Monopsony in Movers

The Elasticity of Labor Supply to Firm Wage Policies

Ihsaan Bassier
Arindrajit Dube
Suresh Naidu

ABSTRACT

We estimate the impact of the firm component of hourly wage variation on separations from matched Oregon employer–employee data. We use both firm fixed effects estimated from a wage equation as well as a matched instrumental variable (IV) event study around employment transitions between firms. Separations decline with firm wage policies: the implied firm-level labor supply elasticities are around 4, consistent with recent quasi-experimental evidence, but three to four times larger than existing estimates using individual wages. We find that monopsonistic competition is pervasive, even in low-wage, high-turnover sectors, but with little heterogeneity by labor market concentration.

I. 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

Ihsaan Bassier is at University of Massachusetts–Amherst. Arindrajit Dube is at University of Massachusetts–Amherst, NBER, and IZA. Suresh Naidu is at Columbia University and NBER. *Dube acknowledges support from the Washington Center for Equitable Growth. The authors gratefully acknowledge the generous help given by the staff at OED. This paper uses confidential data from Oregon's Unemployment Insurance payroll records, which we obtained as part of a data-sharing agreement with the state. To enquire about access, contact the Oregon Employment Department (OED, 503-947-1394).*
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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 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 study, 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 responsiveness 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, even though some recent papers using the traditional approach continue to find much smaller elasticities closer to one.[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 predetermine.

1. In 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.

2. For quasi-experimental estimates, see Caldwell and Oehlsen (2018); Cho (2018); Dube, Giuliano, and Leonard (2019); Dube, Manning, and Naidu (2018); or Kroft et al. (2020). For estimates using the traditional approach, see Bachmann, Demir, and Frings (2018); Booth and Katic (2011); or Webber (2015). Note that some estimates using this method do approach elasticities of 3 and 4, for example, Hirsch, Schank, and Schnabel (2010). A meta-analysis of estimates of labor supply elasticities by Sokolova and Sorensen (2021) reports that the median separations-based labor supply elasticity estimate is 1.7.

In this work, 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 between 2000 and 2017,[3] 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 (for example, Finkelstein, Gentzkow, and 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 19–14 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 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 coworker wages and hence receive different wage bumps. We then track their subsequent reseparation 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. By estimating the wage premia and separations elasticities jointly for the same set of workers, we allow for possibly heterogeneous firm premia and can recover a local average treatment effect (LATE) estimate of the potentially heterogenous separations elasticity.

We find that the firm components 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,

3. This contrasts with other matched employer–employee data set like the Longitudinal Employer Household Dynamics (LEHD) data in the United States or matched employer–employee data in many European countries.

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 percent, while our estimates suggest markdowns of around 20 percent.

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 (2022). The labor supply elasticity rose from around 4.0 during the recessionary period 2008–2010 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 4.5, as opposed to 3.9 in the 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 commuting zone by industry by 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 II describes our data source. Section III describes the research design. Section IV presents the empirical results from the AKM-based model and highlights potential issues with that strategy. Section V presents empirical results from the matched event study approach. Section VI concludes.

II. 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 microdata 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 cover a near census of employee records from the state. The payroll data rely on quarterly contribution reports submitted by the private sector as well as government employers for the purposes of unemployment insurance.

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

We use 18 years of data from 2000–2017, or 72 quarters; this data set 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 and 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 Online Appendix B. Here we provide 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 three 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, similarly 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 data set consists of 87.6 million observations and contains information on 3.4 million workers and 55,000 firms. Table B1 in the Online Appendix summarizes the data by six-year periods (the findings are also discussed below in Section IV.A). Each period has more than 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 made.[5] The average quarterly separation rate is 0.08, and about one-half of all hires come directly from other firms.[6] We observe more than one firm for 40 percent of workers within each six-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-nonemployment 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 percent in 2016, based on data from American Community Survey) is much smaller than the share of workers leaving their jobs in our main sample (26 percent in 2016).

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

6. The 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.

III. 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 in period t, denoted by f_{ijt}, transitions to firm j' in period $t+1$. 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 $y_{ij}=A_ip_j$. 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 $s_{ijt}\equiv 1-\Pr(f_{ijt+1}|f_{ijt})$ is the separations rate. In a stationary distribution, $\Sigma_{j'}\Pr(f_{ij't})\Pr(f_{ijt}|f_{ij't})=\Pr(f_{ijt})$. 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 have:

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

In steady state, a monopsonist will choose wages to pay workers of type i to maximize $\sum_i q_{ij}(A_ip_j - W_{ij})$ subject to $q_{ij}=\dfrac{R_{ij}(W_{ij})}{s_{ij}(W_{ij})}$. The marginal cost of employment of i with probability q_{ij} is $W_{ij}(q_{ij})\left[1+\dfrac{\mathrm{d}w_{ij}}{\mathrm{dlog}(q_{ij})}\right]$, 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 $\dfrac{\mathrm{d}w_{ij}}{\mathrm{dlog}(q_{ij})}=\dfrac{1}{\epsilon_j}$ 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 $\alpha\equiv\log(A_i)$ is the portable component of wages (for example, skill, but it could reflect other factors), while $\phi_j\equiv\log(\beta_jp_j)$ is the firm-specific component of the wage that is chosen by firms, with a markdown of $\beta_j=\dfrac{\epsilon_j}{1+\epsilon_j}$. 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, so the marginal cost of additional employment is $W_{ij}(q_{ij})\left[1+\dfrac{\mathrm{d}\phi_j}{\mathrm{dlog}(q_{ij})}\right]$, or equivalently that labor supply is solely a function of ϕ_j and $\dfrac{\mathrm{d}w_{ij}}{\mathrm{dlog}(q_{ij})}=\dfrac{\mathrm{d}\phi_j}{\mathrm{dlog}(q_{ij})}$. But by the steady-state assumption, $\dfrac{\mathrm{d}\phi_j}{\mathrm{dlog}(q_{ij})}=\dfrac{1}{\gamma(\phi_j)-\eta(\phi_j)}$, where $\gamma(\phi_j)=\dfrac{1}{E[R_{ij}]}\dfrac{\mathrm{d}E[R_{ij}]}{\mathrm{d}\phi_j}$ and $\eta(\phi_j)=\dfrac{1}{E[s_{ij}]}\dfrac{\mathrm{d}E[s_{ij}]}{\mathrm{d}\phi_j}$ 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, along with most subsequent work in this subliterature, then it is easy to see that[7]

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

$-\eta=\gamma$, and thus we have $\varepsilon=-2\eta$, 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 $\omega_j=\frac{s_jN_j}{\sum_j s_jN_j}$ is the share of all separations from firm j.

By imposing firm-specific elasticities that are common to all workers and having output $y_{it}=A_ip_j$ 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 (Card, Cardoso, and Kline 2016). The AKM decomposition would not be identified when pooled across types of workers—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 (in Section V) 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 the average difference in log wages across firms $\overline{w_j}-\overline{w_{j'}}$. 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, so an estimate of the separations elasticity facing the firm will be given by:

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

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 our estimated $\tilde{\eta}$ will generally be attenuated due to measurement error. For example, if Equation 1 were identified under an AKM-based strategy (the approach take in Section III.A below), then $\tilde{\eta}=\sigma\eta$, where $\sigma=\frac{\mathrm{var}(\phi_{j(i)t})}{\mathrm{var}(w_{ijt})}$ is the share of variation in wages that is due to firm effects. 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" (that is, 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 IV.A). 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 $\mathrm{Cov}(\alpha_i, \phi_{j(i)}) \neq 0$, 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 of estimating Equation 1 using AKM firm effects is unlikely to be valid because firms with high ϕ_j may be attracting very different types of workers.

A. 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

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

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, α_t is a time fixed effect, and ϵ_{ijt} is an error term.[8] Card, Heining, and Kline (2013) give a sufficient condition for identification:

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

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 it 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 "nonmonopsony" component h, which includes effects of sorting and strategic-interactions effects that depend on the worker effect α_i and the other firm's 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:

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

Under Equation 4, we have the empirical elasticity given by

$$\frac{1}{E[s_{ij}]}\frac{\mathrm{d}s_{ij}}{\mathrm{d}\phi_j} = -\frac{1}{E[s_{ij}]}\frac{\mathrm{dPr}(f_{ijt}|f_{ijt})}{\mathrm{d}\phi_j} = -\frac{1}{E[s_{ij}]}\frac{1}{2}\frac{\Pr(f_{ijt})}{\mathrm{d}\phi_j} = \hat{\eta}.$$

8. CHK also include an autocorrelation parameter in the error.

Note that any approach that regresses separations on firm effects must rule out pure sorting, that is, $\mathrm{Cov}(\alpha_i, \phi_J) > 0$, if we allow α_i to have an effect on firm assignment f_{ijt}. Sorting is allowed by Equation 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, $G_{jt}(\phi_1, \ldots, \phi_J, \alpha_i) = \epsilon\left(\phi_j, \left\{\phi_{j'}\right\}_{j' \neq j}\right) + h\left(\alpha_i, \left\{\phi_{j'}\right\}_{j' \neq j}\right)$; when we do this, we have an estimated elasticity given by $\hat{\eta} = \frac{1}{E[s_{ij}]}\frac{\mathrm{d}s_{ij}}{\mathrm{d}\phi_j} = \frac{1}{E[s_{ij}]}\int \eta_{\phi_j}\left(\phi_j, \left\{\phi_{j'}\right\}_{j' \neq j}\right)\mathrm{d}H(\{\phi_k\})$, where H is the distribution of the firm wage effects, that is, heterogeneity based on the wage policies of other employers. Note that ϵ or 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 $G_{jt}\left(\left\{\phi_{j'}\right\}, \alpha_i\right) = \epsilon\left(\alpha_i, \left\{\phi_{j'}\right\}_{j' \neq j}, \phi_j\right) + h\left(\alpha_i, \phi_j, \left\{\phi_{j'}\right\}_{j' \neq j}\right)$; 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 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 using AKM. 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.

B. 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 nonemployment. 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):

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

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 nonemployment, 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

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

where θ_S and θ_R give the proportion of separations to and recruits from employment, and $\gamma_0^{EE} = (1-\theta_R)(\gamma^{EE} - \gamma^{EE})$ is the elasticity of the share of recruits out of employment. The last equality follows is 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 "two-times-the-separations-elasticity" approach may yield similar estimates if the elasticity of the share of recruits from nonemployment (${\gamma_\theta}^{UE}$) is small and if the separation elasticities into employment and nonemployment are similarly sized. As we will see below, in practice, this seems to be the case in our sample.

C. 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 six-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 $\hat{\phi}_j^B$. 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:

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

We calculate the firm effects using the AKM approach, by six-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 percent and bottom 2.5 percent of the firm effects distribution. However, as we discuss below, the core elasticity estimates are not substantially affected by the trimming.

9. Sample 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 percent of firms in the pooled connected set are also in the A-connected set, and 99.8 percent of them are in B-connected set. (Moreover, the correlation coefficient between $\hat{\phi}_j^A$ and $\hat{\phi}_j^B$ is 0.965.)

IV. Results from AKM-Based Model

A. 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 Online Appendix Figure B3. During this period, the largest growth in hourly wages occurred at the top (for example, 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. In sum, hourly wage inequality grew in the upper half of the distribution, mirroring other states (for example, Lachowska et al. 2020), even while it fell in the bottom half. The patterns are qualitatively similar if we instead consider quarterly earnings. However, the 90–50 percentile gap in earnings grew somewhat more than the equivalent gap in hourly wages over this period.

Online Appendix Table C1 provides the AKM decomposition in wage and earnings inequality for six-year blocks for 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 percent (14 percent) of the variance of quarterly earnings (hourly wages), and worker effects explain around 48 percent (55 percent) of the variance.

This is similar to the findings of Lachowska et al. (2020), who, using hourly wage data from the state of Washington, estimate the firm effects' share of variance to be 19 percent and 12 percent of log earnings and log wages, respectively. There is also assortative matching of workers and firms, with the covariance term explaining around 14 percent (18 percent) of the variance in log earnings (wages). Consistent with other work (for example, 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 percent (14 percent) in 2000–2005 period to 14 percent (17 percent) 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 Online Appendix C, including an evaluation of limited mobility bias, which we conclude is not a major concern in our context given our relatively long (six-year) and higher frequency sample.

B. 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 Online 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

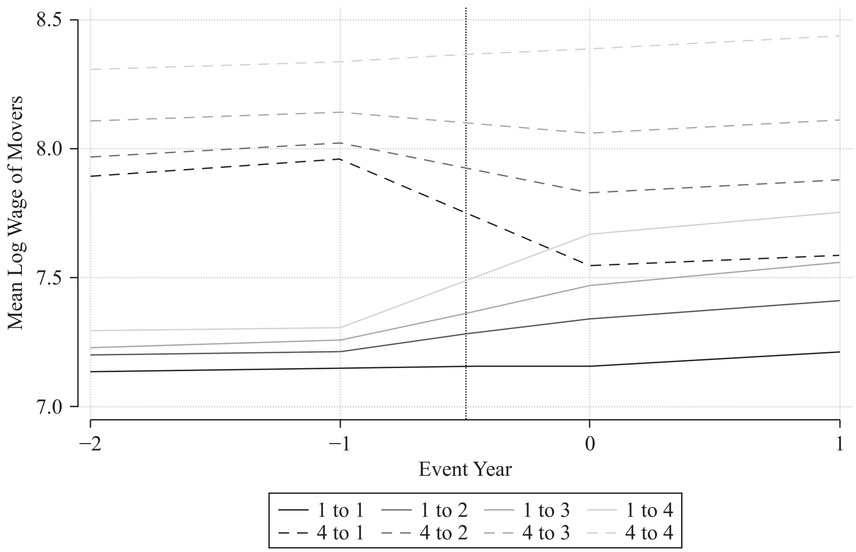

Figure 1

Changes in Hourly Wages across Job Separations for Firm Quartile-to-Quartile Transitions

Notes: 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 six-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 nine 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 six-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.

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 six-year 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 employment-to-employment (E-E) separations, E-E recruits, and the labor supply elasticity in Online Appendix Figures A3–A5.

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 E-E, employment to nonemployment separations (E-N), and employment-

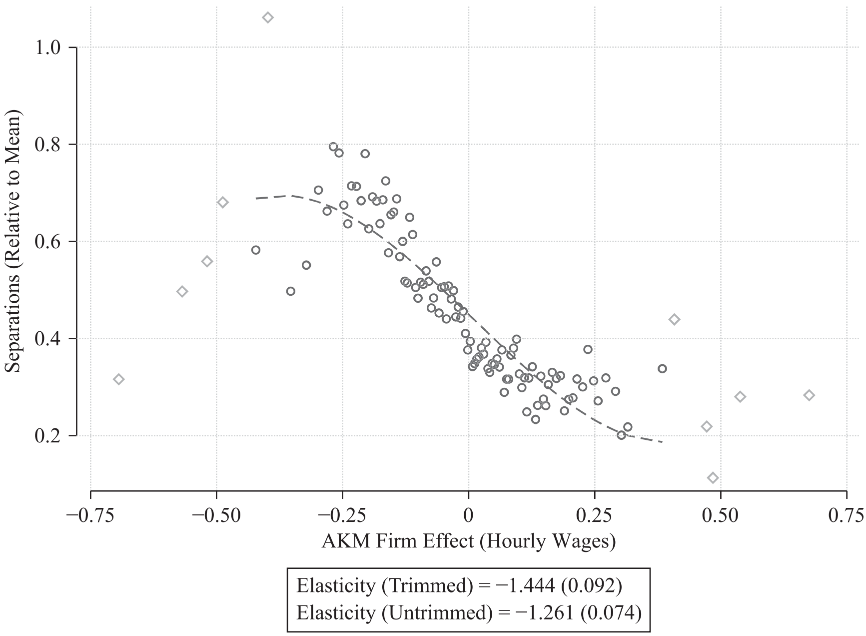

Figure 2

Separations and Firm Wage Effects

Notes: 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 (that is, depicting the partial correlation). The vertical axis is separations divided by mean separations such that the slope of the line represents the elasticity. The circles represent quantiles of the trimmed sample, which excludes the top and bottom 2.5 percent of the firm effects distribution. The diamonds represent quantiles of the excluded sample only, which we consider outliers. The trendline is a cubic polynomial fitted to the trimmed sample.

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 (2021).

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	–0.51	–0.622	−1.342	−1.448
	(0.005)	(0.01)	(0.015)	(0.085)	(0.095)
E-E separations	–0.317	–0.533	–0.753	−1.677	−1.811
	(0.007)	(0.013)	(0.023)	(0.127)	(0.141)
E-N separations	–0.291	–0.422	–0.578	−1.209	−1.303
	(0.005)	(0.01)	(0.014)	(0.075)	(0.085)
E-E recruits	0.266	0.127	0.067	0.413	0.438
	(0.022)	(0.031)	(0.017)	(0.059)	(0.064)
Pct. EE-recruits	0.47	0.47	0.464	0.464	0.465
Labor supply elasticity	0.355	0.879	1.345	2.69	2.912
	(0.024)	(0.037)	(0.039)	(0.199)	(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-statistic					9,792

Notes: The unit of observation for the hazard specifications is an employment spell and for the linear specifications are both 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*-statistic 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.

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 nonemployment 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 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

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	−0.936	−0.776	−0.809	−1.262	−1.228	−1.336	−1.406
	(0.066)	(0.071)	(0.033)	(0.039)	(0.075)	(0.065)	(0.055)	(0.063)
E-E separations	−0.866	−0.913	−0.946	−0.987	−1.607	−1.535	−1.545	−1.553
	(0.057)	(0.061)	(0.053)	(0.065)	(0.115)	(0.109)	(0.08)	(0.102)
N-E separations	−0.709	−0.752	−0.857	−0.739	−1.115	−1.161	−1.191	−1.293
	(0.054)	(0.058)	(0.033)	(0.034)	(0.066)	(0.053)	(0.05)	(0.048)
E-E recruits	0.783	0.832	0.493	0.349	0.354	0.442	0.323	0.338
	(0.112)	(0.121)	(0.042)	(0.045)	(0.071)	(0.064)	(0.064)	(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	0.908	1.348	1.493	2.597	2.429	2.578	2.629
	(0.143)	(0.154)	(0.089)	(0.107)	(0.186)	(0.174)	(0.136)	(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-statistic			4,586	12,043	8,637	9,820	11,015	9,266
Hazard spec.	Y	Y						
Annual earnings			Y					
Quarterly earnings				Y				
No trimming					Y			
Controls								
Tenure trend						Y		
Indus. × County FE							Y	
Indus. × Tenure trends								Y

Notes: The first-stage F-statistic 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 one-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.

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 (one-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.)

C. 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, Equation 2 and Assumption 3 hold. The second is that Equation 4 holds, so the covariation between separations and firm effects is driven by movements along the (possibly heterogeneous) residual labor supply curve, not other omitted variables (for example, 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 the 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 were correct, noncausal 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-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 (that is, worker, firm, average match residuals) in Online Appendix Table A1. In Column 1, we reproduce the baseline ordinary least squares (OLS) estimates from Column 4 of Table 1.[10] In Column 2, we report estimates from regressing separations on the firm 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

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

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 $f_{jt}^{i} = G_{jt}\left(\left\{\phi_{j'}\right\}, \alpha_i, \mu_{ij}\right)$. It follows that estimates of firm effects from $w_{ijt} = \sum_j \phi_j f_{jt}^{i} + \alpha_i + \epsilon_{ijt}$ will be biased because $\mathrm{Cov}(f_{jt}, \mu_{ij}) \neq 0$ and μ_{ij} is component of ϵ_{ijt}.

Card, Heining, and Kline (2013) provide several types of evidence against the importance of match effects. First they show that the unrestricted match effects model—that is, 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-squared 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-squared 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 hold necessarily. We also provide evidence that wage changes from upward and downward movements between quartiles are symmetric (see Online 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 $\hat{\mu}_{ij} = \frac{1}{T_{ij} - t_{ij}} \sum_{r=t_{ij}}^{T_{ij}} w_{ijr} - \hat{\alpha}_i - \widehat{\phi}_j$, 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 1–4, 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 5–8 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

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	0.058	0.060	–0.003	0.156	0.158	0.167	0.060
	(0.003)	(0.003)	(0.004)	(0.000)	(0.007)	(0.006)	(0.009)	(0.001)
Firm effect	0.513	0.430	0.504	0.444	−1.045	−1.202	−1.037	−1.174
	(0.011)	(0.011)	(0.011)	(0.011)	(0.029)	(0.031)	(0.029)	(0.030)
Obs.	1,625,209	1,497,149	1,393,070	1,386,540	1,625,209	1,497,149	1,393,070	1,386,540
Controls								
Industry × county		Y				Y		
Tenure			Y				Y	
Industry × tenure				Y				Y

Notes: 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 eight categories, and tenure indicates a fourth-degree polynomial.

elasticities are severely biased—indeed, they may be approximately correct. However, these failures do suggest 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 (for example, wages, firm assignment, past job stability) 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.

V. Using Matched Movers to Identify the Separations Elasticity

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:

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

where w_{ir} and f_{ikr} 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 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 $E\left[f^i_{jt}\epsilon_{jt}\right] = 0$ where ϵ is from the dynamic equation below:

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

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

$$(10)\quad w_{ijt} - w_{ijt-1} = \tilde{\phi}\left(\overline{w_j} - \overline{w_{j'}}\right)\left(f^i_{jt} - f^i_{j't-1}\right) + L(history_{i,t}) + \nu_{ijt}$$

which is similar to the specification estimated by Finkelstein, Gentzkow, and Williams (2016), but augmented with controls; they show that under the AKM assumptions, the coefficients on the change in log average wage can be interpreted as $\tilde{\phi} = \frac{\phi_j - \phi_{j'}}{(\overline{w_j} - \overline{w_{j'}})}$, which is the share of the mean difference in log wages across firms within a quarter explained by firm effects. However, we do not have to impose this interpretation on the $\tilde{\phi}$ coefficient 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: here the firm pay premium ϕ_j can be heterogeneous (possibly reflecting match effects), allowing different workers to 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:

$$s_{it+k} = \eta \Delta w_{ijt} + L(history_{i,t}) + \epsilon_{ijt+k} \tag{11}$$

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 nonemployment. We illustrate this comparison in a diagram in Online Appendix Figure A8.

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. Additionally, note that this formulation allows the separations elasticity η to be heterogeneous across workers (unlike in the AKM based approach), which means the estimate from Equation 11 can be interpreted as a weighted LATE. This allows for a much wider range of monopsonistic behavior than is admissible under AKM.

The approach above does not nest AKM because it excludes worker effects α_i. However, a sufficiently rich set of both 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 $f_{ij}^i = G_{jt}(\{\phi_k\}, \alpha_i, \{w_{is}, f_{is}\}_{s<t})$, 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).

A. 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 "reseparating" from $I(i)$ to another firm $F(i)$ (or to nonemployment) over the next k quarters (we take 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 $s^I_{i,t+k}$ at date $t+k$ is defined only for workers who had been working at the $I(i)$ firm through $t+k$, we regress

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

Note that L contains a fixed effect for $O(i)$ and includes wages at $O(i)$, so all the variation that identifies η_k 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

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

with a corresponding reduced form given by

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

In other words, we regress an indicator for reseparation 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 (that is, prior to date t) where we match workers based on their past histories, a treatment event (that is, transitioning to different I firms with different average wages at time t), and a post-treatment period where we can track their reseparation responses to a final firm or nonemployment.

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

B. 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

11. In 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 $w_{i,O(i),t-1}$.

Table 4
Separations Elasticities Based on Matched Event Study

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
First stage	0.122	0.148	0.148	0.176	0.173	0.070	0.165	0.171	0.173
	(0.006)	(0.001)	(0.003)	(0.004)	(0.003)	(0.009)	(0.007)	(0.006)	(0.006)
IV estimates									
Separations	−0.761	−2.431	−2.475	−2.100	−2.014	−1.293	−2.084	−2.085	−2.163
	(0.051)	(0.033)	(0.059)	(0.054)	(0.040)	(0.513)	(0.096)	(0.096)	(0.080)
E-E separations	−1.352	−4.000	−4.341	−4.031	−3.606	−1.754	−4.326	−4.379	−4.201
	(0.096)	(0.079)	(0.144)	(0.154)	(0.108)	(1.549)	(0.304)	(0.314)	(0.234)
E-N separations	−0.693	−2.409	−2.415	−2.048	−1.955	−0.761	−2.001	−1.969	−1.956
	(0.057)	(0.041)	(0.072)	(0.071)	(0.052)	(0.568)	(0.123)	(0.124)	(0.099)
Obs.	8.281	7.380	3.078	3.068	4.172	3.068	1.513	1.511	1.868
Movers	852,341	805,633	347,418	346,261	474,817	346,140	160,606	160,443	194,976
F-statistic (IV)	282	7053	1844	1397	2847	46	522	582	542
Coarsened controls					Y				

(*continued*)

Table 4 (*continued*)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Interacted controls									
Time	Y	Y	Y	Y	Y	Y	Y	Y	Y
$\times$ Firm		Y	Y	Y	Y	Y	Y	Y	Y
$\times$ $wage_0$ $\times$ tenure				Y	Y	Y	Y	Y	Y
$\times$ 3 qtr wage lags								Y	
Other controls									
O–I Firm-pair FE						Y			
AKM Worker FE									Y
Sample restricted based on									
Column 4			Y						
Column 8							Y		

Notes: See text for sample construction. The full instrumental variables specification is provided in Equations 12 and 13 in the main text. The outcomes $s^{I}_{i,t+k}$ indicate separation, E-E separation, and E-N separation such that s is missing for all periods after a single reseparation (and E-N reseparating 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 three quarters of pre-separation wages (Origin firm). Fixed effects are divided into eight equal bins, except where coarsened, which indicates that four 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 percent 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 reseparation rate.

to the 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 Online 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 (that is, $O(i)$ 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 eight categories of starting wages and tenure at $O(i)$ firm, along with calendar quarter fixed effects. This saturated specification compares workers who started at $O(i)$ firms in the same quarter, at the same wage, and transitioned to an $I(i)$ firm at the same date d, but with potentially different $I(i)$ firm average wage (of their coworkers). This is a rich set of controls, and we find that for this sample, a 10 percent difference in the $I(i)$ firm average wage leads to a difference in own wage of approximately 1.8 percent. 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 versus −1.4) but also more precise (standard error is 0.054 versus 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. Online Appendix Figure A7 shows the analogous binscatter but for E-E separations. Column 5 coarsens these controls to four categories of starting wages and tenure at the origin firm; this makes little difference to our estimates.

As 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 Online Appendix Figure A6, which shows that firm separations and firm recruits fall broadly along the 45 degree line.

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 percent 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, in addition to the preferred specification controls, with the ending wage at *O*-firm along with an additional three 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.

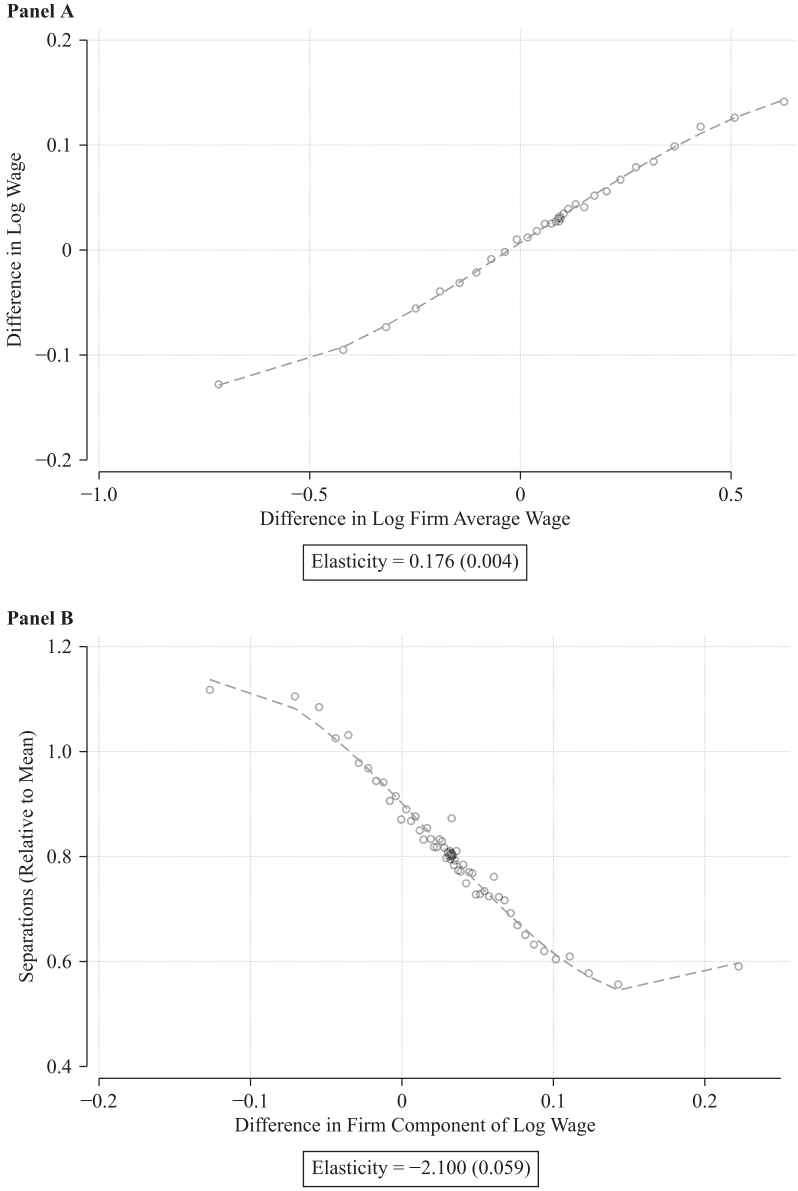

Figure 3

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, that is, 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 (eight bins) by initial wage at the origin firm (eight 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.

We next revisit the specification check we conducted in the previous AKM-based approach in Column 5. We determine whether adding worker wage fixed effects, $\hat{\alpha}_i$, alters the estimated separations elasticity. Recall that in the AKM-based approach, the 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 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 in the Online Appendix 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 visually 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 $\bar{w}_{i,I(i),t} - \bar{w}_{i,O(i),t-1}$, 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 firm fixed effect, the starting wages of workers at $O(i)$ in eight categories, their tenure in eight 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.[12] The coefficient of 0.18 at date t means that, on average, if a worker moves to an *I*-firm with 10 percent higher average wage, the worker's own wage increases by around 1.8 percent. Following Finkelstein, Gentzkow, and Williams (2016), we can interpret this to mean that around 18 percent 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, that is, plotting the impact of having a higher firm-average wage $\bar{w}$ on k-period retention probability for $k \in \{1, 2, \ldots, 16\}$. 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 gray). The gap in the retention probability between the gray and black lines is thus the causal effect of being assigned to a firm with a log point higher firm-average wage; four 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

12. As explained in Online Appendix B, which gives further details on sample construction, we set wages in the actual quarters of transition (dates −1 and 0) to missing because these hourly wage observations likely contain substantial measurement error associated with partly worked quarters.

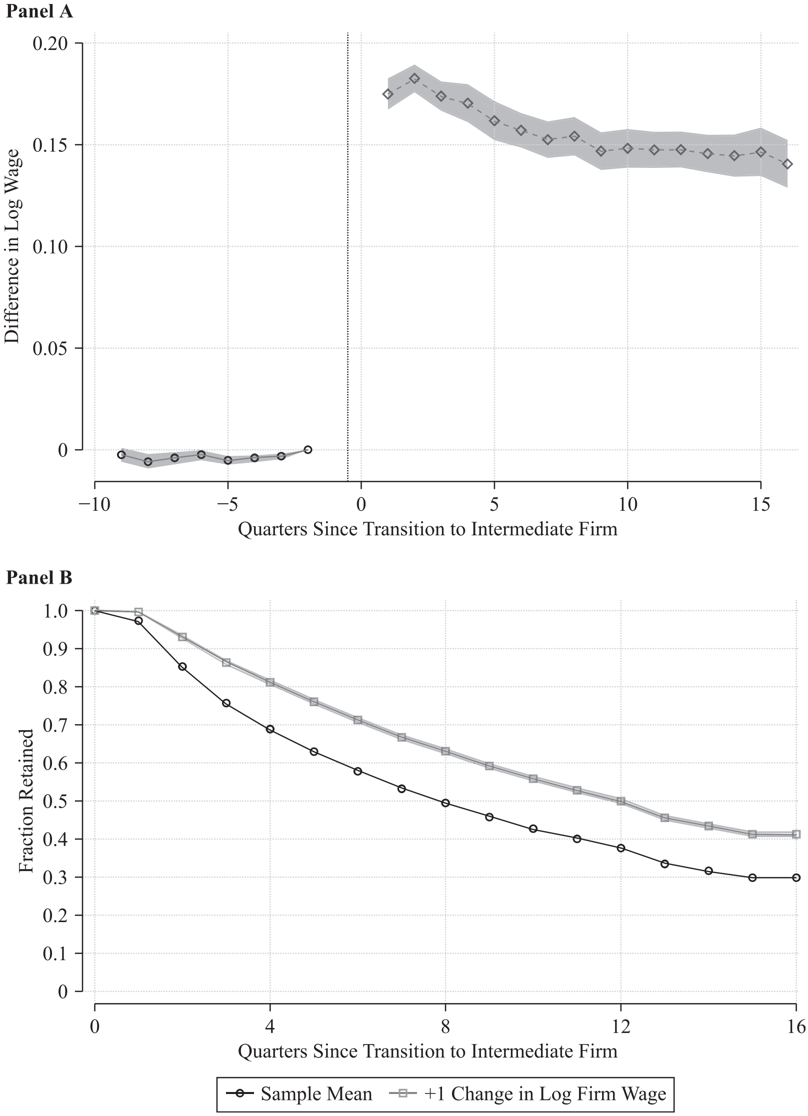

Figure 4

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 (eight bins) by initial wage at the origin firm (eight bins), and are clustered at the level of origin firm by time. Change in own wage is censored at the 1 percent tails. See text for sample construction.

latter are based on the the impact of firm wages ($\bar{w}$) on the hazard of separating at time period k, that is, $\frac{\partial}{\partial \bar{w}}\{\ln[\bar{R}_{t+k}(\bar{w})] - \ln[\bar{R}_{t+k-1}(\bar{w})]\}$. Pooling the impact on the hazard in periods $k \in \{1, 2, \ldots, 16\}$ 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 (for example, because of sorting) would still be counted in the estimated separations 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 for example, match effects) it just makes our IV estimate a (weighted) LATE applicable only to compliers, but still unbiased.

VI. Robustness and Heterogeneity

Table 5 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 −1.99. The separations elasticity with respect to the amenities value is −0.29. 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 the instrumented own-wage change (−1.96) 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 amenity 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 on this

Table 5
Alternative Specifications for Separations Elasticities Based on Matched Event Study

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
First stage	0.176	0.171	0.177			0.324	0.162
	(0.004)	(0.004)	(0.004)			(0.004)	(0.024)
IV estimates							
Separations	–2.100	–1.961	–1.992	–2.027	–0.272	–1.536	–2.322
	(0.054)	(0.057)	(0.054)	(0.072)	(0.012)	(0.037)	(0.527)
E-E separations	–4.031	–3.771	–3.803	–3.996	–0.445	–3.083	–4.100
	(0.154)	(0.161)	(0.153)	(0.210)	(0.026)	(0.115)	(1.068)
E-N separations	–2.048	–1.958	–1.968	–3.178	–0.385	–1.489	–3.228
	(0.071)	(0.076)	(0.072)	(0.152)	(0.026)	(0.049)	(1.035)
Obs. (millions)	3.068	2.999	2.984	3.069	3.073	3.082	0.112
Movers	346,261	340,000	338,562	346,714	347,193	346,684	11,044
F-statistic (IV)	1,397	1,279	1,345	196		4,447	32

(*continued*)

Table 5 (*continued*)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Quarterly earnings						Y	
Mass Layoff IV							
Firm wage	Y	Y	Y			Y	Y
BLM firm cluster				Y			
OLS					Y		
Firm value control		Y					
Firm amenities control			Y				

Notes: 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 eight 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 cluster" 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; that is, 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. Mass layoffs correspond to the quarter of initial transition from the Origin firm and are defined in the full panel (before restrictions based on firm size and short spells) following the WARN Act definition: a firm with at least 100 full time workers has at least either (i) 500 fewer workers in the following four quarters or (ii) one-third fewer workers in the following four quarters. Standard errors shown in parentheses are clustered at the level of Origin firm by initial separation quarter.

basis of 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 ten clusters of the $I(i)$ firm wage distribution (again, conditional on $O(i)$ firm fixed effects). Firms are partitioned into these ten clusters based on the proportion of workers in each ventile of the hourly wage distribution using k-means clustering. Use of the ten clusters as instruments—instead of the firm average wage—does little to change the separations elasticity, which in this case falls slightly to −2.03.

Column 5 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 −0.27 is around one-eighth of the magnitude of the IV estimate, and it 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.

Column 6 reproduces the main specification using quarterly earnings rather than hourly wages. Similar to the AKM-based estimates, the estimates based on quarterly earnings are substantially attenuated, with a separations elasticity of −1.54. This, again, highlights the importance of adjusting for hours.

A final specification in this table (Column 7) addresses selectivity concerns (for example, time varying worker heterogeneity not captured by history) around the *O–I* transition by only considering such transitions induced by mass layoffs. Following the WARN Act definition, we define a mass layoff as when a firm with at least 100 full time workers has either (i) 500 fewer workers in the following four quarters or (ii) one-third fewer workers in the following four quarters. About 11,000 moves occur under these conditions. Overall, we find very similar results to the preferred specification (Column 1) for the first-stage and separations elasticities.

Table 6 presents the heterogeneity in the separation elasticities. Using the one-digit NAICS supersectors, we exclude agriculture, mining, utilities, and construction because these industries have far fewer employees (less than half the number employed in the 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 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 in Webber (2015). It is also worth noting that one may have assumed that low-wage sectors 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 (2022).

We also report elasticities separately for the Portland metro area and the rest of Oregon (Panel B). These two subsamples differ dramatically in levels of labor market

Table 6
Heterogeneity in Separation Elasticities Based on Matched Event Study

	First Stage		Separations		E-E separations		Movers
Panel A: Industry of Destination Firm							
Manufacturing	0.178	(0.01)	–2.287	(0.298)	–4.136	(0.804)	36,919
Wholesale, trade & transport	0.188	(0.008)	–1.394	(0.159)	–3.391	(0.487)	63,158
Prof., business & financial services	0.117	(0.01)	–3.91	(0.267)	–7.974	(0.856)	71,620
Education and health	0.154	(0.006)	–2.148	(0.158)	–3.777	(0.503)	58,072
Art, accommodation & food	0.238	(0.021)	–1.201	(0.255)	–2.301	(0.786)	22,999
Panel B: Geographic Zone of Destination Firm							
Portland metro	0.159	(0.005)	–2.237	(0.132)	–4.584	(0.397)	92,123
Non-Portland metro	0.182	(0.007)	–1.969	(0.142)	–3.648	(0.472)	51,957
Panel C: HHI (Employment)							
0–500	0.172	(0.007)	–1.757	(0.154)	–3.645	(0.5)	46,675
500–1500	0.163	(0.011)	–1.668	(0.277)	–2.701	(0.956)	30,460
1500+	0.159	(0.007)	–2.241	(0.231)	–4.066	(0.732)	48,489
Panel D: HHI (Payroll)							
0–500	0.182	(0.008)	–1.712	(0.157)	–3.597	(0.51)	44,997
500–1500	0.16	(0.01)	–1.437	(0.311)	–3.594	(1.183)	29,222
1500+	0.158	(0.007)	–2.372	(0.206)	–4.086	(0.624)	50,986
Panel E: Period of Initial Separation							
2003–2006	0.17	(0.004)	–2.353	(0.108)	–4.489	(0.277)	91,712
2007–2009	0.171	(0.013)	–2.044	(0.154)	–4.194	(0.406)	69,886
2010–2012	0.178	(0.01)	–2.481	(0.127)	–4.687	(0.306)	79,758
Panel F: Quartile of Pre-separation Wage							
Quartile 1	0.194	(0.004)	–1.46	(0.054)	–2.337	(0.133)	86,475
Quartile 2	0.198	(0.009)	–1.979	(0.1)	–4.088	(0.294)	68,597
Quartile 3	0.168	(0.013)	–2.451	(0.176)	–5.438	(0.571)	66,691
Quartile 4	0.127	(0.006)	–2.282	(0.2)	–3.966	(0.502)	81,470
Panel G: Time horizon							
4-quarter out	0.176	(0.004)	–2.01	(0.051)	–3.082	(0.116)	346,261
8-quarter out	0.176	(0.004)	–2.262	(0.057)	–3.547	(0.132)	346,261
12-quarter out	0.176	(0.004)	–2.149	(0.054)	–3.746	(0.141)	346,261

Notes: Industry is defined at the one-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 four years. Portland metro indicates the Portland metro commuting zone. HHI indicates the annual commuting zone by industry (four-digit) Herfindahl–Hirschman index using employment and payroll, respectively. Time horizon censors the sample at different maximum quarters and presents the average elasticity over that period. Standard errors shown in parentheses are clustered at the level of Origin firm by initial separation quarter.

concentration, where labor markets are defined at the level of commuting zone by four-digit industry by year (following Rinz 2022). In metro Portland, the average employment (payroll) Hirschman–Herfindahl index (HHI) is 0.12 (0.14), while outside of the Portland metro area the average HHI is higher at 0.27 (0.29), confirming that concentration is higher in rural labor markets. We do find some evidence that the implied labor supply elasticities are 15 percent larger in Portland (4.5) than outside (3.9), 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 230 percent 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 commuting zone × industry (four-digit) × year HHI 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 1500, and greater than 1500. 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 1500+ employment HHI category is around 4.5, while the elasticity is around 3.5 in the below 500 employment 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 (2022); Rinz (2022); 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 (2022). 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 (2021) 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 (2022), Depew and Sørensen (2013), and Hirsch, Schank, and Schnabel (2010), even though the magnitudes in our findings are larger than previous U.S. estimates.

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 mostly 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, we restrict the regression sample to different post-period lengths (Panel G). While our preferred estimate uses a post-transition window length of 16 quarters, the separations elasticities are quite stable across windows using 4, 8, or 12 quarters, ranging between −2.01 and −2.26. 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 higher separations elasticities in general 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 6 for space reasons, we find only moderate heterogeneity by pre-Origin number of moves, where the separations elasticity is very similar (−2.09 versus −2.08) and the E-E separations elasticity is somewhat higher (−4.5 versus −3.8) for workers with one or more moves before their switch from Origin to Intermediate compared to workers with none.

VII. 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 "reseparation" 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 attenuation 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 (for example, Sorkin 2018; Lamadon, Mogstad, and Setzler 2019) is that a substantial part of firm fixed effects reflects compensating differentials for firm-specific disamenities. Our study 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 × county and industry × tenure controls, and these controls are likely to correlate with a great deal of amenity variation. 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 firms with higher estimated amenities values do have lower separation rates, controlling for these amenities values 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, the extent to which separations vary with the portable 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 we have adopted with job differentiation and concentration to both unify and disentangle the sources of monopsony power across labor markets.

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