Monopsony in Movers: The Elasticity of Labor Supply to Firm Wage Policies

Ihsaan Bassier
University of Massachusetts Amherst

Arindrajit Dube
University of Massachusetts Amherst, NBER, IZA

Suresh Naidu
Columbia University, NBER

March 19, 2019

Abstract

We provide new estimates of the separations elasticity with respect to hourly wage using matched Oregon employer-employee data. Existing estimates using individual wage variation may be biased by endogenous and mismeasured wages. We calculate AKM firm effects, and use these to estimate the impact of the firm component of wage variation on separations. Separations are a declining function of firm effects: the implied firm-level labor supply elasticities are around 3, consistent with recent experimental and quasi-experimental evidence, and are approximately 2.5 to 4 times larger that those using individual wages. We find lower elasticities for skilled workers.

1 Introduction

How elastic is the supply of labor to a single firm? This is the key parameter measuring the degree of monopsony in the labor market, estimates of which have proliferated in recent years. Small values of this elasticity imply significant degrees of monopsony power, while large values imply close to competitive behavior in labor markets. In models of dynamic monopsony, Manning (2003) shows that the steady-state elasticity of labor supply facing the firm can be expressed as a linear combination of separation and recruitment elasticities, estimates of which are readily available in matched-worker firm data. In this paper, we revisit this estimation strategy using causal effects of firms on hourly wages to address measurement and specification errors that may have biased previous results.

Following Manning (2003), researchers have typically estimated separations and recruitment elasticities with respect to individual earnings, conditional on observable control variables. . However, there are a number of *a priori* reasons to believe this may induce biases in the estimates for the labor supply elasticity, ϵ; moreover, it is not clear whether one can even sign the direction of the bias based on *a priori* judgment. If within-firm wage differences in part reflect unobserved skills, this may bias the estimate for ϵ downward, perhaps explaining why recent quasi-experimental estimates of the labor supply elasticity tend to find values between 2 and 4 (Caldwell and Oehlsen 2018, see also structural estimates from Dube, Manning, and Naidu (2019) and the meta-analysis by Sokolova and Sorensen 2018), even as the most recent papers in the traditional approach (e.g., Webber (2015) and Bachmann, Demir, and Frings (2018)) continue to find significantly smaller elasticities between 1 and 1.2. In contrast, the well-documented presence of fairness concerns within a firm (Card, Mas, et al. 2012; Fehr and Schmidt 1999) is likely to bias the ϵ estimate upward. Consistent with this,Dube, Giuliano, and Leonard (2015) use exogenous, discontinuous raises at a major retailer and find separation elasticities of around 12—but show that these are largely driven by peer concerns. Partialling out the peer effects, they find a firm-level labor supply elasticity of around 4. Finally, use of earnings (instead of hourly wages) in many of the existing papers may affect the bias: on the one hand, use of earnings is likely to attenuate ϵ due to the measurement error from hours; on the other hand, if hours are correlated with unobserved heterogeneity then the direction of bias may be difficult to pre-determine.

In this paper, we propose an alternative approach using a new data source that addresses each of these concerns. First, we use matched employer-employee data from Oregon from 1998-2017, which includes information on hours of work, unlike the Longitudonal Employer Household Dynamics (LEHD) data in the US or matched employer-employee data in many European countries. Second, we use wage variation stemming from firm wage policies to identify plausibly exogenous wage variation that is (1) unlikely to be related to individual level skills, and (2) unlikely to reflect (within firm) peer comparisons. The basic idea is to compare firms that are otherwise similar but happen to pay somewhat differently to similar workers. In particular, we isolate the component of individual wages determined by firm wage policies using Abowd, Kramarz, and Margolis (1999)—hereafter AKM—firm effects, and estimate the effect of this component of the wage on separations. Use of the AKM firm effect allows us to focus on the wage variation that is likely coming from similar workers receiving different pay due to their employers, but not due to other arbitrary wage differences across individuals, for example due to skill. However, as we will be show below, the AKM firm effects are themselves weighted averages of wage changes among movers between firms, with weights that also depend on the separation probabilities (Hull (2018)). When the independent variable is a function of the dependent variable this may induce a mechanical bias. We use sample splitting to overcome this bias, as well as the usual attenuation bias from using a generated regressor.

We find that firm effects are clearly and negatively correlated with the overall and especially job-to-job separation rate, consistent with the AKM effects reflecting firm wage policies. The separations response is fairly log-linear over much of the distribution of firm fixed effects, but becomes substantially more muted at the top, consistent with a job-ladder interpretation where high-paying firms face little competition for their workers. When the rank (within the firm distribution) of the firm fixed effect is used, the relationship becomes much more linear, consistent with simple variants of the Burdett-Mortensen model where job-to-job separations are a linear function of the offer distribution CDF. While using hourly wages substantially increases the precision of the estimates, it does not substantially affect the point estimates. However, use of the firm effects in the separation elasticities increases the labor supply elasticity estimates by a factor of 2.5 to 4 as compared to the standard approach using individual wages. Use of a split-sample instrument that corrects for measurement error in the estimation of the firm effects produces a slightly larger labor supply elasticity, as does using a lagged firm effect instrument which avoids estimating firm effects and separations from the same sample of movers. At the same time, the overall labor supply elasticity is modest, with a baseline estimate of 3.4 suggesting considerable monopsony power in the labor market. Our findings are robust to a variety of estimation techniques, including nonlinear hazard and inclusion of additional covariates. While our estimates are larger than those using the standard approach, we confirm that the labor supply elasticity is procyclical—similar to the findings in Webber (2018). We also find some evidence of heterogeneity across sectors and locations in the extent of labor market power, but overall our effects are remarkably stable. The most notable heterogeneity is by worker skill: we find much larger separations responses in workers in the lowest quartile of the worker-effect distribution, and smaller effects on separations responses in the highest quartile of worker fixed effects. Finally, we find fairly similar estimates of the separation elasticity in the Portland metro area as opposed to more rural parts of Oregon—even though the concentration measures vary widely. This stands as a cautionary note on the strategy of using labor market concentration to proxy for monopsony power.

The remainder of the paper is structured as follows. Section 2 describes our data source. Section 3 describes the research design. Section 4 presents the empirical results. Section 5 concludes.

2 Data

The state of Oregon requires all employers, as part of the state's Unemployment Insurance (UI) payroll tax requirements, to report both the quarterly earnings and quarterly hours worked for all employees.[1] We obtained the micro-data as part of a data sharing agreement with the state of Oregon, allowing us to construct hourly wage information for nearly all workers in the state of Oregon using high quality administrative sources. The resulting administrative matched employer-employee microdata covers a near census of employee records from the state. The payroll data relies on Quarterly Contributions Reports submitted by the private sector as well as government employers for the purposes of unemployment insurance. We have 20 years of data from 1998-2017, or 80 quarters, with about 2 million workers and 120,000 firms in each quarter. An advantage of this data is that we observe quarterly wages as well as hours for each worker, allowing us to gain precision in identifying, for example, higher paid part-time workers from lower paid full-time workers. Observations are at the level of all employer-employee matches, meaning that a worker may have multiple observations in a given quarter at different firms. Oregon has a median household income that is close to the rest of the United States, and which has historically followed a similar trend. Oregon experienced recessions in 2001-2002 and 2008-2009 along with the rest of the country, and this is included in our sample period.

Following Song et al. (2018), we restrict the data to jobs (worker-firm matches) at private-sector firms with more than 20 employees; this allows for meaningful estimation of within-firm statistics. Table A1 in the appendix summarizes the data by 5-year periods (see empirical method below). Each period has over 28 million observations. The national median annual earnings for 2013 reported by Song et al. (2018) is \$36,000, which corresponds to the 2013 median in Oregon of \$39,000, once comparable restrictions are made[2]. The average

[1] Only three other states (Washington, Minnesota and Rhode Island) require employers to similarly report hours of work as part of the UI system.

[2] Song et al. (2018) exclude workers who earn less that the equivalent of minimum wage for 40 hours per week for 13 weeks. Data for the 75th and 90th annual earnings percentiles are comparable too, with national earnings at \$63,000 and \$104,000 respectively compared to Oregon with \$62,000 and \$96,000 respectively.

separation rate is 0.158, which is very similar to the separation rate of 0.15 reported by Webber (2015) using the LEHD. The final column shows the average number of firms an employee works at over the 5-year period. As we explain later, *movers* between firms drive the identification of the firm effects. It gives confidence then that the average worker allows a comparison across 2-3 firms in each of the 5 year periods.

One limitation of using data from a single state is that separations to firms outside of Oregon are not counted as job-to-job separations but rather as job-to-non-employment separations. However, we note that any bias induced by this is likely quite limited due to the relatively small size of cross-state flows as compared to flows across firms.

3 Research design

3.1 Overview of Manning (2003)

The method implemented by Manning (2003) derives from estimates of two equations implied by the Burdett-Mortensen model. These two equations capture the "job-ladder" nature of dynamic monopsony. Let $F(w)$ denote the CDF of firm wage offers, and $G(w)$ denote the wage distribution across workers. One equation governs the separation rate from firms that pay w into either unemployment (at exogenous rate δ) or into firms that are posting higher wages than w (at exogenous rate λ) :

$$s(w) = \delta + \lambda(1 - F(w)) \tag{1}$$

The second equation governs the recruitment rate into firms paying w, and depends on unemployment u. New recruits from unemployment are contacted at a rate λ^u and accept any wage posted in equilibrium. Recruits from already employed workers only happens when the employed worker is at a firm paying below w,and thus depends on the wage distribution $G(w)$. Hence recruits are given by:

$$R(w) = \lambda^u u + \lambda(1-u)G(w)$$

Manning then breaks up these equations into recruitment from and separations into employment and non-employment, exploiting the fact that recruits from employment into a firm must, on average, equal job-to-job transitions out of a firm in steady state. If the recruitment and separation elasticities are constant, then the steady-state assumption implies that the separation elasticity, ϵ_{EE} is equal to the recruitment elasticity from employment ϵ_{EEhire}, even under additional heterogeneity in job preferences across workers.

We begin by estimating the separation elasticity calculated using the standard hazard rate specification, with an exponential distribution where t is defined as the length of employment conditional on termination within the 20 year period:

$$\lambda^y(t \mid \epsilon_y \ln(wage_{it})) = \lambda_0 exp(\epsilon_y \ln(wage_{it})) \tag{2}$$

For comparison we also report an estimate using a linear specification, which is both transparent as well as comparable with our IV results.

$$y_{it} = \epsilon_y \ln(wage_{it}) + v_{it} \tag{3}$$

where y_{it} is a binary variable for any separation (all separations), separation to employment (E-E separations), separation to unemployment (N-E separations), or hire from employment (E-E recruits). Dividing ϵ_y by the sample average of y yields the elasticity of y with respect to wage. The sample includes all workers in each relevant 5-year period, where separations are calculated on a quarterly basis. The E-E separations regression excludes N-E separations, the N-E separations regression excludes E-E separations, and the E-E recruits regressions is restricted to individuals recruited .

The overall labor supply elasticity is estimated following Manning (2003):

$$\epsilon_{Lw} = -(1+\theta)\epsilon_{EE} - (1-\theta)\epsilon_{NE} - \epsilon_{EEhire} \quad (4)$$

where θ gives the proportion of recruits from employment. The labor supply elasticity standard errors are approximated by assuming that the estimate errors of the y-elasticities are normally distributed and that θ is estimated without error. Periods are categorized as 5-year intervals from 1998 to 2017.

3.2 Addressing wage endogeneity with AKM.

The standard approach may be biased due to unobserved heterogeneity. Suppose wages are determined as in Abowd, Kramarz, and Margolis (1999). There are two types of variations in the wage. Part of the wage is a worker component (e.g., skill level), which moves with the worker regardless of what employer she goes to, α_i. A second component reflects wage differences across employers for the same skill level, μ_f, for example due to firm-specific wage or rent sharing policies and their interaction with productivity (we will discuss disamenities below). Therefore, the overall (log) wage is given by $w_i = \alpha_i + \mu_{f(i)}$. In a simple dynamic monopsony model [3], it is not clear why we would expect a worker's separation probability to another firm to be higher if α_i is lower - after all it is the mobile component of wage. We would expect the separation to be higher if it is a "bad job" (i.e., μ_f is lower) because now there is a greater chance of the worker receiving offers that dominate current employment.

From the firm's perspective, the relevant separation elasticity ϵ is based on what happens as the firm changes its wage policy, which in this context is varying μ_f, and so the correct

[3] Engbom and Moser 2018provide a variant of Burdett-Mortensen with heterogeneous workers, where the log wage can be decomposed in the AKM framework as $log(w_{if(i)}) = \alpha_i + \mu_{f(i)}$ where $\mu_{f(i)} = log(p_{f(i)} m_{f(i)})$ is the log productivity of the firm $p_{f(i)}$ marked down by $m_{f(i)} < 1$ which depends on the exogenous Burdett-Mortensen parameters (assumed constant across skill groups α) and the firm's position in the productivity distribution. Note that any monopsony model where marginal product was multiplicative in skill and firm productivity could generate a similar specification, so long as the labor supply elasticity facing the firm was constant across skill types. A profit function of the form $\int (e^{\alpha_i} p_f - w_i) l(w_i) dF(i)$ would generate a wage equation for type i of the form $log(w_i) = \alpha_i + log(mp_f)$,where $m = \frac{\epsilon}{1+\epsilon}$ is the markdown based on the elasticity of labor supply $\epsilon = \frac{w \frac{dl}{dw}}{l}$.

specification is:

$$y_{it} = \epsilon_y^{firm} \mu_{f(i)} + v_{it} \tag{5}$$

However, if we simply use w_i as the key independent variable as in equation 2 then we will identify $\tilde{\epsilon_y} = \sigma \epsilon_y^{firm}$ where $\sigma = \frac{var(\mu_{f(i)t})}{var(w_{it})}$ is the share of variation in wages that is due to firm effects. In our data, firm effects explain roughly 15 percent of the hourly wage variation, slightly below the corresponding 20 percent of earnings (wages $\times$ hours) explained by the firm effects. This suggests that the standard approach may recover an estimate that is roughly a sixth as large, and so the use of individual level wages can significantly overstate the extent of monopsony power. In practice, if mobility patterns are also different for high and low skilled workers, the size of the bias may be smaller or bigger.

The discussion above not only highlights the nature of the problem, but also readily suggests a solution. If what we are interested in knowing is how separation rates from firms responds to variation in wages that are unrelated to individual effects, a natural approach is to use the estimated firm effects $\widehat{\mu_i}$ on the right hand side of equation 5. There are two challenges to this identification strategy. First, in practice, the firm effects are estimated; use of generated regressors will tend to attenuate the estimates. Second, and more subtly, the AKM effects are simply weighted averages of wage changes of movers - where the weights themselves are a function of the separation rates.[4] If the separation rates are stochastic, then their presence on both sides of the equation can induce a spurious correlation that can impart a bias upon the OLS elasticity estimates. This is similar to the well known division bias, as in Borjas 1980, where again the same variable entering both sides of the equation. We show this formally in the Appendix.

We address both of these issues using sample splitting, where we randomly split the workers (in each 5-year period) into two groups A and B, stratified on moving. Using these

[4]Hull (2018)shows formally, that firm fixed effects are weighted averages of the wage changes experienced by new recruits and separators, where the weights depend on the share of moves for each pair of firms in each direction. We reproduce his analysis and relate it to monopsony in the Appendix.

two samples, we generate two sets of AKM firm effects, $\hat{\mu}^A$ and $\hat{\mu}^B$. Next, we take the individuals in sample A and regress y_{it} on $\hat{\mu}^A$ while instrumenting the latter with $\hat{\mu}^B$. This means a worker's separation indicator is not entering into both the right and the left side of the equation, eliminating any mechanical correlation induced by an individual's separation influencing the estimate of $\hat{\mu_j}$. In addition, because the $\hat{\mu}^A$ and $\hat{\mu}^B$ are from separate samples, assuming that the estimation errors are uncorrelated, we can use the latter to instrument the former to the attenuation bias stemming from a generated regressor. As an alternative to the sample splitting, we also use the lagged firm effect as an instrument for the firm effect. Here the $\hat{\mu}_{t-1}$ (from the previous 5-year period) is used to instrument for $\hat{\mu}_t$.

In terms of implementation details, we calculate the firm effects using the AKM approach, by 5 year periods. Estimating the AKM model, we can decompose the variance of the wage in the worker and firm effects, as in Song et al. 2018. Table A2 shows that the firm fixed effect explains a considerable portion of the log wage variation, about 11 percent - in line with estimates in the literature. If instead we used quarterly wages as we demonstrate in table A3, notice that the portion explained by the firm effects *doubles*, from about 11 percent to 22 percent. If we are interested in the wage-rate, then systematic differences in hours as correlated with the firm are important for estimating the firm effects. Without an indicator for hours, low-hour firms will pay less in total wages and this will be calculated as a low firm effect—even if the firm pays the same hourly wage. Our access to hourly earnings data allows us to better estimate the separations elasticities as shown in the results of table 1 below. Table A4 decomposes the wage further, showing that (as in Song et al. 2018 for U.S. overall) segregation between firms has increased steadily in Oregon over the past 20 years. For all reported estimates of the separations and labor supply elasticities (excepted where noted), we exclude public administration and trim the top 2.5% and bottom 2.5% of the firm effects distribution.

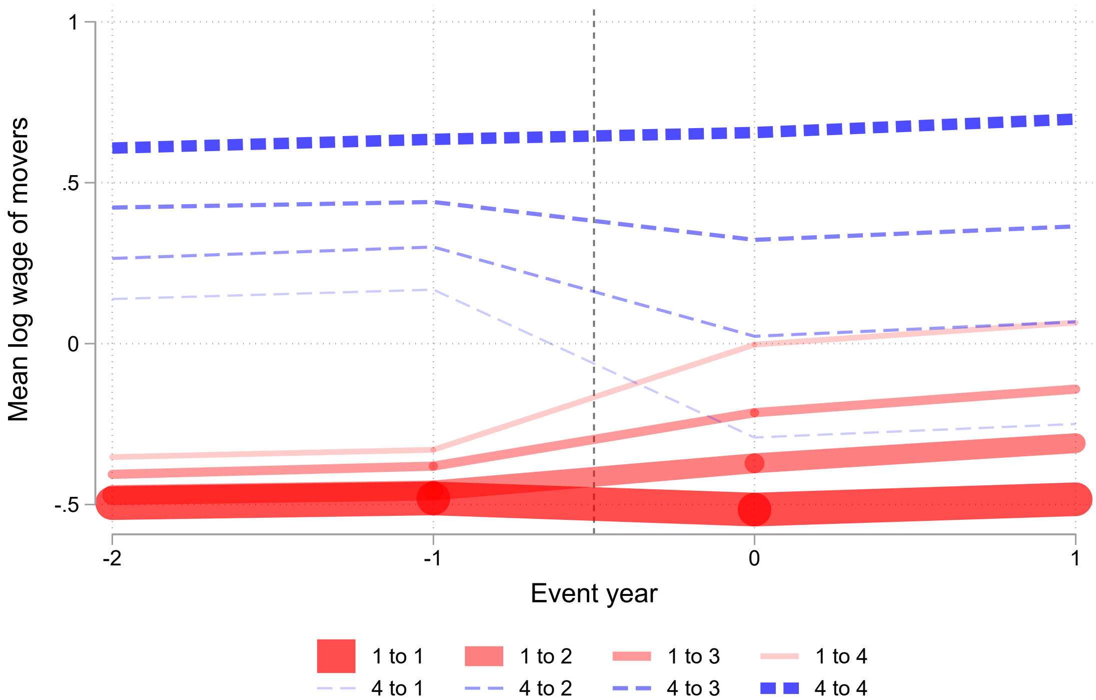
4 Results

Figure 1 captures the key contribution of our paper, replicating the event study figure showing interquartile transitions in Card, Heining, and Kline (2013), but weighting the flows by the number of workers in each pair of quartiles. The event study shows largely parallel trends prior to a transition, similar to Card, Heining, and Kline (2013). In addition, we show that the separation rates of firms in these quartiles behave as expected. Low quartile firms have much higher job-to-job separation rates—as indicated by the thickness of the lines—than the high quartile firms. Moreover, the flows are not symmetric: more workers move from low to high wage quartiles (red solid lines) than vice versa (blue dashed lines), which is consistent with high quartile firms being higher rent jobs. The asymmetric flows across quartiles capture the separation elasticity; increases in wages have more separations than decreases in wages. This figure shows simultaneously the lack of wage changes prior to a move (flat pre-move trends), the effects firms have on wages (the magnitude of an individual wage change after a move) and that the volume of flows between firms are correlated with those effects (the thickness of the lines). Together this suggests that firm wage policies may be identifiable from switchers, even as they influence the direction and volume of switching.

Figure 2 presents a binned scatterplot, showing the full range of employment to employment separations plotted against the firm fixed effects. It shows a tight, if nonlinear relationship between separations and firm effects on log wages, with a precisely estimated average elasticity of -1.7. The flattening of the relationship between separations and firm fixed effects at the top of the distribution is also consistent with the job ladder pattern of transitions implied by Burdett-Mortensen: high-wage firms have lower labor supply elasticities because there is less poaching of workers by even higher wage firms.

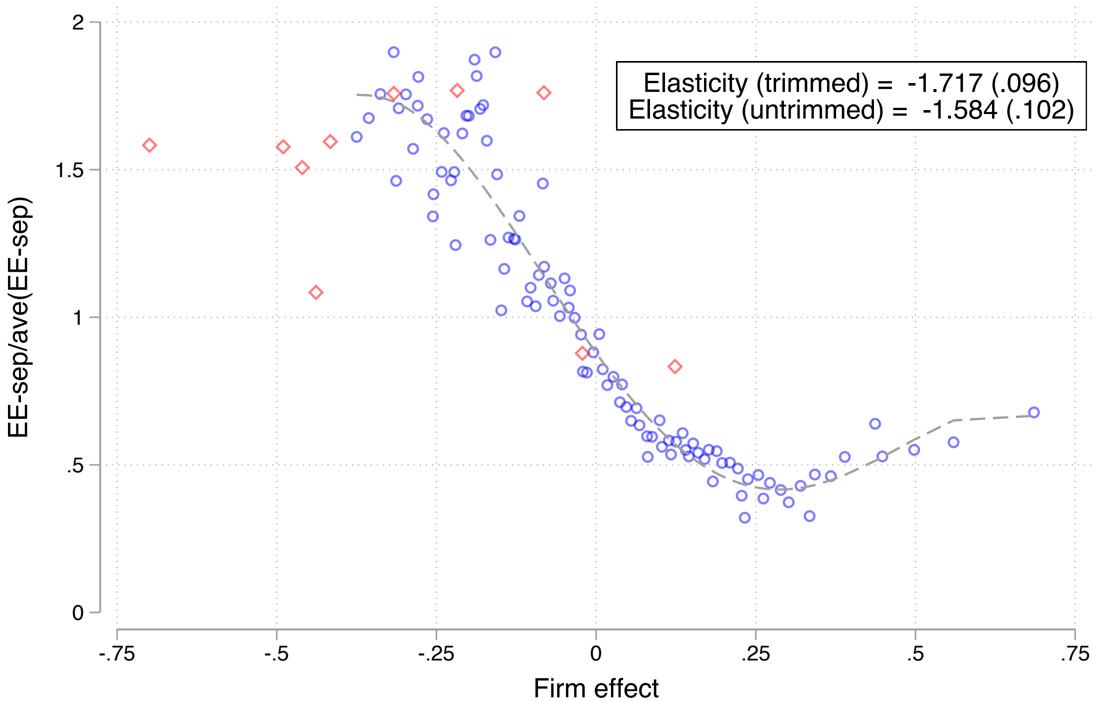
Table 1 shows the results from our regressions using a variety of outcome variables. All regressions are run at the individual level, clustered by firm and control for firm size and quarterly fixed effects. We report estimates using any separation as an outcome variable, as well as employment-to-employment separations (E-E), employment to non-employment

Figure 1: **Changes in hourly wages and incidence of job separations for quartile-to-quartile transitions**



Note: The thickness of the lines is proportional to the number of job-to-job separations between the relevant quartiles over 1998-2017. The legend indicates origin quartile to destination quartile, where quartiles are defined along the firm distribution of average wage in each firm. The change in wage is shown for movers, who are defined as workers who make a job-to-job transition at any point over the period and are observed for at least 9 consecutive quarters before and after. The observation recording the separation and the following observation are omitted since these represent quarters that were partially worked, and are particularly susceptible to measurement error in wages. Event quarters are collapsed into years in sets of 4 around the separation quarter.

Figure 2: **Job-to-job separations and firm wage effects**



Note: The figure illustrates the split sample approach using a control function. Residuals are calculated from a regression of own-sample firm effects on the complement-sample firm effects, and used as a control in a regression of E-E separations on own-sample firm effects. The plotted points show the residualized points of this latter regression (i.e. depicting the partial correlation), re-centred around the original mean values. The blue points represent quantiles of the trimmed sample, which excludes the top and bottom 2.5 percent of the firm effects distribution. The red points represent quantiles of the excluded sample only, which we consider outliers. The trendline is fitted to the trimmed sample and represents the predicted y values of a third degree polynomial.

separations (N-E), and employment-to-employment recruits (E-E recruits). We then present the share of recruits from employment, and use that together with equation 3 to generate a labor supply elasticity facing the firm. Column 1 shows the standard hazard rate specification using quarterly wages, and the implied labor-supply elasticity ϵ is quite low, at 0.71. Column 2 uses hourly wages instead, and while the separation elasticities fall, the recruit elasticities rise, resulting in only a small decrease in the estimated ϵ. Column 3 uses an OLS model instead of the hazard model, and the resulting separations elasticities all increase, with only a small decrease in the recruitment elasticity, with the resulting estimate of ϵ almost doubling relative to columns 1 and 2, but still a low 1.313. The increase in elasticity due to the change in specification is in line with the literature, as reviewed by the meta-analysis of Sokolova and Sorensen (2018).

Columns 4-6 use the firm effects instead of individual wages as the key independent variable, and column 4 shows that this results in positive recruit elasticities, and much larger separations elasticities. The resulting estimates of ϵ are around 3.17, and are quite stable between the OLS specification and the two IV specifications. The IV specification using the split-sample in columns 5 is 3.166, while using the lagged firm effect instrument in column 6 is 3.203. Both are quite similar to the OLS and each other, with unsurprisingly large F-statistics. Finally specification 7 (straddling the last 2 columns) shows estimates where we include both the firm-wage component, $\mu_{f(i)}$, as well as the residual (i.e., $w_i - \mu_{f(i)}$) as independent variables. We find that separations are much more responsive to the firm effect as compared to the residual, which includes individual characteristics unaffected by a firm's wage policy, except through changing the composition of the workforce.

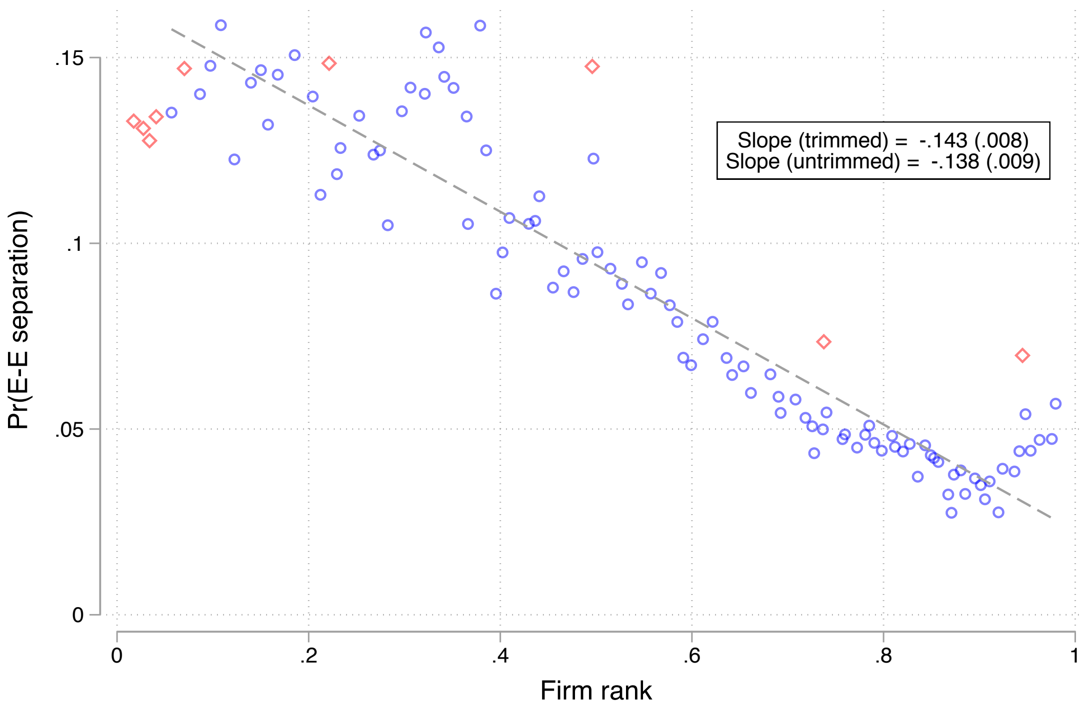
Figure 3 shows the empirical analogue of equation 5, the separation rate as a function of the rank of a firm in the offer distribution. The rank of a firm in the distribution of firm fixed effects corresponds to its position in the offered wage distribution, at least in the simplest variant of the Burdett-Mortensen model (i.e. without heterogeneous workers, endogenous search intensity or job destruction rates). Equation 5 implies a linear relationship between

Table 1: **Separations and recruits elasticities to own wage**

	Wage			Firm FE			Wage Components	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
All separations	-.321	-.244	-.529	-1.343	-1.36	-1.444	-1.051	-.39
	(.011)	(.004)	(.018)	(.069)	(.07)	(.089)	(.066)	(.016)
E-E separations	-.352	-.275	-.701	-1.727	-1.748	-1.769	-1.314	-.561
	(.011)	(.005)	(.027)	(.096)	(.098)	(.125)	(.09)	(.023)
N-E separations	-.298	-.202	-.498	-1.224	-1.241	-1.353	-.985	-.333
	.01	.004	.019	.066	.068	.085	.065	.017
E-E recruits	-.036	-.106	-.019	.057	.059	.113	.093	-.053
	(.017)	(.007)	(.009)	(.037)	(.037)	(.043)	(.035)	(.006)
Pct. EE-recruits	.466	.466	.466	.466	.466	.466	.466	.466
Labor supply ε	.71	.617	1.313	3.128	3.166	3.203	2.359	1.053
	(.023)	(.01)	(.042)	(.15)	(.153)	(.193)	(.141)	(.035)
Obs (millions)	15.9	15.9	108	107	107	73.4	107	107
Hourly wage		Y	Y	Y	Y	Y	Y	Y
Hazard spec.	Y	Y						
Firm FE			Y	Y	Y	Y	Y	
Non-firm wage res.								Y
F-stat					203813	1129	192906	
Split-Sample					Y		Y	
Lag						Y		

Note: The first stage F-stat is given for the row 1 regression. The unit of observation for the hazard specifications is an employment spell, and for the linear specifications is each job-quarter record. Column 1 outcome is quarterly wage. Column 6 uses the lag of the firm FE as an instrument, which excludes period 1 (1998-2002) observations. Columns 7 and 8 (spec. 7) give the elasticities from the firm FE and non-firm wage (hourly wage - firm FE) respectively, for a single regression. Firm fixed effects are censored at the 2.5 percent tails of the firm FE distribution. Jobs are restricted to private firms larger than 20. Standard errors are shown in parentheses.

Figure 3: **Job to job separations and rank of firm wage effect**



Note: The figure illustrates the split sample approach using a control function. Residuals are calculated from a regression of own-sample firm rank on the complement-sample firm rank, and used as a control in a regression of E-E separations on own-sample firm rank. The plotted points show the residualized points of this latter regression (i.e. depicting the partial correlation), re-centred around the original mean values. The blue points represent quantiles of the trimmed sample, which excludes the top and bottom 2.5 percent of the firm effects distribution. The red points represent quantiles of the excluded sample only, which we consider outliers. The linear trendline is fitted to the trimmed sample.

the rank of a firm's wage offer and its separation rate, and Figure 3 shows that the linear fit is indeed quite good, implying substantial search frictions, with $\lambda = .14$. This is in the range of the same parameter, between 0.07 and .15, calibrated by Hornstein, Krusell, and Violante (2011) from monthly job-to-job flows. Note, however, that unlike in the Burdett-Mortensen model, the job-to-job separation rates do not equal zero at the top of the distribution, and some job-to-job transitions are to lower paid jobs.

These results are fairly robust to alternative specifications, as shown in Table 2. Controlling for tenure reproduces a similar pattern: the labor supply elasticity jumps from 1.3 under

Table 2: **Alternative specifications for labor supply elasticities**

	(1)	(2)	(3)	(4)	(5)	(6)
All separations	-1.367	-1.382	-1.234	-1.212	-1.266	-1.249
	(.052)	(.053)	(.074)	(.068)	(.079)	(.07)
E-E separations	-1.299	-1.313	-1.584	-1.564	-1.482	-1.48
	(.054)	(.055)	(.102)	(.094)	(.1)	(.09)
N-E separations	-1.172	-1.186	-1.134	-1.12	-1.072	-1.043
	(.048)	(.049)	(.07)	(.067)	(.07)	(.057)
E-E recruits	.107	.111	.066	.076	.071	.014
	(.069)	(.07)	(.033)	(.036)	(.037)	(.046)
Pct. E-E recruits	.466	.466	.465	.466	.466	.466
Labor supply ε	2.422	2.446	2.861	2.816	2.674	2.712
	(.108)	(.11)	(.158)	(.147)	(.155)	(.143)
Obs (millions)	15.2	15.2	111	107	90	90.1
Firm FE	Y	Y	Y	Y	Y	Y
Split-Sample		Y	Y	Y	Y	Y
F-stat			266453	208721	176352	175030
Hazard specification	Y	Y				
Uncensored			Y			
Controls						
Tenure trend				Y		
Indus.*County FE					Y	
Indus.*Tenure trends						Y

Note: The first stage F-stat is given for the row 1 regression. The unit of observation for the hazard specifications is an employment spell, and for the linear specifications is each job-quarter record. Column 2 uses the split sample in a control function for the hazard specification. Tenure refers to the number of quarters since the job started, is coded as a continuous variable and includes terms up to a quadratic. Industry is defined at the 1-digit level. Firm fixed effects are censored at the 2.5 percent tails of the firm FE distribution. Jobs are restricted to private firms larger than 20. Standard errors are shown in parentheses.

the linear specification to 2.9 under the split sample firm effects specification. Including controls for industry (1-digit level) by county fixed effects results in a labor supply elasticity of 2.7. Even though the linear estimate of the elasticity was double the hazard rate estimate, using firm effects with the hazard rate raises the elasticity from 0.6 to 2.4, which is four times as large and much closer to the linear firm effects estimate. The estimate is similar when using the split sample under the control function approach, where residuals from a linear first-stage regression of $\hat{\mu}^A$ on $\hat{\mu}^B$ are included as covariates in the hazard model. Overall, across a wide range of specifications, we find clear evidence that the separation elasticity using the firm component of wages suggests a larger elasticity typically between 2.4 and 3.2, as compared to estimates using individual wage variation which range between 0.6 and 1.3.

Table 3 presents the heterogeneity in the labor supply elasticity. Using the 1-digit industries, we exclude agriculture as well as mining, utilities and construction as these industries have far fewer employees (less than half of the next smallest industry). Table 3 suggests that manufacturing has the most elastic labor supply, while Art, Accommodation and Food Services (which includes fast food outlets) has the smallest. This is worth noting: one may have thought low-wage sector like Art, Accommodation and Food Services would be more competitive. However, while the degree of competition is high at low wages *conditional on worker characteristics* (i.e., for low firm effects), it is not necessarily the case for unconditionally. Some low wage sectors appear to have substantial monopsony power.

We also report elasticities separately for the Portland metro area, and rest of Oregon. These two subsamples differ dramatically in levels of labor market concentration. In Portland metro, the county $\times$ industry $\times$ quarter employment (payroll) Hirshman-Herfindahl-Index (HHI) is 0.29 (0.56), while average outside of Portland metro area the HHI (weighted by employment) is 0.74 (.99). Despite this large difference in concentration, the labor supply elasticities are very similar, going from 3.8 to 3.6. Since the effect of concentration on wages is mediated by the elasticity of labor supply facing the firm, these results suggest some caution in interpreting recent studies (e.g. Azar, Marinescu, and Steinbaum (2017),Rinz

Table 3: **Heterogeneity in labor supply elasticities**

	Separations ε		Labor supply ε		Obs
Panel A: Industry					
Manufacturing	-1.891	(.24)	4.782	(.507)	12.2
Wholesale, trade and transport	-1.175	(.131)	2.991	(.263)	17.9
FIRE	-1.832	(.192)	4.499	(.421)	17.59
Education and Health	-1.601	(.151)	3.496	(.346)	22.6
Art, Accommodation and Food	-.57	(.153)	.66	(.308)	10.3
Panel B: Period					
1998-2002	-1.059	(.088)	2.528	(.185)	26.8
2003-2007	-1.531	(.098)	3.564	(.212)	26.0
2008-2012	-1.434	(.091)	3.285	(.2)	25.3
2013-2017	-1.509	(.082)	3.48	(.179)	28.6
Panel C: Geographic zone					
Portland metro	-1.585	(.081)	3.804	(.194)	30.2
Standard	-1.588	(.079)	3.667	(.169)	22.5
Panel D: Worker Effect Quartile					
Quartile 1	-.85	(.058)	2.155	(.121)	18.4
Quartile 2	-1.032	(.08)	2.085	(.162)	25.4
Quartile 3	-.823	(.085)	1.833	(.18)	29.4
Quartile 4	-.32	(.109)	.808	(.281)	31.6

Note: Industry is defined at the 1-digit level. "Agriculture", "mining, utility and construction", and "other" industries have been excluded due to low number of workers. Geographic zones are based on the Portland Urban Growth Boundary, which includes most of Multnomah county. Due to data limitations, the zone estimates exclude period 1 (1998-2002) observations and allocate workers to a zone if at least 90 percent of the employees of their firm are working in a single zone. This sample has an overall labor supply ε of 3.75 (se=.135). Worker effect quartiles are calculated on the worker distribution. Observations are given in millions. Firm fixed effects are censored at the 2.5 percent tails of the firm FE distribution. Jobs are restricted to private firms larger than 20. Standard errors are shown in parentheses.

et al. (2018)) showing negative effects of employment concentration on wages. First, even low HHI areas may have substantial monopsony power.In addition, concentration may be picking up other differences between labor markets.

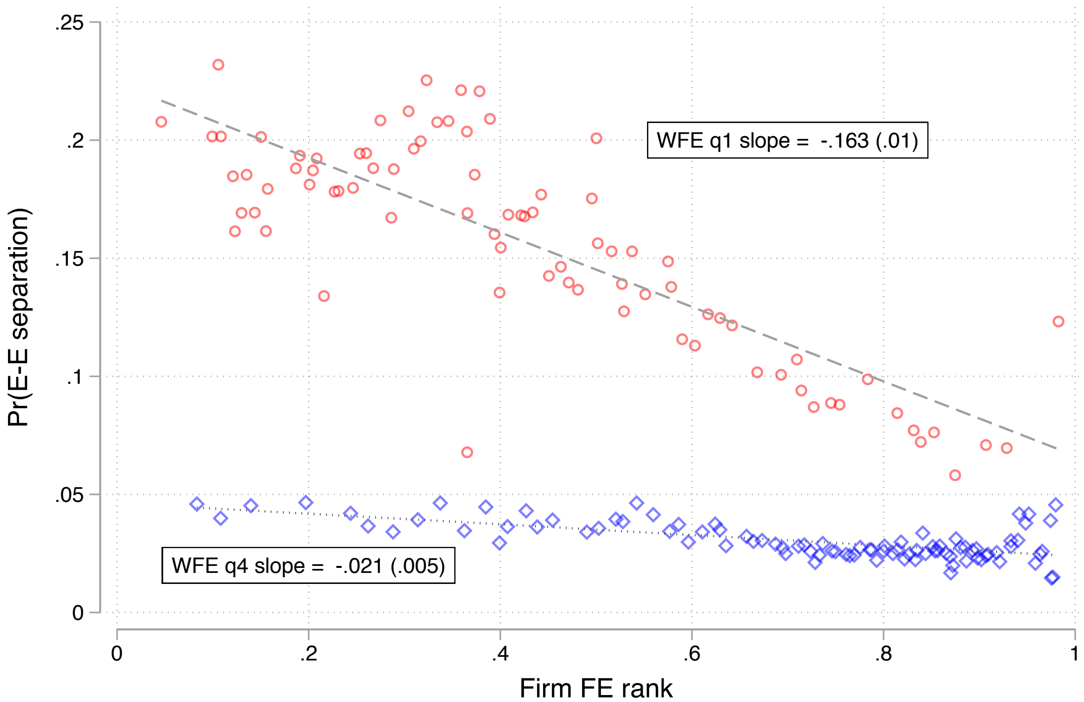
In addition, we find the the labor supply elasticity is procyclical. The implied labor supply elasticity in the most recent period with relatively tight labor market is 3.2 as compared to around 2.7 during the previous downturn. The procyclicality of the labor supply elasticity is consistent with Webber (2018), even though our magnitudes are larger than what he finds.

Figure 4 shows the starkest heterogeneity in our results, plotting the correlation between the separation rate and the firm fixed effect rank separately for the top and bottom quartile of worker fixed effects. While both are linear, consistent again with equation 5, the slope is much steeper for workers in the highest quartile, implying that the degree of monopsony power facing workers with high "skill"—at least as measured by the component of their wages that is constant across employers— is much higher than that facing low skill workers. The bottom panel of Table 3 shows that the elasticity for the top quartile is 0.8, as compared to 2.2 for the bottom quartile. The uniformly smaller magnitudes across all skill subgroups is consistent with specification (7) in Table 1, where composition effects somewhat attenuated the separations response to firm-wage variation. Whether this is due to assortative matching, so that high-skill workers are also at high-wage firms (who face lower competition for their workers) or due to firm-specific skills, tastes, or other investments that make them less responsive to wage differences across employers we cannot further distinguish using this data, as we do not observe a rich set of worker-level covariates.

5 Discussion and Conclusion

The individual separation elasticity with respect to own wage has been taken as evidence for dynamic monopsony power. However, this literature has rarely successfully distinguished between wage variation due to worker heterogeneity and that due to firm wage-setting, even

Figure 4: **Job-to-job separations and rank of firm wage effect, by worker quartile**



Note: The figure illustrates the split sample approach using a control function. Residuals are calculated from a regression of own-sample firm rank on the complement-sample firm rank, and used as a control in a regression of E-E separations on own-sample firm rank. The plotted points show the residualized points of this latter regression (i.e. depicting the partial correlation), re-centred around the original mean values. Quartiles are calculated using the worker effects distribution. The blue points represent quantiles of the highest worker effect quartile, and the red points represent quantiles of the lowest worker effect quartile. Only the trimmed sample is used, and the trend lines are linear fits on the respective quartile samples.

as theory points towards the latter as the relevant component of the wage. We model individual wages, following Abowd, Kramarz, and Margolis 1999 as additively separable in a fixed worker component and a firm fixed effect. We estimate the elasticity of separations with respect to the firm fixed effects, as this component of the wage (due to varying firm "wage policies") is much more likely to be exogenous to worker heterogeneity, and is more clearly motivated by models of wage-setting firms.. However, because the firm fixed effects are weighted averages of changes in wages experienced by workers that change firms (i.e. "Movers"), the separation rates appear on both the right hand and left hand sides of the equation, and may bias the OLS specification in a manner akin to division bias. For this reason we use a split-sample (as well as lagged) AKM firm effects as an instrument, accounting for both this mechanical bias as well as any sampling or measurement error in the generated regressor. Estimating dynamic monopsony using the wage variation generated by. movers links the size of flows between firms and the causal effects of firms on hourly wages: in models with dynamic monopsony, the propensity to move between two firms depends on the differences in firm effects on wages.

As expected, we find that separations are much less sensitive to the non-firm component of wages, highlighting the likely bias from using individual wages.Relative to estimates obtained from this procedure, existing elasticities from individual level separations regressions appear to be downwardly biased. While in principle the bias can go either way, the main avenue seems to be insufficient controls for unobserved individual heterogeneity. Nonetheless, the estimates still suggest substantial amount of monopsony power, with labor supply elasticity around 3.

Examining the response of separations to firm wage effects is also informative about the interpretation of those effects. One view, for example in Sorkin (2018), is that a substantial part of firm fixed reflect compensating differentials for firm-specific disamenities. Our data provides three pieces of evidence against this interpretation. First, if flows are themselves are positively correlated with firm wage effects it suggests that the wage effects are not fully offsetting the disamenities, as the resulting flows across firms would be symmetric, and the separation rate from the lowest quartile of firm effects in Figure 1 would not be so dramatically lower than the separation rate from the highest quartile. Second, unlike most work to date, our AKM effects are in hourly wages, so they are not driven by unobserved hours variation (as would be the case in the LEHD used in (Sorkin (2018)). Finally, Table 2 shows that our point estimates on the separations elasticity are little affected by the inclusion of industry $\times$ county and industry $\times$ tenure controls, and these controls are likely to correlate with a great deal of amenity variation.

Finally, we believe our estimand is closer to what models of monopsony imply. From the perspective of a firm with labor-market power, the extent to which separations vary with the fixed component of worker wages is not something that can be affected with wage policies. But the elasticity of separations with respect to firm wage effects is exactly the constraint governing the wage-setting process of a monopsonistic firm.

References

Abowd, John M, Francis Kramarz, and David N Margolis (1999). "High wage workers and high wage firms". In: *Econometrica* 67.2, pp. 251–333.

Azar, José, Ioana Marinescu, and Marshall I Steinbaum (2017). *Labor Market Concentration.* Tech. rep. National Bureau of Economic Research.

Bachmann, Ronald, Gökay Demir, and Hanna Frings (2018). "Labour Market Polarisation and Monopsonistic Competition". In:

Borjas, George J (1980). "The relationship between wages and weekly hours of work: The role of division bias". In: *The Journal of Human Resources* 15.3, pp. 409–423.

Caldwell, Sydnee and Emily Oehlsen (2018). "Monopsony And the Gender Wage Gap:Experimental Evidence from the Gig Economy". In:

Card, David, Jörg Heining, and Patrick Kline (2013). "Workplace heterogeneity and the rise of West German wage inequality". In: *The Quarterly journal of economics* 128.3, pp. 967–1015.

Card, David, Alexandre Mas, et al. (2012). "Inequality at work: The effect of peer salaries on job satisfaction". In: *American Economic Review* 102.6, pp. 2981–3003.

Dube, Arindrajit, Laura M Giuliano, and Jonathan S Leonard (2015). "Fairness and frictions: The impact of unequal raises on quit behavior". In:

Dube, Arindrajit, Alan Manning, and Suresh Naidu (2019). "Monopsony and employer misoptimization account for round number bunching in the wage distribution". In: *Unpublished manuscript.*

Engbom, Niklas and Christian Moser (2018). "Earnings inequality and the minimum wage: Evidence from Brazil". In:

Fehr, Ernst and Klaus M Schmidt (1999). "A theory of fairness, competition, and cooperation". In: *The quarterly journal of economics* 114.3, pp. 817–868.

Hornstein, Andreas, Per Krusell, and Giovanni L Violante (2011). "Frictional wage dispersion in search models: A quantitative assessment". In: *American Economic Review* 101.7, pp. 2873–98.

Hull, Peter (2018). "Estimating treatment effects in mover designs". In: *arXiv preprint arXiv:1804.06721*.

Manning, Alan (2003). *Monopsony in motion: Imperfect competition in labor markets*. Princeton University Press.

Rinz, Kevin et al. (2018). *Labor market concentration, earnings inequality, and earnings mobility*. Tech. rep. Center for Economic Studies, US Census Bureau.

Sokolova, Anna and Todd Sorensen (2018). "Monopsony in Labor Markets: A Meta-Analysis". In:

Song, Jae et al. (2018). "Firming up inequality". In: *The Quarterly Journal of Economics* 134.1, pp. 1–50.

Sorkin, Isaac (2018). "Ranking firms using revealed preference". In: *The quarterly journal of economics* 133.3, pp. 1331–1393.

Webber, Douglas A (2015). "Firm market power and the earnings distribution". In: *Labour Economics* 35, pp. 123–134.

— (2018). "Employment Adjustment over the Business Cycle: The Impact of Competition in the Labor Market". In:

A Online Appendix

A.1 AKM and Endogenous Separations.

We draw substantially on Hull (2018) in this section. For simplicity, we will consider the case of 2 periods and 2 firms, 0 and j. The AKM specification is thus equivalent to the first-differenced regression for individual worker i given by:

$$\Delta w_i = \tau + \mu_j \Delta D_i^j + \gamma \Delta X_i + \epsilon_{ij}$$

where w denotes log wages, and ΔD_j denotes a change in employment at firm j. Workers can thus be recruits ($\Delta D_j = 1$), separations ($\Delta D_j = -1$) or stayers (ΔD_j=0). As Hull (2018) observes, if wage changes are heterogeneous across these groups, the μ_j will be a combination of these effects. In particular, $\hat{\mu_j} = \frac{Cov(\Delta D_j, \Delta w_i)}{Var(\Delta D_j)} = (E[\Delta w_i | \Delta D_j = 1] - E[\Delta w_i | \Delta D_j = 0])\omega + (E[\Delta w_i | \Delta D_j = -1] - E[\Delta w_i | \Delta D_j = 0])(1 - \omega)$, where $\omega \in (0, 1)$ is a weight that is a nonlinear function of $Pr(\Delta D_j = -1)$ and $Pr(\Delta D_j = 1)$.

The important assumption for identification of μ_j is parallel trends and homogeneity: wage changes would be the same for switchers if they had stayed, and wage changes are of the same magnitude, but opposite sign, between workers that switch from 0 to j and those that switch from j to 0, so that the values of ω are irrelevant for estimates of μ_j. Hull (2018) shows that when homogeneity is not assumed, the weights ω can affect the estimate of μ_j, and when there are more than 2 firms, μ_j may not even by a convex combination of the wage changes of separations and recruits.

Now suppose that the probability of individual i switching is itself a function of the firm wage effect so that:

$$Pr(\Delta D_i^j = -1) = \epsilon_{EE} \hat{\mu_j} + \nu_i$$

$$Pr(\Delta D_i^j = 1) = \epsilon_R \hat{\mu}_j + \nu_i'$$

Note that this specification does not induce bias in the estimation of μ_j, as it does not violate parallel trends in wages for stayers and movers, but makes the probability of moving endogenous to the firm wage policy. But since $Pr(\Delta D_i^j)$ enters both the right hand side and the left hand side of these regressions, this may induce a mechanical correlation, biasing estimates of $\epsilon^S$and ϵ^R. Further μ_j is itself an estimated quantity with sampling error, and would attenuate estimates of the true separation/recruit elasticity. To address both of these problems, we use a split sample approach, where $\hat{\mu_j^A}$ is used as an instrument for $\hat{\mu_j^B}$. This eliminates any mechanical correlation between an individual's probability of leaving and the estimate of the firm fixed effect, as well as addressing sampling error in the generated regressor.

If the AKM assumption of homogeneity didn't hold, there would be an additional source of bias, as the ω weights themselves would depend on μ_j,which would complicate the linear AKM estimation as well as the separations and recruitment elasticities. Hull (2018) presents estimators for Mover Average Treatment Effects (MATE), that estimate the average effect of moving into firm j, and depend on the probability of transitioning from firm k to j and vice versa (e.g. via a propensity score). What estimate of separations elasticity is recovered in this model when there is mutual dependence between the MATEs and the separation/recruitment elasticities is an open question, which we leave to future work.

A.2 Additional Tables and Figures

Table A1: **Summary statistics on Oregon data**

	N	Workers	Firms	Ann. Earnings	Hours	No. firms	Sep.	E-E recruits
1998-2002	29	2.61	22,787	39,972	27.5	2.9	.185	.494
2003-2007	29.4	2.61	21,764	43,340	28.1	2.6	.166	.465
2008-2012	28.6	2.49	20,631	45,523	28.3	2.2	.142	.419
2013-2017	31.9	2.74	22,722	46,224	28.1	2.6	.142	.470
Total	119	10.4	87,904	43,818	28.0	2.6	.158	.465

Note: The first three columns indicate totals (N and workers are in millions) and other columns indicate means. "No. of firms" refers to the average number of firms a worker is at during the 5 year period. Separations and E-E recruits (proportion of hires from employment) are given in percentage terms. Earnings are in real Dollars adjusted to 2017 using the Portland CPI. Jobs are restricted to firms larger than 20.

Table A2: **Basic decomposition of firm and worker effects (hourly wages)**

	Interval 1		Interval 2		Interval 3		Interval 4		Change	
	1998-2002		2003-2007		2008-2012		2013-2017		Interval 5 - 1	
	Comp.	Share	Comp.	Share	Comp.	Share	Comp.	Share	Comp.	Share
Var(Y)	.633		.522		.536		.521		-.112	
Var(WFE)	.22	.348	.22	.421	.23	.43	.232	.445	.011	-.101
Var(FFE)	.075	.118	.056	.107	.063	.118	.056	.107	-.019	.17
Var(Xb)	.004	.006	.005	.01	.002	.004	.006	.012	.002	-.019
Var(Res)	.127	.201	.061	.117	.06	.112	.059	.113	-.068	.608
Cov(WFE,FFE)*2	.064	.101	.073	.139	.078	.146	.086	.165	.022	-.195
Cov(WFE,Xb)*2	-.003	-.004	-.005	-.009	-.002	-.003	-.005	-.01	-.003	.023
Cov(FFE,Xb)*2	0	0	0	-.001	0	-.001	0	0	0	0
Cov(Y,FFE)	.119	.187	.099	.19	.108	.201	.103	.198	-.016	.14
N	28.915		29.227		28.439		31.844		2.928	

Note: All fixed effects are calculated on the given period. Hourly wages are calculated as reported quarterly wages divided by reported hours worked in the quarter. Abreviations: Y (ln of hourly wage), FFE (Firm Fixed Effect), WFE (Worker Fixed Effect), Xb (covariate - here, just quarter fixed effects), Res (residuals). Jobs are restricted to firms larger than 20.

Table A3: **Basic decomposition of firm and worker effects (quarterly wages)**

	Interval 1 1998-2002		Interval 2 2003-2007		Interval 3 2008-2012		Interval 4 2013-2017		Change Interval 5 - 1	
	Comp.	Share	Comp.	Share	Comp.	Share	Comp.	Share	Comp.	Share
Var(Y)	2.03		1.949	1	1.861		1.907		-.122	
Var(WFE)	.73	.359	.733	.376	.718	.386	.694	.364	-.035	.287
Var(FFE)	.458	.226	.444	.228	.497	.267	.461	.241	.002	-.02
Var(Xb)	.009	.005	.01	.005	.004	.002	.009	.005	0	-.002
Var(Res)	.638	.314	.591	.303	.528	.284	.569	.298	-.069	.564
Cov(WFE,FFE)*2	.169	.083	.153	.079	.091	.049	.16	.084	-.009	.073
Cov(WFE,Xb)*2	-.006	-.003	-.01	-.005	-.003	-.002	-.01	-.005	-.004	.036
Cov(FFE,Xb)*2	0	0	-.001	0	-.001	-.001	-.001	0	0	.003
Cov(Y,FFE)	.543	.267	.52	.267	.542	.291	.54	.283	-.002	.02
N	28.915		29.227		28.439		31.844		2.928	

Note: Quarterly wages are reported as the total amount received for the quarter, irrespective of hours. All fixed effects are calculated on the given period. Abbreviations: Y (ln of hourly wage), FFE (Firm Fixed Effect), WFE (Worker Fixed Effect), Xb (covariate - here, just quarter fixed effects), Res (residuals). Jobs are restricted to firms larger than 20.

Table A4: **Between vs within decomposition of firm and worker effects (hourly wages)**

	Interval 1 1998-2002		Interval 2 2003-2007		Interval 3 2008-2012		Interval 4 2013-2017		Change Interval 5 - 1	
	Comp.	Share	Comp.	Share	Comp.	Share	Comp.	Share	Comp.	Share
Var(Y)	.633		.522		.536		.521		-.112	
Between firm										
Var(m_Y)	.236	.372	.227	.434	.232	.433	.231	.444	-.004	.039
Var(m_WFE)	.061	.096	.064	.123	.067	.126	.072	.138	.011	-.101
Var(FFE)	.075	.118	.056	.107	.063	.118	.056	.107	-.019	.17
Var(m_Xb)	.001	.001	.001	.001	0	0	.001	.001	0	-.001
Cov(m_WFE,FFE)*2	.064	.101	.073	.139	.078	.146	.086	.165	.022	-.195
Cov(m_WFE,m_Xb)*2	0	0	-.001	-.001	0	0	-.001	-.001	-.001	.006
Cov(FFE,m_Xb)*2	0	0	0	-.001	0	-.001	0	0	0	0
Within firm										
Var(diff_Y)	.398	.628	.295	.566	.303	.567	.29	.556	-.108	.961
Var(diff_WFE)	.16	.253	.156	.298	.163	.304	.16	.307	0	.001
Var(diff_Xb)	.003	.005	.005	.009	.002	.004	.005	.011	.002	-.018
Var(Res)	.127	.201	.061	.117	.06	.112	.059	.113	-.068	.608
Cov(diff_WFE,diff_Xb)*2	-.003	-.004	-.004	-.007	-.001	-.003	-.005	-.009	-.002	.018
Cov(diff_WFE,Res)*2	.002	.003	.001	.003	.001	.002	.001	.003	0	.001
Cov(diff_Xb,Res)	0	0	0	0	0	0	0	0	0	.001
Segregation $\frac{Var(m_W FE)}{Var(WFE)}$	.277		.291		.291		.310		.033	
N	28.915		29.227		28.439		31.844		2.928	

Note: All fixed effects are calculated on the given period. Abreviations: Y (ln of hourly wage), FFE (Firm Fixed Effect), WFE (Worker Fixed Effect), Xb (covariate - here, just quarter fixed effects), Res (residuals), m (average across workers in the firm), diff (worker specific effect minus average relevant effect across firm). Jobs are restricted to firms larger than 20.

Table A5: **Non-linearities in labor supply elasticities**

	(1)	(2)	(3)
All separations	.964	.952	1.099
	(.024)	(.02)	(.026)
E-E separations	.957	.941	1.153
	(.032)	(.028)	(.038)
N-E separations	.969	.969	1.057
	(.024)	(.017)	(.022)
E-E recruits	1.001	.996	1.008
	(.006)	(.006)	(.007)
Pct. E-E recruits	.477	.501	.497
Labor supply ε	-2.921	-2.892	-3.266
	(.049)	(.044)	(.058)
Obs (millions)	107	107	107
Firm quartile	25	50	75

Note: Quartiles are calculated along the firm distribution. Specification includes cubic terms in firm FE, with elasticity calculated on a 0.1 interval around the firm FE quartile. Firm fixed effects are censored at the 2.5 percent tails of the firm FE distribution. Jobs are restricted to private firms larger than 20. Standard errors are shown in parentheses.