number of offers an individual has received since starting a job—based on the vacancy-to-unemployment rate—to test the predictions of spot market models.[7] Contemporaneous work by Jäger et al. (2018) finds that wages are do not respond to changes in the value of non-employment, suggesting that other wage-setting protocols, including the one investigated in this paper, may be more relevant. This paper differs from the prior literature in its focus on whether firms negotiate with individual workers on the basis of changes in the worker's opportunities at other firms.[8]

This paper is also related to recent work that has found that idiosyncratic changes in firm rents impact the wages of workers at those firms (Abowd and Lemieux, 1993; Van Reenen, 1996; Card et al., 2014; Kline et al., 2018; Mogstad et al., 2017). The small rent-sharing elasticities reported in this literature (.05-.15) suggest that workers may be able to capture a large portion (85-95%) of changes in the value of their outside option (Card et al., 2016*c*). This is because, in simple bargaining models, workers' wages are a weighted average of the rents produced in the match and the workers' outside options. However, this one-to-one relationship might break down if changes in outside options are not verifiable, or if firms are able to commit not to renegotiate (Hall and Lazear, 1984; Manning, 2003). The results in this paper suggest workers capture a much smaller portion of changes in their outside options than most rent-sharing estimates imply.

This paper also contributes to a rapidly growing literature on information transmission through

[6] Within the search literature, this paper is most related to work by Lamadon (2014), who investigated the transmission of both firm- and worker- productivity shocks to wages using a directed, competitive search model estimated on Swedish matched employer-employee data. That paper used the correlation in wage growth between an individual and his current coworkers (who experience the same firm shocks) to separately identify worker- and firm-productivity shocks in the context of a directed search model.

[7] One key difference between this paper and that paper is that we generate a measure of the arrival rate that varies across workers with identical patterns of employment/non-employment.

[8] Caldwell and Danieli (2018) investigate the role of outside options in generating between-group wage inequality. The authors use an assignment model to derive an index of workers' outside options, analogous to concentration indices often used in the industrial organization literature. They then use the cross-sectional distribution of workers to estimate this index.

social networks (Simon and Warner, 1992; Hensvik and Skans, 2016; Giorgi et al., 2016; Bailey et al., forthcoming; Beaman, 2016; Gee et al., 2017; Glitz and Vejlin, 2018). A recent series of papers shows that recently displaced workers and workers entering a new labor market use information obtained from their former coworkers (Glitz, 2013; Saygin et al., 2014), classmates (Zimmerman, Forthcoming), family members (Kramarz and Skans, 2014), and neighbors (Bayer et al., 2008; Schmutte, 2014) to find job opportunities. This paper shows that currently employed workers also use this information. It is most related to Shue (2013); that paper shows that an individual's wages are more related to those of his/her randomly assigned Harvard Business School section-mates than to his/her different-section classmates (2013). The data in that paper contain pay for CEOs and other top executives; we focus on workers who are unable to set their own pay and must, instead, bargain with their employer.[9]

The rest of the paper proceeds as follows: Section 2 develops a theoretical model that incorporates both on-the-job search and a mix of firm wage-setting strategies. It then uses this model to derive empirical predictions, to derive a measure of workers' outside options, and to explain the key identifying assumption. Section 3 describes the institutional features of the Danish labor market and the administrative population-based registers we use. Section 4 explains the empirical strategy and maps variables in our data onto the theoretical objects described in Section 2. Section 5 presents reduced form results on mobility and earnings and Section 6 explores heterogeneity in these results. Section 7 uses the reduced form to identify bargaining parameters and to estimate the extent of bargaining, relative to posting, for workers in different skill groups. Section 8 concludes.

2 Outside Options and Wages

We start by developing a continuous time search model with bargaining and on-the-job search. Our model is based on Flinn and Mullins (2017) and is standard in all but two respects.

First, rather than assuming that all firms and workers bargain over wages, the model allows for two types of firms: those that post wages and those that negotiate and renegotiate wages with workers. This relaxation makes our model more general, since the other leading model of wage-setting under imperfect competition (monopsony) features wage-posting. Further, the speed with which various changes in the labor market—changes in concentration or the enforcement of no-poach agreements—will impact wages depends on whether workers have to switch firms in order to benefit

[9] This paper is also related to a recent series of papers that use linked employer-employee data to investigate the importance of an individual's coworkers in determining wage growth (Cornelissen et al., 2017; Herkenhoff et al., 2018; Jarosch et al., 2018). These papers have shown that individuals appear to learn from their more productive colleagues, and that this learning is reflected in their wages.

from changes in their outside options.

Second, we depart from the model in Flinn and Mullins (2017) and allow workers to learn about job opportunities both through public sources and through their social networks. This is key for our reduced form strategy. For simplicity, we first derive the model assuming all workers—conditional on employment status—learn about jobs at the same rate. In Section 2.4 we show how the addition of social networks allows us to generate a measure of outside options that we can take to the data.

2.1 Model Setup

Workers vary in ability a and firms vary in productivity θ. A worker of ability a matched with a firm of type θ produces $a\theta$. Time is continuous and both firms and workers are risk neutral and discount the future at rate ρ. Matches are dissolved exogenously at rate δ and workers receive ab while unemployed. The parameter b reflects both the value of unemployment benefits and the value of non-work time.[10] Search is undirected and workers learn about new job opportunities at rate λ^E while employed and λ^U while unemployed.

There are two types of firms: posting (P) and renegotiating (R). Posting firms commit ex ante to a wage schedule and do not renegotiate with workers who receive an outside offer. They post wage premia; a firm that posts w pays the worker wa. Renegotiating firms bargain with workers, both at the beginning of the employment relationship and when one of the parties receives a credible outside offer (Cahuc et al., 2006). Renegotiation is costless and occurs only by mutual consent. We follow the prior literature in assuming that worker-firm bargaining at renegotiating firms follows the infinite-horizon alternating offer game in Rubinstein (1982). When an employed worker receives an outside offer, the incumbent and outside firm engage in competition over that worker. By definition, posting firms do not adjust their bids. Then, if the winning firm is a renegotiating firm, the worker and firm bargain over wages. The worker uses the maximum value she could have obtained at the losing firm as her outside option.[11]

We close the model in Appendix B. We assume that, when deciding whether to post a vacancy, firms draw both θ and a vacancy type (P or R). This is somewhat simpler from the setup in Flinn and Mullins (2017), and more suited to the counterfactuals we consider in Section 7.

[10] It is standard to assume that the value of non-employment is proportional to ability. This is reasonable given that most unemployment benefits are based, at least in part, on a worker's wages. However, a direct implication of this assumption is that all workers have the same reservation firm type when unemployed.

[11] If the losing bid came from a renegotiating firm, this is the total value that would have been produced in the match. If the losing bid came from a posting firm, this is the total value the worker would have received at that firm. This may not be equal to the total value of the match.

2.2 Value Functions

We next derive the value functions for workers who: (1) work at renegotiating firms, (2) work at posting firms, or (3) are unemployed. Workers and renegotiating firms bargain over how to split the total surplus produced in the match, $T_R(a\theta)$. The value the worker receives, $V_R(a, \theta, \overline{w})$, depends both on the productivity of the match and the last outside option she used for bargaining, $\bar{w}$. A worker at a posting firm earning w obtains value $V_P(w)$. V_U denotes the value function for an unemployed worker.

It is useful to first state a result from Flinn and Mullins (2017):

Lemma 1. *A worker who receives the total surplus created by the match θ at a renegotiating firm (type R) has the same value as a worker earning θ at a posting firm (type P). That is,*

$$T_R(a\theta) = V_R(a\theta, a\theta) = V_P(a\theta)$$

Proof. See Appendix B. □

The intuition behind this is simple: once a worker at a renegotiating firm receives the full surplus of her match, her wage can no longer adjust at that firm. She will receive the full surplus of the match whenever her last bargaining position was θ, the match productivity.[12] The worker's mobility decisions will be the same as those of a worker at a posting firm earning θ; like that worker, her wage will not adjust at the current firm. This result is useful because it means there is a sufficient statistic that governs workers' mobility patterns and wage growth: the maximum wage they could earn at a firm. At posting firms this is simply the offered wage; at renegotiating firms, this is θ.

Renegotiating Firm Because we have assumed transferable utility, the total surplus of a match between a worker and a renegotiating firm is the sum of the value to the worker and to the firm. At a renegotiating firm of productivity θ this total surplus is:

$$\begin{aligned} \rho T_R(\theta) &= \theta + \underbrace{\delta\left(V_U - T_R(\theta)\right)}_{\text{unemployment}} + \\ & \underbrace{\lambda^E p_R \int \beta\left[T_R(x) - T_R(\theta)\right]^+ dF_\theta(x)}_{\text{poached by renegotiating firm}} + \underbrace{\lambda^E(1-p_R) \int \left[V_P(x) - T_R(\theta)\right]^+ d\Phi(x)}_{\text{poached by posting firm}} \end{aligned} \tag{1}$$

[12]This is a direct implication of the bargaining protocol. It also implies the worker's wages satisfy $\omega(\theta, \theta) = \theta$.

The first term is the output of the match, θ. With probability δ, the worker loses her job. The third and fourth terms measure the additional surplus the worker gets if she meets a firm that outbids her current firm.[13] We use F_θ and Φ to refer to the distributions of offers from renegotiating and posting firms, respectively. The worker receives an offer from a renegotiating firm with probability $\lambda^E p_R$ and an offer from a posting firm with probability $\lambda^E(1-p_R)$. She moves to that firm if (1) the outside firm is a more productive renegotiating firm or if (2) the outside firm is a posting firm that offers her more than she produces with her current firm. If she moves to a new renegotiating firm, she uses the value produced in her current match as her outside option and obtains a fraction β of the rents produced in the new match $(T_R(x) - T_R(\theta))$. We refer to the parameter β as workers' bargaining power. It measures the proportion of rents a worker is able to obtain in bargaining. The free entry condition ensures that if the worker is poached, the value to the incumbent firm is zero.

The firm and worker bargain over how to split this total surplus $T_R(\theta)$. The worker's value function depends not only on the total value produced but on her last bargaining position. If her last offer came from a renegotiating firm, this is

$$V_R(\theta, x) \quad = \quad \underbrace{T_R(x)}_{\text{last outside option}} + \beta \underbrace{(T_R(\theta) - T_R(x))}_{\text{surplus}}$$

Similarly, if her last offer came from a posting firm, this is:[14]

$$V_R(\theta, x) \quad = \quad \underbrace{V_P(\bar{w})}_{\text{last outside option}} + \beta \underbrace{(T_R(\theta) - V_P(x))}_{\text{surplus}}$$

In order to achieve this split, workers and firms agree on wages $\omega(\theta, x)$. The worker's value function depends only on her current wages $(\omega(\theta, x))$, not the total value produced by the match. With probability δ she is unemployed next period. With probability λ^E she receives an outside offer. With probability $\lambda^E(1-p_R)$ that offer is from a posting firm. If the outside offer comes from a firm that is sufficiently good, she is poached. For more moderate values, she renegotiates her wage at

[13] We use the notation $[a]^+ = \max\{a, 0\}$.

[14] This expression illustrates the symmetric relationship between the importance of rents and options mentioned in the introduction. When $T_R(\theta) = \theta$ and $V_R(\theta, w) = w$, $w = (1-\beta)\bar{w} + \beta\theta$. If rents pass through at a rate β, options pass through at a rate $1-\beta$.

her current firm. Her value function is given by:

$$
\begin{aligned}
V_R(\theta,x) \quad = \quad & \{\omega(\theta,x)+\delta V_U \\
& \lambda^E p_R \left(\underbrace{\int_\theta^\infty \left[T_R(\theta)+\beta\left(T_R(x)-T_R(\theta)\right)\right] dF_\theta(x)}_{\text{poached}} + \underbrace{\int_x^\theta \left[\left(T_R(x)+\beta\left(T_R(\theta)-T_R(x)\right)\right] dF_\theta(x)}_{\text{renegotiate}} \right) + \\
& \lambda^E(1-p_R) \left(\underbrace{\int_\theta^\infty V_P(x) d\Phi(x)}_{\text{poached}} + \underbrace{\int_x^\theta \left[V_P(x)+\beta\left(T_R(\theta)-V_P(x)\right)\right] d\Phi(x)}_{\text{renegotiate}} \right) \}/ \\
& \left(\rho+\delta+\lambda^E p_R(1-F_\theta(x)))+\lambda(1-p_R)(1-\Phi(x))\right)
\end{aligned}
$$

Posting Firm The value function of a worker at a posting firm depends only on the wage she receives at that firm:

$$
\begin{aligned}
\rho V_P(w) \quad = \quad & w+\underbrace{\delta\left(V_U-V_P(w)\right)}_{\text{unemployment}} + \\
& \underbrace{\lambda^E p_R \int \beta\left[T_R(x)-V_P(w)\right]^+ dF_\theta(x)}_{\text{poached by renegotiating firm}} + \underbrace{\lambda^E(1-p_R)\int \left[V_P(x)-V_P(w)\right]^+ d\Phi(x)}_{\text{poached by posting firm}}
\end{aligned} \tag{2}
$$

The first term is her wage, w. With probability δ the match is dissolved and the worker becomes unemployed. She receives an offer from a renegotiating (posting) firm with probability λp_R^E (posting firm: $\lambda^E(1-p_R)$). She is poached if (1) the outside firm is a more productive renegotiating firm or (2) the outside firm is a posting firm that offers her more her current firm. The β in the third term reflects the fact that if she moves to a renegotiating firm, she will obtain a fraction β of the rents produced by that match and will use her current value function $V_P(w)$ as her fallback option.

Unemployment The value function for an unemployed worker, V_U, also has a simple recursive formula:

$$
\rho V_U = \underbrace{b}_{\text{benefits}} + \underbrace{\lambda^U p_R \int \beta\left[T_R(x)-V_U\right]^+ dF_\theta(x)}_{\text{offer from renegotiating firm}} + \underbrace{\lambda^U(1-p_R)\int \left[V_N(x)-V_U\right]^+ d\Phi(x)}_{\text{offer from posting firm}}
$$

If a worker is unemployed, she receives benefits b this period. She receives an offer from a renegotiating firm with probability $\lambda^U p_R$. Her wage upon accepting employment at that firm is set such that she receives V_U plus a fraction, β, of the match surplus $(T_R(x) - V_U)$. She receives an offer from a posting firm with probability $\lambda^U(1 - p_R)$.

2.3 Reduced Form Predictions

The continuous time model corresponds to the limit of an analogous discrete time model. To derive the reduced form predictions, we consider what happens in a period of length $t = 1$. In a slight abuse of notation, we use h_θ to refer to the combined distribution of match productivities θ and wage offers w. We also assume that λ is small enough so that the probability of receiving multiple offers in this period of time is negligible. This gives us the following results:

Claim 2. The probability a worker makes a job-to-job transition this period is

$$\Pr(\text{move}) \quad \approx \quad \underbrace{\lambda^E e^{-\lambda^E}}_{\text{arrival rate}} \underbrace{\left[\int_\theta^\infty h_\theta(\theta')d\theta'\right]}_{\text{prob better firm}} = \tilde{\lambda}\tilde{H}(\theta)$$

where $\tilde{H}(\theta) = (1 - H_\theta(\theta))$ and θ is her current employer's type.

Proof. See Appendix B for details. □

The intuition behind Claim 2 is simple: the probability an individual makes a job-to-job transition this period is simply the probability she receives an offer multiplied by the probability that offer came from a firm that was willing to pay her more than her current firm would match. By construction, the probability a worker at a posting firm sees a wage change is identical to the probability she makes a job-to-job transition. However, workers at renegotiating firms may see wage changes, even if they do not move firms.

Claim 3. The probability a worker at a renegotiating firm of type θ sees a wage change is:

$$\Pr(\text{wage change}) \quad \approx \quad \underbrace{\lambda^E e^{-\lambda^E}}_{\text{arrival rate}} \underbrace{\left[\int_q^\infty h_\theta(\theta')d\theta'\right]}_{\text{prob better offer}} = \tilde{\lambda}\tilde{H}(w) > \tilde{\lambda}\tilde{H}(\theta)$$

where q is her last bargaining position.

Proof. See Appendix B for details. □

The probability the worker sees a wage change is the probability she receives an offer, multiplied by the probability that the offer came from a firm that is better than the last offer she used in renegotiation. This is always weakly higher than the probability of making a job-to-job transition because the outside firm doesn't have to be willing to outbid her current firm.

The two predictions are summarized in Figure 1. Offers are ranked according to the maximum value a worker could receive. When a worker at a renegotiating firm receives an outside offer (Panel A), one of three things will occur:

1. **Worker is Poached:** If the outside offer is sufficiently good, the outside firm will 'win' during competition with the incumbent firm. This happens if:

$$\begin{aligned} T(\theta') &> T(\theta) \\ V_P(w) &> T(\theta) \end{aligned}$$

2. **Wage Renegotiation:** If the outside firm loses to the incumbent firm, but would have offered the worker more than her last outside option, the worker will renegotiate her wage with her current firm. This will happen if:

$$\begin{aligned} T(\theta') &\in [T(w'), T(\theta)] \\ V_P(w) &\in [T(w'), T(\theta)] \end{aligned}$$

   where w' is the worker's last bargaining position.

3. **No Change:** If neither of these conditions is met, the worker stays at her current firm and continues to earn her current wage. Renegotiation only occurs by mutual consent; the worker will not initiate wage renegotiation if it would lead to a wage cut.

Panel B shows that, for workers at posting firms, outside offers can only lead to job-to-job transitions.

This figure shows the key empirical predictions. We should see a positive relationship between our measure of outside options and both job-to-job mobility and wage growth. We should also see effects on earnings through a greater portion of the outside options distribution. This is because outside offers only need to dominate whatever a worker last used for negotiation, not the maximum wage that firm would be willing to pay.

2.4 Information Transmission Through Networks

Standard search models assume that all workers in a given labor market face the same job arrival rate. Suppose instead that workers learn about job opportunities through both public sources—which are common to all workers—and through their own social networks. Search through both sources is undirected.

We can then decompose the probability a worker receives an outside offer into two components:

$$\lambda^P + \alpha \underbrace{\int s(x)v(x)dx}_{\text{social ties}} \tag{3}$$

where the first term, λ^P, is the arrival rate of offers through public sources. The second term measures the arrival of offers through networks. We assume that the probability a worker hears about one of the $v(x)$ offers at firms of type x scales with the number of people they know at that firm, $s(x)$.[15] The parameter α is the joint probability of learning about an opening through social ties and receiving an offer.

The probability a worker makes a job-to-job transition or sees a wage change depends on the probability the worker receives a 'good enough' offer. The probability a worker receives an offer better than θ has a similar expression:

$$\lambda^P \underbrace{\int_\theta^\infty p(x)dx}_{\text{public sources}} + \alpha \underbrace{\int_\theta^\infty s(x)v(x)dx}_{\text{social ties}} \tag{4}$$

In section 5 we test whether individuals are more likely to move or earn more in periods when they were more likely to receive an outside offer through one of their connections. Our reduced form measure of an individual's outside options, Ω_{it}, is based on the expression for arrival rates in equation 3. We construct

$$\Omega_{it} = \underbrace{\sum_j \text{Share Coworkers}_{ijt}}_{\text{coworker network}} \times \underbrace{s_{jt}}_{\text{firm demand}} \times \underbrace{\omega_{jt}}_{\text{firm quality}}$$

We weight firm-specific measures of labor demand (s_{jt}) by the share of an individual's former coworkers' at that firm. Our baseline specification sets $\omega_{jt} = 1 \ \forall j, t$. In this case, Ω_{it} is simply a proxy for the arrival rate of offers through the individual's social network. In some specifications, we attempt to measure the probability a worker received a 'good' outside offer by weighting changes in firm demand by the different measures of firm quality. We provide more details on how we construct

[15] We define $v(x)$ such that $\int v(x)dx = 1$. We assume that workers do not strategically pick jobs in order to gain access to better social networks, and that firms do not hire workers to take advantage of their network. In the empirical work we both control for time-invariant individual heterogeneity—which could include a propensity to strategically move to new firms—and for the number of connections in an individual's network.

this measure in Section 4.3.

3 Setting and Data

3.1 Institutional Setting

Several features of the Danish labor market make it a good setting for studying the relationship between outside options and wages. First, job-to-job and occupational mobility rates are high by European standards, and are comparable to those in the United States (Botero et al., 2004; Groes et al., 2014).[16] This flexibility is the result of the Danish "flexicurity" system, which combines low firing costs with a generous social safety net (Andersen and Svarer, 2007).

Second, while wages in Denmark were historically set by sector-level bargaining agreements, wages today are mostly set at the firm level or are negotiated between individual workers and firms (Dahl et al., 2013).[17] Further, private sector collective agreements do not typically cover managers, executives, or university graduates. Instead, these workers negotiate employment conditions individually. Unions still play a significant role in setting sector-level minimum wages, which generally apply to inexperienced or new workers. Denmark has no national minimum wage. Danish unions are also very important in organizing unemployment insurance; most unemployment insurance funds are associated with one of the unions.

One key difference between the American and Danish labor markets is that multiple job-holding is common in Denmark, as it is in most Nordic countries. In 2015 eight percent of Danish workers worked for more than one employer (Pouliakas, 2017). The incidence over an eight year period is significantly higher. Throughout our analysis we focus on the population of single job-holders.

3.2 Primary Data Sources

We combine three types of administrative data: (1) a monthly employer-employee register, (2) person-level demographic registers, and (3) firm-level Customs and Trade registers. We provide more information on the data in Appendix C.

[16] Appendix Figure A1 shows that relative to other OECD countries (those marked in blue have readily available linked employee-employer registers), hiring and separation rates are high.

[17] During the 1990s there was a shift towards the use of collective agreements that specified only general working conditions: working hours, rules regarding hours flexibility, and minimum wages. There are four wage-setting regimes in Denmark. In the standard rate system, agreements determine wages for most workers in a sector. In the minimum wage and minimum pay systems, these agreements only specify wage floors; only very inexperienced workers earn the minimum rate. Under the fourth system, there are no centrally bargained minimum wage rates. Rather, wages are negotiated at the plant or firm level. There is scope for individual-firm bargaining in all but the fourth system. See Dahl et al. (2013) and "The foundation and dynamics of the Danish labour market" for more information.

Most of our data come from a new monthly employer-employee database, known as the BFL (or e-Income) database. Danish firms must report individual hours and earnings to the Danish Customs and Tax Authority on a monthly basis. The BFL register contains the data from these reports. In addition to information on hours and earnings, the data also contain six-digit industry and occupation codes. We have data from January 2008, the start of the register, through March 2016. To construct coworker networks for the first three years of our sample, we supplement these data with a separate monthly employer-employee register (MIA), which contains monthly information on place of employment from 1999-2008.

The data are well-suited for our analysis because they contain high-frequency (monthly) earnings data and because, unlike most employer-employee datasets, they contain firm-reported measures of hours worked. The hours data allow us to examine whether changes in monthly earnings are driven by changes in hours worked, or changes in hourly earnings.

We use unique person identifiers to link the employer-employee data to demographic registers that contain information on age, sex, country of origin, education, and household characteristics. We collapse the education codes in our registers to nine broad level codes and eleven broad field codes following the International Standard Classification of Education (ISCED) codes. We use these codes to distinguish between workers with the same level of education but different skills.

We use firms' unique identifiers to link our employer-employee data to the Danish Foreign Trade Statistics Register. For each firm and month between January 2004 and December 2015 we have the value (in Danish Kroner) of imports and exports disaggregated by product and by origin (imports) or destination (exports). The original data are reported at the eight-digit Combined Nomenclature level; we aggregate flows to the six-digit Harmonized System. We use these data in section 5.6 when we consider measures of outside options based on world demand for each firm's products.

3.3 Descriptive Statistics and Sample Restrictions

Workers Column 1 of Table 1 provides descriptive information on the set of workers who appear at least once in the BFL data between January 2008 and March 2016. The average worker in our sample (weighted by months in sample) is nearly forty years old and has annualized earnings of around $40,000, before taxes. Nearly half of individuals are married or in a registered partnership; more are cohabiting. About a third of the workers have a college degree.

Before constructing coworker networks we restrict the sample to Danish citizens who work in firms with between 2 and 1000 employees. We exclude non-Danish citizens both because our demographic data (especially our education measures) are most complete for Danish citizens and because our data include all employees of Danish firms, including those not residing in Denmark. We exclude connections formed in firms with more than one thousand employees because it is unlikely that an individual knows all of her coworkers in a very large firm. This type of restriction is common in the networks literature.[18]

We impose two additional restrictions to generate our regression sample. First, we focus on prime-age workers between the ages of 25 and 60. Very young (under 25) workers will not have had enough time in the labor market to develop a network; older (over 60) workers are likely close to retirement. Second, we focus attention on workers who are, over the sample period, single job holders. This is a relatively significant restriction given the prevalence of multiple job-holding in Denmark. However, this restriction allows us to remove a significant portion of part-time workers whose earnings fluctuations likely reflect both changes in hourly wages and changes in hours worked. Further, the theoretical framework is about single job holders. Column 3 of Table 1 shows the impact of these restrictions. These workers are more likely to be married or in a couple. They also have higher average annualized earnings because they are more likely to be working full-time.

Firms Workers in our sample are spread across 352,010 distinct firms (tax identifiers). Column 1 of Table 2 shows that the average firm has eleven employees, though there is substantial variance: the standard deviation is over two hundred. Most firms have a single establishment and over half are located in one of two regions: the Capital Region and Central Denmark. These regions contain Copenhagen and Aarhus, Denmark's first and second largest cities, respectively. Column 2 shows that most firms fall within the network sample: they have between two and one thousand employees, on average, throughout our sample period. Most (>99%) of the firms that are excluded from our network sample are single-employee firms. There is much less variation in firm size within the network sample. The average firm has eight employees and the standard deviation is thirty-two.

Column 3 shows that most firms are neither importers nor exporters; only fourteen percent of firms appear in the trade register. However, because the average firm in this register is double the size of the average firm in the full sample, these firms cover a substantial portion of employment. Most exporting firms export a single product. Because our firm size threshold is generous, most firms in the trade register fall within the network sample; less than fifteen percent are excluded

[18] For instance, Hensvik and Skans (2016) only consider firms with less than 500 employees and Eliason et al. (2017) only consider connections formed in firms of fewer than 100 employees. Saygin et al. (2014) include connections formed in all firms with fewer than 3000 workers but consider a smaller set of workers: those involved in mass layoffs. Glitz and Vejlin (2018) include all of an individual's coworkers from the prior ten years but focus only on workers who were hired in a given year. A different set of papers has defined an individual's network by her set of same-citizenship peers (see, e.g. Dustmann et al., 2015).

(Column 3 versus Column 4). Most firms are located in the capital region and Central Denmark; the fraction of employment located in these two regions is even greater. Figure A2 presents a map of the five administrative regions.

3.4 Earnings Outcomes

We examine the impact of changes in workers' outside options on changes in five measures of earnings. The monthly register data contain two measures of monthly earnings: a broad measure, which includes income derived from benefits (e.g. contributions to retirement accounts or fringe benefits) and a narrow measure which captures post-mandatory-contribution take-home pay. We look at log changes in both measures. However, we prefer the broad measure because it does not respond to changes in ATP (mandatory pension) contributions that arise due to changes in legislation or changes in hours worked.[19] Further, using the broad measure allows us to account for the fact that some workers may to with their employer over retirement contributions or fringe benefits. Our third measure is log hourly earnings. While both earnings measures are available for all workers, hours are imputed for roughly a quarter of the sample. We focus on the subset of observations with firm-reported hours.

Finally, we use the panel component of our data to construct the fourth and fifth measures: "bonus pay" and "base pay". We identify bonuses by looking for one-month increases in earnings that are followed by a decrease of approximately the same magnitude. We define base pay as difference between total monthly earnings and any bonuses. We provide more details on how we construct the earnings measures in Appendix C.

4 Empirical Strategy

Our empirical strategy exploits the fact that individuals often learn about new job opportunities through their former coworkers. The logic is simple: individuals should be more likely to hear about job opportunities when their former coworkers' firms are expanding more relative to other periods.

4.1 Graphical Illustration of Empirical Strategy

The strategy can be explained in three pictures. First, Figure 2 shows how this approach identifies variation in outside options within an individual over time. Panel A shows the individual's network:

[19] Individuals pay different rates based on which of four bins their monthly hours falls in: 0-38, 39-77, 78-116, and 117+ hours.

each collection of dots represents a firm and each dot represents a worker. Each blue dot represents one of the individual's former coworkers. Our identification strategy relies on the assumption that the worker is less likely to hear about job openings in Panel B—where she does not know anyone at the expanding firm (displayed in red)—than in Panel C.

In some of our analysis we exploit variation in information about outside labor market opportunities between workers in the same firm (or firm and occupation). Workers who join a firm at the same time will have different networks if they moved to that firm from different firms. Figure 3 shows that within-firm variation also arises due to differences in firm tenure. The figure shows two workers—marked in blue and purple—at firm A in period 1. In the next period, the purple worker moves to firm C, and is replaced by the red worker from firm B. In the final period, firm C expands. Our within-firm analysis relies on the fact that the red worker at firm A is less likely to hear about this expansion than the blue worker. This is because the blue worker has a shared history with someone at firm C. Panel B shows that it may not be the case that the worker with longer tenure at the firm has access to 'better' information.

Figure 4 illustrates how we divide up the monthly panel to create (1) coworker networks and (2) time-specific shocks. In each period we use the prior thirty-six months to generate an individual's network. We then look at how firm-specific shocks between t=0 and t=1, weighted by this network, translate into mobility or wage growth in the same period. Specifically, we examine whether an individual is at a new firm or earns more in period 1 than they did in period 0.

4.2 Coworker Networks

Individual i's coworker network in month t consists of all workers she worked with in the prior three years who are now at different firms.[20] There are two key restrictions. First, we only include connections in firms with between 2 and 1000 people. In large firms, it is unlikely that a worker knows, or shares information with, all of their former coworkers. Second, we exclude connections that were formed more than three years ago both because older connections are likely to be less informative and because, without a fixed window, network size or quality would vary mechanically over the sample window. We examine the robustness to these restrictions in Section 5.7.

[20] Specifically, for each month t, we construct the bipartite adjacency matrix A^t where $A^t_{ij} = 1$ whenever i and j worked together (at the same firm, at the same time) in the previous thirty-six months and $A^t_{ij} = 0$ otherwise. We can then rewrite our measure of outside options using network notation:

$$\sum_{c \in N} \frac{A_{ict}}{\sum_{n \in N} A_{int}} \times s^{+}_{jt} \times \omega_{\psi(c,t),t}$$

Note that we only consider first-degree connections.

We also remove connections who are working at firms individual i worked at in the past three years so that the network does not vary mechanically with mobility. Specifically, if a worker moves from firm A to firm B, we do not include her former coworkers at firm A in the network, unless they move to another firm. This is important: if we did not do this workers who switched firms would, mechanically, see a large increase in network size. Our measure of outside options for that worker would also be heavily weighted towards the firm they just left.

Column 1 of Table 3 shows that, on average, workers have 156 connections, which connect them to 60 distinct firms. However, the distribution is very skewed and the median worker has only 60 former coworkers. Columns 2 and 3 compare the networks of male and female workers and show that women do not appear to have significantly weaker networks by any metric: the number of connections, the number of industries, or the average value added per worker at a connection's firm. However, because Danish firms—like firms in most countries—are somewhat segregated by gender, women's networks primarily consist of other women (Card et al., 2016*a*; Hellerstein et al., 2008).

4.3 Measuring Outside Options

Our measure of outside options is motivated by the theoretical model described in Section 2. We create individual- and time-specific measures of outside options by weighting time-varying measures of firm-specific labor demand by each individual's coworker network. For each individual i and month t we construct:

$$\Omega_{it} = \sum_j \text{Share Coworkers}_{ijt} \times \underbrace{s_{jt}}_{\text{firm demand}} \times \underbrace{\omega_{jt}}_{\text{firm quality}}$$

where s_{jt} is a measure of firm labor demand and the ω_{jt} are firm-quality weights.[21]

Our baseline measure uses the number of new positions at an individual's former coworkers' firms as the measure of firm demand, and weights all firms equally: $s_{jt} = (E_{j,t} - E_{j,t-1})^+$ and $\omega_{jt} = 1 \ \forall j, t$. We focus on the number of new positions, rather than the overall number of hires, which reflects changes in both labor demand and churn. To prevent a mechanical correlation between the change in employment at these firms and an individual's own job-to-job mobility decisions, we use a "leave-out" version where we do not include new positions created for individual i.

[21] Because the weighting functions vary by individual (leading our measure of outside options to vary across workers within a firm), this is somewhat different from a standard "Bartik"-style instrument. There is an ongoing debate on the identifying assumptions behind these instruments (see, e.g. Borusyak et al., 2018; Goldsmith-Pinkham et al., 2018; Adão et al., 2018; Jaeger et al., 2018). Our identifying assumption, described below, is similar to the "exogeneity of shocks" assumption in Borusyak et al. (2018). In particular, we do not require that the shares—the coworker networks—be randomly assigned.

While our baseline specification does not use firm quality weights, the model in Section 2 suggests that the probability an individual moves or renegotiates scales with both the total number of offers and the probability that an offer comes from a sufficiently good firm. In practice, it is difficult to determine which firms are likely to be more attractive because workers may have preferences over non-wage characteristics (Sorkin, 2018) and because firm wage premia may change in response to firm- or market-shocks. In specifications presented in the Appendix we weight positions by the mean wage at the firm, scaled by average wages: $\omega_{jt} = \frac{w_j}{\bar{w}}$. In Appendix Section D.2 we consider the impact of new positions at more and less productive firms, where productivity is measured using value added per worker.

Within an individual, variation in Ω_{it} is driven by changes in firm demand, not changes in network composition. Table A4 shows that the number and characteristics of individuals in a worker's coworker network are highly autocorrelated, with autocorrelations above .9, even after a year. The number of hires and new positions at a firm are significantly less autocorrelated.

In Section 5.7 we show that our results are robust to different definitions of (1) an individual's coworker network, (2) firm demand s_{jt} and (3) firm weights ω_{jt}. In section 5.6 we consider measures of s_{jt} based on world demand for each firm's products.

4.4 Estimating Equations and Identifying Assumptions

The main estimating equation is:

$$y_{it} = \gamma \Omega_{it} + X_{it} + c_{it} + \alpha_i + \alpha_{kt} + \epsilon_{it} \tag{5}$$

where y_{it} is either an indicator for mobility or one of the five measures of wage growth described in Section 3.4. Our measure of an individual's outside options is Ω_{it} and the key coefficient is γ. We control for c_{it}, the number of coworkers in an individual's network, and for individual (α_i) and industry-by-time (α_{kt}) fixed effects. The individual fixed effects allow us to account for non-random sorting of individuals into firms and networks. Our estimates exploit month-to-month changes in labor demand at the firms in an individual's network. The industry-by-month controls absorb variation in demand for specific skills. In our theoretical framework, these changes in demand correspond to variation in the arrival rate of offers through public sources, λ^P. We two-way cluster our standard errors at the person and firm level to account for individual differences in mobility preference and for correlation between the wage growth of employees within a firm.[22]

[22] A future version of this paper will use Conley standard errors to account for correlation in wage growth across the network. This is difficult to implement given the computing resources available, as there are $N \times T$ distinct

The identifying assumption is that, conditional on the included covariates, changes in labor demand at an individual's former coworkers' current firms are uncorrelated with unobserved determinants of mobility or wage growth: $E[\epsilon_{it}\Omega_{i,t}|c_{it},\alpha_i,\alpha_{kt}] = 0$. The primary concern is that there are unobserved changes in the demand for a worker's skill that are correlated with Ω_{it} but not captured by our industry-by-time controls.[23] These changes in demand would lead to both mobility and wage growth in either a competitive model or in a bargaining model. In the next section we perform several distinct empirical exercises that support the identifying assumption.

5 Impacts on Mobility and Earnings

This section presents the main reduced form results. We find that in periods when a worker's former coworkers' firms are expanding that worker is more likely to make a job-to-job transition or to see an earnings gain, even if she does not move. Further, while the mobility results are driven by observations with values of Ω_{it} in the top decile, workers with more moderate values of Ω_{it} also see earnings gains. These are exactly the empirical predictions presented in Figure 1.

The results suggest that firms pay workers less than their marginal product and that workers learn about job opportunities through their former coworkers. Some workers take these new opportunities; others use this information to renegotiate their wages at their current firm. The findings match the predictions of the on-the-job search and bargaining model in Section 2, but are inconsistent with both (1) the frictionless neoclassical model and (2) models of wage-setting where firms post wages and commit not to renegotiate (e.g. monopsony models).

5.1 Graphical Evidence Supporting Theoretical Predictions

We start by presenting non-parametric evidence on the relationship between Ω_{it} and job-to-job mobility decisions and earnings growth. We find empirical support for both theoretical predictions depicted in Figure 1: (1) changes in outside options positively impact both mobility and wage growth and (2) larger changes in outside options are necessary to induce a job-to-job transition than to induce a wage change.

Mobility The top-left panel of Figure 5 plots the raw probability an individual makes a job-to-job transition in a given period by the quality of their outside options. The probability an individual

networks, each of which is an $1 \times N$ matrix (Conley, 1999; Conley and Topa, 2003). Here N is over 1 million, and T is nearly 100.

[23] In the theoretical model described in Section 2, these correspond to unobserved changes in the arrival of offers through public sources, which are correlated with Ω_{it}.

makes any transition is low because of the high frequency (monthly) nature of our data.[24] However, there is a clear positive relationship between an individual's outside options (as measured by Ω_{it}) and their probability of making a job-to-job transition. This is primarily driven by high-value options: those in the top decile. This is consistent with the model in Section 2, where an outside offer only induces a job-to-job transition if the outside firm is willing to outbid the incumbent firm.

The top right panel shows that this increased job-to-job mobility is the result of individuals moving to firms where one of their former coworkers works. Each job-to-job transition can be divided into one of three categories based on whether the move is to (1) a coworker-connected firm, (2) an unconnected but in-sample firm, or (3) an out-of-sample firm. Coworker-connected firms are firms where one of the individual's former coworkers currently works. Out-of-sample firms are firms with more than one thousand employees. The null impact on unconnected firms is consistent with information transmission–without a connection one of these firms, the individual is no more likely to hear about the openings than anyone else. The null impact on out-of-sample firms suggests that there is little selection in or out of our sample.

These patterns do not emerge simply because highly mobile workers have stronger networks and larger values of Ω_{it}. The bottom panel of Figure 5 shows that the main results hold after partialling out individual fixed effects and industry-by-time fixed effects, and controlling for network size. The figure on the left shows a clear positive relationship between the residual probability an individual makes a job-to-job transition and the residualized outside options. The bottom right panel shows that the results are driven by moves to connected firms. There is no such relationship between the residualized outside options and the residual probability an individual leaves their firm and is not immediately employed at another firm.

Earnings Figure 6 shows that there is also a positive relationship between an individual's change in log earnings and Ω_{it}. As before we regress both the outcome—changes in log monthly earnings—and Ω_{it} on individual and industry-by-time fixed effects and on a linear control for the number of coworkers in an individual's network. We plot the mean residuals of our earnings outcomes by percentile of the residual options distribution. The top panel focuses on log wages; the bottom panel focuses on log earnings. The data show a clear positive relationship between earnings changes and outside options in both cases. As with mobility, only large shocks are important; there is no impact on observations below the sixtieth percentile.

[24]This probability is around 2% in our sample. This is similar to the 1.5-2% number reported in Bagger et al. (2014*b*). About half of these transitions are job-to-job transitions, which do not include an intervening spell of unemployment or non-employment.

The theoretical model in Section 2 suggests that larger changes in outside options are necessary to induce a job-to-job transition than to induce a wage change. This is because, in order to induce a worker to switch jobs, the outside firm has to beat the *maximum* the incumbent firm would be willing to pay. In order to induce a wage change, the outside firm only needs to beat the worker's last outside offer. A comparison of Figures 5 and 6 shows that, while only the top decile of (residualized) Ω_{it} impacts job-to-job mobility, values of Ω_{it} in the top three deciles lead to changes in earnings. This is exactly the pattern predicted by the model in Section 2 (see Figure 1).

The primary concern with the interpretation of our estimates is that they might reflect unobserved changes in demand for a worker's skills rather than the pure effect of information about outside options, holding skill demand constant. Appendix Figure A3 shows that the main graphs are unchanged by the addition of occupation-by-time fixed effects.

5.2 Basic Results

We next turn to quantifying the impacts on mobility and wage growth. To scale our results, we examine how a ten unit change in Ω_{it} (approximately one standard deviation) affects the percent chance an individual makes a job-to-job transition or affects the average worker's annual earnings.

5.2.1 Mobility

Panel A of Figure 7 displays estimates of γ from equation 5. The outcome variable is an indicator for whether the individual made a job-to-job transition. We scaled γ so it indicates the impact of a 10-unit increase in Ω_{it} on the percent chance an individual will make a job-to-job transition this month. The estimate at the far left is our preferred specification, which includes both individual and industry-by-time fixed effects, and controls for the number of connections an individual has. These covariates allows us to control for time-invariant differences in the quality of an individual's network and for the fact that some individuals are more mobile than others.

The baseline estimate at the far left indicates that a ten-unit change in Ω_{it} (roughly one standard deviation) leads to a 15% higher probability an individual will move to a new firm this month. This is consistent with the idea that individuals stay in contact with and discuss labor market opportunities with their former coworkers. When new vacancies arise at these former coworkers' firms, the worker is more likely to hear about the vacancy than the average worker. In some cases, the worker receives an offer and decides to move.

One way to assess whether the identifying assumption—that changes in Ω_{it} are uncorrelated with unobserved changes in demand for the worker's skill—is satisfied is to examine how our estimate of

γ changes when we add more detailed non-parametric controls for changes in demand of different skill groups (Altonji et al., 2005). If Ω_{it} reflected changes in demand for a worker's skill, we would expect γ to fall as we added more controls.

The remaining estimates presented in this figure show that our estimate is, in fact, stable when including different non-parametric controls for changes in demand for certain skill groups. The second specification adds time-varying demographic controls—indicators for whether the individual is married or has children—to our baseline specification. The third and fourth specifications use occupation-by-time and industry-by-occupation-by-time fixed effects instead of industry-by-time fixed effects. The stability of our estimates suggests the results are driven by information transmission through social networks, not unobserved demand shocks.

Table 4 presents the raw (unscaled) estimates of γ for each mobility outcome. The fact that the impact on job-to-job mobility is larger than the impact on whether a worker makes any transition (including to non-employment), suggests that some workers use information from their coworkers to avoid short periods of un- or non-employment. This result is consistent with prior work that has shown that newly unemployed or displaced workers use information from their former coworkers to find new employment (Saygin et al., 2014; Glitz and Vejlin, 2018).

The second and third rows show that the increase in job-to-job mobility is entirely driven by increases in moves to coworker-connected firms. There is only a small displacement effect: some individuals who would have moved anyway are more likely to move to a firm where they know a coworker, than to an unconnected firm. However, there is not a significant impact on whether the worker moves to an out-of-sample (i.e. large) firm.

5.2.2 Earnings

Panel B of Figure 7 displays estimates of γ from equation 5 where the outcome variable is the change in log monthly earnings.We have scaled γ so it indicates the impact of on a ten unit change in Ω_{it} on the average worker's annual earnings. The baseline estimate at the far left indicates that a ten unit change in Ω_{it} is associated with a \$50 increase in annual earnings.

As with job-to-job mobility, the estimates are stable across a number of specifications, including different non-parametric controls for changes in demand for different industry or occupation groups. This suggests that, to the extent that there may be omitted changes in demand for a worker's skill, they are not correlated with our measure Ω_{it}. The final column presents the baseline specification estimated on the sample of job-stayers, those who are at the same firm as in the prior month. Not surprisingly, the estimate is nearly identical to the baseline estimate at the far left. Our data are monthly; the vast majority of our sample ($\approx 99\%$) are job-stayers.

Table 5 displays estimates of γ from equation 5 for different earnings outcomes (rows) and for each specification presented in Figure 5 (columns). The fact that we find a larger impact on narrow earnings—which do not include benefits—than our baseline measure suggests that most of this increase is due to changes in take home pay. The third row shows that, among workers with non-imputed hours, there are changes in hours worked. We see significant impacts on both base pay and bonuses. Appendix Table A5 shows that we see similar impacts for job-stayers.

While a \$50 average increase may seem low, this estimate masks the fact that most workers do not see any earnings gains in a given month. If all of the gains were driven by individuals who were driven to renegotiate with their firm (and not gains by those would have renegotiated anyway), the impact on compliers' earnings would be

$$\frac{\beta_{\Delta \log y}}{\beta_{1\{\Delta \log y > 0\}}}$$

Focusing on full time workers, this implies an 11% increase in base pay. Because workers who know they will have a chance to renegotiate their wages with their employer are probably especially likely to seek out information about outside opportunities, this is likely an overestimate.[25]

5.3 Exploiting Within-Firm Differences in Information

Even within an occupation or industry-by-occupation group, there may be substantial variation in workers' skills. For instance, software engineers may differ in their knowledge of Python, Julia, or C and these skills may be valued by different firms. In some cases skill variation within industries or occupations may be the result of training received at certain sets of firms (e.g. learning how to format code a certain way). As a result, individuals with a shared work history may have skills that are similar in ways we cannot observe.

We can address the concern that our industry and occupation controls are not sufficient to absorb time-variation in the demand for workers' skills by adopting a within-firm identification strategy. As discussed in Section 4, workers within the same firm or firm-and-occupation group may have different networks due to differences in tenure at that firm and at other firms. Figure 8 presents estimates of γ that exploit this variation; Panels A and B present results for mobility and earnings, respectively. The first estimate in each panel presents the baseline specification. The second specification adds firm fixed effects; the third replaces the industry-by-time fixed effects with

[25] This is based on coefficients from the baseline regression. The specification with firm-occupation-time fixed effects yields a ratio of 13%.

firm-by-occupation-by-time fixed effects. While the standard errors increase, we cannot reject that the earnings and mobility results are the same as in our baseline specifications. The raw coefficients for both mobility and earnings are presented in Tables 4 and 5, respectively.

We present one additional specification for earnings, focusing on job stayers. The fact that we obtain similar estimates in this sample bolsters the case that earnings growth is the result of worker-initiated renegotiation. An alternative interpretation of our findings is that managers learn about the 'market value' of their employees and raise wages accordingly. This story inconsistent with our finding that, within a firm and occupation, job stayers with more ties to coworkers at expanding firms see larger wage gains. This specification also addresses the concern that Ω_{it} is correlated with unobserved time-specific shocks to an individual's ability at their current firm. Table A5 presents the full set of estimates for job-stayers.

5.4 Exploiting Different Groups of Coworkers

We can provide further evidence that our results are driven by changes in workers' information about their outside opportunities by decomposing Ω_{it} into the portions coming from different subsets of coworkers. Some subsets of coworkers are more likely to be sources of information than others.

5.4.1 Same- and Different-Region Coworkers

Our first test is based on the geographic location of an individual's former colleagues. We would expect an individual's same-region coworkers to be a more valuable source of information for two reasons. First, individuals are more likely to be in contact with their former colleagues who work in the same geographic area. Second, assuming there are costs to moving, individuals are more likely to obtain *actionable* information from their same-region coworkers: information about jobs they would likely take. In both cases we would expect changes in labor demand at an individual's nearby coworkers' firms to matter more. By contrast, if our estimates reflected changes in demand for a worker's skill, both sets of coworkers would be roughly equally valuable.

We run regressions of the form

$$y_{it} = \gamma^{\text{IN}}\Omega_{it}^{\text{IN}} + \gamma^{\text{OUT}}\Omega_{it}^{\text{OUT}} + c_{it}^{\text{IN}} + c_{it}^{\text{OUT}} + \alpha_{kt} + \alpha_i + \epsilon_{it} \tag{6}$$

where Ω_{it}^{IN} and Ω_{it}^{OUT} are based on an individual's same-region or different-region former coworkers. These networks are based on the five Danish administrative regions shown in the map in Figure A2. Workers are assigned to regions based on the location of the firm they worked for in the prior period. Most workers live in the same region in which they work. Note that, by design, individuals without former coworkers in both their own region and in other regions are excluded. This primarily excludes individuals with very few connections.

We report the results in Figure 9. Panel A reports estimates of $\{\gamma^{\text{IN}}, \gamma^{\text{OUT}}\}$ from regressions where the dependent variable is an indicator for whether the worker made a job-to-job transition; Panel B reports analogous results from regressions where the dependent variable is the change in log monthly earnings. Each regression controls for the number of connections the worker has in the same region (c_{it}^{IN}) and in other regions (c_{it}^{OUT}), and for individual and industry-by-time fixed effects.

Both panels clearly show that changes in demand at a worker's same-region coworkers' firms have a significant and positive impact on whether the worker moves to a new firm or experiences earnings growth. New positions at the individual's different-region coworkers' firms have a much smaller effect. For each outcome, we can soundly reject equality of the two coefficients. This is exactly what we expect if individuals are more likely to lose contact with their former coworkers who move to, or start working in, different regions. If, by contrast, Ω_{it} simply reflected changes in the demand for a worker's skill, we would find that $\gamma^{\text{IN}} = \gamma^{\text{OUT}}$. The effect of an individual's different-region coworkers is more precisely estimated than that on an individual's same-region coworkers. This reflects the fact that Ω_{it}^{OUT} is constructed using former coworkers from four regions, relative to a single region. Table 6 presents coefficients for the pooled sample of men and women.

The fact that a worker's outside-region coworkers impact mobility, but not earnings is consistent with the idea that workers may not be able to use outside offers at geographically distant firms as leverage with their current employers. Because our data are monthly, earnings impacts are driven entirely by job-stayers. Employers may doubt a worker's willingness to relocate and may, as a result, not see an outside offer from a distant firm as a credible threat. This may also explain why women see lower earnings gains than men: they are less likely to move (top panel).

The main concern with this test is that, if mobility across regions is low, the coworkers who work in different regions may have a different set of skills from those who stay in the same region. This seems somewhat unlikely in the Danish context: conditional on a job-to-job transition, roughly half of workers start working in a different region (Kristoffersen, 2016). This partially reflects the fact that Denmark is a small country: Denmark's two largest cities, Aarhus and Copenhagen, are just a three hour drive apart. Further, Table A2 shows that a sizable fraction of an individual's former coworkers now work in different regions. A related concern is that labor markets might be very local: even within an industry, firms in different regions may produce different products or use different combinations of workers' skills. We think that this is somewhat less of a concern than in it would be in other contexts, because Denmark is not very large.

5.4.2 Past versus Future Coworkers

Our second test exploits the fact that some coworkers are more or less likely to provide the worker with information, because of *when* they worked together. The logic of this test is simple: because workers may lose contact with their former colleagues over time, coworkers a worker worked with in the more distant past are likely to be less valuable sources of information. Further, while an individual's future coworkers likely have similar skills in ways we can and cannot observe, they are less likely to be a source of information in the current period (because they have not yet worked together).[26] These coworkers therefore give us another way to control for changes in demand for a worker's skills. We construct distinct networks comprising individuals the worker worked with (1) 4-5 years ago, (2) 2-3 years ago, and (3) 1 year ago and workers the worker will work with in (4) 1 year, and (5) 2-3 years. In some specifications we divide the third network into coworkers an individual worked with in the past six months and coworkers an individual worked with between 6 and 12 months ago. We describe how we create measures of Ω_{it} for each of these groups in Appendix C.6.

If our results were driven by information transmission, we would expect to see three patterns. First, changes in labor demand at firms at their more recent former coworkers would matter more than changes at firms of coworkers they worked with in the more distant past. Second, changes at an individual's *future* coworkers' firms would not significantly impact wage growth. Third, the coefficients on past measures of Ω_{it} from the "short" regression—that includes only shocks to an individuals former coworkers' firms—should be equal to those in the "long" regression that adds controls for shocks to individual's future coworkers' firms. By contrast, if the results were driven by unobserved demand shocks, we would expect the coefficients on Ω_{it} to fall significantly when we added controls for changes in demand at an individual's future coworkers' firms.

Table 7 presents estimates of γ^n from equation 7 (columns 2 and 6) and equation 8 (remaining columns) for two outcomes: job to job mobility and changes in log monthly earnings. Column 1 of Table 7 confirms that the average number of coworkers an individual has each year varies in

[26]These future colleagues may still be a source of information if they are connected to the worker in other ways–e.g. through family or education networks.

proportion to the number of years included in the network.

$$y_{it} = \gamma^n \Omega_{it}^n + c_{it}^n + \delta X_{it} + \alpha_i + \alpha_{kt} + \epsilon_{it} \tag{7}$$

$$y_{it} = \sum_n (\gamma^n \Omega_{it}^n + c_{it}^n) + \delta X_{it} + \alpha_i + \alpha_{kt} + \epsilon_{it} \tag{8}$$

We find empirical support for all three predictions. First, the coefficients in column 2 (also presented graphically in Figure 10) show that the effects on mobility decline monotonically as we move from examining the effects of her prior year coworkers to those of the coworkers she last worked with 4-5 years ago. When we include each of these former coworker networks in a single regression (column 3), the same pattern emerges, though some of the standard errors increase, reflecting the fact that there is overlap in the firms covered by each network. Columns 6 and 7 present analogous results for changes in log earnings monthly. Second, changes in demand at her future coworkers' firms are much less important and have no impact on wage growth.[27] Third, the coefficients in the short regressions (columns 3 and 7) are not significantly different from those that include the future network controls (column 4 and 8).

The main concern with this falsification exercise is that an individual's skills may also change over time. While a combination of unobserved demand shocks and rapid changes in skill could explain some of our results, it would not explain the fact that an individual's prior year coworkers influence her wage growth, while her future year coworkers do not.

5.5 Dynamics

We next examine how the effects play out over time. We plot estimates of γ from models of the form

$$y_{i,t+j} = \gamma \Omega_{it} + \gamma_j \Omega_{i,t+j} + c_{ij} + \alpha_i + \alpha_{k,t+j} + \epsilon_{it} \tag{9}$$

The coefficients describe how variation in Ω_{it} impacts mobility decisions and wage growth in subsequent months, after controlling for variation in outside opportunities in those periods.

Mobility Figure 11 shows that moving from an average of one vacancy per former coworker to two vacancies per former coworker increases the probability that an individual will make a job-to-job

[27] While there is a statistically significant impact of changes in demand at an individual's next-year coworkers' firms on mobility (not earnings), the impact is less than a third of the size of that for her prior year coworkers, and is smaller than the impact of her 4-5 year removed coworkers.

transition this period. There is a somewhat negative impact on mobility the next month, but no impact in subsequent months. There is no impact on whether individuals exit to non-employment or unemployment or on whether they move to an unconnected or out-of-sample firm. The results are driven entirely by moves to connected firms. This indicates that, after controlling for the value of Ω_{it} in a given period, there is no additional impact of past values of Ω_{it} (past outside options).[28]

Earnings Figure 12 shows the dynamic effects of Ω_{it} on an individual's base pay (bottom panel) and bonuses (top panel). The top panel shows that there is an immediate impact on idiosyncratic 'bonus' pay. There is a negative impact on bonus pay in future periods, after controlling for future values of Ω_{it}. The bottom panel shows that individuals' base pay takes two months to adjust. This is what we would expect to see if it takes a while to negotiate with one's boss for a raise.

Figure 13 presents estimates of γ from a regression of the change in log base pay in period $t+j$ (relative to period t) on Ω_{it} and on that period's value of $\Omega_{i,t+j}$:

$$y_{i,t+j} - y_{i,t} = \gamma\Omega_{it} + \gamma_j\Omega_{i,t+j} + c_{ij} + \alpha_i + \alpha_{k,t+j} + \epsilon_{it} \tag{10}$$

This figure shows that changes in base pay do not revert in the short run; four months later, the worker is still earning more.

5.6 Exploiting Trade-Induced Changes in Labor Demand

We can also address the concern that our results are driven by unobserved changes in the demand for workers' skills by examining changes in labor demand that are driven by changes in global demand for each firm's exports. Changes in world demand for different products may lead firms to expand employment or raise wages for incumbent workers (Hummels et al., 2014; Garin and Silverio, 2017).

Instrument Because realized changes in a firm's exports may be confounded by changes in firm productivity or changes in local conditions, we follow the prior literature and use world export demand to construct a measure based on predicted firm-level exports. We construct $\hat{\Omega}^{\text{trade}}_{it}$ in three steps. First, we use data from the first six years of our administrative trade register (2004-2009) to calculate the share of Danish exports of each six-digit product p accounted for by each firm j, π^j_p.

[28] The fact that the moves occur very quickly is likely due to the fact that our measure of outside options uses *realized* hires or positions created. These reflect vacancies that were posted one or two months prior. This also reflects the fact that firm labor demand is highly serially correlated; firms that expand in one month often expand in the next. Each regression controls for that period's value of Ω_{it}, which enters significantly. We do not find such fast adjustment with the trade-based measures used in Section 5.6.

Fixing the product shares using pre-period data ensures that our measure of demand for a firm's exports does not respond to changes in firm productivity. Second, we weight monthly measures of total world exports of each product (less exports from Denmark) from COMTRADE by these firm-product weights. Our COMTRADE data begin in 2010 and run through the end of our register.

The first two steps are similar to those used in prior work.[29] In a third step we weight firm-specific measures of log predicted exports by each individual's coworker network. Because most individuals do not work in firms covered by the trade register, we use weights based on the fraction of former coworkers who are in exporting firms. All of our regressions control for the total number of workers in an individual's network that are in exporting firms (c_{it}^{trade}). More information on this instrument is provided in Appendix C.7.

Reduced Form Table 8 presents estimates of γ from the regression:

$$y_{it} = \gamma^{\text{trade}}\hat{\Omega}_{it}^{\text{trade}} + \beta X_{it} + c_{it}^{\text{trade}} + \alpha_{kt} + \alpha_i + \epsilon_{it} \tag{11}$$

for different outcomes y_{it}. All of the regressions control for individual fixed effects to control for time-invariant differences in network quality and trade-register coverage. They also include controls for the number of coworkers in an individual's network and the share covered by the trade register. Standard errors are two-way clustered by individual and firm. Note that the sample in this table differs from that in our usual tables, because individuals who do not have any former coworkers in firms covered by the trade register are excluded.

Columns 1 and 2 show that $\hat{\Omega}_{it}^{\text{trade}}$ is positively related both to (1) a measure of $\Omega_{it}^{\text{trade}}$ computed using actual (not predicted) firm exports and (2) the measure of outside options used in the prior sections. The second result is consistent with earlier research: firms that experience increased demand for their exports expand the size of their labor force.

Columns 4-6 present the reduced form. These columns show that there is also a relationship between $\hat{\Omega}_{it}^{\text{trade}}$ and both mobility and wage growth: when an individual's former coworkers' firms see more demand for their exports, that individual is more likely to move or see an increase in earnings. Panel B shows that this relationship is robust to the inclusion of firm-occupation-time controls. These controls allow us to account for the fact that changes in product demand at an individual's former coworkers' firms is likely correlated with changes in demand for the exports at

[29] In particular, Hummels et al. (2014) also used fixed product shares to weight world demand for exports. However, they also used measures of transportation costs and import demand. They find a negative relationship between changes in log (annual) earnings and changes in log predicted imports (an offshoring index) at the firm level. Our work differs from theirs in that we focus on the pass-through to workers at different firms.

the individual's own firm.

5.7 Robustness Checks

We have conducted a number of robustness checks to ensure that the results in the previous sections are not driven by choices we made in constructing our measure of outside options or in constructing our regression sample. In one set of checks we verified that we obtain similar results when using different definitions of an individual's coworker network. When constructing these networks we had to specify both a window of time over which to define the network, and a firm size cutoff. Appendix Tables A9 and A10 present estimates of γ from equation 5 using three alternate definitions. The first two columns of each table use the baseline firm-size threshold but use a two-year or five-year window. The third column uses the baseline window of time but excludes connections formed in firms with more than 500 workers, rather than 1000 workers.

We also test whether our results are robust to including former coworkers who move to large firms. We removed these coworkers because new positions at very large firms are likely to be known to all workers. We find that relaxing this does not meaningfully impact the results. One reason for this is that many workers do not have any connections to these firms. Figure A4 shows that adding versions of Ω_{it} calculated among the set of former coworkers who now work at large firms ($\Omega_{it}^{\text{large}}$) does not change the main estimate. The coefficient on $\Omega_{it}^{\text{large}}$ is orders of magnitude smaller, and is not stable across specifications. This is exactly what we would expect to see if information about new positions at these firms is common to all workers.

In a second set of checks, we verified that our qualitative findings also emerge when considering alternative measures of firm shocks (s_j) or alternative weighting functions (ω_j). Appendix Tables A7 and A8 compare our baseline estimates of equation 5 to those computed using four alternative measures. The first pair (columns 1 and 2 of each table) is based on the number of new positions at each firm; column 1 presents our baseline measure and column 2 presents the same measure, weighted by the mean wage at each firm. The second pair (columns 3 and 4) uses the leave-out number of hires, rather than the change in employment. While it is difficult to compare magnitudes across columns, the qualitative patterns are the same.

We have run a number of additional checks in addition to those reported in this paper. First, in results not reported here we show that there are similar effects on mobility when we include multiple job-holders, or when we relax our definition of multiple job-holding. However, for individuals disposed to multiple job-holding, changes in information about outside options also lead an individual to obtain an additional job (at a coworker connected firm). We have also verified that the qualitative earnings results are similar for the full worker sample when we sum earnings across all firms an individual works for in a given month. It is hard to interpret these results as our theory is about single job holders. Second, in a separate analysis we also interacted our measure of outside options with year fixed effects. We did not find any systematic differences across years of our sample. Third, we have verified that we obtain similar results using data from an annual employer-employee register (Danish IDA register). Those results are noisy, however, reflecting the fact that we have a very short panel of annual data. We describe some of these checks at more length in Appendix C.

6 Heterogeneity and Mechanisms

We find that the impacts on earnings are primarily driven by changes in hourly earnings, not changes in hours worked and that both job movers and job stayers benefit. We also find that earnings impacts are concentrated among workers in the top half of the skill distribution. The results suggest that firms may not renegotiate wages with low skilled workers who receive outside offers. Within skill groups, women gain less than men.

6.1 Hours versus Hourly Earnings

We first examine whether the results are driven by changes in hours worked or changes in hourly earnings. For this analysis we focus on the subset of observations non-imputed hours.[30] We use the accounting identity

$$d \log y \quad = \quad d \log w + d \log h$$

The overall impact on log earnings depends on how both hours and hourly earnings change.

Figure 14 plots the ratio of the coefficient γ estimated from regressions of log hours (numerator) and log earnings (denominator). As usual, each regression controls for industry-by-time fixed effects, individual fixed effects, and the number of connections included in Ω_{it}.

The estimate at the far left shows that most—more than three quarters—of the impact on monthly earnings is the result of changes in earnings per hour, not changes in hours worked. However, there is heterogeneity across groups. While changes in hours worked explain only 14% of the impact for college-educated workers, they explain nearly 28% of the impact for non-college workers.

[30] We require both this month's hours and the previous month's hours to be reported by the firm. This excludes observations for workers who move between firms with different reporting statuses.

6.2 Movers and Stayers

We next examine the relative returns for job-stayers and movers. While this is a descriptive exercise, it is a useful one. Models where individuals cannot use outside offers to renegotiate wages (e.g. posting models) would predict a ratio of 0. Models where wages perfectly reflect the price of a worker's skill (and there are no 'match' effects) would predict a ratio of 1.

Figure 15 presents estimates of $\frac{\gamma^S}{\gamma^M}$ from

$$\Delta \log w_{it} = \gamma^S \Omega_{it} \times Stay_{it} + \gamma^M \Omega_{it} \times \text{Move}_{it} + \beta X_{it} + \alpha_i + \alpha_{jt} + \alpha_t \times \text{Move} + \epsilon_{it} \quad (12)$$

The baseline specification controls includes all of our baseline controls, as well as time-varying differences in the value of staying or moving. For this exercise, we focus on the subset of workers with non-overlapping job spells, in order to make sure that the earnings changes for movers reflect a full month's pay. The baseline estimate, presented at the far left, shows that, on average, stayers capture 20% the gain of movers. We can firmly reject zero. The remaining columns add additional controls. The second column adds time-varying demographic controls; the remaining columns add combinations of industry and occupation fixed effects. The point estimate remains stable across a variety of specifications.[31]

6.3 Heterogeneity: Skill Groups and Gender

It is not surprising that some workers, such as academic economists, use outside offers as leverage to obtain a raise. However, the implications of many labor market policies depend on the nature of competition in the market for relatively homogeneous workers. In order to examine whether this link between options and wages is important for workers throughout the skill distribution, we examine heterogeneity across different occupations.

We divide workers into 8 categories, corresponding to broad ISCO (International Standard Classification of Occupation) codes: (1) managers, (2) professionals, (3) technicians and associate professionals, (4) clerical support workers, (5) service and sales workers, (6) craft and related trade workers, (7) plant and machine operators, and (8) elementary occupations.[32] We then estimate our baseline regression (equation 5) separately for workers in each group.

[31] Even if movers and stayers with the same value of Ω_{it} were equally likely to have heard about an offer through their former coworkers, those that chose to move should have—according to the model in Section 2—received better offers. The relative return for movers and stayers is likely a lower bound on the fraction of rents workers are able to capture.

[32] There are 10 broad ISCO categories. We do not have data on workers in the armed forces and very few workers are classified under the "skilled agricultural, forestry, and fishery workers" category.

Figure 16 presents estimates of γ for each occupation group and gender. We produced these estimates by interacting Ω_{it} with indicators for whether the worker is male or female. Panel A presents results for mobility; Panel B presents results for earnings. We find that, while there is variation in magnitudes, the impacts on mobility are significant and positive for all subgroups. This suggests that workers throughout the wage distribution use information they obtain from their former coworkers to find new labor market opportunities. Women within each skill group are slightly less responsive than men but the differences are not significant.

We find that the earnings effects are largest for high skilled workers: those in the 'professional' category. There is no impact on assembly workers, manual-skilled workers, or craftsmen. Panel B of Table 9 presents estimates of these parameters, scaled to represent the impact of a ten unit change in Ω_{it} on a worker's annual earnings. Because more skilled workers typically earn more, scaling the parameters by mean earnings magnifies the differences between groups.

Because our earnings estimates are driven by job-stayers, our results suggest that wage renegotiation and bargaining is an important channel of wage growth only for workers in the top half of the skill distribution. By contrast, workers in the lower half of the skill distribution are more likely to be in jobs where wage renegotiation is less important for wage growth. In section 7, we show that this reflects differences in the probability that these individuals are at firms that renegotiate wages (not simply lower bargaining power).

Within skill groups that see earnings gains, women's earnings respond less than men's do. The results are in line with recent research showing that women obtain a smaller portion of changes in firm rents (Card et al., 2016*b*; Kline et al., 2018). There are several possible mechanisms. For instance, we would see this pattern if women are less likely to initiate wage renegotiation in response to an outside offer (Bowles et al., 2007; Babcock and Laschever, 2009). We would also see this pattern if women are equally likely to initiate wage renegotiation, but are less successful in bargaining.

7 Structural Parameters

Finally, we use the reduced form estimates to identify the structural model described in Section 2. The model allows us to estimate two key parameters of interest: (1) worker's bargaining power β and (2) the fraction of offers from 'posting' firms $(1 - p_R)$. This allows us to determine whether the heterogeneity we observed in Section 6.3 was the result of lower skilled workers having lower bargaining power or being less likely to work in firms that are willing to renegotiate. This is important both for distinguishing between classic models of wage setting under imperfect competition (i.e.

monopsony and search models), and for determining how changes in the labor market will influence workers. Greater values of β mean greater pass through from options to wages. However, higher fractions of wage-posting firms mean that workers are only able to see wage gains if they switch jobs. We estimate the model separately for each of the occupation groups described in Section 5. In Section 7.4 we use these estimates to examine how a decrease in the arrival rate for employed workers would impact both the overall level of wages and the level of wage growth.

7.1 Setup

We make a number of parametric assumptions before taking the model described in Section 2 to the data. First, we follow prior work and fix the monthly discount rate at $\rho = \frac{1}{1+.0050} \approx .995$ (Bagger et al., 2014*b*). Second, we allow posting and renegotiating offers to come from two distinct distributions. We assume that both are log normally distributed with means and variances (μ_P, σ_P) and (μ_R, σ_R), respectively.

Workers face different job arrival rates when they are employed and unemployed. The mean arrival rate is λ^U for an unemployed worker and λ^E for an employed worker. There is also variation in arrival rates across workers with the same employment status because some workers have better access to information than others. This varies both within an individual worker over time and across individuals within a time period. If a fraction s of an individual's network is expanding, she faces the arrival rates:

$$\begin{aligned}
\lambda^E(s) &= \lambda^E + \tilde{s}\alpha^E \\
\lambda^U(s) &= \lambda^U + \tilde{s}\alpha^U
\end{aligned}$$

where the α are scaling parameters and $\tilde{s}$ is the deviation of her information quality from that of the average worker. We assume that $\tilde{s}$ is drawn from a normal distribution with mean zero. We cannot separately identify the variance of $\tilde{s}$ and α.

Table 10 lists the 12 parameters we estimate. There are four parameters governing the offer distributions and four parameters governing the job arrival rates. The remaining parameters are: the exogenous job destruction rate, the value of non-employment, the fraction of offers coming from "posting" firms, and the bargaining power parameter.

7.2 Estimation Strategy

We estimate the model using simulated method of moments. The strategy finds values of the structural parameters that minimize the distance between a set of observed moments and the same moments calculated from a simulated version of the model. We use ξ to denote the true value of the parameters in our model. Our estimate of ξ minimizes the weighted distance between the simulated moments (given ξ) and the observed moments S_N:

$$\hat{\xi} = \arg\min_{x} (S_N - S(x))' W (S_N - S(x)) \tag{13}$$

The method is intuitive, but computationally intensive. For each guess of the parameters we simulate a panel of worker histories with 20,000 workers and 100 periods. We then calculate moments implied by this panel. Some of these moments are simple means; others are coefficients from linear regressions using variables in our simulated panel. We then calculate the weighted distance between these simulated moments and those observed in our data. More details are provided in Appendix C.8.

Moments We identify the parameters in Table 10 using three sets of moments. The first two sets are standard in the literature and are based on transition rates and moments of the log wage (and log wage change) distribution. The third set comes from our reduced form estimates. Table 11 lists the full set of moments.

1. **Transition rates** Monthly job-to-job, employment to non-employment, and non-employment to employment transition rates.
2. **Log Wages**
   (a) Mean and residual variance of log wages[33]
   (b) Mean and variance of log wage changes for job stayers
   (c) Mean log wage change associated with a job-to-job transition[34]
   (d) Quantiles of log wage change distribution: 25th, 50th, 75th, and 95th percentiles
3. **Regression coefficients** We also match the coefficient on Ω_{it} from regressions with the following dependent variables:
   (a) 1{Job-to-Job Transition}, estimated on the sample of employed workers
   (b) 1{U2E Transition}, estimated on the sample of un- and non-employed workers

[33] We do not match overall variance of log wages in order to avoid estimating the ability distribution. Instead we match the variance after removing individual fixed effects. This is the same as the approach taken by Jarosch (2015).

[34] A prior version of this paper did not attempt to match this moment. We found that matching this improved the overall fit of the model.

(c) $\Delta \log y_{it}$, estimated on the sample of job-stayers

(d) $1\{\Delta \log y_{it} > 0\}$, estimated on the sample of job-stayers

In our model there is no non-employment; individuals are either employed, or searching for work. We also do not separate non-employment and unemployment spells in our data. Any transition where an individual was not present at a firm in one month but was in the following month is counted as a U2E transition.

We calculate these moments using the subset of workers we observe working full-time jobs (or, in the case of unemployed workers, those whose last job was a full-time job). For earnings, we focus on the base pay measure, which does not contain annual bonuses, severance pay, or other one-time payments that we can identify. Both of these choices are motivated by the fact that our model does not allow for changes in labor supply (or month-to-month fluctuations in hours worked) and the fact that we have not allowed for measurement error in earnings.

Identification We can consider the sources of variation that are used to identify each parameter in Table 10. First, the three transition moments—U2E, J2J and E2U—provide the variation necessary to identify δ, λ^E and λ^U. The employment-to-unemployment transition rate provides information about δ; the job-to-job transition rate provides information about the mean job arrival rate for employed workers, λ^E; and the unemployment-to-employment transition rate provides informative about the mean arrival rate for unemployed workers, λ^U. The reduced form coefficients from the two mobility regressions identify the two α parameters.

The regression coefficients for wages are informative about the key parameters of interest: β and p_R. Intuitively, larger values of p_R decrease both the probability job-stayers see wage gains and the average size of these gains. For a fixed value of p_R, larger values of β increase the value of each outside offer, and increase the size of the wage gains. Finally, the quantiles of the log wage change distribution provide more information about the offer distributions and about our bargaining parameters β and p_R.

7.3 Posting and Bargaining

Table 10 presents parameters we estimated for the full sample.[35] There are two main results.

First, a substantial portion of offers—more than fifty percent—come from firms that would not be willing to renegotiate wages. Table 10 shows that low skilled workers are more likely to be in

[35] Standard errors are a work in progress.

jobs where they cannot renegotiate their wage.[36] This is not simply a feature of the Danish labor market. While low skilled workers are more likely to be in firms or sectors that do not negotiate wages individually (Dahl et al., 2013), Appendix Figure A6 shows that we find similar results when we break down responses from the United States-based survey analyzed in Hall and Krueger (2012).[37] This suggests that models that feature wage posting (e.g. monopsony models) may be more appropriate for lower skilled workers; models that allow for individual-firm bargaining and renegotiation may be more appropriate for higher skilled workers.

Second, job search through networks appears to be more important for non-employed than employed workers: $\alpha_0/\alpha_1 > 1$ for both groups of workers. This is consistent with prior work that has shown that new labor market entrants are more likely to rely on their family networks for employment in economic downturns (Kramarz and Skans, 2014).

7.4 Impact of a Fall in the Job Arrival Rate

Many current policy debates center on regulations that impact workers' ability to receive and take offers from other firms. For instance, changes in the enforcement of non-poach or non-compete clauses directly impact workers' ability to move between firms; changes in antitrust enforcement influence workers ability to receive outside offers, by changing the number of outside firms. Our model allows us to investigate the mechanisms through which these developments would impact different groups of workers.

We re-estimate the model, assuming a 50% reduction in λ_1 and α_1. We then compare the old and new steady states, ignoring transition dynamics. Note that this is a partial equilibrium exercise. The assumption that posted wages do not change in response to a change in the on-the-job arrival rate is somewhat unrealistic. However, it is a useful benchmark if firms that post wages may be unable or unwilling to change these wages in the short run. Recent research suggests that there may in fact be substantial downward nominal rigidity in posted wages for new hires (Hazell and Taska, 2018).

Table 12 presents the main results. The first column shows that, in response to a 50% reduction in the arrival rate of offers for employed workers, mobility falls by less than 50%. This is because

[36] Note that we do not plot $1-p_R$, but the equilibrium fraction of workers at posting firms. This is somewhat more informative. It is, in general, lower than $1-p_R$ because firms that are willing to renegotiate wages do a better job of retaining workers.

[37] We use question 34D from that survey, which asked job seekers: "When you were offered your (current/previous job), did your employer take-it-or leave-it offer or was there some bargaining that took place over the pay?". We calculate the fraction of workers in each occupation who reported that there was some bargaining over pay. More details are provided in Section D.3.

workers correctly anticipate that they will be less likely to receive offers while employed, they impose a higher bar on jobs that they accept out of unemployment. The second column shows that equilibrium wages are now lower. While the absolute magnitude is small ($<1\%$) for both groups, the absolute magnitude of the change in the arrival rate was also small (~1%). Wage growth is significantly lower for both groups.

8 Conclusion

This paper uses a novel empirical strategy to show that changes in an individual's information about their outside labor market opportunities lead to job mobility and wage growth. The results are consistent with search and bargaining models where firms renegotiate wages with workers who receive outside offers. The results are inconsistent with both a competitive neoclassic model, and with models where all firms commit not to renegotiate workers' wages (pure posting models). They also suggest that bargaining is important for a wide range of workers, not just those at the very top of the skill distribution.

The reduced form results have several immediate policy implications. First, our finding that workers are able to leverage changes in their information about labor market opportunities into increased pay suggests that pay transparency policies—which give *workers* information about what they could receive at other firms—may be an effective way to promote wage growth. Second, the results are consistent with recent arguments that increases in labor market concentration or changes in regulations that restrict worker mobility may have detrimental impacts on wages (Council of Economic Advisors, 2016; Krueger, 2017; Ashenfelter and Krueger, 2018). Finally, our results suggest that, when productive firms enter a labor market, their presence can increase the wages of workers at other firms. As a result, policies that encourage productive firms to open a plant in a labor market—through local tax breaks or other incentives—may be an effective way to boost wages of all workers (Acemoglu, 2001; Green, 2015).

One limitation of this paper is that we have limited information about an individual's information set. Future research could use new sources of data from social or employment networking sites to better identify the former coworkers an individual interacts with. A particularly interesting direction would be to work with such a platform to directly vary workers' information about labor market opportunities. A different direction would be to gather direct evidence on the frequency and nature of wage renegotiation, perhaps by collecting survey data analogous to that in Hall and Krueger (2012) for a large sample of employed workers.

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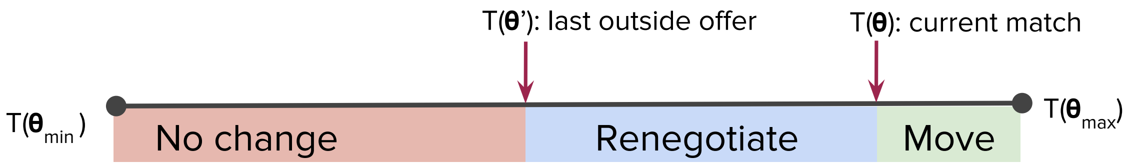
9 Tables and Figures

Figure 1: Main Theoretical Predictions

Panel A: Worker at a Firm that Renegotiates Wages

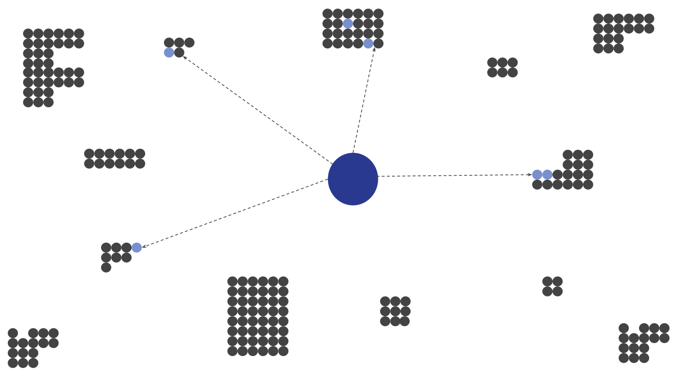


Panel B: Worker at a Firm that Posts Wages

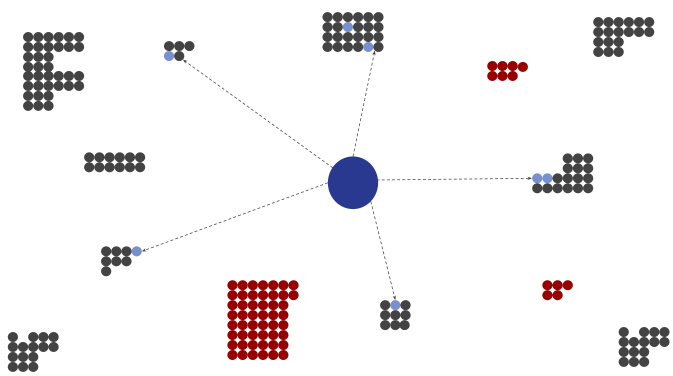
Note: This figure illustrates the main theoretical predictions in Section 2. Offers are ranked according to the maximum w m
age a worker could receive. For offers from renegotiating firms, this is the total value produced by the match; for offers from posting firms, this is the posted wage. Panel A shows what will happen to a worker at a renegotiating firm who receives an outside offer. If the outside offer is higher than the total value of her current match, $T(\theta)$, she will move to the new firm. If the offer is lower than the total value, but is better than whatever she last used to negotiate $(T(w')$, she will renegotiate with her firm for a raise. If the offer is lower than this, she will not initiate renegotiation. Panel B shows that, for workers at posting firms, outside offers can only lead to job-to-job mobility.

Figure 2: Variation in Outside Options over Time

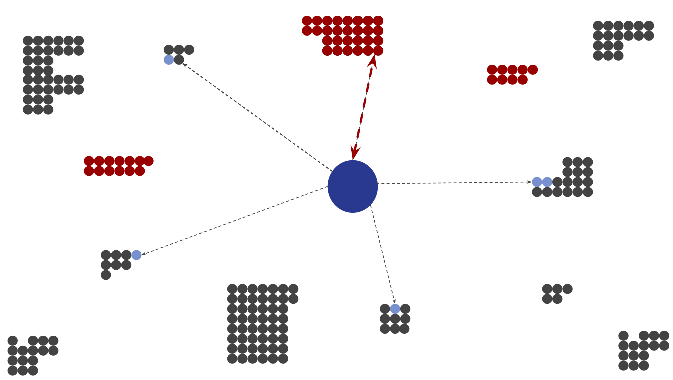
Panel A: Network Structure



Panel B: Only Unconnected Firms are Hiring



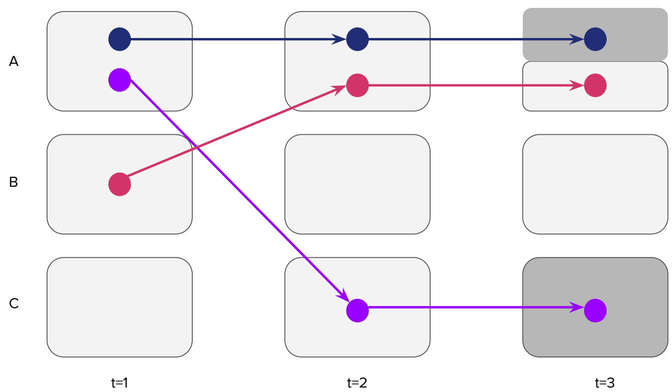
Panel C: Connected and Unconnected Firms are Hiring



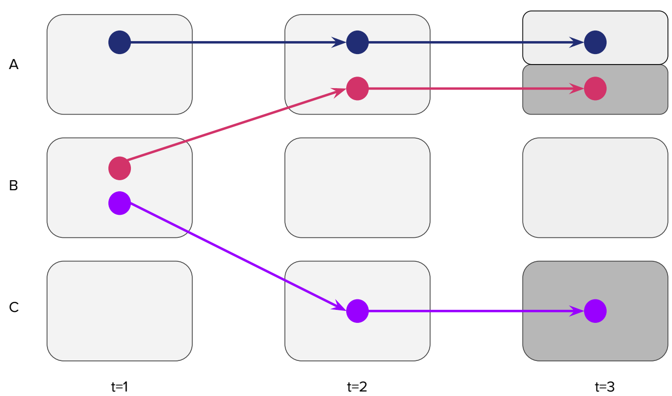
Note: This figure illustrates the identification strategy. Panel A shows the network structure. The big blue dot in the middle represents worker i. Each collection of dots represents a firm; each dot within a collection is a worker. The blue dots are workers that worker i has worked with in the past. Panels B and C depict a scenario where some of the firms (marked in red) expand. In Panel B, worker i does not have any former coworkers at the expanding firms; in Period C she does. Our identification strategy assumes that worker i is more likely to hear about job openings in the situation presented in Panel C than the situation in Panel B.

Figure 3: Variation in Networks Within a Firm

Panel A: Incumbent Worker Has Better Information

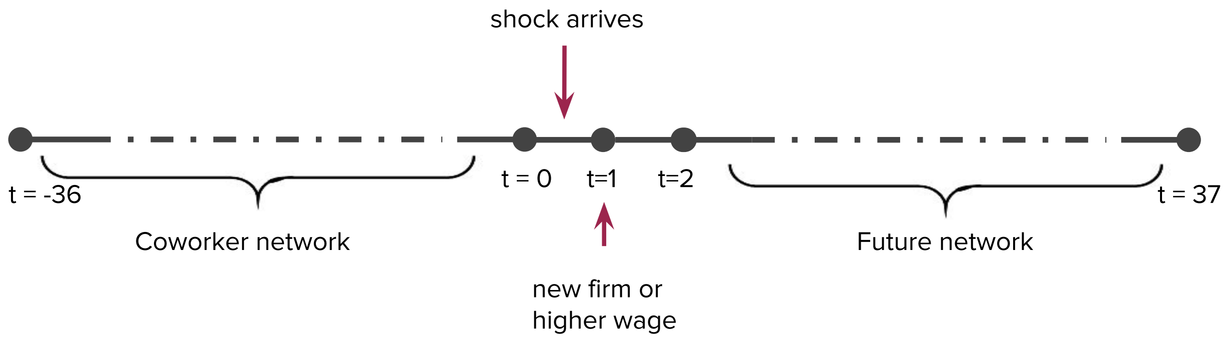


Panel B: New Worker Has Better Information



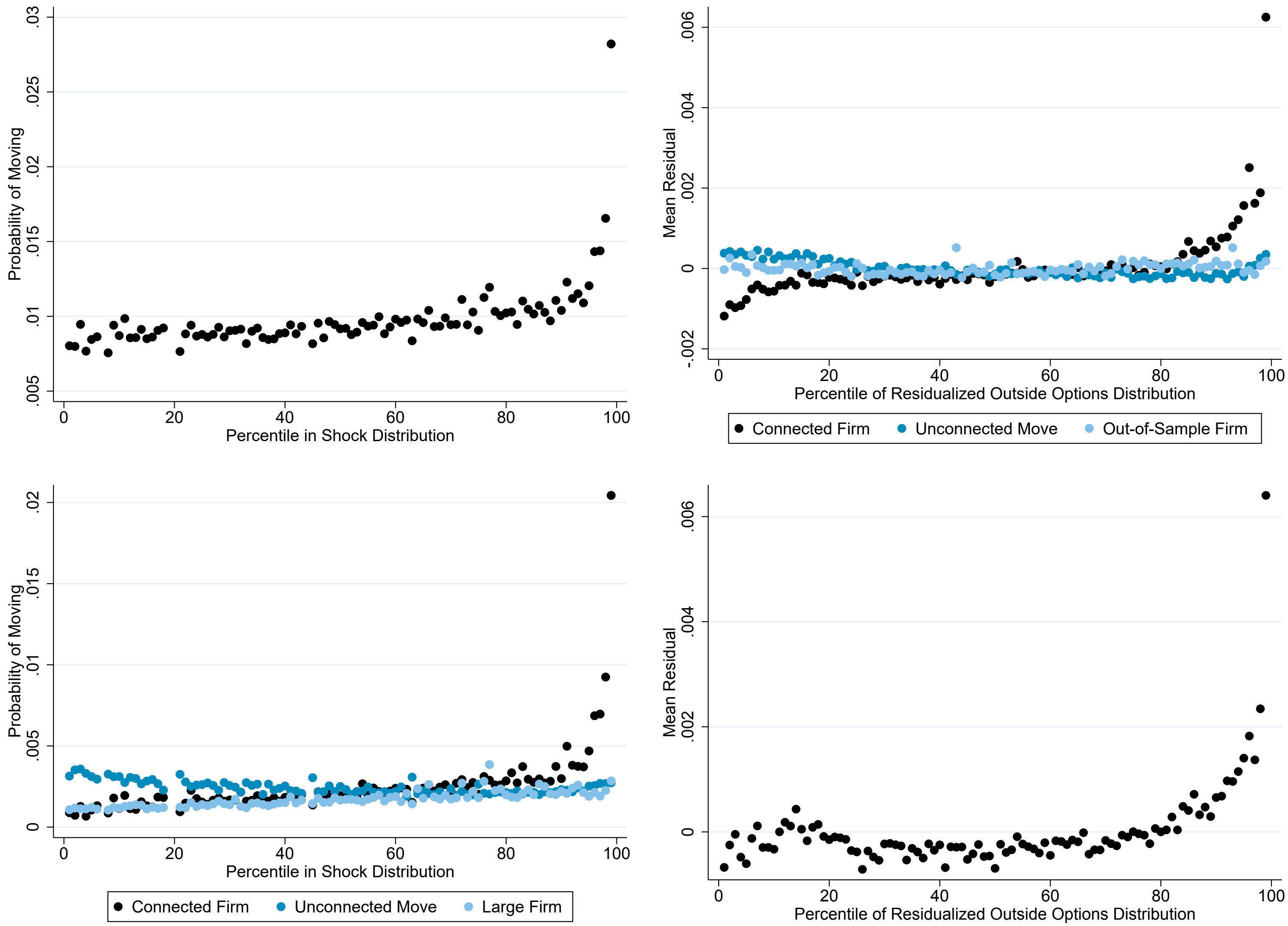
Note: Coworker networks can vary between workers in the same firm both due to their history at other firms and due to differences in tenure at their current firm. This figure shows how networks vary between workers in the same firm due to differences in tenure. Panel A shows an example where the incumbent (blue) worker has better information than a new worker (red). In the first period, the blue worker works with the purple worker at firm A; the red worker is alone at firm B. In period two, the red worker moves to firm A and the purple worker moves to firm C. In the third period the blue worker's coworker network will include the purple worker (firm C) and the red worker's will not. Panel B shows a similar example where the worker with less tenure at firm A (red) is more closely connected to firm C.

Figure 4: Graphical Depiction of Timing



Note: This figure shows the timing of the shocks and coworker networks. For each month, we use data from the previous 36 months to construct the coworker network (excluding a worker's prior firms). We use changes in employment from last period (period 0) to this period (period 1) to construct the firm-specific shock. We look at mobility decisions and earnings changes from period 0 to period 1. We use data from the next 36 months (starting in period 2) to construct the future coworker network. We exclude current coworkers from this network when there is overlap.

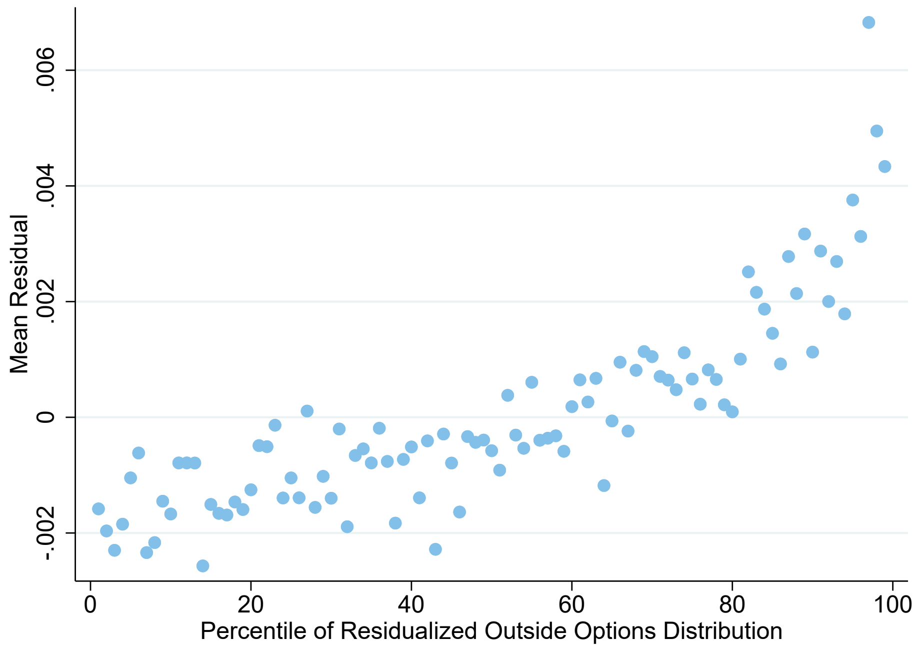
Figure 5: Impact of Outside Options on Probability of Moving



Note: This figure shows how the probability of making a transition depends on Ω_{it}. The percentiles and probabilities in the top panel are computed from the raw data. The percentiles and probabilities in the bottom panel are computed after partialling out individual and four-digit industry-by-time fixed effects. An individual makes a job-to-job transition if they are working at a different firm this month than they were working at last month. A connected move is a job-to-job transition to an in-sample firm where one of the individual's former coworkers works. An unconnected move is a job-to-job transition to an in-sample firm where an individual does not know any employees. An out-of-sample move is a job-to-job transition to a firm whose average employment exceeds 1000 over the sample period.

Figure 6: Impact of Outside Options on Changes in Log Earnings and Hourly Earnings

Panel A: Hourly Earnings

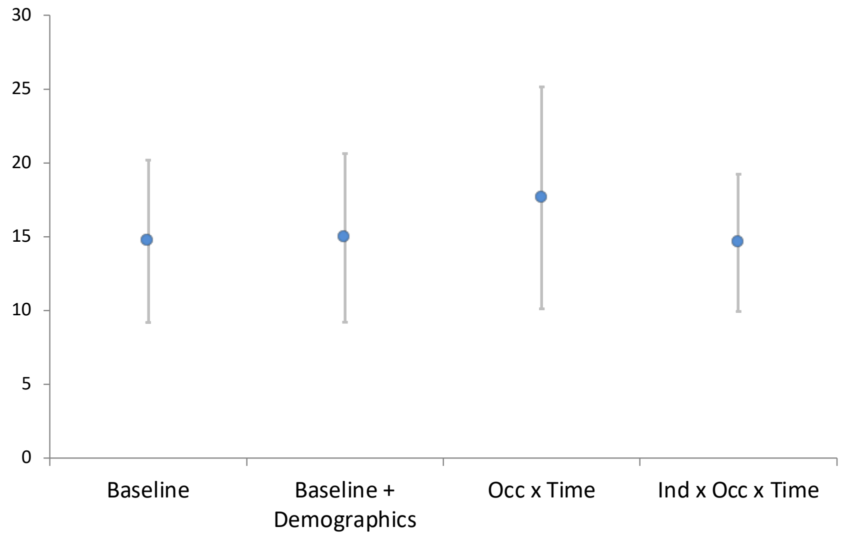


Panel B: Monthly Earnings

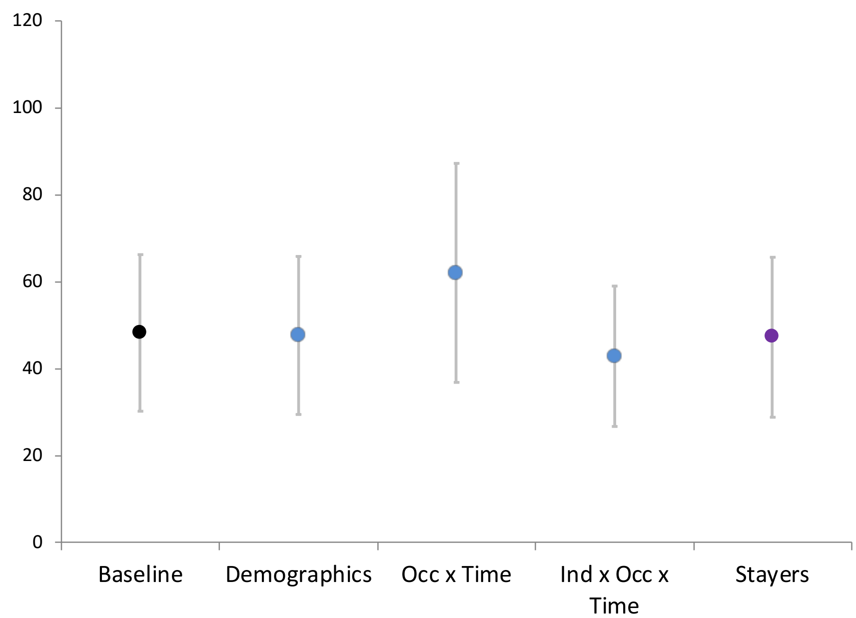
Note: This figure shows how the the change in log earnings in period t depends on the average hiring rates at an individual's former coworkers' firms between $t-1$ and t. The dependent variable in Panel A is log wages and the dependent variable in Panel B is log monthly earnings. The percentiles and earnings changes are computed after partialling out individual and four-digit industry-by-time fixed effects.

Figure 7: Reduced Form Results

Panel A: Job-to-Job Mobility



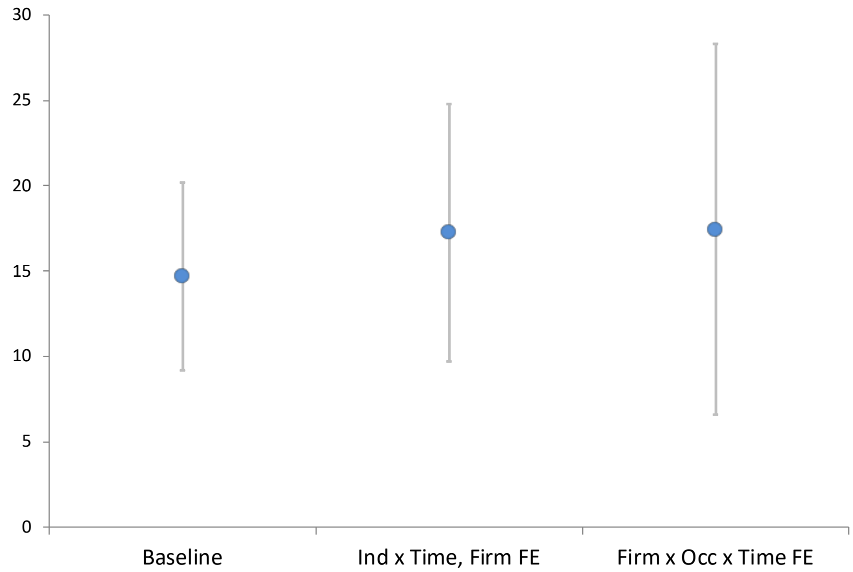
Panel B: Change in Log Monthly Earnings



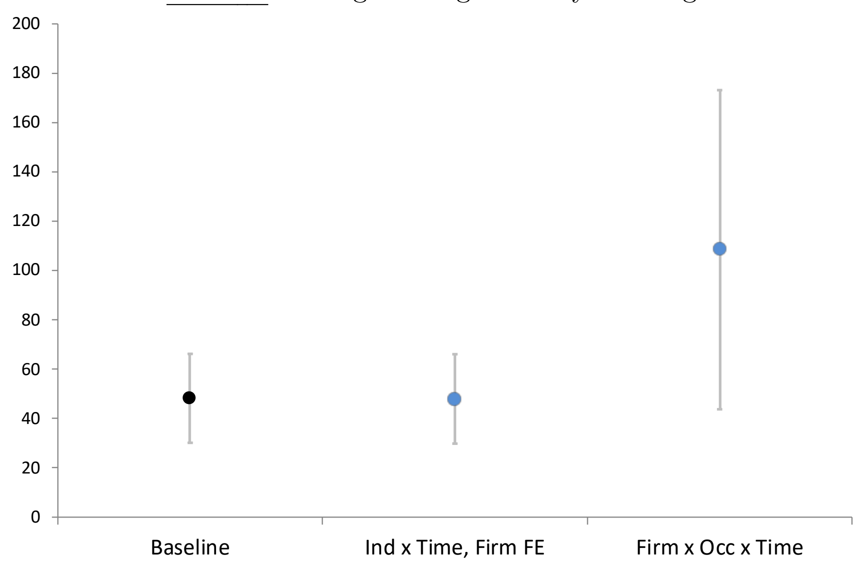
Note: This figure plots scaled estimates of γ from equation 5. The outcome variable in Panel A is an indicator for whether the individual made a job-to-job transition. The figure plots the percent impact of a ten-unit change in Ω_{it} on the probability an individual made a job-to-job transition. The outcome variable in Panel B is the change in log monthly earnings. We scale these coefficients to represent the average impact (in 2016 USD) on the average worker's annual earnings. Each regression controls for worker fixed effects, four digit industry-by-time fixed effects, and the number of connections in an individual's network. Additional controls are as listed on the x-axis. Whiskers indicate 95% confidence intervals. Standard errors are two-way clustered by individual and firm. Raw coefficients for are reported in Table 4 (mobility) and Table 5 (earnings).

Figure 8: Within-Firm Results

Panel A: Job-to-Job Mobility



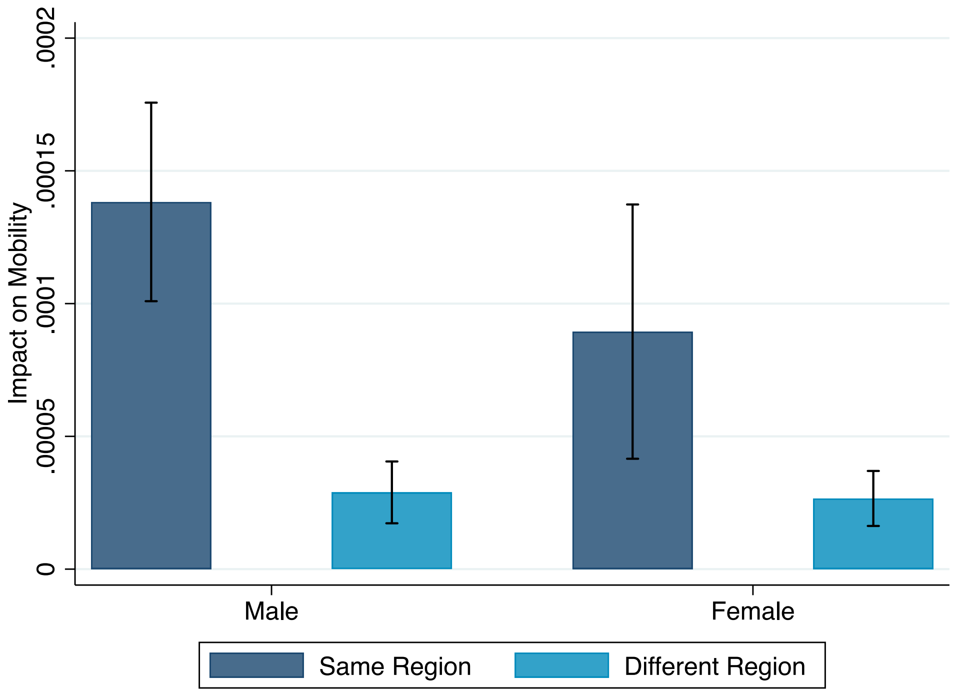
Panel B: Change in Log Monthly Earnings



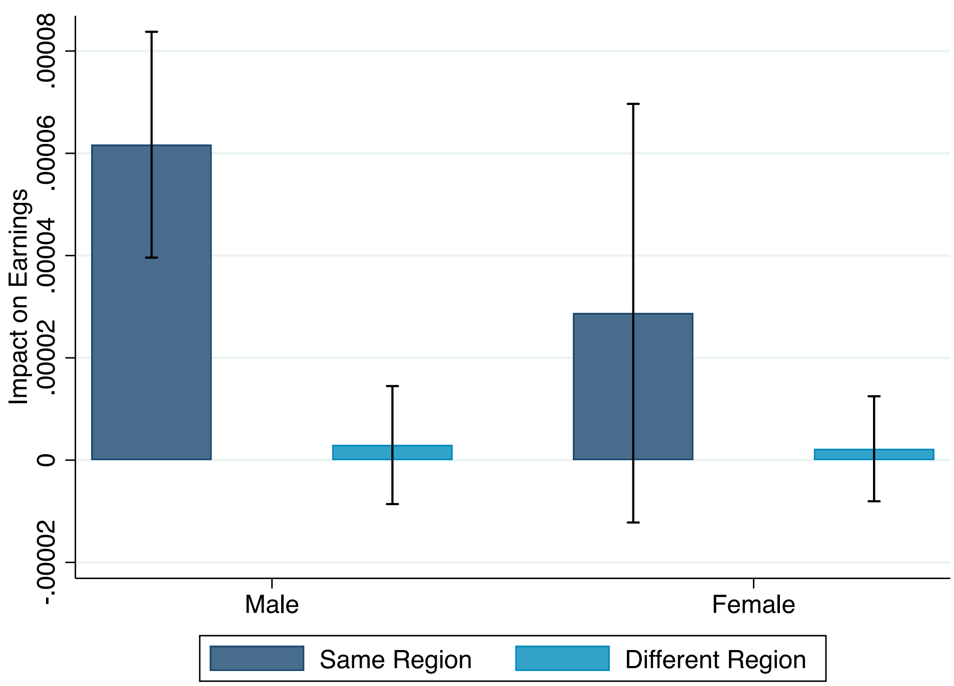
Note: This figure plots the impact of a 10 unit increase in Ω_{it} on the probability of making a job-to-job transition (panel A) or on the change in log monthly earnings (panel B). Each regression controls for worker fixed effects and the number of connections in an individual's network. Additional controls are as indicted. Whiskers indicate 95% confidence intervals. Standard errors are two-way clustered by individual and firm. Raw coefficients are reported in Tables 4 and 5.

Figure 9: Same and Different Region Coworkers

Panel A: Job-to-Job Mobility



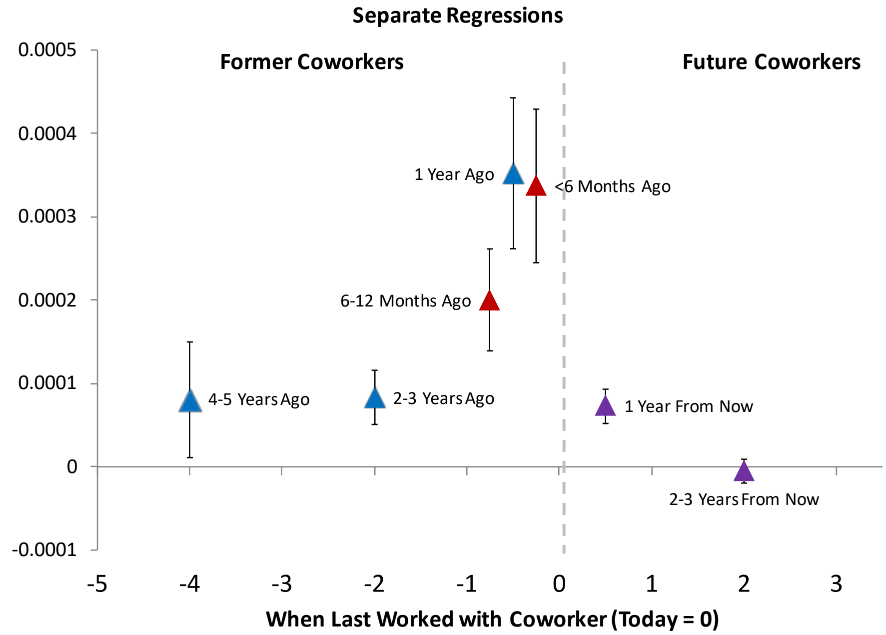
Panel B: Change in Log Monthly Earnings



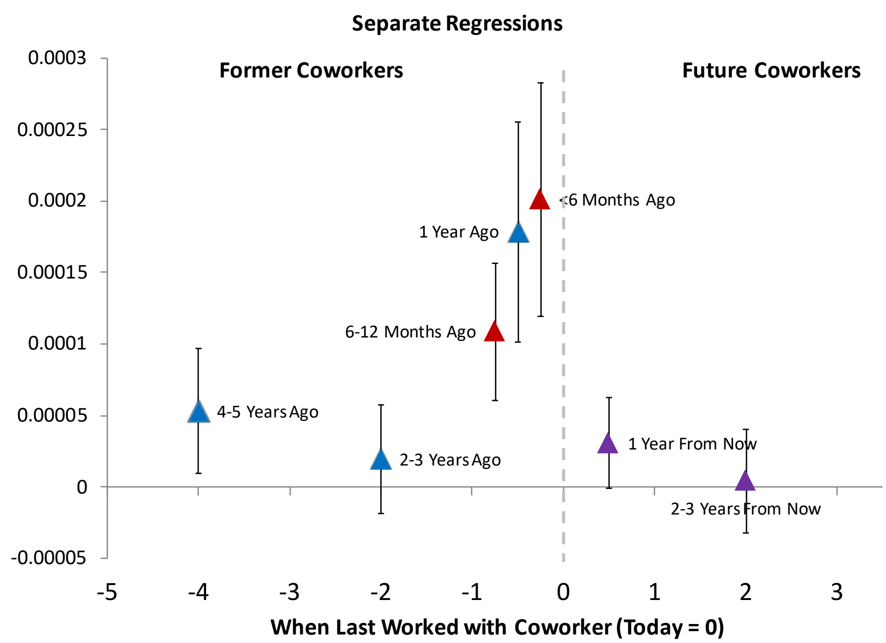
Note: This figure compares the mobility and earnings response to Ω_{it}^{IN} and Ω_{it}^{OUT}, which measure the average number of new positions created among an individual's same-region and different-region coworkers. Each regression controls for worker fixed effects, four-digit industry-by-time fixed effects, and includes linear controls for the number of coworkers in Ω_{it}^{IN} and Ω_{it}^{OUT}. Individuals are not included in these regressions if they do not have any former coworkers working in the same region or in any of the other four regions. Whiskers indicate 95% confidence intervals. Standard errors are two-way clustered by individual and firm.

Figure 10: Impacts of Coworkers From Different Time Horizons: Separate Regressions

Panel A: Job-to-Job Transition

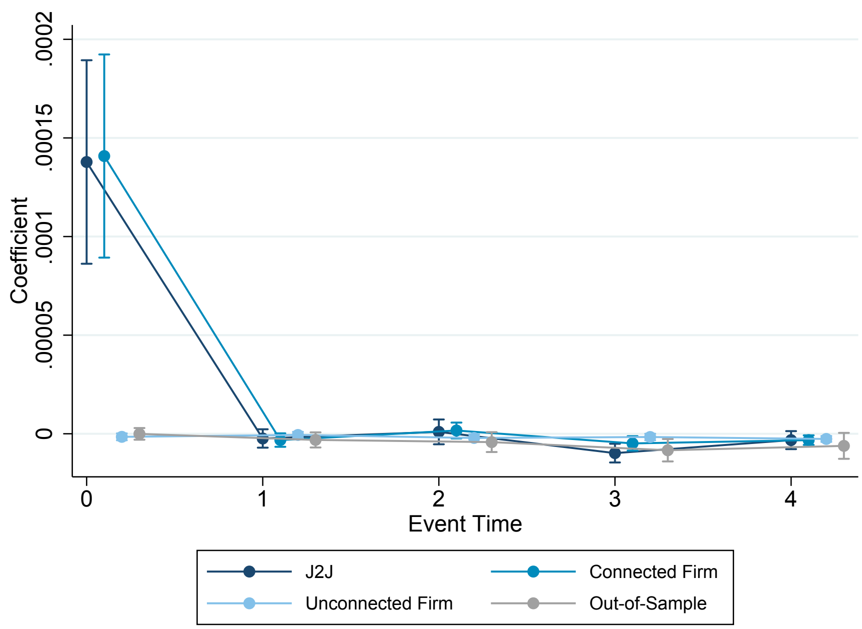


Panel B: Change in Log Monthly Earnings



Note: This figure shows how the impact of Ω_{it} varies based on the length of time since the worker worked with his/her former coworkers or the length of time before the worker starts working with his/her future coworkers. Each figure reports estimates from separate regressions of the outcome variable on each network, as described by in equation 7. Each regression controls for worker fixed effects, four digit industry-by-time fixed effects, and the number of connections in the included network. Whiskers indicate 95% confidence intervals. Standard errors are two-way clustered by individual and firm. Table 7 presents analogous results from regressions which include prior and future networks in the same regression.

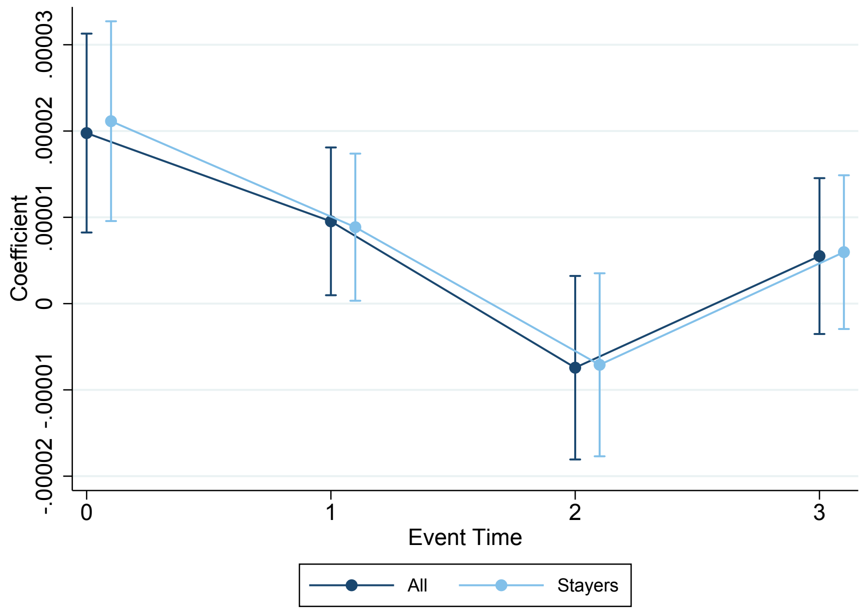
Figure 11: Dynamics: Probability of Moving



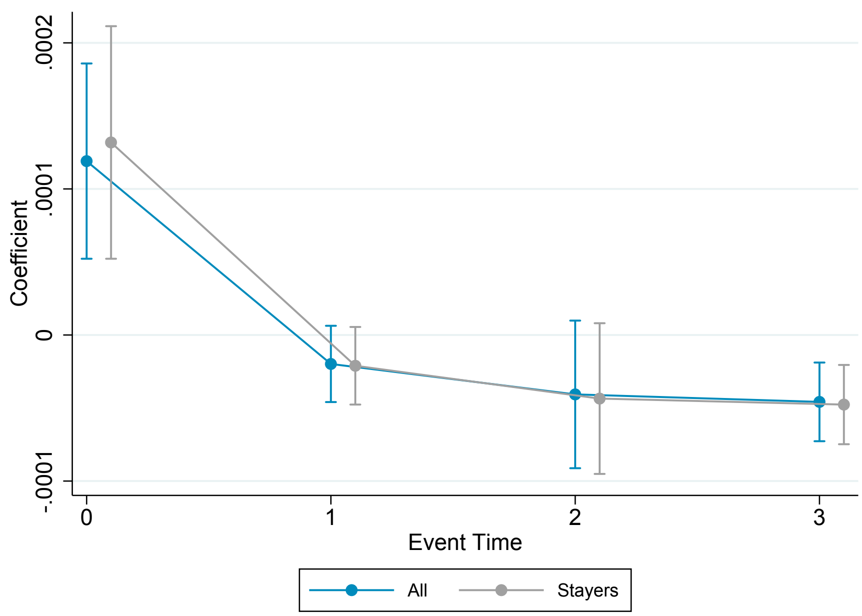
Note: This figure shows how the probability of making a job-to-job transition or moving to a coworker-connected firm depends on their value of Ω_{it} at $t = 0$. The sample includes all individuals that are in the network sample at time $t = 0$. Each dot represents a separate regression. Each regression controls for worker fixed effects, four digit industry-by-time fixed effects, and the number of connections in the individual's network. We also control for that period's value of Ω_{it}. Whiskers indicate 95% confidence intervals. Standard errors are two-way clustered by individual and firm.

Figure 12: Dynamics: Earnings

Panel A: Base Pay

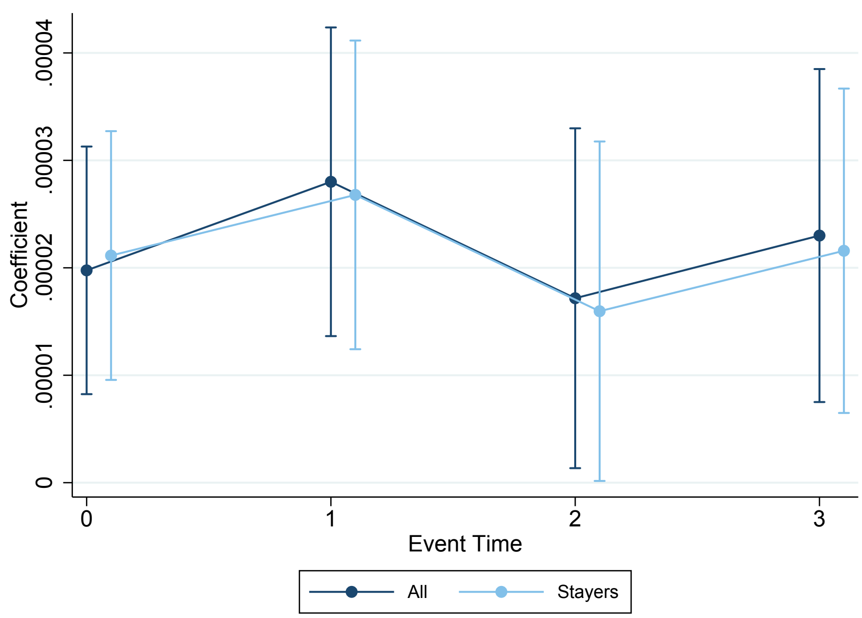


Panel B: Bonus Pay



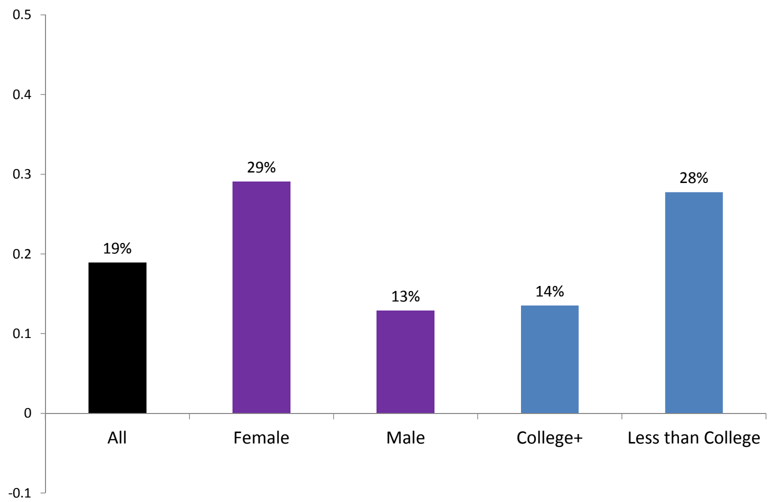
Note: This figure shows how earnings changes depend on their value of Ω_{it} at $t = 0$. The first panel presents results for base pay, which excludes bonuses. The second focuses on bonus pay. Each dot represents a separate regression. Each regression controls for worker fixed effects, four digit industry-by-time fixed effects, and the number of connections in an individual's network. We also control for that period's value of Ω_{it}. Stayers are workers that did not change firms at $t = 0$. Earnings are in kroner. Whiskers indicate 95% confidence intervals. Standard errors are two-way clustered by individual and firm.

Figure 13: Long-Run Impacts on Base Pay



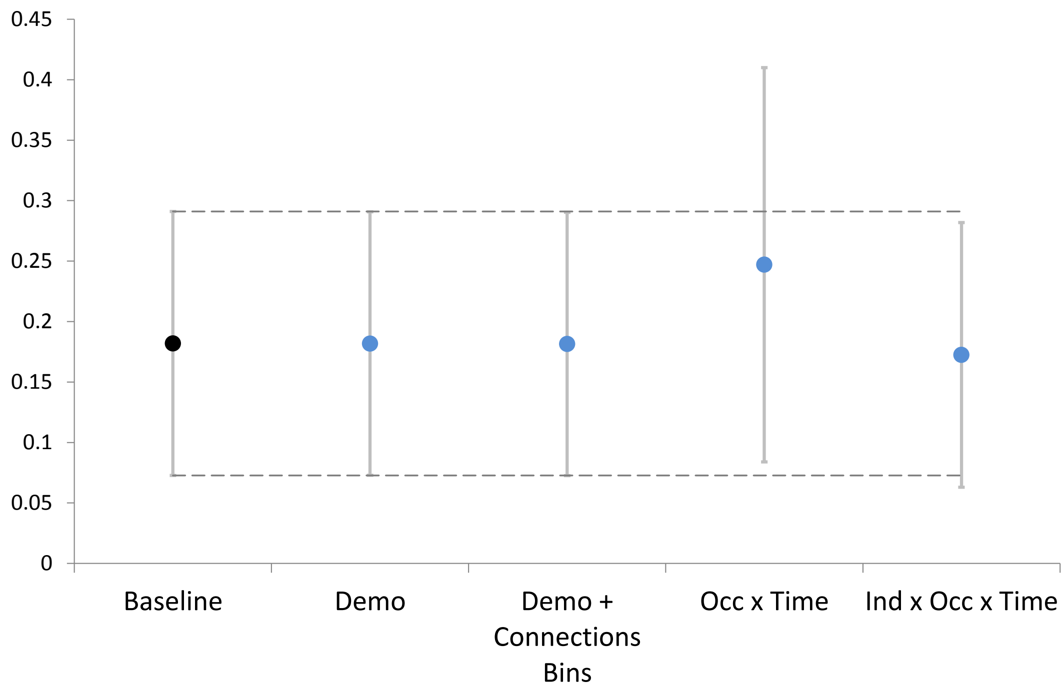
Note: This figure shows how the relationship between earnings at period $t+k$ and earnings at period $t-1$ depends on the value of Ω_{it} at $t=0$. Our measure of earnings is "base pay": earnings without bonuses. Each regression controls for worker fixed effects, four digit industry-by-time fixed effects, and the number of connections in an individual's network. Stayers are workers that did not change firms at $t=0$. Earnings are in kroner. Whiskers indicate 95% confidence intervals. Standard errors are two-way clustered by individual and firm.

Figure 14: Mechanisms: Hours versus Hourly Earnings



Note: This figure shows what portion of our earnings results are driven by changes in hours worked. We use the accounting identity: $d\log y = d\log h + d\log w$. For each demographic group we present the ratio of the coefficients from equation 5. The numerator comes from a regression where the outcome is the change in log hours. The denominator comes from a regression where the outcome is the change in log earnings. The sample differs from that in Table 5 because both the earnings and hours regressions only include observations for workers who had non-imputed hours both this month and in the prior month. The results are discussed in Section 6.1.

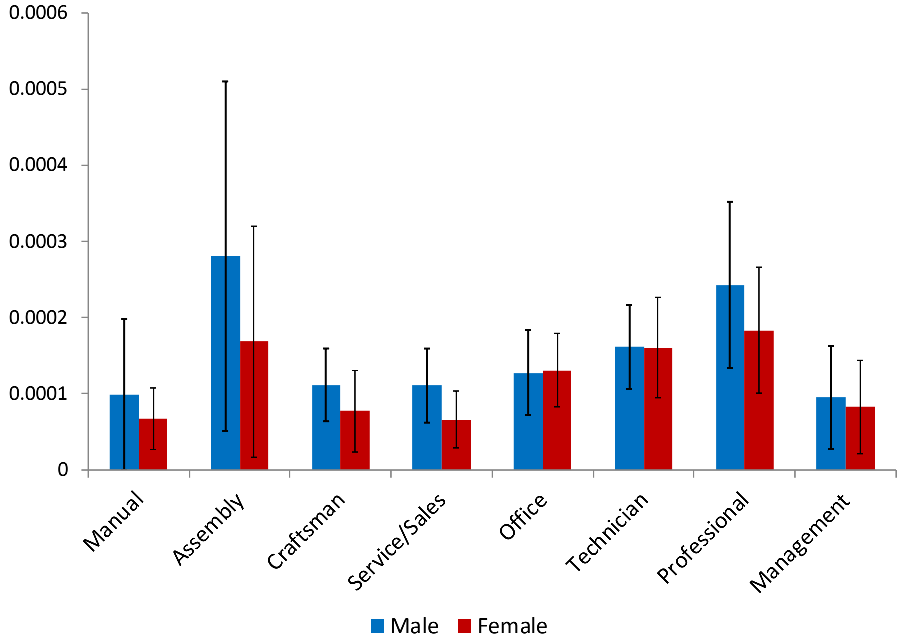
Figure 15: Reduced Form Evidence on Bargaining: Returns for Movers and Stayers



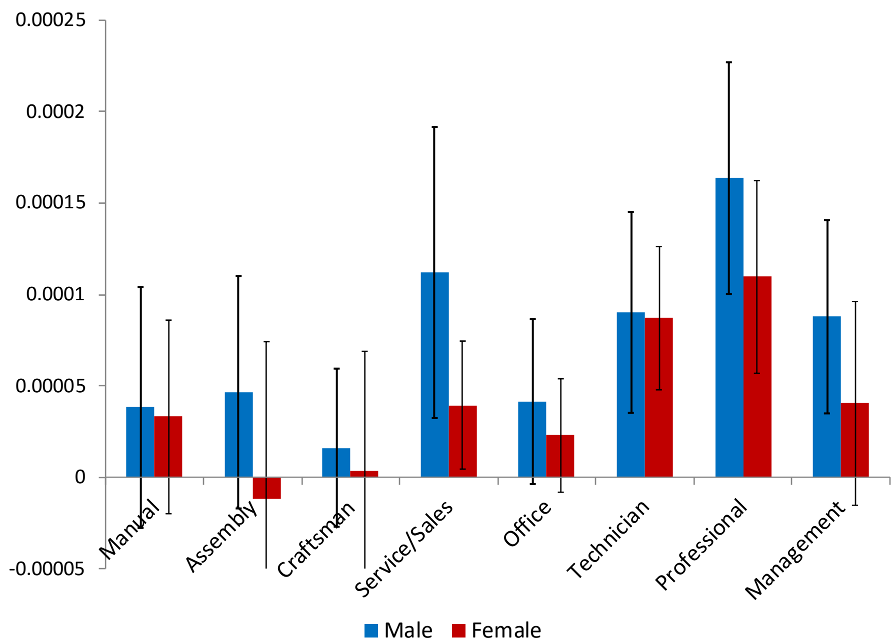
Note: This figure shows how the ratio of returns to information for job stayers and movers changes across different specifications. Each estimate comes from regressions of equation 12. Our sample includes the subset of workers with non-overlapping job spells. The demographic controls are indicators for whether the worker has kids or is married. In the third estimate, we replace the linear control for the number of connections with indicators for deciles of the connections distribution. The fourth estimate includes four-digit occupation-by-time fixed effects instead of the industry-by-time fixed effects. The fifth estimate include four-digit industry by two-digit occupation by time fixed effects.

Figure 16: Heterogeneity by Occupation

Panel A: Any Transition



Panel B: Change in Log Monthly Earnings



Note: This figure shows how mobility and wage responses differ across occupation groups. We group workers according to broad ISCO (International Standard Classification of Occupations) codes. We then estimate equation 5 within each occupation. The dependent variable is as indicated in each panel. Each regression controls for individual fixed effects, four-digit industry-by-time fixed effects and a linear control for the number of connections in an individual's network. Standard errors are two-way clustered by individual and firm. We do not have sufficient data on workers in the military (ISCO 10) or in agricultural occupations (ISCO 6). Table 9 presents coefficients for the pooled (both male and female) sample.

Table 1: Descriptive Statistics: Workers

		Regression Sample		
	All	All	Male	Female
	(1)	(2)	(3)	(4)
Worker-Month Observations	270115520	72130704	43351245	28779459
Workers	3809303	1096761	657086	439675
Demographics				
Danish	91%	1	1	1
	(0.29)	--	--	--
Age	39.89	43.21	43.26	43.13
	(13.55)	(9.50)	(9.56)	(9.40)
Female	0.49	0.40	0.00	1.00
	(0.50)	(0.49)		
Married	0.49	0.57	0.55	0.60
	(0.50)	(0.50)	(0.50)	(0.49)
In a Couple	0.64	0.74	0.73	0.76
	(0.48)	(0.44)	(0.44)	(0.43)
Has Children	0.51	0.54	0.51	0.58
	(0.50)	(0.50)	(0.50)	(0.49)
College +	0.39	0.31	0.27	0.37
	(0.49)	(0.46)	(0.45)	(0.48)
Employment				
Annual Earnings (2016 USD)	$42,650	$53,591	$58,648	$46,015
	(70,592)	(77,747)	(93,935)	(42,261)
Number of Firms	3.41	3.05	3.05	3.05
	(2.89)	(1.28)	(1.32)	(1.23)
Number of Industries	2.69	2.21	2.22	2.20
	(1.96)	(1.45)	(1.52)	(1.33)
Number of Months	95.00	83.12	83.49	82.57
	(44.63)	(23.18)	(23.24)	(23.07)

Note: The first entry in each row is the mean. The standard deviation is in parentheses. The regression sample includes Danish single-job-holders who are working in firms with between 2 and 1000 employees. Annual earnings are computed using the "broad" income measure and includes fringe benefits and mandatory retirement contributions. The number of industries is calculated using 4-digit NACE codes. The number of months in column 1 is the number of firm-month observations; our regression sample contains only single job-holders, who are at a maximum of one firm each month. More details on the variables are provided in the Appendix C.

Table 2: Descriptive Statistics: Firms

	All		Trade Register	
	All	Network	All	Network
	(1)	(2)	(3)	(4)
Number of Firms	352010	272346	49574	43423
Employment	11.07	8.41	41.68	21.11
	(224)	(32)	(544)	(63)
Establishments	1.19	1.12	1.78	1.35
	(4.64)	(1.24)	(11.00)	(2.48)
Firm Accounts				
In Accounting Data	69%	76%	89%	93%
	(0.46)	(0.43)	(0.32)	(0.26)
Revenue (1000 2016 USD)	2.74	2.57	9.58	7.90
	(57)	(49)	(121)	(99)
Value Added/Worker	73.99	74.53	102.24	105.48
	(2049)	(2200)	(4285)	(4458)
Trade Data				
In Trade Register	14%	16%	100%	100%
	(0.35)	(0.37)		
Importer			90%	90%
			(0.30)	(0.29)
Exporter			53%	56%
			(0.50)	(0.50)
Number of Products			3.32	3.29
			(16.81)	(14.78)
Annual Export Value (1000 2016 USD)			279.14	218.39
			(3709.46)	(1907.75)
Location				
Capital Region	33%	32%	36%	35%
	(0.47)	(0.47)	(0.48)	(0.48)
Central Denmark	22%	23%	24%	24%
	(0.42)	(0.42)	(0.42)	(0.43)
North Denmark	11%	11%	10%	10%
	(0.31)	(0.31)	(0.30)	(0.30)
Zealand Region	14%	14%	10%	10%
	(0.34)	(0.34)	(0.31)	(0.31)
Southern Denmark	21%	21%	21%	21%
	(0.41)	(0.41)	(0.40)	(0.41)

Note: This table presents descriptive statistics on the firms in our sample. The first column includes all firms included in our data. The second column restricts to the set of firms in our network sample: those with more than 1 and and fewer than 1000 employees. We used the average number of employees over the sample period to define the network sample. Standard deviations are in parentheses. We calculate the number of establishments at each firm by linking our observations to the annual IDA panel. The firm accounting variables come from the accounting register (FIRE). The trade variables come from the UHDM register. For each firm we calculate the mean number of (six-digit) products each firm exports, across all months the firm is in the trade register. In order to comply with Statistics Denmark privacy regulations, we calculated the median number of products after taking a ten-firm moving average of the data.

Table 3: Characteristics of Coworker Networks

	All (1)	Male (2)	Female (3)	College (4)	Less Than College (5)
Number of Connections	156	146	172	175	148
	(278)	(252)	(312)	(313)	(261)
	[58.9]	[56.9]	[62.4]	[72.2]	[53.5]
Characteristics of Connections					
Fraction Female	39%	29%	55%	47%	36%
	(0.26)	(0.21)	(0.24)	(0.23)	(0.26)
Fraction College+	34%	29%	40%	52%	26%
	(0.25)	(0.24)	(0.25)	(0.24)	(0.20)
Mean Age	43.2	43.6	42.7	43.4	43.1
	(7.4)	(7.1)	(7.9)	(7.3)	(7.5)
Fraction in Trade Register	35%	36%	34%	36%	35%
	(0.28)	(0.28)	(0.28)	(0.32)	(0.26)
Connected Firm Characteristics					
Mean Value Added Per Worker	560.5	567.9	549.4	608.2	539.6
	(2201.1)	(1841.9)	(2653.7)	(3592.7)	(1147.0)
Mean Hourly Earnings (kroner)	215.3	217.1	212.5	228.7	209.4
	(38.4)	(37.5)	(39.6)	(45.1)	(33.4)
Fraction Female	39%	33%	48%	45%	37%
	(0.16)	(0.14)	(0.15)	(0.14)	(0.16)
Dispersion					
Number of Industries	31.9	31.5	32.4	32.4	31.6
	(36.4)	(35.9)	(37.2)	(34.7)	(37.1)
Number of Firms	60.5	58.9	62.8	63.5	59.2
	(92.8)	(89.8)	(97.0)	(92.9)	(92.7)
Observations	1096764	657089	439675	334269	762219

Note: This table describes the characteristics of the coworker networks. Each individual's (time-varying) coworker network consists of individuals he/she has worked with in the past three years. We provide more details on how we constructed these networks in Section 4. The first entry in each row is the mean. Standard deviations are reported in parentheses; medians are reported in brackets. To comply with Statistics Denmark's privacy regulations, we computed the medians after taking a 10-person moving average.

Table 4: Impact of Outside Options on Mobility

	Baseline	With Demographic Controls	Occupation-by-Time	Industry-Occupation-by-Time	Within-Firm Analysis: Baseline + Firm FE	Within-Firm Analysis: Firm-Occupation-Time FE
	(1)	(2)	(3)	(4)	(5)	(6)
Any Transition	1.255 ***	1.399 ***	1.521 ***	1.255 ***	1.468 ***	1.430 ***
	(0.255)	(0.273)	(0.350)	(0.217)	(0.351)	(0.485)
Job-to-Job	1.378 ***	1.400 ***	1.654 ***	1.368 ***	1.618 ***	1.637 ***
	(0.263)	(0.273)	(0.360)	(0.223)	(0.361)	(0.520)
Connected Firm	1.408 ***	1.431 ***	1.656 ***	1.406 ***	1.642 ***	1.685 ***
	(0.263)	(0.273)	(0.352)	(0.222)	(0.354)	(0.521)
Connected Industry	-0.014	-0.013	0.004	-0.018 *	-0.010	-0.020
	(0.010)	(0.010)	(0.012)	(0.009)	(0.012)	(0.015)
Unconnected Firm	-0.015 *	-0.015 *	-0.005	-0.018 **	-0.009	-0.017
	(0.009)	(0.009)	(0.008)	(0.009)	(0.008)	(0.012)
Out-of-Sample Firm	-0.001	-0.003	0.000	-0.001	-0.005	-0.011
	(0.015)	(0.015)	(0.018)	(0.014)	(0.018)	(0.023)
Observations	57922601	55697201	57923303	57595436	57919273	49148828
Individual FE	X	X	X	X	X	X
Additional Controls	Industry-Period FE	Industry-Period FE	Occupation-Period FE	Industry-Occupation-Period FE	Industry-Period FE, Firm FE	Firm-Occupation-Period FE
Level of Occupation Codes	N/A	N/A	2-digit	2-digit	N/A	4-digit
Level of Industry Codes	4-digit	4-digit	N/A	4-digit	2-digit	N/A

Note: This table presents estimates of γ from equation 5. Outcomes vary by row; specifications vary by column. All regressions control for individual fixed effects and for the number of connections in an individual's network. Standard errors are two-way clustered at the individual and firm level. Coefficients are scaled by 10000, for readability. A transition is any observation where an individual is not where they were in the prior month: either at a different firm, or at no firm. A job-to-job transition occurs when the individual is, by the first of the month, at a new firm. Connected (unconnected) firms are those in the network sample where the individual has (does not have) a former coworker. Out of sample firms are firms with more than 1000 employees. Levels of significance: *10%, ** 5%, and *** 1%.

Table 5: Impact of Outside Options on Earnings

	Baseline	With Demographic Controls	Occupation-by-Time	Industry-Occupation- by-Time	Within-Firm Analysis: Baseline + Firm FE	Within-Firm Analysis: Firm-Occ - Period FE
	(1)	(2)	(3)	(4)	(5)	(6)
Δ Log Earnings	0.703 ***	0.694 ***	0.904 ***	0.624 ***	0.699 ***	1.581 ***
	(0.134)	(0.135)	(0.188)	(0.120)	(0.135)	(0.481)
	56134045	54689099	56134776	55806641	56130355	47695803
Δ Log Earnings (Narrow)	0.998 ***	0.975 ***	1.228 ***	0.915 ***	0.995 ***	2.209 ***
	(0.185)	(0.187)	(0.240)	(0.168)	(0.186)	(0.653)
	56153349	54707318	56154079	55825981	56149670	47711986
Δ Log Hours	0.197 ***	0.113 **	0.219 **	0.209 ***	0.197 **	0.044
	(0.076)	(0.055)	(0.099)	(0.073)	(0.077)	(0.166)
	50199730	49027776	50200508	49870846	50195900	43109059
Δ Log "Base Pay"	0.231 ***	0.224 ***	0.299 ***	0.224 ***	0.225 ***	0.307 **
	(0.056)	(0.054)	(0.066)	(0.054)	(0.057)	(0.122)
	55861195	54417929	55861927	55533703	55857498	47453513
Bonus/Base Pay	0.991 ***	1.104 ***	1.248 ***	0.778 **	0.994 ***	2.057 ***
	(0.307)	(0.356)	(0.357)	(0.303)	(0.303)	(0.731)
	57063082	54888239	57063791	56735772	57058999	48403944
Individual FE	X	X	X	X	X	X
Demographic Controls		X				
Additional Controls	Industry-Period FE	Industry-Period FE	Occupation-Period FE	Industry-Occupation-Period FE	Industry-Period FE, Firm FE	Firm-4-digit-Occupation-Period FE

Note: This table presents estimates of γ from equation 5. Outcomes vary by row; specifications vary by column. All regressions control for individual fixed effects and for the number of connections in an individual's network. Additional controls are indicated in the relevant column. Coefficients are scaled by 10000, for readability. Earnings are in kroner. Standard errors are two-way clustered at the individual and firm level. We provide more information about how we decompose the raw earnings measures into base pay and bonuses in Appendix C.4. Table A5 presents results for job-stayers. Levels of significance: *10%, ** 5%, and *** 1%.

Table 6: Impacts by Region of Former Coworker

	Mobility		Earnings	
	Job-to-Job	Connected Move	Change in Log Earnings	Change in Log Wages
	(1)	(2)	(3)	(4)
		A. Full Sample		
Same-Region Coworkers	1.140 ***	1.134 ***	0.470 ***	0.310 ***
	(0.216)	(0.214)	(0.128)	(0.076)
Different-Region Coworkers	0.275 ***	0.127 ***	0.025	0.022
	(0.053)	(0.046)	(0.046)	(0.043)
Observations	48806147	48806147	47367747	47145131
		B. Male Workers		
Same-Region Coworkers	1.383 ***	1.373	0.617 ***	0.327 ***
	(0.191)	(0.189)	(0.113)	(0.108)
Different-Region Coworkers	0.289 ***	0.133	0.029	0.062
	(0.059)	(0.049)	(0.059)	(0.058)
Observations	30394074	30394074	29396003	29254327
		C. Female Workers		
Same-Region Coworkers	0.894 ***	0.894 ***	0.287	0.280 ***
	(0.244)	(0.242)	(0.209)	(0.078)
Different-Region Coworkers	0.266 ***	0.127 ***	0.022	-0.031
	(0.053)	(0.049)	(0.052)	(0.048)
Observations	18410492	18410492	17970161	17889214

Note: This table presents estimates of γ^{IN} and γ^{OUT} from equation 6. Each regression controls for worker fixed effects, four digit industry-by-time fixed effects, and the number of connections an individual has in the same region and in other regions. We assign individuals to regions based on the location of their firm in the prior period. Coefficients are scaled by 10000, for readability. Earnings are in kroner. Individuals who do not have former coworkers in both their own region and outside regions are excluded, by design. Standard errors are two-way clustered by individual and firm. Levels of significance: *10%, ** 5%, and *** 1%. Some of the coefficients are plotted in Figure 9.

Table 7: Impacts by when An Individual Last Worked with the Coworker

		Job to Job Transition				Change in Log Earnings			
	Connections	Separate	Single Regression			Separate	Single Regression		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Former Coworkers									
1 Year Ago	22.2	3.522 ***	5.285 ***	5.281 ***		1.784 ***	2.357 ***	2.364 ***	
		(0.462)	(0.717)	(0.714)		(0.393)	(0.628)	(0.630)	
1-6 Months Ago	11.2	3.370 ***			4.794 ***	2.011 ***			2.847 ***
		(0.470)			(0.751)	(0.417)			(0.706)
7-12 Months Ago	11.0	2.003 ***			2.149 ***	1.085 ***			0.976 ***
		(0.312)			(0.440)	(0.245)			(0.369)
2-3 Years Ago	44.88	0.833 ***	0.789 **	0.761 **	0.829	0.552 ***	0.213	0.217	0.289
		(0.166)	(0.380)	(0.376)	(0.513)	(0.194)	(0.386)	(0.385)	(0.515)
4-5 Years Ago	61.76	0.804 **	0.871	0.878	0.929	0.532 **	0.909 ***	0.910 ***	0.710 *
		(0.354)	(0.607)	(0.598)	(0.801)	(0.223)	(0.338)	(0.338)	(0.418)
Future Coworkers									
1 Year From Now	23.85	0.725 ***		0.598 ***	0.606 ***	0.308 *		-0.062	-0.262
		(0.105)		(0.180)	(0.233)	(0.162)		(0.256)	(0.323)
2-3 Years From Now	56.06	-0.052		-0.880 ***	-1.286 ***	0.040		0.006	-0.052
		(0.073)		(0.194)	(0.261)	(0.185)		(0.299)	(0.355)
Observations		Varies	14670466	14670466	12373445	Varies	14325491	14325491	12109820

Note: This table presents estimates of γ^n from equation 7. Each row contains coefficients from a separate regression. Each regression controls for worker fixed effects, four digit industry-by-time fixed effects, and the number of connections in each included network. Standard errors are two-way clustered by individual and firm. We exclude the first two years and final three years of our regression sample so that network quality does not vary across years of our sample. Levels of significance: *10%, ** 5%, and *** 1%.

Table 8: Trade-Based Measures

	First Stage		Reduced Form			
			Transitions		Earnings	
	Log Exports	New Positions	Job to Job	Connected Firm	Δ Log Hourly Earnings	Δ Log Earnings
	(1)	(2)	(3)	(4)	(5)	(6)
A. Baseline: Industry-Time Controls						
Predicted Log Exports	0.306 ***	0.012 ***	0.161 *	0.183 **	0.291 **	0.386 ***
	(0.032)	(0.003)	(0.090)	(0.080)	(0.133)	(0.147)
	23730053	23730053	23730053	23730053	23109099	23186630
B. Within Firm: Firm-Occupation-Time Controls						
Predicted Log Exports	0.598 ***	0.014 ***	0.026	0.124 *	1.002 ***	1.021 **
	(0.131)	(0.004)	(0.070)	(0.065)	(0.386)	(0.417)
	20586717	20586717	20586717	20586717	20078092	20134138

Note: This table presents estimates of γ from equation 11. Outcome variables vary by column. The outcome variable in the first column is a measure of Ω_{it} based on realized measures of firms' exports. The outcome variable in the second column is our baseline measure Ω_{it}. The third and fourth columns present mobility results. The fifth and sixth columns present earnings results. Coefficients are scaled by 10000, for readability. Earnings are in kroner. Standard errors are two-way clustered by individual and firm. We provide details on how we construct $\Omega_{it}^{\text{trade}}$ in Appendix C. Levels of significance: *10%, ** 5%, and *** 1%. Note that the sample differs from other tables because a large fraction of workers do not have any coworkers in exporting firms.

Table 9: Heterogeneity by Occupation

	Manual	Assembly	Craftsman	Service/Sales	Office	Technician	Professional	Managers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	A. Coefficients							
Job-to-Job Mobility	0.897**	2.575**	1.087***	0.816***	1.302***	1.612***	2.137***	0.915***
	(0.396)	(1.002)	(0.229)	(0.180)	(0.235)	(0.278)	(0.470)	(0.311)
Δ Log Earnings	0.366	0.344	0.151	0.643***	0.280*	0.888***	1.368***	0.743***
	(0.266)	(0.267)	(0.208)	(0.228)	(0.146)	(0.201)	(0.247)	(0.224)
	B. Scaled Impact							
Scaled by Annual Earnings	\$16	\$17	\$8	\$28***	\$14*	\$54***	\$91***	\$67***
	(\$12)	(\$13)	(\$9)	(\$10)	(\$7)	(\$12)	(\$16)	(\$20)
	5284780	5585636	9552430	5753531	7218323	10473806	11960350	4038416

Note: This table shows mobility and wage responses differ across occupations. We group workers according to broad ISCO (International Standard Classification of Occupations) codes. We then estimate equation 5 separately within each occupation. Each regression controls for individual fixed effects, four-digit industry-by-time fixed effects and a linear control for the number of connections in an individual's network. Standard errors are two-way clustered by individual and firm. Coefficients are scaled by 10000, for readability. Earnings outcomes in Panel A are in kroner. We do not have sufficient data on workers in the military (ISCO 10) or in agricultural occupations (ISCO 6). Panel B estimates the impact of a 10 unit change in Ω_{it} on a worker's annual earnings (in dollars). We calculate annual earnings separately for each group. Levels of significance: *10%, ** 5%, and *** 1%. Figure 16 plots coefficients similar to those in Panel A, for each sex.

Table 10: Parameters

Parameters	Description	Manual	Professionals
β	Bargaining Power	.8733	0.8008
$1 - p_R$	Fraction of Offers from Renegotiating Firms	.3094	0.5125
δ	Exogenous Job Destruction Rate	0.0246	0.0269
λ_U	Outside Offer Arrival Rate for Unemployed Workers	0.0374	0.0593
α_U	Connected-Offer Arrival Rate for Unemployed Workers	0.0009	0.0068
λ_E	Outside Offer Arrival Rate for Employed Workers	0.0170	0.0291
α_E	Connected-Offer Arrival Rate for Employed Workers	0.0007	0.00002
$b, \sigma_P, \sigma_R, \mu_P, \mu_R$	Other parameters	—	—

Note: The table above displays the parameters that we estimate in Section 7. We allow these parameters to vary by skill group. We do not estimate the discount rate ρ, but instead fix it at $1/(1 + .005) \approx .995$.

Table 11: Moments

Number	Description
3	Mean transition rates: J2J, U2E, E2U
2	Mean and variance of (residual) log wage changes for job-stayers
1	Mean log wages
1	Residual variance of log wages
2	Mean and variance of (residual) log wage changes
1	Mean log wage gain associated with a job to job transition
2	Regression coefficients for mobility: J2J and U2E
2	Regression coefficients for earnings of job-stayers: $\Delta \log y$ and $1\{\Delta \log y > 0\}$

Note: This table lists the moments used to estimate the model in Section 7. We include a set of moments for each distinct labor market we consider.

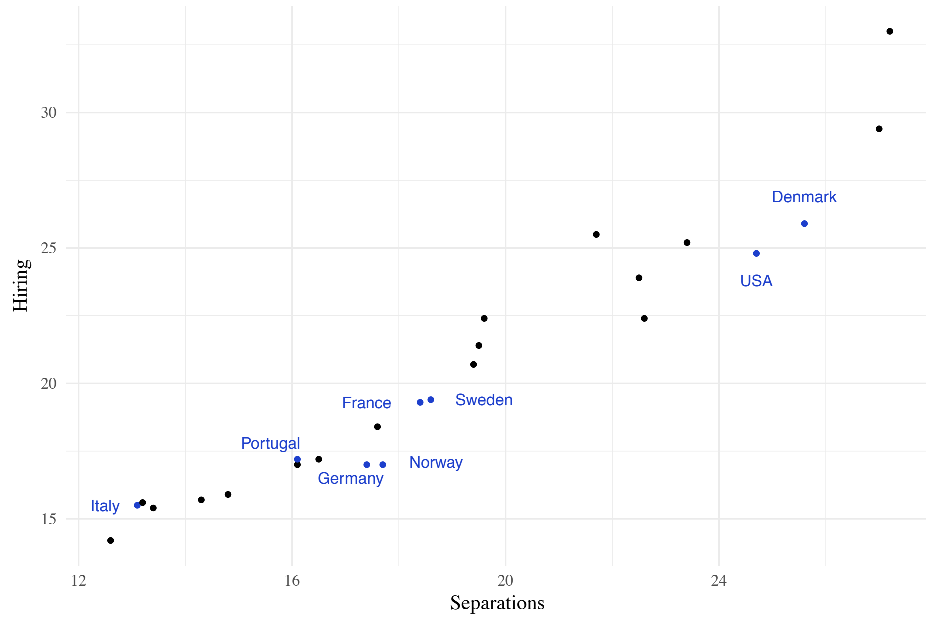
Table 12: Impact of a Decreased Arrival Rate

Group	% ΔMobility	%Δ Wages	% Δ Wage Growth
Professionals	-41%	-.8%	-32%
Manual Skills	-46%	-.3%	-43%

Note: This table shows how a reduction in the job arrival rate influence wages and wage growth for two main groups of workers: professionals and manual skilled workers.

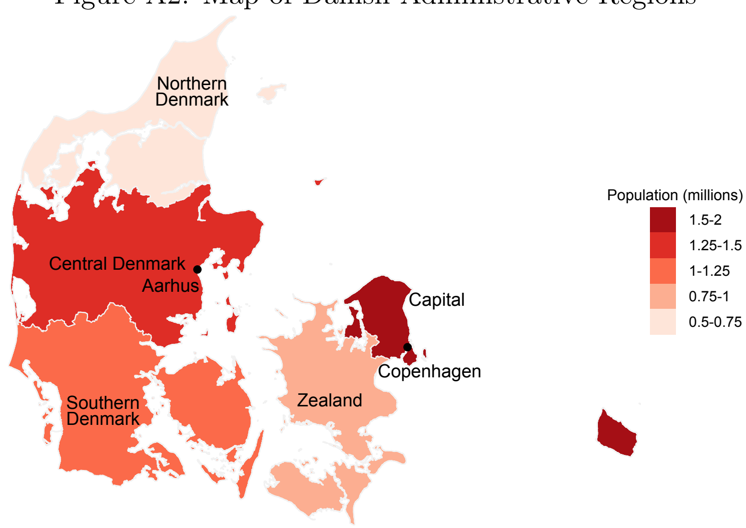
A Appendix Tables and Figures

Figure A1: Labor Market Flexibility in OECD Countries



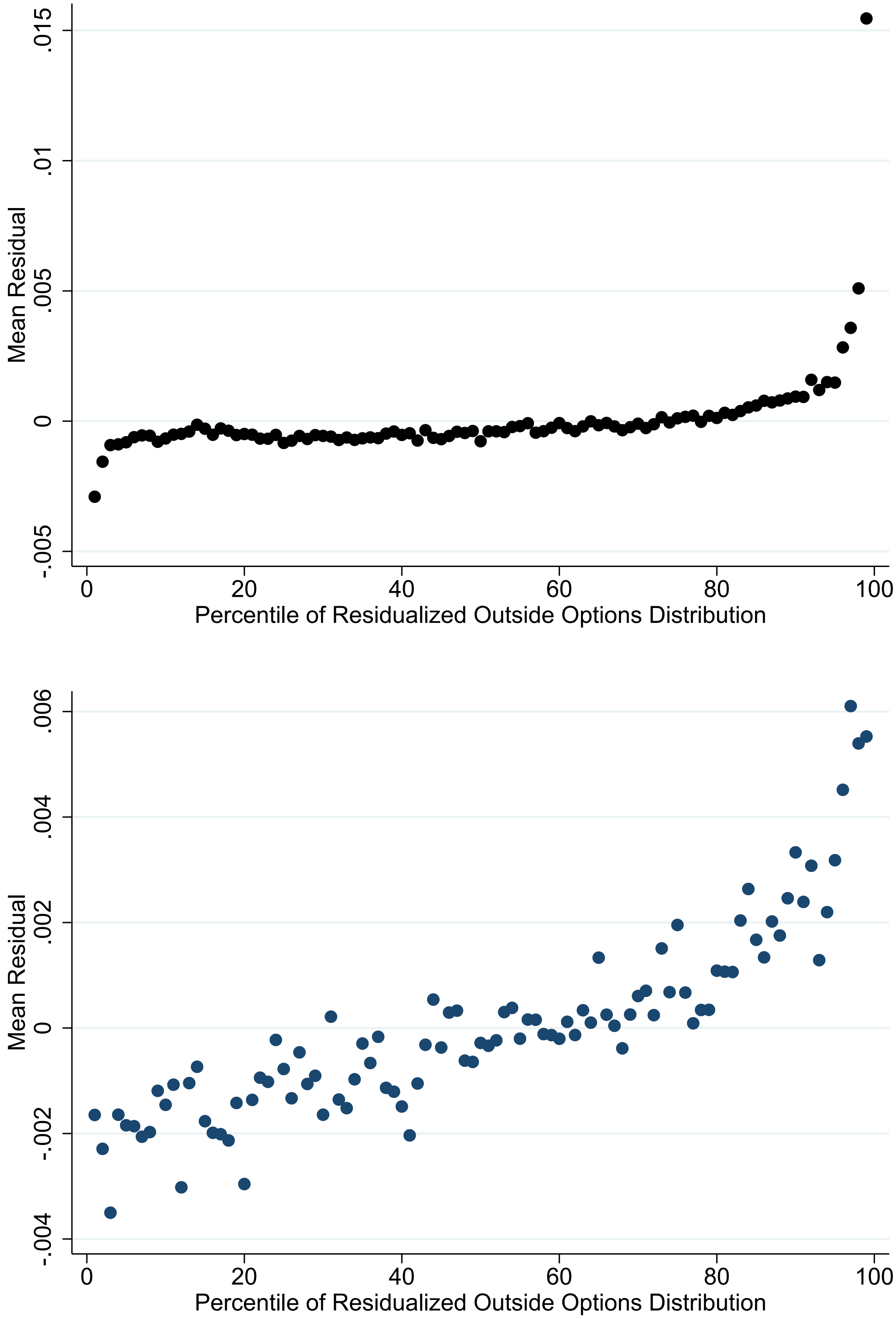
Note: This figure plots mean hiring and separation rates for OECD countries using data from OECD (2004). The original data are adjusted for industrial composition. The years used vary by country. For more details, see OECD (2004).

Figure A2: Map of Danish Administrative Regions



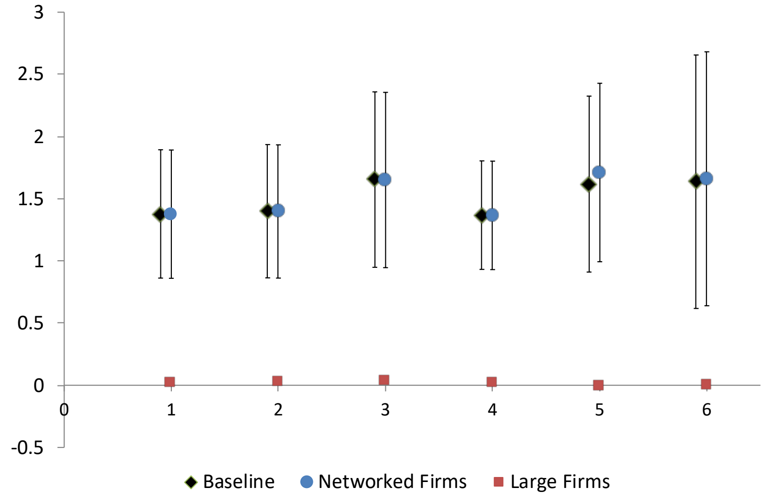
Note: This figure shows the five administrative regions in Denmark. We construct same-region and different-region coworker networks on the basis of these regions. The population data are taken from Statistic Denmark's Statbank.

Figure A3: Robustness: Controlling for Occupation-Time FE



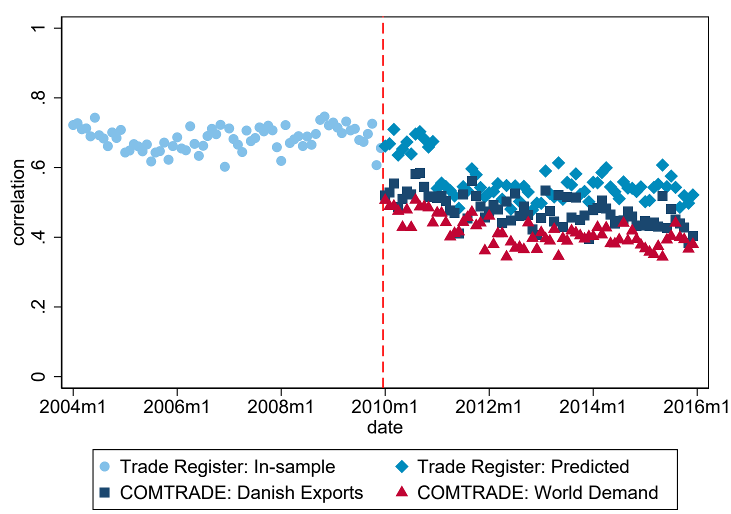
Note: This figure shows how the probability of making a transition or the average change in monthly earnings depends on Ω_{it}. We first residualize both the dependent variables and Ω_{it} on individual fixed effect and industry-by-time and occupation-by-time fixed effects.

Figure A4: Robustness: Value of Connections at Larger Firms



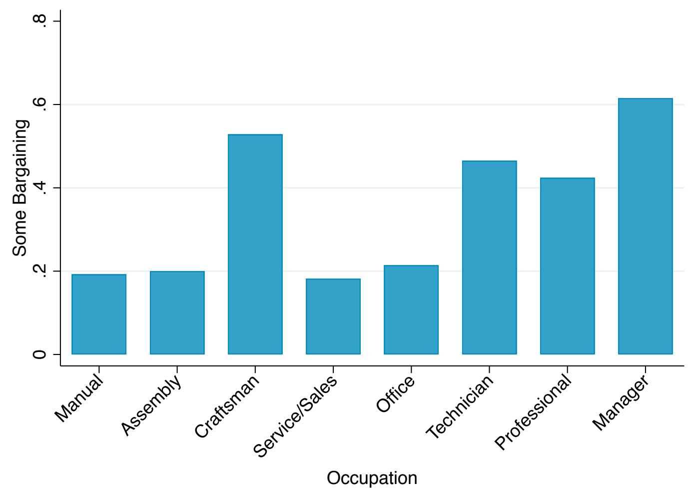
Note: This figure shows that adding measures of Ω_{it} based on connections at large firms (more than 1000 employees) does not change our estimates of γ. The outcome variable is an indicator for whether the worker made a job-to-job transition and coefficients are scaled as in Figure 7. The six specifications correspond to those in Table 4. The black dot presents our baseline estimates. The blue dots show how the estimate of γ changes when we include measures based on connections at large firms (more than 1000) employees as a separate regressor ($\Omega_{it}^{\text{large}}$). The red squares show the coefficient on $\Omega_{it}^{\text{large}}$ in this regression. Standard errors two-way are clustered by worker and firm.

Figure A5: Quality of Trade Prediction



Note: This figure assesses the quality of our firm-level predicted trade measures. We use data from 2004-2009 to fix each firm's share of total Danish exports of each product. The light blue dots show the correlation between the predicted exports–based on total Danish exports of each product as reported in the administrative register– and the firm's actual exports. The dark blue dots show the correlation between the actual exports and those predicted using Danish exports in COMTRADE. The red triangles show the correlation between actual firm exports those predicted using world (minus Denmark) exports in COMTRADE. This is the measure used in $\Omega_{it}^{\text{trade}}$. More details are provided in Section C.7.

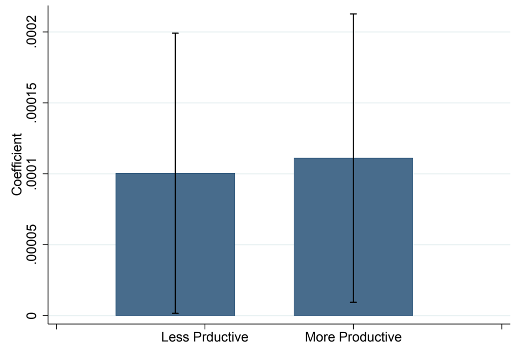
Figure A6: Posting and Bargaining in the United States: Hall and Krueger (2012)



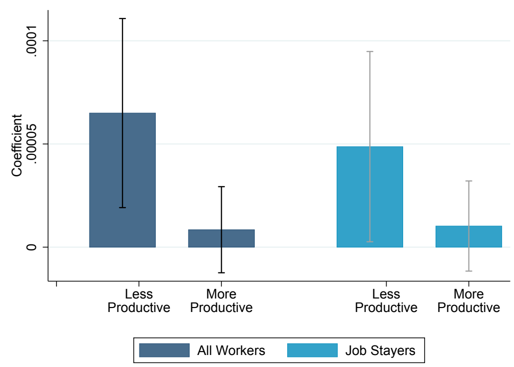
Note: This figure uses survey data from Hall and Krueger (2012) to plot the mean fraction of workers in each occupation group who engage in bargaining at the start of a job spell. More details are provided in Section D.3.

Figure A7: Impacts by Quality of Outside Option

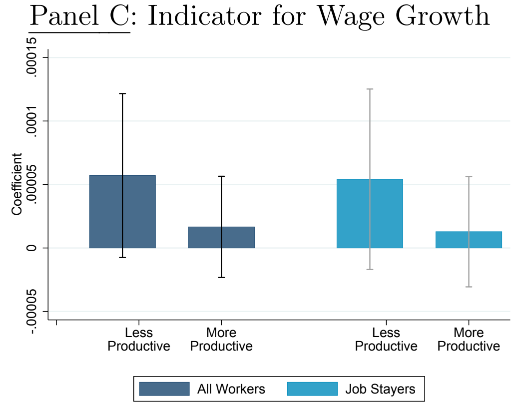
Panel A: Mobility



Panel B: Wage Growth



Panel C: Indicator for Wage Growth



Note: This figure shows how mobility and wage responses differ based on the source of the outside option. We group into twenty vigintiles by their mean value added per worker (in real 2016 kroner) over the sample period. We have these data for roughly three quarters of the firms in our sample. We then estimate equation 14 and plot estimates of γ^{ABOVE} and γ^{BELOW}. Panel A presents estimates where the outcome is an indicator for whether the individual made a job to job transition. Panel B shows results for changes in log monthly earnings; Panel C shows results for whether there was an earnings change. Each regression controls for the number of connections in each network and for individual and four-digit industry-by-time fixed effects. Standard errors are two-way clustered by individual and firm.

Table A1: Constructing Network Sample

	Workers (1)	Worker-Months (2)
Spells Covering 1st of Month	3809303	248252752
Danish Workers	3295211	225944464
Work at a Firm of <=1000 Workers At Least Once	2785351	190373936
Between 25 and 60 Years Old	2111834	142660144
Single Job-Holder	1842082	126885936
At a Firm of <=1000 Workers	1096764	60491824

Note: This table shows how each of our sample restrictions changes the number of observations and individuals in our data. Each row represents an additional restriction, relative to the row above. These restrictions are described in Section 3.3 and in Appendix C.

Table A2: Network Dispersion Across Regions

	Capital Region (1)	Central Denmark (2)	North Denmark (3)	Zealand Region (4)	Southern Denmark (5)
Capital Region	73%	8%	3%	9%	8%
Central Denmark	15%	65%	6%	3%	11%
North Denmark	11%	15%	65%	2%	7%
Zealand Region	32%	6%	2%	53%	7%
Southern Denmark	15%	12%	3%	3%	66%

Note: This table shows the dispersion of an individual's coworker network across different regions of network. The rows indicate the individual's region of work. The columns indicate the region each of their coworkers lives in. Each row sums to 100%.

Table A3: Transition Rates

	All (1)	Men (2)	Women (3)	College (4)	No College (5)
Make a Job to Job Transition	1.0%	1.0%	0.9%	1.0%	0.9%
		<u>Types of Job to Job Transitions</u>			
Connected Firm	57.4%	59.9%	52.9%	50.6%	60.3%
Connected Industry	12.8%	13.7%	11.2%	12.2%	13.1%
Unconnected Firm	12.1%	12.3%	11.6%	11.8%	12.2%
Out-of-Sample firm	17.7%	14.1%	24.3%	25.5%	14.4%

Note: This table shows the raw probability an individual would make each type of transition each period. These transitions are defined in Appendix C.

Table A4: Autocorrelation of Network Characteristics

	Lag											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Number of Connections	0.993	0.987	0.980	0.973	0.967	0.960	0.952	0.942	0.933	0.923	0.913	0.904
Female	0.987	0.977	0.970	0.962	0.955	0.948	0.941	0.935	0.928	0.921	0.915	0.909
College+	0.987	0.978	0.970	0.963	0.956	0.950	0.943	0.936	0.930	0.923	0.917	0.911
Age	0.981	0.967	0.956	0.945	0.935	0.926	0.916	0.906	0.896	0.886	0.877	0.868
Characteristics of Coworkers' Firms												
Mean Value Added (Time-varying)	0.121	0.105	0.058	0.078	0.024	0.033	0.034	0.029	0.025	0.513	0.025	0.039
Mean Hourly Earnings (Time-varying)	0.908	0.869	0.847	0.822	0.802	0.786	0.764	0.748	0.743	0.726	0.711	0.708
Mean Fraction Female	0.970	0.950	0.936	0.923	0.912	0.903	0.893	0.885	0.877	0.869	0.862	0.856

Note: This table shows that the characteristics of an individual's network remain stable over time. The first row looks at the number of coworkers in the network. The remaining rows look at the correlation between the average (across all coworkers) characteristics of an individual's network in a given month.

Table A5: Impact of Outside Options on Earnings: Job Stayers

	All				Full-Time			
	Baseline		Within Firm-Occupation		Baseline		Within Firm-Occupation	
	(1)		(2)		(3)		(4)	
Δ Log Earnings	0.688	***	1.520	***	0.719	***	1.673	***
	(0.137)		(0.490)		(0.171)		(0.461)	
	54266264		46185273		15280379		12549605	
Δ Log Earnings (Narrow)	0.732	***	1.794	***	0.745	***	1.691	***
	(0.144)		(0.551)		(0.174)		(0.465)	
	54284385		46200647		15280506		12549728	
Δ Log Hours	0.101		-0.118					
	(0.516)		(0.130)					
	48718166		41905015					
Δ Log "Base Pay"	0.239	***	0.253	**	0.085	***	0.103	
	(0.056)		(0.123)		(0.033)		(0.069)	
	54001519		45950270		15281563		12549852	
Bonus/Base Pay	1.104	***	2.353	***	1.229	***	2.111	***
	(0.302)		(0.810)		(0.466)		(0.771)	
	54571865		46406029		15290342		12557319	
Individual FE	X		X		X		X	
Additional Controls	Industry-Period FE		Firm-Occupation-Period FE		Industry-Period FE		Firm-Occupation-Period FE	

Note: This table presents estimates of γ from equation 5. Outcomes vary by row; specifications vary by column. All regressions control for individual fixed effects and for the number of connections in an individual's network. Additional controls are indicated in the relevant column. Coefficients are scaled by 10000, for readability. Earnings are in kroner. Standard errors are two-way clustered at the individual and firm level. We explain how we decompose raw earnings measures into base pay and bonuses in Appendix C.4. The sample includes only job stayers: those who are at the same firm as in the prior month. Table 5 presents results for all workers. Levels of significance: *10%, ** 5%, and *** 1%.

Table A6: Impacts on U2E Transitions

	>=1 Month (1)		>=2 Months (2)		>=3 Months (3)	
1 Year Ago	28.748	***	32.039	***	38.461	***
	(2.315)		(2.464)		(3.355)	
2-3 Years Ago	10.662	***	11.184	***	15.999	***
	(1.532)		(2.107)		(4.005)	
4-5 Years Ago	12.033	***	12.314	***	9.333	**
	(1.586)		(2.085)		(3.756)	
Observations	611809		392682		174068	

Note: This table presents estimates of γ from equation 7 for workers who are not currently at a firm. Each column presents a separate regression. In addition to the listed covariates, each regression controls for individual and four-digit industry-by-time fixed effects, and for the number of connections in each included network. The first column contains all individuals who were non-employed in the prior month; the remaining columns condition on remaining non-employed for 2 or 3 months. Standard errors are two-way clustered by individual and firm. Levels of significance: *10%, ** 5%, and *** 1%.

Table A7: Impact on Mobility: Alternative Measures of Outside Options

	Positions				Hires			
	Baseline (1)		Weighted (2)		Unweighted (3)		Weighted (4)	
Any Transition	1.255	***	0.539	***	1.731	***	0.640	***
	(0.255)		(0.175)		(0.312)		(0.180)	
Job-to-Job	1.378	***	0.570	***	1.800	***	0.658	***
	(0.263)		(0.175)		(0.318)		(0.180)	
Connected Firm	1.408	***	0.579	***	1.792	***	0.658	***
	(0.263)		(0.175)		(0.315)		(0.179)	
Unconnected Firm	-0.014		-0.005		-0.009		-0.004	
	(0.010)		(0.003)		(0.009)		(0.004)	
Out-of-Sample Firm	-0.001		-0.001		0.013		0.002	
	(0.015)		(0.006)		(0.015)		(0.005)	
Observations	57922601		57922601		57922601		57922601	

Note: This table presents estimates of γ from equation 5 for different measures of Ω_{it}. The rows correspond to different mobility outcomes. Each regression controls for individual and four-digit industry-by-time fixed effects. Coefficients are scaled by 10000, for readability. Standard errors are two-way clustered by individual and firm. Levels of significance: *10%, ** 5%, and *** 1%.

Table A8: Impact on Earnings: Alternative Measures of Outside Options

	Positions				Hires			
	Baseline		Weighted		Unweighted		Weighted	
	(1)		(2)		(3)		(4)	
Δ Log Earnings	0.703	***	0.243	***	0.147	**	0.069	**
	(0.134)		(0.052)		(0.072)		(0.031)	
Δ Log Earnings (Narrow)	0.998	***	0.333	***	0.488	***	0.164	***
	(0.185)		(0.067)		(0.139)		(0.047)	
Δ Log Base Pay	0.231	***	0.070	***	0.100	***	0.035	**
	(0.056)		(0.018)		(0.038)		(0.015)	
Bonus/Base Pay	0.991	***	0.361	***	0.334		0.134	
	(0.307)		(0.095)		(0.329)		(0.094)	
Observations	56134045		56134045		56134045		56134045	

Note: This table presents estimates of γ from equation 5 for different measures of Ω_{it}.. The rows correspond to the earnings outcomes described in Section C.4. Each regression controls for individual and four-digit industry-by-time fixed effects. Coefficients are scaled by 10000, for readability. Earnings are in kroner. Standard errors are two-way clustered by individual and firm. Levels of significance: *10%, ** 5%, and *** 1%.

Table A9: Impact on Mobility: Robustness to Alternate Network Definitions

	Baseline	Alternative Network Definitions: Past Two Years	Past Five Years	Past Three Years
	(1)	(2)	(3)	(4)
Any Transition	1.255 ***	1.735 ***	1.220 ***	3.195 ***
	(0.255)	(0.269)	(0.269)	(0.554)
Job-to-Job	1.378 ***	1.925 ***	1.401 ***	3.542 ***
	(0.263)	(0.274)	(0.274)	(0.545)
Connected Firm	1.408 ***	1.993 ***	1.405 ***	3.691 ***
	(0.263)	(0.270)	(0.270)	(0.543)
Connected Industry	-0.014	-0.008	-0.001	-0.043 **
	(0.010)	(0.011)	(0.011)	(0.021)
Unconnected Firm	-0.015 *	-0.051 ***	-0.010	-0.040 **
	(0.009)	(0.010)	(0.010)	(0.019)
Out-of-Sample Firm	-0.001	-0.009	0.006	-0.066 ***
	(0.015)	(0.018)	(0.018)	(0.023)
Firm-Size Cutoff	1000	1000	1000	500
Observations	57922601	58819990	44047438	57769832

Note: This table presents estimates of γ from equation 5 using different network definitions. The rows correspond to different mobility outcomes. Each regression controls for individual and four-digit industry-by-time fixed effects. Standard errors are two-way clustered by individual and firm. Coefficients are scaled by 10000, for readability. Levels of significance: *10%, ** 5%, and *** 1%.

Table A10: Impact on Earnings: Robustness to Alternate Network Definitions

	Baseline	Alternative Network Definitions: Past Two Years	Past Five Years	Past Three Years
	(1)	(2)	(3)	(4)
Δ Log Earnings	0.703 ***	1.362 ***	0.777 ***	1.736 ***
	(0.134)	(0.132)	(0.132)	(0.240)
Δ Log Earnings (Narrow)	0.998 ***	1.847 ***	1.050 ***	2.444 ***
	(0.185)	(0.186)	(0.186)	(0.399)
Δ Log Base Pay	0.231 ***	0.300 ***	0.373 ***	0.464 ***
	(0.056)	(0.063)	(0.063)	(0.106)
Bonus/Base Pay	0.991 ***	2.209 ***	0.929 ***	3.481 ***
	(0.307)	(0.354)	(0.354)	(1.065)
Firm Size Cutoff	1000	1000	1000	500
Observations	56134045	57002153	42723997	55986364

Note: This table presents estimates of γ from equation 5 using different network definitions. The rows correspond to the earnings outcomes described in Section C.4. Each regression controls for individual and four-digit industry-by-time fixed effects. Standard errors are two-way clustered by individual and firm. Coefficients are scaled by 10000, for readability. Earnings are in kroner. Levels of significance: *10%, ** 5%, and *** 1%.

Table A11: Mobility: Exploiting Annual Data

	Change in Employment		Hires		Pct Change in Employment		Positions, Scaled by VA	
	(1)		(2)		(3)		(4)	
Job-to-Job	0.031	***	0.044	***	0.014	**	13.229	***
	0.012		0.017		0.007		4.462	
Connected Firm	0.044	***	0.071	***	0.026	***	16.829	***
	0.012		0.018		0.006		3.547	
Connected Industry	-0.008	**	-0.013	**	-0.003		-0.675	
	0.003		0.005		0.002		1.310	
Unconnected Firm	-0.004		-0.014	**	-0.009	***	-2.924	**
	0.004		0.006		0.002		1.308	
Observations	4205495		4205495		4203842		4205495	

Note: This table replicates our main mobility results using annual data from the IDA (the integrated database for labor market research). Coefficients are scaled by 10000, for readability. Earnings are in kroner. More details are provided in Appendix D.

Table A12: Earnings: Exploiting Annual Data

	Change in Employment		Hires		% Change in Employment		Positions, Scaled by VA	
	(1)		(2)		(3)		(4)	
Δ Log Earnings	0.028	***	0.028	***	0.007	**	2.651	
	(0.007)		(0.010)		(0.003)		(2.040)	
Observations	2948997		2948997		2947863		2948997	
Δ Log Daily Earnings	0.013	**	0.003		0.004		8.315	***
	(0.006)		(0.008)		(0.003)		(1.932)	
Observations	2183376		2183376		2182526		2183376	

Note: This table replicates our main earnings results using annual data from the IDA (the integrated database for labor market research). Coefficients are scaled by 10000, for readability. More details are provided in Appendix D.

Table A13: Results By Relative Productivity of Outside Firm

	Full Sample						Job Stayers		
	Job to Job Transition (1)		Change in Log Earnings (2)		1{Δ Log Earnings>0} (3)		Change in Log Earnings (4)		1{Δ Log Earnings>0} (5)
					A. Baseline				
More Productive	1.131	**	0.089		0.159		0.103		0.127
	(0.526)		(0.107)		(0.204)		(0.111)		(0.222)
Less Productive	1.029	**	0.655	***	0.561	*	0.490	**	0.539
	(0.504)		(0.234)		(0.329)		(0.235)		(0.363)
Observations	22361250		21639845		22361250		20863189		21309619
					B. Controlling for Same-Vigintile Impact				
More Productive	1.110	**	0.085		0.166		0.102		0.128
	(0.519)		(0.106)		(0.203)		(0.111)		(0.222)
Less Productive	1.004	**	0.650	***	0.571	*	0.487	**	0.541
	(0.504)		(0.234)		(0.329)		(0.235)		(0.363)
Observations	22361250		21639845		22361250		20863189		21309619

Note: This table shows mobility and wage responses differ based on the productivity of the outside firm. We group firms into vigintiles based on their mean value added per worker (in real terms) over the sample period. We are able to do this for roughly 75% of the firms in our dataset. We then construct measures of Ω_{it} using only firms from higher and lower productivity firms (with strict equality). Panel A presents estimates of γ^{ABOVE} and γ^{BELOW} from equation 14. Panel B adds controls for Ω_{it} based on coworkers in the same vigintile. Because not all individuals have coworkers in the same vigintile, we replace missing values with 0's, and include a dummy for whether an individual has any coworkers in the same vigintile. Each regression controls for individual fixed effects, four-digit industry-by-time fixed effects, vigintile fixed effects, and a linear control for the number of connections in an individual's network. Standard errors are two-way clustered by individual and firm. Coefficients are scaled by 10000, for readability. Levels of significance: *10%, ** 5%, and *** 1%.

Table A14: Model Fit

Description	Target (full sample)	Model (full sample)
J2J Transition Rate	0.0071	.0126
U2E Transition Rate	0.0374	0.0389
E2U Transition Rate	0.0189	0.0198
Mean log monthly earnings (kroner)	10.45	11.27
Residual variance of log monthly earnings	.1623	.6354
Mean change in log earnings	0.0015	.0085
Variance of log earnings changes	0.1129	0.1124
Mean log earnings gain during J2J transition	0.141	0.162
γ^{J2J}: Regression coefficient for J2J mobility	0.000118	0.000159
γ^{U2E}: Regression coefficient for U2E mobility	.000169	.000879
Regression coefficient for earnings of job-stayers: $\Delta \log y$	.00000406	.0000106
Regression coefficient for earnings of job-stayers: $1\{\Delta \log y > 0\}$	0.000027	0.000134

Note: This table examines the fit of our estimates for model described Section 7. Note that in order to comply with Statistics Denmark's privacy regulations, we do not present quantiles. Standard errors are a work in progress.

B Theoretical Appendix

In this section we provide the proofs referenced in the text. Many of the results follow those in Flinn and Mullins (2017).

B.1 Main Text Proofs

Lemma. *1 A worker who receives the total surplus created by the match θ at a renegotiating firm (type R) has the same value as a worker earning θ at a posting firm (type P). That is,*

$$T_R(\theta) = V_P(\theta)$$

Proof. This proof follows that in Flinn and Mullins (2017). Let Φ be the endogenous distribution of offers from posting firms. A worker who is at a posting firm earning w has the following value function:

$$\begin{aligned}
(\rho+\delta)V_N(w) \quad &= \quad w + \underbrace{\lambda_E p_R \int \beta\left[T_R(x) - V_N(w)\right]^+ dF_\theta(x)}_{\text{better offer: renegotiating firm}} \\
&\quad + \underbrace{\lambda_E(1-p_R) \int \left[V_N(x) - V_N(w)\right]^+ d\Phi(x)}_{\text{better offer: posting firm}} \\
&\quad + \underbrace{\delta V_U}_{\text{unemployment}}
\end{aligned}$$

The total surplus created by a match θ at a bargaining firm is similar.

$$\begin{aligned}
(\rho+\delta)T_R(\theta) \quad &= \quad \theta + \underbrace{\lambda_E p_R \int \beta\left[T_R(x) - T_R(\theta)\right]^+ dF_\theta(x)}_{\text{better offer renegotiating firm}} \\
&\quad + \underbrace{\lambda_E(1-p_R) \int \left[V_N(x) - T_R(\theta)\right]^+ d\Phi(x)}_{\text{better offer posting firm}} \\
&\quad + \underbrace{\delta V_U}_{\text{unemployment}}
\end{aligned}$$

This period the match produces θ. With probability $\lambda_E p_R \int \beta\left[T_R(x) - T_R(\theta)\right]^+ dF_\theta(x)$ the worker gets an offer from a bargaining firm with higher overall match surplus. The probability depends on the arrival rate, the fraction of posting firms, and the density of offers that are better. When this occurs, the worker leaves the firm. She is able to use $T_R(\theta)$ as her outside option, and gains a fraction β of the match surplus. Because there is free entry, the firm's value is 0. With probability $\lambda_E(1-p_R) \int \left[V_N(x) - T_R(\theta)\right]^+ d\Phi(x)$ the worker receives a more attractive offer from a posting firm. Her new value at that firm is simply $V_N(\theta)$. Again, the firm's value is zero, by free entry. Both equations will hold if $V_N(\theta) = T_R(\theta)$. $\square$

B.2 Firm Value Functions

If a vacancy of match quality θ offers wage w, the expected discounted profit is the probability the vacancy is filled, multiplied by the discounted stream of profits. The probability the vacancy is filled is:

$$\mathscr{H}(w) = \underbrace{\frac{\lambda_U M_U}{\lambda_U M_U + \lambda_E M_E}}_{\text{unemployed}} \times \underbrace{1}_{\text{accept offer}} + \underbrace{\frac{\lambda_E M_U}{\lambda_U M_U + \lambda_E M_E}}_{\text{employed}} \times \underbrace{G(w)}_{\text{accept offer}}$$

If the firm meets an unemployed worker, the vacancy is filled with probability 1. The firm meets such a worker with probability $\frac{\lambda_U M_U}{\lambda_U M_U + \lambda_E M_E}$. The numerator of this expression is the search intensity of unemployed workers. The denominator is the aggregate search intensity. The worker meets an employed worker with probability $1 - \frac{\lambda_U M_U}{\lambda_U M_U + \lambda_E M_E}$.. The offer is accepted only if the worker is currently at a firm that would be willing to pay her at most w. We use $G(w)$ to denote this function.

The expected discounted profits associated with the offer are $\theta - w$, divided by the expected length of the match. This is $\rho + \delta + \lambda_E \left\{ f_R \tilde{F}_\theta(\theta) + (1 - f_r)\tilde{\Phi}(\theta) \right\}$. The match is exogenously dissolved at rate δ. The final term measures the probability of an endogenous separation.

We use $J(\theta, w)$ to denote the value to a firm of opening this type of vacancy and paying w:

$$J(w, \theta) = \mathscr{H}(w) \times \frac{\theta - w}{\rho + \delta + \lambda_E \left\{ f_R \tilde{F}_\theta(\theta) + (1 - f_R)\tilde{\Phi}(\theta) \right\}}$$

The firm chooses w to maximize this expression. A key contribution of Flinn and Mullins (2017) is to show that, in this setting, the firm's optimal wage offer is given by a deterministic function $\omega(\theta)$ (denoted $\varphi(\theta)$ in that paper) that is monotonically increasing in θ, lower semi-continuous, and almost everywhere differentiable. We do not repeat the proofs here, and direct the reader to that paper. that paper also derives the differential equation that describes this wage offer function.

It is important to note that when we take the model to the data, we simply allow the offers from posting and bargaining firms to come from different distributions. We do not directly estimate the link between the productivity of posting firms and the wages that they post.

B.3 Closing the Model

Flinn and Mullins (2017) close the model by assuming that posting and bargaining firms face different costs of posting a vacancy, and that the costs of posting a vacancy are increasing in the measure of each type. However, for some values of costs, there is no solution.

We take a different approach. We assume that when a firm wants to open a vacancy, it takes a random draw from the (exogenously given) productivity distribution. With probability $1 - p_R$, the firm can post wages for the vacancy. With probability p_R the firm has to bargain with workers. This probability is exogenously given. When we estimate the model, we allow this to vary across skill groups.

This is somewhat simpler than the set-up in Flinn and Mullins (2017) because it allows for a single free-entry condition. We assume the marginal cost of posting a vacancy is c. Then the free entry condition is given by:

$$\begin{aligned} c &= q(\kappa)p_R \int_{\theta^*}^{\theta^{\max}} \int_{\theta^*}^{\theta^{\max}} (1-\beta)\left[T_R(\theta) - T(x)\right]^+ dF_\theta d\hat{G}(x) \\ &\quad + q(\kappa)(1-p_R) \int_{\theta^*}^{\theta^{\max}} J(\theta, \omega(\theta)) dF_\theta d\hat{G}(x) \end{aligned}$$

Firms enter until the cost of posting a vacancy is equal to the expected benefit. This depends on $q(\kappa)$, the rate at which workers meet vacancies, and on the expected rents associated with the match. The rate at which workers meet vacancies depends on the total number of vacancies, and the form of the matching function. When wages are set by bargaining, the firm gets to keep $(1-\beta)$ of the difference between the rents produced in the match, and the rents produced by the hypothetical match the worker used for bargaining. When wages are posted, the firm's value function is $J(\theta, \omega(\theta))$ as defined above.

B.4 Equilibrium

The equilibrium of the model is characterized by a set of flow equations and three steady state distributions:

1. the distribution of workers across renegotiating firms $G(x)$
2. the distribution of 'last best' offers for workers at each type of renegotiating firm $H(q|x)$
3. the distribution of workers across posting firms

Each of these can be derived using the relevant balance condition. We follow Flinn and Mullins (2017) to derive the equilibrium distributions. We omit worker ability for simplicity.

Unemployment

In equilibrium, the flow rates in and out of unemployment must balance. Each period a fraction δ of workers are displaced from their jobs. Using M_U to denote the fraction of workers who are unemployed, we can write

$$\begin{aligned} \underbrace{\delta(1-M_U)}_{\text{Entering}} &= \underbrace{M_U\left(\lambda_U\left(p_R\left[1-F_\theta(\theta^*)\right] + (1-p_R)\left[1-\Phi(\theta^*)\right]\right)\right)}_{\text{Exiting}} \\ M_U &= \frac{\delta}{\delta + \lambda_U\left(p_R\left[1-F_\theta(\theta^*)\right] + (1-p_R)\left[1-\Phi(\theta^*)\right]\right)} \end{aligned}$$

Each period, a mass δ of workers who were unemployed last period, become unemployed. The probability a worker who was unemployed last period becomes employed is the probability she meets a vacancy, multiplied by the probability that vacancy exceeds the value of unemployment.

Contact Rates

In equilibrium, the contact rates are determined via a standard matching function. Unemployed workers come across a vacancy with probability λ_U and employed workers come across a vacancy with probability λ_E.

We define

$$\kappa = \frac{v}{\lambda_U M_U + \lambda_E M_E}$$

to be the market tightness measure. This is the ratio of the number of vacancies to the (search-intensity weighted) number of searchers. If we assume the matching function is Cobb-Douglass, $\lambda_U = \kappa^\gamma$ and the rate at which workers arrive at vacancies is $q(\kappa) = \kappa^{\gamma-1}$.

Distribution of Workers Across Firm Types

We can derive the distribution of workers across firm type by looking at the steady state relationship. $G(x)$ measures the fraction of workers at firms of match quality x or below.

$$\begin{aligned} dG(x) &= \underbrace{M_E G(x)}_{(1)} \left\{ \underbrace{\delta + \lambda p_R \tilde{F}_\theta(x) + \lambda(1-p_R)\tilde{\Phi}(x)}_{(2)} \right\} - \\ &\quad \underbrace{M_U \lambda}_{(3)} \left\{ \underbrace{p_R \left(F_\theta(x) - F_\theta(b)\right) + (1-p_R)\left(\Phi(x) - \Phi(b)\right)}_{(4)} \right\} \end{aligned}$$

The first line measures the flow out of these firms. The measure of workers currently at firms of quality x or lower is given by expression (1): the mass of employed workers multiplied by the fraction at these types of firms. The second term gives us the probability a worker is no longer at one of these firms. This can occur if they are displaced and move into unemployment, or if they receive an offer from a better firm.

The second line measures the flow into these firms. No workers at higher quality firms will ever flow into these firms (directly). The inflow is the product of (3): the measure of unemployed workers who get a job offer and (4): the probability that, conditional on receiving an offer, it is more attractive than unemployment.

In equilibrium inflows equal outflows and $dG(x) = 0$. We can rearrange the above expression to get

$$G(x) = \frac{M_U \lambda \left(p_R \left(F_\theta(x) - F_\theta(b)\right) + (1-p_R)\left(\Phi(x) - \Phi(b)\right)\right)}{M_E \left(\delta + \lambda p_R \tilde{F}_\theta(x) + \lambda(1-p_R)\tilde{\Phi}(x)\right)}$$

We use $G(x, R)$ and $G(x, P)$ to denote the distributions of workers at each type of firm and g_r, g_p to denote the corresponding densities.

B.4.1 Distribution of Best Offers for Workers at Renegotiating Firms of Each Match Quality

We can use a similar logic to derive the distribution of best offers received for workers at each type of firm. This is important because it determines the distribution of wages that we observe: at bargaining firms, workers' wages directly depend not only on the type of firm they are at, but on the best offer they have received. The flow equation for workers at type x firms whose last best offer was from a firm of type at most q is:

$$\begin{aligned} dH(q|x) \times g_r(x) &= -\left(\delta + \lambda_E p_R \tilde{F}_\theta(q) + \lambda_E(1-p_R)\tilde{\Phi}(q)\right) H(q|x)g(x,R)M_E \\ &\quad + \lambda_E p_R f_\theta(x) G(q) M_E + \lambda_U p_R f_\theta(x) M_U \end{aligned}$$

The flow rate into this state depends on the probability an employed worker receives an offer from a firm of q or lower or the probability an unemployed worker receives an offer from a firm of type x. The flow rate out of this state depends on (1) the probability the worker is no longer at a firm of type x and (2) the probability a worker receives an offer better than q.

C Data Appendix

C.1 Data Sources

C.1.1 Individual Characteristics

Demographic Information First, we obtain basic demographic information from the BEF and FAIN registers: sex, year of birth, and country of origin. The two registers draw from different administrative databases, but together provide nearly complete coverage.

Family Structure Next, we use the annual FAM and FAIN registers to determine whether someone has children in a given year. The annual BEF population register provides a unique identifier for each individual's spouse or partner if the individual in question has a valid person identifier in Denmark. While Statistics Denmark does distinguish between types of couples, they provide a partner ID if two people are living together and fall into one of the following four types:

1. Married couple
2. Registered partnership
3. Non-married or registered couple that live together and have at least one child in common
4. Cohabiting couple: two persons of different sex who live together with no other adults and who have an age gap of less than 15 years

Following Statistics Denmark, we consider anyone with a valid partner ID to be in a couple. We consider anyone in a type 1 or 2 relationship (married or in a registered partnership) to be married. We include these variables in our baseline regression in some specifications.

Education Our education variables come from the UDDA ("Uddannelser") register. This register combines information in the student register– which contains information on registered education in Denmark– and the qualification register. The qualification register in turn combines a number of sources including self-reported and imputed information on immigrants' education and information from professional membership registers (e.g. engineering associations). We focus on an individual's highest completed education, and use Statistics Denmark's own crosswalks to convert the detailed education codes (for information from the student register) to ISCED codes.

Occupation We use the BFL and RAS registers to code individuals' occupations. Most observations in the BFL register contain a six-digit occupation code ("DISCO"). We use the first four digits (prior studies including Groes et al. 2014 note that these are roughly equivalent to three-digit SOC codes) or the first two digits. Each table notes which aggregation level we employ. There are a small number of observations that do not have a valid occupation code. For these observations, we supplement the data with information from the RAS ("Registerbaserede arbejdsstyrkestatistik") register.

In Danish register data (both BFL and RAS) there is a break in the occupation coding in 2010.[38] Before this the register data report the codes based on the 1988 coding (which changes over time); after this the register data use the 2008 codes. This does not impact our main analysis because we typically rely on occupation by time fixed effects, allowing the coding to vary by period. To construct the occupation based networks we assign workers to a single occupation based on

[38]This break is not indicated in any of the official online descriptions of the registers.

their most frequently reported occupation over the 2010-2016 time period. To construct firm by occupation by time fixed effects or industry by occupation by time fixed effects, we use 2-digit occupation codes.

C.1.2 Firm Characteristics

We obtain a number of firm characteristics from the BFL, IDA, RAS, FIRM, and FIRE registers.

Industry We obtain information on industry from the BFL. The BFL includes six-digit industry codes for each firm in the data. The six digit industry variables are too detailed for our purposes. For instance, they distinguish between stores that sell women's clothing and men's clothing, and stores that sell both men and women's clothing. In all of our analysis we use the first four digits (which correspond to the NACE code) or first two digits.

Region We use data from IDA (the integrated database for labor market research) to assign each firm to one of Denmark's five administrative regions: (1) the capital region, (2) Southern Denmark, (3) Northern Denmark, (4) Central Denmark, and (5) the Zealand region. We are able to assign most firms to regions using data from the firm-level panel. For the small number of firms with multiple establishments in multiple regions, we use the region where the firm has the greatest number of employees. There are a small number of firms that do not have a district listed in the firm-level panel. For these observations, we use information from the worker panel to assign the firm to the region where the greatest number of employees live. Appendix Figure A2 shows a map of the five regions.

Value Added We use data from a firm accounting register (FIRE/FIRM) to calculate value added per worker. It is straightforward to calculate value-added following the procedure in Bagger et al. (2014*a*). This is the same procedure Statistics Denmark uses to produce national accounts.

We group firms into vigintiles based on mean value added per worker (in real terms) over the sample period.

C.2 Monthly Series

As discussed in section 3, our monthly earnings and hours data come from the administrative monthly employment for employees ("BFL") register. Danish firms are required by law to report wages paid at least once a month to the Danish Customs and Tax Administration daily. Firms also typically report the number of hours worked. The hours data should be of reasonably high quality because the obligatory payments to the Danish supplementary pension fund (ATP) depend on hours worked.[39] Statistics Denmark compiles these data into the BFL register based on the information provided to the Customs and Tax Authority. In cases where the firm does not report any hours, or Statistics Denmark considers the data 'invalid or improbable', hours are imputed. The data imputation flag indicates that approximately 15% of the hours data are imputed.

This register contains all employees in Danish registered companies, regardless of whether the employee lives in Denmark or abroad. The register reports data at the person-month-firm level. A key strength of our data is that for each observation there is start date and an end date (number within the month). Individuals who transition between firms within a month will have earnings

[39]The cost is nominally shared by the employee and employer; payment is taken as a payroll deduction. There are four different bins corresponding to full-time, 2/3 time, 1/3 time, and less than 1/3 time. In terms of monthly hours the bins are: 0-38, 39-77, 78-116, and 117+ hours. In terms of weekly hours the bins are: 0-8, 9-17, 18-27, and 27+.

observations at both firms that month, with the end date at one firm preceding the start date at the second.

In order to create a monthly series, we restrict attention to observations that span the first of each month. This means that moves that occur mid-month will only be captured by the following month's data. We then re-scale earnings, when necessary, so that they are equal to a full month's work. The table below illustrates this. Here, the individual worked at firm A in the first month and for half of the second month. Then, she switched to firm B, where she continued to work in month 3. Our final dataset lists this individual as working at the first firm in the first and second months and the second firm in the third month. Earnings during the second month are multiplied by two so they are equal to a full month's work. If we did not perform this adjustment, we would over-state the wage increase the individual got when he/she switched firms. Based on this dataset, we will first observe that a move has occurred in period 3.

Person	**Period**	**Start**	**End**	**Firm**	**Raw Earnings**	**Adjusted**	**Monthly Panel**
1	1	1	31	A	2000	2000	Yes
1	2	1	15	A	1000	2000	Yes
1	2	16	31	B	1500	3000	No
1	3	1	31	B	3000	3000	Yes

One unique feature of the BFL is that individuals who leave the firm for a short period of time (less than 45 days) but return are reported as still working at the firm, but receiving no earnings. This may occur if the worker is receiving training, or is on a short medical leave. This means that short disappearances and reappearances from the firm will not be counted in the firm-level hiring shock.

There are a very few person-firm-month observations (less than half of a percent) where an individual has two records for a single employer. In ninety-six percent of these problematic observations, there are two records for the individual-firm that month. Most of these observations appear to be for salaried workers. One of the observations corresponds to their salary earnings (160 hours per month when hours are reported); the second observation appears to be the result of additional hours worked, likely due to over-time payments. We create a separate earnings variable: total earnings and total hours.

Comparison with other Administrative Registers Relative to the standard Danish employer-employee dataset (IDA), our data are unique in providing monthly (not annual) data and in containing all employment spells for all workers. The IDA only includes data for workers employed as of the last week of November in the reference year and does not contain start or end dates for each worker-firm-year observation. To deal with these shortcomings, some researchers have used other registers to construct a weekly 'spells' dataset with high frequency information on workers' employment status (but not earnings) (see, e.g. Bagger et al., 2014*b*). The key advantage of the BFL data is the addition of earnings.

Our data also have several advantages, relative to other linked employer-employee datasets. The inclusion of hours data (not hours bins) is unique, relative to administrative registers in Germany and the United States. The fact that we observe all firms is an advantage relative to industry datasets in the United States. Other countries with administrative linked employer-employee data–including Portugal and Italy–have very rigid labor markets. It is not clear results in those contexts would be relevant for the United States.

C.3 Sample Selection Criteria

There are five million people in Denmark. The labor force participation rate is around 65%. Between a fifth and a quarter of workers are part-time. There are nearly three million workers in our dataset. Starting from the raw BFL dataset we make five primary sample restrictions. First, we drop individuals who are not Danish citizens. We do this because the BFL includes observations on all employees of Danish firms, including those who do not work in Denmark. In addition, our demographic information is most complete for Danish citizens.

Second, we drop individuals who never work at firms with fewer than 1000 people. This removes about 15% of the sample. These individuals are not used to construct the networks and do not appear in our regression sample.

Next we remove observations for workers who are younger than 25 or older than 60. This step also removes workers for whom we do not have valid birth year data.

Finally, we restrict our sample to individuals who, over the course of our sample, never work in more than one job at a time. We identify a multiple job holder as someone who works in two distinct firms (tax identifiers) on the first of the month and has positive earnings in both firms. In some cases we see individuals with a one-month long spell at a firm that overlaps another, longer spell. We include these individuals in our main sample. If we did not do this, our final sample would include roughly 800,000 workers. Because we allow for these one-time slips, our final sample includes slightly over one million workers.

We think that our measure of multiple job holding is somewhat conservative. However, we see job-to-job transition rates in this sample that are similar to those reported using other Danish registers. In practice, this restriction removes a large number of part-time workers. We have verified that our analysis is robust to including only the roughly 800,000 workers who meet the more stringent definition of multiple job-holding and to including a broader set of workers.

Finally, our main regression sample focuses on workers who are currently in firms with below 1000 workers. This is somewhat different from the sample of workers who *ever* work in one of these firms over the eight year period. We track whether workers move in or out of the sample but, once they leave, do not continue to follow them. This is because these workers cease to accumulate connections. Our results suggest that the length of time since an individual worked with a connection matters for information transmission. Table A1 shows how each restriction impacts the size of the analysis sample.

C.4 Earnings and Hours Data

The earnings data are reported electronically directly by the firms to the Danish Customs Authority. We scale all earnings and hours to the monthly level using the start and end dates in the register. There are substantial period-to-period changes in hourly earnings in the raw data. This arises for several reasons.

1. **Severance Pay:** Workers in Denmark are often eligible for severance pay when they leave a job. Including this in our measure of earnings would lead us to think that many workers see nominal wage *decreases* upon switching firms.

2. **Fringe Payments:** Second, the broad earnings measure includes payments for fringe benefits, including pay for housing or telephones, pay for vacation, and some contributions to retirement plans. While some of these may be smoothed over a worker's tenure, others may be reported lumpily by the firm. It is hard for us to, without additional data, remove these from a firm's base pay.

3. **Overtime:** Third, hourly earnings may change in response to changes in hourly worked that result in overtime payments.

4. **Annual Bonuses:** Finally, because we examine monthly data, annual bonus payments will lead to large monthly changes in income. If a worker receives a 15% annual bonus in December, for instance, her hourly earnings would more than double that month. She would then see her pay cut in half the following month. A worker who received a 20% annual bonus would see her earnings increase by 200% and then fall by a similar amount.

C.4.1 Earnings Measure

Most of our analysis focuses on a measure of monthly earnings that is processed in two ways:

1. **Severance Pay:** We remove severance pay. White collar workers in Denmark are sometimes eligible for severance pay upon dismissal, depending on their tenure at the firm. They are typically entitled to 1 month of pay after 10 years of tenure or 3 months of pay after 20 years of tenure. Firms may also elect to pay severance pay beyond that required by law. We do not want to include this in our baseline earnings measures as it will lead us to spuriously conclude that an individual received a raise in their last month at the firm.

   In cases where an individual appears to have been given severance pay upon termination, we re-code their final month's earnings with their earnings in the prior month. Specifically, we identify workers whose earnings more than doubled in their final month of employment at the firm (not due to changes in hours). For these observations we re-code final month's earnings with earnings from the prior month, adjusting for differences in hours as necessary. This is illustrated in the figure below. The earnings numbers are deliberately stylized:

Period	1	2	3	4	5	6
Firm	A	A	A	∅	∅	B
Raw	5000	5000	20000			4000
Δ Raw		0	15000			
Clean	5000	5000	5000			4000
Δ Cleaned		0	0			

2. **Double Pay:** We correct a small number of observations where an individual is not paid in one month, but receives twice their normal pay the following month. In this case we spread the earnings evenly over the two months.

In practice we still see substantial volatility in monthly earnings, partially arising from what appear to be annual bonuses. Because we have no reason to expect this volatility to be correlated with our measure of outside options, we prefer to use this measure as is, rather than attempt to smooth it in some way. Measurement error on the left hand side will simply inflate our standard errors.

Wages We construct wages by dividing earnings by hours worked. We only use the subset of the data with firm-reported hours. It is important to note that this measure may still vary with hours worked, if individuals cross ATP-contribution thresholds or if they receive overtime.

Real Earnings Most of our analysis is based on changes in nominal wages. In some descriptive tables we report real earnings measures, converted into US dollars. To convert earnings into real 2016 numbers, we use a consumer price index provided by Statistics Denmark. We then use the 2016 exchange rate between the US dollar and the Danish krone to convert numbers into dollars.

C.4.2 Base Pay and Bonuses

In some of our analysis we further process the data in order to investigate the impacts on base pay and bonuses. Our goal is to separate base pay and bonuses as in the following picture:

Raw	100	120	100	100
Earnings	100	100	100	100
Bonus	0	20	0	0

We define bonuses are one month increases in pay that revert the following month. We identify these bonuses by looking for earnings growth that:

1. Lasts one month and:
   (a) Is not driven by changes in wages. We require there to be more than a 7 DKK (~$1) increase in hourly earnings[40]
   (b) Is not permanent. We require that this month's earnings (wages) are more than 70 DKK (7DKK) larger than next month's. This allows us to ignore raises.
2. In some cases, we see what appears to be both a bonus and a raise. we see bonuses and raises at the same time. In this case we examine how wages change the following month. This is illustrated below:

Raw	100	150	130	130
Earnings	100	130	130	130
Bonus	0	20	0	0

We have done some sensitivity to thresholds of the above. While these decisions do impact the overall distribution of changes, they appear to be uncorrelated with our measure of outside options, and thus do not impact our point estimates.

In practice we think our ability to distinguish between bonuses and base pay is best for salaried workers. For these workers the percentage of observations with a bonus is 6% and the percentage of observations with a raise is 7%. These both seem reasonable: if all individuals received an annual bonus or received raise (even a cost of living adjustment) each year, we would expect to see rates of $\frac{1}{12} \approx 8\%$. The mean bonus is around half of an individual's usual monthly earnings, though there is substantial variation.

C.5 Mobility

There are three possible transitions a currently employed worker could make in a given period:

1. Stay: the worker is at the same firm this month as he/she was at last month

[40] In practice the exact value of this cutoff does not matter.

2. Exit: the worker was employed last period, but is not employed this period. This could mean unemployment or non-employment.

3. Job to Job Transition: the worker is employed in both periods, but at different firms. We decompose these moves into two types:

   (a) Move to a connected firm ("connected move"): the worker moves to a firm where he/she has a former coworker.

   (b) Move to an unconnected firm ("unconnected move"): the worker moves to a firm where he/she does not have a former coworker but that firm still falls within our sample of "not too large" firms

   (c) Move to a large firm ("out-of-sample move").

Note that moves that lead to an individual being not employed over the first of the month will be coded as an exit. This could occur if the individual takes time off between ending one job and starting the next, even if the transition was entirely voluntary.

Because our concept of the firm is a tax-identifier, individuals whose firms are involved in a merger or acquisition may see a change in firm codes. We recode the small number of observations that appear to be associated with this type of move as a non-transition. However, we do not include them in our analysis of stayers' earnings. In practice, this did not affect our analysis in any way. Table A3 presents descriptive statistics on transitions.

C.6 Networks

We use the baseline monthly series to construct an individual's coworker network. We first augment our data with register data from MIA (cleaned in the same way) so that we can construct coworker networks for individuals for the first three years of the BFL series (2008-2011). We then drop observations that correspond to spells in a firm with less than 2 people or more than 1000 people. These are standard restrictions in the empirical networks literature. We then construct the bipartite adjacency matrix A. This is a symmetric matrix where $A_{ij} = 1$ whenever i and j worked together in the past 3 years (excluding the period in question). Because we do not want to include shocks that come from an individual's former firms, we then remove all coworkers who are currently working at these firms (or the individual's current firm).

Note that because we have data from MIA dating back to 2004, we could have, theoretically, extended our analysis of job to job mobility by a few additional years. One issue, however, is that in 2005 and 2007 there were significant changes in some of the establishment identifiers, coincident with a reorganization of Danish municipalities. This is not a problem for how we define the networks because, within a month, we are still able to identify who is working together. However, it does make it more difficult to identify job-to-job transitions.

Future Networks Each individual's future coworker network consists of all individuals she works with in the next year or two-three years who are not currently working at firms she moves to. As we did when constructing an individual's past coworker network, we construct an individual's future coworker network, leaving out connections at firms she moves to in the next three years. As a result, her future coworker network consists of workers who will come join her at her current (or her future) firm, but have, themselves, not moved yet. If we did not do this, the size of the future coworker network would mechanically vary with job to job mobility decisions. We also exclude individuals who are in her former coworker network. The bottom row of Table 7 shows that the number of future coworkers in each year is roughly equivalent to the number of prior coworkers in each year. We only construct these networks for observations from January 2008 to December 2013 (60 months) so that the quality of the measure does not vary across periods in our sample.

Computation To generate each coworker network, we must generate and store an N x N matrix. We use MATLAB's sparse matrix packages to efficiently handle the data.

C.7 Trade Details

Denmark is a small open economy that is thoroughly integrated into the world market. Its main source of imports and destination of exports is Germany, which it shares a border with. Sweden, Britain, Norway and the United States are also important partners.

We merge our data with monthly bilateral trade flow from COMTRADE in order to compute the predicted value of exports for each firm and month, based on world product demand. We have flows from January 2010 to March 2016 at the six-digit Harmonized System level. For each product and month, we calculate the total value of imports of that product by all countries (less Denmark) from countries other than Denmark. We merge these to the administrative data at the firm-product-month level.

C.7.1 Instrument Construction

Step 1: We use data from 2004-2007 to calculate the average share of exports of product p that is accounted for by firm j. We do this by dividing the value of exports of product p by firm j over this time period by the total value of exports of product p by all firms over this time period:

$$\pi_p^j = \frac{\sum_t \sum_c \exp_{j,p,c,t}}{\sum_j \sum_t \sum_c \exp_{j,p,c,t}}$$

Step 2: We merge our administrative trade data with $\exp_{p,t}^{-1}$ from the product database after aggregating to the HS-6 level. We calculate the total predicted value of exports by weighting the leave-out measure from COMTRADE by these firm-specific product shares:

$$\hat{\exp}_{j,t} = \sum_{p,t} \pi_p^j \sum_c \exp_{\mathrm{p,c,t}}^{-1}$$

Step 3: We weight log predicted exports by an individual's former coworker network. The denominator is based on connections who are in firms covered by the trade register.

C.7.2 Instrument Quality

One concern is that firms may change their product or export destination mix over time. To examine this, we look at the correlation between realized and predicted exports over time. Figure A5 shows that the correlation between the observed and predicted measures is high, though declining over the sample period. The light blue dots show the correlation between a firm's (total) exports and that predicted using the product shares π_p^f during the period used to construct the shares. This is high by construction. The blue diamonds show the correlation between a firm's total exports and that predicted by weighting total Danish exports (as reported in the trade register) by π_p^f after the period used to construct the shares. This correlation trends down somewhat over time, but remains around .5 throughout our period, suggesting that firms do not change their product mix too much over time.

The dark blue squares show the correlation between actual firm exports and that predicted by weighting Danish exports in COMTRADE by the firm-level product shares. The correlation is similarly high. We would expect this to differ from the prediction using register data because of differences in reporting thresholds in COMTRADE and in the administrative data. The red triangles show the correlation between actual firm exports and predicted exports based on world demand. The correlation is fairly stable over our sample period.

C.8 Estimating the Model

There are five steps to evaluating the objective function specified in equation 13, given a set of parameters, a set of empirical moments, and a weighting function.

1. Given a guess of parameters, ξ, solve for $V_P(w)$ and $T_R(\theta)$ by value function iteration
2. Given $V_P(w)$ and $T_R(\theta)$, solve for the wage function $\phi(\theta, w)$, which says what wages a worker at a renegotiating firm of type θ will earn, if her last offer was from a firm whose max offer was w.
3. Simulate the model for 10,000 workers and 100 periods
   (a) Set worker's workers initial conditions using the equilibrium distribution of workers
   (b) For t=2:T
      i. With probability δ, an employed worker is exogenously separated from her employer
      ii. With probability $\lambda^E + \alpha s$ or $\lambda^U + \alpha s$ an employed or unemployed worker receives an offer from an outside firm. With probability p_R this is from a renegotiating firm. The worker decides to move, renegotiate, or stay (with no wage change)
4. Calculate the moments $S(\xi)$ using the simulated data
5. Find the weighted distance between the simulated and empirical moments

A common challenge in the literature is specifying the matrix W, which weights the distances between each of the simulated and observed moments. Given standard regularity assumptions, as $N \to \infty$ the estimator $\hat{\xi}$ is consistent and asymptotically normal for any positive-definite W (Gourieroux et al., 1993). However, in finite samples the weighting matrix often matters. We first attempted to use a diagonal matrix where the i^{th} element is the inverse variance of the i^{th} component of S_N.[41] In practice we, like prior authors, found that this led us to under-weight, and not match, key moments of interest (Jarosch, 2015). including transition rates and the variance of low wage changes. In order to match key moments, including the transition rates and variance of log wage changes, we over-weighted the relevant moments.

[41] For means we use bootstrapped standard errors.

D Supplementary Results

D.1 Job Search by the Unemployed and Non-Employed

In this section we present results for unemployed and non-employed workers. Prior work in this literature has shown that workers displaced in a mass layoff use information from their job search networks in order to find new employment (Glitz, 2013; Saygin et al., 2014). Because we have not accounted for selection into unemployment or non-employment, we view this as a purely descriptive exercise. A more comprehensive analysis examining the impacts on workers involved in exogenous separations is beyond the scope of this paper.

We run our standard reduced form regressions on the sample consisting of workers whose last job was at an in-sample firm but who are, as of the first of the month, are not employed at any firm

$$\mathrm{U2E}_{it} = \sum_n \gamma^n \Omega^n_{it} + c_{it} + \alpha_i + \alpha_{jt} + \epsilon_{ijt}$$

The results are presented in Table A6. The first column presents the baseline results. Columns 2 and 3 condition on the individual remaining un/non-employed for at least 2 or 3 months. Overall, the results are similar to those presented in Section 5: an individual's more recent coworkers matter more for their recover from unemployment/non-employment. One thing to note is that the gradient is less steep: This is consistent with the idea that only an individual's very recent coworkers are likely to proactively give them information. However, when an individual is without work, they reach out to a broader set of former colleagues. Note that we deliberately exclude an individual's future coworkers from this specification: if an individual does not find reemployment, they do not have future coworkers.

D.2 Quality of Outside Options

While our baseline measure of Ω_{it} treats all firms equally, the theoretical model in Section 2 suggests that both the number and quality of outside options matter. If all firms renegotiated wages, only offers from higher-productivity firms would matter for mobility; only offers from lower-productivity firms would matter for on-the-job wage growth. For workers at posting firms, offers should only impact wages through mobility.

We use firm accounting data to group firms into vigintiles based on mean value added per worker over the sample period. We then construct measures of $\Omega^{\mathrm{ABOVE}}_{it}$ and $\Omega^{\mathrm{BELOW}}_{it}$ using only connections at firms in higher and lower vigintiles (with strict inequality); we construct $\Omega^{\mathrm{SAME}}_{it}$ using connections at firms in the same vigintile. Note that we are only able to do this using the connections that are covered by the accounting register.[42] We then run our baseline regression, replacing Ω_{it} with these measures and adding fixed effects for the vigintile of an individual's current firm (v_{it}).

$$y_{it} = \gamma^{ABOVE} \Omega^{\mathrm{ABOVE}}_{it} + \gamma^{\mathrm{BELOW}} \Omega^{\mathrm{BELOW}}_{it} + c^{\mathrm{ABOVE}}_{it} + c^{\mathrm{BELOW}}_{it} + v_{it} + X_{it} + \alpha_{kt} + \alpha_i + \epsilon_{it} \quad (14)$$

Table A13 reports estimates of γ^{ABOVE} and γ^{BELOW} from this regression (Panel A) and from regressions that control for the number of new positions created at firms in the same vigintile (Panel B). The coefficients in Panel A are presented graphically in Figure A7.

[42] The results for this exercise are somewhat less precise, reflecting the fact that we have a smaller sample and use a smaller number of coworkers in each network. Column 2 of Table 2 shows that we are able to rank roughly 75% of the firms in our sample. The regressions only include workers who have coworkers both at higher- and lower-productivity firms.

Figure A7 shows that higher and lower productivity firms have similar impacts on job-to-job mobility. By contrast, only positions at less productive firms matter for wage growth. The earnings results are exactly in line with the predictions of the model in Section 2: workers are able to use outside offers from less productive firms as leverage to renegotiate wages at their current firm.

The fact that workers move to positions at both more and less productive firms is somewhat at odds with a simple model where all firms renegotiate wages.[43] However, it is exactly what we would see if some workers are at firms that have committed not to renegotiate wages (Flinn and Mullins, 2017).[44] In this case, mobility is not always efficient: workers may move to less productive firms if the incumbent firm is unwilling to renegotiate.

D.3 Posting and Bargaining by Occupation: Hall and Krueger (2012) Data

Hall and Krueger (2012) provided evidence on the incidence of wage posting and bargaining among workers in the United States. We used survey data from that paper to construct posting and bargaining rates by occupation.[45] Figure A6 plots the unweighted fraction of workers in each broad occupation group that answered "some bargaining over pay" when asked:

> When you were offered your (current/previous job), did your employer make a "take-it-or leave-it" offer or was there some bargaining that took place over the pay?
>
> - Take it or leave it offer
> - Some bargaining over pay

This is question 34D in the survey. Out of 2513 interviewees, there are 1373 workers with valid responses to this question: not all workers were asked the question, and some that were asked refused to respond.

The figure shows that, in the United States, bargaining at the beginning of the job spell is less common among workers in less-skilled occupations. These mirror the results in Section 6.3.

D.4 Robustness: Results using Annual Data

While the monthly data allow us to more precisely identify the timing of employment and job-to-job transitions, one concern with our estimates is that there may be volatility in the monthly earnings data. To alleviate this concern, we show that we obtain similar results using a separate, annual, matched employer-employee database.

We obtain data from 2004-2013 from the Integrated Database for Labor Market Research (IDA) database. This database covers the entire Danish population aged 15-74. Each individual has a unique person identifier. The structure of the data differs from our monthly panel: workers are only included in the database in a given year if they are employed in the last week of November. Each person-year observation includes data on total earnings and total days worked, as well as an estimate of the average hourly wage. The data do not include information on when each job spell started or ended during the year. However, they do contain information on the total number of days worked. When we examine the impacts on wages, we focus on the change in log annual earnings and on the change in log daily earnings.

[43]While many search models predict that workers will be willing to accept pay cuts in order to move to more productive firms, on average, workers in our sample see larger wage gains if they move to more productive firms.

[44]There are other possible explanations for this finding. For instance, lower productivity firms could offer more non-wage benefits, as in Sorkin (2018).

[45]The data are available here. We used a Department of Labor crosswalk to convert the SOC codes in the data to the broad ISCO codes in our Danish data.

For each individual and year, we keep the main employment relationship as of the last week of November. We construct annual coworker networks using a three-year look-back window and a firm-size cutoff of 750. Specifically, we consider individual j to be in individual i's coworker network if we observe them working together (in the annual register) in the prior three years in a firm that has, on average, fewer than 750 employees.

We use data from the firm component of the IDA register (IDAS) to count the change in employment from one year to the next. We have data from this register through 2012. Employment is measured as of the last week of November. We use three summary measures: (1) the number of new positions $\max\{0, \text{Emp}_{jt} - \text{Emp}_{j,t-1}\}$, (2) the percentage change in employment $(\text{Emp}_{jt} - \text{Emp}_{j,t-1})/\text{Emp}_{j,t-1}$, and (3) the total change in employment $\text{Emp}_{jt} - \text{Emp}_{j,t-1}$. We also create two weighting functions, based on the mean wage: $\omega_{jt} = \frac{w_{jt}}{\bar{w}}$ and on firm value added per worker.

Our regression sample includes data from 2007 through 2012 on all Danish workers between 25 and 60 who were employed as of November that year. We exclude the first three years of our sample, which are used only to generate the coworker networks. We exclude the final year because we do not have firm-level employment data from IDA for that year.

Tables A11 and A12 present estimates of γ from equation 5. As before, we include individual and industry-by-time fixed effects in all of our specifications. We cluster standard errors at the individual and firm level. The results are very noisy in this sample, reflecting the fact that we have only a few observations for each worker. While our main analysis uses eight years of monthly data, here we have only five annual observations.