

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

Ihsaan Bassier

Arindrajit Dube

Suresh Naidu

University of Massachusetts

University of Massachusetts

Columbia University, NBER

Amherst

Amherst, NBER, IZA

July 21, 2020

Abstract

We provide new estimates of the separations elasticity, a proximate determinant of the labor supply facing a firm with respect to hourly wage, using matched Oregon employer-employee data. Existing estimates using individual wage variation may be biased by mismeasured wages and use of wage variation unrelated to firm choices. We estimate the impact of the firm component of wage variation on separations using both firm fixed effects estimated from a wage equation as well as a matched IV event study around employment transitions between firms. Separations are a declining function of firm wage policies: we find that the implied firm-level labor supply elasticities generated are around 4, consistent with recent experimental and quasi-experimental evidence, and that they are approximately 3 to 4 times larger than those using individual wages. Further, we find lower separations elasticities for low wage workers, high turnover sectors, and periods of economic downturn but with little heterogeneity by urban status or labor market concentration. We conclude that monopsonistic competition is pervasive, and largely independent of forces driving classical monopsony.

1 Introduction

How elastic is the supply of labor to a single firm? The firm-level labor supply elasticity measures 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 the labor supply facing a firm can be expressed either as twice the separations elasticity (or as a linear combination of separations and job-to-job share of recruits elasticities) estimates of which are readily available in matched-worker firm data. In this paper, we revisit this estimation strategy using plausibly causal effects of firms on hourly wages and high quality administrative data to address measurement and identification shortcomings that may have biased previous results. As we show, adopting this approach makes a substantial difference in the conclusions we can draw about the competitiveness of the U.S. labor market.

Following Manning (2003), researchers have typically estimated separations 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 .¹ The key challenge in quantifying monopsony power is estimating the extent to which separations and recruitment vary when a firm pays a higher versus a lower wage to all its workers, something we refer to as a “wage policy.” However, individual worker’s wages vary for many reasons that go beyond a firm’s wage policy. For example, wage differences across workers reflect permanent differences in skills and other characteristics, or transitory shocks to the job prospects of workers (perhaps reflecting personal health, family circumstances, social networks, changes in schooling or skills, or learning about job opportunities). Measuring the separation response to these components of the wage is not informative about the central question of monopsony power, which measures the responsive-

¹In this paper, for convenience, we will refer to the elasticity of labor supply facing a firm, or residual labor supply elasticity, simply as the “labor supply elasticity.” Note that this is not the elasticity of labor supply to the market.

ness of a firm’s labor supply to the component of wages that is specifically due to arbitrary differences in wages set by employers. This discrepancy may perhaps explain why recent quasi-experimental estimates of labor supply elasticity tend to find values between 2 and 5 (Caldwell and Oehlsen (2018), Cho (2018), Kroft et al. (2020), see also the multiple estimates from Dube, Manning, and Naidu (2019) and the meta-analysis by Sokolova and Sorensen (2018)), even though the most recent papers using the traditional approach (e.g., Webber (2015) and Bachmann, Demir, and Frings (2018)) continue to find much smaller elasticities of between 1 and 1.2.² To the best of our knowledge, no paper has estimated labor supply elasticities using the firm component of pay. The Appendix to Card, Heining, and Kline (2013) considers a regression similar to ours (where they regress tenure on firm effects), without interpreting the coefficients as firm labor supply elasticities.

A final concern is that many of the existing papers rely on quarterly or annual earnings (rather than hourly wages), which may create additional bias. Most importantly, use of earnings is likely to attenuate the estimated labor supply elasticity due to the measurement error associated with hours. On the other hand, if hours are correlated with unobserved heterogeneity in separations, 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 these concerns. Using hourly wage information from matched employer-employee data from Oregon from 2000-2017³, we identify the separation response to firm wage policies: how separations respond for otherwise similar workers who happen to start new jobs at firms paying different wages. This allows us to estimate what happens to the separations rate when firms that hire otherwise similar workers happen to pay somewhat differently. Here we draw on the “mover-based” design used in other recent contexts, such as studying the impact of location on health, intergenerational mobility, and other outcomes (e.g., Finkelstein, Gentzkow, and

²Additionally, Dube, Giuliano, and Leonard (2019) 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.

³This contrasts with other matched employer-employee dataset like the Longitudinal Employer Household Dynamics (LEHD) data in the US or matched employer-employee data in many European countries.

Williams (2016), Chetty and Hendren (2018)).

As a first pass, we isolate the component of individual wages determined by firm wage policies using the log additively separable model proposed by Abowd, Kramarz, and Margolis (1999)—hereafter AKM. We take the estimated firm effects, and estimate the effect of just this component of the wage on separations. Similar to previous work, we find firms play an important role in wage setting, though the use of hourly wages reduces the firm effect contribution to log wage variance from 19 to 14 percent; we also find clear evidence of rising sorting over time between high-wage workers and high-wage firms in Oregon. Use of the AKM firm effect allows us to focus on the wage variation that is likely arising 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 show, firms with different AKM effects may also systematically draw different types of workers, which confounds our ability to use aggregate, firm-level variation in AKM and separation rates to identify labor market power. In addition, there is a concern that the AKM approach does not allow the assignment of workers to firms to be based on “match effects,” something we find in our data. For these reasons, we develop a matched event-study approach in which we consider workers with very similar past histories (in terms of wage levels, growth, past employers, and past tenure) who happen to start new jobs at firms with different co-worker wages and hence receive different wage bumps, and we then track their subsequent re-separation response. This refinement allows us to control for much richer forms of worker-level heterogeneity in both wage and separation dynamics that are predicted by past outcomes and history.

We find that the firm component of wage—as measured using either AKM or our matched event-study approach—are clearly negatively correlated with the overall separation rate and particularly the job-to-job separation rate, consistent with the firm effects reflecting “better jobs.” The baseline AKM-based separations elasticity is around -1.4, where 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 expected. The separations elasticity

estimate from our preferred matched event-study approach is -2.1. These results imply labor supply elasticities of around 3 and 4, respectively. Importantly, use of the firm component of wages increases the labor supply elasticity estimates by a factor of 2.5 to 4 as compared to the standard approach using individual wages. Our preferred labor supply elasticity of 4.2 suggests a moderate amount of monopsony power in the U.S. labor market, but much less than the very high degree of labor market power suggested using the traditional approach—which tends to generate labor supply elasticities that are one-third or one-fourth as large as the ones we find here. To put this in perspective, the traditional approach suggests markdowns of around 50%, while our estimates suggest markdowns of around 20%.

While our labor supply estimates are substantially larger than those using the standard approach, we confirm that the labor supply elasticity is procyclical—similar to the findings in Webber (2018); while it was around 4.0 during the recessionary period 2008-2010, it rose to around 4.8 during the balance of the 2004-2014 period. Importantly, we find that the degree of monopsony power is substantially larger in low-wage labor markets. For example, the labor supply elasticity is around 2.4 in art, accommodation and food services, while it is around 7.8 in professional, business, and financial services. Similarly, we find the labor supply elasticity to be smaller (2.9) in the bottom quartile of prior wages than for the top quartile (4.6). We find some evidence consistent with the relevance of labor market concentration: the labor supply elasticity in the (less concentrated) Portland metro area is around 4.6, as opposed to 4.0 in rest of Oregon. However, these differences are modest and could reflect a wide variety of differences beyond concentration between the urban and rural labor markets. Indeed, when we calculate county-industry-year HHI, we find no evidence that labor supply elasticities are decreasing with concentration, as measured using either payroll or employment. 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 from the

AKM-based model, and highlights potential issues with that strategy. Section 5 presents empirical results from the matched event-study approach. Section 6 concludes.

2 Data

As part of the Oregon’s unemployment insurance (UI) payroll tax requirements, all employers are obliged to report both the quarterly earnings and quarterly hours worked for all employees.⁴ We obtained Oregon’s micro-data as part of a data sharing agreement with the state, allowing us to construct hourly wage information for nearly all workers 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 contribution reports submitted by the private sector as well as government employers for the purposes of unemployment insurance. We use 18 years of data from 2000-2017, or 72 quarters; this dataset consists of around 136 million observations that correspond to 317,000 different firms and 5.3 million workers. An advantage of this data is that we observe quarterly wages as well as hours for each worker, allowing us to gain precision in distinguishing, for example, higher paid part-time workers from lower paid full-time workers. We observe all employer-employee quarterly matches: therefore, in the unprocessed data, a worker may have multiple observations in a given quarter that have been reported by different firms. Oregon has a median household income that is close to the national median, one which has historically followed similar trends. Oregon experienced recessions in 2001-2002 and 2008-2009 along with the rest of the country, and this is included in our sample period.

Our sample construction attempts to follow the literature using matched employer-employee data as exemplified by Card, Heining, and Kline (2013), Lachowska et al. (2020), Lamadon, Mogstad, and Setzler (2019), Song et al. (2018), and Sorkin (2018). We describe the steps and justifications in much greater detail in our Online Appendix B; here we provide

⁴Only three other states (Washington, Minnesota and Rhode Island) require employers to similarly report hours of work as part of their UI systems.

a summary. We drop employment spells (consecutive quarter runs with the same employer) with less than 100 hours per quarter on average over the spell, with any wage less than \$2/hour, and spells that are less than 3 quarters in length (which is the necessary duration to obtain at least one full quarter of wage information). Where spells overlap, we convert to a worker-level quarterly panel by selecting the spell with the highest average earnings. We restrict the data to private-sector firms with more than 20 employees; this is similar to Song et al. (2018), although in our case the restriction is based on state-level employment. This allows for meaningful estimation of within-firm statistics, and as we show, this also mitigates the impact of limited mobility bias in estimating firm effects. After applying these screens, our final dataset consists of 87.6 million observations and contains information on 3.4 million workers and 55,000 firms. Table A1 in the appendix summarizes the data by 6-year periods (the findings are also discussed below in section 4.1). 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 Oregon median of \$39,000, once comparable restrictions are made⁵. The average quarterly separation rate is 0.08, and about half of all hires come directly from other firms.⁶ We observe more than one firm for 40% of workers within each 6-year panel. As we explain later, *movers* between firms drive the identification of the firm effects.

One limitation of using data from a single state is that separations to firms outside Oregon are not counted as job-to-job separations, but rather job-to-non-employment separations. However, we note that for our primary analysis using all separations, the precise destination is immaterial. Moreover, any bias in estimating the job-to-job component of the elasticity is likely limited given the share of workers who likely moved out of Oregon (3% in 2016, based on data from American Community Survey) is much smaller than the share of workers

⁵Song et al. (2018) exclude workers who earn less than 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.

⁶The quarterly separation rate is 0.17 before sample restrictions, which is similar to the separation rate of 0.15 reported by Webber (2015) using the LEHD.

leaving their jobs in our main sample (26% in 2016).

3 Research design

We begin by sketching a simple model of dynamic monopsony, and relate it to statistical models of wage determination (like AKM). Suppose a worker i employed at firm j , denoted by f_{ijt} , transitions to firm j' . As a starting point, assume that worker's marginal product has worker-specific component A_i that is fixed across firms and, crucially for our approach, does not affect transition probabilities across firms. Marginal productivity also has a firm-specific component denoted p_j , with overall match marginal product given by $y_{ij} = A_i p_j$. We denote as $Pr(f_{ij't+1}|f_{ijt})$ the probability of transitioning to firm j' at time $t+1$ given i was at firm j at time t , so $s_{ijt} \equiv 1 - Pr(f_{ij't+1}|f_{ijt})$ is the separations rate. In a stationary distribution, $\sum_{j'} Pr(f_{ij't}) Pr(f_{ijt}|f_{ij't}) = Pr(f_{ijt})$. Rewriting the steady-state condition, defining R_{ij} and q_{ij} as total recruit and employment probabilities, respectively, of type i by firm j , and suppressing time subscripts we can define steady state as:

$$\underbrace{\sum_{j' \neq j} Pr(f_{ij}|f_{ij'}) Pr(f_{ij'})}_{R_{ij}} = \underbrace{Pr(f_{ij})}_{q_{ij}} \underbrace{(1 - Pr(f_{ij}|f_{ij}))}_{s_{ij}}$$

In steady state, a monopsonist will choose wages to pay workers of type i to maximize $\sum_i q_{ij}(A_i p_j - W_{ij})$ subject to $q_{ij} = \frac{R_{ij}(W_{ij})}{s_{ij}(W_{ij})}$. The marginal cost of employment of i at probability q_{ij} is $W_{ij}(q_{ij})(1 + \frac{dw_{ij}}{d \log(q_{ij})})$ where $w_{ij} \equiv \log W_{ij}$. Since the labor-supply elasticity is solely a function of the firm component of wages, we impose that $\frac{dw_{ij}}{d \log(q_{ij})} = \frac{1}{\epsilon_j}$ is constant for all i given j . At the optimum, we will have that the log wage is $w_{ij} = \alpha_i + \phi_j$, where $\alpha_i \equiv \log(A_i)$ is the portable component of wages (e.g., skill, but could reflect other factors) while $\phi_j \equiv \log(\beta_j p_j)$ is the firm-specific component of the wage that is chosen by firms, with a markdown of $\beta_j = \frac{\epsilon_j}{1+\epsilon_j}$. Since the portable component α_i is common across firms, the key assumption we are making is that only the firm-specific component of the wage changes

along with the employer's choice of q , and so the marginal cost of additional employment is $W_{ij}(q_{ij})(1 + \frac{d\phi_j}{d\log(q_{ij})})$, or equivalently that labor supply is solely a function of ϕ_j and $\frac{dw_{ij}}{d\log(q_{ij})} = \frac{d\phi_j}{d\log(q_{ij})}$. But by the steady-state assumption, $\frac{d\phi_j}{d\log(q_{ij})} = \frac{1}{\gamma(\phi_j) - \eta(\phi_j)}$, where $\gamma(\phi_j) = \frac{1}{E[R_{ij}]} \frac{dE[R_{ij}]}{d\phi_j}$ and $\eta(\phi_j) = \frac{1}{E[s_{ij}]} \frac{dE[s_{ij}]}{d\phi_j}$ are the recruitment and separation elasticities, respectively. The labor supply elasticity facing the firm is given by $\epsilon(\phi_j) = \gamma(\phi_j) - \eta(\phi_j)$. Further, if both η and γ are constant, as Manning (2003) imposes in his empirical implementation and most subsequent work in this sub-literature, then it is easy to see that⁷ $-\eta = \gamma$ and so we have $\epsilon = -2\eta$, which ties the separations elasticity to half the labor supply elasticity. Even when the separations elasticity is not constant but the recruitment elasticity is, the recruitment elasticity is a simple weighted average of the separations elasticities for each firm: $\gamma = \sum_j \omega_j \eta_j$ where $\omega_j = \frac{s_j N_j}{\sum_j s_j N_j}$ is the share of all separations from firm j . More generally, even when both the recruit and separation elasticities are heterogeneous, the average recruitment and elasticities are equal for some set of weights (Manning 2003, pp. 96-104).

By imposing firm-specific elasticities that are common to all workers and having output $y_{ij} = A_i p_j$ we are ruling out complementarity in log productivity and heterogeneous firm-labor supply curves across workers within a firm. Both of these would generate worker-firm specific wages, violating the AKM decomposition of wages. Complementarity in log productivity and heterogeneous labor supply elasticities would imply that log wages $w_{ij} = y_{ij} + \beta_{ij}$ where y_{ij} is match-specific productivity and β_{ij} is a match-specific markdown (for example due to firm-specific wage discrimination policies, as in Card, Cardoso, and Kline (2016)). The AKM decomposition would not be identified when pooled across types of workers, and even if attention were limited to exogenous firm switches, it could be a poor fit, and even if firm-effects were estimated, the probability q_{ij} would depend on (all of) w_{ij} , not just the ϕ_j component. But a fact that we will use below is that even without assuming the AKM decomposition, we can isolate the variation in wages changes that are common to workers transitioning to a given firm j , by instrumenting $w_{ij} - w_{ij'}$ for a given worker with

⁷Differentiating the steady-state condition with respect to log wage and summing gives $\sum_j R_{ij} \gamma(\phi_j) = -\sum_j s_{ij} \eta(\phi_j)$ and total recruits must equal total separations.

the average difference in log wages across firms $\bar{w}_j - \bar{w}_{j'}$. Therefore, our general framework allows for a firm-component of wage that may be heterogeneous across worker types, and allows the labor supply elasticity to be heterogeneous as well.

The traditional approach to estimating the separations elasticity is to simply regress a worker’s separation rate (or hazard) on own log wages, and to check robustness to controls. But from the firm’s perspective, the relevant separations elasticity η is based on what happens as the firm changes its wage policy, which in this context is varying ϕ_j , and so an estimate of the separations elasticity facing the firm will be given by:

$$E[s_{ijt}|w_{ijt}] = E[s_{ijt}|\phi_{j(i,t)}] = \eta(\phi_{j(i,t)}) \quad (1)$$

Where s_{ijt} takes on the value of 1 when worker i leaves firm j at time t . We can recover an estimate of the elasticity from the slope of this curve via $\hat{\eta} = \frac{\eta'(\phi_{j(i,t)})}{E[s_j]}$. However, if we simply use w_{ijt} as the key independent variable, instead of isolating the firm-specific component, then we will identify $\tilde{\eta} = \sigma\eta$ where $\sigma = \frac{\text{var}(\phi_{j(i,t)})}{\text{var}(w_{ijt})}$ is the share of the variation in wages that is due to firm effects if equation (1) is correctly specified. 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 component of a worker’s wage that is invariant to the firm. We would expect the separation to be higher if it is a “bad job” (i.e., ϕ_j is lower) because in this case there is a greater chance of the worker receiving offers that dominate current employment. In our data, firm effects explain roughly 14 percent of the hourly wage variation (see section 4.1). This suggests that the standard approach may recover an estimate that is roughly one-seventh as large, and so the use of individual level wages can significantly overstate the extent of monopsony power. In practice, if $Cov(\alpha_i, \phi_{j(i)}) \neq 0$, and there is sorting of workers and firms, the extent of bias will also depend on the covariance term. However, as we will see below, with sorting, the identification strategy based on equation (1) is unlikely to be valid as firms with high ϕ_j may be attracting very different types of workers.

3.1 Approach based on AKM

The previous section establishes the importance of focusing on the firm-specific component of wage variation when estimating the degree of monopsony power in the market. What is the best way to accomplish this? One approach builds on AKM and Card, Heining, and Kline (2013). We begin with the Card-Heining-Kline (henceforth CHK) assumption necessary to identify the coefficients ϕ_j in the wage regression specification given by

$$w_{ijt} = \sum_j \phi_j f_{ijt} + \alpha_i + \alpha_t + \epsilon_{ijt} \quad (2)$$

Where f_{ijt} is an indicator variable denoting whether worker i is employed at firm j at time t , α_i is a worker fixed effect, and ϵ is an error term.⁸ CHK give a sufficient condition for identification:

$$f_{ijt} = E(\mathbf{J}_{it} = j) = E(\mathbf{J}_{it} = j | \epsilon) = G_{jt}(\phi_1, \dots, \phi_J, \alpha_i) \quad (3)$$

Equation 3 says that the probability of a worker being employed by a particular firm is a function of only the firm wage effects and the worker fixed effects. On its own, G does not impose severe economic restrictions on the assignment process between workers and firms, and is consistent with assignment rules that include both sorting of high ability workers to high-wage employers as well as high productivity employers paying higher wages for identical workers. However, to interpret a regression of firm separations on firm wage effects as reflecting the causal separations elasticity facing firms, we need to impose further assumptions on G . Namely, we need f_{ijt} to be a monotonic and increasing function of ϕ_j , independent of the worker’s type and independent of the wage policies of other firms. With these assumptions, we can decompose the assignment function into a monopsonistically competitive “labor supply component” that depends only on the firm effect ϕ_j and a “non-monopsony” component h , which includes effects of sorting and strategic-interactions effects

⁸CHK also include an autocorrelation parameter in the error.

that depend on the worker effect α_i and the other firms ϕ_k . If the residual labor supply curve were the only constraint on the firm, and there was no sorting, equation 1 would obtain with a very strict monopsony-like structure on G that is more than sufficient:

$$Pr(f_{ijt}) = G_{jt}(\phi_1, \dots, \phi_J, \alpha_i) = \epsilon(\phi_{j(i,t)}) = -2\eta(\phi_{j(i,t)}) \quad (4)$$

Under equation (4), we have the empirical elasticity given by $\frac{1}{E[s_{ij}]} \frac{ds_{ij}}{d\phi_j} = -\frac{1}{E[s_{ij}]} \frac{dPr(f_{ijt}|f_{ijt})}{d\phi_j} = -\frac{1}{E[s_{ij}]} \frac{1}{2} \frac{Pr(f_{ijt})}{d\phi_j} = \hat{\eta}$. Note that any approach that regresses separations on firm effects must rule out pure sorting, i.e., $Cov(\alpha_i, \phi_j) > 0$, if we allow α_i to have an effect on firm assignment f_{ijt} . Sorting is allowed by 3 but would violate the identifying assumption needed to recover the causal separation response from a regression of firm separations on firm wage effects. But note that we can allow heterogeneity in η as a function of worker fixed effects and other firm effects, so long as they only interact with the labor-supply component. For example, we can admit a function, $G_{jt}(\phi_1, \dots, \phi_J, \alpha_i) = \epsilon(\phi_j, \{\phi_{j'}\}_{j' \neq j}) + h(\alpha_i, \{\phi_{j'}\}_{j' \neq j})$; when we do this, we have an estimated elasticity given by $\hat{\eta} = \frac{1}{E[s_{ij}]} \frac{ds_{ij}}{d\phi_j} = \frac{1}{E[s_{ij}]} \int \eta_{\phi_j}(\phi_j, \{\phi_{j'}\}_{j' \neq j}) dH(\{\phi_k\})$, where H is the distribution of the firm wage effects. i.e., heterogeneity based on the wage policies of other employers. Note that η cannot depend on the individual worker wage effects in our framework above, because this would induce worker specific markdowns within a firm and violate the additive separability of wages in AKM.

What we cannot admit is a function of the form $G_{jt}(\{\phi_{j'}\}, \alpha_i) = \epsilon(\alpha_i, \{\phi_{j'}\}_{j' \neq j}, \phi_j) + h(\alpha_i, \phi_j, \{\phi_{j'}\}_{j' \neq j},)$; if $h_{\phi_j} \neq 0$, then regressing s_j on ϕ_j does not produce a consistent causal estimate of η because ϕ_j also affects separations via h . For example, h could capture the sorting: the fact that certain workers may be both high α type and sort into firms with higher ϕ_j and be less likely to separate is an example of this bias as in Shimer and Smith (2001). While this form of G is still sufficient to identify AKM, it is not sufficient to identify the separations elasticity. This highlights an important limitation of our purely AKM-based approach, which needs to assume away the ecological fallacy. The issues here are the same as in any ecological regression: a regression of s_j on ϕ_j does not recover the causal effect

of ϕ_j on f_{jt}^i if there is sorting of workers that induces a correlation between separations and firm effects that does not operate through the labor supply elasticity.

3.2 Extension to Include Unemployment

While the approach presented above relies solely on steady states and constant elasticities, it does not apply exactly in the presence of recruits from non-employment. The method implemented by Manning (2003) augments the separation and recruitment functions above to incorporate unemployment. One equation governs the separation rate from firms that pay w into either unemployment (EU) or other employers (EE):

$$s(w) = s^{EU}(w) + s^{EE}(w) \quad (5)$$

The second equation governs the recruitment rate into firms paying w , and similarly, recruits are given by

$$R(w) = R^{UE}(w) + R^{EE}(w)$$

Manning then breaks these equations up 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 negative of the separations elasticity, η^{EE} , is equal to the recruitment elasticity from employment γ^{EE} , and we get

$$\epsilon = -(\theta_R + \theta_S)\eta^{EE} - (1 - \theta_S)\eta^{EU} + (1 - \theta_R)\gamma^{UE} = -(1 + \theta_R)\eta^{EE} - (1 - \theta_R)\eta^{EU} - \gamma_{\theta}^{EE} \quad (6)$$

where θ_S and θ_R give the proportion of separations to and recruits from employment,

and $\gamma_{\theta}^{EE} = (1 - \theta_R)(\gamma^{EE} - \gamma^{UE})$ is the elasticity of the share of recruits out of employment.. The last equality follows because in steady-state, $\theta_S = \theta_R$, since the flows out of employment equal the flows into employment and the total flows between employers nets to 0. The “augmented-Manning-approach” versus the simpler “2-times-the-separations-elasticity” approach may yield similar estimates if the elasticity of the share of recruits from non-employment (γ_{θ}^{UE}) is small and if the separation elasticities into employment and non-employment are similarly sized. As we will see below, in practice, this seems to be the case in our sample.

3.3 Estimation

One additional challenge in implementing the above approach is that the AKM effects are estimated, leading to the usual generated regressor problem. We address this using sample splitting, in which we randomly split the workers (in each 6-year period) into two groups, A and B, stratified on moving. (The sample-splitting approach was also used by Goldschmidt and Schmieder (2017).) Using these two samples, we generate two sets of AKM firm effects, $\hat{\phi}_j^A$ and $\hat{\phi}_j^B$.⁹ Next, we take the individuals in sample *A* and regress s_{ijt} on $\hat{\phi}_j^A$ while instrumenting the latter with $\hat{\phi}_j^B$. This ensures that a worker’s separation indicator is not entering into both the right and the left side of the equation, thus eliminating any mechanical correlation induced by an individual’s separation influencing the estimate of $\hat{\phi}_j$. In addition, because the $\hat{\phi}_j^A$ and $\hat{\phi}_j^B$ are from separate samples, assuming that the estimation errors are uncorrelated, we can use the latter to instrument the former to alleviate the attenuation bias stemming from a generated regressor.

After decomposing wages, we estimate the following equation:

⁹Sample splitting means that the connected sets used to estimate ϕ_j vary in samples *A* and *B*. However, in practice, there is a very high degree of overlap in the connected sets: 99.9% of firms in the pooled connected set are also in the A-connected set; and 99.8% of them are in B-connected set. (Moreover, the correlation coefficient between $\hat{\phi}_j^A$ and $\hat{\phi}_j^B$ is 0.965.)

$$s_{ijt} = \sum_j \eta \hat{\phi}_j f_{jt}^i + X_{it}\Gamma + v_{ijt} \quad (7)$$

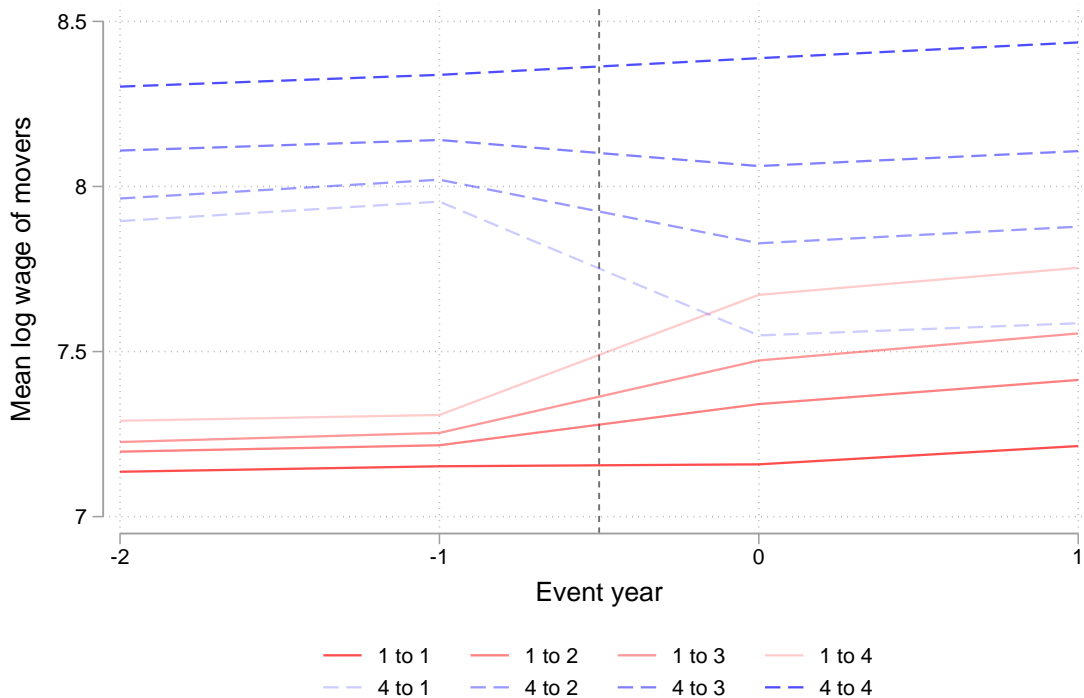
We calculate the firm effects using the AKM approach, by 6-year periods. The details of implementation, including assessment of limited mobility bias, are provided in Online Appendix C. After estimating the AKM model, we decompose the variance of the wage in the worker and firm effects, as in CHK and Song et al. 2018. 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. However, as we discuss below, the core elasticity estimates are not substantially affected by the trimming.

4 Results From AKM-based Model

4.1 Descriptive statistics and wage inequality in Oregon’s administrative data

During the 2000-2017 period, the variance in log hourly wages in our Oregon estimation sample was mostly stable. A similar pattern is observed when we consider hourly or quarterly earnings, and when we consider the full sample of workers or our main estimation sample (restricting by firm size and earnings, as described in the data section). However, the variance of log wages masks considerable heterogeneity in trends by wage percentile, as shown in Appendix Figure B3. During this period, the largest growth in hourly wages occurred at the top (e.g., 90th and 95th percentiles), while the real wage fell in net in the middle (50th percentile). However, during the same period, wages rose faster at the bottom (5th and 10th percentiles); in part, this was likely due to Oregon’s minimum wage policies. So in sum, hourly wage inequality grew in the upper half of the distribution, mirroring other states (e.g., Lachowska et al. (2020)), even while it fell in the bottom half. The patterns

Figure 1: **Changes in hourly wages across job separations for firm quartile-to-quartile transitions**



Note: The legend indicates origin quartile to destination quartile, where quartiles are defined along the distribution of the average firm wage, using only workers who stay at the firm over the 6-year period. The change in wage is shown for movers, who are defined as workers who make a between-firm job-to-job transition at any point during the period and are observed for at least 9 consecutive quarters at the each firm before and after the move. The quarter of separation and the following quarter are omitted. This exercise is repeated for each 6-year period (2000-2005, 2006-2011 and 2012-2017), the mover wage profiles are stacked, and the averages of the event quarter are plotted by quartile-transition categories.

are qualitatively similar if we instead consider quarterly earnings; however, the 90-50 gap in earnings grew somewhat more than the equivalent gap in hourly wages over this period.

Appendix Table C1 provides the AKM decomposition in wage and earnings inequality for 6-year blocks between 2000-2017, as well as for the full panel. For both log quarterly earnings and log hourly wages, there is a slight increase in the overall variance between the 2000-2005 and 2012-2017 periods (0.37 to 0.41 for wages, and 0.59 to 0.64 for earnings). In the full panel, firm effects explain around 19% (14%) of the variance of quarterly earnings (hourly wages), and worker effects explain around 48% (55%) of the variance. This is similar to the

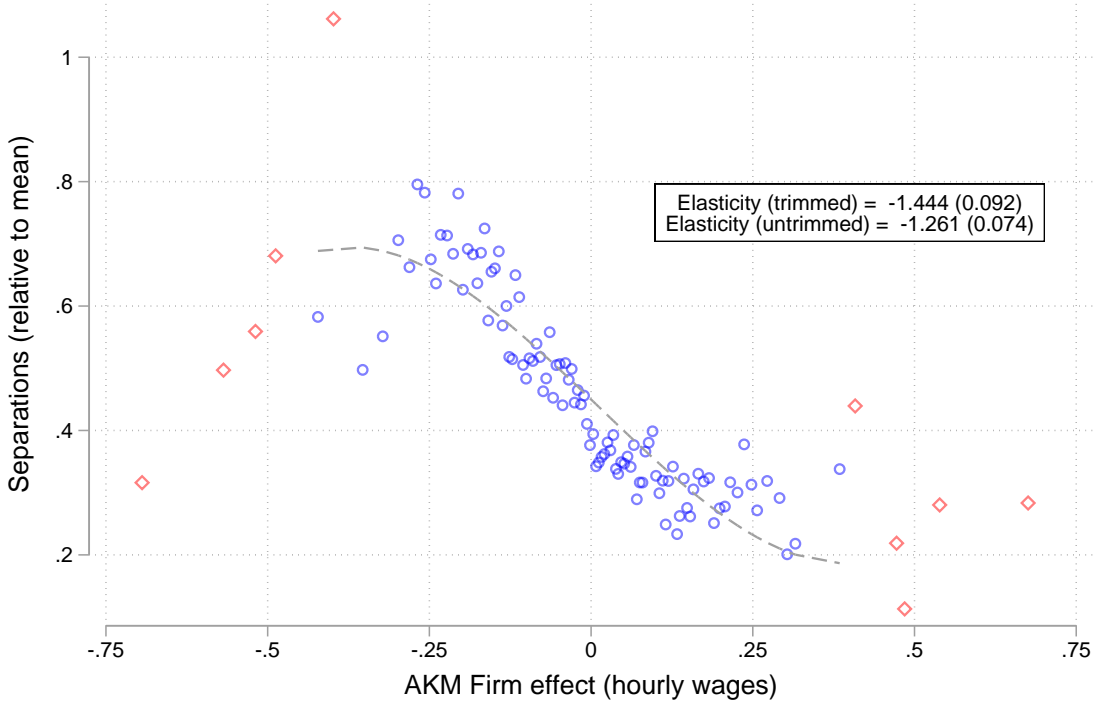
findings of Lachowska et al. (2020) using hourly wage data from the state of Washington; they estimate the firm effects’ share of variance to be 19% and 12% of log earnings and log wages, respectively. There is also assortative matching of workers and firms, with the covariance term explaining around 14% (18%) of the variance. Consistent with other work (e.g., Song et al. 2018), we see a clear increase in the covariance term for both wages and earnings over this period consistent with greater sorting: for quarterly earnings (hourly wages), the contribution of the covariance term rises from 11% (14%) in 2000-2005 period to 14% (17%) in the 2012-2017 period. At the same time, there is a slight increase in the firm component of quarterly earnings variance, but a small decrease in the case of hourly wages. Broadly, again, these trends are similar to the findings of Lachowska et al. (2020) using hourly wage data from Washington. We discuss further details of the AKM estimation in Appendix C, including an evaluation of limited mobility bias, which we conclude is not a major concern in our context given our relatively long (6-year) and higher frequency sample.

4.2 AKM-based separations elasticities

Figure 1 replicates the event study figure illustrating interquartile transitions in Card, Heining, and Kline (2013) and shows largely parallel trends prior to a transition, similar to Card, Heining, and Kline (2013). In Appendix Figure A1 we augment this picture with size of flows, showing that the separation rates of firms in these quartiles behave as expected, where separations from low-wage firms to high-wage firms are more frequent than separations from high-wage firms to low-wage firms, even though the wage changes are symmetric (see figure A2).

Figure 2 presents the key findings of this section. Using a control function approach, the binned scatter plot shows the overall separations rate (divided by the average separations rate) against the AKM firm fixed effects in hourly wages, controlling for the first stage residuals (where AKM firm effects using one sample are instrumented by the firm effects estimated using the other sample). The AKM model is estimated using stacked 6-year

Figure 2: 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 separations on own-sample firm effects. The plotted points show the binned scatter points of this latter regression (i.e., depicting the partial correlation). The vertical axis is separations divided by mean separations such that the slope of the line represents the elasticity. 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 a cubic polynomial fitted to the trimmed sample.

samples, so this is a stacked panel. The figure shows a clear, negative relationship between separations and firm effects on log wages, with a precisely estimated average separations elasticity of -1.4 after trimming 2.5 percent of the sample from above and below. (The untrimmed estimate is -1.3.) We present the analogous figures for E-E separations, E-E recruits, and the Labor Supply Elasticity in Appendix Figures A3-5.

Table 1 shows the results of our regressions using a variety of outcome variables. All regressions are run at the individual worker level, clustered by firm and control for 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 separations (E-N), and employment-to-employment recruits (E-E recruits, which are restricted to observations corresponding to hires only). We then present the share of recruits from employment, and calculate labor supply elasticities based on equation 6, with standard errors calculated via the delta method; but as we will see in our main specifications, they are remarkably similar to those implied by simply doubling the separations elasticity. Column 1 shows the standard hazard rate specification using quarterly earnings: the separations elasticity is -0.282 , and the implied labor-supply elasticity ϵ is very small (0.355). Column 2 uses hourly wages instead and produces somewhat larger magnitudes of separations and labor supply elasticities (-0.510 and 0.879 , respectively), although they are still quite small. Column 3 uses a linear probability model instead of the hazard model, and the resulting separations elasticities all increase (with only a small decrease in the E-E recruitment elasticity); the resulting estimate of ϵ almost doubles relative to columns 1 and 2, but at 1.345 , it is still low. 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 4-5 use firm effects instead of individual wages as the key independent variable, and column 4 shows that this results in larger separations elasticity (-1.342 for all separations). The resulting estimates of ϵ are around 2.69 . Column 5 (preferred AKM-based specification) uses sample splitting to instrument the firm fixed effect in order to correct for attenuation bias of a generated regressor. Doing so increases the magnitude of the separations elasticity modestly to -1.448 and the labor supply elasticity to 2.912 . Importantly, accounting for recruits from non-employment in calculating the elasticity does little to the estimates in columns 4 and 5; instead, had we simply used the rule of multiplying the separations elasticity by -2 , we would have obtained labor supply elasticities that are nearly identical.

Table 2 shows how these results vary based on different specifications and controls. Columns 1 and 2 show that the sample-splitting IV modestly increases the magnitudes

Table 1: **Separations and recruits elasticities to firm component of wage using AKM**

	Wage			Firm FE	
	(1)	(2)	(3)	(4)	(5)
All separations	-0.282 (0.005)	-0.51 (0.01)	-0.622 (0.015)	-1.342 (0.085)	-1.448 (0.095)
E-E separations	-0.317 (0.007)	-0.533 (0.013)	-0.753 (0.023)	-1.677 (0.127)	-1.811 (0.141)
E-N separations	-0.291 (0.005)	-0.422 (0.01)	-0.578 (0.014)	-1.209 (0.075)	-1.303 (0.085)
E-E recruits	0.266 (0.022)	0.127 (0.031)	0.067 (0.017)	0.413 (0.059)	0.438 (0.064)
Pct. EE-recruits	0.47	0.47	0.464	0.464	0.465
Labor Supply Elasticity	0.355 (0.024)	0.879 (0.037)	1.345 (0.039)	2.69 (0.199)	2.912 (0.221)
Obs (millions)	7.348	7.348	69.072	69.072	68.553
Log hourly wage		Y	Y	Y	Y
Hazard spec.	Y	Y			
Firm FE				Y	Y
Split-sample					Y
F-stat					9792

Note: The unit of observation for the hazard specifications is an employment spell, and for the linear specifications is each worker-quarter record. The column 1 regressor is log quarterly wage. Elasticities are reported in each cell for the linear specifications, by dividing the regression coefficient by the corresponding sample mean of the outcome. Pct. E-E recruits indicates the average proportion of hires from employment. The first stage F-stat is given for the row 1 regression. Firm fixed effects are censored at the 2.5 percent tails of the firm FE distribution. Standard errors are shown in parentheses.

of the separations elasticity in the hazard specification as well. Column 3 shows that use of annual (quarterly) earnings in place of hourly wage produces a substantially smaller separations elasticity (-0.776 (-0.809) instead of -1.448 in column 5 of Table 1); this highlights the importance of using hourly wage data. In contrast, the separations elasticity estimates are fairly robust to other changes we consider. Without trimming the firm effects distribution, the separations elasticity is -1.262. Controlling for tenure changes the separations elasticity to -1.228. Including controls for industry (1-digit level) by county fixed effects results in a labor supply elasticity of -1.336; controlling for industry and tenure produces an estimate of -1.406. (We recognize that controlling for past tenure when estimating the separation response is problematic, as it is related to the outcome; we are able to do this much more carefully in our worker-level matched-event study design.)

4.3 Testing the Assumptions of the AKM-based approach

There are two core assumptions at the heart of our approach. The first is that AKM is identified, that is, equations 2 and assumption 3 hold. The second is that equation 4 holds, so the co-variation between separations and firm effects is driven by movements along the (possibly heterogeneous) residual labor supply curve, not other omitted variables (e.g., sorting) that are correlated with firm wages and separations. Let us examine these assumptions in turn.

The first assumption is that there are no other omitted variables contaminating the relationship between s_{ij} and ϕ_j . As discussed above, controlling for worker wage effect α_i should not affect the estimate of η ; the fact that it does could be a violation of the identifying assumption for our separations regression. Even if the assumptions underlying AKM as a statistical model of wages are correct, non-causal sorting of workers can present an important problem for using the relationship between AKM firm effects and separations. For example, if high-wage workers sort to high-wage firms (as is the case empirically), and high-wage workers have different exogenous (to wage) separation rates, it is difficult to separate the firm-versus-

Table 2: **Alternative specifications for separations and recruit elasticities to firm component of wage using AKM**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
All separations	-0.878 (0.066)	-0.936 (0.071)	-0.776 (0.033)	-0.809 (0.039)	-1.262 (0.075)	-1.228 (0.065)	-1.336 (0.055)	-1.406 (0.063)
E-E separations	-0.866 (0.057)	-0.913 (0.061)	-0.946 (0.053)	-0.987 (0.065)	-1.607 (0.115)	-1.535 (0.109)	-1.545 (0.08)	-1.553 (0.102)
N-E separations	-0.709 (0.054)	-0.752 (0.058)	-0.857 (0.033)	-0.739 (0.034)	-1.115 (0.066)	-1.161 (0.053)	-1.191 (0.05)	-1.293 (0.048)
E-E recruits	0.783 (0.112)	0.832 (0.121)	0.493 (0.042)	0.349 (0.045)	0.354 (0.071)	0.442 (0.064)	0.323 (0.064)	0.338 (0.075)
Pct. EE-recruits	0.464	0.465	0.43	0.467	0.463	0.465	0.466	0.465
Labor Supply Elasticity	0.865 (0.143)	0.908 (0.154)	1.348 (0.089)	1.493 (0.107)	2.597 (0.186)	2.429 (0.174)	2.578 (0.136)	2.629 (0.169)
Obs (millions)	7.348	7.304	16.45	77.767	70.609	51.92	41.796	51.629
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y
Split-Sample		Y	Y	Y	Y	Y	Y	Y
F-stat			4586	12043	8637	9820	11015	9266
Hazard spec.	Y	Y						
Annual earnings			Y					
Quarterly earnings				Y				
No trimming					Y			
<i>Controls</i>								
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 hazard specifications is an employment spell, and for the linear specifications, it is each job-quarter record. Column 2 uses the split sample in a control function for the hazard specification. Annual earnings indicates the annualized panel (one observation per worker-year), from which the AKM firm FEs (using log annual earnings) and separations variables are estimated. Quarterly earnings indicates AKM firm FEs estimated with quarterly earnings. Elasticities are reported in each cell for the linear specifications, by dividing the regression coefficient by the corresponding sample mean of the outcome. Tenure refers to the number of quarters since the job started, is coded as a continuous variable and includes control terms up to a quadratic power of tenure. Industry is defined at the 1-digit level. Firm fixed effects are censored at the 2.5 percent tails of the firm FE distribution, except where ‘No trimming’ is indicated. Standard errors are shown in parentheses are clustered at the firm level.

worker component of separations. Moreover, there may be other systematic differences in exogenous separations at high- versus low-wage firms: for example, if workers at higher wage firms tend to be more connected (and hence have greater rates of separations) this could confound the relationship between the firm effect and separation rates. As a test for these concerns, we consider how separations respond to various components of the wage effects (i.e., worker, firm, average match residuals) in Table A1. In column 1, we reproduce the baseline OLS estimates from column 4 of Table 1.¹⁰ In supercolumn 2, we report estimates from regressing separations on firm the fixed effect as well as the worker fixed effect. We find that inclusion of the estimated worker fixed effects greatly reduces the magnitude of the firm effects coefficient (from -1.3 to -0.7). This highlights the challenge that the sorting of high-wage workers to high-wage firms presents for the ecological regression. Moreover, it's not clear that inclusion of the worker fixed effect actually reduces bias. When there are multiple dimensions of heterogeneity in exogenous separations, controlling for one dimension may even increase overall bias. For example, if high wage firms attract both higher skilled workers (with lower exogenous separations) and more connected workers (with higher exogenous separations), simply controlling for the AKM worker fixed effect would tend to exacerbate the bias from the other omitted variable (connectedness). Overall, then, the sensitivity of the separations elasticity to the inclusion of worker fixed effects (in wages) makes it difficult to assess the causal import of the AKM-based findings.

A second issue arises from whether the AKM assumption about mobility does, indeed, hold in our data. An important assumption shared by both our model and the AKM framework generally is that match-specific wage effects are irrelevant for firm assignment. If we denote by μ_{ij} the match-specific component of the wage, in order for AKM to be identified, the assignment probability G_{jt} must not be a function of match effects, μ_{ij} . If it were, then the firm indicator would be correlated with match-effects in the residual. More formally, suppose $f_{jt}^i = G_{jt}(\{\phi_{j'}\}, \alpha_i, \mu_{ij})$. It follows that estimates of firm effects from $w_{ijt} = \sum_j \phi_j f_{jt}^i + \alpha_i + \epsilon_{ijt}$

¹⁰This allows for more comparability between AKM components than the preferred split-sample specification.

will be biased because $Cov(f_{jt}, \mu_{ij}) \neq 0$ and μ_{ij} is a component of ϵ_{ijt} .

CHK provide several types of evidence against the importance of match effects. First they show that unrestricted match-effects model—i.e., a separate μ_{ij} for every pair, instead of firm effects ϕ_j —does not improve the share of explained wages very much. We also find something similar: the adjusted R-square in the unrestricted match-effects model in our sample from 2000-2017 (2012-2017) is 0.88 (0.91) while the AKM model adjusted R-square is 0.84 (0.90). Second, they argue that the wage losses and gains going from lower to higher firm effect quartiles and vice versa are symmetric, and that in general there is little in the way of wage gains when moving within firm effect quartiles. If mobility were driven by match effects, we would not expect the symmetry to necessarily hold. We also provide evidence that wage changes from upward and downward movements between quartiles are symmetric (see Appendix Figure A2).

However, the fact that the μ_{ij} do not improve the share of wages explained is not dispositive about whether assignment of workers to firms depends on match-effects. We can directly test if the pattern of assignment is influenced by match effects. To do so, we compute μ_{ij} as $\hat{\mu}_{ij} = \frac{1}{T_{ij}-t_{ij}} \sum_{r=t_{ij}}^{T_{ij}} w_{ijrt} - \hat{\alpha}_i - \hat{\phi}_j$, which is the mean residual of the wage over a job spell, conditional on worker and firm effects, and check if the firm effect of the subsequent firm $\phi_{j(i,t+1)}$ is correlated with $\hat{\mu}_{ij}$. If these are indeed random effects (as assumed under AKM), they should not predict the direction of future flows. In Table 3, we consider two tests. In columns 1 to 4, the outcome is the subsequent firm’s fixed effect at date $t + 1$, which we regress on the “match effect” (mean residuals) and the firm effect at date t . Without any controls in column 1, we find that match effects are indeed predictive of future firm effects, in violation of AKM assumptions. Including controls for industry and tenure at date t in column 4 renders the coefficient small and insignificant. In columns 5 to 8 we consider the direction of change in the firm effect between dates t and $t + 1$. Here too, we find that high match effects (mean residual wage) positively predicts the direction of change in firm effects upon separation; moreover, while inclusion of industry by tenure controls reduces the

magnitude of the coefficient, it continues to be statistically significant.

Overall, these findings suggest that the assumption for identification of AKM may not hold in our sample. While the quantitative importance of μ_{ij} may be unimportant for explaining wage variation, as discussed above, it may be important for estimating separation elasticities. To clarify, the failure of AKM and the possibility of omitted variables in the separations regression need not imply that that AKM-based separations elasticities are severely biased: indeed, they may be approximately correct. However, these failures do suggests the need for an alternative strategy that does not impose the AKM assumption on the wage generating process, while still isolating the portion of wages due to firm wage policies.

This is exactly what we do in the next section, where we consider worker-level event studies where workers with very similar histories (or wages, firm assignment, past job stability) who then transition to firms with different wages, and we then follow their behavior and measure how separation rates respond to their having received a higher wage boost. Doing so helps us better isolate how separations respond to plausibly exogenous difference in wages accounting for rich forms of worker heterogeneity in both separations and wages.

5 Using Matched Movers to Identify Separations

In this section, we show that controlling for worker wage and employer histories in an event-study approach can addresses the failures in the AKM approach documented above. Instead of equation 3, suppose assignment at time t is governed by the following equation:

$$f_{ijt} = G_{jt}(\{\bar{w}_k\}, \{w_{ir}, f_{ik'r}\}_{r < t}) \quad (8)$$

Where w_{ir} and $f_{ik'r}$ are variables denoting past individual wages and firm assignments, while $\{\bar{w}_k\}$ is a vector of firm average log wages. This assumption says that the firm average wage \bar{w}_j predicts assignment, rather than the firm effect ϕ_j ; therefore, conditional on a rich set of covariates, including past wages and employment histories, the match and worker

Table 3: **Falsification test: Do match residuals predict future AKM firm quality of movers?**

	Future Firm FE				Positive change in Firm FE			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Match effect	0.058 (0.003)	0.058 (0.003)	0.060 (0.004)	-0.003 (0.000)	0.156 (0.007)	0.158 (0.006)	0.167 (0.009)	0.060 (0.001)
Firm effect	0.513 (0.011)	0.430 (0.011)	0.504 (0.011)	0.444 (0.011)	-1.045 (0.029)	-1.202 (0.031)	-1.037 (0.029)	-1.174 (0.030)
Obs	1625209	1497149	1393070	1386540	1625209	1497149	1393070	1386540
<i>Controls</i>								
Industry × county		Y				Y		
Tenure			Y				Y	
Industry × tenure				Y				Y

Note: The match effect is calculated as the average residual from the AKM by worker-firm match. The sample is restricted to E-E separation quarters. The outcomes refer respectively to the AKM firm wage effect at the new firm (columns 1-4), and an indicator for a positive change compared to the previous firm (columns 5-8). Industry has 8 categories, and tenure indicates a fourth degree polynomial.

fixed effects add no predictive value to the assignment function. Whether this assumption is weaker or stronger than the CHK assumption can be debated: CHK allow no role for histories except via a worker fixed effect, while equation 8 imposes that worker fixed effects (as well as match effects) do not matter conditional on controls for history. Unlike CHK, this assumption is non-Markovian, and allows for path-dependence, where a worker’s past employers, employment history, and past wages, influence their probability of matching with a firm j .

This implies $E[f_{jt}^i \epsilon_{ijt}] = 0$ where ϵ is from the dynamic equation below:

$$w_{ijt} = \sum_j \phi_j \bar{w}_j f_{ijt} + \underbrace{L(\{w_{ir}, f_{ik'r}\}_{r < t})}_{L(History_{i,t})} + \epsilon_{ijt} \quad (9)$$

Note that , since the history includes lagged wages and fixed effects for lagged firms,

focusing on the time of transition t , equation 9 can be rewritten as

$$w_{ijt} - w_{ijt-1} = \tilde{\phi}(\bar{w}_j - \bar{w}_{j'})(f_{jt}^i - f_{j't-1}^i) + L(\text{history}_{i,t}) + \nu_{ijt} \quad (10)$$

which is similar to the specification estimated by Finkelstein, Gentzkow, and Williams (2016), but augmented with controls; Finkelstein et al. show that under the AKM assumptions, the coefficient on the change in log average wage can be interpreted as $\tilde{\phi} = \frac{\phi_j - \phi_{j'}}{(\bar{w}_j - \bar{w}_{j'})}$, which is the share of the mean difference in log wages across firms explained by firm effects. However, we do not have to impose this interpretation on the coefficient $\tilde{\phi}$ in this specification and can still use equation 10 as a “first-stage” for the wage. Under our assumptions, and contra AKM, we do not necessarily impose homogeneity of firm effects, so $\tilde{\phi}$ can still be the LATE of firm average wage even if different workers get different raises when they switch to the same firm. Put differently, we do not need to impose that firms have the same effect on wages for all workers in order to use the change in firm average wage as an instrument for own wage changes. We regress the separation rate at time $t + k$ on the wage change at time t associated with the move, while controlling for the pre-move history:

$$s_{it+k} = \eta \Delta w_{ijt} + L(\text{history}_{i,t}) + \epsilon_{ijt+k} \quad (11)$$

with the first-stage given by equation 10. Note here that the separation rate s_{it+k} is defined for workers who are still employed at the firm at time s_{it+k-1} . This approach thus instruments the wage change of a mover, Δw_{ijt} , with the change in the mean wage of the firm, $\Delta \bar{w}_j$. The experiment captured by this specification is that we compare two workers with the same past wage and employment history, both starting at the same “origin” firm j' and look at the wage change each worker receives from transitioning to a high-mean-wage versus a low-mean-wage “intermediate” firm j ; we also look at how long they stay at this intermediate firm before separating again to a final firm or to non-employment.

The advantage of this approach over the AKM-based approach in the previous section

is that the controls $L(history_i)$ effectively remove the bias due to worker-specific separation propensities correlated with firm wages that are not due to the elasticity of labor supply facing the firm. These histories are, we would argue, much richer controls than simply the worker wage effect α_i , and we test this below. Further, these employment histories may eliminate much of the influence of match effects on the direction of matching. Below, we subject this specification to the same tests we did in the “pure” AKM approach.

The approach above does not nest AKM because it excludes worker effects α_i . However, a sufficiently rich set of both of lagged wages and past employment history should control for much of the heterogeneity in wages captured by α_i . In addition, we could in principle estimate a specification that is identified under strictly stronger assumptions than AKM, where assignment is given by $f_{jt}^i = G_{jt}(\{\phi_k\}, \alpha_i, \{w_{is}, f_{is}\}_{s<t})$ and wages are given by

$$w_{ijt} = \sum_j \phi_j f_{jt}^i + \alpha_i + L(history_i) + \epsilon_{ijt}$$

Unfortunately, as is well known, a specification with cross-sectional fixed effects and lagged dependent variables will induce Nickell bias in finite histories, and this could bias our IV estimates. In principle a variety of GMM approaches could be used, but we do not pursue them here. We do examine robustness of our estimates to controlling for estimates $\hat{\alpha}_i$ from a previous period (Chen, Chernozhukov, and Fernández-Val, 2019).

5.1 Estimation

We implement this approach using a stacked event study design. We stack all observations by the date of initial transition (t) when a worker i transitions from an initial firm, called Origin $O(i)$ to another firm, called Intermediate $I(i)$. We then estimate the worker’s subsequent probability of “re-separating” from $I(i)$ to another firm $F(i)$ (or to non-employment) over the next k quarters (we take $k = 16$ to allow for a sufficiently long post-transition period). We take the transitioning worker’s history (fully saturated interactions of indicator for the

Origin firm, octiles of initial wages at $O(i)$ firm, octiles of $O(i)$ firm tenure, calendar quarter of transition to $I(i)$ from $O(i)$ denoted as d) fully interacted with with event time, t . (This means we are comparing workers with nearly identical wage and employment trajectories at the same Origin firm and who transitioned to the Intermediate firm on the same date.) Noting that separation $s_{i,t+k}^I$ at date $t+k$ is defined only for workers who had been working at the firm through $t+k-1$, we regress

$$s_{i,t+k}^I = \eta(w_{i,I(i),t} - w_{i,O(i),t-1}) + L(History_{i,t,d}) \times \mathbf{1}_{t+k} + \epsilon_{i,t+k} \quad (12)$$

Note that L contains a fixed effect for $O(i)$, and includes wages at $O(i)$, so all the variation that identifies δ comes from $w_{i,I(i),t}$.¹¹ To isolate the variation in $w_{i,I(i),t}$ that is due to firm wage policies, we use a first stage equation given by

$$w_{i,I(i),t} - w_{i,O(i),t-1} = \phi(\bar{w}_{i,I(i),t} - \bar{w}_{i,O(i),t-1})(f_{I(i),t} - f_{O(i),t-1}) + L(History_{i,t,d}) + \epsilon_{i,t} \quad (13)$$

with a corresponding reduced form given by

$$s_{i,t+k}^I = \delta(\bar{w}_{i,I(i),t} - \bar{w}_{i,O(i),t-1}) + L(History_{i,t,d}) \times \mathbf{1}_{t+k} + \epsilon_{i,t+k} \quad (14)$$

In other words, we regress an indicator for re-separation from $I(i)$ at date $t+k$ (conditional on still working at the firm at date $t+k-1$) on the wage change obtained from transitioning from $O(i)$ to $I(i)$ at date t , instrumented by the difference in coworker wages between $I(i)$ and $O(i)$. This $O-I-Final$ event-study design allows us to construct a clean “pre-treatment” period (i.e., prior to date t) where we match workers based on their past histories, a treatment event (i.e., transitioning to different I firms with different average wages at time t), and a post-treatment period where we can track their re-separation responses to a final firm or

¹¹In our main specification, we only control for the starting wage at $O(i)$ so in principle there is some variation in $w_{i,O(i),t-1}$. However, in a more saturated specification, we additionally control for $w_{i,O(i),t-1}$.

non-employment.

We report the first stage coefficient ϕ and the separation elasticities below, where the separations elasticity is estimated as $\hat{\eta} = \frac{\delta}{\phi \cdot \bar{s}}$.

5.2 Results

In Table 4, we estimate the separations elasticity from our specification using a 16-quarter window following the $O - I$ transition. Column 1 is the specification that corresponds most closely to the Finkelstein, Gentzkow, and Williams (2016) approach (and to the and AKM approach) where we do not additionally control for worker histories. The first stage coefficient of 0.12 is close to the share of wage variance due to variance in firm hourly wage effects we find in Appendix Table C1. The separations elasticity of -0.76 is smaller than what we found in the AKM-based approach (-1.448 in column 5 of Table 1). However, once we control for the identity of the O -firm in column 2, we find a much larger separations elasticity (-2.475). This highlights the likely importance of heterogeneity of workers moving to high- versus low-wage firms; in particular, past firm assignment (i.e, $O(i)$ fixed effect) seems to encode substantial information about exogenous separation rates that vary across firms with high versus low average wages. Our preferred specification in column 4 additionally interacts the $O(i)$ -firm fixed effect with 8 categories of starting wages and tenure at $O(i)$ firm, along with calendar quarter fixed effects; this saturated specification compares workers we started at $O(i)$ firms in the same quarter, at the same wage, and transitioned to a $I(i)$ firm at the same date d , but with potentially different $I(i)$ firm average wage (of their co-workers). This is a rich set of controls, and we find that for this sample, a 10% difference in the $I(i)$ firm average wage leads to a difference in own wage of approximately 1.8%. The separations elasticity from our preferred specification is -2.1; using the 2-times-separations elasticity rule, this suggests a labor supply elasticity of around 4.2. Comparing this estimate to our preferred separations elasticity estimates from the AKM approach above, the estimates from the matched event study are somewhat larger in magnitude (-2.1 vs -1.4) but also more precise (standard error

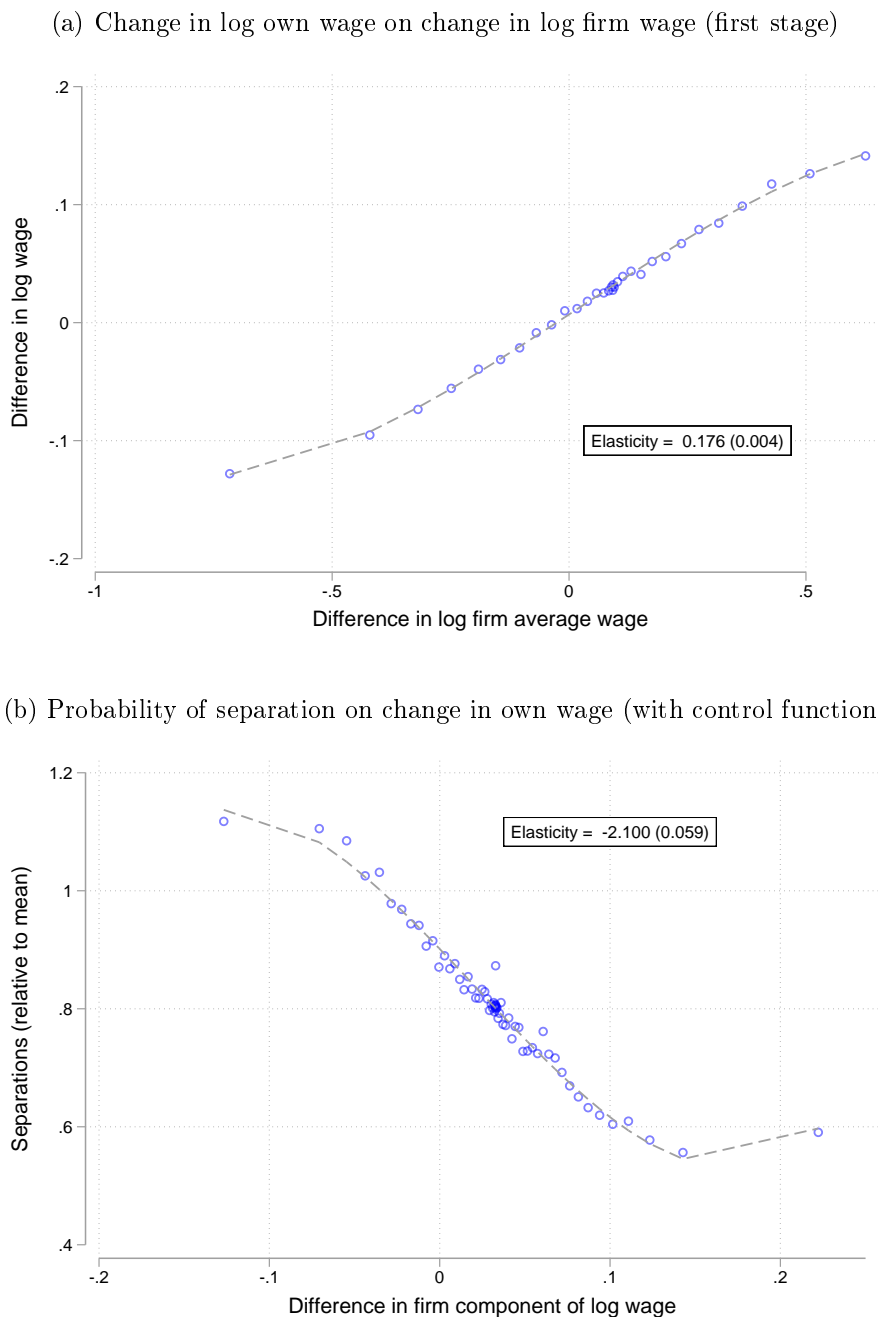
is 0.054 vs 0.095). Figure 3 shows the binned scatterplots of first stage and IV regressions that correspond to column 4 of Table 4, and it is clear there is little need to trim or account for outliers, and the data is much closer to the fitted line and appears close to constant elasticity except in the tails. Appendix figure A7 shows the analogous binscatter but for E-E separations.”¹² Column 5 coarsens these controls to 4 categories of starting wages and tenure at the origin firm; this makes little difference to our estimates.

Column 6 adds the O-I firm-pair fixed effect as a control, and shows that it is the wage difference between two firms, not the specific transition, that drives the reseparation probability. This is a demanding specification that uses changes in firm average wages over time for identification. While the point estimate is smaller in magnitude (-1.293), and the standard errors are much larger (0.513), it’s worth noting that the lower bound of the separations elasticity 95% confidence interval of (-2.3) is similar to the lower bound in our preferred specification in column 4 (-2.2). In Column 8, we fully interact the controls additionally with (1) the ending wage at *O*-firm along with an additional 3 lags in wages (to capture wage dynamics), and find this has little impact on the separations elasticity (-2.085), which suggests our baseline controls are quite successful in finding otherwise similar workers who land at different *I*-firms. Column 7 shows that this is not simply due to sample changes induced by requiring such a rich set of covariates.

We next revisit the specification check we conducted in the previous AKM-based approach in Column 5. We determine whether adding worker fixed wage effects, $\hat{\alpha}_i$ as in Table A1, alters the estimated separations elasticity. Recall that inclusion of the worker wage fixed effects substantially altered the estimate of η , thereby raising concerns about omitted variables in our simple regression of s_{it} on ϕ_j . In column 9, we control for estimates of worker wage effects $\hat{\alpha}_i$ from a pre- t sample, thus eliminating the need to estimate the incidental parameters α_i in the same sample. We find that additionally controlling for the worker’s

¹²As noted above, the AKM-based results suggest the labor supply elasticity estimated from just the separations elasticity is very similar to when it is estimated using E-E separations, E-N separations and E-E recruits. Evidence on the implicit steady state assumption is provided in Appendix Figure A6, which shows that firm separations and firm recruits fall broadly along the 45 degree line.

Figure 3: **Binned scatterplots of separation and firm-component of wages**



Notes: Panel (a) shows the first stage relationship between $\Delta \ln(wage_{i,t+1})$ and $\Delta \ln(\bar{w}_{i,I(i),t})$, where $\Delta \ln(\bar{w}_{i,I(i),t})$ is the change in average firm wage for individual i at E-E separation date $t - 1$ compared to the intermediate firm at date t , and $\Delta \ln(wage_{i,t+1})$ is $\ln(wage_{i,t+1}) - \ln(wage_{i,t-1})$. Panel (b) shows the relationship between separations and $\Delta \ln(wage_{i,t+1})$, instrumenting by $\Delta \ln(\bar{w}_{i,I(i),t})$ using a control function, i.e., controlling for the residuals from a regression of $\Delta \ln(wage_{i,t+1})$ on $\Delta \ln(\bar{w}_{i,I(i),t})$. Separation indicates the probability of separation from the intermediate firm. All specifications include fixed effects $L(History_{i,t,d})$ corresponding to interacted event and calendar time by origin firm by worker tenure at origin firm (8 bins) by initial wage at the origin firm (8 bins), and are clustered at the level of origin firm by time. The sample consists of the first 16 quarters after initial separation from the origin firm. See text for sample construction.

fixed effects (based on data prior to date 0) has very little impact (raising the separations elasticity to -2.163); this stands in sharp contrast to what we found in the AKM-based approach in Table A1 and shows the value of controls for the origin firm and origin firm wages in absorbing the heterogeneity in separations that are correlated with firm wages.

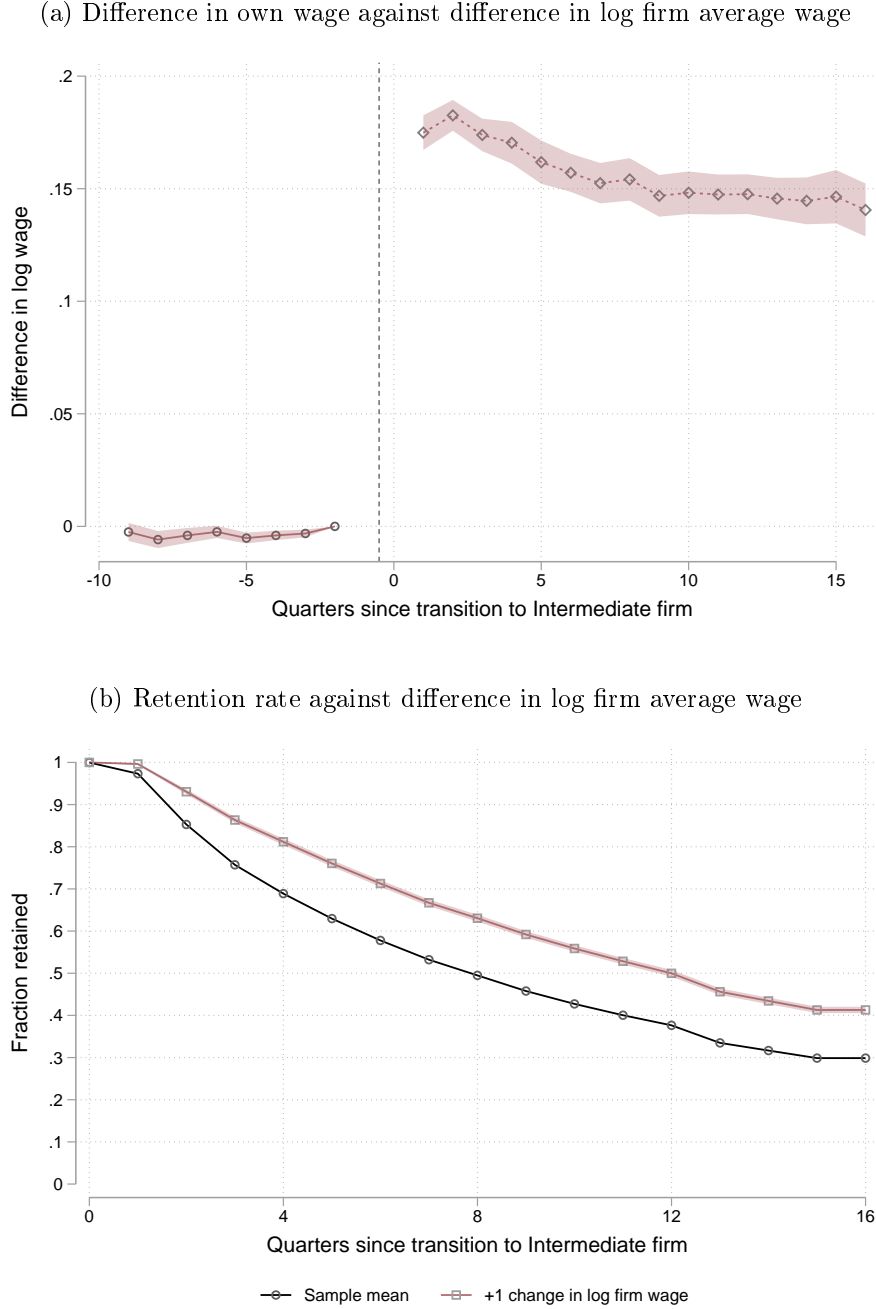
The key findings are shown in Figure 4. In the first panel, we show the “first stage” estimates of the change in wages for workers transitioning from O to I firm. Here we separately regress $w_{i,I(i),t} - w_{i,O(i),t-1}$, the wage changes between event quarter $t - 1$ and event quarters ranging from $t - 9$ to $t + 16$, on $\bar{w}_{iI(i)t} - \bar{w}_{iO(i)t-1}$, the change in the average firm wage between O (date $t - 1$) and I (date t). Here we use the same set of controls as our preferred specification in column 4 of Table 2: fully interacted controls for $O(i)$ firm fixed effect, the starting wages of workers at $O(i)$ in 8 categories, their tenure in 8 categories, and the calendar quarter of transition from $O(i)$ to $I(i)$.

We find that wages of workers going to high- versus low-wage $I(i)$ firms followed parallel trends prior to the $O - I$ transition conditional on controls (recall that in this specification, we controlled for the starting wage at the $O(i)$ firm but not subsequent wages, so there is no mechanical reason for this to be true). At the same time, there is a clear jump in own wages of workers leaving the same $O(i)$ firm after date 0 when they move to a firm with a higher average wage.¹³ The coefficient of 0.18 at date t means that, on average, if a worker moves to an I -firm with 10% higher average wage, the worker’s own wage increases by around 1.8%. Following Finkelstein, Gentzkow, and Williams (2016), we can interpret this to mean that around 18% of the variation in overall wages are due to the firm component, though in our case these are conditional on controls for worker heterogeneity. The gains are persistent, as the first stage coefficient remains around 0.14, even 16 quarters following the $O - I$ transition.

How is separation behavior at the I -firm affected by wages there? Panel B shows this visually using the survival function, i.e., plotting the impact of having a higher firm-average

¹³As explained in the Data Appendix on sample construction, we set wages in the actual quarters of transition (dates -1 and 0) to missing as these hourly wage observations likely contain substantial measurement error associated with partly worked quarters.

Figure 4: **Event study of workers' wages and separation behavior following movement to a higher wage firm**



Notes: Panel (a) plots the first stage regression β coefficients from $\Delta \ln(wage_{i,t+k}) = \beta_k \Delta \ln(\bar{w}_{i,I(i),t}) + L(History_{i,t,d}) \times \mathbf{1}_{t+k} + \nu_{i,t+k}$, separately for each event-time period $k \in [-9, 16]$, where $\Delta \ln(\bar{w}_{i,I(i),t})$ is the change in average firm wage for individual i at E-E separation date $t - 1$ compared to the intermediate firm at date t , and $\Delta \ln(wage_{i,t+k})$ is $\ln(wage_{i,t+k}) - \ln(wage_{i,t-1})$. Panel (b) reports coefficients from the reduced form specification $R_{i,t+k} = \delta_t \Delta \ln(\bar{w}_{i,I(i),t}) + L(History_{i,t,d}) \times \mathbf{1}_{t+k} + \epsilon_{i,t+k}$, where $R_{i,t+k}$ denotes retention at the intermediate firm, separately for each event-time period $k \in [1, 16]$. All specifications include fixed effects $L(History_{i,t,d}) \times \mathbf{1}_{t+k}$ corresponding to interacted event and calendar time by origin firm by worker tenure at origin firm (8 bins) by initial wage at the origin firm (8 bins), and are clustered at the level of origin firm by time. Change in own wage is censored at the 1% tails. See text for sample construction.

wage \bar{w} on k -period retention probability for $k \in \{1, 2, \dots, 16\}$. We plot the average retention probabilities of all workers in the sample in black, and the predicted retention probabilities for workers who are assigned to an $I(i)$ firm with one log point higher firm-average wage (in red). The gap in the retention probability between the red and black lines is thus the causal effect of being assigned to a firm with a log point higher firm-average wage; 4 quarters out, this gap in the separations probability is about -0.1. This gap in probability persists through the 16 quarters following the initial $O - I$ transition. Note that the figure traces out the impact of higher firm wages on the survival function $\bar{R}_{t+k}(\bar{w})$. To relate this to our separation elasticities, note that the latter are based on the the impact of firm wages (\bar{w}) on the hazard of separating at time period k , i.e., $\frac{\partial}{\partial \bar{w}} (\ln(\bar{R}_{t+k}(\bar{w})) - \ln(\bar{R}_{t+k-1}(\bar{w})))$. Pooling the impact on the hazard in periods $k \in \{1, 2, \dots, 16\}$ produces the corresponding (reduced form) separations elasticity.

By focusing on the separations response to the wage change of the compliers, we eliminate the risk of ecological bias in the previous AKM section. This specification recovers the separations elasticity from the change in individual wages driven by the change in firm average wages. Since we are not imposing the AKM separable log additivity, this event study allows for heterogeneity in the wage change experienced by workers, for example match effects. The AKM approach imposed that all workers experience exactly $\phi_j - \phi_{j'}$ log wage change upon transition from j' to j , and then imposed that separations only responded to ϕ_j . Workers who separated for reasons unrelated to wage changes at j (e.g. because of sorting) would still be counted in the estimated separation elasticity. In the event study approach, we are simply using the change in firm wages as an instrument for own wage change, and if there is heterogeneity in the “first-stage” (from e.g. match effects) it just makes our IV estimate a (weighted) LATE applicable only to compliers, but still unbiased.

5.3 Robustness and Heterogeneity

Table 6 probes the robustness of our approach to a variety of other specification choices. Column 1 contains our baseline specification for comparison. Column 3 controls for a measure of firm amenities or attractiveness proposed by Sorkin (2018). Specifically, we construct an amenities value measure using the V^{EE} concept based on the Google Page Rank algorithm. Note V^{EE} is supposed to reflect the overall value of the job to a worker, inclusive of both the wage and amenities components. One measure of the pure amenities component is then the difference between V^{EE} and the AKM firm fixed effect (of the $I(i)$ firm). The inclusion of this amenities measure has a very small impact on the estimated separations elasticity with respect to wage, which changes to -1.992. The separations elasticity with respect to the amenities value is -0.291. As an alternative, in column 2, we instead control for V^{EE} itself. In this case, the separations elasticity with respect to V^{EE} is -0.22 (reported in the table notes); this measures the separations elasticity with respect to the firm amenity value (holding wages constant) and is similar to the estimate in column 3. To obtain the separations elasticity with respect to the firm wage component, we now have to add the coefficient on instrumented own-wage change (-1.961) plus the elasticity with respect to V^{EE} (-0.22), since V^{EE} is supposed to contain the firm wage component as well as amenities value. This implies an amenities-corrected separations elasticity of firm wage of around -2.16, which is virtually identical to our baseline estimate. Overall, we interpret these results to suggest that the separation elasticities with respect to wage gains experienced by movers with otherwise similar histories are not substantially affected by controlling for amenities values as measured by the Sorkin approach.

Our main specification uses changes in mean firm wage as an instrument for wage changes. However, there are other ways of categorizing firm quality, such as the approach taken in Bonhomme, Lamadon, and Manresa (2019), who cluster firms based on their empirical earnings distribution. Following Bonhomme, Lamadon, and Manresa (2019), in Column 4 we replace the instrument from the change in mean firm wages to 10 clusters of the $I(i)$

firm wage distribution (again, conditional on $O(i)$ firm fixed effects). Firms are partitioned to these 10 clusters based on the proportion of workers in each ventile of the hourly wage distribution using k-means clustering. Use of the 10 clusters as instruments—instead of the firm average wage—does little to change the separations elasticity, which in this case falls slightly to -2.027.

Column 4 reports the OLS estimate of separations elasticity with respect to the change in individual wage at date t , without instrumenting with the change in firm wages. Despite having all of the same controls as Column 1, the implied separations elasticity of -0.272 is around one eighth of the magnitude of the IV estimate, and is generally much closer to the findings in the “standard approach” presented in Manning (2003) and the other papers mentioned in the introduction. This highlights the importance of instrumenting the wage with the firm average wage to estimate the degree of monopsony power, even with controls, the standard approach results in residual supply elasticities that are much too small to be credible.

Finally, column 5 reproduces the main specification using quarterly earnings rather than hourly wages. Similar to the AKM-based estimates, the quarterly-earnings-based estimates are substantially attenuated, with a separations elasticity of -1.536; this, again, highlights the importance of adjusting for hours.

Table 7 presents the heterogeneity in the separation elasticities. Using the 1-digit NAICS super-sectors, we exclude agriculture, as well as mining, utilities and construction because these industries have far fewer employees (less than half the number employed in next smallest industry). Panel A suggests that the implied labor supply elasticities (again, using the 2-times-separations-elasticity rule) are larger in manufacturing and especially in the high-wage business, financial and professional services at 4.6 and 7.8, respectively. In contrast, they are small in low-wage sectors of art, accommodation and food services (which includes restaurants) and wholesale, trade and transport (which includes retail) at 2.4 and 2.8, respectively. This sectoral variation in the labor supply elasticity is much larger than the findings

using the traditional approach Webber (2015). It is also worth noting that one may have assumed that low-wage sector like restaurants and retail would be more competitive, especially given the frequency of job changes in those sectors. However, our evidence suggests the opposite: the labor supply facing low-wage, high-turnover sectors appears to be much less elastic than that facing high wage sectors. This pattern has important implications when it comes to considering policies and wage regulations to address labor market monopsony, as discussed in Naidu and Posner (2019).

We also report elasticities separately for the Portland metro area and rest of Oregon (Panel B). These two subsamples differ dramatically in levels of labor market concentration. In metro Portland, the county \times industry \times quarter average employment (payroll) Hirschman-Herfindahl-Index (HHI) is 0.027 (0.053), while the average outside of the Portland metro area the HHI is higher at 0.088 (0.115), confirming that concentration is higher in rural labor markets. We do find some evidence that the implied labor supply elasticities are 15% larger in Portland (-4.6) than outside (-4.0), which is consistent with concentration playing some role in determining labor market power. However, under the Cournot-based interpretation of employment HHI, where the residual labor supply elasticity is the aggregate labor supply elasticity divided by HHI, the residual labor supply elasticity would be expected to be around 330% larger in Portland (using employment HHI), and for plausible aggregate labor supply elasticities the residual labor supply elasticities in the non-Portland sample would be much smaller than the ones we find. Overall, these findings suggest that concentration plays at most a modest role in the overall explanation behind labor market power.

Moreover, there are many differences between metro Portland and rural Oregon other than concentration, including sectoral composition, worker type, mobility costs and labor market tightness. For this reason, we investigate heterogeneity by labor market concentration directly in Panels C and D, where we compute county \times industry \times year HHI by for both employment and payroll. We investigate heterogeneity by cutoffs consistent with high

concentration in the literature, looking at HHIs less than 500, between 500 and 1000 (2000 for wage bill), and greater than 1000 (2000). For comparison, the Horizontal Merger Guidelines consider markets with concentration greater than 1500 to be moderately concentrated and those greater than 2500 to be very concentrated. Arnold (2019), for example, finds effects of mergers at only the highest ventile of his (flows-based) concentration measure, which is greater than 2100. Most of our movers are in low-concentration labor markets but still face a considerable degree of monopsony power, often more than those in more concentrated markets. For example, our implied labor supply elasticity in the 1000+ HHI category is around 4.4, while the elasticity is around 4.0 in the below 500 HHI category.

In traditional Cournot models, the effect of concentration on wages is mediated by the elasticity of labor supply facing the firm. Our results suggest approaching the interpretation of recent studies with some caution (including Azar, Marinescu, and Steinbaum (2017), Rinz et al. (2018), Arnold (2019) and Prager and Schmitt (2019), which show negative effects of employment concentration on wages through the lens of the Cournot model). First, even low concentration areas may have substantial monopsony power, with policy implications as in Naidu and Posner (2019). In addition, the concentration may be picking up other differences between labor markets. Finally, the Cournot model of monopsony may not accurately describe the wage-setting process. Jarosch, Nimczik, and Sorkin (2019), and Schubert, Stansbury, and Taska (2020) both present bargaining-based models in which the effect of concentration on wages is via lowered outside options rather than just the supply elasticity. If wages are set by Nash bargaining in some firms and monopsonistic wage posting in others, as in Flinn and Mullins (2019), then interpreting the effect of concentration solely through its effects on the residual supply elasticity may miss the effect concentration has via lowering outside options in bargaining.

In addition, we find the the labor supply elasticity is procyclical (Panel E). From 2007 to 2010, the period spanning the Great Recession, the implied firm-level labor supply elasticity was around 4.1, while in the prior and subsequent expansionary periods it ranged between

4.7 and 5. The procyclicality of the labor supply elasticity is consistent with Webber (2018), even though the magnitudes in our findings are larger than his.

Importantly, we find that the labor supply elasticities are substantially larger for higher wage workers than for lower wage workers (Panel F). In particular, we divide our sample into quartiles of worker wages at *Origin* firms, and assess the heterogeneity of the separation response to the *Intermediate* firm wage by the wage levels they were earning at *Origin*. In other words, we are comparing how separations at *I* respond to wages at *I* for two workers who were earning identical wages at *O*; but now estimating this separately when the two workers' *O*-wage fell at the bottom of the overall wage distribution versus higher in the distribution. We find a monotonic increase in the magnitudes of the separation (and hence labor supply) elasticities across wage quartiles. The labor supply elasticity for the bottom quartile is 2.9, while for the top quartile, it is much larger at 4.6. Generally, higher wage workers seem to be in more competitive labor markets, which is consistent with our industry-level findings above.

Finally, in we restrict the regression sample to different post-period lengths (Panel G). While our preferred estimate uses a post transition length of 16 quarters, the separations elasticities are quite stable across samples using 4, 8 or 12 quarters. The E-E separations elasticity is increasing in post period length, but remains in a relatively narrow band (-3 at minimum compared to -4 for 16 quarters).

One caveat to our results is that by restricting attention to firm wage policy variation, we necessarily have to focus on “movers”: workers who switch firms. These workers may have in general higher separations elasticities than those who stay at one firm throughout our sample period. As a consequence, our estimated labor supply elasticity (a weighted LATE among movers) may be an upper bound on the degree of dynamic monopsony in the labor market. While omitted from Table 7 for space reasons, we find only moderate heterogeneity by pre-*Origin* number of moves, where the separation elasticity is very similar (-2.09 vs -2.08) and the E-E separation elasticity is somewhat higher (-4.5 vs -3.8) for workers with

one or more moves before their switch from *Origin* to *Intermediate* compared to workers with those with none.

6 Discussion and Conclusion

The individual separations elasticity with respect to own wage has been taken as evidence for dynamic monopsony power. However, the literature estimating separations elasticities has rarely successfully distinguished between the wage variation due to worker heterogeneity and that due to firm wage-setting although the theory points towards firm wage-setting as the relevant component of the wage. We isolate firm wage policies using two different approaches, one that follows Abowd, Kramarz, and Margolis (1999), where wages are additively separable into a fixed worker component and a firm fixed effect, and a second approach that estimates the elasticity of separations with respect to the firm component of wages using a matched-worker event-study approach. 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 tendency of workers to move between two firms depends on differences in firm effects on wages.

Our second approach relies much less on the specific wage decomposition of AKM and instead instruments individual wage changes of movers through the change in log average wage between the origin firm and the new firm, controlling for a rich set of worker history variables including fixed effects for previous firm identity, past wage dynamics and prior tenure. We then examine the “re-separation” probability of the moving worker as a function of their instrumented wage change.

Both approaches lead to broadly similar results; the advantage of the event-study approach is not having to impose the AKM decomposition on wages. Relative to estimates obtained from our procedure, existing elasticities from individual level separations regressions appear to be substantially downwardly biased in magnitude, consistent with attenua-

tion stemming from use of wage variation unrelated to firm choices. Our estimates suggest a moderate amount of monopsony power in the U.S. labor market, with a labor supply elasticity of around 4. Moreover, this is true even in thick urban labor markets. The degree of monopsony power is greater in the low-wage, high-turnover sectors and for low-wage workers generally.

Examining the response of separations to firm wage effects can also inform interpretation of those effects. One view (e.g., Sorkin 2018; Lamadon, Mogstad, and Setzler 2019), is that a substantial part of firm fixed effects reflect compensating differentials for firm-specific disamenities. Our paper provides some evidence against this view. First, 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 or IRS data used in Sorkin (2018) and Lamadon, Mogstad, and Setzler 2019. 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, and most directly, in our event-study approach, we show that our separations elasticity estimates are little affected by controlling directly for a revealed preference measure of job value. While high job value firms do have lower separation rates, controlling for the job value measure does not substantially alter our estimated separations elasticity.

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

In sum, we document that there is pervasive but moderate monopsony power even in thick labor markets, and especially in the low-wage segments; this monopsony power seems at best weakly related to measures of labor market concentration. However, quantitatively the extent of monopsony power is much smaller than has been suggested using the traditional

approach to measuring dynamic monopsony power using individual wages. Future work could profitably combine the dynamic monopsony framework in this paper with job differentiation and concentration to both unify and disentangle the sources of monopsony power across labor markets.

Table 4: Separations elasticities based on matched event study

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
<i>First stage</i>	0.122 (0.006)	0.148 (0.001)	0.148 (0.003)	0.176 (0.004)	0.173 (0.003)	0.070 (0.009)	0.165 (0.007)	0.171 (0.006)	0.173 (0.006)
<i>IV estimates</i>									
Separations	-0.761 (0.051)	-2.431 (0.033)	-2.475 (0.059)	-2.100 (0.054)	-2.014 (0.040)	-1.293 (0.513)	-2.084 (0.096)	-2.085 (0.096)	-2.163 (0.080)
E-E Separations	-1.352 (0.096)	-4.000 (0.079)	-4.341 (0.144)	-4.031 (0.154)	-3.606 (0.108)	-1.754 (1.549)	-4.326 (0.304)	-4.379 (0.314)	-4.201 (0.234)
E-N Separations	-0.958 (0.079)	-3.519 (0.060)	-3.600 (0.108)	-3.312 (0.115)	-2.987 (0.079)	-1.230 (0.918)	-3.551 (0.218)	-3.620 (0.227)	-3.441 (0.174)
Obs	8.281	7.380	3.078	3.068	4.172	3.068	1.513	1.511	1.868
Movers	852341	805633	347418	346261	474817	346140	160606	160443	194976
Fstat (IV)	282	7053	1844	1397	2847	46	522	582	542
Coarsened controls					Y				
<i>Interacted controls</i>									
Time	Y	Y	Y	Y	Y	Y	Y	Y	Y
× Firm		Y	Y	Y	Y	Y	Y	Y	Y
× $wage_0$ × tenure				Y	Y	Y	Y	Y	Y
× 3 qtr wage lags								Y	
<i>Other controls</i>									
O-I Firm-pair FE						Y			
AKM Worker FE									Y
<i>Sample restricted based on</i>									
Column 4			Y						
Column 8							Y		

Note: See text for sample construction. The full instrumental variables specification is provided in equations (12) and (13) in the main text. The outcomes $s_{i,t+k}^I$ indicate separation, E-E separation and E-N separation such that s is missing for all periods after a single re-separation (and E-N re-separating workers are missing for the E-E separation outcome; similarly for the E-N separation outcome). Each of these regressions includes fixed effects as indicated, where ‘×’ indicates that fixed effects are interacted. $Wage_0$ indicates the wage at hire, and 3 qtr wage lags indicates 3 quarters of pre-separation wages (Origin firm). Fixed effects are divided into 8 equal bins, except where coarsened which indicates that 4 bins are used instead. O-I Firm-pair FE indicate fixed effects for every Origin-Intermediate firm pair. AKM Worker FE indicates a continuous control for the AKM worker fixed effect from the previous time period. The sample is restricted to the post- t period. Where indicated, the sample is additionally restricted for comparability to the estimable sample for the corresponding set of fixed effects. Change in own wage is trimmed at the 1% tails. All regressions are clustered at the level of origin firm by initial separation quarter. Only elasticities are reported by dividing regression coefficients by the average relevant sample re-separation rate.

Table 5: **Alternative specifications for separations elasticities based on matched event study**

	(1)	(2)	(3)	(4)	(5)	(6)
<i>First stage</i>	0.176	0.171	0.177			0.324
	(0.004)	(0.004)	(0.004)			(0.004)
<i>IV estimates</i>						
Separations	-2.100	-1.961	-1.992	-2.027	-0.272	-1.536
	(0.054)	(0.057)	(0.054)	(0.072)	(0.012)	(0.037)
E-E Separations	-4.031	-3.771	-3.803	-3.996	-0.445	-3.083
	(0.154)	(0.161)	(0.153)	(0.210)	(0.026)	(0.115)
E-N Separations	-3.312	-3.131	-3.149	-3.178	-0.385	-2.411
	(0.115)	(0.122)	(0.116)	(0.152)	(0.026)	(0.079)
Obs (millions)	3.068	2.999	2.984	3.069	3.073	3.082
Movers	346261	340000	338562	346714	347193	346684
Fstat (IV)	1397	1279	1345	196		4447
Quarterly Earnings						Y
<i>Instrument</i>						
Firm wage	Y	Y	Y			Y
BLM firm cluster				Y		
OLS					Y	
<i>Controls</i>						
Firm value		Y				
Firm amenities value			Y			

Note: Main spec. FE correspond to table 4 column 4 and are firm by event and calendar time by tenure bin by initial wage at hire, all for the origin firm, and where tenure and hire wage are divided into 8 bins. Firm value, V^{EE} is estimated based on the procedure described in Sorkin (2018) over the full sample of observations in the worker-quarter panel, and for the separations regression in column 2 above has elasticity -0.222 (SE=0.033). The firm amenities value is calculated as the difference between the AKM firm effect and firm value, V^{EE} , and the separations elasticity with respect to the amenities value in column 3 is -0.291 (SE=0.038). BLM firm decile is estimated based on the procedure described in Bonhomme, Lamadon and Manresa (2019), and is used as an alternative instrument in place of the firm wage. OLS indicates that the firm wage instrument is not used, i.e., separations are regressed directly on the change in log own wage at initial transition. Quarterly earnings indicates the main specification with quarterly earnings instead of hourly wage, for both the firm and own wage changes. Standard errors shown in parentheses are clustered at the level of Origin firm by initial separation quarter.

Table 6: **Heterogeneity in separation elasticities based on matched event study**

	First stage		Separations		E-E separations		Movers
<i>Panel A: Industry of destination firm</i>							
Manufacturing	0.178	(0.01)	-2.287	(0.298)	-4.136	(0.804)	36919
Wholesale, trade & transport	0.188	(0.008)	-1.394	(0.159)	-3.391	(0.487)	63158
Prof., business & financial services	0.117	(0.01)	-3.91	(0.267)	-7.974	(0.856)	71620
Education and Health	0.154	(0.006)	-2.148	(0.158)	-3.777	(0.503)	58072
Art, Accommodation & Food	0.238	(0.021)	-1.201	(0.255)	-2.301	(0.786)	22999
<i>Panel B: Geographic zone of destination firm</i>							
Portland metro	0.161	(0.005)	-2.288	(0.14)	-4.74	(0.435)	88686
Non-Portland metro	0.175	(0.006)	-2.021	(0.136)	-3.824	(0.453)	55046
<i>Panel C: HHI (employment)</i>							
0-500	0.173	(0.004)	-1.986	(0.106)	-4.027	(0.331)	109520
500-1000	0.158	(0.012)	-1.874	(0.277)	-4.477	(1.229)	21714
1000+	0.222	(0.021)	-2.157	(0.449)	-3.144	(1.427)	7600
<i>Panel D: HHI (payroll)</i>							
0-500	0.178	(0.005)	-1.823	(0.109)	-3.747	(0.327)	100753
500-2000	0.16	(0.01)	-2.049	(0.221)	-4.014	(0.815)	29991
2000+	0.158	(0.024)	-2.835	(0.863)	-3.105	(2.162)	6002
<i>Panel E: Period of initial separation</i>							
2003-2006	0.17	(0.004)	-2.353	(0.108)	-4.489	(0.277)	91712
2007-2009	0.171	(0.013)	-2.044	(0.154)	-4.194	(0.406)	69886
2010-2012	0.178	(0.01)	-2.481	(0.127)	-4.687	(0.306)	79758
<i>Panel F: Quartile of pre-separation wage</i>							
Quartile 1	0.194	(0.004)	-1.46	(0.054)	-2.337	(0.133)	86475
Quartile 2	0.198	(0.009)	-1.979	(0.1)	-4.088	(0.294)	68597
Quartile 3	0.168	(0.013)	-2.451	(0.176)	-5.438	(0.571)	66691
Quartile 4	0.127	(0.006)	-2.282	(0.2)	-3.966	(0.502)	81470
<i>Panel G: Time horizon</i>							
4-quarter out	0.176	(0.004)	-2.01	(0.051)	-3.082	(0.116)	346261
8-quarter out	0.176	(0.004)	-2.262	(0.057)	-3.547	(0.132)	346261
12-quarter out	0.176	(0.004)	-2.149	(0.054)	-3.746	(0.141)	346261

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 movers. Professional, business and financial services includes the Information industry. Period of separation indicates the year of initial separation: the worker is tracked over the following 4 years. Geographic zones are based on the Portland Urban Growth Boundary where non-Portland metro includes all zones other than Portland metro. Time horizon censors the sample at different maximum quarters, and presents the average elasticity over that period. HHI indicates the annual county by industry (8 categories) Herfindahl-Hirschman Index using employment and payroll respectively. Standard errors shown in parentheses are clustered at the level of Origin firm by initial separation quarter.

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.
- Andrews, Martyn et al. (2008). “High wage workers and low wage firms: negative assortative matching or limited mobility bias?” In: *Journal of the Royal Statistical Society: Series A (Statistics in Society)* 171.3, pp. 673–697.
- Arnold, David (2019). “Mergers and acquisitions, local labor market concentration, and worker outcomes”. In: *Local Labor Market Concentration, and Worker Outcomes (October 27, 2019)*.
- 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:
- Bonhomme, Stéphane, Thibaut Lamadon, and Elena Manresa (2019). “A distributional framework for matched employer employee data”. In: *Econometrica* 87.3, pp. 699–739.
- Bonhomme, Stéphane et al. (2020). *How Much Should we Trust Estimates of Firm Effects and Worker Sorting?* Tech. rep. National Bureau of Economic Research.
- Caldwell, Sydnee and Emily Oehlsen (2018). “Monopsony And the Gender Wage Gap:Experimental Evidence from the Gig Economy”. In:
- Card, David, Ana Rute Cardoso, and Patrick Kline (2016). “Bargaining, sorting, and the gender wage gap: Quantifying the impact of firms on the relative pay of women”. In: *The Quarterly Journal of Economics* 131.2, pp. 633–686.
- 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.

- Chen, Shuowen, Victor Chernozhukov, and Iván Fernández-Val (2019). “Mastering panel metrics: causal impact of democracy on growth”. In: *AEA Papers and Proceedings*. Vol. 109, pp. 77–82.
- Chetty, Raj and Nathaniel Hendren (2018). “The impacts of neighborhoods on intergenerational mobility I: Childhood exposure effects”. In: *The Quarterly Journal of Economics* 133.3, pp. 1107–1162.
- Cho, David (2018). *The Labor Market Effects of Demand Shocks: Firm-Level Evidence from the Recovery Act*.
- Dube, Arindrajit, Laura Giuliano, and Jonathan Leonard (2019). “Fairness and frictions: The impact of unequal raises on quit behavior”. In: *American Economic Review* 109.2, pp. 620–63.
- Dube, Arindrajit, Alan Manning, and Suresh Naidu (2019). “Monopsony and employer mis-optimization account for round number bunching in the wage distribution”. In: *Unpublished manuscript*.
- Finkelstein, Amy, Matthew Gentzkow, and Heidi Williams (2016). “Sources of geographic variation in health care: Evidence from patient migration”. In: *The quarterly journal of economics* 131.4, pp. 1681–1726.
- Flinn, Christopher and Joseph Mullins (2019). “Firms Choices of Wage-Setting Protocols in the Presence of Minimum Wages”. In:
- Goldschmidt, Deborah and Johannes F Schmieder (2017). “The rise of domestic outsourcing and the evolution of the German wage structure”. In: *The Quarterly Journal of Economics* 132.3, pp. 1165–1217.
- Jarosch, Gregor, Jan Sebastian Nimczik, and Isaac Sorkin (2019). *Granular search, market structure, and wages*. Tech. rep. National Bureau of Economic Research.
- Kroft, Kory et al. (2020). *Imperfect Competition and Rents in Labor and Product Markets: The Case of the Construction Industry*. Tech. rep. National Bureau of Economic Research.

- Lachowska, Marta et al. (2020). *Do firm effects drift? Evidence from Washington administrative data*. Tech. rep. National Bureau of Economic Research.
- Lamadon, Thibaut, Magne Mogstad, and Bradley Setzler (2019). *Imperfect competition, compensating differentials and rent sharing in the US labor market*. Tech. rep. National Bureau of Economic Research.
- Manning, Alan (2003). *Monopsony in motion: Imperfect competition in labor markets*. Princeton University Press.
- Naidu, Suresh and Eric A Posner (2019). “Labor Monopsony and the Limits of the Law”. In: *Available at SSRN 3365374*.
- Prager, Elena and Matthew Schmitt (2019). “Employer consolidation and wages: Evidence from hospitals”. In: *Washington Center for Equitable Growth Working Paper*.
- Rinz, Kevin et al. (2018). *Labor market concentration, earnings inequality, and earnings mobility*. Tech. rep. Center for Economic Studies, US Census Bureau.
- Schubert, Gregor, Anna Stansbury, and Bledi Taska (2020). “Monopsony and Outside Options”. In: *Available at SSRN*.
- Shimer, Robert and Lones Smith (2001). “Matching, search, and heterogeneity”. In: *The BE Journal of Macroeconomics* 1.1.
- 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 Additional Tables and Figures

Table A1: Relationship between AKM Wage components and separations

	(1)	(2)		(3)		
	Firm	Firm	Worker	Firm	Worker	Match
All separations	-1.342 (0.085)	-0.739 (0.078)	-0.641 (0.016)	-0.746 (0.078)	-0.636 (0.016)	-3.279 (0.04)
E-E separations	-1.677 (0.127)	-1.005 (0.118)	-0.762 (0.023)	-1.019 (0.117)	-0.753 (0.023)	-4.867 (0.069)
E-N separations	-1.209 (0.075)	-0.605 (0.07)	-0.595 (0.014)	-0.613 (0.07)	-0.59 (0.014)	-3.103 (0.053)
E-E recruits	0.413 (0.059)	0.423 (0.05)	-0.058 (0.013)	0.421 (0.05)	-0.056 (0.013)	0.238 (0.009)
Pct. EE-recruits	0.464	0.482	0.482	0.482	0.482	0.482
Labor Supply Elasticity	2.69 (0.199)	1.38 (0.185)	1.496 (0.038)	1.407 (0.184)	1.479 (0.038)	8.582 (0.106)
Obs (millions)	69.072	68.598		68.598		
<i>Regressors</i>						
Firm FE	Y	Y	Y	Y	Y	Y
Worker FE		Y	Y	Y	Y	Y
Match FE				Y	Y	Y

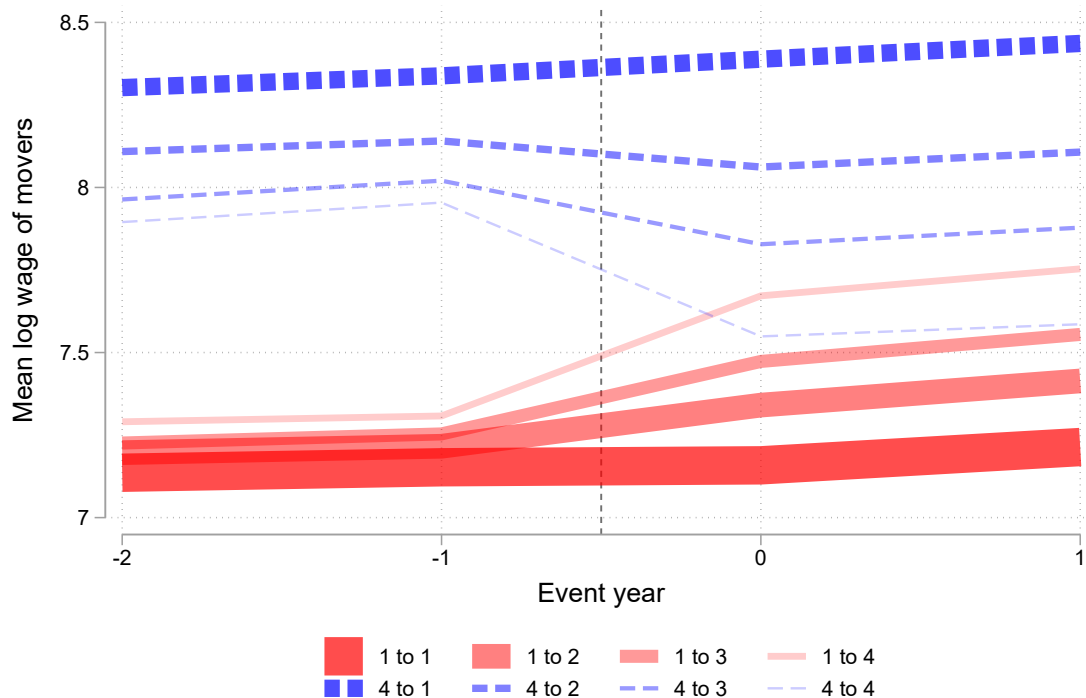
Note: Each supercolumn row indicates a single regression. Specification 1 is reproduced for comparison as the linear specification using the AKM firm fixed effect. Specification 2 adds the AKM worker fixed effect as a regressor. Specification 3 adds the match effect, which is calculated as the average residual per worker-firm match, where the residual is the hourly wage minus the AKM firm and worker fixed effects. Fixed effects are trimmed at their 2.5% tails – see text for sample construction.

Table A2: **Supplementary estimates for AKM firm wage and separations**

	(1)	(2)	(3)
All separations	-1.342 (0.085)	-1.19 (0.076)	-2.059 (0.095)
E-E separations	-1.677 (0.127)	-1.53 (0.116)	-2.565 (0.136)
E-N separations	-1.209 (0.075)	-1.016 (0.064)	-1.843 (0.096)
E-E recruits	0.413 (0.059)	0.353 (0.053)	0.351 (0.064)
Pct. EE-recruits	0.464	0.482	0.464
Labor Supply Elasticity	2.69 (0.199)	2.441 (0.183)	4.392 (0.216)
Obs (millions)	69.072	68.598	69.072
BLM			Y
F-stat			270
<i>Firm FE from</i>			
Main sample	Y		Y
CCK		Y	

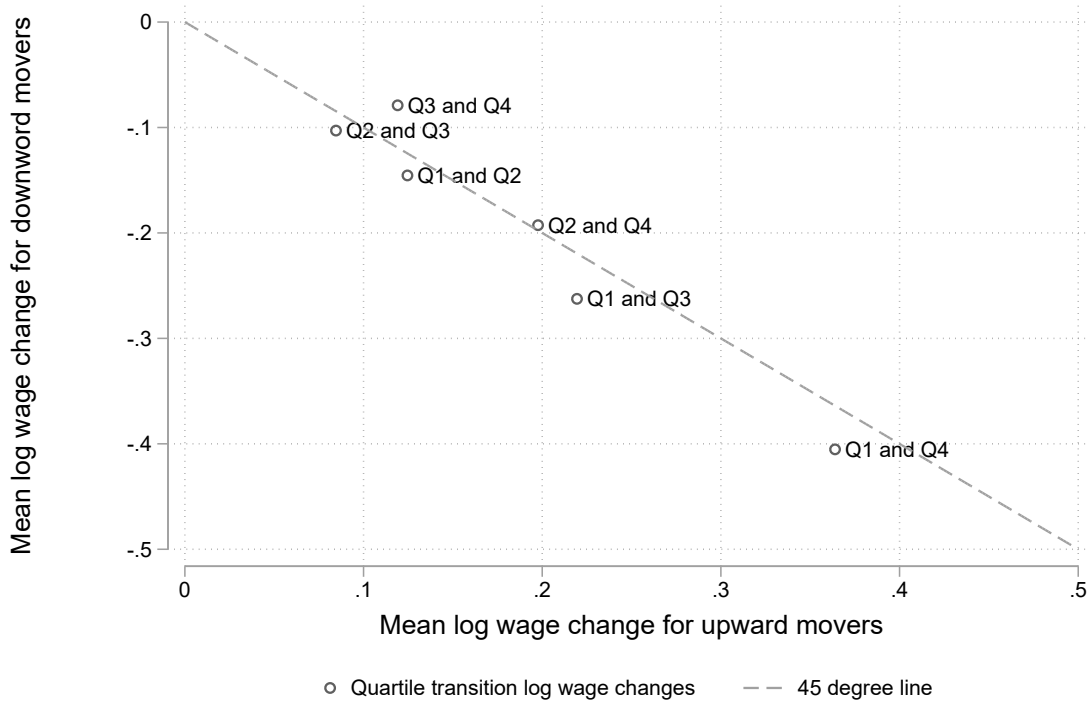
Note: Specification 1 reproduces the AKM linear specification for comparison. Specification 2 uses the firm effects estimated using code from Card, Cardoso and Kline (2016). Specification 3 uses the BLM firm deciles as instruments, based on the procedure described in Bonhomme, Lamadon and Manresa (2019). Fixed effects are trimmed at their 2.5% tails – see text for sample construction.

Figure A1: Changes in hourly wages and incidence of job separations for quartile-to-quartile transitions



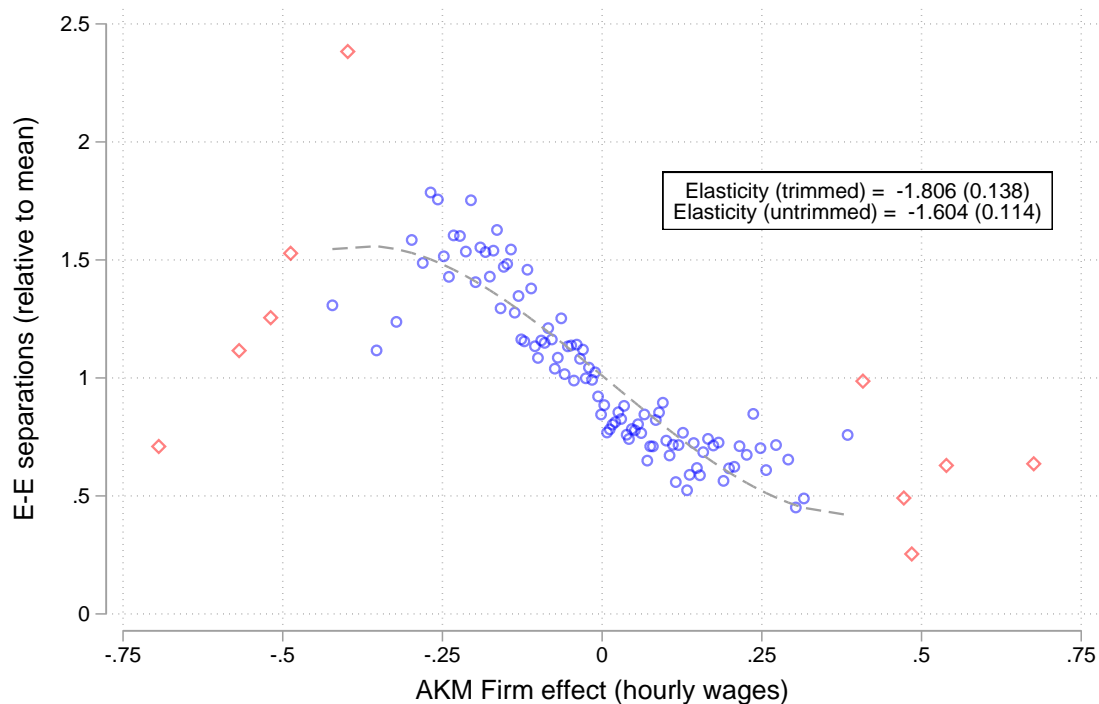
Note: The legend indicates origin quartile to destination quartile, where quartiles are defined along the distribution of the average firm wage, using only workers who stay at the firm over the period. The change in wage is shown for movers, who are defined as workers who make a job-to-job transition at any point over the period and are observed for at least 9 consecutive quarters at the same firm before and after. The quarter of separation and the following quarter are omitted since these represent quarters that were partially worked, and are particularly susceptible to measurement error in wages. This exercise is repeated for each 6-year period (2000-2005, 2006-2011 and 2012-2017), the mover wage profiles are stacked, and the averages of the event quarter by quartile-transition categories are plotted. The thickness of the lines is proportional to the number of job-to-job separations between the relevant quartiles over the full panel 2000-2017 (not restricting by tenure). 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 A2: Symmetry plot of log wage changes for quartile-to-quartile transitions



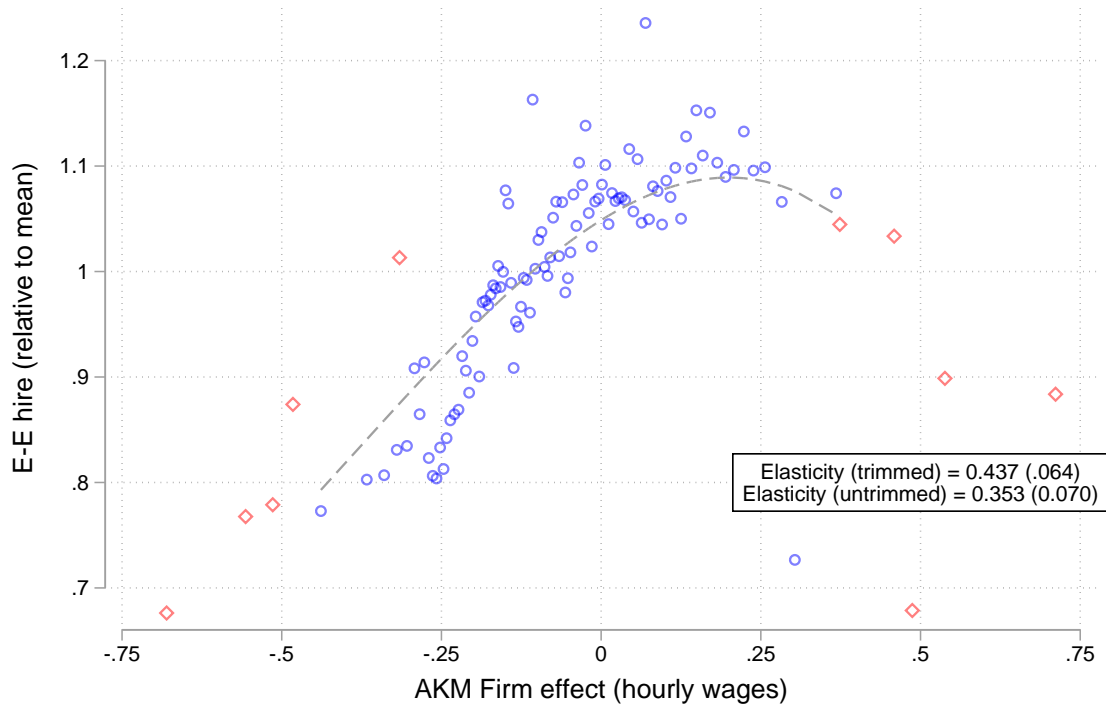
Note: The figure shows the quartile to quartile log wage changes corresponding to the quartile transition event study above. Upward mover indicates that the worker moved from a lower quartile to a higher quartile; downward mover indicates the worker moved to a higher quartile. For example, the point labelled 'Q1 and Q4' shows the average log wage change for movers from quartile 1 to quartile 4 on the horizontal axis, and for movers from quartile 4 to quartile 1 on the vertical axis. The dotted line shows the 45 degree (negative) slope from the origin: symmetric downward and upward log wage changes would lie on this line.

Figure A3: **Job-to-job separations and firm wage effects**



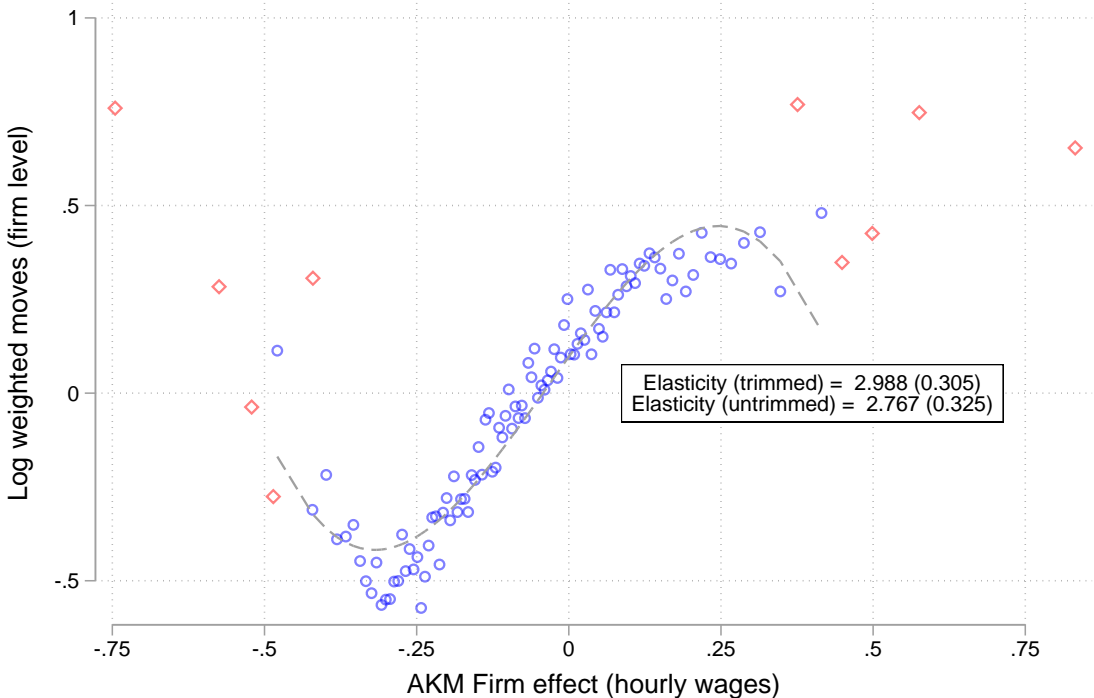
Note: The figure illustrates the split sample approach using a control function. Residuals are calculated from a regression of own-sample firm effects on the complement-sample firm effects, and used as a control in a regression of E-E separations on own-sample firm effects. The plotted points show the binned scatter points of this latter regression (i.e. depicting the partial correlation). The vertical axis is E-E separations divided by mean E-E separations such that the slope of the line represents the elasticity. 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 a cubic polynomial fitted to the trimmed sample.

Figure A4: **Job-to-job hires and firm wage effects**



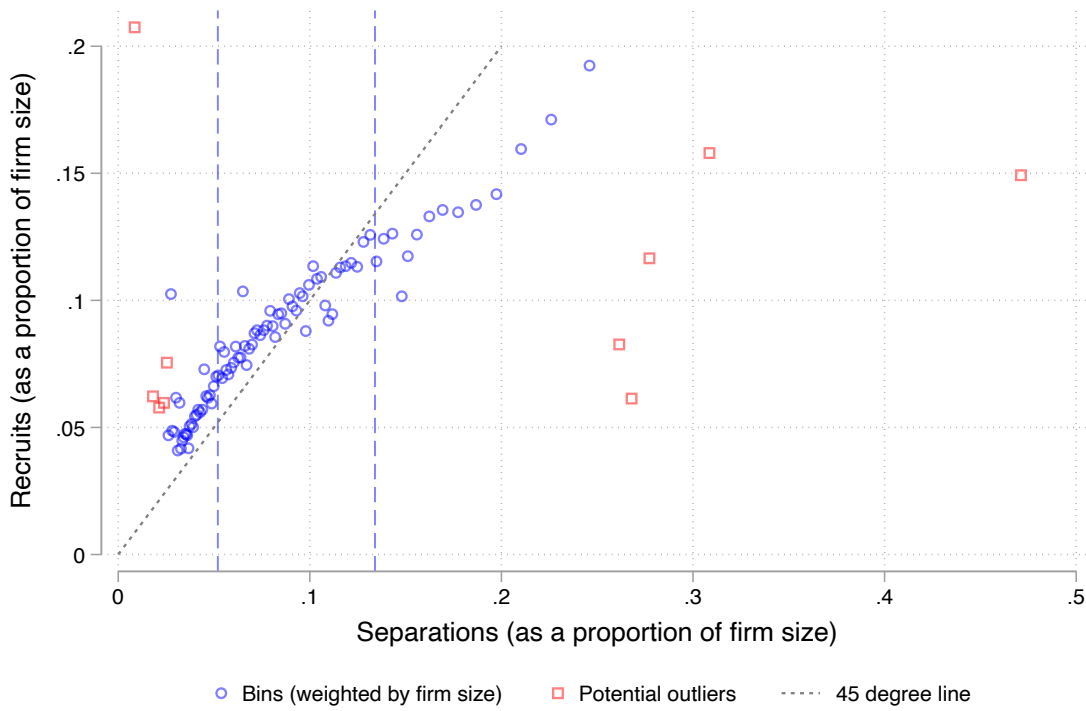
Note: The figure illustrates the split sample approach using a control function. The plotted points show the E-E hires against own-sample AKM firm effects, while controlling for the residuals from a regression of own-sample firm effects on the complement-sample firm effects. The sample is restricted to observations corresponding to hires. The vertical axis is E-E hires divided by mean E-E hires such that the slope of the line represents the elasticity. 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 a cubic polynomial fitted to the trimmed sample.

Figure A5: Labor supply elasticity and firm wage effects



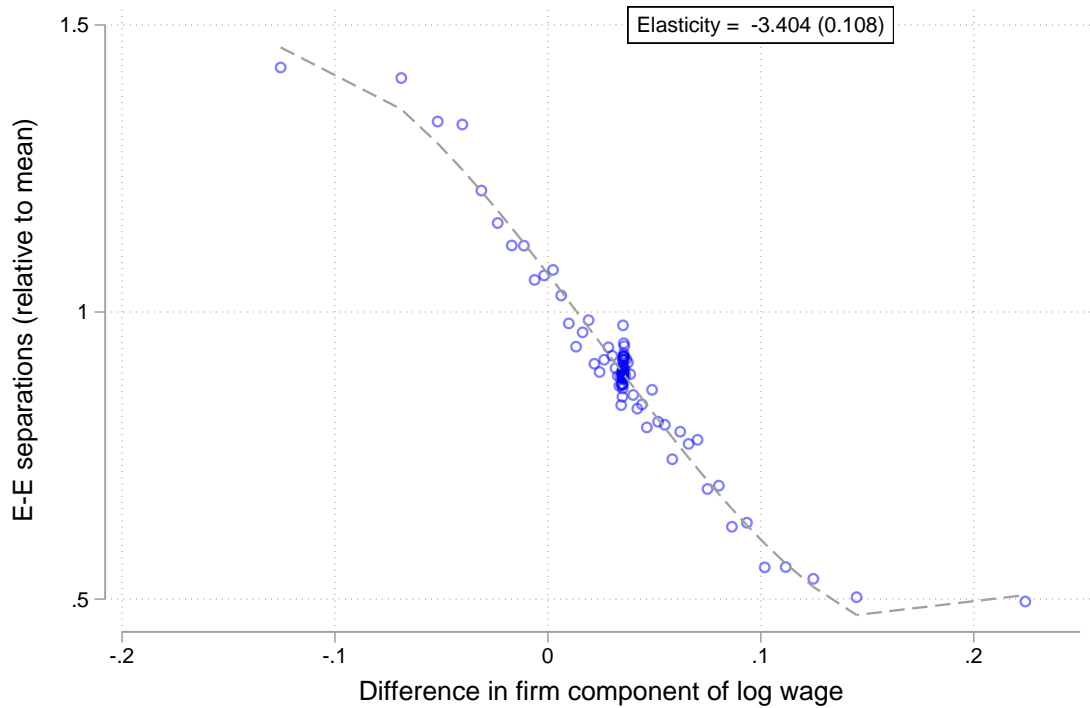
Note: The figure illustrates the split sample approach using a control function, for the labor supply elasticity estimated at the firm (not worker) level. The plotted points show the weighted average of log firm E-E separations, log firm E-N separations and log firm E-E hires against the AKM firm wage effects. The residuals from a regression of own-sample firm effects on the complement-sample firm effects are controlled for. The slope of the line represents the labor supply elasticity, where the reported coefficient corresponds to the fitted bins. The sample is restricted to the trimmed sample, which excludes the top and bottom 2.5 percent of the firm effects distribution. The trendline is a cubic polynomial fitted to the trimmed sample. Points are plotted at the firm level and weighted by firm size.

Figure A6: **Firm separations versus recruits**



Note: The data is plotted at the firm level, with quarterly separations and recruits calculated as a proportion of firm size by firm for each 6 year period. Points are plotted at the firm level and weighted by firm size. Firms are classified as outliers in this figure if they are in the top or bottom 5% tails of the firm separations distribution. The 45 degree line from the origin indicates equal separations and recruits. The dashed vertical lines indicate the interquartile range (p25 and p75 of the separations rate).

Figure A7: Job-to-job re-separations and wages



Note: The plotted points are restricted to the first 16 quarters after initial separation from the origin firm. The vertical axis indicates the probability of E-E separation from the intermediate firm, divided by the average E-E separations. The figure shows the instrumental variables relationship between E-E separations and change in log own wage, using a control function, i.e. controlling for the residuals from a regression of change in log own wage on change in log firm wage. The specification includes fixed effects for interacted calendar time by origin firm by worker tenure at origin firm (8 bins) by initial wage at the origin firm (8 bins), and are clustered at the level of origin firm by calendar time. See text for sample construction.

B Data

This is supplementary material to the data description in the main text. Our data sample covers the period 2000-2017. Oregon experienced recessions in 2001-2002 and 2008-2009 along with the rest of the country: the 2008 recession features prominently with a sharp rise in the unemployment rate and an ensuing decline in the labor force participation rate (see figure B1). We explain in detail the construction of the main sample, present summary statistics, and plot the inequality trends in Oregon using our administrative hourly wage data.

The primary variables in the data by quarterly record are the calendar quarter date, the worker identifier unique to each worker, the firm identifier (where each firm identifier may be associated with multiple establishments within Oregon), number of hours worked in the quarter, the total earnings paid to the worker for the quarter. We also observe the industry of the worker (recorded as a NAICS code), and the location (recorded as the FIPS code)¹⁴, though these are only used for heterogeneity estimates and controls for some robustness checks.

B.1 Sample Construction

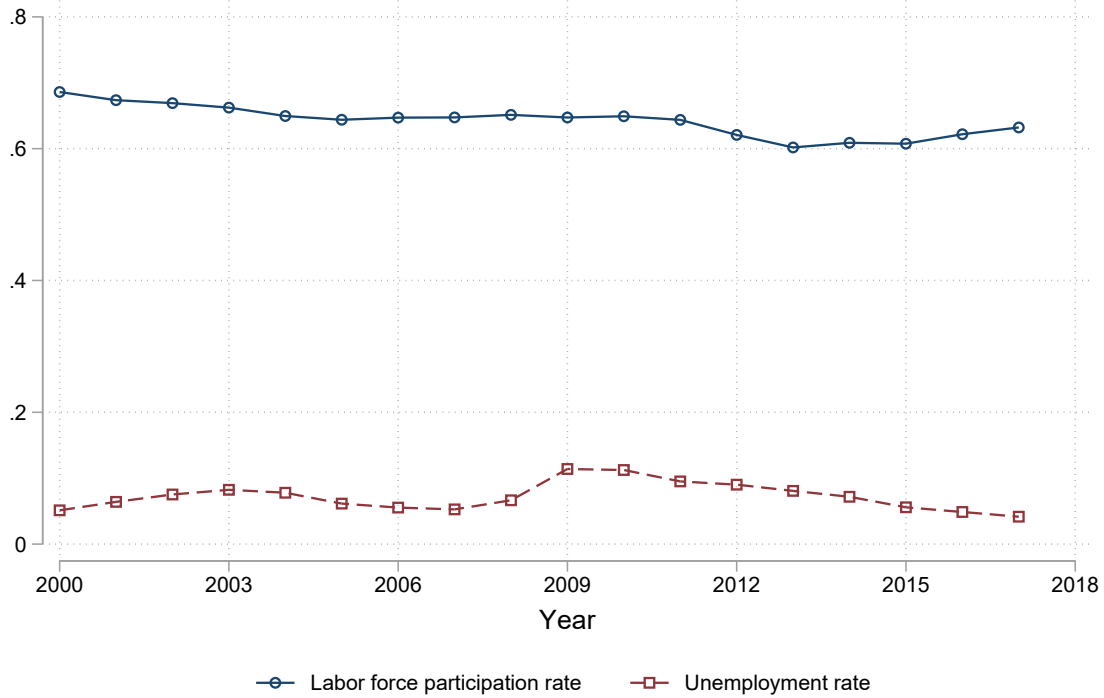
The data were cleaned in the following order, with corresponding summary statistics shown in table 1. We attempt to follow the literature using matched employer-employee data as exemplified by Card, Heining, and Kline (2013), Lachowska et al. (2020), Lamadon, Mogstad, and Setzler (2019), Song et al. (2018), and Sorkin (2018).

1. We begin with records which are uniquely identified by worker-firm-quarter from 2000 quarter 1 to 2017 quarter 4.¹⁵ 136 million such observations exist, corresponding to

¹⁴The county of many workers is missing for a large proportion of the records; additionally due to data limitations restricting the link between specific establishments and workers, the Portland metro zone estimates allocate workers to a zone if at least 90 percent of the employees of their firm are working in a single zone.

¹⁵Although we have access to 1998 and 1999, we discard these years because the wage distributions in these years are implausibly different from the rest of the panel (or corresponding years from other data

Figure B1: Oregon employment, 2000-2017



Note: Data from the monthly CPS for Oregon, for the years 2000-2017 using individual population weights.

317,000 different firms, and 5.3 million workers.

2. We define an employment spell as a group of consecutive quarters for the same worker and firm identifiers.¹⁶ Note that the separations variable, which is important for our main analysis, is defined at this point: separation is equal to one at the end of any employment spell, and Employment to Employment (E-E) separation is equal to 1 if separation is 1 and the worker is employed at another firm in the current or following quarter. Similarly, hire is equal to one at the start of any spell, and E-E hire is equal to 1 if hire is 1 and the worker is employed at another firm in the current or previous quarter. Employment to Non-Employment (N-E) moves are the complement to E-E moves: N-E separations are separations that are not E-E separations, and E-E hires are

sources). This likely reflect problems associated with the first years of data collection.

¹⁶A firm identifier may correspond to several distinct branches within the same firm.

hires that are not E-E hires. We set wages to missing at the beginning and end of any spell, so as to keep comparability of full-quarter wages and avoid severe measurement error in hours due to partial quarters.

3. We drop entire employment spells with

- (a) Less than 100 hours per quarter on average over the employment spell, which is equivalent to less than 8 hours per week. This helps to exclude extremely irregular part time work, and is similar to one of the few other studies that observe hourly wages: Lachowska et al. (2020) drop workers who workers fewer than 400 hours in the year. The number of observations decrease from 136 to 120 million.
- (b) Hourly wage less than \$2 (in 2017 dollars) in any quarter over the employment spell, because it is difficult to imagine a reason this may apply to a regular worker aside from measurement error – this only drops 1 million observations. This restriction is similar to Lachowska et al. (2020) who drop workers with hourly wages below \$2 (2005 dollars). Card, Heining, and Kline (2013), Lachowska et al. (2020), and Sorkin (2018) drop workers with annual earnings below about \$3,000, which for a 40-hour workweek corresponds to \$1.50 per hour (both well below the federal minimum wage). Song et al. (2018) restricts to workers earning the equivalent of minimum wage for 40 hours per week over 13 weeks, and Lamadon, Mogstad, and Setzler (2019) restrict to workers earnings \$15,000 per year.
- (c) Fewer than 3 quarters in length, which drops an additional 9 million observations. This ensures that there is at least one full quarter observation (aside from hiring and separation quarters), giving at least one reliable hourly wage per worker-firm match, which is essential for our analysis. In a similar vein, Sorkin (2018) restricts to at least 2 quarters.

4. We then convert to a worker panel. For any worker-quarter, we keep the observation which belongs to the spell with the highest ave earnings – this corresponds to a dom-

inant employer and keeps spells intact. Note that a separation is still counted if a worker’s spell was cut off. Lamadon, Mogstad, and Setzler (2019), Card, Heining, and Kline (2013), Song et al. (2018), and Sorkin (2018) share this restriction of selecting the highest earning observation for a worker-quarter. We further exclude workers with more than 9 different employers in any year, following Lachowska et al. (2020).

5. By 6-year panel (2000-2005, 2006-2011 and 2012-2017), we drop firms with fewer than 20 workers in any year or firms classified as public administration. Song et al. (2018) restrict to firms with at least 20 employees per year, and Sorkin (2018) chooses a threshold of 15 workers per year. Our large sample restriction is motivated by the estimation of the AKM firm effects, which requires a sufficient number of observations per firm.

Quarterly and hourly wages are each winsorized at the 1st and 99th percentiles to reduce noise from outliers. A limitation shared by most papers with matched employer-employee data is that we cannot distinguish between E-E and E-N moves for workers that move out of state. We also do not observe any non-wage worker characteristics: for example, we do not observe age, so cannot restrict to workers aged 20-60 as in comparable studies (e.g. Card, Heining, and Kline, 2013; Song et al., 2018). We do observe firm industry and location (county level), which we use for heterogeneity in the analysis.

B.2 Summary Statistics of Data

Broadly, our main sample is a quarterly worker-level panel restricted to large private sector firms in Oregon over 2000-2017 (see table B1). In total, we have 87.6 million observations, consisting of 3.4 million workers and 55,000 firms. Compared to the full universe of observations, our main sample has about two-thirds of all workers, and less than one-fifth of the firms (mainly due to the firm size restriction). Average annual worker earnings and weekly hours are substantially higher, again mainly due to the firm size restriction together with the

wage-size correlation. The exclusion of short employment spells decreases the separations rate by about half, as well as the number of firms per worker. In our main sample, the mean separation rate is 8% per quarter, with about half of hires directly from other firms.¹⁷

The AKM analysis is implemented on the connected set of firms, which for this quarterly panel only exclude a few thousand observations. The full panel is divided into 6-year periods, with an AKM regression run on each 6 year panel and its constituent split samples. We observe more than one firm for 40% of worker within each 6-year panel, which facilitates the AKM estimation off movers in the sample. The sample statistics are broadly similar across the panels, with a slight increase in real earnings over time. Employment-Employment hires are lowest in the middle panel, which includes the 2008 recession.

As explained in the main text, the main worker-quarter panel is used to extract a matched event-study panel. All Employment-Employment separations in the main worker-quarter panel are identified, an event-window around each E-E separation is isolated (9 pre-separation and 17 post-separation), and all such event-windows are stacked. The firm before the E-E separation is the Origin firm, the firm after the E-E separation is the Intermediate firm, and the firm after that (to which the worker ‘re-separates’) is the Final firm.

We additionally restrict to workers who were at the Origin firm for at least 4 quarters (whereas in the main worker-quarter panel, spells of 3 quarters are admitted), such that there are at least 2 full quarters of wage observations. This facilitates the main specification which conditions on the initial and end wages at Origin (end wage enters through the transition wage difference with the Intermediate firm). To reduce the impact of outliers, we winsorize the 1% top and bottom tails of the change in own log wage at transition between Origin and Intermediate firms. While the main worker-quarter panel is from 2000 to 2017, note that the 8-quarter pre-transition and 16-quarter post-transition windows imply that the period of admissible transitions between Origin and Intermediate is actually from 2002 to 2013.

Sample statistics for this matched event-study panel are presented in table B2. The full

¹⁷The quarterly separation rate is 17% before sample restrictions, which is similar to the separation rate of 0.15 reported by Webber (2015) using the LEHD.

Table B1: Sample statistics for Oregon 2000-2017

	Obs (total, millions)	Workers (total, millions)	Firms (total)	Earnings (mean, annual)	Hours (mean, weekly)	No. firms per worker (mean)	Separations (mean, quarterly)	E-E hire (mean, quarterly)
<i>Period: 2000-2017</i>								
All	136	5.3	316,910	27,169	27.49	5.71	16.6%	31.4%
Hours<100	120	4.7	302,541	29,636	30.54	4.13	12.1%	33.1%
wage>2	119	4.7	301,997	29,719	30.55	4.13	12.1%	33.1%
Spell>2	110	3.7	249,034	32,057	31.53	2.95	7.6%	35.2%
Priv. large	87.6	3.4	54,663	44,103	32.44	2.53	7.7%	46.9%
Connected	87.6	3.4	54,580	44,101	32.44	2.53	7.7%	46.9%
<i>Period: 2000-2005</i>								
All	27.5	2.1	31,429	42,147	32.66	1.60	8.1%	48.5%
Split 1	13.7	1.0	31,410	42,136	32.66	1.60	8.1%	48.5%
Split 2	13.8	1.0	31,407	42,157	32.66	1.60	8.1%	48.5%
<i>Period: 2006-2011</i>								
All	29.1	2.1	31,788	44,975	32.33	1.55	7.5%	45.2%
Split 1	14.5	1.0	31,772	44,968	32.33	1.55	7.5%	45.1%
Split 2	14.6	1.0	31,772	44,982	32.33	1.55	7.5%	45.2%
<i>Period: 2012-2017</i>								
All	30.9	2.2	32,913	45,023	32.35	1.58	7.6%	46.9%
Split 1	15.5	1.1	32,898	44,993	32.35	1.58	7.6%	46.9%
Split 2	15.5	1.1	32,892	45,053	32.35	1.58	7.6%	46.9%

Note: The first three columns indicate totals (observations and workers are in millions) and other columns indicate means. “No. of firms” refers to the average number of firms a worker is at over the full corresponding period (either 6-year panel or full 18 year panel). Separations and E-E hire (proportion of hires from employment) are given in percentage terms. Earnings are in real dollars adjusted to 2017 using the Portland CPI. The top rows show the consecutive exclusion of employment spells based on hours (less than 100 hours per quarter on average), then wage (spell with any quarter less than \$2 wage), then spell length (less than 3 quarters). Priv. large indicates firms with more than 20 workers and not in public administration. All summary statistics for the 6-year panels refer to the corresponding 6-year panel connected set with the full set of sample restrictions.

sample has nearly 900,000 initial E-E separations, each with an associated event-window, corresponding to just under 700,000 workers and 30,000 Origin firms. There are 175,000 unique Origin firm by calendar quarter ‘events’, with an average of 245 workers each. These workers move out to more intermediate firms (about 40,000). Earnings are roughly similar to the main worker-quarter panel, and hours are slightly higher. Although we use a 16 quarter post window, just over a third of the initial E-E separations end up re-separating to a final firm. These workers have lower average earnings. Note that tenure in table B2 is censored 16 quarters post event.

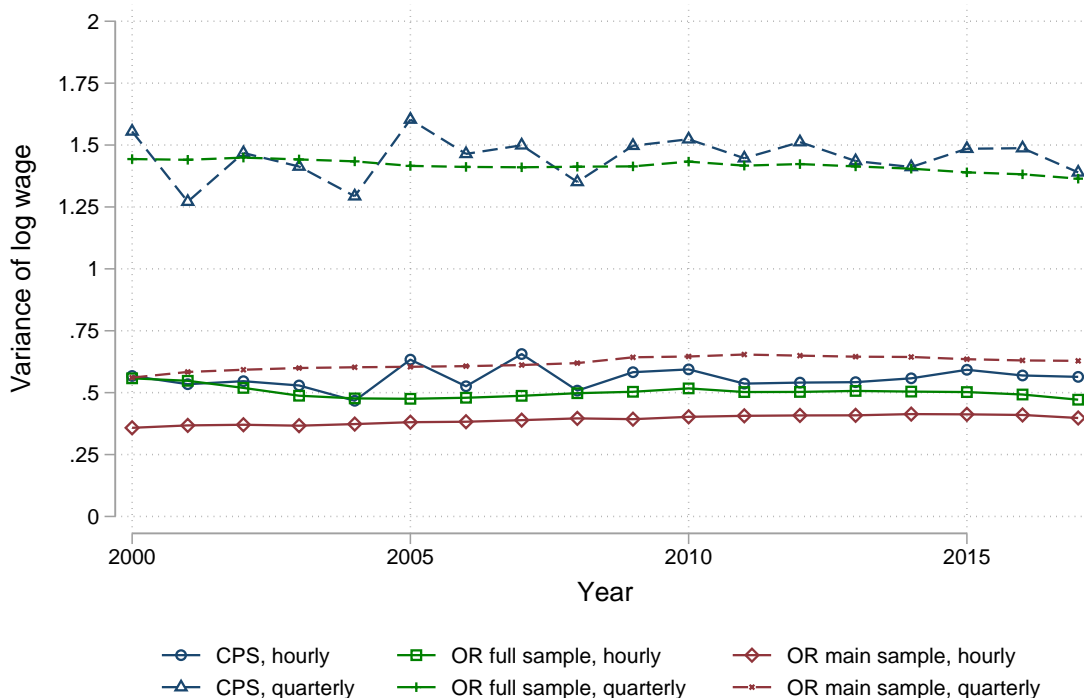
The main estimation specification includes fixed effects for Origin firm by calendar quarter by worker tenure at Origin (8 categories) by wage at hire at Origin (8 categories). The estimable sample is substantially smaller, as it requires sufficient observations in every interacted fixed effects cell (see panel B). About 40% of the initial E-E separations survive, corresponding to 4,000 Origin firms and 21,000 Origin firm quarter events. Over 10,000 Intermediate firms are in this main estimation sample. As for the full sample, about a third of these initial E-E separations end up re-separating to a final firm.

B.3 Inequality Trends

During the 2000-2017 period, the variance in log hourly wages was mostly stable (figure B2). This pattern is similar when we consider hourly or quarterly earnings, and when we consider CPS data or the full universe of workers in our sample. Our main estimation sample (full quarter observations at large firms, as described in the data section) shows a slight increase in log variance. Figure B2 shows that the level of the variance is similar using CPS survey data or the full universe of our records, about 1.5 for log quarterly earnings and 0.5 for log hourly wage. The level of variance for our main sample is much smaller for log quarterly earnings, as expected from the restrictions on part time work (low hours and short spells), and slightly smaller for log hourly wages.

The overall variance of log wages masks considerable heterogeneity in trends by wage

Figure B2: Oregon wage variance, CPS versus UI data



Note: OR indicates our Oregon unemployment insurance data, and CPS indicates CPS-ORG data for Oregon weighted by the population weight that is provided. The CPS and OR full samples include all workers (any firm size), while the OR main sample is used for our main analysis and is described in our data section in text. For CPS, the quarterly wage variable is total income from salary and wages for each survey respondent over the year divided by 4, and hourly wages is further divided by a variable for the usual number of hours worked in a week (multiplied by 13). Wages are deflated to base year 2017 using Portland CPI.

Table B2: **Sample statistics for matched event-study panel**

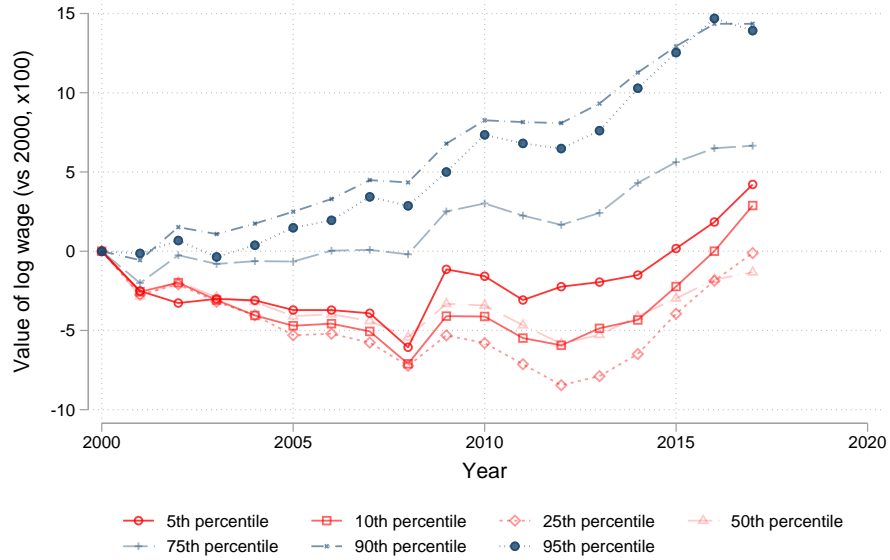
	Obs (total)	Workers (total)	Firms (total)	Events (total)	Workers per event (mean)	Earnings (mean, annual)	Hours (mean, weekly)	Tenure (mean, censored)
<i>Panel A: Full sample</i>								
Origin firm	872228	663279	27869	173257	245	42852	33.67	6.1
Intermediate firm	872228	663279	38522			44331	35.04	8.6
Final firm	313019	204549	23319			39944	35.04	5.0
<i>Panel B: Main estimation sample</i>								
Origin firm	346261	259415	4011	20771	527	43871	34.25	6.0
Intermediate firm	346261	259306	10215			45574	35.45	8.8
Final firm	117765	75964	7674			39581	34.93	5.0

Note: All employment-employment separations in the main worker-quarter panel are identified, an event-window isolated (8 pre-separation and 16 post-separation), and stacked. The first four columns indicate totals and other columns indicate means. ‘Events’ refers to the total number of origin firm-quarters within which workers are compared. Earnings are annualized from quarterly earnings tenure for the origin firm is censored at 8 quarters; and for both the intermediate and final firm are censored at 16 quarters after initial separation. Main estimation sample indicates the estimable sample for the main specification, which includes firm by calendar quarter by tenure (8 categories) by wage at hire (8 categories), all for the origin firm.

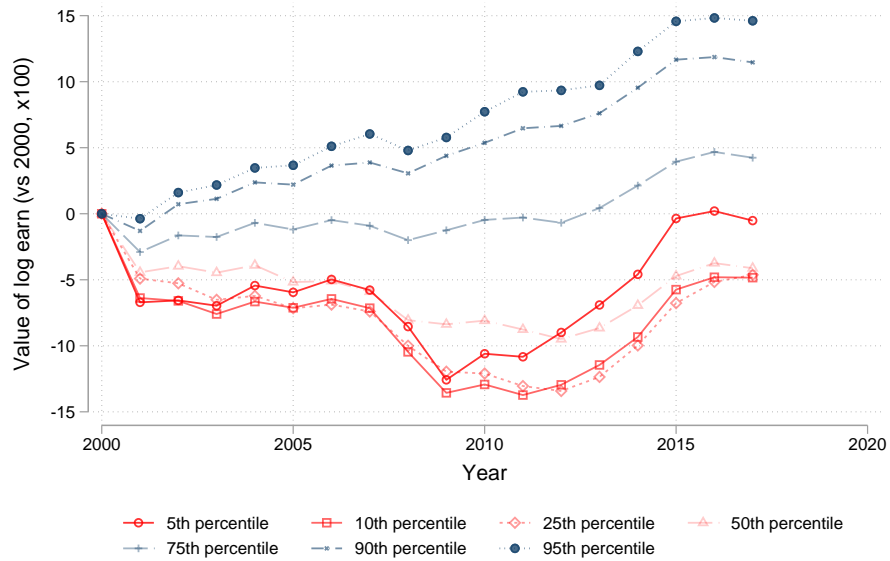
percentile, as shown in Figure B3 (using the full universe of observations). During this period, the largest growth in hourly wages occurred at the top (e.g., 95th percentile and 90th percentiles), while the real wage fell on net at the middle (50th percentile). However, during the same time wages rose at the bottom (5th and 10th percentiles), in part likely due to Oregon’s minimum wage policies. Overall, hourly wage inequality grew in the upper half of the distribution, mirroring other states (e.g. Lachowska et al., 2020), even while inequality fell in the bottom half. The patterns are qualitatively similar when we consider quarterly earnings instead; however, the 90-50 gap in earnings grew somewhat more than the equivalent gap in hourly wages over this period.

Figure B3: Oregon wage percentile trends

(a) Hourly wages



(b) Quarterly earnings



Note: Earnings are in real Dollars adjusted to 2017 using the Portland CPI. The sample corresponds to the main worker-quarter panel (after restrictions).

C AKM

C.1 Procedure

We restrict to the largest connected set using the ‘igraph’ package in R, after which we use the Stata-based high dimensional fixed effects estimator provided by Sergio Correia to regress wages on firm, worker and calendar-quarter fixed effects. This applies to each of the fixed effects samples separately: for example, the firm fixed effects for the first split sample of 2000-2005 are found by restricting the main worker panel to the first split sample in 2000-2005, finding the largest connected set of firms, and then estimating the AKM.

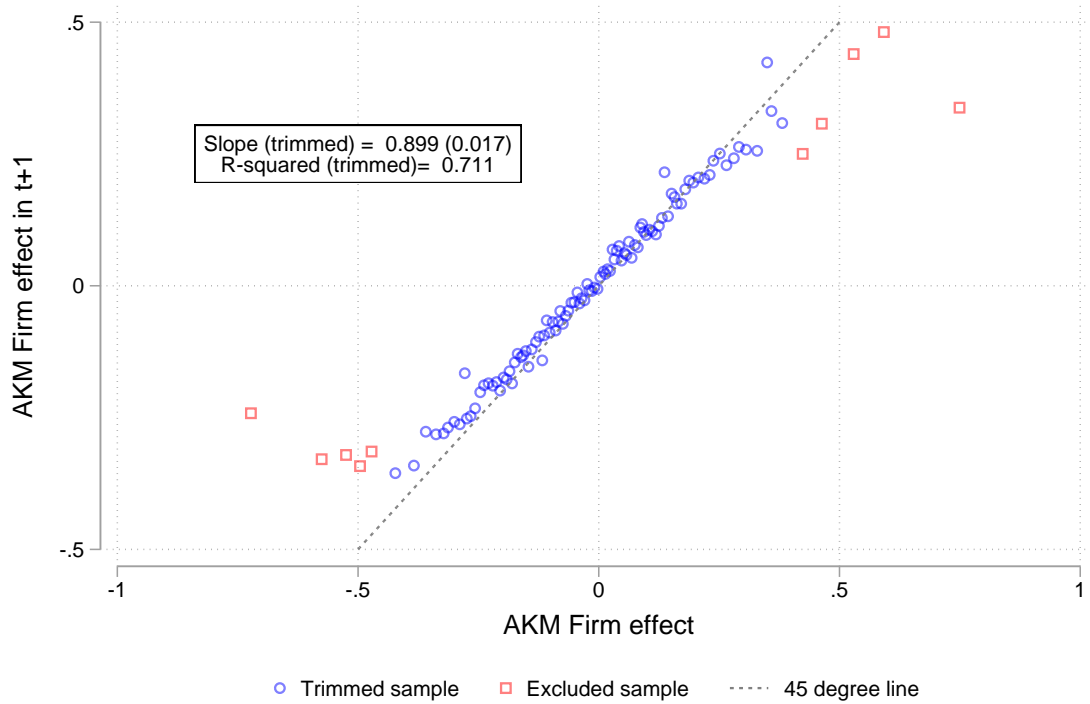
We check the estimates firm fixed effects using the procedure from Card, Cardoso, and Kline (2016), which is downloadable online. The correlation for the firm effects is 0.91, and for the worker effects is 0.99. The wage variance decompositions are also very similar (see below).

The AKM estimates by stacked 6-year sample are persistent. Figure C1 presents a plot of current versus next period firm hourly wage effects, with a resulting trimmed slope of 0.9 and R-squared of 0.7. The persistence across years of firm wage policies is consistent with the findings in Lachowska et al. (2020).

C.2 Decomposition

Table C1 provides the AKM decomposition in hourly wage and quarterly earnings inequality, for 6 year blocks between 2000-2017, as well as for the full panel. For both log quarterly earnings and log hourly wages, there is a slight increase in the overall variance between the 2000-2005 and 2012-2017 periods (0.37 to 0.41 for wages, and 0.59 to 0.64 for earnings). In the full panel, firm effects explain around 19% (14%) of the variance of quarterly earnings (hourly wages), and worker effects explain around 48% (55%) of the variance. There is also assortative matching of workers and firms, with the covariance term explaining around 14% (18%) of the variance. Consistent with other work, we see a clear increase in the covariance

Figure C1: Persistence of AKM firm hourly wage effects



Note: AKM firm wage effects are estimated for each 6 year period (2000-2005, 2006-2011 and 2012-2017) using hourly wages. For each firm, the AKM firm effect is plotted against its firm effect in the next 6-year period, and binned. The red indicates censored firm effects, which represent the 2.5% top and bottom tails of the firm effects distribution. Points are plotted at the firm level and weighted by firm size.

term for both wages and earnings over this period consistent with greater sorting. At the same time, there is a slight increase in the firm component of quarterly earnings variance, but a small decrease in the case of hourly wages. The R-squared is 0.8 to 0.9 for all AKM regressions, and is higher for hourly wages compared to quarterly earnings. It is also not much lower than the R-squared on a comparable match effects model (fixed effects for every job, instead of additive fixed effects for workers and firms as imposed by AKM), which for the 2012-2017 period using hourly wages is 0.91 (0.9 for AKM). This implies that the variation in log wages explained by match effects is small.

Comparable studies find similar AKM decompositions. Using annual earnings data for the US over the years 2000-2008, Sorkin (2018) finds that firm effects explain 14% of the log variance, worker effects explain 51%, and the covariance term explains 10%. Lamadon, Mogstad, and Setzler (2019) and Song et al. (2018) find a lower AKM firm effects share of 9% using annual earnings for a similar period. Lachowska et al. (2020) find using data from Washington over 2002-2014 for their annual log earnings AKM decomposition (plug-in version) that firm effects explain 19%, worker effects 54%, and the covariance term 17%; similarly to us, they also find that the share explained by firm effects decreases (to 11%) when using hourly wages instead of quarterly earnings.

Our preferred AKM specification relies on split-sample estimation. Table C2 provides the decomposition for each split sample using hourly wages, which is very similar across the two split samples and compared to the full sample decomposition above. Panel C shows some cross-sample statistics: the percentage covariance between own-sample and complement-sample fixed effects is lower than the direct firm effects variance in Table C1, and the percentage explained by the covariance between own sample worker effects and complement sample firm effects is higher than the comparable covariance in table C1.

Finally, we show that the AKM decomposition is very similar using code from Card, Cardoso, and Kline (2016) (table C3). As in table C1, for the last period the share explained by firm effects is lowest and the covariance between worker and firm effects is highest. The

Table C1: AKM decomposition

	2000-2005	2006-2011	2012-2017	2000-2017
<i>Panel A: Earnings</i>				
Var(Y)	0.592	0.63	0.639	0.621
% Var(Firm FE)	15%	15%	16%	19%
% Var(Worker FE)	58%	58%	56%	48%
% Var(Residual)	15%	15%	14%	21%
% 2×Cov(Firm FE, Worker FE)	11%	12%	14%	14%
% 2×Cov(Y, Firm FE)	42%	43%	46%	52%
Obs (millions)	22.60	25.20	25.70	73.40
Adjusted R^2	0.836	0.844	0.852	0.79
<i>Panel B: Wage</i>				
Var(Y)	0.37	0.395	0.409	0.392
% Var(Firm FE)	12%	11%	10%	14%
% Var(Worker FE)	62%	63%	63%	55%
% Var(Residual)	13%	11%	10%	17%
% 2×Cov(Firm FE, Worker FE)	14%	16%	17%	18%
% 2×Cov(Y, Firm FE)	37%	37%	38%	45%
Obs (millions)	22.60	25.20	25.70	73.40
Adjusted R^2	0.863	0.888	0.9	0.844

Note: All subsets use the relevant connected set, where the main sample is restricted to private firms larger than 20 workers (full sample description in text). Firm fixed effects are censored at the 2.5 percent upper and lower tails of the firm distribution. For reference, the full jobs model adjusted R^2 for 2000-2017 is 0.88, and for 2012-2017 is 0.91.

Table C2: AKM decomposition for split samples

	2000-2005	2006-2011	2012-2017
Panel A: Sample 1			
Var(Y)	0.37	0.395	0.409
% Var(Firm FE)	12%	12%	11%
% Var(Worker FE)	63%	64%	64%
% Var(Residual)	13%	11%	10%
% 2 Cov(Firm FE, Worker FE)	13%	14%	16%
% 2 Cov(Y, Firm FE)	37%	37%	38%
Obs (millions)	11.259	12.552	12.823
R^2	0.864	0.888	0.9
Panel B: Sample 2			
Var(Y)	0.37	0.395	0.409
% Var(Firm FE)	12%	12%	11%
% Var(Worker FE)	63%	64%	64%
% Var(Residual)	13%	11%	10%
% 2 Cov(Firm FE, Worker FE)	13%	14%	16%
% 2 Cov(Y, Firm FE)	37%	37%	37%
Obs (millions)	11.254	12.557	12.813
R^2	0.864	0.889	0.9
Panel C: Complement sample			
Var(Y)	0.37	0.395	0.409
% Cov($FirmFE_{own}$, $FirmFE_{complement}$)	11%	10%	9%
% 2 Cov($WorkerFE_{own}$, $FirmFE_{complement}$)	16%	17%	19%
Obs (millions)	22.227	24.808	25.33

Note: All subsets use the relevant connected set, where the main sample is restricted to private firms larger than 20 workers (full sample description in text). The main sample is randomly split into two samples, stratifying by whether the worker moved firms and clustering by worker. Firm fixed effects are estimated using log hourly wages, and censored at the 2.5 percent upper and lower tails of the firm distribution. Panel C shows the share of log wage variation explained by the covariance between the firm effects from a worker's own sample and the firm effects estimated using the alternate split-sample estimate for each worker's firm (comparable to the share explained by the variance of the firm effects); and the covariance between each individual's worker effect and the alternate split-sample firm effect estimate.

Table C3: **AKM decomposition using alternative code**

	2000-2005	2006-2011	2012-2017
Var(Y)	0.369	0.394	0.409
% Var(Firm FE)	12%	12%	11%
% Var(Worker FE)	63%	64%	64%
% Var(Residual)	13%	11%	10%
% 2 Cov(Firm FE, Worker FE)	13%	13%	16%
% 2 Cov(Y, Firm FE)	37%	37%	38%
Obs (millions)	22.397	25.037	25.562
Adjusted R^2	0.863	0.896	0.900

Note: AKM firm effects are estimated using Matlab code from Card, Cardoso, and Kline (2015), for log hourly wages in the main worker-quarter panel (full sample description in text). All subsets use the relevant connected set.

separations elasticity using these firm effects is also similar (if slightly lower), presented in table A2.

C.3 Limited Mobility Bias

A prominent threat to the AKM estimation of firm effects is limited mobility bias (Andrews et al., 2008). We replicate the comparisons in Lachowska et al. (2020) for our data to show that limited mobility bias likely becomes less severe with a longer panel and better measurement of wages (table C4).

Our panel has two advantages in addressing limited mobility bias. Firstly, a longer panel allows for more movers between firms, which is the source of identification for the AKM firm effects. The quarterly frequency, as compared to the annual data of many other studies, picks up more movers within the same time period. Secondly, insofar as firm pay policies correspond to *hourly* wages, annual earnings as used by many studies are a noisy measure of the firm effect. We observe hours, which allows us to estimate the firm effects on hourly wages directly.

These advantages of the panel contribute to better measurement of the AKM components. The first two columns show 2-year panels, and should be compared to the second 2 columns which show 6-year panels. The share of variance explained by the firm effects decreases for

the longer panel where more movers are observed, most noticeably for the annual earnings measure where we expect more noise.¹⁸ A similar pattern is observed for the share of variance explained across the panels: within each column, the share explained by firm effects decreases with better wage measures. On the other hand, the covariance between firm and worker effects rises dramatically as the panel length increases and the earnings measure improves.

Lower variance of firm effects and higher covariance between worker and firm effects are the two predictions of reductions in limited mobility bias, which both come through clearly for our data. Overall, comparing column 2 panel A (short panel, annual earnings) to column 4 panel C (longer panel, hourly wage), the share of log variance explained by firm effects decreases from 20% to 10%. The share explained by sorting, i.e. the covariance term, increases from 2% (suggesting very little sorting) to 17% (suggesting substantial sorting). Both features echo the findings of Bonhomme et al. (2020) and Lachowska et al. (2020).

¹⁸The last column shows the full panel, where the share of variance explained increases, likely due to actual increases in the variance, for example since more firms are included.

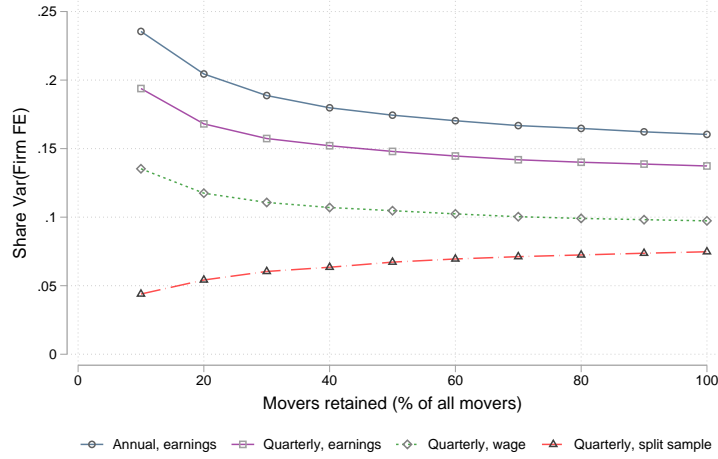
Table C4: AKM variance decomposition by panel length

	2-Year Panels		6-Year Panels		Full panel
	2002-2003	2013-2014	2000-2005	2012-2017	2000-2017
<i>Panel A: Annual earnings</i>					
Var(Y)	0.528	0.584	0.56	0.603	0.596
% Var(Firm FE)	23%	20%	17%	18%	21%
% Var(Worker FE)	80%	75%	62%	59%	49%
% 2×Cov(Firm FE, Worker FE)	-6%	2%	13%	16%	15%
Obs (millions)	1.81	2.02	6.61	7.51	21.80
<i>Panel B: Quarterly earnings</i>					
Var(Y)	0.588	0.639	0.592	0.639	0.621
% Var(Firm FE)	18%	16%	15%	16%	19%
% Var(Worker FE)	70%	66%	58%	56%	48%
% 2×Cov(Firm FE, Worker FE)	1%	7%	11%	14%	14%
Obs (millions)	7.46	8.48	22.40	25.60	73.40
<i>Panel C: Hourly wage</i>					
Var(Y)	0.366	0.41	0.37	0.409	0.392
% Var(Firm FE)	13%	10%	12%	10%	14%
% Var(Worker FE)	70%	72%	62%	63%	55%
% 2×Cov(Firm FE, Worker FE)	8%	12%	14%	17%	18%
Obs (millions)	7.46	8.48	22.40	25.60	73.40

Note: Earnings are quarterly total earnings; wages are quarterly hourly wages. All subsets use the relevant connected subset of the main panel (sample description in text).

Finally, we replicate the mobility bias figure presented in Lamadon, Mogstad, and Setzler (2019), while adding our improved measures of the firm effects for comparison. Figure C2 shows that as the share of movers retained increases, the share of log variance explained by the variance in firm effects decreases substantially for the annualized earnings panel (by about 8 percentage points) – as expected when limited mobility bias is reduced. However, as argued above, the reduction in share explained is lower using quarterly earnings (6 percentage points), or hourly wage (4 percentage points). Moreover, the bias when using our split sample measure (predicting own-sample firm effect by complement-sample firm effect) is in the *opposite* direction: the share of variance explained increases with share of movers retained.

Figure C2: Mobility bias by varying share of movers



Notes: The sample is restricted to the period 2013 to 2017 for comparability to other studies. The figure shows the proportion of wage variance accounted for by the estimated wage premia, where the horizontal axis indicates a subset of the data that randomly retains the corresponding share of movers. All subsets use the relevant connected set of firms. Firm fixed effects are censored at the 2.5 percent tails of the firm distribution. The blue line indicates an annualized panel using total earnings, the purple indicates the quarterly panel using total earnings, and the green indicates the quarterly panel using hourly wages. The red indicates the quarterly panel using hourly wages, where the split sample approach is used such that each firm's wage effect is the predicted value from a regression of own-sample firm effect on the complement sample firm effect.