Employer Power: Consequences for Wages, Inequality and Spillovers

Ihsaan Bassier

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EMPLOYER POWER:
CONSEQUENCES FOR WAGES, INEQUALITY AND
SPILLOVERS

A Dissertation Presented
by
IHSAAN BASSIER

Submitted to the Graduate School of the
University of Massachusetts Amherst in partial fulfillment
of the requirements for the degree of

DOCTOR OF PHILOSOPHY

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Department of Economics
ACKNOWLEDGMENTS

Although it is a very un-materialist sentiment, I feel I have been enormously lucky over the 5 years of my PhD. I came to UMass looking for a model of the labor market that is alive to our reality of inequality and power, and found an advisor that is a leading expert in exactly this. My foremost thanks are to Arindrajit Dube, who has been a true mentor to me. He is a brilliant scholar, an excellent teacher, and a caring advisor. My four years as research assistant to him brought rare opportunities, in learning, funding and ambitions. He taught me to aspire towards frontier research as I would not have otherwise. Through Arin, I met Suresh Naidu, whose work I admire and am inspired by. I loved our weekly dissertation therapy, and am in constant awe of how expansive and creative (and niche!) his insights are; he conveys an infectious wonder in intellectual exploration. Many thanks to Deepankar Basu for teaching me patient scholarship, and to Jasmine Kerrissey for inter-disciplinary grounding. I sometimes go back through my emails, and am amazed at how I landed such an incredible committee.

There’s the official committee, and then there’s the unofficial one. If I had to pick my luckiest PhD-moment, it would be unwittingly arriving at the same time and place as Joshua Budlender. This dissertation would not have happened without him. All I can say is that I hope to enjoy his curiosity, rigour and intellectual clarity for as long as I can, as friend and co-thinker. Thomas Mbewu has been another unofficial advisor: as my resident mathematician, he indulged my pestering through rabbit holes of useless models, and as only a true friend can, he bears the burden of stopping me from becoming an insufferable economist. Leila Gautham painstakingly provided feedback on draft after draft of my chapters, while keeping me sane with transportive
fiction recommendations. And Surbhi Kesar is my lifelong intellectual comrade, with eclectic and radical thought that breathes life into my hopes of contributing to the re-making of a completely different world.

I have had the privilege of institutional support from the Southern Africa Labour and Development Research Unit (SALDRU) at the University of Cape Town. In particular Murray Leibbrandt gave me the once-in-a-lifetime opportunity to contribute meaningfully through policy-making during the devastating pandemic, and has been nothing short of a huge support throughout. Vimal Ranchhod took a chance on an idealistic ANC member one year out of high school, and has been a warm, sharp and formative mentor to me ever since. I have also had the skilled administrative support of the SARS-NT Secure Data Facility in Pretoria, which two of my three dissertation chapters rely on. Amina Ebrahim spent years laying the groundwork, and generously brought me up to speed whenever I needed it. Junior Chiweza, Marlies Piek, Michael Kilumelume, and many others balanced efficiency and data security in a way I am only beginning to appreciate.

Although I only ended up spending three years out of my PhD in the physical UMass environment (due to the pandemic), they were three years of exciting intellectual growth. For this I thank our little labor and development group, including Manuel Garcia, Carly McCann and Anamary Maqueira Linares, as well as the organizers and participants in the many workshops that were often the highlight of the week, especially the Applied Microeconomics and the Analytical Political Economy workshops. I also thank Bridget Diane and Manya Budhiraja for their heartfelt friendship, as well as Esra Ugurlu, Adam Aboobaker, Francisco Perez, Jonathan Jenner, Ragini Jha, and Swati Birla. Of course the most stimulating part of grad school was the coursework, and I am lucky to have been taught by intellectual giants. I am so grateful to have learned from Nancy Folbre, Peter Skott, Samuel Bowles, Carol Heim, Mwangi wa Githinji, Ina Ganguli, and many others.
Finally, grad school in a cold foreign country can be isolating (to be euphemistic), yet I have always had the unconditional love (and treats!) of my family. To my parents (thrice to my mother and once to my father), thank you for raising me with such sacrifice and dedication, to have confidence, high standards and balanced priorities. To my siblings, thank you for navigating the way before me, so that I could enjoy all the perks of a youngest child. And of course a special mention to my unbelievably cute nephews, and my extended family. Outside of Cape Town when I stayed in Johannesburg for two years to work on the tax data, to my delight, I found the love and comfort of (mostly) old friends who had been attracted there for one reason or another: Mahfouz Raffee, Aliya Chikte, Jeremy Beremy, Emma Smith, Rabia Abba Omar, Nishal Robb, Leila Khan, and Zahraa Mohamed. Thank you all.
In several countries, wages have stagnated and union membership declined, even as productivity has increased. The established view of employers helpless to the labor market’s invisible hand has increasingly come under question. Attention has turned towards the power of employers to set wages; yet only recently have the data required to investigate this – observing workers at their employers – become available, and then mostly in richer countries.

My first chapter, “Monopsony in Movers” (co-authored with Arindrajit Dube and Suresh Naidu), proposes a new credible estimation strategy to measure employer monopsony power. We build on the idea that employers with more power can pay lower wages without workers quitting. Using administrative data from Oregon, USA, we compare the quit rates of similar workers who leave the same firm at the same time but land at differently paying new firms. We find that monopsonistic competition is pervasive, even in low-wage, high-turnover sectors.
My second chapter, “Firms and Inequality When Unemployment is High”, investigates employer power using tax data from South Africa. Previously, developing countries received marginal attention in this literature. I find that firm wage policies explain a larger share of wage inequality in South Africa than in richer countries. I estimate that this is driven by higher productivity dispersion, and employer monopsony power, which is in turn linked to higher unemployment. These firm-level dynamics may exacerbate inequality in developing countries more generally, which share similar characteristics.

My third and main chapter, “Collective Bargaining and Spillovers in Local Labor Markets”, considers wage-setting institutions under monopsonistic competition. My model predicts that non-covered firms “close” to collective bargaining firms will increase their wages in tandem with wage agreements. I propose a model-consistent and empirically rich measure of “close” firms, the flow of workers between them, which captures firm strategic interaction. I test my hypotheses across a decade of wage agreements. Observed wages in collective bargaining firms follow sharp increases in prescribed wages, and indeed firms “closer” to bargaining councils increase wages more than firms further away. A microdata simulation suggests that spillovers double the intensive and extensive margin effects of collective bargaining agreements on the full wage distribution.
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CHAPTER 1

MONOPSONY IN MOVERS: THE ELASTICITY OF LABOR SUPPLY TO FIRM WAGE POLICIES
1.1 Introduction

How elastic is the supply of labor to a single firm? The firm-level labor supply elasticity measures the degree of monopsony in the labor market, estimates of which have proliferated in recent years. Small values of this elasticity imply significant degrees of monopsony power, while large values imply close to competitive behavior in labor markets. In models of dynamic monopsony, \cite{130} 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 paper, we revisit this estimation strategy using plausibly causal effects of firms on hourly wages and high quality administrative data to address measurement and identification shortcomings that may have biased previous results. As we show, adopting this approach makes a substantial difference in the conclusions we can draw about the competitiveness of the U.S. labor market.

Following \cite{130}, researchers have typically estimated separations elasticities with respect to individual earnings, conditional on observable control variables. However, there are a number of \textit{a priori} reasons to believe this may induce biases in the estimates for the labor supply elasticity, $\epsilon$. 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, social networks,

\footnote{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.}
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 1.\textsuperscript{2} To the best of our knowledge, no paper has estimated labor supply elasticities using the firm component of pay. The appendix to \textsuperscript{57} considers a regression similar to ours (where they regress tenure on firm effects), without interpreting the coefficients as firm labor supply elasticities.

A final concern is that many of the existing papers rely on quarterly or annual earnings (rather than hourly wages), which may create additional bias. Most importantly, use of earnings is likely to attenuate the estimated labor supply elasticity due to the measurement error associated with hours. On the other hand, if hours are correlated with unobserved heterogeneity in separations, then the direction of bias may be difficult to pre-determine.

In this paper, we propose an alternative approach using a new data source that addresses these concerns. Using hourly wage information from matched employer-employee data from Oregon between 2000 and 2017\textsuperscript{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

\textsuperscript{2}For quasi-experimental estimates, see \textsuperscript{52}, \textsuperscript{62}, \textsuperscript{76}, \textsuperscript{78}, or \textsuperscript{119}. For estimates using the traditional approach, see \textsuperscript{16}, \textsuperscript{40} or \textsuperscript{165}; note that some estimates using this method do approach elasticities of 3 and 4, e.g \textsuperscript{100}. A meta-analysis of estimates of labor supply elasticities by \textsuperscript{154} reports that the median separations-based labor supply elasticity estimate is 1.7.

\textsuperscript{3}This contrasts with other matched employer-employee dataset like the Longitudinal Employer Household Dynamics (LEHD) data in the US or matched employer-employee data in many European countries.
estimate what happens to the separations rate when firms that hire otherwise similar workers happen to pay somewhat differently. Here we draw on the “mover-based” design used in other recent contexts, such as studying the impact of location on health, intergenerational mobility, and other outcomes (e.g., [83], [61]).

As a first pass, we isolate the component of individual wages determined by firm wage policies using the log additively separable model proposed by [2]—hereafter AKM. We take the estimated firm effects, and estimate the effect of just this component of the wage on separations. Similar to previous work, we find firms play an important role in wage setting, though the use of hourly wages reduces the firm effect contribution to log wage variance from 19 to 14 percent; we also find clear evidence of rising sorting over time between high-wage workers and high-wage firms in Oregon. Use of the AKM firm effect allows us to focus on the wage variation that is likely arising from similar workers receiving different pay due to their employers, but not due to other arbitrary wage differences across individuals, for example due to skill. However, as we show, firms with different AKM effects may also systematically draw different types of workers, which confounds our ability to use aggregate, firm-level variation in AKM and separation rates to identify labor market power. In addition, there is a concern that the AKM approach does not allow the assignment of workers to firms to be based on “match effects,” something we find in our data.

For these reasons, we develop a matched event study approach in which we consider workers with very similar past histories (in terms of wage levels, growth, past employers, and past tenure) who happen to start new jobs at firms with different co-worker wages and hence receive different wage bumps. We then track their subsequent re-separation response. This refinement allows us to control for much richer forms of worker-level heterogeneity in both wage and separation dynamics that are predicted by past outcomes and history. 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 component of wage—as measured using either AKM or our matched event study approach—are clearly negatively correlated with the overall separation rate and particularly the job-to-job separation rate, consistent with the firm effects reflecting “better jobs.” The baseline AKM-based separations elasticity is around -1.4, where use of a split-sample instrument that corrects for measurement error in the estimation of the firm effects produces a slightly larger labor supply elasticity, as expected. The separations elasticity estimate from our preferred matched event study approach is -2.1. These results imply labor supply elasticities of around 3 and 4, respectively. Importantly, use of the firm component of wages increases the labor supply elasticity estimates by a factor of 2.5 to 4 as compared to the standard approach using individual wages. Our preferred labor supply elasticity of 4.2 suggests a moderate amount of monopsony power in the U.S. labor market, but much less than the very high degree of labor market power suggested using the traditional approach—which tends to generate labor supply elasticities that are one-third or one-fourth as large as the ones we find here. To put this in perspective, the traditional approach suggests markdowns of around 50%, while our estimates suggest markdowns of around 20%.

While our labor supply estimates are substantially larger than those using the standard approach, we confirm that the labor supply elasticity is procyclical—similar to the findings in [167]; 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 2 describes our data source. Section 3 describes the research design. Section 4 presents the empirical results from the AKM-based model, and highlights potential issues with that strategy. Section 5 presents empirical results from the matched event study approach. Section 6 concludes.

1.2 Data

As part of the Oregon’s unemployment insurance (UI) payroll tax requirements, all employers are obliged to report both the quarterly earnings and quarterly hours worked for all employees. We obtained Oregon’s micro-data as part of a data sharing agreement with the state, allowing us to construct hourly wage information for nearly all workers using high quality administrative sources. The resulting administrative matched employer-employee microdata covers a near census of employee records from the state. The payroll data relies on quarterly contribution reports submitted by the private sector as well as government employers for the purposes of unemployment insurance.

\[\text{Only three other states (Washington, Minnesota and Rhode Island) require employers to similarly report hours of work as part of their UI systems.}\]
We use 18 years of data from 2000-2017, or 72 quarters; this dataset consists of around 136 million observations that correspond to 317,000 different firms and 5.3 million workers. An advantage of this data is that we observe quarterly wages as well as hours for each worker, allowing us to gain precision in distinguishing, for example, higher paid part-time workers from lower paid full-time workers. We observe all employer-employee quarterly matches: therefore, in the unprocessed data, a worker may have multiple observations in a given quarter that have been reported by different firms. Oregon has a median household income that is close to the national median, 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 [57, 121, 122, 155, 156]. We describe the steps and justifications in much greater detail in our Appendix A.2; 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 3 quarters in length (which is the necessary duration to obtain at least one full quarter of wage information). Where spells overlap, we convert to a worker-level quarterly panel by selecting the spell with the highest average earnings. We restrict the data to private-sector firms with more than 20 employees; this is similar to [155], although in our case the restriction is based on state-level employment. This allows for meaningful estimation of within-firm statistics, and as we show, this also mitigates the impact of limited mobility bias in estimating firm effects.

After applying these screens, our final dataset consists of 87.6 million observations and contains information on 3.4 million workers and 55,000 firms. Table A.3 in the Appendix summarizes the data by 6-year periods (the findings are also discussed
Each period has over 28 million observations. The national median annual earnings for 2013 reported by [155] is $36,000, which corresponds to the 2013 Oregon median of $39,000, once comparable restrictions are made. The average quarterly separation rate is 0.08, and about half of all hires come directly from other firms. We observe more than one firm for 40\% of workers within each 6-year panel. As we explain later, movers between firms drive the identification of the firm effects.

One limitation of using data from a single state is that separations to firms outside Oregon are not counted as job-to-job separations, but rather job-to-non-employment separations. However, we note that for our primary analysis using all separations, the precise destination is immaterial. Moreover, any bias in estimating the job-to-job component of the elasticity is likely limited given the share of workers who likely moved out of Oregon (3\% in 2016, based on data from American Community Survey) is much smaller than the share of workers leaving their jobs in our main sample (26\% in 2016).

1.3 Research design

We begin by sketching a simple model of dynamic monopsony, and relate it to statistical models of wage determination (like AKM). Suppose a worker $i$ employed at firm $j$, denoted by $f_{ijt}$, transitions to firm $j'$. As a starting point, assume that worker’s marginal product has worker-specific component $A_i$ that is fixed across firms and, crucially for our approach, does not affect transition probabilities across firms.

\[5\] [155] 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 [165] using the LEHD.
Marginal productivity also has a firm-specific component denoted \( p_j \), with overall match marginal product given by \( y_{ij} = A_i p_j \). We denote as \( Pr(f_{ij't+1}|f_{ijt}) \) the probability of transitioning to firm \( j' \) at time \( t+1 \) given \( i \) was at firm \( j \) at time \( t \), so \( s_{ijt} \equiv 1 - Pr(f_{ij't+1}|f_{ijt}) \) is the separations rate. In a stationary distribution, \( \sum_{j'} Pr(f_{ij't}) Pr(f_{ij't}|f_{ij't}) = Pr(f_{ijt}) \). 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:

\[
\sum_{j' \neq j} Pr(f_{ij}|f_{ij'}) Pr(f_{ij'}) = Pr(f_{ij})(1 - Pr(f_{ij}|f_{ij}))
\]

In steady state, a monopsonist will choose wages to pay workers of type \( i \) to maximize \( \sum_i q_{ij}(A_i p_j - W_{ij}) \) subject to \( q_{ij} = \frac{R_{ij}(W_{ij})}{s_{ij}(W_{ij})} \). The marginal cost of employment of \( i \) with probability \( q_{ij} \) is \( W_{ij}(q_{ij})(1 + \frac{dw_{ij}}{dlog(q_{ij})}) \) where \( w_{ij} \equiv logW_{ij} \). Since the labor-supply elasticity is solely a function of the firm component of wages, we impose that \( \frac{dw_{ij}}{dlog(q_{ij})} = \frac{1}{\epsilon_j} \) is constant for all \( i \) given \( j \). At the optimum, we will have that the log wage is \( w_{ij} = \alpha_i + \phi_j \), where \( \alpha_i \equiv log(A_i) \) is the portable component of wages (e.g., skill, but could reflect other factors) while \( \phi_j \equiv log(\beta_j p_j) \) is the firm-specific component of the wage that is chosen by firms, with a markdown of \( \beta_j = \frac{\epsilon_j}{1+\epsilon_j} \). Since the portable component \( \alpha_i \) is common across firms, the key assumption we are making is that only the firm-specific component of the wage changes along with the employer’s choice of \( q \), and so the marginal cost of additional employment is \( W_{ij}(q_{ij})(1 + \frac{d\phi_j}{dlog(q_{ij})}) \), or equivalently that labor supply is solely a function of \( \phi_j \) and \( \frac{dw_{ij}}{dlog(q_{ij})} = \frac{d\phi_j}{dlog(q_{ij})} \). But by the steady-state assumption, \( \frac{d\phi_j}{dlog(q_{ij})} = \frac{1}{\gamma(\phi_j) - \eta(\phi_j)} \), where \( \gamma(\phi_j) = \frac{1}{E[R_{ij}]} \frac{dE[R_{ij}]}{d\phi_j} \) and \( \eta(\phi_j) = \frac{1}{E[s_{ij}]} \frac{ds_{ij}}{d\phi_j} \) are the recruitment and separation elasticities, respectively. The labor supply elasticity facing the firm is given by \( \epsilon(\phi_j) = \gamma(\phi_j) - \eta(\phi_j) \). Further, if both \( \eta \) and \( \gamma \) are constant, as Manning (2003) imposes in his empirical implementation along with most subsequent work in this sub-literature, then it is easy to see
that $-\eta = \gamma$ and so we have $\epsilon = -2\eta$, which ties the separations elasticity to half the labor supply elasticity. Even when the separations elasticity is not constant but the recruitment elasticity is, the recruitment elasticity is a simple weighted average of the separations elasticities for each firm: $\gamma = \sum_j \omega_j \eta_j$ where $\omega_j = \frac{s_j N_j}{\sum_j s_j N_j}$ is the share of all separations from firm $j$. More generally, even when both the recruit and separation elasticities are heterogeneous, the recruit-weighted quit elasticity is equal to the recruit-weighted recruit elasticity, $\sum_j R_j \epsilon^Q_j = \sum_j R_j \epsilon^R_j$.

By imposing firm-specific elasticities that are common to all workers and having output $y_{ij} = A_i p_j$ we are ruling out complementarity in log productivity and heterogeneous firm labor supply curves across workers within a firm. Both of these would generate worker-firm specific wages, violating the AKM decomposition of wages. Complementarity in log productivity and heterogeneous labor supply elasticities would imply that log wages $w_{ij} = y_{ij} + \beta_{ij}$ where $y_{ij}$ is match-specific productivity and $\beta_{ij}$ is a match-specific markdown (for example due to firm-specific wage discrimination policies, as in [56]). The AKM decomposition would not be identified when pooled across types of workers; and even if attention were limited to exogenous firm switches, it could be a poor fit; and even if firm-effects were estimated, the probability $q_{ij}$ would depend on (all of) $w_{ij}$, not just the $\phi_j$ component. But a fact that we will use below

\[ \sum_j R_j \gamma(\phi_j) = -\sum_j s_{ij} \eta(\phi_j) \] and total recruits must equal total separations.

\[ \sum_j R_j \epsilon_j^Q = \sum_j R_j \epsilon_j^R \]

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

8 Assume $-dP_r(j|j, w) = \frac{d\sum_{k \neq j} P_r(k|j, w)}{dw_j} = \frac{d\sum_{k \neq j} P_r(j|k, w) P_r(k, w)}{Pr(j|j, w) Pr(j, w)}$, which comes from Bayes rule, and is satisfied by $Pr(k|j, w) = \phi(w_k/w_j)$ (as in [56]) and even more specifically by $P(k|j, w) = \frac{\eta^j}{1 + \eta^j}$, then we get:

\[ \sum_j R_j \epsilon^Q_j = \sum_j \frac{R_j}{1 - Pr(j|j)} \frac{-dPr(j|j, w)}{dw_j} w_j = \sum_j \frac{Pr(j) - Pr(j|j, w) Pr(j)}{1 - Pr(j|j, w)} \frac{-dPr(j|j, w)}{dw_j} w_j = \sum_j \frac{dR_j}{dw_j} R_j w_j = \sum_j R_j \epsilon^R_j \]
(in Section 5) 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 $\bar{w}_j - \bar{w}_{j'}$. Therefore, our general framework allows for a firm-component of wage that may be heterogeneous across worker types, and allows the labor supply elasticity to be heterogeneous as well.

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

$$E[s_{ijt}|w_{ijt}] = E[s_{ijt}|\phi_{j(i,t)}] = \eta(\phi_{j(i,t)})$$ (1.1)

Where $s_{ijt}$ takes on the value of 1 when worker $i$ leaves firm $j$ at time $t$. We can recover an estimate of the elasticity from the slope of this curve via $\hat{\eta} = \frac{\eta'(\phi_{j(i,t)})}{E[s_{j}]}$. However, if we simply use $w_{ijt}$ as the key independent variable, instead of isolating the firm-specific component, then our estimated $\tilde{\eta}$ will generally be attenuated due to measurement error. For example, if equation (1.1) were identified under an AKM-based strategy (the approach taken in section 3.1 below), then $\tilde{\eta} = \sigma \eta$ where $\sigma = \frac{\text{var}(\phi_{j(i,t)})}{\text{var}(w_{ijt})}$ is the share of the variation in wages that is due to firm effects. It is not clear why we would expect a worker’s separation probability to another firm to be higher if $\alpha_i$ is lower—after all it is the component of a worker’s wage that is invariant to the firm. We would expect the separation to be higher if it is a “bad job” (i.e., $\phi_j$ is lower) because in this case there is a greater chance of the worker receiving offers that dominate current employment. In our data, firm effects explain roughly 14 percent of the hourly wage variation (see section 4.1). This suggests that the
standard approach may recover an estimate that is roughly one-seventh as large, and so the use of individual level wages can significantly overstate the extent of monopsony power. In practice, if \( \text{Cov}(\alpha_i, \phi_j(i)) \neq 0 \), 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 (1.1) using AKM firm effects is unlikely to be valid as firms with high \( \phi_j \) may be attracting very different types of workers.

1.3.1 Approach based on AKM

The previous section establishes the importance of focusing on the firm-specific component of wage variation when estimating the degree of monopsony power in the market. What is the best way to accomplish this? One approach builds on AKM and [57]. We begin with the Card-Heining-Kline (henceforth CHK) assumption necessary to identify the coefficients \( \phi_j \) in the wage regression specification given by

\[
    w_{ijt} = \sum_j \phi_j f_{ijt} + \alpha_i + \alpha_t + \epsilon_{ijt} \tag{1.2}
\]

Where \( f_{ijt} \) is an indicator variable denoting whether worker \( i \) is employed at firm \( j \) at time \( t \), \( \alpha_i \) is a worker fixed effect, \( \alpha_t \) is a time fixed effect and \( \epsilon \) is an error term. CHK give a sufficient condition for identification:

\[
    f_{ijt} = E(J_{it} = j) = E(J_{it} = j|\epsilon) = G_{jt}(\phi_1, \ldots, \phi_J, \alpha_i) \tag{1.3}
\]

Equation (1.3) says that the probability of a worker being employed by a particular firm is a function of only the firm wage effects and the worker fixed effects. On its own, \( G \) does not impose severe economic restrictions on the assignment process between workers and firms, and is consistent with assignment rules that include both sorting

\[^9\]CHK also include an autocorrelation parameter in the error.
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 $\phi_j$, 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 $\phi_j$ and a “non-monopsony” component $h$, which includes effects of sorting and strategic-interactions effects that depend on the worker effect $\alpha_i$ and the other firms $\phi_k$. If the residual labor supply curve were the only constraint on the firm, and there was no sorting, equation 1.1 would obtain with a very strict monopsony-like structure on $G$ that is more than sufficient:

$$ Pr(f_{ijt}) = G_{jt}(\phi_1, ..., \phi_J, \alpha_i) = \epsilon(\phi_{j(i,t)}) = -2\eta(\phi_{j(i,t)}) $$ (1.4)

Under equation (1.4), we have the empirical elasticity given by

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

Note that any approach that regresses separations on firm effects must rule out pure sorting, i.e., $Cov(\alpha_i, \phi_j) > 0$, if we allow $\alpha_i$ to have an effect on firm assignment $f_{ijt}$. Sorting is allowed by equation 1.3 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 $\eta$ 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, $G_{jt}(\phi_1, ..., \phi_J, \alpha_i) = \epsilon(\phi_j, \{\phi'_j\}_{j\neq j} + h(\alpha_i, \{\phi'_j\}_{j\neq j})$; when we do this, we have an estimated elasticity given by $\hat{\eta} = \frac{1}{E[s_{ij}]} \frac{d s_{ij}}{d \phi_j} = \frac{1}{E[s_{ij}]} \int \eta_{\phi_j}(\phi_j, \{\phi'_j\}_{j\neq j}) dH(\{\phi_k\})$, where $H$ is the distribution of
the firm wage effects, i.e., heterogeneity based on the wage policies of other employers. Note that $\epsilon$ or $\eta$ cannot depend on the individual worker wage effects in our framework above, because this would induce worker specific markdowns within a firm and violate the additive separability of wages in AKM.

What we cannot admit is a function of the form $G_{jt}(\{\phi'_j\}, \alpha_i) = \epsilon(\alpha_i, \{\phi'_j\}_{j\neq j}, \phi_j) + h(\alpha_i, \phi_j, \{\phi'_j\}_{j\neq j}, \phi_j)$; if $h_{\phi_j} \neq 0$, then regressing $s_j$ on $\phi_j$ does not produce a consistent causal estimate of $\eta$ because $\phi_j$ also affects separations via $h$. For example, $h$ could capture the sorting: the fact that certain workers may be both high $\alpha$ type and sort into firms with higher $\phi_j$ and be less likely to separate is an example of this bias as in [152]. 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 $\phi_j$ does not recover the causal effect of $\phi_j$ on $f_{jt}$ if there is sorting of workers that induces a correlation between separations and firm effects that does not operate through the labor supply elasticity.

1.3.2 Extension to Include Unemployment

While the approach presented above relies solely on steady states and constant elasticities, it does not apply exactly in the presence of recruits from non-employment. The method implemented by [130] augments the separation and recruitment functions above to incorporate unemployment. One equation governs the separation rate from firms that pay $w$ into either unemployment ($EU$) or other employers ($EE$):

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

The second equation governs the recruitment rate into firms paying $w$, and similarly, recruits are given by
\[ R(w) = R^{UE}(w) + R^{EE}(w) \]

Manning then breaks these equations up into recruitment from and separations into employment and non-employment, exploiting the fact that recruits from employment into a firm must, on average, equal job-to-job transitions out of a firm in steady state. If the recruitment and separation elasticities are constant, then the steady-state assumption implies that the negative of the separations elasticity, \( \eta^{EE} \), is equal to the recruitment elasticity from employment \( \gamma^{EE} \), and we get

\[ \epsilon = -(\theta_R + \theta_S)\eta^{EE} - (1 - \theta_S)\eta^{EU} + (1 - \theta_R)\gamma^{UE} \]

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

### 1.3.3 Estimation

One additional challenge in implementing the above approach is that the AKM effects are estimated, leading to the usual generated regressor problem. We address this using sample splitting, in which we randomly split the workers (in each 6-year period)
into two groups, A and B, stratified on moving. (The sample-splitting approach was also used by [92].) Using these two samples, we generate two sets of AKM firm effects, \( \hat{\phi}_j^A \) and \( \hat{\phi}_j^B \). Next, we take the individuals in sample A and regress \( s_{ijt} \) on \( \hat{\phi}_j^A \) while instrumenting the latter with \( \hat{\phi}_j^B \). This ensures that a worker’s separation indicator is not entering into both the right and the left side of the equation, thus eliminating any mechanical correlation induced by an individual’s separation influencing the estimate of \( \hat{\phi}_j \). In addition, because the \( \hat{\phi}_j^A \) and \( \hat{\phi}_j^B \) are from separate samples, assuming that the estimation errors are uncorrelated, we can use the latter to instrument the former to alleviate the attenuation bias stemming from a generated regressor.

After decomposing wages, we estimate the following equation:

\[
s_{ijt} = \sum_j \eta \hat{\phi}_j f_{jt}^i + X_{it} \Gamma + \nu_{ijt} \tag{1.7}
\]

We calculate the firm effects using the AKM approach, by 6-year periods. The details of implementation, including assessment of limited mobility bias, are provided in Appendix A.3. After estimating the AKM model, we decompose the variance of the wage in the worker and firm effects, as in CHK and [15]. For all reported estimates of the separations and labor supply elasticities (excepted where noted), we exclude public administration and trim the top 2.5% and bottom 2.5% of the firm effects distribution. However, as we discuss below, the core elasticity estimates are not substantially affected by the trimming.

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\(^{10}\)Sample splitting means that the connected sets used to estimate \( \hat{\phi}_j \) vary in samples A and B. However, in practice, there is a very high degree of overlap in the connected sets: 99.9% of firms in the pooled connected set are also in the A-connected set; and 99.8% of them are in B-connected set. (Moreover, the correlation coefficient between \( \hat{\phi}_j^A \) and \( \hat{\phi}_j^B \) is 0.965.)
1.4 Results From AKM-based Model

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

During the 2000-2017 period, the variance in log hourly wages in our Oregon estimation sample was mostly stable. A similar pattern is observed when we consider hourly or quarterly earnings, and when we consider the full sample of workers or our main estimation sample (restricting by firm size and earnings, as described in the data section). However, the variance of log wages masks considerable heterogeneity in trends by wage percentile, as shown in Appendix Figure A.10. During this period, the largest growth in hourly wages occurred at the top (e.g., 90th and 95th percentiles), while the real wage fell in net in the middle (50th percentile). However, during the same period, wages rose faster at the bottom (5th and 10th percentiles); in part, this was likely due to Oregon’s minimum wage policies. In sum, hourly wage inequality grew in the upper half of the distribution, mirroring other states (e.g., [121]), even while it fell in the bottom half. The patterns are qualitatively similar if we instead consider quarterly earnings; however, the 90-50 gap in earnings grew somewhat more than the equivalent gap in hourly wages over this period.

Appendix Table A.5 provides the AKM decomposition in wage and earnings inequality for 6-year blocks between 2000-2017, as well as for the full panel. For both log quarterly earnings and log hourly wages, there is a slight increase in the overall variance between the 2000-2005 and 2012-2017 periods (0.37 to 0.41 for wages, and 0.59 to 0.64 for earnings). In the full panel, firm effects explain around 19% (14%) of the variance of quarterly earnings (hourly wages), and worker effects explain around 48% (55%) of the variance. This is similar to the findings of [121] using hourly wage data from the state of Washington; they estimate the firm effects’ share of variance to be 19% and 12% of log earnings and log wages, respectively. There is also assortative matching of workers and firms, with the covariance term explaining around 14%
(18%) of the variance. Consistent with other work (e.g., [155]), we see a clear increase in the covariance term for both wages and earnings over this period consistent with greater sorting: for quarterly earnings (hourly wages), the contribution of the covariance term rises from 11% (14%) in 2000-2005 period to 14% (17%) in the 2012-2017 period. At the same time, there is a slight increase in the firm component of quarterly earnings variance, but a small decrease in the case of hourly wages. Broadly, again, these trends are similar to the findings of [121] using hourly wage data from Washington. We discuss further details of the AKM estimation in Appendix A.3, including an evaluation of limited mobility bias, which we conclude is not a major concern in our context given our relatively long (6-year) and higher frequency sample.

### 1.4.2 AKM-based separations elasticities

Figure 1.1 replicates the event study Figure illustrating interquartile transitions in [57] and shows largely parallel trends prior to a transition, similar to [57]. In Appendix Figure A.1 we augment this picture with size of flows, showing that the separation rates of firms in these quartiles behave as expected, where separations from low-wage firms to high-wage firms are more frequent than separations from high-wage firms to low-wage firms, even though the wage changes are symmetric (see Figure A.2).

Figure 1.2 presents the key findings of this section. Using a control function approach, the binned scatter plot shows the overall separations rate (divided by the average separations rate) against the AKM firm fixed effects in hourly wages, controlling for the first stage residuals (where AKM firm effects using one sample are instrumented by the firm effects estimated using the other sample). The AKM model is estimated using stacked 6-year 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
Figure 1.1: Changes in hourly wages across job separations for firm quartile-to-quartile transitions

Note: The legend indicates origin quartile to destination quartile, where quartiles are defined along the distribution of the average firm wage, using only workers who stay at the firm over the 6-year period. The change in wage is shown for movers, who are defined as workers who make a between-firm job-to-job transition at any point during the period and are observed for at least 9 consecutive quarters at the each firm before and after the move. The quarter of separation and the following quarter are omitted. This exercise is repeated for each 6-year period (2000-2005, 2006-2011 and 2012-2017), the mover wage profiles are stacked, and the averages of the event quarter are plotted by quartile-transition categories.
Figure 1.2: **Separations and firm wage effects**

Note: The figure illustrates the split-sample approach using a control function. Residuals are calculated from a regression of own-sample firm effects on the complement-sample firm effects, and used as a control in a regression of separations on own-sample firm effects. The plotted points show the binned scatter points of this latter regression (i.e., depicting the partial correlation). The vertical axis is separations divided by mean separations such that the slope of the line represents the elasticity. The blue points represent quantiles of the trimmed sample, which excludes the top and bottom 2.5 percent of the firm effects distribution. The red points represent quantiles of the excluded sample only, which we consider outliers. The trendline is a cubic polynomial fitted to the trimmed sample.

Table 1.1 shows the results of our regressions using a variety of outcome variables. All regressions are run at the individual worker level, clustered by firm and control for quarterly fixed effects. We report estimates using any separation as an outcome variable, as well as employment-to-employment separations (E-E), employment to non-employment separations (E-N), and employment-to-employment recruits (E-E recruits, which are restricted to observations corresponding to hires only). We then
present the share of recruits from employment, and calculate labor supply elasticities based on equation [1.6] 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 $\epsilon$ 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 $\epsilon$ 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 [154].

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 $\epsilon$ are around 2.69. Column 5 (preferred AKM-based specification) uses sample splitting to instrument the firm fixed effect in order to correct for attenuation bias of a generated regressor. Doing so increases the magnitude of the separations elasticity modestly to -1.448 and the labor supply elasticity to 2.912. Importantly, accounting for recruits from non-employment in calculating the elasticity does little to the estimates in columns 4 and 5; instead, had we simply used the rule of multiplying the separations elasticity by -2, we would have obtained labor supply elasticities that are nearly identical.
Table 1.1: **Separations and recruits elasticities to firm component of wage using AKM**

<table>
<thead>
<tr>
<th></th>
<th>Wage</th>
<th>Firm FE</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>All separations</td>
<td>-0.282</td>
<td>-0.51</td>
</tr>
<tr>
<td></td>
<td>(0.005)</td>
<td>(0.01)</td>
</tr>
<tr>
<td>E-E separations</td>
<td>-0.317</td>
<td>-0.533</td>
</tr>
<tr>
<td></td>
<td>(0.007)</td>
<td>(0.013)</td>
</tr>
<tr>
<td>E-N separations</td>
<td>-0.291</td>
<td>-0.422</td>
</tr>
<tr>
<td></td>
<td>(0.005)</td>
<td>(0.01)</td>
</tr>
<tr>
<td>E-E recruits</td>
<td>0.266</td>
<td>0.127</td>
</tr>
<tr>
<td></td>
<td>(0.022)</td>
<td>(0.031)</td>
</tr>
<tr>
<td>Pct. EE-recruits</td>
<td>0.47</td>
<td>0.47</td>
</tr>
<tr>
<td>Labor Supply Elasticity</td>
<td>0.355</td>
<td>0.879</td>
</tr>
<tr>
<td></td>
<td>(0.024)</td>
<td>(0.037)</td>
</tr>
<tr>
<td>Obs (millions)</td>
<td>7.348</td>
<td>7.348</td>
</tr>
<tr>
<td>Log hourly wage</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>Hazard spec.</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>Firm FE</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Split-sample</td>
<td></td>
<td></td>
</tr>
<tr>
<td>F-stat</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

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

Table 1.2 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 separations elasticity (-0.776 (-0.809) instead of -1.448 in column 5 of Table 1.1); this highlights the importance of using hourly wage data. In contrast,
the separations elasticity estimates are fairly robust to other changes we consider. Without trimming the firm effects distribution, the separations elasticity is -1.262. Controlling for tenure changes the separations elasticity to -1.228. Including controls for industry (1-digit level) by county fixed effects results in a labor supply elasticity of -1.336; controlling for industry and tenure produces an estimate of -1.406. (We recognize that controlling for past tenure when estimating the separation response is problematic, as it is related to the outcome; we are able to do this much more carefully in our worker-level matched-event study design.)
Table 1.2: Alternative specifications for separations and recruit elasticities to firm component of wage using AKM

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
<th>(7)</th>
<th>(8)</th>
</tr>
</thead>
<tbody>
<tr>
<td>All separations</td>
<td>-0.878</td>
<td>-0.936</td>
<td>-0.776</td>
<td>-0.809</td>
<td>-1.262</td>
<td>-1.228</td>
<td>-1.336</td>
<td>-1.406</td>
</tr>
<tr>
<td></td>
<td>(0.066)</td>
<td>(0.071)</td>
<td>(0.033)</td>
<td>(0.039)</td>
<td>(0.075)</td>
<td>(0.065)</td>
<td>(0.055)</td>
<td>(0.063)</td>
</tr>
<tr>
<td>E-E separations</td>
<td>-0.866</td>
<td>-0.913</td>
<td>-0.946</td>
<td>-0.987</td>
<td>-1.607</td>
<td>-1.535</td>
<td>-1.545</td>
<td>-1.553</td>
</tr>
<tr>
<td></td>
<td>(0.057)</td>
<td>(0.061)</td>
<td>(0.053)</td>
<td>(0.065)</td>
<td>(0.115)</td>
<td>(0.109)</td>
<td>(0.08)</td>
<td>(0.102)</td>
</tr>
<tr>
<td>N-E separations</td>
<td>-0.709</td>
<td>-0.752</td>
<td>-0.857</td>
<td>-0.739</td>
<td>-1.115</td>
<td>-1.161</td>
<td>-1.191</td>
<td>-1.293</td>
</tr>
<tr>
<td></td>
<td>(0.054)</td>
<td>(0.058)</td>
<td>(0.033)</td>
<td>(0.034)</td>
<td>(0.066)</td>
<td>(0.053)</td>
<td>(0.05)</td>
<td>(0.048)</td>
</tr>
<tr>
<td>E-E recruits</td>
<td>0.783</td>
<td>0.832</td>
<td>0.493</td>
<td>0.349</td>
<td>0.354</td>
<td>0.442</td>
<td>0.323</td>
<td>0.338</td>
</tr>
<tr>
<td></td>
<td>(0.112)</td>
<td>(0.121)</td>
<td>(0.042)</td>
<td>(0.045)</td>
<td>(0.071)</td>
<td>(0.064)</td>
<td>(0.064)</td>
<td>(0.075)</td>
</tr>
<tr>
<td>Pct. EE-recruits</td>
<td>0.464</td>
<td>0.465</td>
<td>0.43</td>
<td>0.467</td>
<td>0.463</td>
<td>0.465</td>
<td>0.466</td>
<td>0.465</td>
</tr>
<tr>
<td>Labor Supply Elasticity</td>
<td>0.865</td>
<td>0.908</td>
<td>1.348</td>
<td>1.493</td>
<td>2.597</td>
<td>2.429</td>
<td>2.578</td>
<td>2.629</td>
</tr>
<tr>
<td></td>
<td>(0.143)</td>
<td>(0.154)</td>
<td>(0.089)</td>
<td>(0.107)</td>
<td>(0.186)</td>
<td>(0.174)</td>
<td>(0.136)</td>
<td>(0.169)</td>
</tr>
<tr>
<td>Obs (millions)</td>
<td>7.348</td>
<td>7.304</td>
<td>16.45</td>
<td>77.767</td>
<td>70.609</td>
<td>51.92</td>
<td>41.796</td>
<td>51.629</td>
</tr>
<tr>
<td>Firm FE</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td></td>
</tr>
<tr>
<td>Split-Sample</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td></td>
</tr>
<tr>
<td>F-stat</td>
<td>4586</td>
<td>12043</td>
<td>8637</td>
<td>9820</td>
<td>11015</td>
<td>9266</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Hazard spec.</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Annual earnings</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Quarterly earnings</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td></td>
<td></td>
</tr>
<tr>
<td>No trimming</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Note: The first stage F-stat is given for the row 1 regression. The unit of observation for hazard specifications is an employment spell, and for the linear specifications, it is each job-quarter record. Column 2 uses the split sample in a control function for the hazard specification. Annual earnings indicates the annualized panel (one observation per worker-year), from which the AKM firm FEs (using log annual earnings) and separations variables are estimated. Quarterly earnings indicates AKM firm FEs estimated with quarterly earnings. Elasticities are reported in each cell for the linear specifications, by dividing the regression coefficient by the corresponding sample mean of the outcome. Tenure refers to the number of quarters since the job started, is coded as a continuous variable and includes control terms up to a quadratic power of tenure. Industry is defined at the 1-digit level. Firm fixed effects are censored at the 2.5 percent tails of the firm FE distribution, except where 'No trimming' is indicated. Standard errors are shown in parentheses are clustered at the firm level.
1.4.3 Testing the Assumptions of the AKM-based approach

There are two core assumptions at the heart of our approach. The first is that AKM is identified, that is, equation 1.2 and assumption 1.3 hold. The second is that equation 1.4 holds, so the co-variation between separations and firm effects is driven by movements along the (possibly heterogeneous) residual labor supply curve, not other omitted variables (e.g., sorting) that are correlated with firm wages and separations. Let us examine these assumptions in turn.

The first assumption is that there are no other omitted variables contaminating the relationship between $s_{ij}$ and $\phi_j$. As discussed above, controlling for the worker wage effect $\alpha_i$ should not affect the estimate of $\eta$; 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, non-causal sorting of workers can present an important problem for using the relationship between AKM firm effects and separations. For example, if high-wage workers sort to high-wage firms (as is the case empirically), and high-wage workers have different exogenous (to wage) separation rates, it is difficult to separate the firm-versus-worker component of separations. Moreover, there may be other systematic differences in exogenous separations at high- versus low-wage firms: for example, if workers at higher wage firms tend to be more connected (and hence have greater rates of separations) this could confound the relationship between the firm effect and separation rates. As a test for these concerns, we consider how separations respond to various components of the wage effects (i.e., worker, firm, average match residuals) in Appendix Table A.1 In column 1, we reproduce the baseline OLS estimates from column 4 of Table 1.1 In supercolumn 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

---

11 This allows for more comparability between AKM components than the preferred split-sample specification.
worker fixed effects greatly reduces the magnitude of the firm effects coefficient (from -1.3 to -0.7). This highlights the challenge that the sorting of high-wage workers to high-wage firms presents for the ecological regression. Moreover, it’s not clear that inclusion of the worker fixed effect actually reduces bias. When there are multiple dimensions of heterogeneity in exogenous separations, controlling for one dimension may even increase overall bias. For example, if high wage firms attract both higher skilled workers (with lower exogenous separations) and more connected workers (with higher exogenous separations), simply controlling for the AKM worker fixed effect would tend to exacerbate the bias from the other omitted variable (connectedness). Overall, then, the sensitivity of the separations elasticity to the inclusion of worker fixed effects (in wages) makes it difficult to assess the causal import of the AKM-based findings.

A second issue arises from whether the AKM assumption about mobility does, indeed, hold in our data. An important assumption shared by both our model and the AKM framework generally is that match-specific wage effects are irrelevant for firm assignment. If we denote by $\mu_{ij}$ 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, $\mu_{ij}$. If it were, then the firm indicator would be correlated with match effects in the residual. More formally, suppose $f^j_{jt} = G_{jt}(\{\phi_j\}, \alpha_i, \mu_{ij})$. It follows that estimates of firm effects from $w_{ijt} = \sum_j \phi_j f^j_{jt} + \alpha_i + \epsilon_{ijt}$ will be biased because $\text{Cov}(f_{jt}, \mu_{ij}) \neq 0$ and $\mu_{ij}$ is a component of $\epsilon_{ijt}$.

CHK provide several types of evidence against the importance of match effects. First they show that unrestricted match effects model—i.e., a separate $\mu_{ij}$ for every pair, instead of firm effects $\phi_j$—does not improve the share of explained wages very much. We also find something similar: the adjusted R-square in the unrestricted match effects model in our sample from 2000-2017 (2012-2017) is 0.88 (0.91) while the AKM model adjusted R-square is 0.84 (0.90). Second, they argue that the wage losses
and gains going from lower to higher firm effect quartiles and vice versa are symmetric, and that in general there is little in the way of wage gains when moving within firm effect quartiles. If mobility were driven by match effects, we would not expect the symmetry to necessarily hold. We also provide evidence that wage changes from upward and downward movements between quartiles are symmetric (see Appendix Figure A.2).

However, the fact that the $\mu_{ij}$ 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 $\mu_{ij}$ as $\hat{\mu}_{ij} = \frac{1}{T_{ij} - t_{ij}} \sum_{r=t_{ij}}^{T_{ij}} w_{ijr} - \hat{\alpha}_i - \hat{\phi}_j$, which is the mean residual of the wage over a job spell, conditional on worker and firm effects, and check if the firm effect of the subsequent firm $\phi_{j(i,t+1)}$ is correlated with $\hat{\mu}_{ij}$. If these are indeed random effects (as assumed under AKM), they should not predict the direction of future flows. In Table 1.3, we consider two tests. In columns 1 to 4, the outcome is the subsequent firm’s fixed effect at date $t + 1$, which we regress on the “match effect” (mean residuals) and the firm effect at date $t$. Without any controls in column 1, we find that match effects are indeed predictive of future firm effects, in violation of AKM assumptions. Including controls for industry and tenure at date $t$ in column 4 renders the coefficient small and insignificant. In columns 5 to 8 we consider the direction of change in the firm effect between dates $t$ and $t + 1$. Here too, we find that high match effects (mean residual wage) positively predicts the direction of change in firm effects upon separation; moreover, while inclusion of industry by tenure controls reduces the magnitude of the coefficient, it continues to be statistically significant.
Table 1.3: **Falsification test: Do match residuals predict future AKM firm quality of movers?**

<table>
<thead>
<tr>
<th></th>
<th>Future Firm FE</th>
<th>Positive change in Firm FE</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>Match effect</td>
<td>0.058</td>
<td>0.058</td>
</tr>
<tr>
<td></td>
<td>(0.003)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>Firm effect</td>
<td>0.513</td>
<td>0.430</td>
</tr>
<tr>
<td></td>
<td>(0.011)</td>
<td>(0.011)</td>
</tr>
<tr>
<td>Obs</td>
<td>1625209</td>
<td>1497149</td>
</tr>
</tbody>
</table>

**Controls**

<table>
<thead>
<tr>
<th></th>
<th>Y</th>
<th>Y</th>
</tr>
</thead>
<tbody>
<tr>
<td>Industry × county</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Tenure</td>
<td>Y</td>
<td></td>
</tr>
<tr>
<td>Industry × tenure</td>
<td>Y</td>
<td></td>
</tr>
</tbody>
</table>

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

Overall, these findings suggest that the assumption for identification of AKM may not hold in our sample. While the quantitative importance of $\mu_{ij}$ may be unimportant for explaining wage variation, as discussed above, it may be important for estimating separation elasticities. To clarify, the failure of AKM and the possibility of omitted variables in the separations regression need not imply that that AKM-based separations elasticities are severely biased: indeed, they may be approximately correct. However, these failures do 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 (e.g. 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.

1.5 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 1.3, suppose assignment at time $t$ is governed by the following equation:

$$ f_{ijt} = G_{jt}(\{\bar{w}_k\}, \{w_{ir}, f_{ikr}\}_{r<t}) $$

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 $\phi_j$; 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 1.8 imposes that worker fixed effects (as well as match effects) do not matter conditional on controls for history. Unlike CHK, this assumption is non-Markovian, and allows for path-dependence, where a worker’s past employers, employment history, and past wages, influence their probability of matching with a firm $j$.

This implies $E[f_{ijt}^t \epsilon_{ijt}] = 0$ where $\epsilon$ is from the dynamic equation below:
\[ w_{ijt} = \sum_j \phi_j \bar{w}_j f_{ijt} + L(\{w_{ir}, f_{ikr}\}_{r<t}) + \epsilon_{ijt} \] (1.9)

Note that, since the history includes lagged wages and fixed effects for lagged firms, focusing on the time of transition \( t \), equation 1.9 can be rewritten as

\[ w_{ijt} - w_{ijt-1} = \bar{\phi}(\bar{w}_j - \bar{w}_{j'}) (f_{jt} - f_{j't-1}) + L(history_{i,t}) + \nu_{ijt} \] (1.10)

which is similar to the specification estimated by [83], but augmented with controls; Finkelstein et al. show that under the AKM assumptions, the coefficient on the change in log average wage can be interpreted as \( \bar{\phi} = \frac{\phi_j - \phi_{j'}}{(\bar{w}_j - \bar{w}_{j'})} \), which is the share of the mean difference in log wages across firms within a quarter explained by firm effects. However, we do not have to impose this interpretation on the coefficient \( \bar{\phi} \) in this specification and can still use equation 1.10 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 \( \phi_j \) 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:

\[ s_{it+k} = \eta \Delta w_{ijt} + L(history_{i,t}) + \epsilon_{ijt+k} \] (1.11)

with the first-stage given by equation (1.10). 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, \( \Delta w_{ijt} \), with the change in the mean wage of the firm, \( \Delta \bar{w}_j \). The experiment captured by this specification is
that we compare two workers with the same past wage and employment history, both starting at the same “origin” firm \( j' \) and look at the wage change each worker receives from transitioning to a high-mean-wage versus a low-mean-wage “intermediate” firm \( j \); we also look at how long they stay at this intermediate firm before separating again to a final firm or to non-employment.

The advantage of this approach over the AKM-based approach in the previous section is that the controls \( L(history_i) \) effectively remove the bias due to worker-specific separation propensities correlated with firm wages that are not due to the elasticity of labor supply facing the firm. These histories are, we would argue, much richer controls than simply the worker wage effect \( \alpha_i \), and we test this below. Additionally, note that this formulation allows the separations elasticity \( \eta \) to be heterogeneous across workers (unlike in the AKM based approach), which means the estimate from equation (1.11) 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 \( \alpha_i \). However, a sufficiently rich set of both of lagged wages and past employment history should control for much of the heterogeneity in wages captured by \( \alpha_i \). In addition, we could in principle estimate a specification that is identified under strictly stronger assumptions than AKM, where assignment is given by \( f_{jt} = G_{jt}(\{\phi_k\}, \alpha_i, \{w_{is}, f_{is}\}_{s<t}) \) and wages are given by

\[
\begin{align*}
  w_{ijt} &= \sum_j \phi_j f_{jt} + \alpha_i + L(history_i) + \epsilon_{ijt}
\end{align*}
\]

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 [60].

31
1.5.1 Estimation

We implement this approach using a stacked event study design. We stack all observations by the date of initial transition \((t)\) when a worker \(i\) transitions from an initial firm, called Origin \(O(i)\) to another firm, called Intermediate \(I(i)\). We then estimate the worker’s subsequent probability of “re-separating” from \(I(i)\) to another firm \(F(i)\) (or to non-employment) over the next \(k\) quarters (we take \(k = 16\) to allow for a sufficiently long post-transition period). We take the transitioning worker’s history (fully saturated interactions of indicator for the Origin firm, octiles of initial wages at \(O(i)\) firm, octiles of \(O(i)\) firm tenure, calendar quarter of transition to \(I(i)\) from \(O(i)\) denoted as \(d\)) fully interacted with with event time, \(t\). (This means we are comparing workers with nearly identical wage and employment trajectories at the same Origin firm, and who transitioned to the Intermediate firm on the same date.)

Noting that separation \(s_{i,t+k}^I\) at date \(t + k\) is defined only for workers who had been working at the \(I(i)\) firm through \(t + k\), we regress

\[
s_{i,t+k}^I = \eta(w_{i,I(i),t} - w_{i,O(i),t-1}) + L(History_{i,t,d}) \times 1_{t+k} + \epsilon_{i,t+k} \tag{1.12}
\]

Note that \(L\) contains a fixed effect for \(O(i)\), and includes wages at \(O(i)\), so all the variation that identifies \(\delta\) comes from \(w_{i,I(i),t}^{12}\). To isolate the variation in \(w_{i,I(i),t}\) that is due to firm wage policies, we use a first stage equation given by

\[
w_{i,I(i),t} - w_{i,O(i),t-1} = \phi(\bar{w}_{i,I(i),t} - \bar{w}_{i,O(i),t-1})(f_{I(i),t} - f_{O(i),t-1}) + L(History_{i,t,d}) + \epsilon_{i,t} \tag{1.13}
\]

with a corresponding reduced form given by

\[\]

\[12\]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 \(w_{i,O(i),t-1}\).
\[ s_{i,t+k}^I = \delta(\bar{w}_{i,I(i),t} - \bar{w}_{i,O(i),t-1}) + L(History_{i,t,d}) \times 1_{t+k} + \epsilon_{i,t+k} \]  

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

We report the first stage coefficient \( \phi \) and the separations elasticities below, where the separations elasticity is estimated as \( \hat{\eta} = \frac{\delta}{\phi^2} \).

### 1.5.2 Results

In Table 1.4, 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 [83] approach (and 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 Appendix Table A.5. The separations elasticity of -0.76 is smaller than what we found in the AKM-based approach (-1.448 in column 5 of Table 1.1). However, once we control for the identity of the \( O - \) firm in column 2, we find a much larger separations elasticity (-2.475). This highlights the likely importance of heterogeneity of workers moving to high- versus low-wage firms; in particular, past firm assignment (i.e, \( O(i) \) fixed effect) seems to encode substantial information about exogenous separation rates that vary across firms with high versus low average wages.
Table 1.4: *Separations elasticities based on matched event study*

<table>
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<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
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<th>(6)</th>
<th>(7)</th>
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<td><strong>First stage</strong></td>
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<td>0.148</td>
<td>0.148</td>
<td>0.176</td>
<td>0.173</td>
<td>0.070</td>
<td>0.165</td>
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<td></td>
<td>(0.006)</td>
<td>(0.001)</td>
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<td>(0.004)</td>
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<td>(0.009)</td>
<td>(0.007)</td>
<td>(0.006)</td>
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<td><strong>IV estimates</strong></td>
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<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
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<td></td>
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<tr>
<td>Separations</td>
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<td>-2.431</td>
<td>-2.475</td>
<td>-2.100</td>
<td>-2.014</td>
<td>-1.293</td>
<td>-2.084</td>
<td>-2.085</td>
<td>-2.163</td>
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<td>(0.051)</td>
<td>(0.033)</td>
<td>(0.059)</td>
<td>(0.054)</td>
<td>(0.040)</td>
<td>(0.513)</td>
<td>(0.096)</td>
<td>(0.096)</td>
<td>(0.080)</td>
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<td>-4.000</td>
<td>-4.341</td>
<td>-4.031</td>
<td>-3.606</td>
<td>-1.754</td>
<td>-4.326</td>
<td>-4.379</td>
<td>-4.201</td>
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<td>(0.096)</td>
<td>(0.079)</td>
<td>(0.144)</td>
<td>(0.154)</td>
<td>(0.108)</td>
<td>(1.549)</td>
<td>(0.304)</td>
<td>(0.314)</td>
<td>(0.234)</td>
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<td>(0.057)</td>
<td>(0.041)</td>
<td>(0.072)</td>
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<td>(0.568)</td>
<td>(0.123)</td>
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<td>1844</td>
<td>1397</td>
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<td>46</td>
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<tr>
<td>O-I Firm-pair FE</td>
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**Note:** See text for sample construction. The full instrumental variables specification is provided in equations (12) and (13) in the main text. The outcomes $s_{i,t+k}^I$ indicate separation, E-E separation and E-N separation such that $s$ is missing for all periods after a single re-separation (and E-N re-separating workers are missing for the E-E separation outcome; similarly for the E-N separation outcome). Each of these regressions includes fixed effects as indicated, where ‘×’ indicates that fixed effects are interacted. Wage₀ indicates the wage at hire, and 3 qtr wage lags indicates 3 quarters of pre-separation wages (Origin firm). Fixed effects are divided into 8 equal bins, except where coarsened which indicates that 4 bins are used instead. O-I Firm-pair FE indicate fixed effects for every Origin-Intermediate firm pair. AKM Worker FE indicates a continuous control for the AKM worker fixed effect from the previous time period. The sample is restricted to the post-t period. Where indicated, the sample is additionally restricted for comparability to the estimable sample for the corresponding set of fixed effects. Change in own wage is trimmed at the 1% tails. All regressions are clustered at the level of origin firm by initial separation quarter. Only elasticities are reported by dividing regression coefficients by the average relevant sample re-separation rate.
Our preferred specification in column 4 additionally interacts the $O(i)$—firm fixed effect with 8 categories of starting wages and tenure at $O(i)$ firm, along with calendar quarter fixed effects; this saturated specification compares workers 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 co-workers). This is a rich set of controls, and we find that for this sample, a 10% difference in the $I(i)$ firm average wage leads to a difference in own wage of approximately 1.8%. The separations elasticity from our preferred specification is -2.1; using the 2-times-separations elasticity rule, this suggests a labor supply elasticity of around 4.2. Comparing this estimate to our preferred separations elasticity estimates from the AKM approach above, the estimates from the matched event study are somewhat larger in magnitude (-2.1 versus -1.4) but also more precise (standard error is 0.054 versus 0.095). Figure 1.3 shows the binned scatterplots of first stage and IV regressions that correspond to column 4 of Table 1.4, and it is clear there is little need to trim or account for outliers, and the data is much closer to the fitted line and appears close to constant elasticity except in the tails. Appendix Figure A.7 shows the analogous binscatter but for E-E separations. Column 5 coarsens these controls to 4 categories of starting wages and tenure at the origin firm; this makes little difference to our estimates.

---

13 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 Appendix Figure A.6 which shows that firm separations and firm recruits fall broadly along the 45 degree line.
Figure 1.3: Binned scatterplots of separation and firm-component of wages

(a) Change in log own wage on change in log firm wage (first stage)

(b) Probability of separation on change in own wage (with control function)

Notes: Panel (a) shows the first stage relationship between $\Delta \ln(wage_{i,t+1})$ and $\Delta \ln(\bar{w}_{i,I(i),t})$, where $\Delta \ln(\bar{w}_{i,I(i),t})$ is the change in average firm wage for individual $i$ at E-E separation date $t-1$ compared to the intermediate firm at date $t$, and $\Delta \ln(wage_{i,t+1})$ is $\ln(wage_{i,t+1}) - \ln(wage_{i,t-1})$. Panel (b) shows the relationship between separations and $\Delta \ln(wage_{i,t+1})$, instrumenting by $\Delta \ln(\bar{w}_{i,I(i),t})$ using a control function, i.e., controlling for the residuals from a regression of $\Delta \ln(wage_{i,t+1})$ on $\Delta \ln(\bar{w}_{i,I(i),t})$. Separation indicates the probability of separation from the intermediate firm. All specifications include fixed effects $L(\text{History}_{i,t,d})$ corresponding to interacted event and calendar time by origin firm by worker tenure at origin firm (8 bins) by initial wage at the origin firm (8 bins), and are clustered at the level of origin firm by time. The sample consists of the first 16 quarters after initial separation from the origin firm. See text for sample construction.
Column 6 adds the O-I firm-pair fixed effect as a control, and shows that it is the wage difference between two firms, not the specific transition, that drives the reseparation probability. This is a demanding specification that uses changes in firm average wages over time for identification. While the point estimate is smaller in magnitude (-1.293), and the standard errors are much larger (0.513), it’s worth noting that the lower bound of the separations elasticity 95% confidence interval of (-2.3) is similar to the lower bound in our preferred specification in column 4 (-2.2). In Column 8, we fully interact the controls, in addition to the preferred specification controls, with the ending wage at \(O\)–firm along with an additional 3 lags in wages (to capture wage dynamics), and find this has little impact on the separations elasticity (-2.085), which suggests our baseline controls are quite successful in finding otherwise similar workers who land at different \(I\)–firms. Column 7 shows that this is not simply due to sample changes induced by requiring such a rich set of covariates.

We next revisit the specification check we conducted in the previous AKM-based approach in Column 5. We determine whether adding worker 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 \(\eta\), thereby raising concerns about omitted variables in our simple regression of \(s_{it}\) on \(\phi_j\). 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 \(\alpha_i\) 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 A.1 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.
Figure 1.4: Event study of workers’ wages and separation behavior following movement to a higher wage firm

(a) Difference in own wage against difference in log firm average wage

(b) Retention rate against difference in log firm average wage

Notes: Panel (a) plots the first stage regression \( \beta \) coefficients from \( \Delta \ln(wage_{i,t+k}) = \beta_k \Delta \ln(\bar{w}_{i,I(i,t)}) + L(History_{i,t,d}) \times 1_{t+k} + \nu_{i,t+k} \), 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 \( R_{i,t+k} = \delta_t \Delta \ln(\bar{w}_{i,I(i,t)}) + L(History_{i,t,d}) \times 1_{t+k} + \epsilon_{i,t+k} \), 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 \( L(History_{i,t,d}) \times 1_{t+k} \) corresponding to interacted event and calendar time by origin firm by worker tenure at origin firm (8 bins) by initial wage at the origin firm (8 bins), and are clustered at the level of origin firm by time. Change in own wage is censored at the 1% tails. See text for sample construction.
The key findings are shown visually in Figure 1.4. 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 $\bar{w}_{I(i)_t} - \bar{w}_{O(i)_{t-1}}$, 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 1.2: fully interacted controls for $O(i)$ firm fixed effect, the starting wages of workers at $O(i)$ in 8 categories, their tenure in 8 categories, and the calendar quarter of transition from $O(i)$ to $I(i)$.

We find that wages of workers going to high- versus low-wage $I(i)$ firms followed parallel trends prior to the $O - I$ transition conditional on controls (recall that in this specification, we controlled for the starting wage at the $O(i)$ firm but not subsequent wages, so there is no mechanical reason for this to be true). At the same time, there is a clear jump in own wages of workers leaving the same $O(i)$ firm after date 0 when they move to a firm with a higher average wage $^{14}$ The coefficient of 0.18 at date $t$ means that, on average, if a worker moves to an $I$-firm with 10% higher average wage, the worker’s own wage increases by around 1.8%. Following $^{83}$, we can interpret this to mean that around 18% of the variation in overall wages are due to the firm component, though in our case these are conditional on controls for worker heterogeneity. The gains are persistent, as the first stage coefficient remains around 0.14, even 16 quarters following the $O - I$ transition.

How is separation behavior at the $I$-firm affected by wages there? Panel B shows this visually using the survival function, i.e., plotting the impact of having a higher firm-average wage $\bar{w}$ on $k$-period retention probability for $k \in \{1, 2, ..., 16\}$. We plot

$^{14}$As explained in Appendix A.2 which gives further details on sample construction, we set wages in the actual quarters of transition (dates -1 and 0) to missing as these hourly wage observations likely contain substantial measurement error associated with partly worked quarters.
the average retention probabilities of all workers in the sample in black, and the predicted retention probabilities for workers who are assigned to an $I(i)$ firm with one log point higher firm-average wage (in red). The gap in the retention probability between the red and black lines is thus the causal effect of being assigned to a firm with a log point higher firm-average wage; 4 quarters out, this gap in the separations probability is about -0.1. This gap in probability persists through the 16 quarters following the initial $O - I$ transition. Note that the figure traces out the impact of higher firm wages on the survival function $R_{t+k}(w)$. To relate this to our separation elasticities, note that the latter are based on the the impact of firm wages ($w$) on the hazard of separating at time period $k$, i.e., $\frac{\partial}{\partial w} (\ln(R_{t+k}(w)) - \ln(R_{t+k-1}(w)))$. Pooling the impact on the hazard in periods $k \in \{1, 2, ..., 16\}$ 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 $\phi_j$. Workers who separated for reasons unrelated to wage changes at $j$ (e.g. 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 e.g. match effects) it just makes our IV estimate a (weighted) LATE applicable only to compliers, but still unbiased.
1.5.3 Robustness and Heterogeneity

Table 1.5 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 [156]. 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 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 amenities values as measured by the Sorkin approach.
Table 1.5: **Alternative specifications for separations elasticities based on matched event study**

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
<th>(7)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>First stage</strong></td>
<td>0.176</td>
<td>0.171</td>
<td>0.177</td>
<td>0.324</td>
<td>0.149</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.004)</td>
<td>(0.004)</td>
<td>(0.004)</td>
<td>(0.004)</td>
<td>(0.022)</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>IV estimates</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Separations</td>
<td>-2.100</td>
<td>-1.961</td>
<td>-1.992</td>
<td>-2.027</td>
<td>-0.272</td>
<td>-1.536</td>
<td>-2.358</td>
</tr>
<tr>
<td></td>
<td>(0.054)</td>
<td>(0.057)</td>
<td>(0.054)</td>
<td>(0.072)</td>
<td>(0.012)</td>
<td>(0.037)</td>
<td>(0.584)</td>
</tr>
<tr>
<td></td>
<td>(0.154)</td>
<td>(0.161)</td>
<td>(0.153)</td>
<td>(0.210)</td>
<td>(0.026)</td>
<td>(0.115)</td>
<td>(1.167)</td>
</tr>
<tr>
<td>E-N Separations</td>
<td>-2.048</td>
<td>-1.958</td>
<td>-1.968</td>
<td>-3.178</td>
<td>-0.385</td>
<td>-1.489</td>
<td>-3.228</td>
</tr>
<tr>
<td></td>
<td>(0.071)</td>
<td>(0.076)</td>
<td>(0.072)</td>
<td>(0.152)</td>
<td>(0.026)</td>
<td>(0.049)</td>
<td>(1.035)</td>
</tr>
<tr>
<td>Obs (millions)</td>
<td>3.068</td>
<td>2.999</td>
<td>2.984</td>
<td>3.069</td>
<td>3.073</td>
<td>3.082</td>
<td>0.110</td>
</tr>
<tr>
<td>Movers</td>
<td>346261</td>
<td>340000</td>
<td>338562</td>
<td>346714</td>
<td>347193</td>
<td>346684</td>
<td>11102</td>
</tr>
<tr>
<td>Fstat (IV)</td>
<td>1397</td>
<td>1279</td>
<td>1345</td>
<td>196</td>
<td>.</td>
<td>4447</td>
<td>37</td>
</tr>
<tr>
<td>Quarterly Earnings</td>
<td>Y</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mass layoff</td>
<td></td>
<td>Y</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Firm wage IV</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td></td>
<td>Y</td>
<td>Y</td>
<td></td>
</tr>
<tr>
<td>BLM cluster IV</td>
<td></td>
<td></td>
<td></td>
<td>Y</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>OLS</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>Y</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Firm value control</td>
<td>Y</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Firm amenities control</td>
<td>Y</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

**Note:** Main spec. FE correspond to table 4 column 4 and are firm by event and calendar time by tenure bin by initial wage at hire, all for the origin firm, and where tenure and hire wage are divided into 8 bins. Firm value, $V^{EE}$ is estimated based on the procedure described in Sorkin (2018) over the full sample of observations in the worker-quarter panel, and for the separations regression in column 2 above has elasticity -0.222 (SE=0.033). The firm amenities value is calculated as the difference between the AKM firm effect and firm value, $V^{EE}$, and the separations elasticity with respect to the amenities value in column 3 is -0.291 (SE=0.038). BLM firm decile is estimated based on the procedure described in Bonhomme, Lamadon and Manresa (2019), and is used as an alternative instrument in place of the firm wage. OLS indicates that the firm wage instrument is not used, i.e., separations are regressed directly on the change in log own wage at initial transition. Quarterly earnings indicates the main specification with quarterly earnings instead of hourly wage, for both the firm and own wage changes. 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 (a) 500 fewer workers in the following 4 quarters, or (b) 1/3 fewer workers in the following 4 quarters. Standard errors shown in parentheses are clustered at the level of Origin firm by initial separation quarter.
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 [39], who cluster firms based on their empirical earnings distribution. Following [39], in Column 4 we replace the instrument from the change in mean firm wages to 10 clusters of the $I(i)$ firm wage distribution (again, conditional on $O(i)$ firm fixed effects). Firms are partitioned into these 10 clusters based on the proportion of workers in each ventile of the hourly wage distribution using k-means clustering. Use of the 10 clusters as instruments—instead of the firm average wage—does little to change the separations elasticity, which in this case falls slightly to $-2.03$.

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 is generally much closer to the findings in the “standard approach” presented in [130] 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 quarterly-earnings-based estimates 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 (e.g., time varying worker heterogeneity not captured by history) around the Origin – Intermediate 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 (a) 500 fewer workers in the following 4 quarters,
or (b) 1/3 fewer workers in the following 4 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 1.6 presents the heterogeneity in the separation elasticities. Using the 1-digit NAICS super-sectors, we exclude agriculture, as well as mining, utilities and construction because these industries have far fewer employees (less than half the number employed in 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 the high-wage business, financial and professional services at 4.6 and 7.8, respectively. In contrast, they are small in low-wage sectors of art, accommodation and food services (which includes restaurants) and wholesale, trade and transport (which includes retail) at 2.4 and 2.8, respectively. This sectoral variation in the labor supply elasticity is much larger than the findings using the traditional approach in [165]. It is also worth noting that one may have assumed that low-wage sector like restaurants and retail would be more competitive, especially given the frequency of job changes in those sectors. However, our evidence suggests the opposite: the labor supply facing low-wage, high-turnover sectors appears to be much less elastic than that facing high wage sectors. This pattern has important implications when it comes to considering policies and wage regulations to address labor market monopsony, as discussed in [141].
Table 1.6: Heterogeneity in separation elasticities based on matched event study

<table>
<thead>
<tr>
<th>Panel A: Industry of destination firm</th>
<th>First stage</th>
<th>Separations</th>
<th>E-E separations</th>
<th>Movers</th>
</tr>
</thead>
<tbody>
<tr>
<td>Manufacturing</td>
<td>0.178 (0.01)</td>
<td>-2.287 (0.298)</td>
<td>-4.136 (0.804)</td>
<td>36919</td>
</tr>
<tr>
<td>Wholesale, trade &amp; transport</td>
<td>0.188 (0.008)</td>
<td>-1.394 (0.159)</td>
<td>-3.391 (0.487)</td>
<td>63158</td>
</tr>
<tr>
<td>Prof., business &amp; financial services</td>
<td>0.117 (0.01)</td>
<td>-3.91 (0.267)</td>
<td>-7.974 (0.856)</td>
<td>71620</td>
</tr>
<tr>
<td>Education and Health</td>
<td>0.154 (0.006)</td>
<td>-2.148 (0.158)</td>
<td>-3.777 (0.503)</td>
<td>58072</td>
</tr>
<tr>
<td>Art, Accommodation &amp; Food</td>
<td>0.238 (0.021)</td>
<td>-1.201 (0.255)</td>
<td>-2.301 (0.786)</td>
<td>22999</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Panel B: Geographic zone of destination firm</th>
</tr>
</thead>
<tbody>
<tr>
<td>Portland metro</td>
</tr>
<tr>
<td>Non-Portland metro</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Panel C: HHI (employment)</th>
</tr>
</thead>
<tbody>
<tr>
<td>0-500</td>
</tr>
<tr>
<td>500-1500</td>
</tr>
<tr>
<td>1500+</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Panel D: HHI (payroll)</th>
</tr>
</thead>
<tbody>
<tr>
<td>0-500</td>
</tr>
<tr>
<td>500-1500</td>
</tr>
<tr>
<td>1500+</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Panel E: Period of initial separation</th>
</tr>
</thead>
<tbody>
<tr>
<td>2003-2006</td>
</tr>
<tr>
<td>2007-2009</td>
</tr>
<tr>
<td>2010-2012</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Panel F: Quartile of pre-separation wage</th>
</tr>
</thead>
<tbody>
<tr>
<td>Quartile 1</td>
</tr>
<tr>
<td>Quartile 2</td>
</tr>
<tr>
<td>Quartile 3</td>
</tr>
<tr>
<td>Quartile 4</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Panel G: Time horizon</th>
</tr>
</thead>
<tbody>
<tr>
<td>4-quarter out</td>
</tr>
<tr>
<td>8-quarter out</td>
</tr>
<tr>
<td>12-quarter out</td>
</tr>
</tbody>
</table>

Note: Industry is defined at the 1-digit level. “Agriculture”, “mining, utility and construction”, and “other” industries excluded due to low number of movers. Prof., business and financial services includes Information. Panel E indicates year of initial separation. Time horizon censors the sample at different maximum quarters, and presents the average. Standard errors in parentheses, clustered at Origin firm by initial separation quarter.
We also report elasticities separately for the Portland metro area and rest of Oregon (Panel B). These two subsamples differ dramatically in levels of labor market concentration, where labor markets are defined at the level of commuting zone by 4-digit industry by year (following [148]). In metro Portland, the average employment (payroll) Hirschman-Herfindahl-Index (HHI) is 0.12 (0.14), while the average outside of the Portland metro area the 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% 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% 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 \times industry (4-digit) \times 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. [11], 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 [15], [148], [11] and [146], 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 [141]. 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. [109], and [150] 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 [84], 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 [167], [67], [100], 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 in general higher separations elasticities than those who stay at one firm throughout our sample period As a consequence, our estimated labor supply elasticity (a weighted LATE among movers) may be an upper bound on the degree of dynamic monopsony in the labor market. While omitted from Table 1.6 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 those with none.
1.6 Discussion and Conclusion

The individual separations elasticity with respect to own wage has been taken as evidence for dynamic monopsony power. However, the literature estimating separations elasticities has rarely successfully distinguished between the wage variation due to worker heterogeneity and that due to firm wage-setting although the theory points towards firm wage-setting as the relevant component of the wage. We isolate firm wage policies using two different approaches, one that follows [2], where wages are additively separable into a fixed worker component and a firm fixed effect, and a second approach that estimates the elasticity of separations with respect to the firm component of wages using a matched-worker event study approach. Estimating dynamic monopsony using the wage variation generated by movers links the size of flows between firms and the causal effects of firms on hourly wages: in models with dynamic monopsony, the tendency of workers to move between two firms depends on differences in firm effects on wages.

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

Both approaches lead to broadly similar results; the advantage of the event study approach is not having to impose the AKM decomposition on wages. Relative to estimates obtained from our procedure, existing elasticities from individual level separations regressions appear to be substantially downwardly biased in magnitude, consistent with 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 (e.g., [156, 122]), is that a substantial part of firm fixed effects reflect compensating differentials for firm-specific disamenities. Our paper provides some evidence against this view. First, unlike most work to date, our AKM effects are in hourly wages, so they are not driven by unobserved hours variation, as would be the case in the LEHD or IRS data used in [156] and [122]. Table 1.2 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 in this paper with job differentiation and concentration to both unify and disentangle the sources of monopsony power across labor markets.
CHAPTER 2

FIRMS AND INEQUALITY WHEN UNEMPLOYMENT IS HIGH
2.1 Introduction

How relevant are firm pay policies to wage inequality in contexts of high unemployment? How does this relate to monopsony power in the labor market? And how do these answers relate to findings from countries close to full employment? While a fast-growing literature has investigated the role of firms in explaining labor market inequality, much of the attention has focused on the US and a handful of European contexts, partly due to a paucity of matched employer-employee administrative data \[38, 55\]. This raises the question of whether we can extrapolate the findings of the literature to developing countries, especially when they are characterized by unemployment or labor surplus; and whether unemployment may in fact exacerbate wage inequality through the channel of firm wage policies. To make progress on these questions, I provide the first estimates of the wage inequality due to firms and associated mechanisms in South Africa.

Using matched employer-employee tax data from 2011 to 2016 for the universe of South African formal sector workers, I show that the dispersion of firm wage premia is high. I follow the literature in decomposing earnings into firm and worker wage premia, including a battery of validation checks \[2, 117, 122\]. I find that firm wage premia explain 28% of the total wage variance. Accounting for sorting of high-wage workers to high-wage firms, firms explain more than a third of wage inequality in South Africa. The dispersion of firm wage premia is substantially higher than comparable estimates for other countries, and even more so when considering the raw variance rather than the proportion explained, given that South Africa is one of the most unequal countries in the world \[172\].

Since these firm-worker variance decompositions have been estimated predominantly in high income countries, I highlight two lessons from estimation in the South African context of high unemployment and informality. Firstly, a key validation test of this additive model of firm wage premia, the symmetry of wage gains and losses
for joiners and leavers to a firm, performs much worse when restricting to workers with a gap in formal employment (and better otherwise). The R-squared from their corresponding wage premia estimation is also lower (again, higher otherwise). This is consistent with the possibility that long unemployment or informality spells entail a penalty, which contributes to asymmetry in wage changes and biases the wage premia estimates. Secondly, the exclusion of the informal sector, which has lower wages, will tend to cause underestimates of the raw variance in firm wage premia. Rough calculations suggest a substantial increase of more than a quarter.

What explains the high dispersion in firm wage premia in South Africa relative to other, high income countries? The relevance of imperfect competition under conditions of labor surplus may be counter-intuitive, since one may expect firms to recruit unemployed workers at a low constant wage. Yet models of monopsonistic competition which incorporate on-the-job search suggest firms may still contribute substantially to wage dispersion, and in fact higher unemployment may exacerbate this [130]. To guide the discussion, I set out a framework close to [55] where variation in firm wage premia is due to two sources: firm productivity dispersion and the firm labor supply elasticity (the key measure of monopsony power). Variation in firm-level log value added per worker (with market-specific coefficients) explains nearly half of the dispersion in firm wage premia. Using firm balance sheet information in these tax data, I provide evidence that South Africa has high firm productivity dispersion, as found for other developing countries [102].

I then focus on the firm labor supply elasticity. I estimate it based on a variety of worker separations designs from the literature, including cross-sectional variation, productivity shocks, and instrumenting wages of matched workers with firm wages in an event-study of movers [24, 122, 168]. My estimates of the firm labor supply elasticity are lower than comparable estimates from several countries [154]. Indeed, my framework, as in the literature, relates a lower firm labor supply elasticity to a
higher firm wage dispersion. One important reason is that it mediates a higher pass-through of productivity dispersion to firm wage dispersion, since under a constant competitive wage firms would pay workers similarly regardless of firm productivity. I directly check this implication by separately estimating the pass-through (or rent-sharing elasticity), and correspondingly find this is towards the high end of the range in the literature. The firm labor supply elasticity also affects firm wage dispersion through other channels – interacting with other constraints and through heterogeneity in the elasticity itself – and I briefly discuss their relevance.

Finally I suggest that this low labor supply elasticity may be related to the high unemployment in South Africa. This is motivated by strong theoretical links, as well as tentative cross-regional correlations (a persuasive case is left to future work). This suggests that South Africa’s two world-ranking labor market features, among the highest unemployment and inequality, may be linked through a firm-based mechanism, whereby unemployment contributes to a low labor supply elasticity which drives up the firm wage dispersion. An instructive comparison is Brazil, which has high inequality and informality: [7] show that firm wage premia and sorting account for a high proportion of total wage variance, and the pass-through of productivity to wages is also high. More generally, I discuss how the links I have identified in this paper, between inequality and typical developing country features of low formal sector employment rates and high dispersion in firm productivity, may be part of the development process through firm-level mechanisms.

After describing the data. Section 2.3 provides estimates of the wage premia, including several validation checks on identification and cross-country comparisons.

\[1\] To my knowledge, no papers have discussed the full thread of these links, or explored its implications for high unemployment settings. Several papers have showed the empirical links between firm wage dispersion and rent-sharing [56, 57], and others between rent-sharing and labor market power [116, 149]. [99] have considered the link between labor market power and unemployment, but only in the context of time series cyclical unemployment in Germany.
Section 2.4 discusses reasons for the higher dispersion in firm wage premia relative to other countries, by laying out a framework, then focusing on the firm labor supply elasticity, and motivating its links to unemployment. I end with further discussion of the informal sector, and the relevance of firm-based inequality to the development process more generally.

2.2 Description of data

I use six years of matched worker- and firm-level data from South African administrative tax records between 2011 and 2016, made available through a confidential data-sharing agreement with South Africa’s National Treasury and UNU-WIDER [144, 145]. A collection of papers describing and using this dataset appears in a special issue of the South African Journal of Economics in the first quarter of 2018. I describe in more detail my sample construction in Appendix B.2, including comparisons to nationally representative surveys and how my formal sector data relate to the informal sector.

The cleaned dataset used for my analysis consists of 8-9 million workers in each year, summarized in Table 2.1. The median real annual wage is stagnant, growing about 0.2% per year, compared to much faster growth at the 90th percentile of about 1.5% per year. This pattern of growth is consistent with trends based on publicly available tax tables [25]. Over a third of workers separate from firms each year, in line with findings for the same data by [112]. Close to half of the workers who separate go to other firms (E-E or Employment to Employment separations) and the rest are not employed in the following year (E-N or Employment to Non-employment separations).

I observe unique identifiers for workers as well as establishments. However, balance sheet information is only reported at the firm level, i.e. for each firm-based collection of establishments. In the analysis below, “firm” wage premia would more accurately
be named “establishment” wage premia (section 2.3); similarly, the estimated firm labor supply elasticity uses individual level separations defined at the establishment level (section 2.4.2). On the other hand, variation in value added is observed at the firm-level, not by establishment.

<table>
<thead>
<tr>
<th></th>
<th>Workers (freq.)</th>
<th>Real earnings (ZAR)</th>
<th>Separations (mean)</th>
</tr>
</thead>
<tbody>
<tr>
<td>2011</td>
<td>8,353,791</td>
<td>87,426</td>
<td>357,735</td>
</tr>
<tr>
<td>2012</td>
<td>8,681,995</td>
<td>87,805</td>
<td>365,590</td>
</tr>
<tr>
<td>2013</td>
<td>8,900,366</td>
<td>86,377</td>
<td>366,006</td>
</tr>
<tr>
<td>2014</td>
<td>8,981,113</td>
<td>86,158</td>
<td>370,980</td>
</tr>
<tr>
<td>2015</td>
<td>9,150,558</td>
<td>87,527</td>
<td>381,560</td>
</tr>
<tr>
<td>2016</td>
<td>8,999,547</td>
<td>88,632</td>
<td>385,823</td>
</tr>
</tbody>
</table>

Table 2.1: Summary statistics of tax panel data

Notes. Wages are annualized and adjusted for inflation (base year 2016). Earnings include wage benefits such as overtime and annual bonus. A separation occurs when a worker is no longer recorded at the same firm in the following year. Observations are restricted to workers at firms with more than 20 workers. Source: Own calculations, South African tax records, 2011-2016.

Overall, these administrative data contain no sampling error, are probably more reliable for wages than surveys\(^2\) and provide a unique opportunity to track a panel of workers at their firms over an extended period\(^3\). However, this dataset is not representative of all workers in South Africa. My sample may be better described as workers at sizable formal firms, since I focus on firms with more than 20 workers for reliable estimation of the wage premia. Importantly, informal, unreported work such as domestic workers and informal traders are excluded, which together comprise a third of all employment, and have relatively lower incomes and conditions. Given all

---

\(^2\) Surveyed wages are typically dependent on respondents remembering exact figures across many months (including once-off payments like annual bonus) in the right definitions (e.g. net or before tax) and being willing to give up socially private information.

\(^3\) There are no other matched employer-employee dataset available in South Africa. The National Income Dynamics Survey is the other possibility for panel data analysis for individuals in South Africa, but suffers from small sample size and is only collected every other year.
these caveats, the data still reflect the actual incomes of over half of all workers in South Africa for six years.

Workers’ unemployment spells are not directly observed, making these tax data unsuitable for unemployment analysis. In this paper, I also study unemployment in terms of regional heterogeneity, merged from survey data. I estimate unemployment at the level of local municipalities, of which there are 226 in South Africa, using the 2011 National Census for precision [159]. I measure unemployment as the unemployment to population ratio, a broad measure which circumvents the ambiguous classification of informal sector jobs. As a secondary measure of unemployment, I use the tax data to estimate duration of unemployment through gaps between employment spells. However, this is extremely imprecise since the tax records are annual, and so duration is measured in full year unemployment gaps between observed formal sector jobs.

2.3 Dispersion in wage premia

In this section, I study the dispersion in wage premia for the South African labor market. I begin by laying out the estimation method and associated validation checks, then I present the variance decomposition of wage premia, and end with comparisons to other estimates in the literature.

2.3.1 Estimation and validation of wage premia

The idea that firms have specific wage premia was first empirically investigated by [2] or AKM, followed by several papers since then [57, 155]. I follow these papers in estimating firm and worker wage premia using the AKM wage equation, which imposes that a worker’s wage can be additively decomposed into “firm effects” $\phi_j$ (used interchangeably with “firm wage premia” in this paper), “worker effects” $\alpha_i$, and an error term as shown in equation [2.1]. The outcome is log annualized wages for
individual $i$ in firm $j$ for year $t$. To account for life cycle and time effects, I control for $X_{ijt}$ as up to a cubic in age, as well as year fixed effects. All analysis is restricted to the largest connected set of firms.\footnote{Following [50], I exclude the linear term in age, since age and year fixed effects are perfectly co-linear, and normalize the squared and cubic terms by age 40.}

\[
\ln(wage_{ijt}) = c + \sum \alpha_i + \sum \phi_j + X_{it}\rho + \nu_{ijt} \tag{2.1}
\]

Intuitively, worker effects $\alpha_i$ are the portable component of the worker’s wage, the part that the worker is paid no matter which firm she is at. Firm effects $\phi_j$ are added to the wage of all workers currently at firm $j$, regardless of their worker effect. Identification of the firm effects in this setup relies on movers, that is, workers who switch between firms. \footnote{Firms A, B and C are connected if the same worker is observed at A and B, and a (potentially different) worker is observed at both B and C. This enables comparison between wage premia of the firms, based on the differences in wages experienced by movers.} illustrates this with a simple two firm, two period case: assuming parallel trends (i.e. the counterfactual wage growth of a mover is that of a stayer) and impersistence (i.e. the mover’s wage at the new firm is the same as if she had always been there), then the firm 2 effect is just a weighted average of the wage gain experienced by movers from firm 1 to firm 2, and the wage loss experienced by movers from firm 2 to firm 1. The additive structure of AKM imposes that the wage gain and loss are equal.

There are a number of tests to evaluate whether the structure imposed in equation 2.1 is appropriate. The most well-known follows [50], presented in Figure 2.1 Panel A as a non-parametric justification for the AKM structure. The figure depicts the average wages of workers before and after switching firms. The sample is restricted to those continuously employed over the period, for three years at their original firm preceding their move, and three years after their move. For both the original and
destination firms, workers are classified by quartiles in the co-worker firm wage distribution, i.e. leaving out the wage of each worker.

![Figure 2.1: Wage profiles of movers by firm co-worker quartiles](image)

**Notes:** The legend shows worker moves from origin quartile to destination quartile. Quartiles are calculated as the mean co-worker quartile in the firm, i.e. leaving own-wage out of mean firm wage. In Panel A, only origin quartiles 1 and 4 are plotted, and only employment-to-employment movers are included, such that each worker stayed at the same firm from 2011 to 2013, then moved to a new firm and stayed there from 2014 to 2016. Event year 0 (or tax year 2014) represents wages at the new firm. Wages of the full sample (including stayers) are residualized on year effects before plotting.

In the three years before the switch, wages for movers across the distribution are stable, which is consistent with the parallel trends assumption. There is little evidence of a substantial “Ashenfelter Dip”, i.e. the possibility that workers systematically experience a negative event just prior to switching firms, which registers as below-average wages in $T - 1$ and causes them to switch firms, thus spuriously indicating a firm wage gain on switching. The stability of wages after the move supports the impersistence assumption, such that the average wage of workers directly after moving is similar to the average wage at the same firm 2 years later.
Figure 2.2: Symmetry of wage changes by firm co-worker quartiles

Notes: The figure shows the quartile to quartile log wage changes corresponding to the quartile transition event study in Figure 2.1. Upward mover indicates that the worker moved from a lower quartile to a higher quartile; downward mover indicates the worker moved to a higher quartile. For example, the point labeled “Q1 and Q4” shows the average log wage change for movers from quartile 1 to quartile 4 on the horizontal axis, and for movers from quartile 4 to quartile 1 on the vertical axis. The dotted line shows the 45 degree (negative) slope from the origin: symmetric downward and upward log wage changes would lie on this line. Panel A shows all quartile wage changes across the firm switch for movers, and Panel B shows the same quartile wage changes for workers with a one year gap from formal sector employment.

The AKM regressions rest on several other assumptions, many of which can be tested. I take these up in Appendix B.3 including checking for patterns in regression...
residuals, limited mobility bias, and compensating differentials \cite{3, 38, 122}. I find that the tests are roughly satisfied. To help address remaining concerns of endogeneity, I also present estimates from a set of firm closings which may represent more exogenous worker moves. As recommended by \cite{117} or KSS, I use as my primary set of estimates their leave-out estimator which corrects for the mismeasurement of the fixed effects.

I highlight two lessons for estimating AKM regressions particularly regarding developing country contexts where formal sector matched employer-employee data exclude a large part of the labor force. Firstly, in principle the AKM statistical model and validation tests should hold even when a significant proportion of transitions is not observed; for example in the case of out-migration as in studies of US states \cite{121}. However, the estimation of firm wage premia is methodologically more reliable when based off \textit{continuously employed} movers in the matched sample, i.e. the observed formal sector. In contexts with high unemployment and informality, like South Africa, many observed transitions within the formal sector follow relatively long gaps of more than a year.\footnote{Calibrating to QLFS data \cite{158}, the monthly job-finding rate $\lambda \approx 0.056$. The implied frictional unemployment duration is about $1/\lambda = 18$ months, which is plausible in South Africa. Of course the mean job-finding rate and duration may diverge strongly from the median.} Such gaps may well be penalized by hiring firms, violating the additive structure of the AKM wage premia. Indeed, Figure 2.2 Panel B shows that the wage losses are systematically greater than the wage gains for workers who have such a gap, leading to far less symmetry in wage changes than for workers who are continuously employed as in Panel A. Such gaps, whether due to unemployment or informal sector employment, are not considered in standard AKM estimation, and could only partially be controlled for by duration of the gap (since this again imposes an assumption on the shape of the non-formal employment penalty across time, worker types, and firms).
Secondly, the exclusion of the informal sector will affect the variance decomposition of the wage premia. The informal sector wage distribution is substantially lower than the formal sector wage distribution, though plausibly with similar wage-setting patterns to the above for informal sector wage-workers. This will likely increase the raw variance of firm wage premia, meaning that estimates based on formal sector data will underestimate the contribution of firms to wage inequality, particularly compared to higher income countries which do not have large informal sectors. Section [2.5.1](#) provides further discussion on this, including rough calibrations of the magnitude of this effect on my estimates.

This subsection has motivated the simple framework of assigning firm-specific wage premia. The additive AKM structure passes several validation tests recommended in the literature, yet with some important differences related to South Africa’s large unemployment and informal sector characteristic of a developing country. Next I demonstrate that these firm wage premia play an important role in the South African labor market.

### 2.3.2 Variance decomposition of wages

Firm wage premia in South Africa explain a high proportion of total wage inequality. Table 2.2 summarizes the variance decomposition of wages. Column 1 presents the standard AKM set of wage premia: of the total wage variance, the variance in firm wage premia accounts for 29%, the variance in worker wage premia accounts for 44%, and the covariance between the firm and worker wage premia accounts for an additional 8%. Other terms, such as age and year effects, account for 7%. The R-squared in this baseline regression is 0.88, suggesting the additive AKM model fits the data well. My preferred set of estimates corrects for limited mobility bias using the KSS estimator. Column 2 shows the percentage variance explained by firm wage premia is similar (28%), as for the covariance (8%) and other terms (7%), but the
variation explained by worker wage premia is lower (37%) and by the residual in turn higher (19%). This suggests that, under a counterfactual of no firm wage premia, wage inequality would be 36% lower (28% plus 8%).

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Observations (m)</td>
<td>42</td>
<td>42</td>
<td>13</td>
<td>12</td>
<td>25</td>
</tr>
<tr>
<td>Var(LnWage)</td>
<td>1.32</td>
<td>1.32</td>
<td>1.07</td>
<td>1.49</td>
<td>1.11</td>
</tr>
<tr>
<td>% Var(Firm FE)</td>
<td>29%</td>
<td>28%</td>
<td>29%</td>
<td>24%</td>
<td>33%</td>
</tr>
<tr>
<td>% Var(Worker FE)</td>
<td>44%</td>
<td>37%</td>
<td>47%</td>
<td>28%</td>
<td>43%</td>
</tr>
<tr>
<td>% 2xCov(Firm,Worker)</td>
<td>8%</td>
<td>8%</td>
<td>2%</td>
<td>9%</td>
<td>3%</td>
</tr>
<tr>
<td>% Other terms</td>
<td>7%</td>
<td>7%</td>
<td>10%</td>
<td>8%</td>
<td>13%</td>
</tr>
<tr>
<td>% Residual</td>
<td>13%</td>
<td>19%</td>
<td>11%</td>
<td>30%</td>
<td>9%</td>
</tr>
<tr>
<td>Method</td>
<td>AKM</td>
<td>KSS</td>
<td>KSS</td>
<td>KSS</td>
<td>KSS</td>
</tr>
<tr>
<td>Sample</td>
<td>All</td>
<td>All</td>
<td>Closings</td>
<td>Gaps</td>
<td>Full time</td>
</tr>
</tbody>
</table>

Table 2.2: Decomposition of firm and worker effects

Notes. This is a variance decomposition following Equation 2.1. The first column gives the baseline AKM decomposition [2], while the other columns correct for limited mobility bias using the KSS method [117]. Columns 1 and 2 pertain to all workers. Column 3 restricts to the subsample of firm closings, i.e. stayers (workers who stayed at the same firm throughout), or workers who separated from a firm which was not observed for at least the next two years. Column 4 restricts to stayers or workers who were not observed for at least a gap of one year. Column 5 restricts to workers who were recorded as working full-time. Other terms refer to squared and cubic terms in age (centered at 40), as well as year fixed effects. All samples are limited to the largest connected set of firms with more than 20 employees. Source: Own calculations, South African tax records, 2011-2016.

As discussed earlier, the specification assumptions may be more plausible when using the subsample of firm closings. Column 3 shows a very similar percent variance explained by firm wage premia (29%). The other components change somewhat, notably the sorting term declines, which may indicate the relevance of endogenous transitions to where workers locate in the full sample (as [24] discuss, this does not affect the assumptions required for AKM). The percent variance explained by firm wage premia is also similar when including firms of any size (25%, with 49 million observations), or when demeaning by industry (26%).

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The KSS correction has little effect on the estimated percentage of variance explained by firm wage premia, in contrast with [38]. However, this is consistent with their caveat that limited mobility bias is smaller in longer panels, such as mine of 6 years.
To demonstrate the relevance of unemployed workers to the estimation of these firm wage premia, I restrict to the subsample of workers with a gap in observed formal employment (column 4). The firm and worker components are lower for workers with gaps (24% and 28% respectively), compared to the full sample. A key difference is a larger proportion explained by the residual (30% compared to 19% for all workers), which indicates a worse fit of the specification to the data and is consistent with unaccounted-for terms in Equation 2.1 associated with a gap in formal employment. On the other hand, when I restrict to the subsample of workers who are recorded as working full-time every year to avoid workers with employment gaps, the variance accounted for by the residual drops down to 9% (column 5). In this full-time subsample, the firm wage premia term rises to 33%.

The covariance term in Table 2.2 implies that there is substantial “sorting”, meaning that workers with a high worker component in wages are disproportionately located in firms with high firm wage premia. This is visualized in Appendix Figure B.1 where I plot the distribution of worker wage premia for each decile of firm wage premia. Still, some workers with low worker premia are in high paying firms, which is a key requirement of this AKM framework where there exist “good” and “bad” jobs available to the same worker. As an aside, the figure also shows that nearly a quarter of workers are located at the highest paying 10% of firms, which hints at the dynamics explored in section 2.4.2 on the firm labor supply elasticity.

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8I do not use this as my preferred set of estimates since it is a less representative sample, with 25 million observations compared to 42 million in the full sample. Note the selection on full-time workers may contribute to the decline in the sorting term.

9Another aspect of sorting is the “match” effects that may come about, for example if high premia workers produce more when at high premia firms as in the model of [122]. One way of testing this is to compare the R-squared from the AKM model (0.88) to the R-squared from a fully interacted “jobs” regression which will include match effects (0.94). While there is a difference, it is relatively small and suggests marginal importance as in [56].
Figure 2.3: Decomposition of worker and firm fixed effects, by decile of income

Notes. Worker and firm effects are estimated from the full sample AKM regression using the KSS method (column 2 in table 2.2). Decile of unconditional income is calculated by year, and for each decile the average worker and firm effects are plotted. Source: Own calculations, South African tax records, 2011-2016.

Another way of expressing the importance of the role of firms in wage inequality is through a comparison of average wage components by decile of income (Figure 2.3). The distribution of firm wage premia substantially increases between-decile income inequality compared to just considering invariant worker characteristics (i.e. worker wage premia). Comparing deciles 1-4 to deciles 5-8 of unconditional income, the proportion of the income gap explained by the average differences in firm wage premia is greater than the proportion explained by average differences in worker wage premia[^10]. This is a noteworthy finding for the South African literature which focuses on skills in explaining inequality, given that the worker fixed effect includes all invariant worker-specific characteristics such as education[^25].

[^10]: Average worker premia however play a dominant role for gaps with the top two deciles.
In sum, firm wage premia in South Africa explain over a quarter of the total wage variance in sizable formal sector firms, and over a third when accounting for sorting.

### 2.3.3 Comparison to estimates in other countries

How does this variance decomposition in South Africa compare to estimates for other countries? As highlighted by the recent review of [38], care needs to be taken in selecting comparable estimates since not correcting for limited mobility bias can overestimate the share explained by firm wage premia and underestimate the share explained by sorting.\(^{11}\) While the difference between my uncorrected (AKM) and corrected (KSS) estimates in Table 2.2 are negligible, [38] show that the corrections make a substantial difference in other settings. In Figure 2.4, I plot the 37 estimates from 11 countries compiled by [38], along with my own, and with a focus on the most comparable estimates\(^{12}\).

My estimate for the share explained by firm wage premia in South Africa (28%) is well above the comparable KSS estimates for Austria, Italy, Norway, Sweden, and USA, which range between 6% and 16% (Panel A). When considering uncorrected estimates, my estimate is towards the upper part of the distribution (85th percentile), though this comparison is not as informative given these estimates are upwardly biased in complex ways.

The role of firms in South African wage inequality is even more prominent when considering the raw variance rather than the share explained, since South Africa has

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\(^{11}\)The literature focuses on the role of firms in wage inequality, such that worker wage premia are only of interest as they relate to sorting. [38] do not include the estimated share of variance explained by worker wage premia in their cross-country review.

\(^{12}\)The construction of my panel is very similar to the harmonized estimates from [38]: annual earnings panels, estimated over 6 years, and with the worker’s highest earnings employer taken for each year. A source of difference is that they use a minimum earnings cutoff to address part-time employment, whereas I annualize earnings. I also include a minimum firm size restriction, though for the KSS estimates [38] show very little sensitivity to this (my estimate of the variance explained by firm wages is 25% when dropping this threshold).
one of the highest rates of overall inequality worldwide (Panel B). In this compiled list of estimates, South Africa has by far the highest wage variance of 1.32, where the closest is USA in [155] at 0.92. The raw variance in South Africa (0.37) is therefore much larger than other estimates, whether considering the comparable KSS estimates (maximum of 0.03) or the upwardly biased estimates (maximum of 0.23).

Figure 2.4: Variance of firm wage premia, comparison to other countries

Notes. Estimates are compiled from [38], adding the estimates from this paper. KSS indicates the estimation method in [117] is followed. Share of variance is the share of total wage variance explained, and raw variance is the direct variance of that component.

Regarding sorting, my estimates are more in line with other countries (Appendix Figure B.2). The share explained by sorting in South Africa is within the range of estimates for other countries. The raw variance due to sorting (0.14) is indeed higher than the other KSS estimates (maximum of 0.05), and towards the upper part of the range of uncorrected estimates (note these are downwardly biased).

Overall, my estimates for the firm wage dispersion in South Africa stand out, even before considering the likely underestimation due to excluding the informal sector. I focus on this for the rest of the paper, leaving discussion of variance due to sorting and worker wage premia to future work. A key difference between South Africa and the estimates for the countries reviewed above is the level of development, with the comparable KSS estimates all coming from high income countries, and similarly for
the rest of the estimates (except Brazil). In the next section, I provide some evidence in explaining the dispersion in firm wage premia, including links to a structural feature of South Africa shared by other less developed countries – high unemployment.

2.4 Reasons for the high firm wage dispersion

In this section, I discuss explanations for the higher dispersion in firm wage premia in South Africa as compared to other, mostly high income countries. I begin by setting out a simple framework to guide the discussion of the sources of firm wage dispersion, and provide some evidence on why these sources may contribute more in South Africa. I focus on one mechanism, a low firm labor supply elasticity, and its link to high unemployment.

2.4.1 Framework

The most popular motivations for the prevalence of firm-specific wage premia involve imperfect competition, such as firm monopsony power based on taste heterogeneity [55, 122] or search frictions [130]. Here I use a partial equilibrium setup close to [55], as well as [69], with more details provided in Appendix B.4.1.

Firm $j$ faces downwards sloping firm-specific product demand parameterized by $\eta$, and has exogenous productivity for each worker type $i$ equal to a firm term $T_j$ times by a worker type term $A_i$. For example, production is given by $Y_{ij} = \eta^{-1} A_i T_j N_{ij}^{1-1/\eta}$ for $N_{ij}(w_{ij})$ the number of workers of type $i$ at firm $j$. Firms maximize profit by setting the wage $w_{ij}$ for each worker type $i$, $\max_{w_{ij}} \pi_{ij} = \frac{\eta}{\eta-1} A_i T_j N_{ij}^{1-1/\eta} - w_{ij} N_{ij}$ subject to an upwards sloping firm labor supply constraint, $N_{ij} = w_{ij}^{\varepsilon}$ with constant firm labor supply elasticity $\varepsilon$. A lower $\varepsilon$ implies more monopsony power. Then, setting log marginal revenue product equal to log marginal cost of labor, wages are given by:
\[
\ln(w_{ij}) = \underbrace{\frac{\eta}{\eta + \varepsilon} \ln(A_i)}_{\alpha_i} + \underbrace{\frac{\eta}{\eta + \varepsilon} \ln(T_j)}_{\phi_j} + \underbrace{\frac{\eta}{\eta + \varepsilon} \ln\left(\frac{\varepsilon}{1 + \varepsilon}\right)}_c
\]  

(2.2)

Where \( \alpha_i = \frac{\eta}{\eta + \varepsilon} \ln(A_i) \) is a worker-specific effect, \( \phi_j = \frac{\eta}{\eta + \varepsilon} \ln(T_j) \) is a firm-specific component of wages, and \( c = \frac{\eta}{\eta + \varepsilon} \ln\left(\frac{\varepsilon}{1 + \varepsilon}\right) \) is a constant. This is an additive model of wage-setting consistent with the Equation 2.1 statistical model of wage premia in [2].

While there are many other factors relevant to wages, this framework allows me to focus on the role of firms, as in [55] and [69], such that variation in firm wage premia \( \phi_j \) is due to two sources: firm productivity dispersion and the firm labor supply elasticity. In particular, \( \text{var}(\phi_j) = \left(\frac{\eta}{\eta + \varepsilon}\right)^2 \text{var}(\ln(T_j)) \). This is not to take the functional form too seriously, but rather to suggest that firm wage premia have a positive relationship with firm productivity, an inverse relationship with \( \varepsilon \), and \( \varepsilon \) mediates the pass-through of firm productivity. As I discuss later, the \( R^2 \)-squared from a simple regression of the estimated firm wage premia on log value added (a proxy for \( \ln(T_j) \)) is nearly a quarter, and together with market-specific fixed effects accounts for half of the total variation in firm wage premia in South Africa.

Firm productivity \( T_j \)

Several papers highlight a greater dispersion in firm productivity of developing countries, leading to higher allocative inefficiencies [6, 102, 103]. In my framework, higher firm productivity dispersion also leads to greater dispersion in firm wage premia. Indeed in South Africa, this relationship specified in Equation B.1 is supported by a strong linear relationship between firm productivity, variously defined, and firm wage premia (see section on rent-sharing in Appendix B.6).

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13In a slight abuse of notation, I use \( \phi_j \) in both Equations 2.1 and B.1 to refer to the firm wage premia. The latter imposes a functional form on \( \phi_j \), while the former is purely a statistical representation of a firm indicator term. In my analysis, I make use of the estimated firm wage premia from section 2.3. The same applies to \( \alpha_i \).

14Following [55], I do not focus on \( \eta \).
How does the productivity dispersion in South Africa compare to other countries? I show a variety of estimates for South Africa in Appendix Table B.1. In Appendix Figure B.3, I show that the productivity dispersion in my data is higher than comparable estimates for 18 out of 20 other countries drawn from [19, 20, 102]. This uses the standard deviation in log total factor productivity (TFP), but other measures provide a similar picture – for example, [20] document an inter-quartile range in labor productivity across countries generally between 0.5 and 1, while my comparable estimate is 1.01 in South Africa. In another study, [118] also note high dispersion of within-sector firm productivity dispersion in South Africa. Similarly to the estimates regarding firm wage premia, the inclusion of informal sector firms would likely increase the estimated dispersion. Overall, this suggests high dispersion in firm productivity is an important part of why dispersion of firm wage premia is high in South Africa.

The other high-dispersion estimates generally also concern developing countries; for example, [102] show a standard deviation in log TFP of 0.49 for USA, compared to 0.67 for India and 0.63 for China (my estimate for South Africa is 0.64). The greater variation in productivity in developing countries is potentially driven by diverse factors such as managerial talent, average labor quality, investment, spillovers, competition and input markets [37, 161]. The further documentation of the productivity dispersion and its sources in South Africa and elsewhere is an important avenue for future work.

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I attempt to make the estimates as comparable as possible. Productivity is measured across all sources as total factor productivity (TFP) within each industry, i.e. value added residualized by a regression on 4-digit industry specific terms of the mean, capital, labor, and material costs, and restricted to the broad manufacturing sector. In a review, [161] notes that despite the voluminous literature on productivity measurement, fortunately “when studies have tested robustness directly, they typically find little sensitivity to measurement choices.” In Appendix Table B.1, I show my estimates have little sensitivity to industry disaggregation or firm size thresholds, though dispersion in total factor productivity is generally lower than labor productivity (as found elsewhere, [20]).
Firm labor supply elasticity $\varepsilon$

The parameter $\varepsilon$ is crucial in this framework, as it measures the degree of frictions in the labor market and parameterizes firms’ monopsony power. It affects the variance of firm wage premia in at least three ways. Firstly, the pass-through from productivity variance $\text{var}(\ln(T_j))$ to firm wage premia variance $\text{var}(\phi_j)$ is mediated by the firm labor supply elasticity $\varepsilon$. The intuition is that firms with higher marginal revenue product $T_j$ gain more from employing more workers, and this incentivizes firms to shift wages more at the margin when $\varepsilon$ is low in order to attract more workers. A lower $\varepsilon$ can increase the pass-through of firm productivity variance substantially. To illustrate its potential importance, $\varepsilon = 6$ compared to $\varepsilon = 1.5$ implies a variance in firm wage premia a third as much, given the same variance in productivity – note the squared term in $\text{var}(\phi_j) = \left(\frac{\eta}{\eta + \varepsilon}\right)^2 \text{var}(\ln(T_j))$.

Secondly, the firm labor supply elasticity may influence the dispersion in firm wage premia by interacting with constraints. As Alan Manning writes in his review of the literature, it is unclear “whether employers exercise this monopsony power as a simple profit-maximizing model would suggest or whether other factors act as a constraint” [132, p.9]. The markdowns given by the pass-through coefficient on firm productivity $\ln(T_j)$ and by the constant $c$ in Equation B.1 may vary, potentially bounded or anchored by these terms as they represent optimal wages to the firm. Such constraints include fairness norms [76, 149], national wage-setting [98], and union premia [72]. Once again, if these terms do serve as a reference point, then a

---

16 These values of $\varepsilon$ illustrative, though in line with the literature and my estimates below. I assume a firm-specific product demand elasticity of 5, which is in the range of [55] who calibrate using values between 3 and 10. Of course, one should not take the functional form of Equation B.1 too seriously, and rather treat this as suggestive of the importance of $\varepsilon$.

17 This is not modeled explicitly in Equation B.1. For example, the union premium case could be modeled as a Nash-Bargained solution between the firm’s monopsony wage and the union’s preferred wage.
lower $\varepsilon$ will increase the contribution of varying use of potential monopsony power to the variance of $\phi_j$.

Thirdly, the firm labor supply elasticity $\varepsilon$ itself could vary, giving it a subscript $\varepsilon_j$, for example if frictions differ by local labor market, by informal versus formal sector, or even by firm. How would this affect Equation [B.1]? The modified pass-through coefficient $\frac{\eta}{\eta + \varepsilon_j}$ will be one source of higher firm wage dispersion, but is likely to have limited effect.\(^{18}\) The modified constant $c_j$ would be another source, with a potentially large effect. The variance of $\phi_j$ is increased by variance in $c_j$, in turn increased by a greater variance in $\varepsilon_j$ and by a lower average value of $\varepsilon_j$. That is, firm wage dispersion can also increase through heterogeneity in $\varepsilon_j$ itself, again with pass-through in $\text{var}(\varepsilon_j)$ mediated by a lower average $\varepsilon_j$.

In sum, this framework suggests greater dispersion in firm wage premia is due to greater dispersion in firm productivity, and a lower (and more heterogeneous) firm labor supply elasticity $\varepsilon$. Above I provided some evidence on a greater dispersion in firm productivity in South Africa. In the next subsection, I estimate the firm labor supply elasticity $\varepsilon$ in South Africa, and show that it may be particularly low, thereby contributing to the high firm wage dispersion.

2.4.2 Monopsonistic wage-setting: The firm labor supply elasticity

Estimation framework

An upwards-sloping firm labor supply elasticity grants firms wage-setting power, which is a major source of firm wage dispersion and for which evidence has grown rapidly over the last decade [13, 53, 132]. I estimate the firm labor supply elasticity from worker separations responses to their wage, an approach widely used in the literature [17]. The intuition is that a lower quit response of workers to a cut in their firm’s wages indicates a less competitive market, and more monopsonistic wage-

\(^{18}\)See Appendix B.4.1 for arguments based on Taylor approximations.
setting power. The basic specification is shown in Equation 2.3, where $s_{ijt}$ denotes a separation of worker $i$ from firm $j$ in year $t$, and $w_{ijt}$ is her corresponding wage.

$$\ln(s_{ijt}) = \alpha + \varepsilon_{sep}\ln(w_{ijt}) + \Gamma X_{ijt} + \nu_{ijt}$$  \hspace{1cm} (2.3)

Following [130], the firm labor supply elasticity ($\varepsilon_{LS}$) can either be computed as double the separations elasticity, $\varepsilon_{LS} \approx -2 \cdot \varepsilon_{sep}$, or as a combination of elasticities with regard to other types of transitions (Equation 2.4, where the weights $\theta$ are the proportion of all hires that are from employment).\(^{19}\)

$$\varepsilon_{LS} = -(1 + \theta)\varepsilon_{EEsep} - (1 - \theta)\varepsilon_{ENsep} - \varepsilon_{EErecruits}$$  \hspace{1cm} (2.4)

In the ideal experiment, wages are exogenously assigned to workers and their different separation responses give the separations elasticity. In my first empirical strategy (OLS), which is comparable to much of the literature, I use an OLS regression with worker’s wage as the primary regressor, and the controls indicated by $X_{ijt}$ include the worker type (i.e. the worker wage premium estimated in the AKM regression), as a means of capturing invariant worker characteristics of the wage, as well as time effects. Any unobserved heterogeneity which influences both the wage and the separation rate, such as if workers who are paid highly have access to better outside options, would violate the assumed conditional exogeneity of the wage.

As a second strategy (First Difference), I use separations responses to productivity shocks which affect the firm average wage. I take the first-differenced change in firm value added per worker as an instrument for the first-differenced firm average wages. I compare this change in wages to the first-differenced change in the separation rate of workers at the firm. In contrast to the OLS strategy, this is a firm-level regression

\(^{19}\)Elasticities for other types of transitions are estimated analogously to Equation 2.3. That is, replace $s_{ijt}$ in equation 2.3 with $y_{ijt}$, where $y_{ijt}$ indicate any separation as in equation 2.3 (yielding $\varepsilon_{sep}$), or employment to employment separations (yielding $\varepsilon_{EEsep}$), or employment to non-employment separations (yielding $\varepsilon_{ENsep}$), or the recruitment elasticity from employment (yielding $\varepsilon_{EErecruits}$).
which allows me to trace out the firm labor supply curve through isolating shifts in the firm-specific marginal revenue curve. This identification strategy also uses variation from within each firm across time rather than from the cross-section of workers.

As a last strategy (Movers), which in my view is the most credible but has fewer comparable estimates in the literature, I estimate the separations elasticity using the matched event-study of worker movers following (see Appendix B.5 for details). I track similar workers from the same firm who then separate in the same year to different firms. Here, the identification assumption is that the firm average wage at the new firms are random, conditional on the workers’ respective histories, and so the workers’ separation rates from these new firms are well-identified — an instrumented difference in differences specification for equation 2.3.

Estimates of the firm labor supply elasticity

![Figure 2.5: Firm separation rate and wages](image)

**Notes.** Firm wage refers to the average annualized wages of workers by firm, and are centered around 0 for plotting. Firm separation rate is the average proportion of workers who separate by firm in a year. The plot uses a control function to control for average worker quality.

---

20 Studies which use firm shifters in firm revenue include who use patents, who use tax changes, and who use export shocks. My estimates are conceptually similar, though not as well-identified since I use any statistical change in firm value added per worker rather than a policy induced one. similarly use statistical variation.
As *prima facie* evidence of an upwards-sloping firm labor supply elasticity, there is a strongly linear relationship between higher firm wages and lower worker separation rates (Figure 2.5). A similar relationship appears using the Movers design, between differenced firm wages and differenced separations (Appendix Figure B.11).

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Separations</td>
<td>-0.308</td>
<td>-0.261</td>
<td>-0.319</td>
<td>-0.794</td>
</tr>
<tr>
<td></td>
<td>(0.014)</td>
<td>(0.005)</td>
<td>(0.127)</td>
<td>(0.055)</td>
</tr>
<tr>
<td>Firm LSE</td>
<td>0.858</td>
<td>0.773</td>
<td>0.742</td>
<td>1.59</td>
</tr>
<tr>
<td></td>
<td>(0.032)</td>
<td>(0.015)</td>
<td>(0.256)</td>
<td>(0.110)</td>
</tr>
<tr>
<td>Fstat</td>
<td></td>
<td></td>
<td></td>
<td>14.961</td>
</tr>
<tr>
<td>Obs (m)</td>
<td>36.2</td>
<td>36.2</td>
<td>0.1</td>
<td>0.48</td>
</tr>
<tr>
<td>OLS</td>
<td>Y</td>
<td>Y</td>
<td></td>
<td></td>
</tr>
<tr>
<td>First Difference</td>
<td>Y</td>
<td>Y</td>
<td></td>
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<tr>
<td>Movers</td>
<td></td>
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<td>Controls</td>
<td>Worker type</td>
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<td></td>
<td>Indus X Geo FE</td>
<td>Y</td>
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</table>

Table 2.3: **Firm labor supply elasticities**

*Notes.* The Separations row presents the separations elasticity $\varepsilon_{sep}$. The firm labor supply elasticity (Firm LSE) row combines the estimates from separate regressions, shown in Appendix Table B.2, to produce $\varepsilon_{LS}$ using Equation 2.4. See above for the explanations of the OLS (worker level), First Difference (weighted firm level) and Movers (worker level) specifications. Further specifications for the Movers design in Appendix B.5. The worker type control adds the AKM worker fixed effect as a continuous variable control. The industry by geography control includes 221 by 20 fixed effects respectively. Workers are limited to connected firms with more than 20 employees. Standard errors in parentheses. Source: Own calculations, South African tax records, 2011-2016.

I estimate a low firm labor supply elasticity across the three strategies, which is consistent with the high firm wage dispersion found earlier. Table 2.3, column 1, shows a separations elasticity of $-0.31$ from the OLS regression with controls for worker type, which yields a firm labor supply elasticity of 0.86. Adding industry by region market fixed effects, which may also proxy for potentially different amenities by industry and location, decreases the combined firm labor supply elasticity to 0.77 (column 2). The estimation strategy using firm-level first-differences gives similar results, with an estimated separations elasticity of $-0.32$ and a firm labor supply
elasticity of 0.74 (column 3). The movers event-study strategy yields higher estimates, with a separations elasticity of \(-0.79\), and produces a firm labor supply elasticity of \(\varepsilon_{LS} = 1.6\) (column 4, with further results reported in Appendix B.5) \(^{21}\).

This range of estimates suggests that the firm labor supply elasticity is lower in South Africa than other, especially high-income, settings. As in the firm wage premia literature, comparisons are tricky: a comprehensive meta-analysis by \(^{154}\) finds large systematic differences due to statistical methods. The authors report best-practice estimates for the separations-based approach between 6.4 and 9.9, based on 29 published studies from 9 countries.\(^ {22}\) The movers strategy estimate is also low compared to the only other comparable estimate of 3 for Oregon, USA \(^{24}\).

**Implications for wage dispersion**

As discussed in subsection 2.4.1, the firm labor supply elasticity affects firm wage dispersion through at least three channels.

One channel is that the pass-through of productivity increases when the firm labor supply elasticity is lower.\(^ {23}\) The low firm labor supply elasticity estimated

\(^{21}\)The Appendix Table B.7 shows the full range of estimates for each of these specifications, including the different transition elasticities to the wage. Note however that \(\varepsilon_{EEsep}\) contains some measurement error associated with missing formal sector to informal sector employment transitions. As expected, \(\varepsilon_{EEsep}\) are all greater in magnitude than \(\varepsilon_{sep}\), but \(\varepsilon_{ENsep}\) are lower in magnitude. The movers regression just uses \(\varepsilon_{LS} \approx -2 \cdot \varepsilon_{sep}\) since the strategy is not amenable to estimating \(\varepsilon_{ENsep}\), following \(^{24}\).

\(^{22}\)The authors kindly shared their dataset of estimates with me, allowing me to narrow the estimates to those most comparable to mine. Restricting to peer-reviewed estimates based on worker separations, my OLS and first-differenced estimates are in the 24th percentile; restricting further to those published in top journals only one out of 10 studies finds lower estimates than me, i.e. \(^{77}\), which is a minimum wage sector-wide shock not comparable to firm-level estimates. Top journals are defined by the authors, as top 5 economics general interest journals or the Journal of Labor Economics. All estimates are from high income countries, except one (Brazil).

\(^{23}\)This is the case in the framework above, as well as across a number of other models \(^{55,122,130}\). See Appendix B.6. It is worthwhile estimating the rent-sharing elasticity separately, rather than using calibrations, since the pass-through from productivity to wages may have a different functional form to my framework, and is also governed by several constraints. For example, collective bargaining or fair wage considerations may substantially alter the rent-sharing elasticity predicted purely from the firm labor supply elasticity, meaning there is no one-to-one relationship between the two.
above implies a high pass-through. There is a substantial literature estimating this pass-through or rent-sharing elasticity for several countries, with estimates which broadly range from 0.05 to 0.15 \[55\]. I follow this literature to estimate the rent-sharing elasticity for South Africa, and provide details in Appendix \[B.6\]. In brief, my main specification follows \[56\] in a cross-sectional regression of estimated firm wage premia from section \[2.3\] on log firm value added per worker. I find a pass-through or rent-sharing elasticity of 0.14, which is towards the upper end of the comparable range reported above, and is robust to different measures of firm quasi-rents (profit per worker and estimated total factor productivity) and controls (year, industry and location). As an alternative specification, I use the panel and regress the differenced log firm average wage on the differenced log firm value added per worker. The estimated rent-sharing elasticity is 0.17, which includes fixed effects for industry by location to isolate firm-specific shocks from market level shocks. In sum, the low firm labor supply elasticity estimated above corresponds with a (separately estimated) high pass-through channel which contributes towards higher dispersion in firm wage premia.

I do not provide direct evidence on the other two channels, the constrained use of potential monopsony power and heterogeneity in the firm labor supply elasticity itself. There is little existing evidence on either of these, and a careful approach is left to future work. However, I make two comments here. Firstly, whether due to constrained use or heterogeneity (or something outside of the model), it seems likely that variation in the pass-through coefficient and constant in Equation \[B.1\] exists. As an illustration, returning to the example of union constraints, I show in Appendix Table \[B.3\] that firms with higher union density have a cross-sectionally higher pass-through coefficient or rent-sharing elasticity, and this is robust to controls for worker quality, industry and location. Secondly, it seems variation in these terms are important sources of firm wage dispersion, measured in terms of explanatory power. A simple regression
of the estimated firm wage premia on log value added yields an $R$-squared of 0.22. To allow the firm labor supply elasticity to differ by market, adding either market-specific coefficients or market-specific constants increases the $R$-squared to 0.46, and adding both increases it to 0.5\textsuperscript{24}. Of course there is undoubtedly much else driving this variation, including an entirely different functional form to Equation B.1 and so this only motivates the possible importance of these sources of dispersion in the parsimonious framework of Equation B.1.

In this subsection, I estimated a firm labor supply elasticity that is low compared to other countries, with corresponding implications for a high pass-through rate and firm wage dispersion. The next subsection poses a possible reason for this low firm labor supply elasticity – high unemployment.

2.4.3 Monopsony and unemployment

Motivation

Why does South Africa have a low firm labor supply elasticity? One explanation arises from the clear theoretical link with South Africa’s high unemployment rate. For example, search models such as in [44] provide a monotonic relationship between lower job offer rates $\lambda$ (indicating a lower $\varepsilon$), and higher steady-state unemployment, $1 - \frac{\lambda}{\delta + \lambda}$. More generally, when unemployment is high, workers may have fewer forthcoming wage offers, and therefore be reluctant to quit in response to a wage cut. And since firms are able to draw more on the unemployed, they have less wage competition with other firms, and they can mark down wages further.

Equation 2.4 above suggests that this link is valid for any source of unemployment, whether structural in nature or frictional. Consider setting $1 - \theta$ (the proportion of hires from unemployment) to the unemployment rate $u$, which implies that hires are

\textsuperscript{24}Note that measurement error in log value added as an imperfect proxy for productivity will drive down the $R$-squared. This also does not account for any firm-specific characteristics besides log value added. Market is defined as industry by location cells.
drawn at random from the available labor force. Consider also that the value of the lowest firm wages may be sufficiently above the value of unemployment, that changing the wage does not induce more exit from unemployment. This suggests the elasticity of separation to non-employment may be close to zero, $\varepsilon_{EN sep} \approx 0$, and similarly for the elasticity of recruits from employment compared to non-employment, $\varepsilon_{EE recruits} \approx 0$. Substituting these values into equation 2.4, the firm labor supply elasticity can be written succinctly in terms of the unemployment rate and the employment to employment separations elasticity, $\varepsilon_{LS} = -(2-u)\varepsilon_{EE sep}$. That is, the firm labor supply elasticity decreases as the unemployment rate increases (note $\varepsilon_{EE sep} < 0$).

In practice, $\varepsilon_{EN sep}$ can be substantial in magnitude (see Appendix Table B.2), E-N transitions in the formal sector data may in reality include formal to informal sector transitions, and firm hires are likely to be disproportionately drawn from the employed. The subtraction of $u$ may be better thought of as an approximate discounting of a labor supply elasticity based on $\varepsilon_{EE sep}$, to account for the idea that even if a formal sector firm’s wage was decreased, the other option (be it unemployment or the informal sector) would still generally be substantially worse.

**Suggestive evidence**

I present some suggestive cross-regional correlations in Table 2.4 with corresponding figures in Appendix Figure B.4. I estimate the separations elasticity for each of the 226 local municipalities in South Africa, and regress on the relevant census unemployment rate. Consistent with the discussion, there is a negative relationship between the firm labor supply elasticity and the unemployment rate (columns 1-2), with much more precision when using the employment to employment separations elasticity $\varepsilon_{EE sep}$ (columns 3-4).\footnote{Employment to employment separations observed in the data may still include employment to non-employment separations since the data is observed at the annual level. The precision of the relationship with unemployment may be attenuated when using any separations, because such separations...}

\[ \]
controls, they are subject to many biases – at the local area level, many variables are correlated – and so should just be considered suggestive evidence.\textsuperscript{26}

A few other studies find a similar relationship when considering heterogeneity by unemployment. \cite{99} find a decrease in the firm labor supply elasticity with increases in unemployment for West Germany using time series variation over cyclical unemployment ranging from 6-12\%. Additionally, \cite{62} estimates the firm labor supply elasticity in the US using demand shocks associated with the American Recovery and Reinvestment Act during the Great Recession, and disaggregates the estimates into a low elasticity of 1.8 for the highest quartile of unemployment and a higher elasticity of 6.5 for other quartiles. My results add to these findings by theoretically motivating the link between unemployment and monopsony power, and showing the correlation persists across a wide range of unemployment rates pertinent to developing countries with structural unemployment. Note these estimates all consider heterogeneity in a way that is vulnerable to the biases discussed above.

With the same caveats as above, the empirical cross-regional correlation within South Africa support implications of variation in firm labor supply elasticities too. Higher unemployment regions are correlated with lower average firm wage premia as expected (columns 5-6). If high unemployment drives a lower labor supply elasticity, separations include workers who retire or have longer unemployment spells governed by different considerations.

These correlations are subject to several additional biases. Firstly, the estimation of the separations elasticities uses the specification in column 1 of Table \ref{tab:1} as the least demanding specification for heterogeneous estimates (the overall estimated elasticity is similar for the first-differences strategy, and the movers specification requires too large a sample size to be estimated for each local municipality). Even if there is bias in this specification, as long as the bias is not systematically correlated with municipality characteristics, the correlations with unemployment are valid, as argued regarding heterogeneity analysis by \cite{123}. However, measurement error at least is likely to be correlated with municipality characteristics, when there are few firms in a region. Secondly, this uses a particular unemployment definition – broad unemployment to population ratio. There are several other candidate definitions, such as including the informal sector. I provide an alternative definition as the average duration out of observed formal employment in the data (see Appendix Figure \ref{fig:B.5}). Thirdly, any spillovers due to cross-regional migration will bias these estimates, mitigated only to the extent that labor is not mobile \cite{63}. Fourthly, any general equilibrium effects will not be captured as they are absorbed into the intercept.

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which in turn drives higher firm wage dispersion, then one may expect higher unemployment regions have higher firm wage dispersion (columns 7-8).\footnote{An additional reason why this should be taken as suggestive correlations rather than anything causal is that there is another link between unemployment and firm wage dispersion: standard search models imply that the reservation wage increases with firm wage dispersion \citep{101}. The intuition is that dispersion increases the option value of waiting, since a lucky draw could result in a very good job. With higher reservation wages, the observed unemployment rate is higher for a given firm wage dispersion. Note this causal pathway is very different to the firm labor supply elasticity mechanism, potentially with feedback effects, but both predict a positive correlation between unemployment and firm wage dispersion.} As above, these correlations do not disappear with controls, or when using unemployment duration rather than the unemployment rate (see Appendix Figure \ref{fig:unemployment}).

\begin{table}[h]
\centering
\begin{tabular}{lcccccccc}
\hline
 & (1) & (2) & (3) & (4) & (5) & (6) & (7) & (8) \\
Unemployment & -0.956 & -0.940 & -1.313** & -2.387*** & -0.441** & -0.248** & 0.283** & 0.243* \\
 & (0.644) & (0.577) & (0.562) & (0.670) & (0.189) & (0.112) & (0.127) & (0.126) \\
Obs & 164 & 164 & 164 & 164 & 211 & 210 & 164 & 164 \\
Controls & Y & Y & Y & Y & Y & Y & Y & Y \\
Outcome & Sep & Sep & E-E Sep & E-E Sep & Firm FE & Firm FE & var($\phi_j$) & var($\phi_j$) \\
\hline
\end{tabular}
\caption{Unemployment and regional indicators of labor market power}
\end{table}

Notes. Unemployment is measured as the municipal unemployment to population ratio from the Census 2011. The separations elasticities are estimated by local municipality, by regressing firm separations on firm wages controlling for AKM worker effects, for all separations (columns 1-2) and employment to employment separations (columns 3-4). Regions with fewer than 30 firms are omitted. Firm FE are the estimated KSS Firm wage premia from Section \ref{sec:firm FE}, and var($\phi$) are the regional variance in these firm wage premia. Controls refer to regional average firm size, value added per worker, population density, and industry composition. Source: Own calculations, South African tax records, 2011-2016.

In summary, firms potentially link two of the most outstanding features of the South African labor market: high wage inequality and high unemployment. I have argued that a large part of South Africa’s wage inequality is due to dispersion in firm wage premia, and a simple framework shows that this is partly explained by higher dispersion in firm productivity and partly explained by more monopsonistic wage-setting power. I provide evidence on the latter by estimating a low firm labor
supply elasticity, and in this subsection I have motivated theoretically and through suggestive correlations how this low firm labor supply elasticity may be linked to the high unemployment.

2.5 Discussion

In this section I discuss in further detail how the informal sector affects the analysis above, as well as the implications of my argument for firms and inequality in developing countries more generally.

2.5.1 Imperfect competition in the informal sector

My main analysis has focused on formally employed labor, even though a characteristic feature of South Africa and developing countries generally is a substantial proportion of informally employed labor. More than 60% of the world’s employed population works in the informal economy, located overwhelmingly in developing countries [107]. This subsection explores the competitive dynamics in the informal sector and its interaction with the formal sector, using survey panel data for South Africa. I discuss further details in Appendix B.7.

As mentioned in section 2.3, excluding the informal sector from the firm wage premia decomposition will tend to underestimate the raw variance due to firm wage premia. Although survey data cannot identify firm wage premia as well as the matched employer-employee data, I can illustrate this with some rough calculations. Informal sector workers account for 30% of employment, earn about 28% less than formal sector workers, and are part of informal enterprises with a firm wage variance of 0.64. Assuming these are two combined normal distributions, this would increase the raw variance in firm wage premia from 0.37 to 0.4728. That is, ignoring the informal sector

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28 According to quarterly panel data from household labor force surveys [158], informal sector workers earn about 0.28 log points less per hour than formal sector workers, adjusting for observable worker characteristics (monthly rather than hourly earnings will further exacerbate differences).
means I may underestimate firm wage dispersion by a quarter, which further widens the gap compared to higher income countries.

The framework used to explain the firm wage dispersion relies on some competition in the labor market. How well does this apply to the informal sector? Firstly, wage differences are correlated with transition patterns as in figure 2.1 above. Nearly 1 in 5 workers transition from the informal to formal sector every quarter, compared to only 1 in 25 in the reverse direction (see Appendix Tables B.10 and B.11). Moreover, workers who transition from the informal to the formal sector report substantially larger wage gains, while workers transitioning in the reverse direction report wage losses of a similar magnitude. These patterns are consistent with low productivity informal sector enterprises offering low firm wage premia, which fall towards the bottom of a job ladder comprised of all jobs (informal or formal). Secondly, I estimate separation elasticities, and find that the relative magnitudes show similar labor supply elasticities for formal and informal workers, suggesting comparable degrees of monopsony (Appendix Table B.12). Thirdly, I find a rent-sharing elasticity using informal enterprise survey data of 0.26, which is also suggestive of wage-setting power. Though these estimates are likely biased by inadequate controls for worker quality, they suggest that the imperfectly competitive dynamics in the informal sector may not be too different from the formal sector investigated in my main analysis.

Two recent papers analyze the interaction between the formal and informal sectors with search frictions in the context of Brazil [136, 164]. These papers also argue that the informal and formal sectors share a common labor market, compete over similar workers, and face similar competitive dynamics. The differences in the sectors are

Informal enterprise surveys suggest a standard deviation of 0.64. The combined variance is a weighted average of the sum of the standard deviations and the differences in means.

Survey panel data has the advantage of tracking informal sector transitions, but the disadvantage of measurement error resulting in less credible estimates. I assume the relative magnitudes for the separations elasticities for workers in formal and informal jobs are still interpretable, as argued regarding heterogeneity by [123].
that informal sector firms are less productive, and face fewer administrative costs (but are scale-constrained). The informal sector may actually in part be a consequence of a low firm labor supply elasticity. If low infrastructure or information access drives down the firm labor supply elasticity, then the aggregate market inefficiently allocates more jobs towards low-wage, low-productivity firms such as in the informal sector.

In summary, while my primary data are not suited towards the important analysis of informal employment, my analysis using survey data suggests that informal workers may face similar competitive dynamics which contribute to greater inequality.

2.5.2 Firms, unemployment and development

The features and relationships identified in this paper are likely to be relevant to the development process more generally. As documented above, the two sources of firm wage dispersion that I focus on for South Africa – a higher dispersion in firm productivity and a low firm labor supply elasticity – are characteristics shared by other developing countries. For example, both Brazil and Mexico have a high dispersion in firm wage premia and a low firm labor supply elasticity [7, 65]; and [102] document a much larger dispersion in productivity for China and India relative to the US, implying large effects on wage inequality of any pass-through onto wages.

The possibility of firm-level monopsonistic dynamics contrasts with much of the analysis in classical development models which typically take place at the sectoral level, where an industrial sector draws on surplus labor at a constant subsistence wage [26]. This case is nested in standard monopsony models; more generally though, the introduction of firm-level heterogeneity – and in particular on-the-job search by workers between these firms – sharply changes the optimal wage, incentivizing higher productivity firms to post higher wages in order to attract more workers, and resulting in a large dispersion in firm wages [44].
A particular mechanism may be relevant, in the spirit of Kuznets and Lewis, but with mechanisms which operate through optimal firm wage-setting derived from monopsony power (see details in Appendix B.4.2). Classical models of development predict high dispersion in firm productivity as some sectors lead the industrialization process before equalizing across the rest of the economy\[30\]. Initially, at the onset of industrialization, assume a small proportion of firms have high productivity. Such higher productivity firms optimally pay higher wages, set along their upwards sloping firm labor supply curve. These high-productivity firms will still only attract a minority of workers, since they initially constitute such a small proportion of firms, meaning wage inequality is initially low. As the higher productivity technologies spread to more firms, total employment in higher productivity firms increases, and wage inequality also increases (then eventually declines). Appendix Figure B.9 illustrates this transition path.

This mechanism shows how higher wage inequality may fit into the development process, especially for partially industrialized economies such as South Africa and Brazil, driven by optimal firm wage-setting along with frictions in the labor and technology markets. The firm wage premium here is derived through the wage pass-through from higher productivity firms, which follows from a finite labor supply elasticity; this is unlike the wage premium in classical models, where the premium is usually assumed. This contribution of firms to inequality is not inevitable: collective bargaining or regulation via minimum wages are countervailing mechanisms. \[7\] show that a reduction in the variance of firm wage premia accounts for 40% of the dramatic decline in Brazil’s inequality between 1996 and 2012, a period over which the real minimum wage increased substantially.

\[30\]\[128\] narrates, “Capital and new ideas are [...] highly concentrated at a number of points, from which they spread outwards.” This is not to say classical models correctly characterize the development process; see e.g. \[114\] for a critique.
2.6 Concluding thoughts

This paper investigates the role of firms in inequality in the context of high un-
employment, using matched employer-employee data from South Africa. I find that
firms play a larger role in determining wages than in other, high income countries.
I estimate a high firm wage variance, and argue this is related to the high firm pro-
ductivity dispersion and low firm labor supply elasticity. One implication of a low
firm labor supply elasticity is a high pass-through rate: to illustrate its importance,
plausible calibrations imply firm wage variation is a third as large for a given firm
productivity dispersion.\[31\] I do separately estimate a high pass-through rate (though
the gap is not as large). I also suggest that the low firm labor supply elasticity may
be related to the high unemployment rate, as motivated by theory and cross-regional
correlations.

This paper poses several links for future research. Firstly, there are many other
channels through which the firm labor supply elasticity could affect firm wage dis-
ersion. I briefly discuss two in this paper, interactions with other constraints and
heterogeneity in the elasticity itself. One could also evaluate the relative contribution
of each channel (including productivity dispersion) to the gap in firm wage disper-
sion. Secondly, the evidence in this paper on unemployment is only suggestive, and a
persuasive case requires identified estimates. Thirdly, this paper has not focused on
the sorting of high wage workers to high wage firms in the framework or discussion of
sources, partly because the percentage variation explained by sorting did not stand
out nearly as much against other country estimates compared to firm wage premia.
However, the allocation of workers and any match effects are surely important for
developing country firm dynamics.

\[31\] I comparing my highest estimate of the firm labor supply elasticity (\(\varepsilon = 1.6\)) to the lowest in
the best-estimate range of the meta-study of \[154\] (\(\varepsilon = 6.4\)), and use the framework in Section 2.4.1
The role of firm wage-setting power is conspicuously underemphasized in the academic literature and policy in South Africa, which typically focus on the supply side to address the country’s high levels of inequality and unemployment. My wage premia variance decompositions imply that firms account for a fifth of overall income inequality in South Africa.\textsuperscript{32} The high degree of monopsony power estimated for South Africa also implies lower wages, an important aspect out of the scope of this paper. Anti-monopsony policy could substantially decrease this firm-level inequality, while also increasing employment and wages [\textsuperscript{132}]. Such policy includes addressing labor market concentration, reducing barriers to employment mobility as in the search-based dynamic monopsony model, and institutionalizing counterbalances to monopsonistic wage setting such as collective bargaining.

This study of South Africa poses links from firm productivity dispersion, firm labor supply constraints and high unemployment to high inequality, channels that may generalize to developing country contexts. Costs of migration, poor transport infrastructure and a limited supply of skills may lead to a larger role for frictions. Indeed, in a large scale experimental evaluation of the spillover effects from India’s public employment program NREGS, [\textsuperscript{140}] note that “finding positive effects on employment forced us to question the default assumption of competitive labor markets, and look for credible ways to test this assumption.” Monopsony power based on search frictions are consistent with recent experimental evidence in South Africa [\textsuperscript{1}], and the high cost of searching for work [\textsuperscript{137}]. This role of monopsony power may turn out to be a pervasive and important feature in the development process, and needs to be further modeled and tested in developing countries.

\textsuperscript{32}[\textsuperscript{124}] estimate that income from the labor market accounts for 85\% of the Gini coefficient in household income, 62\% of which is due to earnings inequality (rather than inequality between the employed and unemployed). Assuming the percentage explained from the Gini is close to the percentage explained of total wage variance, firms roughly explain 36\% (28\% variance plus 8\% sorting) of 62\% of 85\%, or 19\% of total household income inequality.
CHAPTER 3

COLLECTIVE BARGAINING AND SPILLOVERS IN LOCAL LABOR MARKETS
3.1 Introduction

Collective bargaining institutions are pervasive, with dozens of countries recording over 30% coverage \[106\]. A classical question is the impact on non-covered wages, usually attributed to union threat or displaced labor supply effects \[89, 127\]. In this paper, I focus on a third mechanism: outside options. I present a framework of wage transmissions from firms which raise wages, and a corresponding empirical measure of spillovers through nearby outside options. Using sharp changes in collective bargaining wage agreements in South Africa, I find observable wage gains for workers in both covered firms and firms connected to them through their local networks of worker flows.

I begin by sketching a theoretical framework, starting with a static model featuring preference heterogeneity where employers have wage-setting power. I show that when a large part of the market (such as a bargaining council) is treated by an increase in wages, there is an upward shift in other firms’ residual labor supply curves, which generates substantial spillover wage responses. That is, following a wage increase for covered firms, the expected wage of the outside option for nearby non-covered workers rises, making their jobs less attractive than before, and firms have to increase wages to retain workers. I adapt this to a dynamic framing through what is essentially a repeated static model, by allowing job offers to be drawn from the relevant labor market segment. A key insight is that spillovers at a non-treated firm are proportional to the flow of workers with the treated firms.

My matched employer-employee data allow me to trace these worker flows. Firms more connected through worker flows are geographically closer as expected, but are also similar among a number of other characteristics such as proportion of women and indicators of productivity. In fact, workers frequently switch to firms across industrial and geographic boundaries, creating dependencies between firms in seemingly disparate industry-by-location cells. However, these flows are typically concentrated
among a few firms. They reveal workers’ viable outside options by incorporating information far beyond what the econometrician usually observes as the relevant labor market segment for spillovers.

To test the spillover responses directly, I merge in over 11 years of wage agreements across 34 collective bargaining councils in South Africa. My stacked event study reveals a sharp wage increase of about 4% for firms covered by agreements. Using flows between covered and non-covered firms, I find similar wage increases of about 3.5% for firms with high flows compared to no flows. The estimated spillover wage effects decrease as firms have lower worker flows to bargaining councils, as predicted. This traces out the response function of firms under varying degrees of competition as given by inter-firm flows, and I show more evidence of this pattern in heterogeneity analysis. In addition, I find that profit margins decrease by a magnitude that plausibly indicates a wage-profit trade-off for these firms. These estimates have no evidence of pre-trends, and are robust to alternative sub-samples, controls (including by industry to avoid broad confounding shocks), and specifications.

This picture of the labor market may be surprising given South Africa’s high unemployment. While in other work I discuss monopsonistic competition in this context [21], I show here that firms tend to hire certain types of workers, partially disregarding the large queue of unemployed in South Africa. I am not able to pin down the effects of bargaining council prescribed wages on firm size since the standard errors for my estimates are large.

My study contributes two sets of insights to our understanding of spillovers. Firstly, the specific institution of collective bargaining deserves attention as a pervasive feature of labor markets. An older, more data-constrained literature debated

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1The own wage elasticity, i.e. the firm size effect divided through by the wage effect, is −0.09 (standard error of 0.26) for bargaining council firms, and −0.2 (standard error of 0.28) for spillover firms.
the question of union wage spillovers, but recent studies on spillovers have focused on minimum wages. Following a minimum wage increase, [59, 81, 87] show shifts well into the overall wage distribution, [79, 94] show within-firm spillovers, and [157] show cross-workplace spillovers in the hospital sector. Yet [28] point out that spillovers may be low if targeted workers are a relatively small group of minimum wage workers. Collective bargaining on the other hand usually concerns a large proportion of workers in the middle part of the earnings distribution. Since I study the universe of formal firms in South Africa, I can directly evaluate the importance of collective bargaining and spillovers for the overall distribution. Spillovers roughly double the simulated effect of bargaining council wage increases on the wage distribution, both in terms of magnitude (from 5% to 10%) and the affected proportion of workers (from 40% to 70%).

Secondly, I contribute to the literature on spillovers by addressing the key question of how to define “close” firms in measuring spillovers. I provide a model-consistent and data-driven approach by showing that spillovers occur through bilateral networks of worker flows between treated and non-treated firms. I view my model as complementary to [29], since I rely on similar features of the labor market, but provide a flows-based framework amenable to clean identification. My measure enables more precise measurement through isolating the empirically relevant labor market segment, and I show this directly against more typical measures. Indeed, my cross-wage elasticity is generally higher than other studies which measure spillovers in nearby industry-by-region cells, since these estimates are likely attenuated by firms which are not candidates for spillovers. Moreover, if spillovers do in fact spread through

\[2\] My framework makes progress on both strategic interaction and the limited information of alternative jobs available to searching workers, two aspects which [53] previously highlighted as needing further work in his review of the monopsony literature.

\[3\] My cross-wage elasticity is approximately 0.8, compared to about 0.35 in the firm study of [157], or 0.36 in the sector study of [169]. On the other hand, my estimates are close to the elasticity implied by the distributional analysis of union threat effects in [87], who avoid this attenuation.
worker flows (or similar networks), then it is hard to interpret the magnitude of any spillover without knowing how connected the affected firms are to the directly treated firms.

Beyond the literature on spillovers, this study contributes to our understanding of firms covered by collective bargaining. Firstly, the identification of firms likely to receive spillovers allows me to exclude them from the control group, avoiding violations of the Stable Unit Treatment Value Assumption (SUTVA) required for credible difference in differences evaluations. Secondly, I present evidence that higher productivity treated firms grow more in response to the minimum wage, a re-allocation effect consistent with monopsonistic competition [79]. Thirdly, my event-studies show sharp declines in within-firm wage inequality, though small effects on overall inequality. Finally, I provide the first firm-level profile of South Africa’s bargaining council system, since previous studies have relied on household survey data [129].

In the next section, I outline a theoretical framework of firm-level wage increases and spillovers. I provide institutional context in section 3.3 and describe South Africa’s bargaining councils and the spillover measure of worker flows in section 3.4. I evaluate the effects of large changes in prescribed wages on bargaining council firms in section 3.5 and on spillover firms in section 3.6. I discuss additional findings on robustness, heterogeneity and re-allocation in section 3.7, and conclude in section 3.8 with a simulation of aggregate effects.

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Concern by using the share unionized in a labor market. They report a union premium close to 20%, and “threat effect” spillover on wages of non-union workers close to this for men, suggesting an elasticity close to 1 (though smaller and less clear for women).
3.2 Theoretical framework

3.2.1 Motivation of model features

My theoretical framework combines monopsonistic competition and labor market networks. The contributions of this model are to illustrate the static case of spillovers, and to provide a tight theoretical and empirical link between flows-based monopsonistic competition and the spillover wage responses of firms.

Monopsonistic competition is widely modeled through preference heterogeneity, where the characteristics of a firm (such as location or coworkers) provide idiosyncratic utility in addition to the wage offered [55, 122, 123]. Firms are aware that there is a distribution of such idiosyncratic utilities, even though they cannot observe them for individual workers, and so optimize wages with the knowledge that cuts to the wage have little impact on the utility of workers with high idiosyncratic preferences. The logit model provides a classical representation of this setup [134, 135]. Strategic competition comes out naturally from the logit model as long as firms are non-atomistic, that is, have sizable employment share in the labor market. [12, 28, 109], amongst others, have documented such non-atomistic firms resulting in concentrated labor markets.

Secondly, labor market networks are associated with a vast literature on job search exploring the idea that not all firms are available to workers as outside options [44, 49]. I operationalize this as a worker’s “consideration set” of available firm outside options [50]. I assume that a worker’s consideration set can be represented by the firm-specific historical flows of workers to other firms, similar to the concept of co-worker outside options explored by [51, 108]. I provide evidence on firm-specific flow networks in section 3.4. Labor markets that are constituted by worker flow networks have been

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4This framing refers to workers at a current firm $j$, but can easily apply to unemployed workers (e.g. if $j$ is their previous firm or the consideration set is made up of offers due to other search frictions). While the consideration set is taken as exogenous, a simple framework can endogenize this by incorporating more stable mobility constraints such as geographic distance.
studied by 12 109 150. Such networks provide an intuitive measure of connectedness that is not confined to industrial or regional boundaries 86 133.

These two features of the model complement each other. The connectedness of labor markets, measured precisely by worker flows, motivates the non-atomistic nature of firms relative to their relevant labor market segments. The main result of the model is that strategic interaction takes place along these connections. My model is perhaps closest to 29, who also rely on monopsonistic and strategic competition between firms with non-negligible market share. Their CES production function is closely linked to my logit framing 8, and I make the extension to a dynamic flows-based setting. I also focus more on minimum wages covering firms that are not necessarily at the bottom of the wage distribution, allowing for larger firms to be bound by the regulation, and therefore for the overall wage dynamics to be driven more strongly through spillovers.

I first outline the static logit model, then take it to a dynamic context. The analysis applies to any firm or set of firms raising their wages, and I end this section with the application to my case of collective bargaining. A more detailed discussion of the model aspects summarized below are in Appendix C.6.

### 3.2.2 Static logit

Let the utility of workers be expressed as $V(w_j) = \beta \ln(w_j) + \nu_{ij}$, where $\beta$ parameterizes the latent monopsony power (i.e. the responsiveness of worker utility to wages), and $\nu_{ij}$ follows a Gumbel distribution indicating idiosyncratic preferences for the firm. The distribution yields the probability a worker is employed at firm $j$, or equivalently the firm share of $j$, in log terms $\ln p_j = \beta \ln w_j - \ln (\sum_i \nu_{ij}^\beta)$.

This setup is standard in the literature, e.g. 55, and I make three modifications. Firstly, while an assumption of atomistic competition is usually made, enabling us to treat the term $\ln (\sum_i \nu_{ij}^\beta)$ as a constant, I retain this term as it is essential for the strategic interaction that generates spillovers. Secondly, to justify non-atomistic com-
petition, I explicitly incorporate the consideration set of outside options by restricting to the relevant labor market, indexed by $S$ as in the term $ln(\sum_S^S w_i^\beta)$.

Thirdly, I allow the treatment effect on labor allocated to covered set of firms $k$ in the consideration set to be a free parameter $n_k$, which I assume varies positively with wages.

For example, treated firm employment may become demand-constrained or be determined through efficient bargaining. The modified probability of employment at firm $j$ is $lnp_j = \beta ln(w_j) - ln(n_k + \sum_S^S w_i^\beta)$.

Following the assumption of non-atomistic competition, the firm labor supply elasticity is $\varepsilon_{jj}^n = \frac{\partial lnw_j}{\partial lnw_j} = \beta(1 - p_j) > 0$. The cross-employment elasticity is negative, $\varepsilon_{jk}^n = \frac{\partial lnw_j}{\partial lnw_k} < 0$, i.e. jobs are substitutes. The treated set of firms raise their wages, and see an increase in own employment. Firm $j$, faced with this wage increase from its competitors, can trade off raising its own wages or losing its workers. This wage-employment locus is depicted in figure C.41.

The optimal wage response of firm $j$ is pinned down by its wage setting function. Firms set wages $w_j$ to maximize profits $\pi_j = \max_{w_j} \frac{1}{1-\eta}A_j(p_j(w_j)N)^{1-\eta} - w_j \cdot p_j(w_j)N$, where $\eta$ parametrizes the downwards-sloping firm demand, $N$ is the aggregate labor supply constraint, and $p_j(w_j)$ is the firm share constrained by wages as above. This yields the associated wage and wage-cross-wage elasticity denoted as $\varepsilon_{jk}^w$:

$$lnw_j = \frac{1}{1 + \eta^\beta} (lnA_j + \eta ln(n_k + \sum_l^S w_i^\beta) - \eta ln(N) + ln(\frac{\varepsilon_{jj}^n}{1 + \varepsilon_{jj}^n}))$$ (3.1)

Another way to present this is through an adjacency matrix $S$, where $S_{jl} = 1$ if the firm $l$ is part of $j$’s consideration set, and $S_{jl} = 0$ otherwise. Then the term $ln(\sum_l^S w_i^\beta)$ becomes $ln(\sum_l^S (w_i^\beta S_{jl}))$.

The adjacency coefficients $S_{jl}$ could also represent the degree of connectedness, i.e. a continuous measure as discussed in the dynamic context below. For simplicity of exposition, I just index over $S$ here.

$n_k$ is not equal to the total employment in covered firms. Firstly, $n_k$ is still subject to the aggregate labor supply constraint $N$. For example, if $n_j = Aw_j^\beta$ for any firm $j$, $p_j = n_j / \sum_l^S n_l$, and $A$ doubled: then $n_j$ would double, but $p_j$ would remain the same. For aggregate labor supply constraint $N$, total employment $p_jN$ would remain the same. Secondly, employment may decline at firm $k$ even as the labor allocated to firm $k$ rises, for example if we allow for job queuing (see appendix C.6.3).
Diagrammatic representation. The setup above can be represented by the usual diagram for wage-setting of monopsonistic firms (figure 3.1). The Residual Labor Supply (LS) to the firm is given by

\[ \ln w^\text{LS}_j = \frac{1 + \eta}{1 + \beta^p_k} \left( \frac{-\varepsilon^p_{jk} \eta}{\varepsilon_{jj}(1 + \varepsilon_{jj})(1 + \eta)} + \frac{-\varepsilon^p_{jk} \beta p_j}{\varepsilon_{jj}(1 + \varepsilon_{jj})(1 + \eta)} \right) > 0 \] (3.2)

Comparative statics. Assuming treated firms are supply-constrained and using the parameter values \( \beta = 6, p_k = 0.5, p_j = 0.1, \) and \( \eta = 0.5, \) the cross-wage elasticity

\[ \varepsilon^w_{jk} = \frac{d \ln w_j}{d \ln w_k} = \frac{1 + \eta \beta}{1 + \eta^2 \beta^p_k} \left( \frac{-\varepsilon^p_{jk} \eta}{\varepsilon_{jj}(1 + \varepsilon_{jj})(1 + \eta)} + \frac{-\varepsilon^p_{jk} \beta p_j}{\varepsilon_{jj}(1 + \varepsilon_{jj})(1 + \eta)} \right) > 0 \]

The more general expression representing the two channels for the cross wage elasticity is

\[ \varepsilon^w_{jk} = -\eta \frac{d \ln p_j}{d \ln w_k} + \frac{\varepsilon^p_{jk} d \ln w_k}{\varepsilon_{jj}(1 + \varepsilon_{jj}) + \eta^2 \beta^p_k} \] (3.2)

The first term represents the upward shift of the Residual Labor Supply curve, and the second term represents the decrease in the markdown (i.e. the gap between the Marginal Cost of Labor curve and the Residual Labor Supply curve). The decrease in the markdown will tend to dampen the employment decline. These terms contain recursive elements, which in Equation 3.2 appears as a multiplier on the two terms. For \( p_k \) which is logit supply-constrained, \( \varepsilon^w_{jk} = \frac{3}{1 + \eta} \left( \frac{\varepsilon_{pp} \eta}{\varepsilon_{jj}(1 + \varepsilon_{jj}) + \eta^2 \beta^p_k} \right) \). Note that I assume that the multiplier feedback effects are not so great as to elicit second-round wage responses from the covered firms \( k \), though the increase in wages by uncovered firms will shift up firm \( k \)’s residual LS and moderate the initial employment increase.
Figure 3.1: Diagram of wage floor effects on covered and non-covered firms

(a) Initial treatment effect on covered firms

Notes: The equation for the Marginal Revenue Product of Labor Curve (MRPL) is $\ln(mrpl) = \ln(A_j - \eta \ln(p_jN))$ for firm $j$, aggregate labor supply $N$, proportion of employment $p_j$ and firm-specific productivity $A_j$. The equation for the Residual Labor Supply (LS) is $\ln(w_{LS}) = \frac{1}{\beta} (\ln(p_jN) - \ln N + \ln(n_k + \sum_S w_i^\beta))$, where $n_k$ denotes the treated firms’ employment. The equation for the Marginal Cost of Labor (MCL) is $\ln(mcl) = \ln(w_{LS}) + \ln(1+\varepsilon)$ or the Residual LS plus $\ln(1+\varepsilon)$. The unconstrained firm-specific employment is found at $\ln(mrpl) = \ln(mcl)$, with wage set by the corresponding point on the Residual LS curve, given by Equation 3.1 in text. Panel A presents the usual effect of a wage floor on a monopsonistic firm $k$, such that the wage increases from $w_{k0}$ to $w_{k1}$ and employment increases from $p_{k0}$ to $p_{k1}$. Panel B presents the spillover effect if $k$ is in $j$’s consideration set, where the Residual LS curve shifts up from $LS_{j0}$ to $LS_{j1}$, which represents the cumulative spillover response, and similarly for the MCL curve. This raises the wage to $w_{j1}$, but decreases employment to $p_{j1}$. 

(b) Cumulative spillover effect on non-covered firms
is $\varepsilon_{jk}^w = 0.62$. This suggests that in response to firm $k$ raising its wages by 5%, firm $j$ increases its wage by 3%. The cross-wage elasticity is very similar even if firm $j$’s share is negligible, e.g. $p_j = 0.001$ implies $\varepsilon_{jk}^w = 0.6$. If the primary firm share $p_k$ decreases, the cross-wage elasticity also decreases but remains substantial: $p_k = 0.1$ with $p_j = 0.001$ implies a cross-wage elasticity of $\varepsilon_{jk}^w = 0.23$. If both firm shares are small, the cross-wage elasticity is close to 0 (holding $\beta$ constant), and, under perfect competition with $\beta \to \infty$, then $\varepsilon_{jk}^w \to 1$.

**Profits.** As wage-setters, the first-order effects of an increase in $w_k$ on $k$’s profits $\pi_k$ are zero by the envelope theorem if supply-constrained (wages move along the labor supply line tangent to the iso-profit curve). However, as noted by [31], this is not the case for wage externalities. Firm $j$ is not optimizing along $w_k$, and therefore the first order effects of an increase in $w_k$ on $j$’s profits $\pi_j$ are negative (the labor supply line itself moves upward, such that it is tangent to a lower value iso-profit curve), approximately equal to the wage markdown times the employment effect. The predicted effects on profits are therefore more negative for spillover firms than bargaining council firms.

**Alternative functional forms.** A range of optimal wage-setting curves are possible. For example, [79] use a production function $A_j ln(p_j(w_j)N)$, which implies a much larger magnitude of $\varepsilon_{jk}^w = 0.94$. We can also account for market-level employment responses (which may be important in high-unemployment South Africa) by allowing local market employment to respond to the average wage. This is much like a nested logit function, closer to [29], which dampens the spillover responses as it relieves pressure from the aggregate labor supply constraint. If there are fair or effi-

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8 Although difficult to measure, these parameter values are consistent with rough estimates from the firm data. The regression coefficient from sales per worker on firm size (which using the profit equation would identify $\eta$), suggest a value of $\eta$ between 0.5 and 0.7 across a variety of cross-sectional, first-differenced and fixed-effects specifications. Regarding the firm share proportions, recall these are from the relevant consideration sets – see section 3.4 for descriptives on firm to firm flows.
ciency wage considerations, non-covered firms are further incentivized to raise wages, which increases \( \varepsilon_{jk}^w \). The entire labor supply curve may be different, such as for firms along a unit circle as in [31] or [30]. The upshot is that the wage-employment locus faced by firm \( j \) provides a range of feasible cross-wage elasticities which under plausible parametrizations are large.

### 3.2.3 Dynamic logit

The standard logit can be adapted to a dynamic setting allowing for worker flows, following a simplified version of the models presented by [50, 123].

This is essentially a repeated static model from above. Periods are joined by a subset of workers taking a fresh random draw of idiosyncratic preferences \( \epsilon_{ij} \) every period from their relevant labor market segment, and reallocating randomly in logit proportions. With probability \( 1 - \lambda \) the worker receives no offers, and with probability \( \lambda \) the worker receives offers from their consideration set \( S \) of connected firms (including itself).\(^9\) \( p_j \) represents the probably of employment in firm \( j \), with corresponding equations as above. Then quits and recruits are given by:

\[
q_j(w_j) = \lambda \left( 1 - p_j(w_j) \right)
\]

\[
R_j(w_j) = \sum_{l \neq j} p_j(w_j) \cdot \lambda p_l(w_l) N
\]

Where \( N \) indicates the total number of workers in the relevant consideration set. These expressions are related to the baseline logit through \( n_j = R_j(w_j)/q_j(w_j) \).

Taking logs, and the derivative of firm \( j \) with respect to a wage change in firm \( k \):

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\(^9\)For simplicity, I consider a self-contained consideration set \( S \). However, in practice and in my empirical implementation the consideration set \( S_j \) is firm-specific, and intersect in unrestricted ways across firms. See appendix version of model.
\[
\frac{\partial \ln(n_j)}{\partial \ln w_k} = \frac{\partial \ln(R_j)}{\partial \ln w_k} - \frac{\partial \ln(q_j)}{\partial \ln w_k} \\
= -\beta \cdot \left( p_k \cdot \lambda p_j \sum_{i \neq j}^{S} (p_i N) R_j \right) + \frac{p_k \cdot \lambda p_j}{q_j} \\
= -\beta \cdot f_{jk}
\]

Where \(f_{jk}\) represents firm \(j\)'s average hires from and quits (flows) to firm \(k\). Firm 
\(j\)'s employment response to firm \(k\)'s wage increase is therefore proportional to \(f_{jk}\), 
the flows between firm \(j\) and firm \(k\). Given that any wage response from firm \(j\) is 
determined by the impact on its own employment, the wage response too is a function 
of \(f_{jk}\).

I assume that consideration sets are persistent. Indeed, [14, 96] show that bi-
lateral worker flow networks are persistent, representing static edges which act as a 
“scaffolding” of worker mobility, in Finland and Mexico. In my empirical implement-
tation, to alleviate remaining concerns of the connected set persistence, I instrument 
post-period worker flows with pre-period flows (and also find high persistence).

3.2.4 Application to collective bargaining

In this paper, I study the particular case of collective bargaining, where a wage 
increase is prescribed for a group of firms that are part of a larger (connected) labor 
market. I largely abstract from the endogeneity of the minimum wage in the main 
analysis, though I discuss this possibility.

My framework relies on a positive net effect on labor allocated to covered firms 
to yield positive wage spillovers. For an individual firm as in figure 3.1 panel A, the 
employment effect is positive as long as the prescribed wage is below the intersection 
point of the marginal cost and revenue curves. Thus the net employment effect will 
depend on how binding the wage agreement is relative to firm productivity for the 
mass of firms, which will vary by agreement and firm type. However, wage spillovers
may still be positive if the net employment effect is negative. In appendix C.6.3, I incorporate into the model queuing for rationed jobs, which would increase labor allocated to firms \( k \) even as employment at \( k \) declines. Insofar as low productivity firms face employment losses, they will tend to temper the wage increases through the classic union spillover mechanism, i.e. wages are pushed up in the covered sector but labor is displaced to the uncovered sector, which pushes down wages there [89]. Note feedback effects on the covered firms will moderate the employment increase, and so a small employment effect in the covered sector, along with a wage increase in the uncovered sector, is consistent with the spillover mechanism.

The non-covered firms are not bound by these wage agreements, and are modeled as above with spillovers proportional to the average quits and hires \( f_{jk} \) to the treated firms. In the empirical implementation that follows, I measure \( f_{jk} \) in the pre-period. In my event study of collective bargaining wage increases, multiple firms raise their wages. An advantage of my measure of spillovers, compared to for example geographical distance, is that worker flows are additive. This allows me to aggregate hires from and quits to any treated firm, where \( k \) denotes the bargaining council \( BC \). My primary measure of connectivity is therefore \( f_{jBC} \). What are plausible values of \( f_{jBC} \)? Medium-sized firms in particular have small sets of connected firms, for example firms with between 10 and 50 workers have on average less than 4 distinct firms which workers separate to in a given year. This motivates the calibration values for the cross wage elasticities above.

The theoretical and empirical link between a firm’s responses in the dynamic logit and its flows \( f_{jk} \) is a key contribution of this paper. It is a flexible measure of the outside options of a worker, which allow for any patterns of industry and geolocation mobility, and thereby identify the magnitude and mechanisms more precisely than previous work on spillovers.
3.3 Context and data construction

3.3.1 South African bargaining council context and literature

Bargaining councils have perhaps been the central institutional feature of the South African labor market since the early 1980s when Apartheid restrictions on Black worker unionization were significantly repealed. Today, there are 39 legally recognized private sector bargaining councils in South Africa, each covering a specific industry-region \cite{73}. I provide institutional details in Appendix section C.8. The relationship between unionization and bargaining councils is intersecting: a large proportion of unionized workers are part of selected industry-regions covered by bargaining councils (since union workers negotiate the agreements), but these bargaining councils extend to many workers which are not unionized and not all unionized are covered. Many formal sector workers are neither unionized nor part of bargaining councils, and a third of all employed are informal, i.e. outside any regulation.

There have been over a dozen studies of union and collective bargaining premia in South Africa, with the earliest by \cite{138, 151, 46}. \cite{171} provide an excellent review of the union premium literature. They document a unionization rate of about 30% in the South African labor market, and estimate a union wage premium of 25-30%. In a related paper, \cite{113} argue that while unions tend to increase wages more for lower wage union members, most union members are in the upper middle parts of the wage distribution for the country as a whole, and this results in an inequality-increasing effect from union wage premia. As I discuss below, I find a similar pattern for bargaining council workers, though spillovers temper this disequalizing effect.

The literature on bargaining councils has been more limited than the union literature, partly because it is difficult to cleanly identify bargaining councils. A contribution of this paper is to compile a publicly available dataset classifying industries and regions into each bargaining council, with wages for each bargaining council separately by year. In addition, the existing papers use household survey data, which
are limited by non-representativeness at the bargaining council level (as opposed to the tax data where I observe the full population of firms).

A major study in this literature on South African bargaining councils is written by [129], who finds negative employment effects which are concentrated among smaller firms (and insignificant for larger firms in the main specification). He uses a spatial regression discontinuity design, identifying the employment effects from either side of the boundaries of bargaining councils. I find similar indications that there are employment losses at smaller firms – but, using the firm characteristics in my data, I also find employment growth at higher productivity firms, suggestive of re-allocation effects (see section 3.7.3). This provides an alternative reading of the evidence that bargaining councils decrease employment in small firms. For example if workers are mostly re-allocating to more productive firms this would increase total production as well as wages, and would be welfare improving. As [103] note, developing countries tend to have too many small firms. The extent to which this is countered by employment losses in small firms is an empirical question, and my results are suggestive of small effects.

Regarding the South African literature on wage spillovers, these have been considered in passing in papers on regulatory policies, such as government-set minimum wage effects [33, 71]. I argue in this paper that such spillovers need careful consideration, since they may be crucial in identifying policy effects. Identifying a clean control group, especially with substantial cross-industry spillovers, may be tricky. To date, it is my understanding that no paper in the South African literature has considered the effects of bargaining councils at the firm level, their spillovers on other firms, or their overall impact on the labor market structure.

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10 As the author notes, this design is sensitive to spillovers, which I estimate to be substantial in this paper. This may bias employment effects upwards, and wage estimates down. Although extensively addressed, it remains unclear the extent to which the endogeneity of bargaining council spatial location biases the results, a key assumption in the design.
3.3.2 Construction of matched panel data

I provide detail on how I construct my main matched firm panel in the Data Appendix C.7, including summary statistics on the firm and individual panels. I summarize this briefly here.

I collect bargaining council agreements from 2008 to 2018, record the industry, location and wage by year for each bargaining council, and match these to firms as demarcated by industry and location in the tax data. I record the annual wages in these agreements using the actual bargaining council wage agreements published in official government gazettes, and also cross-check these wages against a tabulation of wages by bargaining council and year provided to me by the Labor Research Service. I then match these industry-by-location wages to a matched employer–employee data set I have constructed from worker and firm tax records.

In my analysis, I focus on private sector bargaining councils. It is unclear how much the profit-optimization dynamics of wage-setting outlined in the model earlier apply to the public sector, and in addition public sector balance sheet data is not recorded in the tax dataset. Note that workers in the public sector account for a substantial proportion of all workers (about a quarter of the formal sector), as well as of bargaining council workers (about 30%).

The matching between the tax data and the bargaining council agreements is imperfect since there is no direct correspondence between industry and location codes in the two sources. Another source of measurement error is that the tax data lack occupational classification (wages are bargained by occupation), meaning that I only use the “general labor” wage for each year in the bargaining council. However, as will be shown later in an event-study setup, observed wages track large sharp changes in

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11 Regions are defined at different levels, where 21 councils are national in scope, 5 are provincial and 13 are based on districts. In terms of industry, most bargaining councils are defined at the 3-digit industry level, though some are defined at the 2 or 4-digit levels.
the prescribed bargaining council wages, which gives confidence that the identification of bargaining council firms is not too noisy. In my analysis of spillovers, I exclude firms from the broader industry of the bargaining council to guard against contamination from identification error.

3.4 Descriptive data

3.4.1 Descriptives on bargaining council firms

I profile bargaining council firms in my data in appendix C.1.1 and provide a summary here. Bargaining council firms cover 40% of formal sector workers, and account for as much of total revenue. On average, bargaining council firms carry a wage premium of about 15%, and have much less within-firm wage inequality than uncovered firms (columns 4 and 5). These are characteristics consistent with the literature [48], which I show later are causally linked to responses to wage agreement changes.

Bargaining council workers are concentrated mainly in manufacturing, construction, and transport, but are spread across all sectors of the economy. They are mostly in the upper-middle parts of the firm earnings distribution. Part of this is endogenous, since wages are higher due to agreements, but part of this is also because the unconditional value added per worker for bargaining council firms is higher. I also show that bargaining council minimum wages constitute a large part of firm average wages. This ranges from the full wage for the lowest value added firms to about half the firm wage for the highest value added firms.

One concern for this paper in the South African context may be the relevance of wage competition when unemployment is so high. As one indicator, Appendix figure C.3 shows that the share of hires from non-employment declines with the wage paid, including for bargaining council firms. The high average unemployment rate may be a poor proxy for the availability of workers, given the skills requirements of
In addition, the classic job ladder model with search frictions only requires some responsiveness of labor supply to the wage for monopsonistic wage competition to be relevant.

Overall, the matching of agreements into the tax data reveals a profile of bargaining councils that shows higher firm wages, through with wide variation across several characteristics. The next section describes firms locally connected to these bargaining councils.

### 3.4.2 Descriptives of spillover firms

As motivated by my model, I use worker flows to delineate the empirically relevant firm-specific labor market segment for spillovers. A similar flow measure is used to measure spillovers in the product market space by [36]. [12] also uses flows to define labor markets. I use flows as a measure of distance between individual firms, where firms “further away” are less suitable outside options for workers at those firms.

**Worker flows as a measure of distance.** How does the flows measure compare to other measures of distance between firms? For every firm I compute the proportion of flows to every other firm in the data. I estimate the relationship between these firm-to-firm flows and several measures of “distance” in firm characteristics, and include firm fixed effects such that the relationships are averaged within firms. I find strong linear relationships, and in most cases there is a sharp kink in flows at zero distance.

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12A similar point was made by a bargaining council union official in informal discussions.

13In deriving a measure of labor market concentration (akin to the Herfindahl–Hirschman Index or HHI) that is not dependent on discontinuous industry and location boundaries, [12] defines the value of a job in industry-location A relative to a job in industry-location B as the flow of workers from B to A divided by the total number of workers in A. In my case, industry-location A can be considered the bargaining council, and flows are defined at the firm level. The major difference is then dividing by the size of A, which I show does not change the results substantially. Theoretically, the division by size is motivated by that idea that the option value of a job in A for a worker in B may decrease with A’s size since flows are more likely to a larger A, conditional on the same value. In my data, the coefficient in a regression of log wages on log value of the firm (defined following [12]) is 0.1.
indicating that deviation in each firm’s characteristic is appropriate (Appendix figure C.9). Indeed, although sharing the same industry or location are important predictors of flows between firms, sharing many other firm characteristics such as size, AKM wage premia, and the proportion of women are comparably important. Workers move to similar firms along many dimensions.

This demonstrates a key advantage of the flows measure of connectivity between firms. There are dozens of characteristics which determine whether another firm poses a viable outside option for a worker, only some of which are observed by the researcher. Worker flows show the “revealed” outside options, combining all relevant factors. This is a data-driven method that also avoids subjective judgements, such as where to draw an arbitrary industry or geographic boundary around the labor market. In Appendix figure C.4 I show that using typical characteristics such as location and industry are poor predictors of worker flows, a finding shared by [97].

These empirical results also provide some guidance towards economic theory on job search. Firstly, in standard search models, offers are drawn randomly from firms. Yet flows are highly concentrated among firm-specific networks, highlighting the importance of job “consideration sets” from which offers are drawn. Secondly, while job ladder models imply that higher wage firms are more likely to draw workers from firms with much lower wages, here flows are concentrated instead among similar firms. This is illustrated starkly in figure C.9 where the subfigure on firm wage premia (which controls for a worker quality proxy) shows that fewer workers come from firms with different wage premia, including lower-premium firms.

**Firms close to bargaining councils.** What do firms which are “close” to bargaining councils, in terms of high worker flows, look like? While bargaining council firms are concentrated in the upper middle deciles of earnings, spillover firms are distributed much more evenly. A similar pattern appears for industries. The implication
is that there is a lot of movement across firm earnings classes as well as industries — only 30% of switches are within the same 1-digit industry.

Figure [3.2] illustrates concretely the value a flows approach adds, by comparing firms with a high share of bargaining council firms in their same industry-location and firms with high flows to bargaining councils. I show the proportion of bargaining council workers by earnings decile for reference (green). Firms with high share and high flows do account for a substantial portion of workers (dark purple), but many high-share firms actually have low flows (light purpler) and many low-share firms actually have high flows (blue).

I discuss further descriptive results in Appendix C.1.2. There are about as many workers and firms in bargaining councils as in the spillover firms connected to them, and they overlap in characteristics such as wages, churn rates, and profit per worker.

### 3.5 Treatment effects of prescribed wage increases

#### 3.5.1 Empirical design

I test for the causal effects of prescribed minimum wages on outcomes of bargaining council firms by following a stacked event study design, with careful attention on constructing a clean set of controls, stacked data structure, and event period. In addition to being of direct interest, these wage results constitute a first-stage for the spillover effects investigated in the next section.

Bargaining council agreements typically specify wages by ad hoc region-by-occupation cells, where percentage increases are often “across the board” and indexed to inflation. I identify events as large real minimum wage increases, where “large” is defined as greater than 3%. I exclude similarly large increases across the preceding 3 years (to ensure a clean pre-period). There are 47 different events, with 33

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The 3% threshold is arbitrary, and other cuts presented in the heterogeneity section yield similar core results across different average wage increases associated with the events (see figure C.14).
Figure 3.2: Distribution of bargaining council, spillover and other workers, by decile of earnings

Notes. Share refers to the percentage of workers in the same broad industry and location that belong to a bargaining council. Flow refers to the worker flows between non-covered firms and firms in bargaining councils. The figure shows the density of workers by earnings decile in each of the following classifications: “Bargaining council” firms subject to wage agreements; “High share & high flow” firms, i.e. high share of covered firms in the same industry-location as well as high flow of workers to bargaining councils; “High share & low flow” firms, i.e. high share of covered firms in the same industry-location, but low flow of workers to bargaining councils; “Low share & high flow” firms, i.e. low share of covered firms in the same industry-location, but high flow of workers to bargaining councils; and “Other firms”, which include all other formal sector firms not listed above. The sample is all formal sector firms in South Africa from 2008 to 2018.

unique wage increases (some bargaining councils have multiple separately bargained industry-regions), which I combine in a stacked event-study design \([59, 66]\). Appendix figure \([C.14]\) shows the distribution of all real bargained wage increases, concentrated just above 0, as well as the selected event-wage increases.

The control sample contains all firms not covered by bargaining council agreements but in the same calendar year and larger region and industry as the relevant bargaining council. Given that spillovers may increase wages in the non-covered firms (see next section), I exclude non-covered firms with more 1% of worker flows to bargaining councils from the regression sample. I also restrict the sample of bargaining council
and control firms to be balanced across the event-years, and for firms to have at least 10 workers.

My main specification below includes fixed effects for each firm \((\phi_j)\), event by calendar year \((\tau_t)\), location by year \((\theta_{\text{location} \times t})\), pre-event firm size and wage by year \((\gamma_{\text{firmsize} \times t} \text{ and } \alpha_{\text{wage} \times t})\), as well as the pre-event change in log firm size \((\beta_{\Delta \ln \text{firmsize}_{t<1} \times t})\) and log firm wage \((\psi_{\Delta \ln \text{wage}_{t<1} \times t})\). All regressions are unweighted, run at the firm level, and clustered at the level of bargaining council by event (treated and untreated are separate clusters).

\[
y_{j,t} = \sum_{t=-3}^{-2} \delta_t (\tau_t \times \text{treat}_j) + \sum_{t=0}^{2} \delta_t (\tau_t \times \text{treat}_j) + \phi_j + \theta_{\text{event} \times \text{loc} \times t} + \gamma_{\text{firmsize}_{t=-2} \times t} + \alpha_{\text{wage}_{t=-2} \times t} + \beta_{\Delta \ln \text{firmsize}_{t<1} \times t} + \psi_{\Delta \ln \text{wage}_{t<1} \times t} + e_{j,t}(3.4)
\]

For intuition, identification of the main coefficients of interest \(\delta_t\) arises from comparing changes in bargaining council firms to changes in similar firms (in terms of size and wage) within the same location at the same time. Pre-event \(\delta_{t<1}\) is a test of pre-trends up to three years prior, and \(\delta_t\) are all interpreted relative to the outcome in \(t = -1\).

The reason for the rich set of controls is that it is difficult to find control firms with similar pre-trends in firm size, without explicitly conditioning on them. [47] formally discuss conditioning on pre-treatment controls (including outcomes), which is common to many studies. Estimates are valid as long as the conditional post-treatment distributions of counterfactual treated and observed untreated are the same. I show robustness on this in section 3.7.1, including a doubly robust strategy. Note that my controls for pre-treatment change in size and wage do not force \(\delta_{t<1}\) to zero, since the coefficients are common to both treated and untreated firms.
A concern in this paper is the possible endogeneity of the prescribed wage increases. For example, wage increases may be confounded by industry profit booms. A number of tests provide some re-assurance. Firstly, I find that, in my full set of bargaining agreement wages and conditional on my controls, prescribed wage increases are not predicted by pre-event firm characteristics such as earlier changes in profits. Secondly, within the event studies, there do not seem to be any systematic pre-trends in value added or profit margin per worker suggestive of opportunistic timing. Thirdly, there are different responses in firm size by firm value added, indicating that these minimum wages are binding for at least some firms. Finally, this is less of a concern for the spillover analysis that follows, since none of the spillover firms are part of the bargaining process (by definition); in addition, I perform a robustness check (with similar results) using industry fixed effects.

3.5.2 Results

Wages. Figure 3.3 shows large actual wage increases following the prescribed wage changes. The 25th percentile of within-firm wages increases by 4% post implementation, with flat pre trends. The increase is slightly lower for median within-firm wages (3%), and in general decreases as I consider higher percentiles of within-firm wages (though at the 80th percentile, there is still a 2% increase). Figure C.15 shows further wage outcomes, with new hires also paid 2-3% more after the event, again with reassuringly flat pretrends. While these wage responses are largely expected, this is the first dynamic evidence of such effects across the firm distribution for South African bargaining councils. More generally, this shows a positive causal effect of bargained minimums on average wages, which is not clear in other settings \[35, 70\].

\[13\] suggests another issue: core employers that negotiate the contracts may be more accommodating of wage increases than non-party firms to which the agreement is extended. Although we may expect different effects for these two groups of employers, I cannot distinguish between these core and non-party firms. Again this should not bias the spillovers analysis, and such a dynamic may just mean the prescribed wage increases are more exogenous to non-party firms.
Figure 3.3: Effect of prescribed wage increases on wages of bargaining council firms

Notes. The figure shows the main estimates from the event-study evaluating direct effects on covered firms from 47 bargaining council wage increases between 2011 and 2016 (see Equation 3.4). The regression is run at the unweighted firm-level, restricted to balanced firms with more than 10 workers in the pre-period, and excludes firms with more than 1% of worker flows to covered firms from the set of control firms. Standard errors are clustered at the level of bargaining council treatment by event. Panel A shows the estimated effect on the 25th percentile of within-firm log wages for each event-period. Panel B shows the final-period coefficients from separate event-study regressions estimated for each decile of within-firm log wages.

These wage increases vary substantially by the pre-event wage of the firm: at lower quantiles of average firm wages, there is little impact of the prescribed wage increases, but the effect is higher than average for mid-waged firms (40-70th percentiles) showing point estimates of 5-7% wage increases. The wage increases are lower for the top quantiles, perhaps because the prescribed minimum wage increases are less binding for these higher-wage firms. There is a very similar pattern for wage increases by firm size: for the smaller firms, up until about 15 workers, there are no statistically detectable wage effects, whereas for mid-sized firms between 15 and 100 workers, there appear to be large effects. The increases are once again lower and not statistically significant for the largest firms.\textsuperscript{16}

\textsuperscript{16}The low response for low-wage and small firms may be due to exemption clauses in several bargained wage agreements for smaller firms, and due to the institutional enforcement of these wages – inspectors are more likely to be called by unionized firms, and small firms are less unionized.\textsuperscript{19}
**Other outcomes.** I show other firm responses to bargained wage increases in figure C.16. Average separations decrease by about 2% at the event year, after flat pre-trends. Interpreted directly, this implies a firm labor supply elasticity of about 1.5 which suggests considerable monopsony power in line with [21]. However, this is very likely biased as an estimate of a reduction in turnover since I cannot differentiate between voluntary quits and involuntary fires. Indeed, average firm size does not show detectable changes, with a confidence interval between $-0.02$ and $0.01$. In addition, as pointed out by [28], a labor market with spillovers complicates the relationship between the reduced form and structural labor supply elasticity. The own wage elasticity, i.e. the firm size effect divided through by the wage effect, is $-0.09$ (standard error of $0.26$).

Unemployment insurance payments increase strongly, with flat pre-trends and an increase of about 1.5%. This is mechanical in theory, as UI payments are a percentage of wages, subject to a ceiling. As we noted in figure 3.3 above, wages increase most at the bottom of each firm, and so within-firm wage inequality decreases: the gap between 80th and 20th percentile of wages decreases sharply by 2-3 log points, following a flat pre-trend. Finally, there do not seem to be any systematic pre-trends in value added or the profit margin per worker, which is re-assurance that these estimates do not carry substantial bias from possible endogeneity of bargaining agreements to prior firm performance. In addition, the point estimates on the profit margin effect are close to zero, in line with the model prediction that, as wage-setters, the first order effects on profits of wages changes are negligible; however the standard errors are large and so this can only be suggestive.

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17 Using the 3% wage effect, yielding a separations elasticity of $-75$, and the formula in [130] that the firm labor supply elasticity is -2 times the separations elasticity.
3.6 Spillover effects of prescribed wage increases

3.6.1 Empirical design

To quantify the untreated firm wage responses, or spillovers, a key question is how the treatment dosage is defined. I follow the model-based measure of spillovers in section 3.2 above, and specifically the term $f_{jk}$ in equation 3.3. Intuitively, if the same workers are employable at different firms, this set of firms defines a fluid local labor market. Labor constrained firms are competing strategically over this same labor pool, meaning that wage spillovers operating through the labor market should transmit through this channel of worker flows.

In terms of specification, I follow much of equation 3.4 used for bargaining council effects, except I replace the main variable of interest (previously the bargaining council treatment indicators) by $\tilde{f}_{j(c)}BC$ for every firm $j(c)$ where $c$ is the industry-location cell. That is, for each each industry-location cell, I take the average proportion of worker flows between each uncovered firm and the relevant covered firms in the event-study pre-period, denoted $f_{j(c)}BC$. For interpretability, I then normalize these flows by the top decile of connected firms such that $\tilde{f}_{j(c)}BC = f_{j(c)}BC/\bar{f}_{j(c)}BC$ (where $\bar{f}_{j(c)}BC \approx 0.1$). This flow measure represents the treatment dosage.¹⁸

\[
y_{j,t} = \sum_{t=-3}^{-2} \delta_t (\tau_t \times \tilde{f}_{j(c)}BC) + \sum_{t=0}^{2} \delta_t (\tau_t \times \tilde{f}_{j(c)}BC) + \phi_j + \theta_{event \times loc. \times t} + \gamma_{\text{firmsize}_{t=-2} \times t} + \alpha_{\text{wage}_{t=-2} \times t} + \beta_{\Delta \ln \text{firmsize}_{t-1} \times t} + \psi_{\Delta \ln \text{wage}_{t-1} \times t} + e_{j,t} (3.5)\]

¹⁸The event-sample is of firms in the same location as treated firms, as well as non-treated firms that are in the same flow-estimated labor market. I cluster firms by network, and include a network if any treated firm is a part of it (except for the five largest networks, which are less meaningful). The aim is to avoid excluding firms that are connected to treated firms but outside of the immediate geographical location.
Identification now arises from variation in pre-event connectivity: comparing non-treated firms of varying degrees of connectivity to bargaining councils but within the same location and of similar firm size. That is, do non-covered firms that are more strongly connected to treated firms exhibit outcome responses to the prescribed wage events? I also estimate a binary version of equation 3.5 where I compare highly connected firms (greater than 10% flows) with unconnected firms.

As in the earlier specification, I make sure to exclude contaminated controls. I exclude firms that have low connectivity to the local bargaining council, but high connectivity to another bargaining council (perhaps a different industry in the same location). [104] cautions about two other sources of bias when estimating spillovers. Firstly, mismeasurement in the treatment status can upwardly bias spillover estimates. I am careful to exclude potential bargaining council firms in these regressions, by excluding all firms from the spillover regression that are in a similar industry to the bargaining council. Secondly, [104] also cautions that not accounting for other sources of spillovers may cause bias too. One test is to check for spillovers where the model predicts none: I test for spillovers in low-connected firms, finding points estimates very close to zero (see appendix figure C.19).

I use a split-sample IV strategy for the main estimates to reduce measurement error in the $\tilde{f}_{ij(c)BC}$ variable. As a generated regressor there will be noise in this variable compared to the true value of the firm connectivity, and this will attenuate the coefficient towards 0. I avoid this attenuation bias by randomly splitting firms in each industry-location, and instrumenting the average flow for the firm’s own sample.

\footnote{For example, if a bargaining council is defined by the 3-digit industry code, I exclude all firms in the same 2-digit industry code. This means that adjacent 3-digit industry codes that may be included in bargaining agreements do not enter the regression. Since the regressor is a continuous variable of flow connectivity, this should not affect the magnitude of the estimates if the effects are broadly linear in the flows.}
by the average flow for the complement sample within each industry-location. The split sample instrument has been used for example by [23, 92].

3.6.2 Results

**Wages.** Figure 3.4 shows large spillover effects from prescribed bargaining council wages on uncovered firm average wages, with further wage outcomes given in Appendix figure C.17. To interpret this, the figure shows that average wages in industry-locations with an average of about 10% pre-event worker flows to the relevant bargaining council experienced an average increase of nearly 4% at the 25th percentile of within-firm wages, and 3.5% at the 50th percentile of within firm wages. This is as large as the comparable wage effects on directly treated firms, suggesting a cross-wage elasticity for highly connected firms of over 0.8. However, for a firm with only 1% flows to the bargaining council, median wages increase by 0.35%, demonstrating the drop-off in spillover responses by connectivity to covered firms.
Figure 3.4: Effect of prescribed wage increases on wages of uncovered firms

Notes. The figure shows the main wage estimates from the event-study evaluating spillover effects on uncovered firms from 47 bargaining council wage increases between 2011 and 2016 (see Equation 3.5). The regression is run at the unweighted firm-level, restricted to balanced non-covered firms with more than 10 workers in the pre-period, and excludes firms in the same industry as the bargaining council. Standard errors are clustered at the level of 3-digit industry by location by event. I use a split sample approach to reduce measurement error in the regressor, where the average worker flows to bargaining councils of randomized firms within local labor markets is instrumented by the average flows at the complement set of firms. Panel A shows the estimated effect on the log firm 25th percentile wages using the continuous regressor for each event period. Panel B shows the same outcomes using a binary version of the continuous regressor. Panel C shows final-period estimated effects on log wages, by decile of wages within each firm. Panel B shows the final-period estimated effects on log wages, by decile of wages across different firm using a RIF regression.

I also show wage spillovers under a binary treatment specification, with similar results. The two-year out wage spillovers are substantial and significant across the distribution of within-firm wages, with smaller effects for better paid workers within these firms. The p80-p20 within-firm wage gap declines by nearly 4%, consistent
with earlier studies on union spillovers \cite{88, 110}. There are also substantial effects for spillover firms across quantiles of the between-firm wage distribution, with similar patterns to what I found for bargaining council firms: the effects are not statistically significant for the lowest or highest wage firms, but the wage effects reach as high as 10\% in the middle waged firms. The wage effects are also strongly concentrated among the mid-sized firms, with the figure showing much larger effects for firms with 25 to 100 workers.

**Other outcomes.** Figure 3.5 shows other firm outcomes, with further outcomes shown in figure C.18. Firm size may decrease, with a point estimate of −2\%, but this is not statistically significant. The own wage elasticity, i.e. the firm size effect divided through by the wage effect, is −0.2 (standard error of 0.28). As for bargaining council firms, unemployment insurance payments increase strongly, up by 2\% by the final event year. For each of these outcomes, including the wages, the effect appears strongest in the final event year. This implies some kind of lagged response, as wages increase by the first year after the event in bargaining council firms, and may take a year to propagate outwards through worker flows.

Once again, the profit margin per worker does not exhibit pre-trends, which is reassurance against differential prior firm performance driving these results. The post-period decline in profits for these firms highlights a potentially sharper trade-off between profits and wages for spillover firms than for bargaining council firms, as found in an earlier study \cite{41}. This is consistent with the theory, that first order effects of wage *externalities* are negative, even if first order effects of own-wage changes on profits are negligible by the envelope theorem. Such a trade-off between profits and wages is also consistent with prescribed wage increases which tend to be about splitting rent \cite{74}.\footnote{How plausible is the observed trade-off? I perform a counterfactual simulation where I increase each firm’s wagebill by 3\%, and then reduce firm profits by the same amount in absolute terms.}
Figure 3.5: Effect of prescribed wage increases on other outcomes of uncovered firms

Notes. The figure shows the estimates of non-wage outcomes from the event-study evaluating spillover effects on uncovered firms from 47 bargaining council wage increases between 2011 and 2016 (see Equation 3.5). The regression is run at the unweighted firm-level, restricted to balanced non-covered firms with more than 10 workers in the pre-period, and excludes firms in the same industry as the bargaining council. Standard errors are clustered at the level of 3-digit industry by location by event. The regressor is the I use a split sample approach to reduce measurement error in the regressor, where the average worker flows to bargaining councils of randomized firms within local labor markets is instrumented by the average flows at the complement set of firms. Panel A shows estimated effects on the log of the number of workers per firm for each event period. Panel B shows estimated effects on the log of the profit margin for each event period, which is defined as each firm’s total profit over their total value added.

Appendix figure C.18 shows effects on the composition of new hires, which tend to come more from other firms rather than non-employment. This is consistent with the framework in this paper, where firms operate in a monopsonic local environment with some degree of wage competition over employed workers. Although general unemployment in South Africa is extremely high, unemployment relevant to these

While this exercise omits several dynamic considerations such as adjustments in firm size, changes in composition, or effort effects, it is re-assuring that the implied reduction in profit per worker is 3.3%. This is not far from the reduction estimated for spillover firms, which ranges from 3% to 7%. Note Figure 3.5 shows the profit margin, not profit itself. Another way to explore the plausibility of the profit reduction is by considering theoretical predictions: broadly, one may expect the profit-wage trade-off to be proportional to the capital-labor ratio, as proxied by the wage share. At about 50%, this suggests close to a one-to-one trade-off consistent with the effects estimated for wages and profits above.
firms may be lower, and these figures show a response along the margin of competition over workers.

Finally, spillover wage effects are gradually increasing by quantile of flow connectivity, such that changes in wages rise with connectivity (appendix figure C.19). This serves as a placebo or falsification test similar to [58], who test for minimum wage effects on populations that should not be affected. Here, firms with low flows to bargaining council firms show negligible wage effects, as predicted.

Altogether, these results provide strong evidence that spillovers exist, that they operate through local labor market networks of worker flows, and that they are substantial in magnitude for “nearby” firms.

3.7 Additional findings
3.7.1 Robustness

I summarize notable robustness analysis below, with tests concerning respectively the bargaining council analysis, spillover analysis, and alternative samples and specifications relevant to both sets of analyses. Further details are provided in the appendix.

Bargaining council firms

Aside from the main outcomes described above, a few others are worth mentioning. Annualized wages exhibit a similar event-time profile to the raw wages in the main specification. The AKM worker fixed effects of new hires appears to increase, suggesting some compensation in worker quality. And the director’s salary decreases sharply, though this variable is only observed for a small set of firms.

I provide further discussion and tests in Appendix C.4.1. Firstly, the wage effects are not sensitive to the exclusion of the pre-period controls, though the firm size effects are. Secondly, when we fail to exclude contaminated controls, the wage effect attenuates to 3%; we can actually use these estimates to back out a rough indirect
estimate of spillovers.\footnote{Noting that there are about 20,000 bargaining council firms, and a further 25,000 high spillover firms, the decrease in wage effect from 4% to 3% implies a spillover wage effect of about 2.2%. This is very close to several of the direct estimates of wage spillovers.} Lastly, using a simulation of perfect compliance with the minimum wage, I show that the observed wage estimates follow the prescribed wage changes quite closely.

**Spillover firms**

I provide further discussion and tests in Appendix C.4.2. Figure 3.6 shows noteworthy wage estimates.

*Alternative controls.* The main results are robust to controls for the firm labor churn rate, and industry fixed effects, the latter of which allays concerns about industry-level dynamics that may be correlated with the bargaining council. Here the identifying variation comes from firms within the same industry that are differentially connected to the bargaining council, for example due to location. I also check sensitivity to the pre-event trend control, such that I control for the difference in pre-event firm wage and size across event periods $-3$ and $-2$ instead of $-3$ and $-1$. In the full event study, event-time $0$ still has small, insignificant estimate of $-0.004$ (SE of $-0.008$).

*Alternative specifications.* The results are similar when using as the regressor the post-period worker flows instrumented by the pre-period worker flows. This relieves the concern that, as flows change in response to the bargaining council wage increase, pre-period flows do not dynamically represent workers’ outside options. In reality it makes little difference, and the first stage between post- and pre-period flows is 0.77. The results are robust to using a completely different regressor, the Input-Output (I-O) proportion of trade connections between the relevant sectors of firms. To test if the results are driven by the specification directly, rather than the identifying variation...
in worker flows to the bargaining council, I randomly scramble the spillover flow regressor across firms as a placebo, and find a precise zero effect.

Figure 3.6: Alternative estimates of two-year out spillover wage effects

Notes. The figure shows alternative estimates from the event-study evaluating spillover effects from 47 bargaining council wage increases between 2011 and 2016 (see Equation 3.5). Each estimate is the final period effect on firm median wages for the relevant specification. “Main” is the main set of results. As alternative controls: “Add churn rate” and “Add industry” include the firm’s churn rate and industry fixed effects respectively as controls in the main specification; and “Alt pre-event trend” uses as a control the pre-event trend between periods \(-3\) and \(-2\) instead of between \(-3\) and \(-1\) as in the main specification. As alternative regressors: “Post- on pre-flow IV” instruments the post-period worker flows be the pre-period worker flows, instead of the split-sample approach focusing on the pre-period as in the main specification; “I-O prop. trade” uses the proportion of trade between firms as given by sectoral Input-Output tables; and “Scrambled flows (placebo)” randomly re-assigns worker flows to firms in the main specification. As binary regressors: “IV” defines highly connected firms as the treatment, and maintains the split-sample approach of the main specification (normalized for comparability to the “Main” IV estimate); “OLS” omits the instrument; and “Industry-geo share” defined treatment as uncovered firms in industry-geographies with a high share of bargaining council firms.

Binary regressors. Finally, a similar set of results come from a binary treatment definition, where the instrumented split samples are the set of highly connected firms.
This is robust to using OLS instead of IV, though with substantial attenuation. If instead I define treatment as nearby industries and locations, comparable to previous work, I find no wage effects. As a slightly less crude measure, I define treatment as a high share of bargaining council firms in industry-locations, and this gives an attenuated estimate with large standard errors.

**Alternative samples and specifications**

I provide further discussion of tests relevant to both sets of analyses in Appendix C.4.3. Figure 3.7 shows noteworthy wage estimates.

*Alternative samples.* Firstly, lagged dynamic effects of previous agreements may bias estimates, so I restrict to events without large wage increases across event years -4 to -3. Half the events are excluded, but the pre-trends are flatter. Secondly, agreements themselves may be endogenous to local economy trends, so I restrict to those negotiated at the national level. Thirdly, firms with between 20 and 100 workers have the largest effects.

*Alternative specifications.* As robustness on the regression matching based on pre-period covariates, I use propensity score re-weighting on pre-period firm characteristics instead of regression-based controls, and follow [10] by including both regression controls and propensity weights. This addresses concerns with either propensity score or regression matching. Secondly, I allow the regression controls to vary by event, resulting in cleaner pre-periods. Thirdly, I weight by per-period firm size for an indication of worker-level aggregate effects. Fourthly, I find similar effects at the worker-level, where I restrict to workers with bottom-tercile wages in the pre-period, and control only for fixed effects by individuals, event-year demographics and location. This shows the firm-level wage effects are not purely about worker composition or differential wages for new hires. Lastly, I check the extent to which the pre-period
coefficients preclude pre-trends (including non-linearities) that alter the post-period estimates, following [147].

Figure 3.7: Alternative estimates of two-year out wage effects on covered and uncovered firms

Notes. The figure shows alternative estimates from the event-study evaluating direct treatment effects on bargaining council firms (see Equation 3.4) as well as spillover effects on uncovered firms (see Equation 3.5). Each estimate is the final period effect on firm median wages for the relevant specification. “Main” is the main set of results. As alternative samples: “Cleaner pre-period” excludes events of bargaining councils which had a large change in prescribed wages in event periods −4 or −5; “National agreements” restricts to events of bargaining councils which negotiate wage agreements at the national level; “Mid-size firms” restricts to firms with pre-event firm size of between 20 and 100 workers. As alternative specifications: “Double robust” weights the main specification by the propensity score, estimated on pre-event firm characteristics; “Event specific cont” interacts all controls in the main specification with each event; “Size-weight” weights the main specification by firm size; “Worker level” is a worker level regression, with the same regressors, restricting to workers with bottom-tercile wages in the pre-period, and controlling only for fixed effects by individuals, event-year demographics and location.

Across these checks, the wage effects for both bargaining council and spillover firms suggest a stable but high range of cross-wage elasticities. On the other hand,
the impacts on firm size are consistently insignificant, and include positive point estimates. The results on separations and profits are more consistently negative and significant, though with a few exceptions.

3.7.2 Heterogeneity

I discuss heterogeneity in these main results, with the purpose of relaxing the constant treatment effects imposed in the event-study design. There are several model-consistent reasons for heterogeneous treatment effects, which I discuss in section C.4.4. I highlight two sets of result here.\footnote{Estimates on heterogeneity are not quasi-experimentally motivated, meaning that these patterns should be taken as suggestive; in addition, since the sample is reduced, power decreases, and since more tests are run, some statistical anomalies are more likely. Indeed, many of these differences are within each other’s standard error bounds as figure C.32 illustrates for wages.}

I consider heterogeneity by the Kaitz index, that is, the minimum to local median wage ratio. In monopsonistic labor markets, a minimum wage set just above the current wage has positive employment effects whereas a wage set far above will be more negative (see section 3.2). I restrict to national bargaining councils, and test for differential effects within each event by the Kaitz index. For firms with a low Kaitz index, the wage effects are more muted, but with little change in firm size (narrow confidence intervals). On the other hand, for high Kaitz index markets, there are stronger effects, but in addition the own-wage employment elasticity is larger and more negative. This suggests there are constraints to minimum wage interventions even in monopsonistic markets.

Secondly, one measure of the characteristics of the connected set of the firm (see section 3.2), is to look at the highest share of workers flowing from each firm to each other firm on a consistent basis. I find that firms with a low share have lower wage spillovers, which is consistent with the idea that firms with more hiring options need
to raise wages less. I find similar results when considering heterogeneity by the total number of distinct firms connected to each firm, as measured by worker transitions.

### 3.7.3 Re-allocation

In the modeling section 3.2, I treated the effect on total bargaining council employment $p_k$ as largely homogenous, with some discussion in subsection C.6.3 of differential effects by firm productivity. Although the average effect on bargaining council firm size is imprecisely estimated, I show that there are significant decreases in employment for low wage firms, and provide suggestive evidence that these are offset by increases in firm size for higher wage firms.

Theoretically, re-allocation of workers from low wage to higher wage firms can occur as follows. High-wage firms are labor constrained and so an increase in their wages leads to expansion; while low-wage firms have low productivity, cannot pay the minimum wage, and so are forced to downsize. These need not be exactly the same workers who transfer from low-wage to higher wage firms, especially if the connected sets of these respective firms do not overlap. These dynamics are recorded elsewhere in response to minimum wages, for example Germany [79] and Sweden [45].

Figure 3.8 shows the results from a regression which uses the main bargaining council specification 3.4 by decile bin of firm value added (in the pre-period). The outcome is the number workers in the firm in each period as a percentage of the pre-period labor force. The figure shows reductions in employment for the lowest value added firms, but increases in employment for the highest value added firms. The difference in employment effect for above- and below-median firms is statistically significant at the 1% level. I show robustness on this in Appendix C.4.5.
Figure 3.8: Event-study effects on number of workers, by decile of value added per worker

Notes. The main specification and sample for effects on bargaining council firms are used (see equation 3.4), with the following modifications: Event-periods are collapsed into the pre- vs post-period, and the primary treatment indicator is interacted with an indicator for each decile of pre-period firm value added per worker (the omitted category is the 5th decile). The main outcome is the count of workers in each firm, as a percentage of the pre-period labor force. The green bars show the estimated coefficients for each decile, with thin green bars showing the corresponding confidence intervals. The red line shows the cumulative employment effects by adding up the coefficients from the lowest to highest deciles.

This should also give an approximation of the aggregate effect on worker employment, rather than firm-level regressions shown previously. The total employment effect is positive and significant, though the magnitudes are small (total effect is 0.12 of a percentage point). In addition, in Appendix figure C.36 I show suggestive evidence of neutral aggregate employment effects on informal sector workers and unemployment. Note that there are positive aggregate productivity effects, as workers move towards higher value added firms.
3.8 Discussion

3.8.1 Aggregate labor market effects

How important are the bargaining council wage increases for the aggregate earnings distribution, considering both the direct and spillover wage effects? As table C.2 shows, the number of workers and firms in bargaining councils and their highly connected sets are similar; together with the similar cross-wage effects, this suggests that spillovers may be as important for the earnings distribution as the direct effects on bargaining councils.

Figure 3.9 plots counterfactual wages by AKM worker quantile, constructed through a microsimulation based on firm characteristics and the estimated effects of wage agreements. The figure shows in blue the direct effect of bargaining councils on the aggregate earnings distribution, on average about 5%. The largest effects are for the mid-quantile workers, with the smallest effects at the top of the worker distribution. The spillover effects more than double the total impact of these bargaining council wages, adding about 7% to the wages of all worker quantiles. 87 find very similar effects of incorporating spillovers. These spillover effects are more evenly spread through the distribution, reflecting the location of spillover firms as shown in figure 3.2. Similarly, due to the mix in locations of bargaining council and spillover firms along the firm earnings distribution, the effects on inequality are negligible in this simulation. Appendix C.5 gives further details, including similar results when accounting for employment effects or within-bargaining council spillovers. Overall, this microsimulation suggests bargaining councils increase average wages by over 10% across the distribution, and neglecting to account for spillovers would make the bargaining council effects appear to be much smaller.
Figure 3.9: Microsimulation of prescribed wage effects on wage distribution

Notes. The figure simulates the wage effects of bargaining council wage agreements on the overall wage distribution, by quantile of the AKM worker fixed effect. The baseline of 0 represents the observed wage distribution. The blue line shows counterfactual wages based on adding in the bargained wage increases between 2008-2018 relevant to each covered worker. The red line shows counterfactual wages based on the blue line plus the implied spillovers relevant to non-covered workers, as estimated using the flows from each firm to bargaining councils along with an estimated cross-wage elasticity of 0.8.

What would the wage spillovers look like if there were fewer frictions limiting workers’ consideration sets? Figure C.39 shows a counterfactual where non-treated firms have flows to bargaining councils at least equal to the share of bargaining council workers in their industry-location. This seems like a reasonable counterfactual consistent with the model above. The aggregate effect on the wage distribution increases by 4 percentage points, suggesting substantial gains to lowering labor market frictions.
3.8.2 Concluding thoughts

This paper demonstrates the direct and indirect impact of collective bargaining on the labor market. I find that, following a large wage increase mandated in bargaining council agreements, observed bargaining council firm wages increase. Firms that are strongly connected to the same local labor market as these bargaining council firms see wage increases of a similar magnitude, together with a decrease in profit margins. A simple simulation suggests that such spillover effects double the direct impact of these bargaining council agreements. Together with the evidence of re-allocation effects, this highlights the broad ranging impact of institutional regulation on the aggregate wage structure.

The methodological contribution of this paper is to ground the measure of spillovers in a view of the labor market that has monopsonistic competition and localized labor markets, as supported by recent empirical work. I present a static model of wage transmissions, consider this in a dynamic context, and derive a corresponding empirical measure of spillovers. While this spillover mechanism focuses on the flow of workers connecting firms through overlapping local labor markets, there are several complementary mechanisms such as norms of fairness, or union threat effects that may be explored in future work. Such mechanisms may be important in rationalizing wage spillovers when there are negative net employment effects from covered firms.

These findings on spillovers occur in a global context of declining union density. Jobs polarization has been widely documented across several countries [93], and as for South Africa, [64] notes “there was almost no employment growth among middle-income machine operators and assemblers, which dealt a terrible blow to the gains made by the unionised black working class.” The spillovers across industries, and on low-wage workers that are not well-unionized, is an important consideration for this literature. In general, the labor market dynamics discussed in this paper highlight the potential power of regulation, whether in the form of minimum wages or collec-
tive bargaining, to influence the wage structure in a monopsonistically competitive labor market. Collective bargaining councils or wage boards are a popular policy recommendation to constrain monopsony power \cite{75,160}, and South Africa’s collective bargaining councils are thus an illuminating example. Altogether the propagation of centralized wage regulation through connected labor markets provides a possible institutional lever for reversing the trend of rising between-firm inequality in several countries \cite{57,155}.
APPENDIX A

SUPPLEMENTARY MATERIAL: MONOPSONY IN MOVERS
### A.1 Additional Tables and Figures

Table A.1: **Relationship between AKM Wage components and separations**

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<th>(2) Firm Worker</th>
<th>(3) Firm Worker Match</th>
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<tr>
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<td>-1.342</td>
<td>-0.739</td>
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<td>(0.016)</td>
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<tr>
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<td>(0.075)</td>
<td>(0.07)</td>
<td>(0.014)</td>
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<tr>
<td>E-E recruits</td>
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<td>0.423</td>
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<td>(0.05)</td>
<td>(0.013)</td>
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<tr>
<td>Pct. EE-recruits</td>
<td>0.464</td>
<td>0.482</td>
<td>0.482</td>
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<tr>
<td>Labor Supply Elasticity</td>
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<td>1.496</td>
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<td>(0.185)</td>
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<td>Obs (millions)</td>
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**Regressors**

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

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<td>Pct. EE-recruits</td>
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**Firm FE from**

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</tbody>
</table>

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

Note: The legend indicates origin quartile to destination quartile, where quartiles are defined along the distribution of the average firm wage, using only workers who stay at the firm over the period. The change in wage is shown for movers, who are defined as workers who make a job-to-job transition at any point over the period and are observed for at least 9 consecutive quarters at the same firm before and after. The quarter of separation and the following quarter are omitted since these represent quarters that were partially worked, and are particularly susceptible to measurement error in wages. This exercise is repeated for each 6-year period (2000-2005, 2006-2011 and 2012-2017), the mover wage profiles are stacked, and the averages of the event quarter by quartile-transition categories are plotted. The thickness of the lines is proportional to the number of job-to-job separations between the relevant quartiles over the full panel 2000-2017 (not restricting by tenure). Low quartile firms have much higher job-to-job separation rates as indicated by the thickness of the lines than the high quartile firms. Moreover, the flows are not symmetric: more workers move from low to high wage quartiles (red solid lines) than vice versa (blue dashed lines), which is consistent with high quartile firms being higher rent jobs. The asymmetric flows across quartiles capture the separations elasticity: increases in wages have more separations than decreases in wages. This figure shows simultaneously the lack of wage changes prior to a move (flat pre-move trends), the effects firms have on wages (the magnitude of an individual wage change after a move) and that the volume of flows between firms are correlated with those effects (the thickness of the lines). Together this suggests that firm wage policies may be identifiable from switchers, even as they influence the direction and volume of switching.
Figure A.2: Symmetry plot of log wage changes for quartile-to-quartile transitions

Note: The figure shows the quartile to quartile log wage changes corresponding to the quartile transition event study above. Upward mover indicates that the worker moved from a lower quartile to a higher quartile; downward mover indicates the worker moved to a higher quartile. For example, the point labelled ‘Q1 and Q4’ shows the average log wage change for movers from quartile 1 to quartile 4 on the horizontal axis, and for movers from quartile 4 to quartile 1 on the vertical axis. The dotted line shows the 45 degree (negative) slope from the origin: symmetric downward and upward log wage changes would lie on this line.
Figure A.3: Job-to-job separations and firm wage effects

Note: The figure illustrates the split sample approach using a control function. Residuals are calculated from a regression of own-sample firm effects on the complement-sample firm effects, and used as a control in a regression of E-E separations on own-sample firm effects. The plotted points show the binned scatter points of this latter regression (i.e. depicting the partial correlation). The vertical axis is E-E separations divided by mean E-E separations such that the slope of the line represents the elasticity. The blue points represent quantiles of the trimmed sample, which excludes the top and bottom 2.5 percent of the firm effects distribution. The red points represent quantiles of the excluded sample only, which we consider outliers. The trendline is a cubic polynomial fitted to the trimmed sample.
Figure A.4: **Job-to-job hires and firm wage effects**

Note: The figure illustrates the split sample approach using a control function. The plotted points show the E-E hires against own-sample AKM firm effects, while controlling for the residuals from a regression of own-sample firm effects on the complement-sample firm effects. The sample is restricted to observations corresponding to hires. The vertical axis is E-E hires divided by mean E-E hires such that the slope of the line represents the elasticity. The blue points represent quantiles of the trimmed sample, which excludes the top and bottom 2.5 percent of the firm effects distribution. The red points represent quantiles of the excluded sample only, which we consider outliers. The trendline is a cubic polynomial fitted to the trimmed sample.
Figure A.5: **Labor supply elasticity and firm wage effects**

Note: The figure illustrates the split sample approach using a control function, for the labor supply elasticity estimated at the firm (not worker) level. The plotted points show the weighted average of log firm E-E separations, log firm E-N separations and log firm E-E hires against the AKM firm wage effects. The residuals from a regression of own-sample firm effects on the complement-sample firm effects are controlled for. The slope of the line represents the labor supply elasticity, where the reported coefficient corresponds to the fitted bins. The sample is restricted to the trimmed sample, which excludes the top and bottom 2.5 percent of the firm effects distribution. The trendline is a cubic polynomial fitted to the trimmed sample. Points are plotted at the firm level and weighted by firm size.
Figure A.6: **Firm separations versus recruits**

*Note:* The data is plotted at the firm level, with quarterly separations and recruits calculated as a proportion of firm size by firm for each 6 year period. Points are plotted at the firm level and weighted by firm size. Firms are classified as outliers in this figure if they are in the top or bottom 5% tails of the firm separations distribution. The 45 degree line from the origin indicates equal separations and recruits. The dashed vertical lines indicate the interquartile range (p25 and p75 of the separations rate).
Figure A.7: Job-to-job re-separations and wages

Note: The plotted points are restricted to the first 16 quarters after initial separation from the origin firm. The vertical axis indicates the probability of E-E separation from the intermediate firm, divided by the average E-E separations. The figure shows the instrumental variables relationship between E-E separations and change in log own wage, using a control function, i.e. controlling for the residuals from a regression of change in log own wage on change in log firm wage. The specification includes fixed effects for interacted calendar time by origin firm by worker tenure at origin firm (8 bins) by initial wage at the origin firm (8 bins), and are clustered at the level of origin firm by calendar time. See text for sample construction.
A.2 Data

This is supplementary material to the data description in the main text. Our data sample covers the period 2000-2017. Oregon experienced recessions in 2001-2002 and 2008-2009 along with the rest of the country: the 2008 recession features prominently with a sharp rise in the unemployment rate and an ensuing decline in the labor force participation rate (see figure A.8). We explain in detail the construction of the main sample, present summary statistics, and plot the inequality trends in Oregon using our administrative hourly wage data.

The primary variables in the data by quarterly record are the calendar quarter date, the worker identifier unique to each worker, the firm identifier (where each firm identifier may be associated with multiple establishments within Oregon), number of hours worked in the quarter, the total earnings paid to the worker for the quarter. We also observe the industry of the worker (recorded as a NAICS code), and the location (recorded as the FIPS code\textsuperscript{1}, though these are only used for heterogeneity estimates and controls for some robustness checks.

A.2.1 Sample Construction

The data were cleaned in the following order, with corresponding summary statistics shown in table 1. We attempt to follow the literature using matched employer-employee data as exemplified by \cite{57,121,122,155,156}.

\footnote{The county of many workers is missing for a large proportion of the records; additionally due to data limitations restricting the link between specific establishments and workers, the Portland metro zone estimates allocate workers to a zone if at least 90 percent of the employees of their firm are working in a single zone.}
Figure A.8: Oregon employment, 2000-2017

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

1. We begin with records which are uniquely identified by worker-firm-quarter from 2000 quarter 1 to 2017 quarter 4. 136 million such observations exist, corresponding to 317,000 different firms, and 5.3 million workers.

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

3. We drop entire employment spells with

(a) Less than 100 hours per quarter on average over the employment spell, which is equivalent to less than 8 hours per week. This helps to exclude extremely irregular part time work, and is similar to one of the few other studies that observe hourly wages: [121] drop workers who workers fewer than 400 hours in the year. The number of observations decrease from 136 to 120 million.

(b) Hourly wage less than $2 (in 2017 dollars) in any quarter over the employment spell, because it is difficult to imagine a reason this may apply to a regular worker aside from measurement error – this only drops 1 million observations. This restriction is similar to [121] who drop workers with hourly wages below $2 (2005 dollars). [57, 121, 156] drop workers with annual earnings below about $3,000, which for a 40-hour workweek corresponds to $1.50 per hour (both well below the federal minimum wage). [155] restricts to workers earning the equivalent of minimum wage for 40 hours per week over 13 weeks, and [122] restrict to workers earnings $15,000 per year.
(c) Fewer than 3 quarters in length, which drops an additional 9 million observations. This ensures that there is at least one full quarter observation (aside from hiring and separation quarters), giving at least one reliable hourly wage per worker-firm match, which is essential for our analysis. In a similar vein, [156] restricts to at least 2 quarters.

4. We then convert to a worker panel. For any worker-quarter, we keep the observation which belongs to the spell with the highest ave earnings – this corresponds to a dominant employer and keeps spells intact. Note that a separation is still counted if a worker’s spell was cut off. [122, 57, 155, 156] share this restriction of selecting the highest earning observation for a worker-quarter. We further exclude workers with more than 9 different employers in any year, following [121].

5. By 6-year panel (2000-2005, 2006-2011 and 2012-2017), we drop firms with fewer than 20 workers in any year or firms classified as public administration. [155] restrict to firms with at least 20 employees per year, and [156] chooses a threshold of 15 workers per year. Our large sample restriction is motivated by the estimation of the AKM firm effects, which requires a sufficient number of observations per firm.

Quarterly and hourly wages are each winsorized at the 1st and 99th percentiles to reduce noise from outliers. A limitation shared by most papers with matched employer-employee data is that we cannot distinguish between E-E and E-N moves for workers that move out of state. We also do not observe any non-wage worker characteristics: for example, we do not observe age, so cannot restrict to workers aged 20-60 as in comparable studies [57, 155]. We do observe firm industry and location (county level), which we use for heterogeneity in the analysis.
A.2.2 Summary Statistics of Data

Broadly, our main sample is a quarterly worker-level panel restricted to large private sector firms in Oregon over 2000-2017 (see table A.3). In total, we have 87.6 million observations, consisting of 3.4 million workers and 55,000 firms. Compared to the full universe of observations, our main sample has about two-thirds of all workers, and less than one-fifth of the firms (mainly due to the firm size restriction). Average annual worker earnings and weekly hours are substantially higher, again mainly due to the firm size restriction together with the wage-size correlation. The exclusion of short employment spells decreases the separations rate by about half, as well as the number of firms per worker. In our main sample, the mean separation rate is 8% per quarter, with about half of hires directly from other firms.\(^4\)

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

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

\(^{\text{4}}\)The quarterly separation rate is 17% before sample restrictions, which is similar to the separation rate of 0.15 reported by [165] using the LEHD.
Table A.3: **Sample statistics for Oregon 2000-2017**

<table>
<thead>
<tr>
<th>Period: 2000-2017</th>
<th>Obs (total, millions)</th>
<th>Workers (total, millions)</th>
<th>Firms (total)</th>
<th>Earnings (mean, annual)</th>
<th>Hours (mean, weekly)</th>
<th>No. firms (mean)</th>
<th>Separations (mean, quarterly)</th>
<th>E-E hire (mean, quarterly)</th>
</tr>
</thead>
<tbody>
<tr>
<td>All</td>
<td>136</td>
<td>5.3</td>
<td>316,910</td>
<td>27,169</td>
<td>27.49</td>
<td>5.71</td>
<td>16.6%</td>
<td>31.4%</td>
</tr>
<tr>
<td>Hours&lt;100</td>
<td>120</td>
<td>4.7</td>
<td>302,541</td>
<td>29,636</td>
<td>30.54</td>
<td>4.13</td>
<td>12.1%</td>
<td>33.1%</td>
</tr>
<tr>
<td>wage&lt;2</td>
<td>119</td>
<td>4.7</td>
<td>301,997</td>
<td>29,719</td>
<td>30.55</td>
<td>4.13</td>
<td>12.1%</td>
<td>33.1%</td>
</tr>
<tr>
<td>Spell&lt;2</td>
<td>110</td>
<td>3.7</td>
<td>249,034</td>
<td>32,057</td>
<td>31.53</td>
<td>2.95</td>
<td>7.6%</td>
<td>35.2%</td>
</tr>
<tr>
<td>Priv. large</td>
<td>87.6</td>
<td>3.4</td>
<td>54,663</td>
<td>44,103</td>
<td>32.44</td>
<td>2.53</td>
<td>7.7%</td>
<td>46.9%</td>
</tr>
<tr>
<td>Connected</td>
<td>87.6</td>
<td>3.4</td>
<td>54,580</td>
<td>44,101</td>
<td>32.44</td>
<td>2.53</td>
<td>7.7%</td>
<td>46.9%</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Period: 2000-2005</th>
<th>Obs (total, millions)</th>
<th>Workers (total, millions)</th>
<th>Firms (total)</th>
<th>Earnings (mean, annual)</th>
<th>Hours (mean, weekly)</th>
<th>No. firms (mean)</th>
<th>Separations (mean, quarterly)</th>
<th>E-E hire (mean, quarterly)</th>
</tr>
</thead>
<tbody>
<tr>
<td>All</td>
<td>27.5</td>
<td>2.1</td>
<td>31,429</td>
<td>42,147</td>
<td>32.66</td>
<td>1.60</td>
<td>8.1%</td>
<td>48.5%</td>
</tr>
<tr>
<td>Split 1</td>
<td>13.7</td>
<td>1.0</td>
<td>31,410</td>
<td>42,136</td>
<td>32.66</td>
<td>1.60</td>
<td>8.1%</td>
<td>48.5%</td>
</tr>
<tr>
<td>Split 2</td>
<td>13.8</td>
<td>1.0</td>
<td>31,407</td>
<td>42,157</td>
<td>32.66</td>
<td>1.60</td>
<td>8.1%</td>
<td>48.5%</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Period: 2006-2011</th>
<th>Obs (total, millions)</th>
<th>Workers (total, millions)</th>
<th>Firms (total)</th>
<th>Earnings (mean, annual)</th>
<th>Hours (mean, weekly)</th>
<th>No. firms (mean)</th>
<th>Separations (mean, quarterly)</th>
<th>E-E hire (mean, quarterly)</th>
</tr>
</thead>
<tbody>
<tr>
<td>All</td>
<td>29.1</td>
<td>2.1</td>
<td>31,788</td>
<td>44,975</td>
<td>32.33</td>
<td>1.55</td>
<td>7.5%</td>
<td>45.2%</td>
</tr>
<tr>
<td>Split 1</td>
<td>14.5</td>
<td>1.0</td>
<td>31,772</td>
<td>44,968</td>
<td>32.33</td>
<td>1.55</td>
<td>7.5%</td>
<td>45.1%</td>
</tr>
<tr>
<td>Split 2</td>
<td>14.6</td>
<td>1.0</td>
<td>31,772</td>
<td>44,982</td>
<td>32.33</td>
<td>1.55</td>
<td>7.5%</td>
<td>45.2%</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Period: 2012-2017</th>
<th>Obs (total, millions)</th>
<th>Workers (total, millions)</th>
<th>Firms (total)</th>
<th>Earnings (mean, annual)</th>
<th>Hours (mean, weekly)</th>
<th>No. firms (mean)</th>
<th>Separations (mean, quarterly)</th>
<th>E-E hire (mean, quarterly)</th>
</tr>
</thead>
<tbody>
<tr>
<td>All</td>
<td>30.9</td>
<td>2.2</td>
<td>32,913</td>
<td>45,023</td>
<td>32.35</td>
<td>1.58</td>
<td>7.6%</td>
<td>46.9%</td>
</tr>
<tr>
<td>Split 1</td>
<td>15.5</td>
<td>1.1</td>
<td>32,898</td>
<td>44,993</td>
<td>32.35</td>
<td>1.58</td>
<td>7.6%</td>
<td>46.9%</td>
</tr>
<tr>
<td>Split 2</td>
<td>15.5</td>
<td>1.1</td>
<td>32,892</td>
<td>45,053</td>
<td>32.35</td>
<td>1.58</td>
<td>7.6%</td>
<td>46.9%</td>
</tr>
</tbody>
</table>

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

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

Sample statistics for this matched event study panel are presented in table A.4. The full sample has nearly 900,000 initial E-E separations, each with an associated event-window, corresponding to just under 700,000 workers and 30,000 Origin firms. There are 175,000 unique Origin firm by calendar quarter ‘events’, with an average of 245 workers each. These workers move out to more intermediate firms (about 40,000). Earnings are roughly similar to the main worker-quarter panel, and hours are slightly higher. Although we use a 16 quarter post window, just over a third of the initial E-E separations end up re-separating to a final firm. These workers have lower average earnings. Note that tenure in table A.4 is censored 16 quarters post event.

The main estimation specification includes fixed effects for Origin firm by calendar quarter by worker tenure at Origin (8 categories) by wage at hire at Origin (8 categories). The estimable sample is substantially smaller, as it requires sufficient observations in every interacted fixed effects cell (see panel B). About 40% of the initial E-E separations survive, corresponding to 4,000 Origin firms and 21,000 Origin firm quarter events. Over 10,000 Intermediate firms are in this main estimation
Table A.4: **Sample statistics for matched event study panel**

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

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

### A.2.3 Inequality Trends

During the 2000-2017 period, the variance in log hourly wages was mostly stable (figure A.9). This pattern is similar when we consider hourly or quarterly earnings, and when we consider CPS data or the full universe of workers in our sample. Our main estimation sample (full quarter observations at large firms, as described in the data section) shows a slight increase in log variance. Figure A.9 shows that the level of the variance is similar using CPS survey data or the full universe of our records, about 1.5 for log quarterly earnings and 0.5 for log hourly wage. The level of variance for our main sample is much smaller for log quarterly earnings, as expected from the
Figure A.9: Oregon wage variance, CPS versus UI data

<table>
<thead>
<tr>
<th>Year</th>
<th>CPS, hourly</th>
<th>OR full sample, hourly</th>
<th>OR main sample, hourly</th>
<th>CPS, quarterly</th>
<th>OR full sample, quarterly</th>
<th>OR main sample, quarterly</th>
</tr>
</thead>
<tbody>
<tr>
<td>2000</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2005</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2010</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2015</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

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

restrictions on part time work (low hours and short spells), and slightly smaller for log hourly wages.

The overall variance of log wages masks considerable heterogeneity in trends by wage percentile, as shown in Figure [A.10](#) (using the full universe of observations). During this period, the largest growth in hourly wages occurred at the top (e.g., 95th percentile and 90th percentiles), while the real wage fell on net at the middle (50th percentile). However, during the same time wages rose at the bottom (5th and 10th
percentiles), in part likely due to Oregon’s minimum wage policies. Overall, hourly wage inequality grew in the upper half of the distribution, mirroring other states [121], even while inequality fell in the bottom half. The patterns are qualitatively similar when we consider quarterly earnings instead; however, the 90-50 gap in earnings grew somewhat more than the equivalent gap in hourly wages over this period.
Figure A.10: Oregon wage percentile trends

Note: Earnings are in real Dollars adjusted to 2017 using the Portland CPI. The sample corresponds to the main worker-quarter panel (after restrictions).
A.3 AKM

A.3.1 Procedure

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

We check the estimates firm fixed effects using the procedure from [56], which is downloadable online. The correlation for the firm effects is 0.91, and for the worker effects is 0.99. The wage variance decompositions are also very similar (see below).

The AKM estimates by stacked 6-year sample are persistent. Figure A.11 presents a plot of current versus next period firm hourly wage effects, with a resulting trimmed slope of 0.9 and R-squared of 0.7. The persistence across years of firm wage policies is consistent with the findings in [121].

A.3.2 Decomposition

Table A.5 provides the AKM decomposition in hourly wage and quarterly earnings inequality, for 6 year blocks between 2000-2017, as well as for the full panel. For both log quarterly earnings and log hourly wages, there is a slight increase in the overall variance between the 2000-2005 and 2012-2017 periods (0.37 to 0.41 for wages, and 0.59 to 0.64 for earnings). In the full panel, firm effects explain around 19% (14%) of the variance of quarterly earnings (hourly wages), and worker effects explain around 48% (55%) of the variance. There is also assortative matching of workers and firms, with the covariance term explaining around 14% (18%) of the variance. Consistent with other work, we see a clear increase in the covariance term for both wages and
Figure A.11: Persistence of AKM firm hourly wage effects

Note: AKM firm wage effects are estimated for each 6 year period (2000-2005, 2006-2011 and 2012-2017) using hourly wages. For each firm, the AKM firm effect is plotted against its firm effect in the next 6-year period, and binned. The red indicates censored firm effects, which represent the 2.5% top and bottom tails of the firm effects distribution. Points are plotted at the firm level and weighted by firm size.
earnings over this period consistent with greater sorting. At the same time, there is a slight increase in the firm component of quarterly earnings variance, but a small decrease in the case of hourly wages. The R-squared is 0.8 to 0.9 for all AKM regressions, and is higher for hourly wages compared to quarterly earnings. It is also not much lower than the R-squared on a comparable match effects model (fixed effects for every job, instead of additive fixed effects for workers and firms as imposed by AKM), which for the 2012-2017 period using hourly wages is 0.91 (0.9 for AKM). This implies that the variation in log wages explained by match effects is small.

Comparable studies find similar AKM decompositions. Using annual earnings data for the US over the years 2000-2008, [156] finds that firm effects explain 14% of the log variance, worker effects explain 51%, and the covariance term explains 10%. [122] [155] find a lower AKM firm effects share of 9% using annual earnings for a similar period. [121] find using data from Washington over 2002-2014 for their annual log earnings AKM decomposition (plug-in version) that firm effects explain 19%, worker effects 54%, and the covariance term 17%; similarly to us, they also find that the share explained by firm effects decreases (to 11%) when using hourly wages instead of quarterly earnings.

Our preferred AKM specification relies on split-sample estimation. Table A.6 provides the decomposition for each split sample using hourly wages, which is very similar across the two split samples and compared to the full sample decomposition above. Panel C shows some cross-sample statistics: the percentage covariance between own-sample and complement-sample fixed effects is lower than the direct firm effects variance in Table A.5, and the percentage explained by the covariance between own sample worker effects and complement sample firm effects is higher than the comparable covariance in table A.5.

Finally, we show that the AKM decomposition is very similar using code from [56] (table A.7). As in table A.5, for the last period the share explained by firm effects is
Table A.5: AKM decomposition

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Panel A: Earnings</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Var(Y)</td>
<td>0.592</td>
<td>0.63</td>
<td>0.639</td>
<td>0.621</td>
</tr>
<tr>
<td>% Var(Firm FE)</td>
<td>15%</td>
<td>15%</td>
<td>16%</td>
<td>19%</td>
</tr>
<tr>
<td>% Var(Worker FE)</td>
<td>58%</td>
<td>58%</td>
<td>56%</td>
<td>48%</td>
</tr>
<tr>
<td>% Var(Residual)</td>
<td>15%</td>
<td>15%</td>
<td>14%</td>
<td>21%</td>
</tr>
<tr>
<td>% 2×Cov(Firm FE, Worker FE)</td>
<td>11%</td>
<td>12%</td>
<td>14%</td>
<td>14%</td>
</tr>
<tr>
<td>% 2×Cov(Y, Firm FE)</td>
<td>42%</td>
<td>43%</td>
<td>46%</td>
<td>52%</td>
</tr>
<tr>
<td>Obs (millions)</td>
<td>22.60</td>
<td>25.20</td>
<td>25.70</td>
<td>73.40</td>
</tr>
<tr>
<td>Adjusted $R^2$</td>
<td>0.836</td>
<td>0.844</td>
<td>0.852</td>
<td>0.79</td>
</tr>
</tbody>
</table>

|                  |           |           |           |           |
| **Panel B: Wage** |           |           |           |           |
| Var(Y)           | 0.37      | 0.395     | 0.409     | 0.392     |
| % Var(Firm FE)   | 12%       | 11%       | 10%       | 14%       |
| % Var(Worker FE) | 62%       | 63%       | 63%       | 55%       |
| % Var(Residual)  | 13%       | 11%       | 10%       | 17%       |
| % 2×Cov(Firm FE, Worker FE) | 14% | 16% | 17% | 18% |
| % 2×Cov(Y, Firm FE) | 37% | 37% | 38% | 45% |
| Obs (millions)   | 22.60     | 25.20     | 25.70     | 73.40     |
| Adjusted $R^2$   | 0.863     | 0.888     | 0.9       | 0.844     |

*Note:* All subsets use the relevant connected set, where the main sample is restricted to private firms larger than 20 workers (full sample description in text). Firm fixed effects are censored at the 2.5 percent upper and lower tails of the firm distribution. For reference, the full jobs model adjusted $R^2$ for 2000-2017 is 0.88, and for 2012-2017 is 0.91.
Table A.6: AKM decomposition for split samples

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Panel A: Sample 1</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Var(Y)</td>
<td>0.37</td>
<td>0.395</td>
<td>0.409</td>
</tr>
<tr>
<td>% Var(Firm FE)</td>
<td>12%</td>
<td>12%</td>
<td>11%</td>
</tr>
<tr>
<td>% Var(Worker FE)</td>
<td>63%</td>
<td>64%</td>
<td>64%</td>
</tr>
<tr>
<td>% Var(Residual)</td>
<td>13%</td>
<td>11%</td>
<td>10%</td>
</tr>
<tr>
<td>% 2 Cov(Firm FE, Worker FE)</td>
<td>13%</td>
<td>14%</td>
<td>16%</td>
</tr>
<tr>
<td>% 2 Cov(Y, Firm FE)</td>
<td>37%</td>
<td>37%</td>
<td>38%</td>
</tr>
<tr>
<td>Obs (millions)</td>
<td>11.259</td>
<td>12.552</td>
<td>12.823</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.864</td>
<td>0.888</td>
<td>0.9</td>
</tr>
<tr>
<td><strong>Panel B: Sample 2</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Var(Y)</td>
<td>0.37</td>
<td>0.395</td>
<td>0.409</td>
</tr>
<tr>
<td>% Var(Firm FE)</td>
<td>12%</td>
<td>12%</td>
<td>11%</td>
</tr>
<tr>
<td>% Var(Worker FE)</td>
<td>63%</td>
<td>64%</td>
<td>64%</td>
</tr>
<tr>
<td>% Var(Residual)</td>
<td>13%</td>
<td>11%</td>
<td>10%</td>
</tr>
<tr>
<td>% 2 Cov(Firm FE, Worker FE)</td>
<td>13%</td>
<td>14%</td>
<td>16%</td>
</tr>
<tr>
<td>% 2 Cov(Y, Firm FE)</td>
<td>37%</td>
<td>37%</td>
<td>37%</td>
</tr>
<tr>
<td>Obs (millions)</td>
<td>11.254</td>
<td>12.557</td>
<td>12.813</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.864</td>
<td>0.889</td>
<td>0.9</td>
</tr>
<tr>
<td><strong>Panel C: Complement sample</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Var(Y)</td>
<td>0.37</td>
<td>0.395</td>
<td>0.409</td>
</tr>
<tr>
<td>% Cov($FirmFE_{own}$, $FirmFE_{complement}$)</td>
<td>11%</td>
<td>10%</td>
<td>9%</td>
</tr>
<tr>
<td>% 2 Cov($WorkerFE_{own}$, $FirmFE_{complement}$)</td>
<td>16%</td>
<td>17%</td>
<td>19%</td>
</tr>
<tr>
<td>Obs (millions)</td>
<td>22.227</td>
<td>24.808</td>
<td>25.33</td>
</tr>
</tbody>
</table>

Note: All subsets use the relevant connected set, where the main sample is restricted to private firms larger than 20 workers (full sample description in text). The main sample is randomly split into two samples, stratifying by whether the worker moved firms and clustering by worker. Firm fixed effects are estimated using log hourly wages, and censored at the 2.5 percent upper and lower tails of the firm distribution. Panel C shows the share of log wage variation explained by the covariance between the firm effects from a worker’s own sample and the firm effects estimated using the alternate split-sample estimate for each worker’s firm (comparable to the share explained by the variance of the firm effects); and the covariance between each individual’s worker effect and the alternate split-sample firm effect estimate.
Table A.7: AKM decomposition using alternative code

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Var(Y)</td>
<td>0.369</td>
<td>0.394</td>
<td>0.409</td>
</tr>
<tr>
<td>% Var(Firm FE)</td>
<td>12%</td>
<td>12%</td>
<td>11%</td>
</tr>
<tr>
<td>% Var(Worker FE)</td>
<td>63%</td>
<td>64%</td>
<td>64%</td>
</tr>
<tr>
<td>% Var(Residual)</td>
<td>13%</td>
<td>11%</td>
<td>10%</td>
</tr>
<tr>
<td>% 2 Cov(Firm FE, Worker FE)</td>
<td>13%</td>
<td>13%</td>
<td>16%</td>
</tr>
<tr>
<td>% 2 Cov(Y, Firm FE)</td>
<td>37%</td>
<td>37%</td>
<td>38%</td>
</tr>
<tr>
<td>Obs (millions)</td>
<td>22.397</td>
<td>25.037</td>
<td>25.562</td>
</tr>
<tr>
<td>Adjusted $R^2$</td>
<td>0.863</td>
<td>0.896</td>
<td>0.900</td>
</tr>
</tbody>
</table>

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

lowest and the covariance between worker and firm effects is highest. The separations elasticity using these firm effects is also similar (if slightly lower), presented in table A2.

A.3.3 Limited Mobility Bias

A prominent threat to the AKM estimation of firm effects is limited mobility bias [9]. We replicate the comparisons in [121] for our data to show that limited mobility bias likely becomes less severe with a longer panel and better measurement of wages (table [A.8]).

Our panel has two advantages in addressing limited mobility bias. Firstly, a longer panel allows for more movers between firms, which is the source of identification for the AKM firm effects. The quarterly frequency, as compared to the annual data of many other studies, picks up more movers within the same time period. Secondly, insofar as firm pay policies correspond to hourly wages, annual earnings as used by many studies are a noisy measure of the firm effect. We observe hours, which allows us to estimate the firm effects on hourly wages directly.
These advantages of the panel contribute to better measurement of the AKM components. The first two columns show 2-year panels, and should be compared to the second 2 columns which show 6-year panels. The share of variance explained by the firm effects decreases for the longer panel where more movers are observed, most noticeably for the annual earnings measure where we expect more noise. A similar pattern is observed for the share of variance explained across the panels: within each column, the share explained by firm effects decreases with better wage measures. On the other hand, the covariance between firm and worker effects rises dramatically as the panel length increases and the earnings measure improves.

Lower variance of firm effects and higher covariance between worker and firm effects are the two predictions of reductions in limited mobility bias, which both come through clearly for our data. Overall, comparing column 2 panel A (short panel, annual earnings) to column 4 panel C (longer panel, hourly wage), the share of log variance explained by firm effects decreases from 20% to 10%. The share explained by sorting, i.e. the covariance term, increases from 2% (suggesting very little sorting) to 17% (suggesting substantial sorting). Both features echo the findings of [38, 121].

---

5 The last column shows the full panel, where the share of variance explained increases, likely due to actual increases in the variance, for example since more firms are included.
Table A.8: AKM variance decomposition by panel length

<table>
<thead>
<tr>
<th></th>
<th>2-Year Panels</th>
<th>6-Year Panels</th>
<th>Full panel</th>
</tr>
</thead>
<tbody>
<tr>
<td>Panel A: Annual earnings</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Var(Y)</td>
<td>0.528</td>
<td>0.584</td>
<td>0.56</td>
</tr>
<tr>
<td>% Var(Firm FE)</td>
<td>23%</td>
<td>20%</td>
<td>17%</td>
</tr>
<tr>
<td>% Var(Worker FE)</td>
<td>80%</td>
<td>75%</td>
<td>62%</td>
</tr>
<tr>
<td>% 2×Cov(Firm FE, Worker FE)</td>
<td>-6%</td>
<td>2%</td>
<td>13%</td>
</tr>
<tr>
<td>Obs (millions)</td>
<td>1.81</td>
<td>2.02</td>
<td>6.61</td>
</tr>
<tr>
<td>Panel B: Quarterly earnings</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Var(Y)</td>
<td>0.588</td>
<td>0.639</td>
<td>0.592</td>
</tr>
<tr>
<td>% Var(Firm FE)</td>
<td>18%</td>
<td>16%</td>
<td>15%</td>
</tr>
<tr>
<td>% Var(Worker FE)</td>
<td>70%</td>
<td>66%</td>
<td>58%</td>
</tr>
<tr>
<td>% 2×Cov(Firm FE, Worker FE)</td>
<td>1%</td>
<td>7%</td>
<td>11%</td>
</tr>
<tr>
<td>Obs (millions)</td>
<td>7.46</td>
<td>8.48</td>
<td>22.40</td>
</tr>
<tr>
<td>Panel C: Hourly wage</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Var(Y)</td>
<td>0.366</td>
<td>0.41</td>
<td>0.37</td>
</tr>
<tr>
<td>% Var(Firm FE)</td>
<td>13%</td>
<td>10%</td>
<td>12%</td>
</tr>
<tr>
<td>% Var(Worker FE)</td>
<td>70%</td>
<td>72%</td>
<td>62%</td>
</tr>
<tr>
<td>% 2×Cov(Firm FE, Worker FE)</td>
<td>8%</td>
<td>12%</td>
<td>14%</td>
</tr>
<tr>
<td>Obs (millions)</td>
<td>7.46</td>
<td>8.48</td>
<td>22.40</td>
</tr>
</tbody>
</table>

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

Finally, we replicate the mobility bias figure presented in [122], while adding our improved measures of the firm effects for comparison. Figure A.12 shows that as the share of movers retained increases, the share of log variance explained by the variance in firm effects decreases substantially for the annualized earnings panel (by about 8 percentage points) – as expected when limited mobility bias is reduced. However, as argued above, the reduction in share explained is lower using quarterly earnings (6 percentage points), or hourly wage (4 percentage points). Moreover, the bias when using our split sample measure (predicting own-sample firm effect by
complement-sample firm effect) is in the opposite direction: the share of variance explained increases with share of movers retained.

**Figure A.12: Mobility bias by varying share of movers**

![Graph showing mobility bias by varying share of movers](image)

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

SUPPLEMENTARY MATERIAL: FIRMS AND INEQUALITY WHEN UNEMPLOYMENT IS HIGH
### B.1 Additional Tables and Figures

Table B.1: Measures of productivity dispersion in South Africa

<table>
<thead>
<tr>
<th></th>
<th>TFP-IQR</th>
<th>TFP-SD</th>
<th>TFP-p90p10</th>
<th>LP-IQR</th>
<th>LP-SD</th>
<th>LP-p90p10</th>
</tr>
</thead>
<tbody>
<tr>
<td>2-d (all)</td>
<td>.78</td>
<td>.77</td>
<td>1.65</td>
<td>1.15</td>
<td>1.07</td>
<td>2.39</td>
</tr>
<tr>
<td>2-d (big)</td>
<td>.83</td>
<td>.78</td>
<td>1.72</td>
<td>1.23</td>
<td>1.1</td>
<td>2.51</td>
</tr>
<tr>
<td>3-d (all)</td>
<td>.75</td>
<td>.73</td>
<td>1.55</td>
<td>1.1</td>
<td>1.03</td>
<td>2.29</td>
</tr>
<tr>
<td>3-d (big)</td>
<td>.76</td>
<td>.72</td>
<td>1.54</td>
<td>1.1</td>
<td>1.01</td>
<td>2.26</td>
</tr>
<tr>
<td>4-d (all)</td>
<td>.75</td>
<td>.72</td>
<td>1.54</td>
<td>1.09</td>
<td>1.02</td>
<td>2.26</td>
</tr>
<tr>
<td>4-d (big)</td>
<td>.75</td>
<td>.72</td>
<td>1.52</td>
<td>1.11</td>
<td>1.01</td>
<td>2.2</td>
</tr>
</tbody>
</table>

*Notes.* The table shows that within each column measure estimates are similar. TFP refers to total factor productivity, i.e. value added residualized by a regression on industry specific terms of the mean, capital, labor, and material costs. LP refers to labor productivity, i.e. value added per worker residualized by a regression on industry specific constant. Big indicates that only firms with more than 20 workers are considered. Measures are averaged across all sectors (not just manufacturing).
Table B.2: **Alternative specifications for labor supply elasticity**

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Separations</td>
<td>-0.308</td>
<td>-0.261</td>
<td>-0.319</td>
<td>-0.794</td>
</tr>
<tr>
<td></td>
<td>(0.014)</td>
<td>(0.005)</td>
<td>(0.127)</td>
<td>(0.055)</td>
</tr>
<tr>
<td>E-E separations</td>
<td>-0.509</td>
<td>-0.424</td>
<td>-0.34</td>
<td>-1.073</td>
</tr>
<tr>
<td></td>
<td>(0.018)</td>
<td>(0.010)</td>
<td>(0.171)</td>
<td>(0.074)</td>
</tr>
<tr>
<td>E-N separations</td>
<td>-0.294</td>
<td>-0.292</td>
<td>-0.221</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.022)</td>
<td>(0.008)</td>
<td>(0.171)</td>
<td></td>
</tr>
<tr>
<td>E-E recruits</td>
<td>0.041</td>
<td>0.002</td>
<td>-0.129</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.013)</td>
<td>(0.003)</td>
<td>(0.051)</td>
<td></td>
</tr>
<tr>
<td>Perc E-E recruits</td>
<td>0.451</td>
<td>0.451</td>
<td>0.442</td>
<td></td>
</tr>
<tr>
<td>Firm LSE</td>
<td>0.858</td>
<td>0.773</td>
<td>0.742</td>
<td>1.59</td>
</tr>
<tr>
<td></td>
<td>(0.032)</td>
<td>(0.015)</td>
<td>(0.256)</td>
<td>(0.110)</td>
</tr>
<tr>
<td>Fstat</td>
<td></td>
<td></td>
<td>14.961</td>
<td></td>
</tr>
<tr>
<td>Obs (m)</td>
<td>36.2</td>
<td>36.2</td>
<td>0.1</td>
<td>0.48</td>
</tr>
<tr>
<td>OLS</td>
<td>Y</td>
<td>Y</td>
<td></td>
<td></td>
</tr>
<tr>
<td>First Difference</td>
<td></td>
<td></td>
<td>Y</td>
<td></td>
</tr>
<tr>
<td>Movers</td>
<td></td>
<td></td>
<td>Y</td>
<td></td>
</tr>
<tr>
<td>Controls</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Worker type</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td></td>
</tr>
<tr>
<td>Indus X Geo FE</td>
<td></td>
<td></td>
<td>Y</td>
<td></td>
</tr>
</tbody>
</table>

**Notes.** The top four rows represent separate regressions on all separations, employment-to-employment separations, employment to non-employment separations, and employment to employment recruits respectively. The Firm LSE row combines estimates from these separate regressions. The worker type control adds the AKM worker fixed effect as a continuous variable control regressor. The industry by geography control includes 221 by 20 fixed effects respectively. The First Difference specification is run at the firm-level (weighted by number of workers), and compares the change in separations within a firm to the change in average wages, as instrumented by change in log value added per worker. Workers are limited to connected firms with more than 20 employees. Standard errors are given in parentheses. Source: Own calculations, South African tax records, 2011-2016.
Table B.3: Unions and rent-sharing elasticity

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Union</td>
<td>0.689</td>
<td>0.414</td>
<td>0.148</td>
<td>0.454</td>
<td>0.269</td>
<td>-0.007</td>
</tr>
<tr>
<td></td>
<td>(0.093)</td>
<td>(0.098)</td>
<td>(0.201)</td>
<td>(0.069)</td>
<td>(0.039)</td>
<td>(0.085)</td>
</tr>
<tr>
<td>log(VA pe)</td>
<td>0.113</td>
<td>0.112</td>
<td>0.101</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.006)</td>
<td>(0.006)</td>
<td>(0.006)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Union X log(VA pe)</td>
<td>0.186</td>
<td>0.189</td>
<td>0.067</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.030)</td>
<td>(0.031)</td>
<td>(0.027)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Obs (mill.)</td>
<td>23</td>
<td>22.1</td>
<td>22.1</td>
<td>14.2</td>
<td>13.9</td>
<td>13.9</td>
</tr>
<tr>
<td>$R^2$</td>
<td>.12</td>
<td>.16</td>
<td>.21</td>
<td>.29</td>
<td>.33</td>
<td>.39</td>
</tr>
<tr>
<td>Within $R^2$</td>
<td>0.12</td>
<td>0.08</td>
<td>0.05</td>
<td>0.29</td>
<td>0.25</td>
<td>0.17</td>
</tr>
<tr>
<td>Worker control</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>Industry FE</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Location FE</td>
<td></td>
<td>Y</td>
<td></td>
<td></td>
<td></td>
<td>Y</td>
</tr>
</tbody>
</table>

Notes. The outcome is the estimated AKM firm wage premia. VApe refers to value added per employee as a proxy for rent. Union density is averaged from survey data and merged into the individual data at the location by industry by year cell. Variable values are centred around 0. Union density is instrumented by its lag to reduce measurement error. The third row indicates the interaction between union density and log value added per employee. Industry contains 10 industry categories, and location contains 221 categories. All specifications are run at the individual level, include a continuous variable control for the estimated AKM worker wage premia, and are clustered at the municipality by industry cell. Standard errors are given in parentheses. Workers are limited to those at connected firms with more than 20 employees. Source: Own calculations, South African tax records, 2011-2016, and Quarterly Labor Force Survey, 2010-2015.
**Figure B.1:** Distribution of worker and firm fixed effects

![Graph](image)

*Notes.* Worker and firm effects are estimated using the AKM regression. Deciles of worker effects are calculated with one observation per worker, and deciles of firm effects are calculated with one observation per firm. One observation per worker is plotted, where deciles of worker effects are plotted in increasing order by decile of firm effect (dark red is the lowest worker effect, dark blue is the highest). Workers are limited to connected firms with more than 20 employees.

**Figure B.2:** Variance due to sorting, comparison to other countries

![Graph](image)

(a) *Share of variance*  
(b) *Raw covariance*

*Notes.* Sorting refers to the covariance between firm and worker wage premia. Estimates are compiled from [38], adding the estimates from this paper. KSS indicates the estimation method in [117] is followed. Share of variance is the share of total wage variance explained, and raw variance is the direct variance of that component.
Figure B.3: Standard deviation of firm productivity, comparison to other countries

Notes. Estimates refer to the manufacturing sector across 21 countries and 23 studies. Productivity is measured across all sources as log total factor productivity (TFP) within each industry, i.e. log value added residualized by a regression on 4-digit industry specific log terms of the mean, assets, firm size, and material costs. The legend notes the sources of the estimates, referring to [19, 20, 102], as well as this paper.
Figure B.4: Unemployment and regional indicators of labor market power

(a) Separations elasticity

(b) E-E separations elasticity

(c) Variance in firm wage premia

Notes. Municipal unemployment rate from Census 2011. EPOP unemployment denotes unemployment to population ratio. The separations elasticities are estimated by local municipality, by regressing firm separations on firm wages controlling for AKM worker effects. Panel A uses all separations, and panel B only uses Employment to Employment separations. Panel C calculates regional variance in the estimated KSS Firm wage premia. Controls include local area average firm size, value added per worker, population density, and industry composition.
Figure B.5: **Firm premia and unemployment duration**

(a) **E-E sep elasticity and duration**  
(b) **Variance of wage premia and duration**

*Notes.* Unemployment duration is measured as the years between being observed in the full sample of formal sector firms in the tax data. Source: Own calculations, South African tax records, 2011-2016.
B.2 Data sample and construction

Data access

The datasets used for this paper have restricted access, due to their confidential nature as tax records of workers and firms. The data are managed jointly by the South African National Treasury and UNU-WIDER under the project “Southern Africa – Towards Inclusive Economic Development (SA-TIED)”. They may be accessed by responding to one of the public call for proposals, which involves writing a motivation for a project and being accepted by the administrators of the relevant work stream. Accepted researchers must be physically present at the data centre, which is in Pretoria, South Africa, and any output is screened for confidential information before being sent to researchers from the data centre.

My project was accepted under the inequality workstream in 2018, and an earlier version was published by agreement as a working paper under the project in 2019. Return access is allowed conditional on a strong motivation towards publication, such as revisions requested by a journal.

Data construction

I combine two sources of data from the South African Revenue Service to form a matched employer-employee panel. The main source records individual job certificates submitted by firms on behalf of any employee earning over R2,000 a year (a low threshold of under $150). The second source of data is a firm-level panel based on corporate income tax data, which provides me with the firm-level value added used for estimating productivity dispersion and rent-sharing elasticities.

\[ \text{[112] uses this dataset in studying job churn over 2011-2014, and [80] assess the impact of the Earnings Tax Incentive, a youth wage subsidy recently implemented in South Africa.} \]

\[ \text{[118] uses this dataset in finding total factor productivity trends in manufacturing firms, and [82] in calculating concentration in the manufacturing product market.} \]
In constructing my data sample, I begin with all job certificates which comprise of about 15 million per year. I restrict to workers who are between 20 and 60 years old to reduce the likelihood that wages consist of part time work or pension income, which reduces the certificates to about 14 million per year. I match these records to firm-level data using a correspondence table provided with the data; for reasons not completely clear to the data administrators, this matching is imperfect and fails to match about 1 million observations. Next, I convert job-level records to the worker-level by selecting only the job for which wage income is highest\(^3\). I restrict to firms that have more than 20 workers, following [155]. The empirical strategy relies on estimating firm fixed effects, which may not be estimated well in small firms with idiosyncratic behaviour; limited mobility bias is also worse in the case of small firms. While excluding the majority of firms, this restriction maintains over 70% of workers and over 85% of total reported revenue. I provide some robustness analysis without this firm size restriction.

I observe unique identifiers for workers as well as establishments. However, balance sheet information is only reported at the firm level, i.e. for each firm-based collection of establishments. In the analysis below, “firm” wage premia would more accurately be named “establishment” wage premia.” Thus in principle the “firm” effects are run at the establishment level, which should be differently reported within each region. In practice, this is not exactly correct as the data administrators note some firms submit worker claims at the head office rather than individual establishments.

I adjust all ZAR-denominated variables by the national consumer price index inflation tables provided by the national statistics agency, Statistics South Africa. I construct earnings by adding wage income and wage benefits, such as overtime, med-

\(^3\)This decision rule is also used by [38 156 160].
ical aid and annual bonus, and annualize earnings by multiplying reported earnings by the inverse of the reported fraction of the year employed.

The main firm-level variable I use is value added per worker, which I construct as sales minus total non-labor costs, and serves as a proxy for rent as in \([122, 56]\). As secondary measures of rent, I construct profits as value added minus labor costs. 80% of firms have non-missing value added. The incompleteness of this variable may introduce bias, but the direction is ambiguous. Loss-making firms may be drawn disproportionately, since losses are deductible from taxes on the following year’s profits. On the other hand, firms which fail have no incentive to report losses or sales. It is reassuring that the firms who do report cover disproportionately more workers (i.e. bigger firms report more often).

**Data descriptives**

How does this compare to survey data? The national statistics agency, conducts several publicly available surveys. I compare to the Income and Expenditure Survey (IES) of 2010/11, which is aimed at providing accurate income data and has a sample of over 90 thousand people, as well as to the Quarterly labor Force Survey which has a smaller sample size but is conducted quarterly[^4]. The IES records 12 million employed, and the QLFS shows 13-14 million employed per year over the sample period. This implies excellent coverage in the tax data which records 10-11 million workers employed (before restrictions), yet excludes informally employed workers who are counted survey data. The IES records median and 90th percentile wages of about 80% of those reported in table 1, and the QLFS similarly records p50 and p90 wages of 70-80% of those reported in table 1. The difference is likely due to a combination of measurement error and the distribution being shifted up with the exclusion of

[^4]: I use version 3.2 of the Post-Apartheid labor Market Series, which standardizes the QLFS \([111]\). For the remainder of the paper where I compare to survey data, I use this dataset.
informal workers and small firms. Overall it is reassuring that the employment and wages correspond roughly to survey data.

Table B.4: **Summary statistics on data cleaning**

<table>
<thead>
<tr>
<th>Panel A</th>
<th>Jobs</th>
<th>Sample</th>
<th>Age</th>
<th>E-E separations</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(freq)</td>
<td>(freq.)</td>
<td>(mean)</td>
<td>(mean)</td>
</tr>
<tr>
<td>2011</td>
<td>10,100,000</td>
<td>8,353,791</td>
<td>36.98</td>
<td>0.171</td>
</tr>
<tr>
<td>2012</td>
<td>10,400,000</td>
<td>8,681,995</td>
<td>36.93</td>
<td>0.175</td>
</tr>
<tr>
<td>2013</td>
<td>10,600,000</td>
<td>8,900,366</td>
<td>36.93</td>
<td>0.167</td>
</tr>
<tr>
<td>2014</td>
<td>10,600,000</td>
<td>8,981,113</td>
<td>36.95</td>
<td>0.174</td>
</tr>
<tr>
<td>2015</td>
<td>10,800,000</td>
<td>9,150,558</td>
<td>37.11</td>
<td>0.171</td>
</tr>
<tr>
<td>2016</td>
<td>10,700,000</td>
<td>8,999,547</td>
<td>37.20</td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Panel B</th>
<th>All firms</th>
<th>Panel Firms</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Firms</td>
<td>Sales</td>
</tr>
<tr>
<td></td>
<td>(freq.)</td>
<td>(ZAR, total)</td>
</tr>
<tr>
<td>2011</td>
<td>223,054</td>
<td>1.11E+13</td>
</tr>
<tr>
<td>2012</td>
<td>226,855</td>
<td>1.32E+13</td>
</tr>
<tr>
<td>2013</td>
<td>229,068</td>
<td>1.35E+13</td>
</tr>
<tr>
<td>2014</td>
<td>231,982</td>
<td>1.46E+13</td>
</tr>
<tr>
<td>2015</td>
<td>236,243</td>
<td>1.46E+13</td>
</tr>
<tr>
<td>2016</td>
<td>239,024</td>
<td>1.48E+13</td>
</tr>
</tbody>
</table>

*Note:* Panel A: Jobs refer to distinct worker-firm-year matches restricted to ages 20-60. Column 2 selects the highest wage of worker-firm-year matches per worker, to convert into a worker panel. An E-E separation occurs when a worker is registered at one firm and then registered at a different firm in the following year. Panel B: Firm-level summary statistics. The main restriction for in-sample firms are to firms with more than 20 workers. Sales and value added totals are given as a percentage of all firms. Source: Own calculations, South African tax records, 2011-2016.

It is worth understanding what role informal workers play compared to the sample of formal workers captured in the data. Using the Quarterly Labor Force Survey from 2011 to 2016, I define a worker as in the formal sector if the employment contract is written or deductions are made for pension or medical aid. A worker is also in the formal sector if they are self-employed with a business that is registered for tax. A worker is informally employed if they do not satisfy any of these definitions.
To get a sense of transitions between the informal and formal sector, I link workers to their future response by exploiting the 25% out-rotation quarterly panel design (Table B.5). 91% of formal workers remain in formal employment by the following quarter, whereas only 4% leave to the informal sector. This suggests transitions from formal to informal employment is not a major concern.

Table B.5: Dual sector transitions

<table>
<thead>
<tr>
<th></th>
<th>NEA</th>
<th>Unemployed</th>
<th>Informal</th>
<th>Formal</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td>NEA</td>
<td>83.9</td>
<td>12.7</td>
<td>2.0</td>
<td>1.4</td>
<td>100.0</td>
</tr>
<tr>
<td>Unemployed</td>
<td>15.4</td>
<td>73.8</td>
<td>6.0</td>
<td>4.8</td>
<td>100.0</td>
</tr>
<tr>
<td>Informal</td>
<td>4.3</td>
<td>9.6</td>
<td>72.2</td>
<td>13.9</td>
<td>100.0</td>
</tr>
<tr>
<td>Formal</td>
<td>1.6</td>
<td>3.0</td>
<td>4.3</td>
<td>91.1</td>
<td>100.0</td>
</tr>
<tr>
<td>Total</td>
<td>30.3</td>
<td>23.1</td>
<td>12.5</td>
<td>34.1</td>
<td>100.0</td>
</tr>
</tbody>
</table>

(Note: NEA indicated not economically active. Unemployed follows the expanded definition of unemployment, i.e. it includes those who would like a job but have not sought employment in the last week. Rows represent employment status in the initial period, and columns represent status for the same individual in the following period. Each row adds up to 100 percent. The sample includes all adults aged 18-64, following the national official definition of the working age population, and corresponds to weighted observations from the QLFS 2011-2016.)

Finally, while the summary statistics tables on the data give a sense of the distribution of wages (e.g. table 1 in the main text), table B.6 summarizes wage growth. The actual movement and wage growth of workers by firm movement hint at the high dispersion in firm wage premia. Workers who switch firms have substantially higher wage growth on average, especially for higher deciles. This is consistent with a job ladder, where workers switch jobs when they find a better offer. Workers who stay at the same firm, perhaps those in collective bargaining units, also experience high growth. However, at the cross-section median growth remains low, reflecting wage dynamics of job losers and precarious work.

As an aside, there is an interesting finding here for the literature on South African wages which may be worth exploring in further work. What explains this difference
in median wage growth for workers who are continuously employed versus workers as a cross-sectional distribution? The proportion of hires from non-employment is extremely high, about 64%, and there is a wage penalty associated with unemployment. This also affects the AKM estimation, as noted in the main text. Thus, the 0.27% growth rate at the cross-sectional median can be decomposed into a large positive growth rate for those who stay in employment, and a negative growth rate for those who do not (this also includes workers entering the labor force at lower wages than workers who leave, as expected from life cycles).

Table B.6: Individual wage growth by separation status

<table>
<thead>
<tr>
<th></th>
<th>Workers</th>
<th>Real wage growth (p50)</th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(freq p.a.)</td>
<td>(all)</td>
<td>(Dec. 1-4)</td>
<td>(Dec. 5-8)</td>
<td>(Dec. 10)</td>
</tr>
<tr>
<td>Stayer</td>
<td>2,220,000</td>
<td>3.13%</td>
<td>2.34%</td>
<td>3.27%</td>
<td>3.38%</td>
</tr>
<tr>
<td>E-E sep</td>
<td>2,460,000</td>
<td>4.15%</td>
<td>-0.69%</td>
<td>5.46%</td>
<td>6.69%</td>
</tr>
<tr>
<td>Cross-section</td>
<td>8,844,562</td>
<td>0.27%</td>
<td>1.49%</td>
<td>1.36%</td>
<td>1.54%</td>
</tr>
</tbody>
</table>

Note: Stayers are workers who remain at the same firm for the full period. E-E sep are workers who are employed for the full period but separate to a different firm at some point. Cross-section is the cross sectional wage growth by year. The lower overall median for the cross-section reflects South Africa’s U-shaped percentile growth – higher growth at the top and bottom, and lower growth in the middle. Deciles are categorized by year. Workers are limited to those at firms with more than 20 employees. Source: Own calculations, South African tax records, 2011-2016.
B.3 Validation of AKM specification

I implement several further checks on the structure of the AKM equation 1 in the main text. Firstly, the residuals should have mean zero conditional on worker and firm effects ($E[\nu_{ijt}|\alpha_i, \phi_j, X_{ijt}\rho] = 0$). Figure B.6 shows the median residuals by deciles of firm and worker effect for workers who move across firms. The AKM additive structure fits poorly at the bottom decile of the worker and firm effects distributions — but otherwise, the residuals are negligible in magnitude (0-2% of wages), especially in comparison to the magnitude of the changes in firm wage premia suggested by the Figure 1 event study in the main text. The poor fit at the bottom of the distribution may be explained by minimum wages, as pointed out by [55] in reference to a similar pattern in Portugal.

Secondly, limited mobility bias is a key concern in this literature. As recommended by [117], I use as my primary set of estimates for the firm wage premia their leave-out estimator which corrects for the mismeasurement of the firm effects. The issue is that mis-measurement of the firm effects spuriously increases its variance, which is important when estimating the proportion firms account for in the total wage variance. [122] show that the firm wage premia are more likely to be mismeasured with fewer movers, and so in unreported robustness I restrict another set of leave-out estimated premia based on a set of firms with at least ten movers in each year (with similar results). I follow [122] and show in Figure B.7 that the variance of the firm wage premia increases when estimated on smaller shares of movers within each firm. Note however that the variance shows little movement once 60% or more movers are included. This is consistent with the analysis in [121], who use administrative data from Washington to argue that limited mobility bias is less of a concern when using longer time series; my six-year panel therefore gives some confidence. I also run the procedure provided by [91] as a parametric correction, again with similar results.
Thirdly, as discussed in [3], the event analysis in the Figure 1 event study in the main text may still be consistent with some endogenous mobility which biases the firm wage premia. If wages depend on idiosyncratic firm-worker matches, and workers who are at badly matched firms tend to move towards firms that offer better matches, then firm wage premia will be overestimated. Reassuringly, [39] find using Swedish matched data that endogenous moves make little difference to their firm wage premia decompositions, despite strongly detecting the existence of such moves. Nevertheless, one strategy to address this is restricting the AKM sample of workers to separations from firm closings (to any firm) as an alternative set of firm wage premia. These separations are plausibly less endogenous, yet exhibit very similar estimates to the main set of firm wage premia. [139] similarly consider firm wage premia for displaced workers, using matched administrative data from Ohio in the United States. Across my sample period, about 10,000 firms are not observed in the following year, and workers separate from these firms to a large network of firms. I provide estimates of the wage premia using this approach in the main text, Table 2. A weakness of this approach is that some observed closures may in fact reflect other events such as mergers.

Fourthly, I investigate the possibility that compensating differentials offset the wage premia with non-wage amenities [122, 156]. Since I observe different sources of income for wages in the tax data, I can compare the monthly wage to the total compensation package (including annual bonus, medical aid and overtime). If amenities offset wages in the firm premia, we would also expect the “total compensation” premia to be more compressed than the “monthly wage” premia. The small role of

5With mean-zero match effects, relative firm wage premia can be estimated as the average wage change for workers who move between two firms. However, if movers earn a below-average income at the original firm (negative match effect), or are attracted to an above-average income at the destination firm (positive match effect), then the wage change will over-estimate the firm effect.

6Figure B.8 initially suggests that this may be the case, with a slope of 0.93 indicating that an increase in monthly wage premia is accompanied by less than a 1:1 increase in total compensation.
amenities is affirmed by Figure B.8, which shows the difference between the earnings and wage firm effects (a proxy for wage amenities) against the wage firm effects – there is no apparent pattern (slope of 0.07, standard error 0.14). While wage amenities do not reflect general amenities, this evidence does suggest that the wage premia may represent actual differences in firm value.

Figure B.6: AKM residuals by worker and firm deciles

Notes. Residuals are calculated from the AKM regression on worker and firm effects, as well as year dummies and age controls. Deciles of worker effects are plotted in increasing order by decile of firm effect (dark red is the lowest worker effect, dark blue is the highest). Workers are limited to separations to employment from connected firms with more than 20 employees. Source: Own calculations, South African tax records, 2011-2016.

premia. However, the estimate may purely be a result of measurement error attenuation; and in fact the reverse regression suggests this is the case. Without measurement error, the coefficient in the reverse regression should be the inverse, i.e. 1.08, yet the actual coefficient from the regression of wage on compensation is 0.9.
Figure B.7: Variance of Firm FE by share of movers included

Notes. An AKM regression is run for samples with increasing proportion of movers retained. Movers (workers who move firms) are randomly selected and dropped so that each firm only retains the given share of movers. Var(Firm FE) is the variance of the resulting firm fixed effects, estimated without limited mobility bias correction. Workers are limited to those who separate from firms with more than 20 employees. Source: Own calculations, South African tax records, 2011-2016.

Figure B.8: Compensating differentials

(a) Total earnings and wages

(b) Amenities and wages

Notes. Total compensation includes medical aid, overtime, bonus, share options and other monetary compensation. Monthly wage includes only income categorized as monthly income. The regression of compensation on wage and the reverse regression are both reported to highlight measurement error. Workers are limited to those at connected firms with more than 20 employees. Source: Own calculations, South African tax records, 2011-2016.
B.4 Models

B.4.1 Framework details

This section provides a few more details of the framework in the main text. This is a simple partial equilibrium model meant to guide the empirical analysis.

My only substantive departure from [55] and [69] is the inclusion of the worker-type productivity effect $A_i$, which simply generalizes the two-type setup in [55] for close analogy to worker wage premia. Many of the assumptions follow [55]. Firstly, wages are assumed to be set by employers to maximize profits, subject to constraints on the relationship between wages and the supply of labor. Secondly, I assume that, given the labor supply constraint, firms are only aware of the shape of the curve and cannot set wages so as to wage-discriminate on an individual worker basis. Thirdly, firms hire any worker (of the given type) who is willing to accept a job at the posted wage. I do not model substitution across worker types. Fourthly, for simplicity I ignore capital and intermediate inputs.

Unlike [55], I provide no explicit micro-foundation for the critical parameter $\varepsilon$, the firm labor supply elasticity. I could follow the approach in [55], such that jobs are static differentiated products which workers value through a utility term associated with wages and an idiosyncratic utility term drawn from a Type 1 extreme value distribution. Workers are fully informed about job opportunities. This is then transformed into a logit choice probability, which together with a large number of firms yields a simple firm labor supply elasticity constraint. However, I rather take $\varepsilon$ as given, as in [69], which has the disadvantage of being opaque on its source but the advantage of not taking a strong position on its source (for example, alternatively search frictions could be the source).

To characterize the firm problem and optimal wage: Firm $j$ faces downwards sloping firm-specific product demand parameterized by $\eta$, and has exogenous productivity for each worker type $i$ equal to a firm term $T_j$ times by a worker type term $A_i$. 
For example, production is given by \( Y_{ij} = \frac{\eta}{\eta-1} A_i T_j N_{ij}^{1-1/\eta} \) for \( N_{ij}(w_{ij}) \) the number of workers of type \( i \) at firm \( j \). Firms maximize profit by setting the wage \( w_{ij} \) for each worker type \( i \), \( \max_{w_{ij}} \pi_{ij} = \frac{\eta}{\eta-1} A_i T_j N_{ij}^{1-1/\eta} - w_{ij} N_{ij} \) subject to an upwards sloping firm labor supply constraint, \( N_{ij} = w_{ij}^\varepsilon \) with firm labor supply elasticity \( \varepsilon \).

The marginal cost of labor is given by \((1/\varepsilon)\ln(N_{ij}) + \ln(1 + 1/\varepsilon)\), and marginal revenue product of labor is equal to \( \ln(A_i) + \ln(T_j) - (1/\eta)\ln(N_{ij}) \); see [131] for the same derivations. Then, setting these equal to each other, employment and wages are given by:

\[
\ln(N_{ij}) = \frac{\varepsilon \eta}{\eta + \varepsilon} \ln(A_i) + \frac{\varepsilon \eta}{\eta + \varepsilon} \ln(T_j) + \frac{\varepsilon \eta}{\eta + \varepsilon} \ln\left(\frac{\varepsilon}{1 + \varepsilon}\right)
\]

\[
\ln(w_{ij}) = \frac{\eta}{\eta + \varepsilon} \ln(A_i) + \frac{\eta}{\eta + \varepsilon} \ln(T_j) + \frac{\eta}{\eta + \varepsilon} \ln\left(\frac{\varepsilon}{1 + \varepsilon}\right) + \alpha_i + \phi_j + c
\quad \tag{B.1}
\]

Where the wage equation substitutes the firm labor supply constraint, \( \ln(N_{ij}) = \varepsilon \ln(w_{ij}) \), into the optimal employment equation. Here, \( \alpha_i = \frac{\eta}{\eta + \varepsilon} \ln(A_i) \) is a worker-specific effect, \( \phi_j = \frac{\eta}{\eta + \varepsilon} \ln(T_j) \) is a firm-specific component of wages, and \( c = \frac{\eta}{\eta + \varepsilon} \ln\left(\frac{\varepsilon}{1 + \varepsilon}\right) \) is a constant. This is an additive model of wage-setting consistent with the above statistical model of wage premia in [2].

A few caveats apply to this equation. Firstly, if \( \varepsilon \) is firm-specific, i.e. \( \varepsilon = \varepsilon_j \), then the last term \( c \) also is firm specific, \( c = c_j \), and the firm wage premium becomes \( \phi_j = \frac{\eta}{\eta + \varepsilon} \ln(T_j) + \frac{\eta}{\eta + \varepsilon} \ln\left(\frac{\varepsilon_j}{1 + \varepsilon_j}\right) \). An example of a firm-specific \( \varepsilon_j \) is given by the case of the simple logit model [135], \( \varepsilon_j = \varepsilon(1 - s_j) \) for firm employment share \( s_j \). Note that in this case the \( \alpha_i \) will also be partly firm-specific, in which case this framework does not map as easily to the AKM statistical model. Secondly, firms may vary wages for other reasons not captured in the production function or in the profit-maximization [44], but my aim here is to provide a comparable setup to the literature. In particular, this approach ignores efficiency wage explanations for firm wage premia, which can

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emerge e.g., as a result of monitoring problems. Thirdly, this framework also has no complementarities in worker and firm productivity, i.e. no “match effects”, as assumed by the AKM model, in contrast with for example [122]. This simplifies the setup and framework discussion, but has the disadvantage of not directly allowing a mechanism towards sorting.

Sources of firm wage premia distortion

The framework section in the main text outlines how firm wage dispersion is influenced by productivity dispersion and by the firm labor supply elasticity. For the latter, three channels are relevant. Regarding the third channel, heterogeneity in the firm labor supply elasticity itself, in the main text I referred the reader to this section for details on the likely effects.

The modified pass-through coefficient $\frac{\eta}{\eta + \varepsilon_j}$ will be one source of higher firm wage dispersion, but is likely to have limited effect. A first-order Taylor approximation of $x = \frac{\eta}{\eta + \varepsilon_j}$ suggests $\text{var}(x) = \frac{\eta^2}{(\eta + \bar{x})^4} \text{var}(\varepsilon_j)$, which for $\eta = 5$ and $\bar{x} = .7$ (i.e. $\varepsilon = 2$), gives 0.02 times $\text{var}(\varepsilon_j)$, which is likely to be small. Then the Taylor approximation of the variance of the product of two random variables, $x = \frac{\eta}{\eta + \varepsilon}$ and $y = \ln(T_j)$, is given by $\bar{y}^2 \text{var}(x) + 2\bar{x}\bar{y} \text{cov}(x, y) + \bar{x}^2 \text{var}(y)$. The first term is negligible (given the previous approximation, also noting $y$ is normalized), then there is some dependence on the covariance, and the third term remains as discussed above.

The modified constant $c_j$ would be another source, with a potentially large effect. A first-order Taylor approximation of $y = \ln(\frac{\varepsilon_j}{1+\varepsilon_j})$ suggests $\text{var}(y) = \frac{1}{(\bar{y}(1+\bar{y}))^2} \text{var}(\varepsilon_j)$, which for $\eta = 5$ and $\bar{y} = -.4$ (i.e. $\varepsilon = 2$), gives 17 times $\text{var}(\varepsilon_j)$. Then with $x = \frac{\eta}{\eta + \varepsilon}$, the Taylor approximation (see above) has a positive covariance, and a potentially large third term. Note the firm wage component would be modified to $\phi_j = \frac{\eta}{\eta + \varepsilon} \ln(T_j) + \frac{\eta}{\eta + \varepsilon} \ln(\frac{\varepsilon_j}{1+\varepsilon_j})$, and so the covariance between $\frac{\eta}{\eta + \varepsilon} \ln(T_j)$ and $c_j$ would
also contribute to \( \text{var}(\phi_j) \). The variance of \( \phi_j \) is therefore increased by variance in \( c_j \), in turn increased by a greater variance in \( \varepsilon_j \) and by a lower average value of \( \varepsilon_j \).

### B.4.2 Firm wage inequality and development

The following toy model is intended to provide some intuition as to a simple dynamic between development and firm wage inequality, proceeding from the assumption of a finite firm labor supply elasticity. Initially, at the onset of industrialization, a small portion of firms have high productivity and this attracts a set of workers. Higher productivity firms optimally pay higher wages along the upwards sloping firm labor supply curve. With most workers in low-productivity work, wage inequality is initially low; as industry develops and employment in the higher productivity sector increases, wage inequality first increases and then declines.

The analysis is in the spirit of Kuznets and Lewis, but the mechanisms explicitly operate through optimal firm wage-setting derived from monopsony power. The mechanism yields two insights. Firstly, the rise and fall of wage inequality across the transition from low to high productivities still occurs in a multi-firm setup. Secondly, the firm wage premium is derived through the wage pass-through from higher productivity, which follows from a finite labor supply elasticity; unlike the wage premium in classical models, where the premium is assumed.\footnote{Other mechanisms have derived the wage premium under weaker assumptions, such as the labor turnover model of Stiglitz.}

Some implications that follow from this mechanism are that the arc of wage inequality may be legislated against (as in the case of collective bargaining and minimum wages), and that the speed of transition towards the industrialized sector may be substantially slowed down.

**Firm wage-setting**

Assume the labor supply to the firm \( \varepsilon_{LS} = \partial \ln L_j / \partial \ln w_j \) is less than perfectly elastic. As Manning (2003) illustrates, this may be derived in various ways, such as
from a cost of training and recruitment that increases with firm size, or from a fixed offer probability received by workers per firm. I use a simpler production function than in the main framework above, for expositional purposes: \( Y_j = A_j L_j \), and profits \( \pi_j = Y_j - w_j L_j \), profits are maximized by the firm setting wages which are given optimally as:

\[
w_j = A_j \frac{\varepsilon_{LS}}{1 + \varepsilon_{LS}}
\]  

(B.2)

Wages are therefore set higher for higher firm productivities, unlike in the case of firms as wage-takers. For example, in classical models wages are set at a reservation wage (usually with an assumed premium) following from a perfectly elastic labor surplus.

**Two-sector statics**

Assume there are two sectors, one high productivity sector often termed the modern sector \( M \) in classical development models, and a low productivity sector referred to as the subsistence sector \( S \). To be clear, \( S \) may refer to a range of scenarios, including actual rural subsistence, unemployment, informal work, or a sector with inferior technology.\(^8\) Sector \( S \) faces similar constraints as sector \( M \), except at a lower productivity.\(^9\)

The variance of log wages in this economy is as follows:

\[
var(\ln(w_j)) = (1 - s)\sigma_M + s\sigma_S + s(1 - s)(\mu_M - \mu_S)^2
\]  

(B.3)

---

\(^8\)For simplicity, I do not model the goods sector (alternatively I assume a competitive open goods market), though Ros (2013) suggests interesting dynamics in the interaction of the goods and labor markets.

\(^9\)Alternative assumptions also work, such as a competitive subsistence sector or an average marginal product, as long as the productivity is substantially lower.
where subscripts indicate the sector, $s$ is the proportion of labor in sector $S$, $\sigma_k$ denotes the variance of wages in sector $k$, and $\mu_k$ denote mean wages in sector $k$.

**Dynamics**

I model the transition between sectors $S$ and $M$ by following [4].

They model the industrialization process as the randomized take-up of the modern sector technology, depending on a random arrival process $\gamma$ of exogeneous adoption and being surrounded by at least $k$ modern sector firms out of $m$ total neighbors. Whereas their model focuses on skilled versus unskilled labor, I focus on firm wages as above.

The change in $s$, i.e. adoption of modern technology, is determined by:

$$s_{t+1} = s_t - \gamma - (1 - s_t) \left( \sum_{j=k}^{m} \binom{m}{j} (1 - s_t)^j s_t^{m-j} \right)$$  \hspace{1cm} (B.4)

Combining equations [B.2] [B.3] and [B.4], the variance of wages evolves as the modern sector spreads. In the simple case of $\alpha = 0$, and letting the productivities differ by a factor of 2, $A_M = 2$, $A_S = 1$ (with equal variance of 1), there is a wage arc. Figure [B.9] illustrates this. Initially, very few firms have the modern sector technology. As time passes, and surrounding firms learn, sector $M$ grows and this increases total wage inequality as the population-level between-sector gap grows. However, as the modern sector technology passes half of the population, wage inequality declines again.

---

10 As an alternative, we can incorporate the firm labor supply elasticity $\varepsilon_{LS}$ in as follows. Assume that technology transfer occurs through worker transitions and learning-by-doing. Then the rate that firms adopt the new technology depends on the firm labor supply elasticities. This however requires a flow of workers from high-wage $M$ sector firms to low-wage $S$ sector firms.
Figure B.9: Simulation of firm wage inequality as high productivity technology spreads

(a) Share in low-productivity

(b) Variance in wage premia by LSE

Notes. Panel A depicts the share in the low productivity sector $S$ as opposed to the high productivity sector $M$ over time. I use the values $\gamma = 0.4$, $k = 3$, $m = 5$ in Equation B.4 and a firm labor supply elasticity of $\varepsilon = 2$ for Equation B.2.
B.5 Matched worker mover event-study

These estimates follow the event-study approach in [24], henceforth BDN. An event-study panel is constructed as follows. All Employment to Employment (E-E) separators in the year 2013 are isolated, along with these workers’ surrounding annual records (a maximum of 2008 to 2018). The sample is further restricted to workers who were at the same “Origin Firm” in 2012 as in 2013. Event-time is indexed such that 0 indicates 2014, i.e. pre-periods are at the Origin Firm and post-periods are at the Destination Firm.

B.5.1 First stage: Firm wage variation

Firm average wages are estimated for Destination Firms based on stayers, i.e. excluding workers who are hired or separate in each year. The first stage regression consists of the change in a worker’s wage across the Origin and Destination Firms as the outcome, and the change in firm average wages as regressor (Equation B.5). The primary benefit of this approach is that interacted fixed effects \( L(history_{i,t}) \) can transparently be included to control for confounders, unlike for AKM, such that estimates compare workers matched on these characteristics. By comparing the change in own wage \((w_{i,D(i),t} - w_{i,O(i),t-1})\) to change in firm wage \((\bar{w}_{i,D(i),t} - \bar{w}_{i,O(i),t-1})\) for finely matched workers who leave the same Origin firm \(O\) towards different Destination firms \(I\), this approach measures how much of the variation in own wage across firm switches is due to differences in firm wage policies.

\[
w_{i,D(i),t} - w_{i,O(i),t-1} = \phi(\bar{w}_{i,D(i),t} - \bar{w}_{i,O(i),t-1})(f_{D(i),t} - f_{O(i),t-1}) + L(History_{i,t,d}) + \epsilon_{i,t}
\]  

(B.5)

In brief, this matches workers who leave the same firm at the same time with similar characteristics (wage, tenure, age, gender), and compares the change in their respective wages relative to the changes in average firm wages at the new firms. While
this approach is most suited to estimating the separations elasticity (see next section), it also measures as a first stage how much of the variation in own wages across firm switches is due to differences in firm wage policies. The advantages over the AKM structure are that this approach allows a much richer set of controls regarding worker histories, and also does not require constant firm wage premia for all workers in a firm.

Figure B.10 shows a flat pre-trend, which is an analogous falsification check on the exogeneity assumption of the destination firm wage as in Figure 1 of the main text. The wage trend after the move is also stable, indicating that concerns such as tenure profiles are not important.

![Figure B.10: First stage: Difference in log wage on difference in firm average wages](image)

**Notes.** See Online Appendix D for sample construction and specification details. The regression includes fixed effects for Origin firm, salary bins (12), fraction of year employed in bins (8), tenure bins (3), gender bins (2), and age bins (8). Origin firms are restricted to a firm size of at least 20 workers. The difference in individual and firm wages are censored at the 1% tails. Observations are restricted to Origin firm (before the event) and Destination firm (after the event).

Row 1 of Table B.7 presents results from this first-stage regression using specifications with increasing controls. The unconditional coefficient is 0.53 (column 1), and this increases to 0.56 when conditioning on the same Origin firm. The preferred
estimate of about 0.65 compares workers leaving the same firm in the same year, earning similar wages, having been at the firm for similar tenure, and sharing demographic characteristics of gender and age (columns 3-7). With industry controls the first stage coefficient is even higher (0.73, column 8), but this may be a bad control in the sense that choice of industry is within the causal pathway. The coefficient of 0.65 from this regression of change in own wage on change in firm wage is high compared to [24], who use an identical design to find an estimate of 0.32 for Oregon, USA. That is, two thirds of the variation in the wages of closely matched workers across firm switches is due to differences in firm wage policies in South Africa.

B.5.2 Second stage: Labor supply elasticity

BDN develop this approach to measure the firm labor supply elasticity from worker re-separations as follows. The equation above forms the first stage of an instrumental variables regression. The reduced form has an indicator for worker separation from the Destination firm in period $t+k$ as the outcome ($s_{i,t+k}^D$), and the change in average firm wage ($\bar{w}_{i,D(i),t} - \bar{w}_{i,O(i),t-1}$) as regressor. Identification arises from comparing matched workers from the same Origin firm who separate to different Destination firms, each being treated with a different average firm wage at their Destination firm, and this leads to correspondingly different separation rates.

$$s_{i,t+k}^D = \delta(\bar{w}_{i,D(i),t} - \bar{w}_{i,O(i),t-1}) + L(\text{History}_{i,t,d}) \times 1_{t+k} + \epsilon_{i,t+k} \quad (B.6)$$

Table B.7, column 1, shows the implied labor supply elasticity estimate of 0.8 using the unconditional change in wages is very similar to the main AKM estimate of about 0.9. Adding fixed effects progressively increases this estimate, with a labor

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11This is comparable to the KSS-based estimate, which decomposes the total wage variation into 30% firms, 9% covariance, 40% workers, and 21% residual. Since the movers approach compares similar workers, it removes the worker and covariance terms. The comparable KSS magnitude is therefore $0.3/(0.2 + 0.21) = 0.59$, which is very close to the movers estimates of 0.65.
supply elasticity based on the separations elasticity in the range of 1.3 to 1.6. Comparing workers who leave the same Origin firm in the same year yields an elasticity of 1.3 (column 2), adding wage, tenure and demographic controls increases this to 1.4 (column 3), adding further covariates hardly changes this estimate (column 5), and adding prior wage growth of the worker in the pre-period increases this to 1.6 (column 6).

Including industry controls increases the estimate to 1.8, but once again it is unclear if this is a good control to include. The elasticity based on E-E separations is higher, for the corresponding controls (columns 4 and 7), which is in line with the discussion in the main paper.

As for BDN, my preferred estimate of 1.6 is higher than my AKM estimate. Relative to Oregon where the estimate is $\varepsilon_{LS} = 3$ using earnings as in BDN, this estimate for South Africa is low – consistent with the claim in this paper that South Africa has relatively high monopsony power. The instrumental variables relationship between change in firm average wage and separations is shown in figure B.11.
Table B.7: Separations estimates

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
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<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
<th>(7)</th>
<th>(8)</th>
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</thead>
<tbody>
<tr>
<td><strong>First stage</strong></td>
<td>0.534</td>
<td>0.562</td>
<td>0.645</td>
<td>0.646</td>
<td>0.700</td>
<td>0.643</td>
<td>0.645</td>
<td>0.729</td>
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<td></td>
<td>(0.015)</td>
<td>(0.011)</td>
<td>(0.014)</td>
<td>(0.015)</td>
<td>(0.017)</td>
<td>(0.017)</td>
<td>(0.018)</td>
<td>(0.043)</td>
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<td><strong>IV estimate</strong></td>
<td>-0.399</td>
<td>-0.628</td>
<td>-0.713</td>
<td>-0.948</td>
<td>-0.720</td>
<td>-0.794</td>
<td>-1.073</td>
<td>-0.896</td>
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<td></td>
<td>(0.021)</td>
<td>(0.019)</td>
<td>(0.043)</td>
<td>(0.059)</td>
<td>(0.048)</td>
<td>(0.055)</td>
<td>(0.074)</td>
<td>(0.083)</td>
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<td><strong>Implied LSE</strong></td>
<td>0.8</td>
<td>1.26</td>
<td>1.43</td>
<td>1.9</td>
<td>1.44</td>
<td>1.59</td>
<td>2.14</td>
<td>1.79</td>
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<td></td>
<td>(0.042)</td>
<td>(0.038)</td>
<td>(0.086)</td>
<td>(0.12)</td>
<td>(0.096)</td>
<td>(0.11)</td>
<td>(0.15)</td>
<td>(0.17)</td>
</tr>
<tr>
<td><strong>Obs</strong></td>
<td>0.582</td>
<td>0.580</td>
<td>0.501</td>
<td>0.437</td>
<td>0.282</td>
<td>0.480</td>
<td>0.419</td>
<td>0.225</td>
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<tr>
<td><strong>Movers</strong></td>
<td>268445</td>
<td>267090</td>
<td>189845</td>
<td>164077</td>
<td>101104</td>
<td>170670</td>
<td>147387</td>
<td>76218</td>
</tr>
<tr>
<td><strong>Fstat (IV)</strong></td>
<td>1068</td>
<td>2058</td>
<td>1556</td>
<td>1269</td>
<td>1050</td>
<td>920</td>
<td>715</td>
<td>262</td>
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<tr>
<td><strong>E-E seps</strong></td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
</tbody>
</table>

**Interacted controls**

| Year | Y | Y | Y | Y | Y | Y | Y | Y |
| Firm | Y | Y | Y | Y | Y | Y | Y | Y |
| Wage × tenure | Y | Y | Y | Y | Y | Y | Y | Y |
| Covariates | Y | Y | Y | Y | Y | Y | Y | Y |
| Add. covariates | Y |
| Prior wage growth | Y | Y |
| Dest. industry | Y |

*Notes.* Covariates refer to gender bins (2), and age bins (8). Additional covariates refer to annual bonus bins (8) and wage bins exclusive of benefits (8). Origin firms are restricted to a firm size of at least 20 workers.
Figure B.11: Instrumental variables bin scatter of separations on difference in firm average wages

Notes. The regression includes fixed effects for Origin firm, salary bins (12), fraction of year employed in bins (8), tenure bins (3), gender bins (2), and age bins (8). It also includes a control for worker skill, as proxied by the AKM worker fixed effect. Origin firms are restricted to a firm size of at least 20 workers. The difference in individual and firm wages are censored at the 1% tails. The instrumental variables regression is implemented using the control function approach.
B.6 Firm rent-sharing elasticity

B.6.1 Estimation framework

In models of monopsonistic labor markets, rent-sharing is an optimal wage-setting outcome \[55, 122\]. Firms with higher marginal revenue product (or rents) gain more from employing more workers, which requires increases in the wage to attract more workers. Since wages are more sensitive to firm-specific rents in more monopsonistic markets (see framework above), the relatively low estimated firm labor supply elasticity suggests a high rent-sharing elasticity.

In this section, I estimate the passthrough coefficient on firm productivity to firm wage premia, also known as the rent-sharing elasticity (\(\varepsilon_{\text{rent}}\)). I follow the literature in using value added per worker as a proxy for these “quasi-rents”. My main specification follows \[56\] in regressing the estimated firm wage premium \(\phi_j\) on log firm value added per worker \((VA_j)\), with controls \(X_j\) for time, industry and location.

\[
\phi_j = \alpha + \varepsilon_{\text{rent}} \ln(VA_j) + \Gamma X_j + r_j
\]  

(B.7)

The estimation equation is in essence a firm-level and cross-sectional. However, following \[56\], I run this at the individual level while appropriately clustering at the firm level. The results are very similar when running at the firm level.

Assuming as in the AKM model that individual wages can be decomposed into an invariant worker effect and a firm effect, any firm specific effect should reflect as differences in the firm component of the wage. The earlier literature on rent-sharing tended to use wages as the outcome, which yielded an upwardly biased rent-sharing coefficient since more profitable firms tend to employ more workers with higher invariant worker effects. Wages are then higher due to selection on worker effects as well as rent-sharing. The AKM firm wage premium effectively controls for individual characteristics, including unobservables. To illustrate the bias, I also
estimate the Equation B.7 using log firm average wage instead of the estimated firm wage premium.

Omitted variables correlated with profits and the firm wage premia may still bias the results. As an alternative specification that also does not rely on the estimated firm wage premia, I use a differenced equation as follows:

\[
\ln(w_{j,t}) - \ln(w_{j,t-s}) = \alpha + \varepsilon_{\text{rent}}(\ln(VA_{j,t}) - \ln(VA_{j,t-s})) + \Gamma X_{j,t} + \epsilon_{j,t} \quad (B.8)
\]

Equation B.8 shows the change in log firm average wage, the change in log firm value added per worker, and controls which vary by firm and time. I run this at the firm by year level, weighted by firm size and clustered by firm. The source of variation in Equations B.7 and B.8 are different, allowing us to assess sensitivity to method. Identification in a differenced setting also includes incumbent workers rather than just the movers used to estimate the wage premia in AKM. One challenge is that annual movements in value added are subject to measurement error, for example adjustments to the balance sheet related to asset purchases rather than actual profits. I therefore use as a baseline one period difference \((s = 1)\), but supplement this by taking longer differences \((s = 3)\) following \[95\]. This reduces measurement error by increasing the signal-to-noise ratio in the differenced regressor. I also include fixed effects for industry by location to isolate variation from firm-specific shocks as opposed to market level shocks.

B.6.2 Estimates of rent-sharing

Firm wage premia increase strongly with firm rents. The associated rent-sharing elasticity is high relative to other industrialized countries, and this variation in value added per worker explains about 25% of the total variance in firm wage premia.
Across the distribution of log firm value added per worker, there is a strong linear relationship with log wages (Figure B.12).

Figure B.12: Non-parametric scatter of firm wage premia and log value added per worker

Notes. Firm average wage refers to directly recorded wages for each worker, and are centered around 0 for plotting. Firm fixed effects are estimated using the AKM regression. The plot is generated from firm-level data weighted by number of workers and limited to connected firms with more than 20 employees.

Table B.8 presents estimates of the rent-sharing elasticity, all demonstrating a strong correlation with p-values below 0.001. Column 1 shows an elasticity of 0.3 using the raw wage, which is upwardly biased since high wage workers tend to locate at high value added firms. This is also confirmed by a direct regression of the worker wage premia on log value added, with a coefficient of 0.12 suggesting positive sorting. Using the firm wage premia instead of raw wages, the coefficient is 0.14 (column 2), and similarly when restricting to firm wage premia estimated from firm closings (column 3).
ln(VApe) & 0.301 & 0.141 & 0.140 & 0.141 & 0.191 & 0.171 \\  & (0.011) & (0.008) & (0.010) & (0.036) & (0.039) & (0.024) \\ 
Obs & 28 mill. & 26 mill. & 26 mill. & 127,289 & 66,832 & 66,532 \\ 
$R^2$ & .21 & .25 & .21 & .13 & .23 & .336 \\ 
log(wage) & Y & Y & Y & Y & Y & Y \\ 
Firm FE & Y & Y & Y & Y & Y & Y \\ 
All firms & & & & & & \\ 
Closings & & & & & & \\ 
First diff. & & & & & & \\ 
Long diff. & Y & & & & & \\ 
Market × Year FE & & & & & & \\ 

Table B.8: Rent-sharing elasticities

Notes. VApe is value added per employee as a proxy for rent, calculated as sales minus non-labor expenses. Firm FE refer to the estimated AKM firm wage premia from section 3, respectively estimated off of the full sample (“all”) and the sample of workers from firm closings (“Closings”). The final 3 specifications are run at the firm-level (weighted by number of workers), comparing differenced outcomes within firms over time. The long difference ($\ln va_t - \ln va_{t-3}$) is taken over three periods. Market FE refer to industry by location fixed effects. Workers are limited to connected firms with more than 20 employees. Standard errors are given in parentheses. Source: Own calculations, South African tax records, 2011-2016.

The first-differences specification is again similar with an estimate of 0.14 (column 4). However, adjusting for measurement error by taking the long difference across time periods increases the coefficient substantially to 0.19. To ensure this is not driven by industry-level adjustments, I control for market-level differences [122]. The estimate rent-sharing elasticity is robust to this control, giving my preferred estimate of 0.171 (standard error of 0.024).

Table B.9 provides further robustness. The AKM estimates are similar when using gross profits as a measure of rent instead of value added (elasticity of 0.12), or total factor productivity as proxied by the residual of a regression of sales on cubics in firm size, assets and material costs (elasticity of 0.14). Winsorizing or adding market fixed effects reduces the estimate slightly to 0.11. Focusing on the wages of hires only increases the estimate to 0.16.
Table B.9: Alternative specifications for rent-sharing elasticity

<table>
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<tr>
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<th>(4)</th>
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<tbody>
<tr>
<td>ln(Rent)</td>
<td>0.123</td>
<td>0.143</td>
<td>0.112</td>
<td>0.115</td>
<td>0.159</td>
</tr>
<tr>
<td></td>
<td>(0.008)</td>
<td>(0.020)</td>
<td>(0.007)</td>
<td>(0.006)</td>
<td>(0.042)</td>
</tr>
</tbody>
</table>

Rent measure
- Value Added: Y
- Profit: Y
- TFP: Y
- Winsor: Y
- Market FE: Y
- Firm First Diff: Y
- Hires only: Y

Obs: 2.52e+07 2.18e+07 2.83e+07 2.61e+07 103197

Note: Rent is the regressor, measured as indicated by value added per employee (sales minus non-labor costs, per worker), profit per worker (sales minus non-labor costs and labor costs), or total factor productivity (calculated as the firm effect residual from a regression of sales on cubic terms in firm size, assets, intermediate expenses and year effects). Winsor indicates the firm wage premia are winsorized at the 5 percent tails. Market FE are industry by geo fixed effects, i.e. 20 by 221 categories. The final specification is run at the firm-level in first-differences, where the outcome is the average wage for new hires only. Workers are limited to those at connected firms with more than 20 employees. Source: Own calculations, South African tax records, 2011-2016.

Overall, these estimates are towards the upper end of the range found in the AKM rent-sharing literature of 0.05 to 0.15\[55\]. They are consistent with the high firm wage dispersion and labor supply elasticities estimated above.

B.6.3 Relating rent-sharing and labor supply elasticities

A broad class of models finds that rent-sharing increases in more monopsonistic markets. Intuitively, when markets are competitive, workers are close to their marginal product and firms can adjust on the employment margin only. Any wage premia quickly disappear as workers flow to the highest-paying firms and market wages adjust. In more monopsonistic markets, workers are paid further below marginal product and are unable to compete away wage markdowns. Firms-specific shocks can
affect firm-specific wages more persistently as firms adjust on both the wage and employment margins. To illustrate this relationship between the rent-sharing elasticity ($\varepsilon_{\text{rent}}$) and the labor supply elasticity ($\varepsilon_{\text{LS}}$), I consider the simplest case and then compare three models proposed in the recent literature.

In the simplest case, a profit-maximizing firm with an upwards sloping labor supply curve has the profit function $\pi = pTQ(L) - wL(w)$, where $T$ indicates productivity. The profit maximizing wage $w = \frac{\varepsilon_{\text{LS}}}{1+\varepsilon_{\text{LS}}}Tp$ implies a markdown on productivity of $\frac{\varepsilon_{\text{LS}}}{1+\varepsilon_{\text{LS}}}$ and a rent-sharing elasticity $\varepsilon_{\text{rent}} = \frac{\partial \ln w}{\partial \ln T} = 1$. Taking derivatives, the $\varepsilon_{\text{rent}}$ is independent of the $\varepsilon_{\text{LS}}$, and the predicted $\varepsilon_{\text{rent}} = 1$ is much larger than the range of estimates in the literature to date, $0.05 < \hat{\varepsilon}_{\text{rent}} < 0.15$ \cite{55}. However, we are interested in models which allow for firm-specific shocks, examples of which are given below.

In my framework in the main text, as in \cite{55} and \cite{130} pp. 338-341, firms have downward-sloping firm-specific product demand. With $Y = \frac{1}{1+1/\eta}T_jN^{1-1/\eta}$ and $\ln(w) = 1/\varepsilon\ln(N_j)$, log marginal factor cost $(1/\varepsilon)\ln(N_j) + \ln(1+1/\varepsilon)$ equates to log marginal revenue $\ln T_j + (1/\eta)\ln(N_j)$, and the implied rent-sharing elasticity equation is $\frac{d\ln(w)}{d\ln(T_j)} = \frac{\eta}{\eta + \varepsilon}$, where $\eta$ is the downwards-sloping firm-specific demand elasticity and $\varepsilon$ is the firm labor supply elasticity. This implies $\frac{d\ln(w)}{d\ln(T_j)}d\varepsilon = -\frac{\eta}{(\eta + \varepsilon)^2} < 0$, meaning that the rent-sharing decreases with an increase in the firm labor supply elasticity.

A second model is proposed by \cite{122}, with parameters $\beta$, the standard deviation in idiosyncratic tastes for a firm; $\lambda$, an exogenous tax parameter; and $\rho$, the independence of tastes within a labor market. They model that the wage can be written as $w = \frac{1}{1+\frac{\lambda}{\rho}}p$, where $p$ is the marginal product of labor. Given the standard markdown equation, this implies that $\varepsilon_{\text{LS}} = \frac{\lambda}{\rho}$. For estimating these parameters, they model that $\varepsilon_{\text{rent}} = \frac{1}{1+\frac{\lambda}{\rho}}$ which implies that $\varepsilon_{\text{rent}} = \frac{1}{1+\varepsilon_{\text{LS}}}$. The $\varepsilon_{\text{rent}}$ and $\varepsilon_{\text{LS}}$ are negatively correlated – an equation very similar to and consistent with the previous model. Using their parametrization, my estimate of $\varepsilon_{\text{rent}} = 0.23$ implies $\varepsilon_{\text{LS}} = 3.3$; alternatively,
my estimate of $\varepsilon_{LS} = 2$ implies $\varepsilon_{rent} = 0.33$. Additional constraints may reconcile these different predictions of the magnitudes of $\varepsilon_{rent}$ and $\varepsilon_{LS}$.

A third model can be derived from the multinomial logit, as in [135]. Let the utility of workers be expressed as $V(w_j) = \beta \ln(w_j) + \nu_{ij}$, where $\beta$ parameterizes the latent monopsony power (i.e. the responsiveness of worker utility to wages), and $\nu_{ij}$ follows a Gumbel distribution indicating idiosyncratic preferences for the firm. The distribution yields the probability a worker is employed at firm $j$, or equivalently the firm share of $j$, in log terms $\ln p_j = \beta \ln(w_j) - \ln(N)$ (assuming firms are atomistic). The optimal wage response is pinned down by the firm’s wage-setting function. Firm’s maximize profits $\pi_j = \max_{w_j} \frac{1}{1-\eta} T_j (p_j(w_j)N)^{1-\eta} - w_j \cdot p_j(w_j)N$, which yields the associated wage $\ln w_j = \frac{1}{1+\eta^3} (\ln(\varepsilon_{jj} + \ln T_j)$. This is similar to the first model of [55], both derived from logit idiosyncratic preferences. The implied rent-sharing is $\frac{d \ln(w)}{d \ln(T_j)} = \frac{1}{1+\eta^3}$, which as before is increasing in monopsony power.

This brief comparison of selected models illustrates the negative relationship between $\varepsilon_{rent}$ and $\varepsilon_{LS}$, derived in a variety of ways. The exact functional forms of this negative relationship depends on the model, however, which serves as some motivation for the separate estimation of $\varepsilon_{rent}$ and $\varepsilon_{LS}$. Moreover, other constraints are likely to play a role. As an illustration, a crude calibration using the framework in this paper motivates a rent-sharing elasticity of $\frac{d \ln(wage)}{d \ln(MRPL)} = \frac{\eta}{\eta + \varepsilon}$, as in [55, 130], which depends on other parameters ($\eta$). Using $\eta = 3$ to $\eta = 10$ as in [55], and a firm labor supply elasticity of $\varepsilon = 2$, the implied range of the rent-sharing elasticity is between 0.6 and 0.83 — which is much higher than the empirical range reviewed in [55]. (Of course, if $\eta$ was closer to a half, that would rationalize the estimates in this paper.)
B.7 Imperfect competition in the informal sector

B.7.1 Worker transitions across sectors

Given that the tax data are restricted to formal employment, I use survey data. The survey design includes a 25% outrotation panel component, which allows me to link workers over time. While the clear advantage is observing informal and unemployed workers, the main disadvantages relative to the tax data are a lack of firm identifiers (meaning I am unable to control for AKM firm fixed effects), and larger measurement error in wages.

I define a formal worker as any worker with a written employment contract, deductions for benefits (such as unemployment insurance, pension or medical aid), or deductions for income tax. The informally employed consist of all other workers. I separate out informal single-person business owners (identified as self-employed, employing no paid labor, and with no tax registration), as these are likely survivalist enterprises that simply subsist. Informal workers include paid workers in both formal and informal enterprises.

To set the context, Table B.10 presents the frequency of workers by transition and sector. Out of every 10 economically active workers, 5 are formally employed, 2 are informally employed, and 3 are unemployed. The transitions indicate that informal sector workers are more likely to move out of their jobs than their formal sector counterparts. Panel A shows that formal employment is the most stable category, with 91% of formally employed workers remaining in their category by the following quarter, compared to only 65% of the informally employed. Nearly 1 in 5 workers transition from the informal to formal sector, compared to only 1 in 25 in the reverse direction.

12 The public release of the QLFS includes a household identifier which is consistent for repeated respondents. However, person identifiers within households may change if household composition has changed. I validate the person identifier by requiring consistency in gender, age, race, and educational attainment.
Table B.10: Panel B only considers workers who separate from their job. Note that quarterly job separations for workers in the formal sector are 7%, compared to about 20% for informal and subsistence workers. Even conditional on a separation, formal sector workers tend to stay more in their category and switch less to the other sector, than informal sector workers.

Table B.10: **Frequency of worker transitions, by sector**

<table>
<thead>
<tr>
<th>Sector in t+1 (row %)</th>
<th>Formal</th>
<th>Informal</th>
<th>Subsist</th>
<th>Unemployed</th>
<th>NEA</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Panel A: All workers</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Formal</td>
<td>91</td>
<td>4</td>
<td>1</td>
<td>3</td>
<td>2</td>
<td>100</td>
</tr>
<tr>
<td>Informal</td>
<td>18</td>
<td>65</td>
<td>3</td>
<td>10</td>
<td>4</td>
<td>100</td>
</tr>
<tr>
<td>Subsist</td>
<td>5</td>
<td>7</td>
<td>75</td>
<td>8</td>
<td>6</td>
<td>100</td>
</tr>
<tr>
<td>Unemployed</td>
<td>5</td>
<td>5</td>
<td>1</td>
<td>74</td>
<td>15</td>
<td>100</td>
</tr>
<tr>
<td>NEA</td>
<td>1</td>
<td>1</td>
<td>1</td>
<td>13</td>
<td>84</td>
<td>100</td>
</tr>
<tr>
<td><strong>Panel B: Separations only</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Formal</td>
<td>25</td>
<td>5</td>
<td>1</td>
<td>46</td>
<td>23</td>
<td>100</td>
</tr>
<tr>
<td>Informal</td>
<td>6</td>
<td>22</td>
<td>1</td>
<td>52</td>
<td>19</td>
<td>100</td>
</tr>
<tr>
<td>Subsist</td>
<td>5</td>
<td>7</td>
<td>11</td>
<td>46</td>
<td>31</td>
<td>100</td>
</tr>
</tbody>
</table>

**Notes.** NEA indicates not economically active. Unemployed follows the expanded definition of unemployment, i.e. it includes those who would like a job but have not sought employment in the last week. Subsist refers to informal single-worker business owners. The average separations proportion for formal, informal and subsist are (respectively) 7%, 20% and 18%.

Moreover, the wage changes across these transitions suggest that informal sector jobs pay lower than formal sector jobs for the same worker. Table B.11 Panel B highlights gains of 2.5% for workers who switch across formal sector jobs (as shown in the main analysis), but large losses of −9.2% for workers who switch from formal to informal sector jobs. On the other hand, workers who switch from informal to formal sector jobs experience large wage gains of 11.2%. This asymmetry rules out that differences are mainly driven by, for example, wage losses on quitting. Rather, they imply that these formal sector jobs simply have higher wage premia.
Table B.11: Wage change over worker transitions, by sector

<table>
<thead>
<tr>
<th></th>
<th>Formal</th>
<th>Informal</th>
<th>Average</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Panel A: All workers</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Formal</td>
<td>1.2%</td>
<td>-3.1%</td>
<td>1.0%</td>
</tr>
<tr>
<td>Informal</td>
<td>3.4%</td>
<td>-0.4%</td>
<td>0.1%</td>
</tr>
<tr>
<td><strong>Panel B: Separations only</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Formal</td>
<td>2.5%</td>
<td>-9.2%</td>
<td>1.1%</td>
</tr>
<tr>
<td>Informal</td>
<td>11.2%</td>
<td>1.4%</td>
<td>3.2%</td>
</tr>
</tbody>
</table>

Notes. NEA and unemployed are omitted as workers earn no wage in these categories. Subsist refers to informal single-worker business owners.

B.7.2 Informal sector firm labor supply elasticities

How does the existence of the informal sector affect the firm labor supply elasticities $\varepsilon$ estimated in the main results? Firstly, one method of estimating the $\varepsilon$ is to find the elasticity of any separation to the wage, regardless of destination. This means that the existence of the informal sector should not affect the main results, as long as they are considered as a firm labor supply elasticity for formal sector firms only.

Nonetheless, it is worth investigating the elasticity of formal job separations to informal jobs, as well as the elasticity of informal jobs to other states. Before discussing the results, note that the elasticities from these survey data are not comparable to the main results due to attenuation bias from error in measuring wages as well as fewer controls. The labor supply elasticities reported here are around 0.3 (with controls), which is far lower than the elasticities using the tax data of around 0.9 (or 1.6 using the movers specification). Of interest in this table are therefore the relative elasticities (assuming the bias is approximately constant across groups), and a similar approach is used in heterogeneity analysis in [123].

Controls do affect the relative estimates substantially (Table B.12 columns 1-4). However, separations from a job in the formal sector to a job in the informal sector appear to be more sensitive to wages ($\varepsilon_{sep}^F = -0.27$) than separations to either formal
sector jobs \((\varepsilon_{sep}^{F-F} = -0.17)\) or unemployment \((\varepsilon_{sep}^{F-N} = -0.16)\) (column 4). This could indicate informal firms poaching underpaid formal workers, or it could indicate that lower paid formal sector workers, after quitting, are more likely to have comparable informal jobs to work that are better than unemployment.

Workers employed in informal sector firms also appear to be sensitive to lower wages. The labor supply elasticity with controls of 0.3 is similar to that of formal sector workers for this dataset (column 6 compared to column 4). Recall that these are not subsistence single-worker business owners, but rather employees. They include, for example, domestic workers in private households, paid workers from informal sector enterprises, and informally employed workers in the formal sector. Perhaps informally employed workers may not face such different wage dynamics to their formal sector counterparts.
### Table B.12: Firm labor supply elasticities, by sector

<table>
<thead>
<tr>
<th>Separations to:</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Any job</td>
<td>-0.338</td>
<td>-0.164</td>
<td>-0.338</td>
<td>-0.164</td>
<td>-0.159</td>
<td>-0.151</td>
</tr>
<tr>
<td></td>
<td>(0.013)</td>
<td>(0.015)</td>
<td>(0.013)</td>
<td>(0.015)</td>
<td>(0.012)</td>
<td>(0.014)</td>
</tr>
<tr>
<td>Formal job</td>
<td>-0.245</td>
<td>-0.167</td>
<td>-0.245</td>
<td>-0.167</td>
<td>-0.092</td>
<td>-0.157</td>
</tr>
<tr>
<td></td>
<td>(0.030)</td>
<td>(0.034)</td>
<td>(0.030)</td>
<td>(0.034)</td>
<td>(0.055)</td>
<td>(0.062)</td>
</tr>
<tr>
<td>Informal or no job</td>
<td>-0.376</td>
<td>-0.169</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.014)</td>
<td>(0.017)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Informal job</td>
<td>-0.522</td>
<td>-0.273</td>
<td>-0.177</td>
<td>-0.167</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.048)</td>
<td>(0.056)</td>
<td>(0.027)</td>
<td>(0.030)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>No job</td>
<td>-0.366</td>
<td>-0.162</td>
<td>-0.176</td>
<td>-0.163</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.015)</td>
<td>(0.018)</td>
<td>(0.015)</td>
<td>(0.017)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Labor supply elast.</td>
<td>0.676</td>
<td>0.328</td>
<td>0.676</td>
<td>0.328</td>
<td>0.318</td>
<td>0.302</td>
</tr>
<tr>
<td></td>
<td>(0.052)</td>
<td>(0.060)</td>
<td>(0.052)</td>
<td>(0.060)</td>
<td>(0.048)</td>
<td>(0.056)</td>
</tr>
<tr>
<td>Obs (thousands)</td>
<td>79</td>
<td>79</td>
<td>79</td>
<td>79</td>
<td>31</td>
<td>31</td>
</tr>
<tr>
<td>Controls</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
</tbody>
</table>

**Notes.** The entries show separations elasticities where workers separate from a job in the “current sector” (last 2 rows) to a job (or no job) in the sector denoted in the estimate row heading. All separations refer to job separations, not sector. Coefficient rows indicating separate regressions pertaining to the sector of the new job (or no job). The labor supply elasticity is computed as -2 times the separations elasticity from the “any job” estimate. Controls refer to fixed effects for time, sex, years of education, race, and age.

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**B.7.3 Informal sector firm rent-sharing elasticities**

As a last piece of insight into informal sector wage-setting, I investigate rent-sharing elasticities of informal sector firms. A cross-sectional firm-level survey is run every 4 years of informal businesses, by re-interviewing respondents in the QLFS who identified as informal business owners. Owners respond with information on sales, profits and employees. Most informal sector firms, 80%, are single-worker owner run businesses – termed survivalist or subsistence above, and these firms tend to have lower sales per worker.
Table B.13: Firm rent-sharing elasticities, informal firms

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Rent share elast</td>
<td>0.324</td>
<td>0.259</td>
<td>0.406</td>
<td>0.351</td>
<td>0.447</td>
<td>0.350</td>
</tr>
<tr>
<td></td>
<td>(0.038)</td>
<td>(0.035)</td>
<td>(0.033)</td>
<td>(0.037)</td>
<td>(0.038)</td>
<td>(0.040)</td>
</tr>
<tr>
<td>Obs</td>
<td>657</td>
<td>630</td>
<td>660</td>
<td>636</td>
<td>606</td>
<td>588</td>
</tr>
<tr>
<td>Rent measure</td>
<td>sales</td>
<td>sales</td>
<td>profit</td>
<td>profit</td>
<td>sales</td>
<td>sales</td>
</tr>
<tr>
<td>Includes owner</td>
<td>Y</td>
<td>Y</td>
<td></td>
<td></td>
<td>Y</td>
<td></td>
</tr>
<tr>
<td>Controls</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>Y</td>
<td>Y</td>
</tr>
</tbody>
</table>

Notes. Sales and profit are measures in log terms per worker. Rent share elasticity refers to the coefficient on the rent measure in a regression where the outcome is the establishment average wage (as in Equation B.7). Controls include the industry, the number of employees by age, race and sex categories, as well as the owner’s education.

Informal sector firms that employ workers appear to increase wages as sales or profits per worker increase. Table B.13 report rent-sharing elasticities with controls in the range of 0.26 to 0.35. While fewer controls likely biases these estimates upwards, for example inadequate controls for worker quality, these magnitudes are high but in line with estimates for the formal sector. For example, my main results report a rent-sharing elasticity of 0.17 as the preferred estimate and 0.3 without worker quality controls using the tax data (see Table B.8) – which is close to the range reported in Table B.13. Including the owner salaries increases the rent-sharing estimates, suggesting that owners disproportionately increase their own salaries more (as would be expected in profit maximizing firms).
APPENDIX C

SUPPLEMENTARY MATERIAL: COLLECTIVE BARGAINING AND SPILLOVERS IN LOCAL LABOR MARKETS
C.1 Appendix: Descriptive data

C.1.1 Descriptives of bargaining council firms

Bargaining council firms cover 40% of formal sector workers, and account for as much of total revenue. Table C.1 presents some comparisons between bargaining council and other firms. On average, bargaining council firms carry a wage premium of about 15% (column 3), and have much less within-firm wage inequality than uncovered firms (columns 4 and 5). These are characteristics consistent with the literature \[48\], which I show later are causally linked to responses to wage agreement changes.

Where are bargaining councils located in the economy? Figure C.1 shows the proportion of bargaining council workers by industry and earnings decile. Bargaining councils are concentrated mainly in manufacturing (such as Metals and Engineering or Chemical), construction (such as Building or Civil Engineering), and transport (such as Road Freight and Logistics) in addition to covering the public sector (the major part of social services); but are spread across all sectors of the economy. Bargaining council workers are mostly in the upper middle parts of the firm earnings distribution, increasing in proportion from about 20% in the lowest decile up to 70% in the 8th decile and then dropping off to 30% in the uppermost decile. Part of this is endogenous: bargaining council firms are higher in the firm earnings distribution \textit{because} they are bargaining council firms, i.e. wages are higher from negotiated agreements. But part of this is also the types of firms, as the \textit{unconditional} value added for bargaining council firms is higher. Either way, this figure shows that marginal changes in the wage premium are likely to affect upper middle parts of the firm earnings distribution more.

Table C.14 in the Data Appendix (section C.7) provides a detailed breakdown of the characteristics of each bargaining council used in my main analysis. The largest

\footnote{The outcome in this regression is the AKM firm wage premium, following \[2\], and the regression controls for a proxy of unionization.}
is the Metals and Engineering Industry Bargaining Council with nearly a million workers, but there are several smaller bargaining councils with only a few thousand workers. There is substantial variation across most characteristics, with minimum wages as low as R2,500 per month or as high as R10,000 per month.\footnote{The PPP exchange rate from South African ZAR to US Dollar was about 7 in 2020, and the nominal exchange rate was about R15. The equivalent PPP-adjusted minimum wage range for these bargaining councils is between about $350 and $1,500 per full time month, or between $2 and $8.50 per hour.} In general, bargaining councils with cross-sectionally higher value added per worker appear to have higher minimum wages, though profits are not strongly related.

In figure C.2 I follow \cite{54} in decomposing log wages into the components due to the baseline wage, the firm’s relevant minimum wage, the firm’s average wage, and the worker’s own wage. The first panel shows that the minimum wage component accounts for the majority of the firm average wage in bargaining council firms, ranging from the full average wage for the lowest value added firms to about half the firm wage for the highest value added firms. The second panel of figure C.2 displays the cross-sectional profile of workers as they age, showing a steep initial slope in the firm average wage and own wage components, and a much steadier incline due to the minimum wage component.

One concern for this paper in the South African context may be the relevance of wage competition when unemployment is so high. As one indicator, figure C.3 shows that the share of hires from non-employment declines with the wage paid, including for bargaining council firms. The high average unemployment rate may be a poor proxy for the availability of workers, given the skills requirements of firms.\footnote{A similar point was made by a bargaining council union official in informal discussions.} In addition, the classic job ladder model with search frictions only requires \textit{some} responsiveness of labor supply to the wage for monopsonistic wage competition to be relevant.
Overall, the matching of agreements into the tax data reveals a profile of bargaining councils that shows higher firm wages, with wide variation across value added, minimum wages, and other characteristics. Bargaining council minimum wages constitute a large part of firm average wages. The next section describes firms locally connected to these bargaining councils.

Table C.1: **Cross-sectional effects of bargaining council status on firm outcomes**

<table>
<thead>
<tr>
<th></th>
<th>AKM Firm FE</th>
<th></th>
<th>Within-firm wage inequality</th>
<th></th>
<th>Value Added pp</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>(1) (2) (3)</td>
<td>p90-p50 (4) p50-p10 (5)</td>
<td>(6)</td>
<td></td>
</tr>
<tr>
<td>Bargaining council</td>
<td>0.297**</td>
<td>0.315***</td>
<td>-0.153***</td>
<td>-0.111**</td>
<td>-0.010</td>
</tr>
<tr>
<td></td>
<td>(0.060)</td>
<td>(0.041)</td>
<td>(0.061)</td>
<td>(0.048)</td>
<td>(0.051)</td>
</tr>
<tr>
<td>Sect. Determination</td>
<td>-0.198***</td>
<td>0.098</td>
<td>-0.155***</td>
<td>0.067</td>
<td>-0.079</td>
</tr>
<tr>
<td></td>
<td>(0.057)</td>
<td>(0.060)</td>
<td>(0.069)</td>
<td>(0.042)</td>
<td>(0.053)</td>
</tr>
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<td>Obs</td>
<td>644789</td>
<td>639710</td>
<td>544864</td>
<td>1678578</td>
<td>1678550</td>
</tr>
<tr>
<td>Outcome</td>
<td>ffe</td>
<td>ffe</td>
<td>ffe</td>
<td>lnwagep90p50</td>
<td>lnwagep50p10</td>
</tr>
</tbody>
</table>

**Controls**

<table>
<thead>
<tr>
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<th>Y</th>
<th>Y</th>
<th>Y</th>
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</tr>
</thead>
<tbody>
<tr>
<td>Worker quality</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Indus + Loc FE</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
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<tr>
<td>Union</td>
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</tbody>
</table>

**Notes.** Bargaining council firms are identified by industry and district council stipulated in wage agreements. Sectoral Determination firms are identified from government regulatory notices. The omitted category is uncovered formal sector firms. The controls for industry refer to 1-digit SIC codes, and for location refer to district councils. AKM firm FE refer to the firm component from a regression of log annualized wages on two-way fixed effects for workers and firms. The control for union density firms is estimated for the same worker industry and district council in the Quarterly labor Force Surveys of 2010-2016. The control for worker quality is the average worker fixed effect from an AKM regression. Regressions are weighted by firm size. The sample is all formal sector firms from 2008 to 2018 using the SARS tax data.
Figure C.1: Distribution of workers by regulatory regime

(a) By industry

Notes. I divide firms into mutually exclusive “regulatory regimes” of bargaining councils (wages bargained over between worker unions and employer collectives), non-bargaining council unions (wages bargained by unions within each firm), sectoral determinations (wages set by government), and formal sector firms with no coverage. The sample is all formal sector firms from 2008 to 2018 using the SARS tax data.

(b) By firm average earnings decile

Notes. I divide firms into mutually exclusive “regulatory regimes” of bargaining councils (wages bargained over between worker unions and employer collectives), non-bargaining council unions (wages bargained by unions within each firm), sectoral determinations (wages set by government), and formal sector firms with no coverage. The sample is all formal sector firms from 2008 to 2018 using the SARS tax data.
Figure C.2: **Decomposition of wages into prescribed wages and firm wage premia**

(a) **Comparison of regimes**

(b) **Age profile**

Notes. The analysis follows the decompositions suggested in Card and Cardoso (2021). I decompose the wage into a baseline (percentile 1 of wages), the gap between the baseline and the bargaining council wage floor, the gap between the wage floor and the firm average wage (“firm cushion”), and the gap between the firm average wage and the worker’s own wage. The top figure compares the floors and firm cushions at firms in bargaining councils, sectoral determinations and that are not covered. The bottom figure shows the worker-level cross-section of each component by age. The sample is all formal sector firms from 2008 to 2018 using the SARS tax data.

Figure C.3: **Firm wages and share of hires from non-employment**

Notes. The figure shows hires from Non-Employment are measured as the observed proportion of workers who are hired at the firm while not observed at any firm in the previous year, as a proportion of all hires. Each bin represents a quantile of the firm wage distribution.
C.1.2 Descriptives of spillover firms

Worker flows as a measure of distance. Figure C.4 shows that although sharing the same industry or location are important predictors of flows between firms, sharing many other firm characteristics such as size, AKM wage premia, and the proportion of women are comparably important. 12 of these 15 predictors selected in a LASSO regression penalized for sparcity. Workers transition to similar firms among many dimensions. The figures are very similar considering firm separations or hires rather than gross worker flows (figure C.7), and the predictors shown here explain about half of all firm-to-firm flows.

A related question is the diversity of connected firms. Recall from section 3.2 the flow of workers to other firms depends on the connected set of any firm. As an illustration of this, I calculate for each firm the share of workers that go to other firms. If a firm is in the connected set and offers wages, but is rejected by all workers, then I will not observe this firm as part of the connected set. On average, the number of connected firms is about a tenth of the size of the firm. Figure C.12 plots the maximum share of these connected firms, as well as the concentration of those shares. In both cases, the plots show that connectivity increases tightly with firm size.

Figure C.4 provides a demonstration of the advantage of the flows measure of connectivity. In the spirit of [58], I compare the predictive power of different firm characteristics to classify spillover firms against the metric of worker flows. Using just location or location and industry [29] are poor predictors of worker flows: the probability that a firm sharing industry/location actually has high flows (precision)

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4Another advantage, important for this paper, is that this allows aggregation in the case of multiple treated firms. It is not obvious how to combine, for example, the spatial distance between one firm and a dozen treated firms that are spatially dispersed. On the other hand, the flows from each firm to the collection of such treated firms are readily observable.

5This exercise relies on worker flows being the conduit for spillovers, as modelled above. Insofar as other mechanisms are correlated with worker flows, these results are approximate; insofar as other mechanisms are orthogonal to worker flows, the results may be misleading.
is low, leading to attenuation bias; and the coverage of firms with high flows (recall) is low, making the estimates less representative of the affected sample. Using more predictors (shown in green) increases both precision and recall considerably. Using flows directly as I do implies a horizontal line at 100% precision for any level of recall. Similarly test and reject that flow patterns can be described by industry or location.

**Firms close to bargaining councils.** Figure C.13 shows the analogous decomposition by industry to Figure 3.2 in the main text. In fact, restricting figure C.4 above to bargaining council firms, industry turns out to be a much less important predictor for flows from other, non-bargaining council firms than plotted above (figure C.8). In figure C.10 I compare cruder definitions of spillover firms: for firms in the top decile of worker flows to bargaining council firms, only 10% share the same location and industry, and about a third share the same location.

To describe firms with high flows to bargaining councils more, figure C.6 shows predictors of worker flows, with manufacturing and male-dominated firms predicting higher flows. Table C.2 shows the characteristics of firms by the proportion of its flows to bargaining councils: firms with over 5% of flows to bargaining councils have close to the number of firms in the relevant bargaining councils (25,000 compared to 19,000), as well as the number of workers (4 million compared to 5 million). Characteristics such as wages, churn, AKM worker fixed effects (as a proxy for worker quality), and profit per worker are similar. On the other hand, bargaining councils have a very low proportion of women — but the proportion of women in connected spillover firms is larger, even though smaller than in unconnected firms. This highlights the importance of accounting for spillover effects when evaluating the aggregate causal effects of bargaining councils, in this case on gender wage inequality.

Where are the high spillover firms geographically located compared to the bargaining council firms? Figure C.11 shows the geographic location of Metals and En-
gineering Industry Bargaining Council firms as an example. Bargaining council firms are located in the urban centers (Gauteng region features most prominently), and this is replicated in the map of spillover firms. Even the region straddling the Northern Cape and North West, which is not a major urban center, also shows spillover firms in the same area – consistent with figure C.4 above which demonstrates that flows are higher for geographically closer firms.
Figure C.4: Predictors of firm-to-firm worker flows

Notes. For each firm, total worker flows to each other firm is averaged as a proportion of all flows. Panel A presents the coefficients from a single regression of firm-to-firm flows on the deviation in firm covariates. Fixed effects for each firm are included, and covariates are measured as the deviations from the primary firm average (absolute value of the difference in covariates), except for worker age and firm size which are included as the linear differences. Distance between firms is estimated using the centroid of postal codes. All values are normalized by their standard deviations. Panel B compares the precision and recall where “true” treatment is defined as high worker flows. Precision is defined as the share which actually have high flows out of those predicted as having spillovers; and recall is defined as the share predicted to have spillovers out of those with high flows. “All predictors” refers to all covariates given in Panel A, “Lasso” excludes three of these covariates, “Same industry & location” classifies a firm as treated if it is in the same 1-digit industry and location; “Same location” classifies only based on location; and “Distance” classifies based on low distance (this curve contains the “Same location” point).
Figure C.5: Distribution of worker flows from uncovered firms to Bargaining Councils

Notes. The figure illustrates the connectivity of non-covered to bargaining council firms. The left figure uses the full firm panel, allowing for flows to any bargaining councils. The right figure uses the main stacked event study regression sample, restricted to firms with at least 10 workers. The flow connectivity measure is normalized by the average for high flow firms, defined as the top 10% most connected firms, with proportions above one aggregated in the final bin.
Figure C.6: **Predictors of worker flow connectivity at uncovered firms**

*Notes.* The figure plots coefficients from a regression in the panel of firms of the *magnitude* of flows towards bargaining council firms, on firm characteristics. The industries listed are at the one-digit level. Abbreviations are as follows: Manuf. is manufacturing, Util and constr. is utilities and construction, trade & logis. is trade and logistics, FIRE is finance, insurance and real estate, and CSP is community, social and public administration.
### Table C.2: Characteristics of firms, by proportion of flows to bargaining council

<table>
<thead>
<tr>
<th>statistic</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
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<td>Flow to BC firms</td>
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<td>0.145</td>
<td>0.070</td>
<td>0.024</td>
<td>0.002</td>
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<tr>
<td>workers</td>
<td>5.3e+06</td>
<td>7.3e+05</td>
<td>3.3e+06</td>
<td>1.7e+07</td>
<td>6.1e+07</td>
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<td>number of firms</td>
<td>1.9e+04</td>
<td>6473.000</td>
<td>1.9e+04</td>
<td>9.8e+04</td>
<td>3.5e+05</td>
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<td>Firm size: 1–10</td>
<td>0.069</td>
<td>0.063</td>
<td>0.065</td>
<td>0.067</td>
<td>0.070</td>
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<td>Firm size: 10–30</td>
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<td>0.504</td>
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<td>0.317</td>
<td>0.303</td>
<td>0.300</td>
<td>0.295</td>
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<td>Firm size: 100+</td>
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<td>0.121</td>
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<td>0.166</td>
<td>0.157</td>
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<td>wage</td>
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<td>1.0e+05</td>
<td>1.1e+05</td>
<td>1.2e+05</td>
<td>1.2e+05</td>
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<tr>
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<td>wage p90p50</td>
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<td>3.124</td>
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<td>0.044</td>
<td>0.056</td>
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<tr>
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<td>0.083</td>
<td>0.077</td>
<td>0.083</td>
</tr>
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<td>Flow measure (S1)</td>
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<td>1.600</td>
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<td>0.018</td>
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<td>Flow measure (S2)</td>
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<td>0.786</td>
<td>0.266</td>
<td>0.018</td>
</tr>
<tr>
<td>Profit pp</td>
<td>2.2e+05</td>
<td>2.4e+05</td>
<td>2.6e+05</td>
<td>2.4e+05</td>
<td>2.5e+05</td>
</tr>
<tr>
<td>Value added pp</td>
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<td>4.1e+05</td>
<td>4.4e+05</td>
<td>4.3e+05</td>
<td>4.5e+05</td>
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<td>E-E separations</td>
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<td>0.101</td>
<td>0.118</td>
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<td>0.094</td>
<td>0.100</td>
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<td>0.115</td>
</tr>
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<td>0.399</td>
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<td>0.478</td>
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<td>Worker FE</td>
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<td>0.062</td>
<td>0.052</td>
<td>0.099</td>
<td>0.133</td>
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<td>Firm FE</td>
<td>−0.070</td>
<td>−0.207</td>
<td>−0.184</td>
<td>−0.222</td>
<td>−0.183</td>
</tr>
</tbody>
</table>

**Notes.** The sample is event year -1 of the main stacked event by firm by year balanced panel regression sample (restricted to firms above 10 workers as in the main specification). Treated indicates bargaining council firms, and high, medium, low and unconnected indicate decreasing degrees of connectedness to the relevant bargaining council. The “flow to BC firms” statistic refers to the main regressor, the proportion of worker flows towards bargaining council firms. The percentiles of wages refer to mean within-firm wage percentiles. Churn is the sum of separations and hires, over the firm size (subtracted out the change in firm size). Worker and firm FE are the respective components from an AKM regression of log wages on worker and firm fixed effects.
Figure C.7: Predictors of firm-to-firm hires and separations

Notes. For each firm, worker flows from each firm to each other firm is averaged as a proportion of all flows. The figure presents coefficients from a single regression of these worker flows on the deviation in firm covariates. Fixed effects for each firm are included, and covariates are measured as the deviations from the primary firm average (absolute value of the difference in covariates), except for worker age and firm size which are included as the linear differences. Distance between firms is estimated using the centroid of postal codes. All values are normalized by their standard deviations. The figures follow figure C.4, with the modifications that instead of the outcome of gross flows between firms, Panel A shows the outcome of firm to firm worker hire flows, and Panel B shows the outcome of firm to firm worker separation flows.
Figure C.8: Predictors of spillover firm connectivity

Notes. For each firm, total worker flows from each firm to each other firm is averaged as a proportion of all flows. The figure presents coefficients from a single regression of these worker flows on the deviation in firm covariates. Fixed effects for each firm are included, and covariates are measured as the deviations from the primary firm average (absolute value of the difference in covariates), except for worker age and firm size which are included as the linear differences. Distance between firms is estimated using the centroid of postal codes. All values are normalized by their standard deviations. The main modification to the figure [C.4] is that the sample of primary firms is restricted to bargaining council firms, and the sample of secondary firms (for which flows from the primary firms are measured) are restricted to uncovered firms not part of bargaining councils.
Figure C.9: Firm-to-firm flows and measures of distance

(a) Spatial distance
(b) Value added per worker
(c) Worker AKM FE
(d) Firm wage (median)
(e) Firm wage premium
(f) Proportion of women

Notes. For each firm, total worker flows from each firm to each other firm is averaged as a proportion of all flows. Fixed effects for each firm are included. Difference is measured as own firm characteristic minus other firm’s characteristic. Panel A shows worker flows by spatial distance between firms. Panel B shows worker flows by difference in value added per worker between firms. Panel C shows worker flows by difference in average AKM worker fixed effects between firms. Panel D shows worker flows by difference in firm median wage between firms. Panel E shows worker flows by difference in AKM firm wage premium between firms, and additionally includes fixed effects for the AKM worker premium. Panel F shows worker flows by difference in firm proportion of women between firms.
Figure C.10: **Predictors of spillover firm connectivity**

Notes. The figure compares the proportion of each decile of worker flows which are classified as treated under each of the following definitions. “High BC worker flows” defines treatment as high worker flows to bargaining councils, as used in the main paper. “High BC share” refers to a high share of workers who are covered by bargaining councils in the same industry and location. “Same industry & location” classifies a firm as treated if it is in the same 1-digit industry and location. “Same location” classifies only based on location.
Figure C.11: Location of MEIBC firms and spillovers

Notes. The maps show municipalities in South Africa (approximately 232 distinct areas). The average proportion of bargaining council (left) and spillover (right) is plotted, with darker shades corresponding to higher proportions. Firms are classified as high flow firms if they have more than 5% of flows to bargaining council firms, i.e. the high and medium flow categories in the table above. The sample is of the Metals and Engineering Industry Bargaining Council (MEIBC) firms and their connected spillovers firms.

Figure C.12: Worker flow connectivity and firm size

Notes. The figures illustrate how connectivity to other firms varies with firm size. Panel A defines connectivity as the average maximum share that another firm $k$ hires from a given firm $j$, shown by firm $j$’s size. Panel B defined connectivity as the concentration (as measured by the HHI) in the share firms that hire from firm $j$, shown by firm $j$’s size. The axes are in log scale.
Figure C.13: Distribution of bargaining council, spillover and other workers, by industry

Notes. Share refers to the percentage of workers in the same broad industry and location that belong to a bargaining council. Flow refers to the worker flows between non-covered firms and firms in bargaining councils. The figure shows the density of workers by 1-digit industry in each of the following classifications: “Bargaining council” firms subject to wage agreements; “High share & high flow” firms, i.e. high share of covered firms in the same industry-location as well as high flow of workers to bargaining councils; “High share & low flow” firms, i.e. high share of covered firms in the same industry-location, but low flow of workers to bargaining councils; “Low share & high flow” firms, i.e. low share of covered firms in the same industry-location, but high flow of workers to bargaining councils; and “Other firms”, which include all other formal sector firms not listed above. The industries listed are at the one-digit level. Abbreviations are as follows: Agri is agriculture, Util and constr. is utilities and construction, trans & logis. is transport and logistics, and FIRE is finance, insurance and real estate. The sample is all formal sector firms in South Africa from 2008 to 2018.
C.2 Appendix: Treatment effects on bargaining council firms

Figure C.14: Bargaining council wage increases and selected events, 2008-2018

Notes. Out of all annual bargaining council wage increases, events are selected based on (i) A minimum of a 3% real wage increase, (ii) At least 3 pre and 3 post periods (implying only 2011-2016 admitted), and (iii) No real wage increases greater than 3% in the pre-period. The final bar in the figure includes all increases greater than 15%.
Figure C.15: Effect of prescribed wage increases on wages of bargaining council firms

(a) Within firms: p25 and p50

(b) By quantile within firms and new hires

(c) By quantile across firm wages and firm size

Notes. The figure shows the main estimates from the event-study evaluating direct effects on covered firms from 47 bargaining council wage increases between 2011 and 2016 (see Equation 3.4). The regression is run at the unweighted firm-level, restricted to balanced firms with more than 10 workers in the pre-period, and excludes firms with more than 1% of worker flows to covered firms from the set of control firms. Standard errors are clustered at the level of bargaining council treatment by event. Panel A outcomes are the 25th and 50th percentiles of within firm wages. Panel B plots the coefficients at event-year 2 for separate regressions by quantiles of with firm wages and quantile of firm in the firm distribution of 25th percentile within-firm wages. Panel C shows the outcome of mean wages of new hires at each firm, as well as event year 2 coefficients for separate regressions by firm size category.
Figure C.16: Effect of prescribed wage increases on other outcomes in bargaining council firms

Notes. The figure shows the main estimates from the event-study evaluating direct effects on covered firms from 47 bargaining council wage increases between 2011 and 2016 (see Equation 3.4). The regression is run at the unweighted firm-level, restricted to balanced firms with more than 10 workers in the pre-period, and excludes firms with more than 1% of worker flows to covered firms from the set of control firms. Standard errors are clustered at the level of bargaining council treatment by event. All outcomes are logged. Panel A shows the log of the proportion of a firm’s workers that separate in each year. Panel B shows the log of the number of workers per firm. Panel C shows Unemployment Insurance (UI) co-payments are amounts paid towards a worker’s UI fund to be paid out in the case of retrenchment. Panel D the wage gap within firms between percentil 80 and 20 wages. Panel E shows log value added per worker, which is defined as firm sales minus capital and intermediate materials costs, all divided by firm size. Panel F shows log profit margin, which is defined as the firm’s total profit over the firm’s total value added.
C.3 Appendix: Spillover effects on non-covered firms
Figure C.17: Spillover effects on wages (using continuous flows regressor)

Notes. The figure shows the wage estimates from the event-study evaluating spillover effects on uncovered firms from 47 bargaining council wage increases between 2011 and 2016 (see Equation 3.5). The regression is run at the unweighted firm-level, restricted to balanced non-covered firms with more than 10 workers in the pre-period, and excludes firms in the same industry as the bargaining council. Standard errors are clustered at the level of 3-digit industry by location by event. I use a split sample approach to reduce measurement error in the regressor, where the average worker flows to bargaining councils of randomized firms within local labor markets is instrumented by the average flows at the complement set of firms. Panel A shows the 25th percentile of within-firm wage effects. Panel B shows the 50th percentile of within-firm wage effects. Panel C shows two-year out wage effects by category of firm size. Panel D shows two-year out wage effects by decile of wages across firms.
Figure C.18: **Spillover effects on other outcomes (using continuous flows regressor)**

(a) **Unemployment Insurance co-payments**  
(b) **Within-firm wage gap (p80-p20)**  
(c) **Firm separations**  
(d) **Firm E-N hires**

*Notes.* The figure shows estimates from the event-study evaluating spillover effects on uncovered firms from 47 bargaining council wage increases between 2011 and 2016 (see Equation 3.5). The regression is run at the unweighted firm-level, restricted to balanced non-covered firms with more than 10 workers in the pre-period, and excludes firms in the same industry as the bargaining council. Standard errors are clustered at the level of 3-digit industry by location by event. I use a split sample approach to reduce measurement error in the regressor, where the average worker flows to bargaining councils of randomized firms within local labor markets is instrumented by the average flows at the complement set of firms. All outcomes are logged. Panel A shows the Unemployment Insurance (UI) co-payments, which are amounts paid towards a worker’s UI fund to be paid out in the case of retrenchment. Panel B shows the P80-p20 wage gap, estimated within each firm. Panel C shows the log of the proportion of a firm’s workers that separate in each year. Panel D shows the log of the Employment-to-Non-Employment hires.
Figure C.19: **Spillover wage effects by connectivity to bargaining council**

(a) **Binscatter of changes in median wages**
(b) **DiD effects by quantiles of flows**

Notes. The figure shows the estimates from the event-study evaluating spillover effects on uncovered firms from 47 bargaining council wage increases between 2011 and 2016 (see Equation 3.5). The regression is run at the unweighted firm-level, restricted to balanced non-covered firms with more than 10 workers in the pre-period, and excludes firms in the same industry as the bargaining council. Standard errors are clustered at the level of 3-digit industry by location by event. I use a split sample approach to reduce measurement error in the regressor, where the average worker flows to bargaining councils of randomized firms within local labor markets is instrumented by the average flows at the complement set of firms. Panel A plots the change in log firm wages against the regressor (gross worker flows between each firm and bargaining council firms). The residual of a regression of split-sample measures of connectivity is included as a control, as an implementation of the control function approach. Panel B implements the main spillover specification on log firm wages, by quantile of the regressor representing unconnected to highly connected firms to bargaining councils.
C.4 Appendix: Additional findings

C.4.1 Robustness of bargaining council effects

The main specification given by equation 3.4 includes pre-period controls of wages and firm size in both levels and trends. Figure C.20 shows that these controls are not necessary for the wage effects, where using only firm and location by time fixed effects still results in flat pre-trends and a 4% increase in wages as in the main results above. However, using this sparser specification, firm size exhibits a pretrend which disappears with the additional controls. Figure C.20 also shows what happens when we fail to exclude potentially “contaminated” controls: the wage effect on is about 3% instead of 4%. This hints strongly at the spillover results presented in the following section.

Alternative specifications are presented in table C.3. Compared to the main estimates (column 1), there is a steep pre-trend when only considering firm fixed effects (column 2), but this disappears as soon as location fixed effects are introduced (column 3). The pre-trends and flat and with similar post-period coefficients of around 4% when clustering at a larger level (column 5, similar standard errors) or expanding the sample to unbalanced firms (column 6). Large firms exhibit large point estimates of over 6%.

Although these results show a sharp impact of the prescribed wages, the average change in prescribed wages can be above (due to weak enforcement) or below (due to higher wage firms also responding) the actual change in firm average wages. As a bounding exercise for this direct effect of prescribed wages, I consider a counterfactual simulation of perfect compliance where I set a worker’s wage equal to the relevant legal wage if below it, or leave the wage as is otherwise. Using the primary specification

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6 We can actually use these estimates to back out a rough indirect estimate of spillovers. Noting that there are about 20,000 bargaining council firms, and a further 25,000 high spillover firms, the decrease in wage effect from 4% to 3% implies a spillover wage effect of about 2.2%. This is very close to several of the direct estimates of wage spillovers in the next section.
above with this simulated outcome, the post-period effect on median within-firm wages is only slightly higher (3.5%) than when using observed wages (3%). While the level of wages may exhibit substantial non-compliance across these firms, this implies that the dynamic changes in wages follow the prescribed wages quite closely.

Figure C.20: **Effect of prescribed wage increases: Rejected specifications**

Notes. The figure shows estimates from the event-study evaluating direct effects on covered firms from 47 bargaining council wage increases between 2011 and 2016 (see Equation 3.4), except for the following adjustments. Panel A includes only firm and event-year fixed effects, i.e., it does not have fixed effects for firm size by year, and growth in pre-period wage and employment by year. Panel B includes high spillover firms in the control (left), with the main specification excluding the control shown for comparison (right).
Table C.3: Effects from bargaining council wage agreements, event year 2 coefficients from alternative specifications

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<thead>
<tr>
<th>Spec</th>
<th>Main FE</th>
<th>Only Firm FE</th>
<th>Firm &amp; place FE</th>
<th>Cont. w/ spill</th>
<th>Large cluster</th>
<th>Unbalanced firms</th>
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Notes. The table shows the estimates from the event-study evaluating direct effects on covered firms from 47 bargaining council wage increases between 2011 and 2016 (see Equation 3.4). The regression is run at the unweighted firm-level, restricted to balanced firms with more than 10 workers in the pre-period, and excludes firms with more than 1% of worker flows to covered firms from the set of control firms. Standard errors are clustered at the level of bargaining council treatment by event. Each cell in the table refers to the coefficient in event year 2 of a separate regression. The first column refers to the main estimates for the regression, the second includes controls only for firms, the third includes fixed effect controls for firms and location, the third includes the main set of controls but the sample is “contaminated” by including the likely spillover firms, and fifth clusters at the larger level of the event (rather than event by industry), the sixth includes unbalanced firms in the sample, and the seventh restricts the sample to large firms only (more than 50 workers in the pre-period).
C.4.2 Robustness of spillover effects

In tables C.4, C.5 and C.6 I provide alternative estimates for the final period of these spillover regressions. Figure 3.6 shows selected wage estimates only. The estimates are robust, especially the wage estimates, and are reinforced by placebo tests.

Tables C.4 adds various controls. Column 1, the main IV specification shown for reference, provides responses for the 25th and 50th percentiles of within-firm wages, as well as firm size and profit margin. Columns 2 and 3 consider the role of churn in a firm’s connectivity to the bargaining council. In the model framework presented above, a firm with higher churn may have a higher λ, i.e. proportion of workers with offers, which translates into a faster adjustment rate and would be correlated with the spillover outcomes. Column 2 shows outcomes when the regressor is multiplied by the churn of the firm in the pre-period, with similar magnitudes for the wage spillovers. This regressor is close to the connectivity measure suggested by [12]. Column 3 shows a specification where churn is controlled for, giving similar results to the main specification. The corresponding figures are presenting in figure C.23. Column 4 adds fixed effects for the spillover firm’s industry, to allay concerns about industry-level dynamics that may be correlated with the bargaining council. Here the identifying variation comes from firms within the same industry that are differentially connected to the bargaining council, for example due to location. The results are robust and similar to the main specification. Note that some shared industry-by-time effects may be “true” causal effects, but will be subtracted from this regression. Column 5 considers regression matching only based on the growth in wages and employment over periods -3 and -2 (rather than -3 and -1), to address concerns of over-fitting, with similar results.

In table C.5 I provide robustness on the specification by using different spillover regressors. Column 1 shows the estimates using OLS instead of IV, demonstrating
that there is substantial attenuation associated with the generated flow regressor, and corrected by the IV split-sample strategy. However, as expected relative to an IV regression, the OLS standard errors are somewhat smaller, and still provide strong evidence of cross-firm spillovers. Figure C.22 shows the corresponding event studies. In column 2, I estimate the spillover flows based on finer geographical units (postal codes rather than municipalities), with similar results to the main specification. In column 3, the regressor is the post-period worker flow and the instrument is the pre-period worker flow. This relieves the concern in the main specification that, as flows change in response to the bargaining council wage increase, pre-period flows do not dynamically represent workers’ outside options. In reality it makes little difference: the first stage between post- and pre-period flows is 0.77, and the estimates are extremely similar to the main specification, as are the pre-trends.

Column 4 uses a completely different regressor: the trade connections between firms, as proxied by Input-Output tables. I merge an externally produced matrix of trade flows into my dataset, and I assign values to spillover firms as represented by the industry of the bargaining council and the industry of the spillover firm. While the estimates are smaller, the wage effects actually follow a similar pattern to the main specification. The trade connection measure is also strongly correlated with the worker flows measure, with a coefficient of 0.6. Finally in column 5, I provide a placebo test which just shows that the results are not driven by the specification itself: I randomly re-assign the spillover flow regressor to firms. The coefficients are all tiny in magnitude compared to the main estimates, with relatively large standard errors.

Table C.6 shows further regressors of spillovers. Columns 1-3 show the binary treatment, where high spillover firms are compared to low spillover firms. Column 1 shows the IV specification, as in figure 3.4. Column 2 shows the OLS specification of this binary regressor, again demonstrating the substantial attenuation. Column 3
shows a crude definition of highly treated firms: as discussed earlier, previous papers estimating wage spillovers have estimate wage responses in nearby industries and locations, a measure which we expect to be attenuated given that some firms with high flows are missed out and some firms with low flows are classified as treated. Indeed, using a binary version of this, no effects are detected. However, considering industry-locations with high shares of bargaining council firms, some spillovers are suggested. Compared to the OLS binary specification, the point estimates are smaller and with larger standard errors, consistent with the attenuated measure and demonstrating directly the value of identifying the relevant measure of spillovers. Column 4 tests for spillovers within bargaining council firms, which may also react through a spillover mechanism. Using the main specification on the sample of bargaining council firms, the estimates have clean pre-trends and are actually somewhat larger than the main specification. Finally, columns 5 interacts the regressor with the HHI in firms flows, where one may expect the estimates to be mediated by the composition of the flow connectivity of spillover firms. While the magnitudes are less interpretable, the significance of the results suggest this is partly the case.
<table>
<thead>
<tr>
<th>Spec</th>
<th>Main</th>
<th>Churn reg</th>
<th>Churn FE</th>
<th>Indus FE</th>
<th>Growth FE</th>
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<tr>
<td>(1.759)</td>
<td>(2.080)</td>
<td>(2.620)</td>
<td>(2.389)</td>
<td>(1.757)</td>
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<td>4.593</td>
<td>8.340</td>
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<td>3.257</td>
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<tr>
<td>(1.158)</td>
<td>(1.377)</td>
<td>(1.788)</td>
<td>(1.575)</td>
<td>(1.154)</td>
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</tr>
<tr>
<td>Firm size</td>
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<td>-5.306</td>
<td>-3.263</td>
<td>-0.516</td>
<td>-0.931</td>
</tr>
<tr>
<td>(1.475)</td>
<td>(1.667)</td>
<td>(1.963)</td>
<td>(1.989)</td>
<td>(1.495)</td>
<td></td>
</tr>
<tr>
<td>(6.204)</td>
<td>(5.970)</td>
<td>(7.941)</td>
<td>(10.391)</td>
<td>(6.216)</td>
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</tbody>
</table>

Notes. The table shows the estimates from the event-study evaluating spillover effects on uncovered firms from 47 bargaining council wage increases between 2011 and 2016 (see Equation 3.5). The regression is run at the unweighted firm-level, restricted to balanced non-covered firms with more than 10 workers in the pre-period, and excludes firms in the same industry as the bargaining council. Standard errors are clustered at the level of 3-digit industry by location by event. I use a split sample approach to reduce measurement error in the regressor, where the average worker flows to bargaining councils of randomized firms within local labor markets is instrumented by the average flows at the complement set of firms. Each cell in the table refers to the coefficient in event year 2 of a separate regression. The first column (main) refers to the split-sample strategy for the regression, the second controls for the firm rate of churn, the third uses a regressor which is the product of the main regressor and churn of the firm in the pre-period (normalized), the fourth adds industry fixed effects, and the fifth estimates the controls for growth in size and wage over only periods -3 and -2 (not -3 and -1).
Table C.5: **Spillover effects from prescribed wage agreements, alternative regressors**

<table>
<thead>
<tr>
<th></th>
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<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
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</thead>
<tbody>
<tr>
<td>Wage (p25)</td>
<td>2.427</td>
<td>2.336</td>
<td>4.054</td>
<td>1.839</td>
<td>0.065</td>
</tr>
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<td></td>
<td>(0.710)</td>
<td>(3.025)</td>
<td>(1.114)</td>
<td>(1.104)</td>
<td>(0.181)</td>
</tr>
<tr>
<td>Wage (p50)</td>
<td>2.466</td>
<td>4.159</td>
<td>4.098</td>
<td>1.957</td>
<td>0.057</td>
</tr>
<tr>
<td></td>
<td>(0.506)</td>
<td>(2.045)</td>
<td>(0.781)</td>
<td>(0.802)</td>
<td>(0.125)</td>
</tr>
<tr>
<td>Firm size</td>
<td>–0.574</td>
<td>–1.795</td>
<td>–0.947</td>
<td>–1.082</td>
<td>–0.008</td>
</tr>
<tr>
<td></td>
<td>(0.581)</td>
<td>(2.518)</td>
<td>(0.896)</td>
<td>(0.901)</td>
<td>(0.148)</td>
</tr>
<tr>
<td>Profit</td>
<td>–6.100</td>
<td>–24.420</td>
<td>–10.196</td>
<td>4.099</td>
<td>0.093</td>
</tr>
<tr>
<td></td>
<td>(2.157)</td>
<td>(13.872)</td>
<td>(3.484)</td>
<td>(3.542)</td>
<td>(0.534)</td>
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<tr>
<td>Spec</td>
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<td>Post IV</td>
<td>I-O</td>
<td>Placebo</td>
</tr>
<tr>
<td>codes</td>
<td></td>
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<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

**Notes.** The table shows the estimates from the event-study evaluating spillover effects on uncovered firms from 47 bargaining council wage increases between 2011 and 2016 (see Equation 3.5). The regression is run at the unweighted firm-level, restricted to balanced non-covered firms with more than 10 workers in the pre-period, and excludes firms in the same industry as the bargaining council. Standard errors are clustered at the level of 3-digit industry by location by event. I use a split sample approach to reduce measurement error in the regressor, where the average worker flows to bargaining councils of randomized firms within local labor markets is instrumented by the average flows at the complement set of firms. Each cell in the table refers to the coefficient in event year 2 of a separate regression. The first column (main) refers to the split-sample strategy for the regression, the second (OLS) is OLS with the same specification, the third uses a regressor which indicates trade connections using input-output tables, the fourth calculates flows by smaller geographical units (postal costs), and the fifth is a placebo where the flow values are randomized across firms.
Table C.6: Spillover effects from prescribed wage agreements, alternative measures

<table>
<thead>
<tr>
<th>Spec</th>
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<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Wage (p25)</td>
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<td>1.960</td>
<td>1.631</td>
<td>11.144</td>
<td>42.015</td>
</tr>
<tr>
<td></td>
<td>(2.247)</td>
<td>(1.041)</td>
<td>(2.498)</td>
<td>(4.221)</td>
<td>(12.156)</td>
</tr>
<tr>
<td>Wage (p50)</td>
<td>4.956</td>
<td>2.127</td>
<td>1.392</td>
<td>5.227</td>
<td>42.958</td>
</tr>
<tr>
<td></td>
<td>(1.571)</td>
<td>(0.838)</td>
<td>(2.002)</td>
<td>(2.566)</td>
<td>(9.213)</td>
</tr>
<tr>
<td>Firm size</td>
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<td>–0.493</td>
<td>–2.150</td>
<td>–0.162</td>
<td>–17.037</td>
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<tr>
<td></td>
<td>(1.868)</td>
<td>(1.084)</td>
<td>(2.481)</td>
<td>(3.615)</td>
<td>(10.114)</td>
</tr>
<tr>
<td></td>
<td>(8.855)</td>
<td>(2.665)</td>
<td>(5.618)</td>
<td>(9.461)</td>
<td>(39.093)</td>
</tr>
</tbody>
</table>

Notes. The table shows the estimates from the event-study evaluating spillover effects on uncovered firms from 47 bargaining council wage increases between 2011 and 2016 (see Equation 3.5). The regression is run at the unweighted firm-level, restricted to balanced non-covered firms with more than 10 workers in the pre-period, and excludes firms in the same industry as the bargaining council. Standard errors are clustered at the level of 3-digit industry by location by event. I use a split sample approach to reduce measurement error in the regressor, where the average worker flows to bargaining councils of randomized firms within local labor markets is instrumented by the average flows at the complement set of firms. Each cell in the table refers to the coefficient in event year 2 of a separate regression. The first column (main) refers to the split-sample strategy for the regression, the second and third (HHI) consider the relevance of HHI in worker flows from the firm, as an interacted regressor and control respectfully, the fourth and fifth (Huber) consider the Huber (2022) method of finding spillover effects, first showing treatment and then spillover effects respectfully.
Figure C.21: Categorical spillover effects: High flow firms

(a) Firm wages: p25 and p50

(b) Firm size and profit margin

Notes. The table shows the estimates from the event-study evaluating spillover effects on uncovered firms from 47 bargaining council wage increases between 2011 and 2016 (see Equation 3.5). The regression is run at the unweighted firm-level, restricted to balanced non-covered firms with more than 10 workers in the pre-period, and excludes firms in the same industry as the bargaining council. Standard errors are clustered at the level of 3-digit industry by location by event. I use a split sample approach to reduce measurement error in the regressor, where the average worker flows to bargaining councils of randomized firms within local labor markets is instrumented by the average flows at the complement set of firms. All outcomes are logged. The regression compares high flow to unconnected non-bargaining council firms, where high flow is defined as firms with more than 10% of flows to the bargaining council. Flows are measured at the industry by location level, using a split-sample instrument for the binary indicator of high flows. Panel A presents the main estimates when controlling for the churn in the firm’s pre-period, respectively the effects on firm median wage and log firm size. Panel B presents the estimates where the main flows regressor is multiplied by the churn variable as a composite regressor, respectively the effects on firm median wage and log firm size.
Notes. The figure shows the estimates from the event-study evaluating spillover effects on uncovered firms from 47 bargaining council wage increases between 2011 and 2016 (see Equation 3.5). The regression is run at the unweighted firm-level, restricted to balanced non-covered firms with more than 10 workers in the pre-period, and excludes firms in the same industry as the bargaining council. Standard errors are clustered at the level of 3-digit industry by location by event. All outcomes are logged. The regression compares firms with high worker flows bargaining councils with firms unconnected by worker flows to non-bargaining council firms, where high flow is defined as firms with more than 10% of flows to the bargaining council. Panel A shows firm wages at the 25th and 50th percentiles. Panel B shows log firm size and log profit margin.
Figure C.23: Churn and flows in connectivity: Wages and firm size

(a) Controlling for firm churn rate

(b) Composite flow-churn regressor

Notes. The table shows the estimates from the event-study evaluating spillover effects on uncovered firms from 47 bargaining council wage increases between 2011 and 2016 (see Equation 3.5). The regression is run at the unweighted firm-level, restricted to balanced non-covered firms with more than 10 workers in the pre-period, and excludes firms in the same industry as the bargaining council. Standard errors are clustered at the level of 3-digit industry by location by event. I use a split sample approach to reduce measurement error in the regressor, where the average worker flows to bargaining councils of randomized firms within local labor markets is instrumented by the average flows at the complement set of firms. All outcomes are logged. Panel A presents the main estimates when controlling for the churn in the firm’s pre-period, respectively the effects on firm median wage and log firm size. Panel B presents the estimates where the main flows regressor is multiplied by the churn variable as a composite regressor, respectively the effects on firm median wage and log firm size.
C.4.3 Robustness relevant to both sets of analyses

No prior events. (Table C.7 column 2) One weakness of the bargaining council event study is that the number of years before the event is chosen arbitrarily, in my case as a balance between having enough years to judge a pre-trend while retaining enough events. Bargaining councils generally re-negotiate annually, with major rounds in 3-year intervals and wages often set across these 3 years. Here I exclude major events across event years -4 to -3, which is just outside the primary event study period but would adjust for the previous major bargaining round. For example, a concern is that lagged dynamic effects of the prescribed changes in the previous round would show up as pre-trends in my pre-period. While about half of the number of events are lost in this cut, the results for bargaining council and spillover firms are very similar. Figure C.24 shows the event studies, notably a flatter pre-trend for the wage spillovers.

National wage-setting. (Table C.7 column 3) I address the concern that bargaining council agreements may be endogenous to local economy trends by restricting to events from bargaining council agreements that are negotiated at the national level, thereby excluding more local-level bargaining. This check has the additional advantage of addressing measurement concerns, since location for multi-branch firms may not always be accurately recorded.\(^7\) Results are similar to the main specification.

Additional time periods. (Table C.7 column 4) In figure C.26 I show event studies with two extra time periods. This cuts down the sample size substantially under the balanced firm specification, and therefore is not used as the primary set of estimates. Wages in bargaining councils seem to be higher in a stable way, while it appears as if the wage shock to spillover firms erodes over time. Importantly, the firm

\(^7\)Firms are legally required to input the address of the branch of the worker. However, the data suggest that some firms are simply putting the address of headquarters instead of each branch. Data administrators at the tax data facility are continuing to investigate this issue.
size effects in this reduced sample appear to be less indicative of some employment loss, with a recovery in the bargaining council point estimates, and neutral effects for spillover firms.

**Mid-sized firms.** (Table C.7 column 5) The profiles of wage effects by size of firm suggested that mid-sized firms are the most affected by the prescribed wage agreements, whether bargaining council or spillover. Indeed, the wage effects are larger for both sets of firms, as are the effects on profit margins.

**Linear pre-trends.** (Table C.8 column 2) To address concerns about bias from pre-trends, I present the results from a specification which includes firm-specific linear pre-trends, constructed over the pre-period. This renders the pre-trend test meaningless (since there is no pre-trend by construction), but partials out any pre-trend if that was a concern in any of the previous event studies. The estimates are extremely similar as expected.

**Propensity score re-weighting.** (Table C.8 columns 3-4) The regression matching in my main specification allows potentially less comparable firms to act as controls, which has the advantage of including more control firms (where valid) and disadvantage of including less comparable control firms (where invalid). I use an alternative specification based on propensity score re-weighting instead of regression matching. I regress the bargaining council firm indicator on several pre-period characteristics to produce a propensity score, and analogously for high spillover firms. I then use the primary specification in (equation 3.4), without the fixed effects for pre-period levels and growth in wages and employment. In this regression, I am only controlling for firm fixed effects and location by time fixed effects, but I am additionally matching on pre-period characteristics. That is, I investigate wages in bargaining council firms compared to similar firms in the same location, by period. Figure C.25 shows the event-studies, with relatively flat pre-trends aside from the bargaining council firm size effects. The estimates are qualitatively similar but quantitatively larger for bar-
gaining council firms. The wage effect is smaller and not statistically significant for spillover firms, but is statistically significant using other measures such as annualized firm wages. I also run a “double robust” specification, that is, running the main specification (equation 3.4) with the full set of fixed effects as specified there, and with the additional weighting of firms by the propensity score weights. The advantage is that this specification is robust to concerns with both propensity score and regression match. Results are once again similar.

Event-specific controls. (Table C.8, column 5) In figure C.27, I show results from a variant on the primary specification (equations 3.4 and 3.5): I allow the period-specific continuous controls, pre-period firm size and firm wage, to vary by each event. The event-time estimates are nearly identical for the bargaining council firms. For spillover firms, the estimates indicate cleaner pre-period point estimates, i.e. more appropriate identification of the control. The estimates for wages are similar, are again for firm size suggestive of more neutral employment effects. There is still suggestive evidence of a negative effect on profits (which is consistent with the increase in wages).

Weighting by firm size. (Table C.8, column 6) I weight the main specification by pre-period firm size, for an indication of worker-level effects. The point estimate of the firm size effect is more negative, though the 95% confidence interval is large and includes up to a 0.07 increase in log size for both bargaining council and spillover firms. I show event study figures of these estimates, disaggregated by above and below median wage in the pre-period, in figure C.30.

“Honest pre-trends”. [147] point out that event-study plots with the heuristic test of pre-period significant coefficients may not preclude pre-trends that substantially alter the post-period estimated coefficients. They provide a helpful package.

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8Results available on request.
which I use to implement hypothetical pre-trends with 50% and 80% probability of being rejected against the assumption of parallel trends (as recommended), presented in figure C.28. The bargaining council firm wage estimates are both significant at 50% power, and event time 1 is significant at 80% power. The spillover wage estimates are significant at event time 2 for both 50% and 80% power. Figure C.29 suggests that the estimates are also robust to some non-linearity in the pre-trend. The point estimates for firm size, for both bargaining council and spillover firms, show that the non-significant decline in firm size is compatible with pre-trends with only 50% probability of being rejected (not shown).

**Individual level.** Figure C.31 shows that these wage effects are detected for individuals, both in terms of the first stage bargaining council effect and in terms of spillovers on high flow firms. I use a sparse specification, controlling only for worker fixed effects, and fixed effects in event-year demographics (tenure, age, sex) and location.

Across these checks, the wage effects for both bargaining council and spillover firms are extremely robust, suggesting a range of cross-wage elasticities that are nevertheless always high in magnitude and relative to the literature. On the other hand, the impacts on firm size are consistently insignificant, are sometimes even positive as a point estimate. The results on separations and profits are more consistently negative and significant, though with a few variations as exceptions.
Figure C.24: No prior large prescribed wage increase

Notes. The figure shows alternative estimates from the event-study evaluating direct treatment effects on bargaining council firms (see Equation 3.4) as well as spillover effects on uncovered firms (see Equation 3.5). The modification is that events with large minimum wage increases just before the pre-period window are excluded. This leaves about half the number of events. Panel A shows the effect on bargaining council firm wages, respective percentile 25 and 50 within firms. Panel B shows the effect on spillover wages and firm size, respectively.
Table C.7: Alternative samples for bargaining council and spillover effects

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<tr>
<th>Outcome</th>
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<tr>
<td>Ln Wage (p50)</td>
<td>0.030</td>
<td>0.034</td>
<td>0.046</td>
<td>0.041</td>
<td>0.036</td>
</tr>
<tr>
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<td>(0.010)</td>
<td>(0.014)</td>
<td>(0.010)</td>
<td>(0.011)</td>
<td>(0.010)</td>
</tr>
<tr>
<td>Ln firm size</td>
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<td>-0.004</td>
<td>-0.011</td>
<td>-0.004</td>
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<td>(0.010)</td>
<td>(0.009)</td>
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<tr>
<td>Ln separations</td>
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<tr>
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<td>(0.020)</td>
<td>(0.015)</td>
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<tr>
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<tr>
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<td>(0.020)</td>
<td>(0.028)</td>
<td>(0.034)</td>
<td>(0.030)</td>
</tr>
<tr>
<td>Ln Wage (p50)</td>
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<td>0.023</td>
<td>0.040</td>
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<td>(0.012)</td>
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<td>(0.064)</td>
<td>(0.062)</td>
<td>(0.313)</td>
<td>(0.095)</td>
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<td>CWE</td>
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<td>0.675</td>
<td>0.881</td>
<td>2.005</td>
<td>1.747</td>
</tr>
</tbody>
</table>

Notes. The figure shows alternative estimates from the event-study evaluating direct treatment effects on bargaining council firms (see Equation 3.4) as well as spillover effects on uncovered firms (see Equation 3.5). Column 1 (main) presents the main IV estimates, column 2 (nopreBC) excludes events with large wage increases in the period just before the event study, i.e. event year -4, column 3 (nation) restricts events to bargaining council events where wages are set nation-wide, column 4 (long4) includes the balanced set of firms with 4 post-period years, and column 5 (midsize) restricts to mid-sized firms with between 20 and 100 workers in the pre-period. The top panel presents results for bargaining council firms, and the bottom panel gives results for spillover firms. The cross-wage elasticity (CWE) divides the spillover wage coefficient by the bargaining council wage coefficient. The lnwagep50 outcome is the median within-firm wage. The CWE is missing where the wage effects are not significant.
Table C.8: **Alternative specifications for bargaining council and spillover effects**

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<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
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<tr>
<td>Ln Wage (p50)</td>
<td>0.030</td>
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<td>0.061</td>
<td>0.047</td>
<td>0.029</td>
<td>0.043</td>
</tr>
<tr>
<td></td>
<td>(0.010)</td>
<td>(. )</td>
<td>(0.011)</td>
<td>(0.013)</td>
<td>(0.010)</td>
<td>(0.020)</td>
</tr>
<tr>
<td>Ln firm size</td>
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<td>-0.007</td>
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<tr>
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<td>(0.009)</td>
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<td>(0.010)</td>
<td>(0.011)</td>
<td>(0.009)</td>
<td>(0.085)</td>
</tr>
<tr>
<td>Ln separations</td>
<td>-0.015</td>
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<td>-0.032</td>
<td>-0.016</td>
<td>-0.012</td>
<td>0.004</td>
</tr>
<tr>
<td></td>
<td>(0.013)</td>
<td>(. )</td>
<td>(0.014)</td>
<td>(0.013)</td>
<td>(0.012)</td>
<td>(0.028)</td>
</tr>
<tr>
<td>Ln profit margin</td>
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<td>.</td>
<td>-0.066</td>
<td>-0.047</td>
<td>-0.006</td>
<td>0.088</td>
</tr>
<tr>
<td></td>
<td>(0.022)</td>
<td>(. )</td>
<td>(0.027)</td>
<td>(0.025)</td>
<td>(0.023)</td>
<td>(0.094)</td>
</tr>
<tr>
<td>Ln Wage (p50)</td>
<td>0.039</td>
<td>0.024</td>
<td>0.014</td>
<td>0.028</td>
<td>0.041</td>
<td>0.078</td>
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<td>(0.012)</td>
<td>(0.005)</td>
<td>(0.023)</td>
<td>(0.022)</td>
<td>(0.012)</td>
<td>(0.053)</td>
</tr>
<tr>
<td>Ln firm size</td>
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<td>-0.006</td>
<td>-0.042</td>
<td>-0.051</td>
<td>-0.004</td>
<td>-0.111</td>
</tr>
<tr>
<td></td>
<td>(0.015)</td>
<td>(0.006)</td>
<td>(0.025)</td>
<td>(0.026)</td>
<td>(0.015)</td>
<td>(0.119)</td>
</tr>
<tr>
<td>Ln separations</td>
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<td>-0.004</td>
<td>0.025</td>
<td>0.025</td>
<td>-0.022</td>
<td>0.116</td>
</tr>
<tr>
<td></td>
<td>(0.022)</td>
<td>(0.009)</td>
<td>(0.055)</td>
<td>(0.055)</td>
<td>(0.028)</td>
<td>(0.084)</td>
</tr>
<tr>
<td>Ln profit margin</td>
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<td>-0.050</td>
<td>-0.216</td>
<td>-0.215</td>
<td>-0.098</td>
<td>-0.112</td>
</tr>
<tr>
<td></td>
<td>(0.062)</td>
<td>(0.022)</td>
<td>(0.136)</td>
<td>(0.137)</td>
<td>(0.069)</td>
<td>(0.167)</td>
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<td>CWE</td>
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<td>.</td>
<td>.</td>
<td>.</td>
<td>1.415</td>
<td>.</td>
</tr>
</tbody>
</table>

**Notes.** The figure shows alternative estimates from the event-study evaluating direct treatment effects on bargaining council firms (see Equation 3.4) as well as spillover effects on uncovered firms (see Equation 3.5). Column 1 (main) presents the main IV results, column 2 (pretrend) includes firm by time linear trend fixed effects based on the pre-period, column 3 (P-weight) presents the propensity score specification, column 4 (DR-weight) presents the double-robust propensity score specification (i.e. main sample fixed effects and weighted by propensity scores), column 5 (event cont.) interacts the continuous controls from the main specification by each event, and column 6 (size weight) weights the regression by firm size. The top panel presents results for bargaining council firms, and the bottom panel gives results for spillover firms. The cross-wage elasticity (CWE) divides the spillover wage coefficient by the bargaining council wage coefficient. The lnwagep50 outcome is the median within-firm wage. The CWE is missing where the wage effects are not significant.
Figure C.25: Propensity score weighting

(a) Bargaining council firm wages (p25 and p50)

(b) Spillover firm wages and size

Notes. The figure shows alternative estimates from the event-study evaluating direct treatment effects on bargaining council firms (see Equation 3.4) as well as spillover effects on uncovered firms (see Equation 3.5). The main modification to these specifications is that regressions are propensity score weighted, based on pre-period characteristics. There are only fixed effects for firm and event-by-location-by-time. Panel A shows the effect on bargaining council firm wages, respective percentile 25 and 50 within firms. Panel B shows the effect on spillover wages and firm size, respectively.
Figure C.26: **Longer event-time**

(a) **Bargaining council firm wages: p25 and p50**

(b) **Spillover wages: p25 and p50**

(c) **Firm size: Bargaining council and spillover firms**

*Notes.* The figure shows alternative estimates from the event-study evaluating direct treatment effects on bargaining council firms (see Equation [3.4]) as well as spillover effects on uncovered firms (see Equation [3.5]). The main modification to the specification is that the event study contains two extra event years. To ensure balance, this cuts down the sample size by over half compared to the primary sample. Panel A shows the effect on bargaining council firm wages, respective percentile 25 and 50 within firms. Panel B shows the effect on spillover wages and firm size, respectively. Panel C shows the effect of firm size, for bargaining council and spillover firms respectively.
Figure C.27: Spillovers: Event-specific continuous controls

Notes. The figure shows alternative estimates from of the spillover effects on uncovered firms (see Equation 3.5). The main modification is that the specification includes event-specific continuous controls for pre-period firm size and firm wages. Only spillover results are shown, since the results for bargaining councils are nearly identical to the main specification. Panel A shows the effect on uncovered firm wages, respective percentile 25 and 50 within firms. Panel B shows the effect on uncovered firm size and profit margin, respectively.
Figure C.28: “Honest pretrends” sensitivity checks

Notes. The figure shows sensitivity checks on the main estimates from the event-study evaluating direct treatment effects on bargaining council firms (see Equation 3.4) as well as spillover effects on uncovered firms (see Equation 3.5). The estimates correspond to the main specification for the bargaining council estimates and the OLS specification for the spillover estimates (the Roth package requires the covariance matrix, i.e. simultaneous estimation of the event-period effects rather than separate regressions as in the IV specification). The figures show a red line which represents a hypothetical pre-trend, and a the blue line which represents the hypothetical pre-trend conditional on passing the pre-trend test. These are constructed with 50% and 80% probability of being rejected, respectively. Panel A shows the sensitivity for bargaining council wage estimates. Panel B shows sensitivity for spillover wage estimates.
Figure C.29: “Honest pretrends” non-linearity checks

(a) Bargaining council firms  (b) Spillover firms

Notes. The figure shows sensitivity on the main wage estimates from the event-study evaluating direct treatment effects on bargaining council firms (see Equation 3.4) as well as spillover effects on uncovered firms (see Equation 3.5). The figures test for sensitivity to the assumption of linear pre-trends. M represents the degree of non-linearity allowed in the hypothetical pre-trend, where 0 indicates a linear pre-trend. The figures present the estimated wage effects on the y-axis, by increasing assumptions of non-linearity on the x-axis. Panel A shows the sensitivity for bargaining council wage estimates. Panel B shows the sensitivity for spillover wage estimates.
Figure C.30: Event studies, worker-weighted

Notes. The figure shows alternative estimates from the event-study evaluating direct treatment effects on bargaining council firms (see Equation 3.4) as well as spillover effects on uncovered firms (see Equation 3.5). The main modification to the specification is that regressions are weighted by pre-event firm size. All outcomes are logged. All firms are divided into above and below median of log wage in the pre-period of the event-study. Panel A shows the effect on bargaining council firm wages and size, respectively. Panel B shows the effect on spillover wages and firm size, respectively.
Figure C.31: **Individual level wage effects**

(a) **Bargaining council worker wages**

(b) **Spillover worker wages**

*Notes.* The figure shows alternative estimates from the event-study evaluating direct treatment effects on bargaining council firms (see Equation 3.4) as well as spillover effects on uncovered firms (see Equation 3.5). The specification has several modifications: It is run at the worker-level, with time-varying fixed effects for demographics, location, industry and event, as well as constant fixed effects for individuals. The regression sample is restricted to workers initially earnings in the lowest tercile of worker wages. Treatment for spillover workers is defined following the high-flow definition used for figure C.21. Panel A shows effects on bargaining council worker wages. Panel B shows effects on spillover worker wages.
C.4.4 Heterogeneity

I discuss heterogeneity in these main results, with the purpose of relaxing the constant treatment effects imposed in the event-study design. There are several model-consistent reasons for heterogeneous treatment effects, which I discuss below. Note however that the estimates on heterogeneity are not quasi-experimentally motivated, meaning that these patterns should be taken as suggestive; in addition, since the sample is reduced, power decreases, and since more tests are run, some statistical anomalies are more likely.

**Firm wage premia.** (Table C.9 columns 1-2) First, I run an AKM regression on all firms in rolling 3-year periods, which allows me to divide firms into high and low AKM firm fixed effects based on event pre-periods. Columns 1 and 2 refer to below and above median firm fixed effects respectively. While wage effects are statistically significant in both cases of bargaining council firms, the firm size and separations effects are significant and of opposite signs – consistent with the re-allocation effects I presented earlier (subsection 3.7.3), where lower wage firms decrease in firm size and higher wage firms expand. A similar contrast is observed for spillover firms, though not as stark. The magnitude of the wage spillovers are much larger for low firm effect firms, and the own-wage elasticities driven by the firm size effects are of opposite signs.

**Exporters.** (Table C.9 columns 3-4) Non-exporting firms are more likely to produce non-tradables, which may be subject to less product market competition, and thereby allow for wage-setting power associated with spillovers. I define exporters as firms with positive reported sales to foreign countries. The wage spillovers appear

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9This follows [2], where the firm wage premium is the firm component of a two way fixed effects regression of log wages on firm and worker fixed effects. See [21] for more details and an application in the context of these South African tax data.
stronger in non-exporting firms, even though the bargaining council wage effects are stronger in the exporting firms.

**Kaitz Index.** (Table C.9, columns 5-6) I consider heterogeneities by Kaitz index, that is, the minimum wage to local median wage ratio. One advantage of collective bargaining councils is that locally negotiated wages may be set in a way that is more optimal for efficiency in a *competitive* local labor market. I test this directly by restricting to national bargaining councils, where wages are set across regions, and testing for differential effects where minimum wages were set relatively higher or lower than the local labor market median. Columns 3 and 4 stratify within each event the industry-regions with low versus high Kaitz ratios respectively. For firms with a low minimum wage relative to the median local wage, effects are generally more muted: the bargaining council wages only increase by 3%, and spillover firm wages increase by 1.5%, with little change in firm size in either case (narrow confidence intervals). On the other hand, where the minimum wage is high relative to the median local wage, the wage effects are much larger (as expected), but so are the decreases in firm size. This heterogeneity suggests that there are binding constraints to large minimum wage interventions even in monopsonistic markets.

**Share of connected firms.** (Table C.10, columns 1-2) One measure of the characteristics of the connected set of the firm, following the model framework presented at the beginning, is to look at the highest share workers flowing from each firm to another firm on a consistent basis. A low share indicates a more equitable distribution of workers across the connected set. Such firms have slightly lower wage spillovers, compared to firms with a high share. Given that this maximum share likely indicates the relative share of workers going to bargaining council firms, this pattern is consistent with the idea that firms with more options can raise wages less and instead draw on other, non-bargaining council firms. I find similar results when considering het-
erogeneity by the total number of distinct firms connected to each firm, as measured by worker transitions.

**Labor supply elasticity.** (Table C.10, columns 3-4) As above, we may be interested in understanding how these dynamics vary with labor market power. Measuring monopsony power as the firm labor supply elasticity within each bargaining councils, I show bargaining council events with above and below median monopsony power (low and high labor supply elasticities respectively). The low monopsony power firms (high labor supply elasticity) show much stronger responses on wages and separations, while spillovers appear substantially stronger for high monopsony power events. This is perhaps surprising given that we may expect greater spillovers where wage competition is more intense. The finding is replicated for another indicator of monopsony power, the percentage of hires from employment or poaching (columns 7-8). Again here one expects more spillovers where there is a higher degree of poaching, yet to opposite is the case. These findings may well be confounded, and so remain speculative.

**Gender.** (Table C.10 columns 5-6) Finally, I compare effects on men and women. I restrict each of the outcomes, by firm, to men (column 7) and women (column 8) separately and run the primary specifications as for the main results. Wage effects in bargaining council firms appear stronger for women, with greater increases in firm size. The wage spillovers are similar across men and women. These differential results may be driven by a larger proportion of women being located in the lower wage bargaining council firms, as described in section 3.3.

I look at heterogeneity by union federation too, comparing bargaining councils where workers are represented by COSATU and SAFTU (see section 3.3 for context). Those represented by SAFTU have far stronger compliance (bargaining council wage increases in line with agreements) and spillovers. SAFTU represents the key private sector workers in manufacturing, which are strongholds of the union movement and therefore this is not entirely surprising. Of course, none of the heterogeneity in this
section should be over-interpreted, since many of these differences are within each other’s standard error bounds as figure C.32 illustrates for wages.

Figure C.32: Heterogeneity in estimates for wages

Notes. The figure shows alternative estimates from the event-study evaluating direct treatment effects on bargaining council firms (see Equation 3.4) as well as spillover effects on uncovered firms (see Equation 3.5). The legend identifies estimates from the following specifications: above-median characteristics for bargaining councils (dark green), above-median characteristics for spillover firms (dark blue), below-median characteristics for bargaining council firms (light green), and below-median characteristics for spillover firms (light blue). The characteristics are respectively as follows, shown on the y-axis: AKM firm fixed effects, an indicator for whether firms export, the Kaitz index (minimum to median wage ratio), the concentration of firm-to-firm flows within each firm, the labor supply elasticity of the local industry-location estimated based on worker separation responses to firm wages, the sex of the worker, and the percentage of Employment to Employment hires that the firm makes out of all hires.
Table C.9: Heterogeneity in bargaining council and spillover effects

<table>
<thead>
<tr>
<th>Outcome</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Low</td>
<td>High</td>
<td>Low</td>
<td>High</td>
<td>Low</td>
<td>High</td>
</tr>
<tr>
<td>Ln Wage (p50)</td>
<td>0.036</td>
<td>0.017</td>
<td>0.024</td>
<td>0.035</td>
<td>0.027</td>
<td>0.085</td>
</tr>
<tr>
<td></td>
<td>(0.014)</td>
<td>(0.008)</td>
<td>(0.010)</td>
<td>(0.014)</td>
<td>(0.011)</td>
<td>(0.024)</td>
</tr>
<tr>
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<td>0.008</td>
<td>-0.083</td>
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<td>(0.011)</td>
<td>(0.010)</td>
<td>(0.012)</td>
<td>(0.012)</td>
<td>(0.010)</td>
<td>(0.025)</td>
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<td>-0.034</td>
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<tr>
<td></td>
<td>(0.014)</td>
<td>(0.018)</td>
<td>(0.010)</td>
<td>(0.023)</td>
<td>(0.019)</td>
<td>(0.019)</td>
</tr>
<tr>
<td>Ln profit margin</td>
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<td>-0.000</td>
<td>-0.004</td>
<td>0.013</td>
<td>-0.070</td>
</tr>
<tr>
<td></td>
<td>(0.032)</td>
<td>(0.034)</td>
<td>(0.023)</td>
<td>(0.053)</td>
<td>(0.041)</td>
<td>(0.028)</td>
</tr>
<tr>
<td>Ln Wage (p50)</td>
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<td>0.123</td>
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<tr>
<td></td>
<td>(0.019)</td>
<td>(0.013)</td>
<td>(0.013)</td>
<td>(0.032)</td>
<td>(0.016)</td>
<td>(0.027)</td>
</tr>
<tr>
<td>Ln firm size</td>
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<td>0.039</td>
<td>-0.023</td>
<td>-0.025</td>
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<td>(0.017)</td>
<td>(0.017)</td>
<td>(0.037)</td>
<td>(0.020)</td>
<td>(0.036)</td>
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<tr>
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<td></td>
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<td>(0.029)</td>
<td>(0.028)</td>
<td>(0.041)</td>
<td>(0.022)</td>
<td>(0.116)</td>
</tr>
<tr>
<td>Ln profit margin</td>
<td>-0.136</td>
<td>-0.131</td>
<td>-0.183</td>
<td>-0.112</td>
<td>-0.101</td>
<td>0.004</td>
</tr>
<tr>
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<td>(0.105)</td>
<td>(0.075)</td>
<td>(0.074)</td>
<td>(0.124)</td>
<td>(0.054)</td>
<td>(0.257)</td>
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<td>1.865</td>
<td>.</td>
<td>0.984</td>
<td>1.456</td>
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</table>

Notes. The figure shows alternative estimates from the event-study evaluating direct treatment effects on bargaining council firms (see Equation 3.4) as well as spillover effects on uncovered firms (see Equation 3.5). The divisions are made below and above the median of each measure. Columns 1 and 2 (ffeq) divides the sample below and above the median AKM firm fixed effect; columns 3 and 4 (exporter) divide firms into non-exporters and exporters; columns 5 and 6 (kaitz, i.e. the minimum wage to local median wage ratio) are restricted to events with nationally set wages, and divide within each event firm in areas where the Kaitz ratio is low (kaitz0, i.e. relatively high median wages) and where the Kaitz ratio is high (kaitz1, i.e. relatively low median wages). The top panel presents results for bargaining council firms, and the bottom panel gives results for spillover firms. The cross-wage elasticity (CWE) divides the spillover wage coefficient by the bargaining council wage coefficient. The CWE is missing where the wage effects are not significant.
Table C.10: **Heterogeneity in bargaining council and spillover effects (continued)**

<table>
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<tr>
<th>Outcome</th>
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<th>Women</th>
<th>E-E hires</th>
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<tr>
<td>Ln Wage (p50)</td>
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<td>0.026</td>
<td>0.018</td>
<td>0.040</td>
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<tr>
<td></td>
<td>(0.011)(0.012)</td>
<td>(0.013)(0.013)</td>
<td>(0.012)(0.014)</td>
<td>(0.013)(0.013)</td>
</tr>
<tr>
<td>Ln firm size</td>
<td>–</td>
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<td>–</td>
<td>0.013</td>
</tr>
<tr>
<td></td>
<td>(0.011)(0.017)</td>
<td>(0.012)(0.013)</td>
<td>(0.015)(0.022)</td>
<td>(0.017)(0.013)</td>
</tr>
<tr>
<td>Ln separations</td>
<td>–</td>
<td>0.004</td>
<td>–</td>
<td>0.003</td>
</tr>
<tr>
<td></td>
<td>(0.015)(0.015)</td>
<td>(0.013)(0.018)</td>
<td>(0.012)(0.013)</td>
<td>(0.011)(0.019)</td>
</tr>
<tr>
<td>Ln profit margin</td>
<td>–</td>
<td>0.028</td>
<td>–</td>
<td>0.027</td>
</tr>
<tr>
<td></td>
<td>(0.027)(0.035)</td>
<td>(0.043)(0.024)</td>
<td>(.)</td>
<td>(.)</td>
</tr>
<tr>
<td>Ln Wage (p50)</td>
<td>0.033</td>
<td>0.045</td>
<td>0.110</td>
<td>0.023</td>
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<tr>
<td></td>
<td>(0.017)(0.015)</td>
<td>(0.034)(0.012)</td>
<td>(0.013)(0.013)</td>
<td>(0.042)(0.012)</td>
</tr>
<tr>
<td>Ln firm size</td>
<td>–</td>
<td>0.011</td>
<td>–</td>
<td>0.025</td>
</tr>
<tr>
<td></td>
<td>(0.022)(0.019)</td>
<td>(0.040)(0.016)</td>
<td>(0.016)(0.015)</td>
<td>(0.050)(0.015)</td>
</tr>
<tr>
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<td>0.039</td>
<td>0.019</td>
</tr>
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<td></td>
<td>(0.027)(0.036)</td>
<td>(0.040)(0.027)</td>
<td>(0.067)(0.047)</td>
<td>(0.047)(0.025)</td>
</tr>
<tr>
<td>Ln profit margin</td>
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<td>0.271</td>
<td>0.194</td>
</tr>
<tr>
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<td>(0.087)(0.085)</td>
<td>(0.189)(0.064)</td>
<td>(.)</td>
<td>(.)</td>
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<tr>
<td>CWE</td>
<td>0.929</td>
<td>1.753</td>
<td>5.983</td>
<td>0.590</td>
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</table>

**Notes.** The figure shows alternative estimates from the event-study evaluating direct treatment effects on bargaining council firms (see Equation 3.4) as well as spillover effects on uncovered firms (see Equation 3.5). The divisions are made below and above the median of each measure. Columns 1 and 2 (maxshare) divide by the share of the most connected firm; columns 3 and 4 (lse) divide events into bargaining councils with low firm labor supply elasticities (more monopsonistic market) and bargaining councils with high firm labor supply elasticities (more competitive market); columns 5 and 6 (fem) restrict the firm-level outcomes to men and women workers at each firm respectively. Profit margin outcome is missing for the sex regression because this is a firm-level outcome and cannot be disaggregated by sex. The top panel presents results for bargaining council firms, and the bottom panel gives results for spillover firms. The cross-wage elasticity (CWE) divides the spillover wage coefficient by the bargaining council wage coefficient. The CWE is missing where the wage effects are not significant.
C.4.5 Re-allocation

Differential effects by relative firm rents are also shown in figure C.33 (event studies show in figure C.34). I show the coefficients of the main bargaining council firm specification 3.4, with additional interacted indicators for tercile of the bargaining council wage distribution. There are clear wage effects for the middle and bottom terciles. The bottom tercile shows a statistically significant decrease in firm size; in contrast, the middle tercile shows a negligible change in firm size, and the top tercile a positive (marginally significant) change. Adjusting for the different wage increases by dividing these firm size changes by the wage coefficient, the own-wage elasticity for the bottom tercile is large and negative at -0.67 compared to the middle tercile which is positive at 0.14.

There is evidence that the large low-wage firms in this bottom tercile differentially increase their use of labor brokers (i.e. outsource or subcontract workers), meaning to some extent these decreases in employment may reflect a shift rather than decline in production. A similar pattern emerges for spillover firms, with the largest effects in the lowest wage firms, and an own-wage-elasticity which is large and negative for these firms but smaller for higher wage firms. I also show there is some differential adjustment by value added of the firm, based on worker quality (figure ??).

Finally, some of these patterns are mirrored in the heterogeneity estimates below by firm wage premia (Table C.9 columns 1-2). First, I run an AKM regression on all firms in rolling 3-year periods, which allows me to divide firms into high and low AKM firm fixed effects based on event pre-periods\(^\text{10}\). Columns 1 and 2 refer to below and above median firm fixed effects respectively. While wage effects are statistically significant in both cases of bargaining council firms, the firm size and separations

\(^{10}\)This follows [2], where the firm wage premium is the firm component of a two way fixed effects regression of log wages on firm and worker fixed effects. See [21] for more details and an application in the context of these South African tax data.
effects are significant and of opposite signs, where lower wage firms decrease in firm size and higher wage firms expand. A similar contrast is observed for spillover firms, though not as stark. The magnitude of the wage spillovers are much larger for low firm effect firms, and the own-wage elasticities driven by the firm size effects are of opposite signs.
Figure C.33: Summary of re-allocation effects, by pre-event wage

Notes. The figure shows estimates from the event-study evaluating direct treatment effects on bargaining council firms (see Equation 3.4) as well as spillover effects on uncovered firms (see Equation 3.5). In each specification, the treatment coefficients are interacted by tercile of firm pre-event wage. Coefficients plotted are for the average post period effect, and the own-wage elasticity (OWE) defined as the firm size coefficient divided by the wage coefficient is displayed above. Where omitted, the wage coefficient is not significant. Panel A shows the effects for bargaining council firms. Panel B shows the effects for spillover firms.
Figure C.34: Re-allocation effects, by pre-event firm wage

Notes. The figure shows estimates from the event-study evaluating direct treatment effects on bargaining council firms (see Equation 3.4) as well as spillover effects on uncovered firms (see Equation 3.5). In each specification, the treatment coefficients are interacted by tercile of firm pre-event wage. Coefficients plotted are for the each event-period effect. Panel A shows the effects for bargaining council firms on wages and firm size respectively. Panel B shows the effects for spillover firms, on wages and firm size respectively.
Notes. The figure shows estimates from the event-study evaluating direct treatment effects on bargaining council firms (see Equation 3.4) as well as spillover effects on uncovered firms (see Equation 3.5). In each specification, the treatment coefficients are interacted by tercile of firm pre-event value added per worker. The outcome is the average firm AKM worker fixed effects. Coefficients plotted are for the each event-period effect. Panel A shows the effects for bargaining council firms. Panel B shows the effects for spillover firms.
Figure C.36: **Aggregate employment effects of bargaining council wages increases**

(a) **Informality rate**  
(b) **Unemployment rate**

*Notes.* Informality and unemployment are merged in from the Quarterly Labour Force Surveys 2008-2018 to the main event study dataset. Samples are collapsed to the level of the municipality, and the main regression specification for direct effects is used (see Equation 3.4), with controls at the municipality level and treatment equal to the share of bargaining council firms in the municipality.
C.5 Appendix: Aggregate labor market effects

I simulate the effects on the distribution using the actual characteristics of firms in the tax data. I add on a bargaining council premium to bargaining council firms, estimate the flows from each non-covered firm to bargaining council firms, and then use the causally estimated cross wage elasticity from section 3.6 to add on an associated spillover effect. This micro-simulation exercise does not take into account other effects, for example on assignment of workers to firms. What is a meaningful bargaining council wage premium? One way to estimate it is to use the actual wage agreements over the period 2008-2018. In total, the agreements increased real wages by 15% over the 11 year span. Average non-covered firm growth is close to zero over this period, and recall that the pass-through of prescribed wages to actual wages is close to 1. It turns out the OLS estimate of the bargaining council premium from a regression of AKM firm fixed effects on a bargaining council indicator is very close to 15% (table C.1).

The effect on employment is omitted from figure 3.9 in the main text. The earlier estimates on employment suggested a negative point estimate on employment, with estimated own wage elasticities of \(-0.1\) for bargaining council firms and \(-0.2\) for spillover firms. However, since the standard errors for both estimates included 0, I have not adjusted for this in the main figure. In the appendix figure C.38 I do consider such employment effects using the point estimates only, and the resulting wage effects (including zeroes for those disemployed) are attenuated to about 4% for the direct effects and 10% inclusive of the indirect effects.

\footnote{Flows from non-covered firms to bargaining council firms are higher in the full panel because of the way the clean event-study for the regression estimates was constructed. Specifically, (a) flows to any bargaining council firm are considered, not just one relevant bargaining council, and (b) control firms were intentionally included that had negligible flows to bargaining councils, to allow for cleaner comparisons, and which resulted in lower average flows from all firms to bargaining councils.}
Another aspect that I have not emphasized is that there may well be large within-bargaining council spillovers; that is, higher wage bargaining council firms may increase wages despite not being bound to do so, in order to maintain their position as a high wage firm relative to bound bargaining council firms. And indeed, nearly a third of flows from bargaining council firms are to other firms in the same bargaining council, highlighting the potential for similar mechanisms discussed in this paper. This is also the sense in which [54] consider spillovers. Although my data are much more constrained in that I do not observe exactly which jobs are bound, I perform an approximate simulation in figure [C.37]. I add a line to figure 3.2 showing the prescribed wage increase due to workers who are at most double the minimum wage; even though this is conservative (most occupation-specific wages thresholds are less than this), the simulation shows considerable wage gains for bargaining council workers paid much higher than those legally bound for a wage increase.
Figure C.37: Simulated effect on firm wage distribution, with compliance

Notes. The figure simulated the effect of bargaining councils by quantile of the AKM worker fixed effect. The baseline is observed wages. The blue shows counterfactual wages which add in the relevant bargained wage increases between 2008-2018 for workers between the minimum wage and double that value; the red line shows such increases across all workers. The green bar further add in the spillovers implied by the flows to bargaining councils and the cross-wage elasticity above. The blue, red and green lines have average of 4%, 5% and 12% respectively.
Figure C.38: Simulated effect on firm wage distribution, with employment effects

Notes. The figure simulated the effect of bargaining councils by quantile of the AKM worker fixed effect. The baseline is observed wages. The blue line excludes cross-firm spillovers effects, and the red line includes these. Compared to figure 3.9, employment effects are considered by subtracting out the point estimates of the disemployment effects. The own wage elasticities for bargaining council firms of $-0.1$ and for spillover firms of $-0.2$ are used, as discussed in main text.
Figure C.39: Simulated effect on firm wage distribution, counterfactual of more equal flows

Notes. The figure simulated the effect of bargaining councils by quantile of the AKM worker fixed effect. The baseline is observed wages. The blue line excludes cross-firm spillovers effects, and the red line includes these. Compared to figure 3.9, the green line is added which shows a counterfactual as follows: Firms have worker flows to bargaining councils at least equal to the share of bargaining council workers in their own industry-location.
C.6 Derivations of theoretical framework

C.6.1 Logit static model

C.6.1.1 Main setup

The logit model is widely used to model monopsony [55, 79]. Utility of a worker may be expressed as

$$V(w_j) = \beta \ln(w_j) + \nu_{ij},$$

where $\beta$ parameterized the latent monopsony power (i.e. the responsiveness of worker utility to wages), and $\nu_{ij}$ follows a Gumbel distribution indicating idiosyncratic preferences for the firm. The source of monopsony power is a firm’s knowledge of the distribution of $\nu_{ij}$, as cuts to the wage have little impact on the utility of workers with high idiosyncratic preferences.

The distribution yields the following probability $p_j$ a worker is employed at firm $j$, out of $J$ total firms:

$$p_j = \frac{w_j^\beta}{\sum_{l=1}^{J} w_l^\beta}$$  \hspace{1cm} (C.1)

This setup is standard in the literature, e.g. [55], and I make three modifications. Firstly, while an assumption of atomistic competition is usually made, enabling us to treat the term $\ln(\sum_l w_l^\beta)$ as a constant, I retain this term as it is essential for the strategic interaction that generates spillovers. Secondly, to justify non-atomistic competition, I explicitly incorporate the consideration set of outside options by restricting to the relevant labor market, denoted by $S$ as in the term $\ln(\sum_{l \in S} w_l^\beta)$. Thirdly, while I assume employment varies positively with wages at the treated firm or set of firms $k$, I allow the combined treated employment in the consideration set to be a free

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12 Another way to present this is through an adjacency matrix $S$, where $S_{jl} = 1$ if the firm $l$ is part of $j$'s consideration set, and $S_{jl} = 0$ otherwise. Then the term $\ln(\sum_l w_l^\beta)$ becomes $\ln(\sum_{l \in S} (w_l^\beta S_{jl}))$. The adjacency coefficients $S_{jl}$ could also represent the degree of connectedness, i.e. a continuous measure as discussed in the dynamic context below. For simplicity of exposition, I just index over $S$ here.
parameter \( n_k \). For example, the treated firms need not remain supply-constrained. The modified probability of employment at firm \( j \) is
\[
\ln p_j = \beta \ln(w_j) - \ln(n_k + \sum_l w_l^\beta) .
\]

Taking logs, we can compute the firm labor supply elasticities:
\[
\ln p_j = \beta \ln(w_j) - \ln(n_k + \sum_l w_l^\beta)
\]
\[
\varepsilon_{nj}^n = \frac{\partial \ln p_j}{\partial \ln w_j} = \beta (1 - \frac{w_j^\beta}{n_k + \sum_{l \neq k} w_l^\beta}) = \beta (1 - p_j)
\] (C.2)

Since I assume employment varies positively with wages for treated firms \( k \), then \( \varepsilon_{jk}^n = \frac{\partial \ln p_j}{\partial \ln w_k} < 0 \). In the notation above, \( \varepsilon_{jk}^n = -\frac{\partial n_k/\partial \ln w_k}{n_k + \sum_{l \neq k} w_l^\beta} \), and \( \partial n_k/\partial \ln w_k > 0 \). In the case of \( k \) logit supply-constrained as for other firms, then \( \varepsilon_{jk}^n = -\beta (\frac{w_k^\beta}{\sum_j w_j^\beta}) = -\beta p_k \). An increase in the wage of a connected firm \( k \) (holding \( w_j \) constant) decreases the employment in firm \( j \).

The expressions for \( \varepsilon_{jj}^n \) and \( \varepsilon_{jk}^n \) match those in standard derivations of the logit, such as in [163]. It demonstrates that a firm \( k \) which raises its own wage will see an increase in own employment. Another firm \( j \), faced with this wage increase from firm \( k \), can trade-off raising its own wages or losing its workers. For the supply-constrained case, given a change in firm \( k \)’s wage \( \Delta \ln w_k \), firm \( j \)’s share will change by \( \Delta \ln p_j = \varepsilon_{jk} \Delta \ln w_k = -\beta p_k \Delta \ln w_k \) while firm \( j \)’s wage \( w_j \) remains the same.

Alternatively, firm \( j \) may seek to maintain its previous employment share, through raising its wage by an equivalent amount, \( \Delta \ln w_j = \Delta \ln w_k \).}

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13 This is not equal to the total employment in covered firms, since \( n_k \) is still subject to the aggregate labor supply constraint \( N \). For example, if \( n_j = Aw_j^\beta \) for any firm \( j \), \( p_j = n_j/\sum_l n_l \), and \( A \) doubled: then \( n_j \) would double, but \( p_j \) would remain the same. For aggregate labor supply constraint \( N \), total employment \( p_j N \) would remain the same.

14 See this from \( \Delta \ln p_j = \beta \ln(w_j \cdot (1 + \Delta w_j)) - \ln(\sum_k (w_k \cdot (1 + \Delta w_j))^\beta) - \ln p_j = \beta \ln(w_j) + \beta \ln((1 + \Delta w_j) - \ln(\sum_k w_k^\beta) - \beta \ln((1 + \Delta w_j) - \ln p_j = 0. \)
The wage response chosen by \( j \) is determined by the firm’s wage setting function. Abstracting from product market competition and normalizing the price to 1, the profit optimization equation and optimal wage are:

\[
\pi_j = \max_{w_j} \frac{1}{1 - \eta} A_j(p_j(w_j)N)^{1-\eta} - w_j \cdot p_j(w_j)N
\]

\[
\ln w_j = \ln\left(\frac{\varepsilon^n_j}{1 + \varepsilon^n_j}\right) + \ln A_j - \eta \ln(p_jN)
\] (C.3)

Where \( \eta \) parametrizes the downwards-sloping firm demand, \( N \) is the total pool of labor, \( p_j(w_j) \) is the firm share constrained by wages as above, and \( \varepsilon_{jj} = \beta(1 - p_j) \) is given above. The final term includes \( p_j \) which is a function of all wages of firms in the local labor market.\(^{15}\) This wage-setting equation nests the constant returns production function (\( \eta = 0 \)) yields the standard markdown formula \( W_j = A_j \frac{\varepsilon_{ij}}{1 + \varepsilon_{ij}} \).

We can derive the wage cross wage elasticity, denoted as \( \varepsilon^w_{jk} \), by using equation [C.3]

\[
\varepsilon^w_{jk} = \frac{d\ln(w_j)}{d\ln(w_k)} = \frac{-\varepsilon^n_{jk}\beta p_j}{\varepsilon_{jj}(1 + \varepsilon_{jj})} - \varepsilon^n_{jk}\eta - \eta\beta\varepsilon^w_{jk} + \eta\beta \sum_{l\neq k} p_l\varepsilon^w_{lk}
\]

\[
\varepsilon^w_{jk} = \frac{d\ln(w_j)}{d\ln(w_k)} = \frac{1 + \eta\beta}{1 + \eta\beta p_k} \left( \frac{-\varepsilon^n_{jk}\eta}{1 + \eta\beta} \right) + \frac{-\varepsilon^n_{jk}\beta p_j}{\varepsilon_{jj}(1 + \varepsilon_{jj})(1 + \eta\beta)} > 0
\] (C.4)

Where I substitute in \( \varepsilon^n_{jk} \) from its full expression. Since \( p_j \) is a function of \( w_j \) and \( w_k \), this has second order effects as each is a function of the other. The final expression for \( \varepsilon^w_{jk} \) makes the simplifying assumption that \( \varepsilon^w_{jk} = \varepsilon^w_{lk} \), i.e. the cross-wage elasticity is similar for non-treated firms. This assumption is purely made for expositional clarity, and is equivalent to assuming that the changes due to the effects

\(^{15}\)Equivalently, \( \ln w_j = \frac{1}{1 + \beta}(\ln(\frac{\varepsilon_{jj}}{1 + \varepsilon_{jj}}) + \ln A_j - \eta \ln(N) + \eta \ln(\sum w^\beta_l)) \), where the \( w_l \) are still a function of other wages in the local labor market.
of \( k \)'s wage increase on the own elasticities are equal. In practice, the impact on \( \varepsilon_{jk}^n \) of the change due to the effect on \( \varepsilon_{jj}^n \) is an order of magnitude smaller than the effect due to the shift in the Residual LS. Secondly, for brevity in expression 3.2 I have omitted the second order effects on \( \varepsilon_{jj}^n \) contained in expression 3.1. Again, this makes little difference. The full expression with second order effects, instead of equation 3.2 is \( \varepsilon_{jk}^n = \frac{x}{1+x} \), with \( x = \left( \frac{-\varepsilon_{jk}^n \beta_{pj} \varepsilon_{jj}^n}{\varepsilon_{jj}^n(1+\varepsilon_{jj})} - \varepsilon_{jk}^n \eta \right) \). The only difference is the addition of \( \frac{-\varepsilon_{jk}^n \beta_{pj}}{\varepsilon_{jj}^n(1+\varepsilon_{jj})} \) in the denominator, which is negligible in magnitude. In simulations across a range of value inputs, the difference between the expressions can only be seen at the fourth decimal.\(^{16}\)

Diagramatic representation.\(^{16}\) The intuition for these positive cross wage elasticities is illustrated by Figure 3.1 in the main text. The setup above can be represented by a downwards sloping Marginal Revenue Product of Labor (MRPL) curve, \( \ln(mrpl) = \ln A_j - \eta \ln(p_jn) \), and an upwards sloping Residual Labor Supply (LS) to the firm, \( \ln w_{jLS} = \frac{1}{\beta}(\ln(p_jn) - \ln N + \ln(n_k + \sum_l S_l w_{il} \beta)) \), with corresponding Marginal Cost of Labor equal to the LS curve plus \( \ln(1+\varepsilon) \). The optimal employment is set at \( \ln(mrpl) = \ln(mcl) \), with corresponding wage on the supply curve. This solves as in C.3 This sets up the correspondence between the logit setup above and the usual representation of wage and labor choice faced by monopsonistic firms.

Panel A should be familiar in this setup, where a monopsonistic firm \( k \) treated by an incremental wage floor initially increases its wage and employment \(^{130}\). There is a feedback effect through a shift up in the Residual LS, such that the initial employment increase is moderated. The intuition is that uncovered firms which raise their wages retain some of the workers who would have been poached by the covered firms.

\(^{16}\) The possibility of second round wage responses in the treated firms is not concerning, since the wage increase we consider here for firm \( k \) is imposed and takes firm \( k \) off its optimal wage setting curve. For firm \( k \)'s optimal wage to be higher than its new minimum wage once feedbacks are accounted for, the cross wage elasticity needs to be greater than 1. In this case, equating the response functions of the firms as in Bertrand competition would imply that the initial cross wage elasticities are multiplied by small factor.
Panel B illustrated the spillover effect on firm $j$. An increase in firm $k$’s wage does not affect the MRPL curve, but does shift the Residual LS curve of firm $j$ up through the term $\ln(n_k + \sum_i^S w_i^\beta)$ – representing the increase in outside options for workers in $j$’s consideration set. This in turn increases the MCL curve of firm $j$. In addition, the gap $\ln\left(\frac{\varepsilon_{jj}^n}{1+\varepsilon_{jj}}\right)$ between the new Residual LS and MCL curves narrows, because the firm labor supply elasticity $\varepsilon_{jj}^n$ increases, and this increases the wage through decreasing the markdown. Finally, there are second-round or multiplier effects as firm $j$ responds to adjustments of other firms. The cumulative increase in firm $j$’s wage is given by Equation [C.4] with a decrease in employment.\footnote{This highlights that some inelastic product demand elasticity is necessary for a sizeable cross wage elasticity. The more general expression representing the two channels for the cross wage elasticity is $\varepsilon_{jk}^w = -\eta \frac{d \ln p_k}{d \ln w_k} + \frac{d \varepsilon_{jj}}{d \ln w_k} \varepsilon_{jj}^n (1+\varepsilon_{jj})$. The first term represents the upward shift of the Residual Labor Supply curve, and the second term represents the decrease in the markdown (i.e. the gap between the Marginal Cost of Labor curve and the Residual Labor Supply curve). The decrease in the markdown will tend to dampen the employment decline. These terms contain recursive elements, which in Equation (3.2) appears as a multiplier on the two terms. Note that I assume that the multiplier feedback effects are not so great as to elicit second-round responses from the covered firms. For $p_k$ which is logit supply-constrained, $\varepsilon_{jk}^w = \frac{1+\eta\beta}{1+\eta\beta} \left(\frac{\beta p_k \eta}{1+\eta\beta} + \frac{\beta p_k \beta p_k}{\varepsilon_{jj}^n (1+\varepsilon_{jj}) (1+\eta\beta)}\right)$.}

C.6.1.2 Illustrative cases and sensitivity

Figure [C.4] illustrates the positively sloped locus which gives the trade-off that connected firms face between losing employment and raising its wage. The optimal wage-setting response is plotted from equation [C.4] as a negatively sloped dashed line, and intersects with the locus to pin down the optimal wage. The blue lines use the parameter values $\beta = 6$, $p_k = 0.5$, $p_j = 0.1$, and $\eta = 0.5$. Then, following firm $k$ raising its wage by 5%, firm $j$ can lose up to 15% of its own employment (if it does not respond by raising its wage at all), or retain its employment share and instead match the wage increase at 5%. The cross wage elasticity pinned down by the optimal wage-setting equation is $\varepsilon_{jk}^w = 0.62$, suggesting that firm $j$ increases its wage by about
Figure C.40: Diagram of wage floor effects on covered and non-covered firms

Notes: The equation for the Marginal Revenue Product of Labor Curve (MRPL) is \( \ln(mrpl) = \ln(A_j) - \eta \ln(p_j N) \) for firm \( j \), aggregate labor supply \( N \), proportion of employment \( p_j \), and firm-specific productivity \( A_j \). The equation for the Residual Labor Supply (LS) is \( \ln(w_{LS} j) = 1/\beta (\ln(p_j N) - \ln N + \ln(n_k + \sum_s w_s^j)) \), where \( n_k \) denotes the treated firms’ employment. The equation for the Marginal Cost of Labor (MCL) is \( \ln(mcl) = \ln(w_{LS} j) + \ln(1 + \varepsilon) \) or the Residual LS plus \( \ln(1 + \varepsilon) \).

The unconstrained firm-specific employment is found at \( \ln(mrpl) = \ln(mcl) \), with wage set by the corresponding point on the Residual LS curve, given by Equation 3.1 in text. Panel A presents the usual effect of a wage floor on a monopsonistic firm \( k \), such that the wage initially increases from \( w_{k0} \) to \( w_{k1} \) and employment increases from \( p_{k0} \) to \( p_{k1} \). The feedback effect on covered firms is captured by the shift from \( LS_{k0} \) to \( LS_{kC} \), with cumulative effect on employment to \( p_{kC} \) and wage remaining the same. Panel B presents the spillover effect if \( k \) is in \( j \)’s consideration set, where the Residual LS curve initially shifts up from \( LS_{j0} \) to \( LS_{j1} \), and similarly for the MCL curve. This raises the wage to \( w_{j1} \), but decreases employment to \( p_{j1} \). The cumulative spillover response is given by further shifts to \( LS_{jC} \) and \( MCL_{jC} \), which raises wages to \( w_{jC} \) and decreases employment to \( p_{jC} \).
3% and loses just about 6% employment. As usual, the calibration magnitudes in this simple model should be interpreted only illustratively.

The wage-cross-wage elasticity $\varepsilon_{wj}^w$ is positive, and sensitive to increases in other’s firm share $p_k$, the product demand elasticity parameter $\eta$, and higher competitiveness $\beta$ (it has low sensitivity to changes in $p_j$). $\varepsilon_{wj}^w$ is very similar even if firm $j$’s share is negligible, e.g. $p_j = 0.001$ implies $\varepsilon_{wj}^w = 0.6$ (compared to $\varepsilon_{jk}^w = 0.62$ above). If the primary firm share $p_k$ decreases, the cross wage elasticity also decreases but remains substantial even at low shares. For example, $p_k = 0.1$ with $p_j = 0.001$ replicates the cross wage elasticity of $\varepsilon_{wj}^w = 0.23$ from large employer wage increases found in [68].

If both firm shares are small, however, the cross wage elasticity is close to 0 as one expects (holding $\beta$ constant). Finally, under perfect competition with $\beta \to \infty$,
then $\varepsilon_{jk}^w \to 1$ even if all firms have negligible share. In all cases, the total profit of
the firm decreases.

**Functional forms.** In the figure, the intersection point depicting the optimal
response is dependent on the particular wage-setting curve chosen. A range of optimal
wage-setting curves are possible, which may imply other points on this locus. For
example, the functional form of the profit function could be different. [79] in their
appendix show a logit model to demonstrate re-allocation, and use a production
function which is increasing in $\ln(N)$, $A_j \ln(p_j(w_j)N)$ instead of $\frac{1}{1-\eta} A_j(p_j(w_j)N)^{1-\eta}$
above. For the calibration values above of $\beta = 6$ and $p_k = 0.5$, this implies a much
larger magnitude of $\varepsilon_{jk}^w = 0.94$.

**Fair wages.** Alternatively, the profit function could include other consider-
ations. If there are fair or reservation wage considerations, the production function
may follow instead $\frac{1}{1-\eta} A_j \left(\frac{w_j}{w_\star} p(w_j)N\right)^{1-\eta}$, a variant of [5] where $w_\star$ is a reference wage.
Again for the calibration values above, this implies a magnitude of $\varepsilon_{jk}^w = 0.75$.

Reservation wages could enter directly into the worker’s utility function, such that
$p_j = \frac{(w_j-b)^\beta}{\sum_{i=1}^N (w_i-b)^\beta}$. The expressions are similar to before, with $\varepsilon_{jk}^w = 0.9$.
Many other wage-setting equation variants are possible, and the upshot is that the locus provides

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18 As is standard for the literature, they assume in their model that all firms have negligible share and so do not consider spillovers. Including spillovers, the wage equation is $\ln w_j = \frac{1}{1+\beta} \left(\ln \left(\frac{w_j}{w_\star}\right) + \ln A_j - \ln(N) + \ln(\sum w_i^\beta)\right)$. The cross wage elasticity expression is $\varepsilon_{jk}^w = \frac{\beta p_j - \beta p_k}{1+\beta p_k} \left(\frac{\beta \phi_j}{\varepsilon_j(1+\varepsilon_j)} + 1\right)$. Both of these expressions are identical to above with $\eta = 1$. Note the parameterization value used above is $\eta = .5$. While the production function $\frac{1}{1-\eta} A_j(p_j(w_j)N)^{1-\eta}$ with $\eta = 1$ is strictly not defined, $\eta \to 1$ implies for both functions that production increases with employment like $1/N$. My main functional specification, $\eta = 0.5$, allows for a greater sensitivity ($1/\sqrt{N}$).

19 The wage equation becomes $\ln w_j = \frac{1}{1+\beta} \left(\frac{1}{\eta} \ln A_j - \frac{1}{\eta} \ln(w_\star) - \ln(N) + \ln(\sum w_i^\beta)\right)$. The cross wage elasticity expression is $\varepsilon_{jk}^w = \frac{\beta p_j - \beta p_k}{1+\beta p_k}$. Interestingly, this expression is identical to above with no own-elasticity terms and $\eta = 1$.

20 The cross wage elasticity becomes $\varepsilon_{jk}^w = \frac{\beta \phi_j}{1+\beta(1-\sum \phi_i)} \left(\frac{\beta \phi_j (1-p_j) + (1-p_j) p_k}{\varepsilon_j(1+\varepsilon_j)} + \eta p_k\right)$ where $\phi_j = \frac{w_j}{w_\star} - b$ is simply an additional factor, $\varepsilon_j = \beta \phi_j (1-p_j)$, and $\varepsilon_{jk} = -\beta \phi_k p_k$. The calibrated $\varepsilon_{jk}^w = .9$ follows for firms paying approximately five times the reservation wage, $w_j = 5b$ (and $p_j = 0.1, p_k = 0.5, \beta = 5, \eta = 0$). The elasticity is higher for low-wage firms, e.g. $\varepsilon_{jk}^w = 1.3$ for $w_j = 3b$.
a range of feasible cross-wage elasticities which under plausible parametrizations are large.

**Nested labor markets.** This baseline model of logit with strategic interactions can be extended in many ways. If the local market is responsive to the average wage, then employment need not decline by as much in the spillover firms. We can express this as

\[ n_j = p_j \bar{w}^\theta, \]

where the average wage is the firm-share weighted geometric mean \( \bar{w} = \Pi_{i=1}^S w_i^{p_i}. \) Then

\[ \ln(n_j) = \theta \ln(\bar{w}) + \beta \ln(w_j) - \ln(\sum_i \bar{w}^{\beta}) + e_{jj}^n = \theta p_j + \beta(1-p_j), \]

and \( e_{jk}^n = \theta p_k - \beta p_k \) (if logit supply-constrained). The cross-wage elasticity, using the wage setting equation C.3 also carries the extra \( \theta \) term,

\[ e_{jk}^w = \frac{1}{1+\eta \theta p_k + \theta(1-p_k)} (\frac{(\beta-\theta)p_k p_j}{\epsilon_{jj}(1+\epsilon_{jj})} + (\beta - \theta)\eta p_k). \]

The cross wage elasticity is slightly lower, though with a smaller employment decline. The intuition is that the aggregate market supply increases in response to the higher average wages, which in turn implies a lower absolute decrease in firm \( j \)'s employment. Since firm \( j \)'s optimal wage is primarily responding to changes in its own employment, its wage response is correspondingly smaller.

**Additional utility factors.** Several other factors, most obviously in this application distance, into the utility function of workers from their job in addition to the wage. We can pose this as

\[ V(w_j) = \beta \ln(w_j) + \theta X + \nu_{ij}, \]

where \( \theta X \) represents any non-wage utility factors. It turns out that following the working above, the expressions remain the same.

### C.6.1.3 Implications for outcomes and heterogeneity

**Profits.** As wage-setters, the first-order effects of an increase in \( w_k \) on \( k \)'s profits \( \pi_k \) are zero by the envelope theorem (wages move along the labor supply line tangent to the iso-profit curve). However, as noted by [31], this is not the case for wage externalities. Firm \( j \) is not optimizing along \( w_k \), and therefore the first order effects of an increase in \( w_k \) on \( j \)'s profits \( \pi_j \) are negative (the labor supply line itself moves
inward, such that it is tangent to a lower value iso-profit curve), approximately equal to the wage markdown times the employment effect. The predicted effects on profits are therefore more negative for spillover firms than bargaining council firms.

**Competitiveness.** How do the effects vary by the degree of competition in the labor market, parameterized by $\beta$? For the treated firm $k$, the effects of binding wages do not depend on $\beta$, though the employment response $\varepsilon_{jj}$ is greater for higher $\beta$. By the envelope theorem, for low $\beta$, the effects on profits are small, but more negative for higher $\beta$.

The effects on spillover firms are more complicated. Assume the production function is constant across differently competitive markets, though it may not be in practice. By equation 3.2 for the supply-constrained case, $\varepsilon_{jk}^{w} = \frac{p_{k}}{1/\beta + \eta p_{k}} \frac{p_{j}}{(1-p_{j})(1+\beta(1-p_{j}))} + \frac{p_{k} \eta}{1/\beta + \eta p_{k}}$. An increase in $\beta$ increases the second term and has an ambiguous effect on the first term; however, as mentioned earlier, the second term dominates the expression, and so $\frac{\partial \varepsilon_{jk}^{w}}{\partial \beta}$ is positive.

The employment effect can be backed out using this effect and the slope of the trade-off in Figure C.41. Here, $\varepsilon_{jk}^{n} = \beta p_{k}(\varepsilon_{jk}^{w} - 1)$, giving $\frac{\partial \varepsilon_{jk}^{n}}{\partial \beta} = p_{k}(\varepsilon_{jk}^{w} - 1 + \beta \frac{\partial \varepsilon_{jk}^{w}}{\partial \beta})$. The sign is ambiguous, with $\varepsilon_{jk}^{w} < 1$ and $\frac{\partial \varepsilon_{jk}^{w}}{\partial \beta} > 0$. Since the effect on profits depends on the employment effect, $\frac{\partial \ln(\pi_{j})}{\partial \ln(w_{k})} \frac{\partial w_{k}}{\partial \beta}$ is of the same sign and therefore also ambiguous.

C.6.2 Logit dynamic model

The standard logit can be adapted to a dynamic setting allowing for worker flows, following a simplified version of the models presented by [50, 123]. Every period workers take a fresh draw of idiosyncratic preferences $\epsilon_{ij}^{21}$. With probability $1 - \lambda$ the worker receives no offers, and with probability $\lambda$ the worker receives offers from

---

21This is an extreme assumption but may be reframed in several ways. Instead of $\lambda$ as offers, this could be the proportion whose preferences are redrawn. Or the $\epsilon_{ij}$ can follow a random walk, with small shocks every period, which would result in small proportion of switching.
a firm-specific consideration set $S_j$ of connected firms (always including itself).\textsuperscript{22}

Referring to equation C.1\textsuperscript{23} where firms $k = 1, ..., J$ represent the choices available to the worker, let $p_{j,S_j}$ represent the probably of employment in firm $j$ within set $S_j$.\textsuperscript{24}

The connected set $S_j$ of firm $j$ need not be the same as the connected set $S_l$ of the firms from which firm $j$ recruits. Quits and recruits are given by:

$$q_{j,S_j}(w_j) = \lambda \left(1 - p_{j,S_j}(w_j)\right)$$

$$R_{j,S_j}(w_j) = \sum_{l \neq j} p_{j,S_l}(w_j) \cdot \lambda p_{l,S_l}(w_l) N_{l,S_l}$$

\textsuperscript{22}The role of the consideration set is to provide the “local” aspect of the worker’s outside options. Standard models allow for all firms in the labor market to enter the worker’s choice set; I argue below with empirical evidence on worker flows that this is an extreme assumption. Instead, I assume a worker has a limited set of outside options, meaning that the share of each outside option is non-negligible. A simple framework can accommodate this, by incorporating geographic distance as a constraint. Utility net of transport costs is $V_j = \beta \ln(w_j - tx_i) - \ln(\sum_l (w_l - tx_i)^\beta)$, which is exactly as in the reservation wage setup above with $b = tx_i$ representing any matrix of distances $x_i$ for worker $i$ with associated costs $t$. A worker never accepts a negative net wage (i.e. $w_j - tx_{ij} > 0$), disqualifying such outside options. For example, if traveling a kilometre costs R10, then a minimum wage job paying R4,000 a month would yield a negative wage net of transport costs just 8 kilometers away. This only 4% of the City of Johannesburg Municipality.

\textsuperscript{23}Alternatively, $p_{j,S}$ may follow a more general class of distributions as in Langella and Manning (2021).

\textsuperscript{24}The assumption that the worker’s choice follows equation C.1, even under Logit preferences, ignores the dynamic aspect of the worker’s choice. For example, if firm $j$ has a better consideration set than firm $k$ (where “better” implies access to higher wage firms), then the worker may choose firm $j$ even if $w_j + \epsilon_{ij} < w_k + \epsilon_{ik}$. We can justify this assumption by requiring the consideration set to be exogenous to the firm. That is, the worker values firm $j$ as $V_j = \frac{1}{\rho + \lambda_{j,S_j}} [\beta \ln(w_j) + \epsilon_{ij} + \lambda_{j,S_j} A_j]$, where $A_j = \ln(\sum_{k=1}^S \exp(V_k))$ is the maximum value of Gumbel-distributed firm choices evaluated across consideration set $S$ (representing possible future switches to the best firm in the set). If firm consideration sets are all identical, either because they share the same labor market or because the worker does not have knowledge of it and so treats all consideration sets as identical in expectation, then this term cancels out and we return to the expression C.1. If consideration sets differ by firm but are exogeneous, then firm values are augmented by this term, $p_j = \frac{A_j w_j^\beta}{\sum_{k=1}^S A_k w_k^\beta}$ and the remainder of the analysis is the same. We may justify exogeneity of the consideration set through for example distance and production constraints, where the worker is outside of the consideration set if she would get negative utility.
Where \( N_{l,S_l} \) indicates the total number of workers in firm \( l \)'s connected set \( S_l \).

Quits are given by the proportion \( \lambda \) of the firm who receive an offer, who then choose firms in the network \( S_j \) according to \( (1 - p_{j,S_j}) \), since \( p_{j,S_j} \) is the probability the current firm is the best option in the worker’s choice set using the logit preferences earlier. Similarly, recruits are that proportion of such firm quits that choose \( j \) from the workers’ respective choice sets \( S_l \). These expressions are related to the baseline logit through \( n_{j,S_j} = R_{j,S_j}(w_j)/q_{j,S_j}(w_j) \). Taking logs, and the derivative of firm \( j \) with respect to a wage change in firm \( k \):

\[
\frac{\partial \ln(n_{j,S_j})}{\partial \ln w_k} = \frac{\partial \ln(R_{j,S_j})}{\partial \ln w_k} - \frac{\partial \ln(q_{j,S_j})}{\partial \ln w_k} = \left( \frac{\lambda \varepsilon_{jk} p_j \sum_{l \neq j}^{S_j} (p_l N_{l,S_l})}{R_{j,S}} - \left( -\frac{\varepsilon_{jk} \lambda p_j}{q_{j,S_j}} \right) \right) = -\beta \cdot \left( p_k \cdot \lambda \sum_{l \neq j}^{S_j} (p_l N_{l,S_l}) \right) + \left( q_{j,S_j} \right)
\]

Where the elasticity \( \varepsilon_{jk} \) is from equation ?? above, and \( f_{jk} \) represents the firm \( j \)'s average hires from and quits (flows) to firm \( k \).\(^{25}\) Firm \( j \)'s employment response to firm \( k \)'s wage increase is therefore proportional to \( f_{jk} \), the flows between firm \( j \) and firm \( k \). Given that any wage response from firm \( j \) is determined by the impact on

\(^{25}\)Equation C.5 assumes in the derivative with respect to recruits that other firm \( l \) size is constant. Taking changes in firm \( l \) size into consideration (as would be the case in the new steady state equilibrium) only changes the expression slightly: replace \( \sum_{l \neq j}^{S_j} (p_l N_{l,S_l}) \) with \( (2 \sum_{l \neq j}^{S_j} (p_l N_{l,S_l}) - N_{k,S'}) \). In particular, if the connected sets \( S \) are the same across all firms, noting \( k \) is a part of \( S \) too, this expression is identical to the original in equation C.5. The intuition is that any decreases in other firm’s sizes are just transitions towards increasing firm \( k \)'s size, which does not affect firm \( j \)'s net recruitment.
its own employment (using the static model of section C.6 above), the wage response too is a function of $f_{jk}$ \[26\]

How large is $f_{jk}$? The consideration set this depends on can be small or concentrated, allowing for large values. For example, if $S_j$ only includes the firms from which a worker receives an offer, the worker could face a choice similar to a 2 or 3 firm market in the static logit above. This relates to the values assigned to firms in the calibration of plausible cross wage elasticities above, justifying firms with substantial market shares\[27\].

While $\lambda$ does not appear explicitly in the final expression of equation C.4, it is potentially an important part of the firm’s response.\[28\] It determines the speed of the adjustment to the new firm steady state: the replacement rate $r$ of the firm’s workers as governed by the adjusted flows is given by $r = 1 - (1 - \lambda)^t$. For example, if $\lambda = 1/3, r = 1/3$ of the firm’s workers would be replaced in one period, and $r = 2/3$ would only be replaced in $t = 2.7$ periods. Measured over a discrete period of time then, firm $j$’s responses are also initially proportional to $\lambda$. Moreover, $\lambda$ may vary by firm. In a firm with a larger connected set $S$, we may expect the probability of receiving an offer $\lambda$ to be larger. Let $\lambda_{j,S} = \lambda(S)$, where $\lambda'(S) > 0$ leaves the offer function general besides that it increases with the number of firms in firm $j$’s set $S$.

\[26\]Note that the effect on flows differs depending on the wage of firms $k$ and $j$. If firm $k$ is a high wage firm and $j$ is a low wage firm, then firm $j$ has more separations to $k$ than hires from $k$. When $k$ raises its wage, $j$ will primarily experience the effect through workers who separate more towards $k$, since $j$’s hires will primarily be affected by a decrease in hires from other lower wage firms (since those hires are more likely to go to $k$). In the reverse situation, where $k$ has low wages and $j$ has high wages, then $j$ will primarily experience its change in employment through a decrease in direct hires from $k$.

\[27\]Markets in any case may be highly concentrated, even without the additional restrictions in choice created by search frictions. Berger, Herkenhoff and Mongey (2021) estimate an HHI of approximately 0.1 for a range of US local labor markets, implying a market share of 10% for equally sized firms.

\[28\]If we allow exogenous separations, and re-define $q_{j,S_j}(w_j) = \lambda(p_{j,S_j}) + \delta$ and $R_{j,S_j}(w_j) = \sum_{j \neq j} \lambda p_{j,S_j} \cdot p_{v,S_j} N_{v,S_j} + \lambda_u N_{u} p_{j}$, where $N_u = \delta/(\delta + \lambda_u)$, then $\partial \ln(n_{j,S_j})/\partial \ln(w_k) = -\beta (f_{jk} + p_k p_j \lambda_u \delta/((\delta + \lambda_u)))$. That is, $\lambda$ does appear, but only for the offer rate to the unemployed and as an additive term.
Then this provides a direct relationship between the firm’s responses (including its labor supply elasticity) and the firm’s connectivity.

The theoretical and empirical link between firm responses in the dynamic logit and its flows \( f_{jk} \) is a key contribution of this paper. In the empirical implementation that follows, I measure \( f_{jk} \) in the pre-period. In my event study of collective bargaining wage increases, multiple firms raise their wages: their proportions are additive. I therefore measure hires from and quits to any firm \( k \) part of a bargaining council \( BC \). My primary measure of connectivity is therefore \( f_{jBC} \), which is a revealed choice set of workers in firm \( j \). This is a flexible measure of the outside options of a worker, which allow for any patterns of industry and geolocation mobility.

### C.6.3 Application to collective bargaining and minimum wages

The model above pertains to the general equilibrium effects of a firm raising its wage in a simplified setup with strategic interaction. In this paper, I study the particular case of collective bargaining, where a minimum wage is agreed to by a group of firms that are part of a larger (connected) labor market. I largely abstract from the endogeneity of the minimum wage in the main analysis, though I discuss such possibilities.

**Assumption on labor allocation effect to covered firms.** My framework relies on a positive net labor allocation effect for covered firms to yield positive wage spillovers. For an individual firm as in by figure **3.1** panel A, the employment effect is positive as long as the prescribed wage is below the intersection point of the marginal cost and revenue curves. Thus the net employment effect will depend on how binding the wage agreement is relative to firm productivity for the mass of firms, which will vary by agreement and firm type. Insofar as low productivity firms face employment losses, they will tend to temper the wage increases through the classic union spillover mechanism, i.e. wages are pushed up in the covered sector but labor is displaced to
the uncovered sector, which pushes down wages there [89]. Note feedback effects on the covered firms will moderate the employment increase, and so a small employment effect in the covered sector, along with a wage increase in the uncovered sector, is consistent with the spillover mechanism.

Endogenous minimum wage bargaining may also ensure the negative employment effects category is negligible (given the interests of both firms and workers), though there are counter-tendencies (an example outlined in [138]). Note that even with no net employment change, there can still be substantial firm size and worker re-allocation effects for bargaining council workers. The scope for firms whose employment increases following adherence to a higher minimum wage is larger in more monopsonistic firms where the markdown of the wage from productivity is higher.

**Incorporating job queuing.** Positive wage spillovers in the uncovered sector are still possible if we allow for job queuing. We may modify the model with the following assumptions. At the beginning of the period, firms post their desired wage and employment (by unconstrained or wage agreement), subject to the usual system of labor supply constraints and demand constraints, and knowledge of the distribution of workers’ idiosyncratic preferences. Next, workers choose a firm, with the knowledge their own idiosyncratic utility as well as each firm’s desired wage and employment. There is no on-the-job search, and a worker may choose to go to a firm where the worker knows there are more workers that jobs, i.e. queuing, such that they face a risk of unemployment. Finally, firms randomly choose workers to fill its desired employment slots. If firms are not on their supply constraint, then the chosen workers will be paid at the posted wage, and the rest of the workers in the queue are unemployed.

This is illustrated in figure [C.42] in the case of a minimum wage on a covered sector firm. I focus on the case of a demand-constrained firm, since queuing does not occur at supply constrained firms. The diagram above suggests that firms ini-
tially choose the point \( p_k, w_k \). However, allowing for queuing, workers know that a point slightly to the right of this would give an expected wage that is equal. The curve of equal expected wage is traced out by the Expected Wage curve, by

\[
\ln w_E = \ln w_{k1} + \ln(p_{k1}N) - \ln(pN)
\]

where \( \ln w_E \) is the expected wage at \( \ln w_{k1}, \ln(p_{k1}N) \). Workers choose the point of intersection between the Residual LS curve and the Expected Wage curve, since further to the right no more workers are willing to work at the corresponding expected wage. The number of workers that choose firm \( k \) is therefore \( p_{kS} \), given by

\[
\ln(p_{kS}N) = \frac{1}{1+\beta} (\beta \ln w_{k1} + \beta \ln(p_{k1}N) + \ln(N) - \ln(\sum_{i} w_i^\beta))
\]

and corresponding expected wage on the Residual LS. Once firms randomly choose from this queue of workers, \( p_k \) workers are employed at wage \( w_{k1} \), and \( p_{kS} - p_k \) workers are unemployed.

The upshot is that even when a prescribed increase in the covered firm wage induces an employment contraction, whether due to demand constraints or being off either of the curves, more workers will queue than jobs available in hopes of getting the higher wage. As in the case for a positive employment effect discussed in the main text, this will causes the residual labor supply for uncovered firms to shift up, giving positive wage spillovers. Of course, if the wage floor is too high, and corresponding employment at firm \( k \) too low, then the number of workers who choose firm \( k \) will be lower than \( p_{k1} \). This wage is above the intersection of the MCL and MRPL curves; the gap between this neutral-effect wage floor and this intersection is

\[
\ln\left(\frac{1+\varepsilon_{jj}}{\varepsilon_{jj}}\right) \frac{\eta}{1-\eta} > 0
\]

\((0 < \eta < 1)\), i.e. higher for when the firm labor supply elasticity is lower (steeper Residual LS curve) and when the product demand elasticity is higher (steeper MRPL curve).

**Measuring flows to covered firms.** The non-covered firms are not bound by these wage agreements, and are modeled as above with spillovers proportional to the average quits and hires \( f_{jk} \) to the treated firms. In the empirical implementation that follows, I measure \( f_{jk} \) in the pre-period. In my event study of collective bargaining
Figure C.42: Diagram of wage floor effect on covered firms, with queuing

Notes: The equation for the Marginal Revenue Product of Labor Curve (MRPL) is \( \ln(mrpl) = \ln(A_j) - \eta \ln(p_jN) \) for firm \( j \), aggregate labor supply \( N \), proportion of employment \( p_j \) and firm-specific productivity \( A_j \). The equation for the Residual Labor Supply (LS) is \( \ln(w_{LS,j}) = \frac{1}{\beta}(\ln(p_jN) - \ln(N) + \ln(n_k + \sum_l S_l w_l^\beta)) \), where \( n_k \) denotes the treated firms’ employment. The equation for the Marginal Cost of Labor (MCL) is \( \ln(mcl) = \ln(w_{LS,j}) + \ln(1 + \varepsilon) \) or the Residual LS wage plus \( \ln(1 + \varepsilon) \). The unconstrained firm-specific employment is found at \( \ln(mrpl) = \ln(mcl) \), with wage set by the corresponding point on the Residual LS curve, given by Equation 3.1 in text. The equation for the Expected Wage Curve is given by \( \ln(w_E) = \ln(w_{k1}) + \ln(p_{k1}N) - \ln(pN) \) where \( \ln(w_E) \) is the expected wage at \( \ln(w_{k1}) \), \( \ln(p_{k1}N) \). With queuing, the number of workers allocated to firm \( k \) is set where the Expected Wage Curve intersects with the Residual LS curve, such that \( p_k \) workers are employed at \( k \) at wage \( w_{k1} \), and \( p_{kS} - p_{k1} \) workers are unemployed.
wage increases, multiple firms raise their wages. An advantage of my measure of spillovers, compared to for example geographical distance, is that worker flows are additive. This allows me to aggregate hires from and quits to any treated firm, where \( k \) denotes the bargaining council \( BC \). My primary measure of connectivity is therefore \( f_{jBC} \). What are plausible values of \( f_{jBC} \)? Medium-sized firms in particular have small sets of connected firms, for example firms with between 10 and 50 workers have on average less than 4 distinct firms which workers separate to in a given year. This motivates the calibration values for the cross wage elasticities above.

The theoretical and empirical link between a firm’s responses in the dynamic logit and its flows \( f_{jk} \) is a key contribution of this paper. It is a flexible measure of the outside options of a worker, which allow for any patterns of industry and geo-location mobility, and thereby identify the magnitude and mechanisms more precisely than previous work on spillovers. Bargaining council firms plausibly account for 50% of a firm’s labor market, implying a cross wage elasticity of 0.6 for even small connected firms.

C.6.4 Alternative models

C.6.4.1 Ronald McDonald monopsonies

[31] propose a simple model of monopsony with wage externalities. The model is actually closely analogous to above, except with a different wage function. It draws transparently on the Hotelling or Salop models, and when there is no “transportation” cost (\( t = 0 \) below) then it reduces to Bertrand competition.

\( N \) firms are imagined to be equally spaced around a unit circle (think of the line as a one-dimenstional propensity score of attractive characteristics), at distance \( 1/N \) apart. In this simplest version for expositioanal purposes, assume no entry or exit, and no reservation wages. With a “transportation” cost of \( t \) per unit distance, a worker on the circle will prefer firm \( j \) over firm \( k \) when they are at most distance \( x^0 \) away
from firm $i$, such that $w_j - tx^0 > w_k - t(1/N - x^0)$. This gives labour supply of $n_j = 1/N + (w_j - w_k)/t$, an own-employment elasticity $\varepsilon^n_{jj} = \frac{\partial n_j}{\partial w_j} = w_j/(t/n + w_j - w_k)$, and cross-employment elasticity $\varepsilon^n_{jk} = -w_k/(t/n + w_j - w_k)$.

The authors use a production function $\phi(r/p)$ which takes as input $k(r/p)$, i.e. capital which is already optimized. Then maximizing w.r.t. wage, $w_j = (\phi - t/n + w_k)/2$, and with symmetric wages, $w^* = \phi - t/n$. Profits decrease because the wage externalities have first order negative effects, even though the direct wage effects are negligible due to the envelope theorem.

In my context of partial coverage of the minimum wage changes, I allow for different firm wages. Recall, using the earlier production function, the wage is set as $\ln w_j = \ln(\frac{\varepsilon_{jj}}{1+\varepsilon_{jj}}) + \ln A_j - \eta \ln(p_j N)$ (equation 3.1), with cross-wage elasticity $\varepsilon^w_{jk} = \frac{d \varepsilon_{jj}}{d \ln w_k} \frac{\varepsilon_{jj} (1+\varepsilon_{jj}) - \eta \frac{d \ln n_j}{d \ln w_k}}{\varepsilon_{jj} (1+\varepsilon_{jj}) - \eta \frac{d \ln n_j}{d \ln w_k}}$. Using $\varepsilon_{jj}$ and $n_j$ for this model, the specific cross-wage elasticity is then given by $\varepsilon^w_{jk} = \frac{w_k}{1+\eta w_j/(n_j)} \left( \frac{1}{t/n + tw_j - w_k} + \frac{\eta}{n_j} \right)$.

In the simulations, setting $w_j = 5, w_k = 6, \eta = .5, n = 10$ and $t = 20$ (such that cost to travel between two neighbouring firms is $t/n = 2$), then the labor supply elasticity is $\varepsilon^w_{jj} = 5$ and $\varepsilon^w_{jk} = 1.14$. This is larger than in the main model above, similarly driven by the $\eta$ term though with relatively greater contribution through the change in elasticity (ratio of 3 to 1). The labor supply elasticity is more sensitive to the competitor wage in this model.

C.6.4.2 Search models

Another popular framework for modelling monopsony is the class of search models [43, 130]. In pure wage posting models without oligopsonistic firms, a firm $j$ connected to a firm $k$ (the treated firm, which raises its wage $w_k$) does not change its own wage $w_j$, for example if it follows wage-setting equation 3.1 with negligible firm shares. There is no incentive for such firms to react to other firms’ wage increases, even if
it leads to lower employment, since employment and the elasticity of labor supply to the wage are not dependent on other firms’ wages.

However, there are wage externalities in models of search with re-negotiation. Workers who receive a better offer from another firm may choose not to quite and instead re-negotiate up wages \[49\]. An increase in the wage of firm \( k \) may then lead to an increase in wages of firm \( j \). For a simplified exposition, I follow \[51\] and allow for both posting and renegotiating firms. In my setup, bargaining council firms bound by minimum wage agreements post these prescribed wages, while other potential spillover firms renegotiate with workers. For the simple two-period case where workers have no bargaining power, and receive offers at rate \( \lambda \) from bargaining council firms, the expected wage in period 2 for worker \( i \) at firm \( j \) depends on the firm’s productivity \( p \) and the last offer \( w_1 \) that the worker received:

\[
E[w_2(w_1, p)] = (1 - \lambda e^\lambda)w_1 + \lambda e^\lambda (\int_{w_1}^{p} x \cdot dF(x) + \int_{p}^{p_{\text{max}}} x \cdot dF(x))
\]

where \( F(x) \) is the distribution of wages in posting firms. \( w_2 \) will be higher after an increase in bargaining council prescribed wages if this results in more offers within the wage renegotiation range \((w_1, p)\), i.e. the mass of bargaining council firms increases within this range. It will also lead to an increase in the probability that the worker switches to another firm.

There are important differences with the logit model. The wage externalities depend on the distribution of posting firms, and may in fact decrease wages with alternative distributions. The firms are also able to engage in individual wage-setting with counter-offers, which may not be realistic especially in large low-wage firms. Extending the model past this highly stylized case, workers may actually take lower wages in the hopes of switching to higher wage bargaining council firms. For these reasons I do not use this as my main framework.
C.7 Data construction

The SARS administrative tax dataset provides a near-universe of formal sector individual labor market wage outcomes and firm balance sheet information. It is periodically updated, with the latest around of available years extending from financial year 2008 to 2018. It is easily one of the richest sources of economic data for South Africa’s formal sector economy. However, a key limitation is that the data were collected for the purposes of taxation only and by design misses out on key covariates essential to the analysis of many important economic questions. For example, there are no data on worker occupation, race or education; on the outcomes of non-workers pertaining to unemployment; or on whether a policy applies to a given worker or firm (e.g. individual grants or investment subsidies).

The purpose of this data appendix is to outline how I use bargaining council (BC) data which I compile and which are external to the tax data, and match them into the tax data. More generally for policy questions, the limits in the tax data may be partially mitigated by matching in information available from other sources, such as Statistics South Africa’s Quarterly labor Force Survey (QLFS) or government gazettes. This makes it possible to attain second-best estimates on a much wider range of key policy questions.

The matched firm-level dataset is available to any researcher at the National Treasury Secure Data Facility in Pretoria. Please contact me with any questions regarding where to find the data or associated code. The most important caveat is that the matching is imperfect. It relies particularly on the quality of the location and industry data. There has been some work on the industry codes and locations of firms, but some imprecision remains.

\[29\]
C.7.1 Compiling bargaining council agreements

The government gazette publishes bargaining council agreements, which may be found online at [https://www.greengazette.co.za/](https://www.greengazette.co.za/). Bargaining council agreements are generally set in 3-year terms, and pegged to inflation (plus a negotiated amount). By going through at least one agreement in detail per bargaining council, I record the industry and location of each. I supplement this with the compilation of wages provided to me by the labor Research Service, and check each wage against the actual 3-year agreements as published in the government gazette.

I match 34 bargaining councils, which correspond to 32 of the 38 private sector bargaining councils and 2 public sector bargaining councils. For each bargaining agreement, I select the SIC 5 classification code that best matches the wording in the agreement. This may be at the 3, 4 or 5 digit level depending on the industry descriptions. Similarly for location, I select area based on the description in each bargaining agreement, at the national (all locations), province or district council level. Note that some bargaining councils are defined at the municipal level, but I use district council as the lowest level for simplicity.

Each bargaining council agreement may have many clusters of locations, where each cluster may have a different set of bargained wages or conditions. There are 145 clusters in total. As a consequence of the clusters, the majority of the clusters are defined by district council locations even though nearly half of the bargaining councils are nationally based. In total, I consolidate nearly 1600 records of wages from different bargaining council clusters by industry and location.

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The following bargaining councils were not merged because I could not find the corresponding gazetted agreements: Amanzi, Grain, and Sugar Manufacturing and Refining. The Building BC (East London) and the Motor Ferry BC were excluded because they overlap directly on industry-location with other BCs (Building of Southern and Eastern Cape, and Motor respectively). The Furniture BC (South Western Districts) had no firm level matches in the tax data. The public sector bargaining councils are the Public Service Coordinating BC (PSCBC) and the South African Local Government BC (SALGBC).
Table C.11 provides these details on industry and location for each bargaining council. The average wage for all bargaining councils is about R4,500, provided in the table for 2018 (adjusted to 2016 ZAR). The wage selected is the minimum wage bargained in the agreement, which usually covers occupations such as “laborer” (41% of listed wages) or “general worker” (30% of listed wages). I cross-check these categories with the QLFS data, though this is a limited check given that the QLFS data are not representative at the district council level, they do not have as much detail on industry, and their correspondence with occupational categories in the agreements is not straight-forward. The minimum wages are highest in the New Tyre Manufacturing, Transnet and Public Service Coordinating bargaining councils, and lowest in hairdressing and furniture manufacturing for Kwa-Zulu Natal.

C.7.2 Matching bargaining council agreements to tax data

I construct the tax panel by combining all available worker-level variables for tax years, restricting to workers of ages 20 to 60 years old, restricting to one job per worker, and merging in firm level variables. The wage variable is just the wage code 3601. While the focus of this data project is not the panel, it is worth mentioning that the years 2008 and 2009 are likely incomplete and thus not fully comparable to other years. For example, the separation rates for 2008 and 2009 are much higher, which is unsurprising since incomplete records would result in workers dropping in and out of the panel as if they were separating from jobs.

Table C.12 briefly summarizes the tax data individual level panel between 2008 and 2018. The number of workers in the panel ranges from 6.7 million to 10 million, with 5-10% of the sample cut out due to restricting to firms with at least 10 workers. About 40% of workers are women on average, and workers are middle-aged at 37 years old. Most job durations are for 0.8 of the year, though this is clustered around full time jobs and a then a spread for partial duration. The 90th percentile of wages are
about 13 times the 25th percentile of wages, indicating the vast degree of inequality in the South African formal sector economy.

Table C.13 briefly summarizes the tax data firm panel between 2008 and 2018. From the firm balance sheet side, the profit and turnover are slightly higher in 2009, but are otherwise stable. The firm exit rate (firm is not observed in the next year) and outsource probability (defined when a third or more than 500 workers switch to another firm at the same time) are similarly higher for the incomplete years.

There are a few caveats to the matching process. Firstly, the industry variable is crucial and I follow the best practice as laid out in [43]. Within this industry code, I select the SIC 5 industry classification system. Secondly, the location variable is just as crucial for the matching. I focus on the IRP5 individual level business location variable, since the bargaining councils are defined by the location of the firm not the worker. A key limitation is that this variable is largely missing for the earlier years from 2008 to 2012. To impute location, I aggregate workers by payroll identification number (payereferenceno) and select the modal district council as the preferred value of the location. Within the same firm, under the assumption that firms do not change location, I assign the location of later years for each firm for records of earlier years. Given this near-complete location variable for district council, I then assign province based on the district council. A key problem with this approach is that within a payroll number, worker-level records suggest several associated locations. Perhaps some plants file taxes only at a head office, or payroll identification is itself an aggregation of many plants.

Thirdly, the bargaining council industry descriptions vary in how narrow the industry and location scopes are. For example, in terms of industry the food retail bargaining councils cover a narrow set of workers and can be assigned a four-digit industry code (6211). On the other hand, the metal and engineering bargaining council covers a range of manufacturing activities relating to metals production and is most
accurately described at the broad two-digit level (35). The examples in terms of location vary from national bargaining councils (chemical) to district council based bargaining councils (laundry). Note that the division between clusters and bargaining councils reflects organizational rather than classification differences. The clothing manufacturing industry for example is one bargaining council, but assigns different wages for 33 industry-location clusters. The building industry on the other hand seems to register entirely separate bargaining councils for locations, such as Bloemfontein compared to the Cape of Good Hope.

Out of a total of 1595 total potential records (11 years by 145 bargaining council clusters), I have matched 90% with wage records. In terms of number of workers over the entire panel, about 30% are presumed to be covered by bargaining councils and 70% uncovered. This is in line with the estimate in [42] using survey data.

C.7.3 Matching bargaining council agreements to tax data

What is the quality of the final matched data? One indication is the strong “first stage” estimated in section 3.5. Wages jump sharply as expected when there are large increases in prescribed bargaining council minimum wages. While this does not preclude error, i.e. both firms that are left out and firms that are mistakenly classified as part of a bargaining council, it does give assurance that the indicator is meaningful enough for information to pass through from the agreements to the observed wages of workers. Section 3.3 describes the matched data in further detail, comparing bargaining council firms with other formal sector firms in the economy.

In table C.14 I show characteristics by bargaining council focusing on those with an event or large wage increase at some point, as highlighted in my main analysis above. There is considerable variation across the different bargaining councils for each characteristic. The largest non-government bargaining council is the Metals and Engineering Industry (MEIBC), both in terms of workers (over 800,000) and
firms (16,000). There are several other large bargaining councils with hundreds of thousands of workers, such as Civil Engineering, Road Freight and Logistics, Motor Industry, and Chemical. There are also several small bargaining councils, which are more locally defined and in narrower industries, such as Laundry in KwaZulu-Natal, Meat Trade in Gauteng or Hairdressing.

The most profitable on a per person basis are the Meat Trade, Road Freight and Logistics, and Road Passenger bargaining councils. However, all of the bargaining councils have a high average per person profit that is far above the average wage. The bargaining council minimum wages go as low as around R30,000 per year or R2,500 per month (2016 inflation-adjusted). Incidentally, this is far below the 2019 national minimum wage of R3,500. There are also higherminimums, such as in the Tyre BC (R120,000 per year or R10,000 per month) and MEIBC (R76,000 per year or R6,300 per month). It is worth noting the low proportion of women in bargaining councils generally, with an average of 30% compared to over 50% for other firms. Indeed, the large bargaining councils listed above all have less than a quarter women, except for Chemical. Lastly, regarding labor market parameters, the firm level rent-sharing is generally lower than in other firms, perhaps because wages are set more sectorally, through some industries have high rent sharing elasticities (such the Tyre BC). The firm labor supply elasticity is closer to the average, though again with substantial variation.

Using this table C.14, we can use cross-sectional regressions to summarize the associations (weighted by the number of firms). These coefficients should of course not be interpreted causally, but rather as descriptively. Higher value added is strongly associated with higher minimum wages (p-value= 0.01), and marginally significantly associated with profits (p-value= 0.13). Wages and firm wage premia are also strongly associated with the minima (p-values of 0.00 and 0.02), and so is the average worker quality (as proxied by AKM worker effects, p-value= 0.00). Separations are not asso-
ciated with the minimum wages, which may be surprising, though they are strongly negatively associated with firm wage premia (p-value=.00).

Overall, the matched data provide a rich picture of the variation across bargaining councils across several characteristics (minimum wages, number of firms,), as well as some common features (a low proportion of women and increasing minima with value added per worker).
Table C.11: Bargaining council industry and location

<table>
<thead>
<tr>
<th>Bargaining Council</th>
<th>SIC5 code</th>
<th>Location</th>
<th>Wage</th>
<th>Clusters</th>
</tr>
</thead>
<tbody>
<tr>
<td>Building (Bloemfontein)</td>
<td>504</td>
<td>District Council</td>
<td>R 4,053</td>
<td>2</td>
</tr>
<tr>
<td>Building (Cape of Good Hope)</td>
<td>504</td>
<td>District Council</td>
<td>R 4,670</td>
<td>2</td>
</tr>
<tr>
<td>Building (Kimberley)</td>
<td>504</td>
<td>District Council</td>
<td>R 2,272</td>
<td>4</td>
</tr>
<tr>
<td>Building (NW Boland)</td>
<td>504</td>
<td>District Council</td>
<td>R 3,470</td>
<td>4</td>
</tr>
<tr>
<td>Building (SE Cape)</td>
<td>504</td>
<td>District Council</td>
<td>R 3,190</td>
<td>2</td>
</tr>
<tr>
<td>Canvas Goods</td>
<td>312</td>
<td>Province</td>
<td>R 5,325</td>
<td>1</td>
</tr>
<tr>
<td>Chemical</td>
<td>3354</td>
<td>National</td>
<td>R 5,995</td>
<td>1</td>
</tr>
<tr>
<td>Civil Engineering</td>
<td>502</td>
<td>National</td>
<td>R 6,055</td>
<td>1</td>
</tr>
<tr>
<td>Clothing Manufacturing</td>
<td>314</td>
<td>District Council</td>
<td>R 4,382</td>
<td>33</td>
</tr>
<tr>
<td>Contract Cleaning Services</td>
<td>8893</td>
<td>Province</td>
<td>R 3,178</td>
<td>1</td>
</tr>
<tr>
<td>Diamond Cutting</td>
<td>3426</td>
<td>National</td>
<td>R 1,975</td>
<td>1</td>
</tr>
<tr>
<td>Electrical</td>
<td>5032</td>
<td>District Council</td>
<td>R 3,516</td>
<td>16</td>
</tr>
<tr>
<td>Fishing</td>
<td>13</td>
<td>National</td>
<td>R 3,450</td>
<td>1</td>
</tr>
<tr>
<td>Food Retail and Restaurant</td>
<td>6211</td>
<td>District Council</td>
<td>R 2,997</td>
<td>9</td>
</tr>
<tr>
<td>Furniture</td>
<td>391</td>
<td>Province</td>
<td>R 2,635</td>
<td>10</td>
</tr>
<tr>
<td>Furniture Manufacturing (EC)</td>
<td>391</td>
<td>District Council</td>
<td>R 2,220</td>
<td>2</td>
</tr>
<tr>
<td>Furniture Manufacturing (KZN)</td>
<td>391</td>
<td>District Council</td>
<td>R 1,343</td>
<td>4</td>
</tr>
<tr>
<td>Furniture Manufacturing (WC)</td>
<td>391</td>
<td>District Council</td>
<td>R 2,634</td>
<td>2</td>
</tr>
<tr>
<td>Hairdressing</td>
<td>9902</td>
<td>Province</td>
<td>R 1,340</td>
<td>7</td>
</tr>
<tr>
<td>Laundry (Cape)</td>
<td>9901</td>
<td>District Council</td>
<td>R 3,626</td>
<td>1</td>
</tr>
<tr>
<td>Laundry (KZN)</td>
<td>9901</td>
<td>Province</td>
<td>R 4,227</td>
<td>1</td>
</tr>
<tr>
<td>Leather</td>
<td>3162</td>
<td>National</td>
<td>R 3,997</td>
<td>1</td>
</tr>
<tr>
<td>Local Government</td>
<td>913</td>
<td>National</td>
<td>R 6,715</td>
<td>1</td>
</tr>
<tr>
<td>Meat Trade (Gauteng)</td>
<td>6121</td>
<td>Province</td>
<td>R 3,185</td>
<td>1</td>
</tr>
<tr>
<td>Metal and Engineering</td>
<td>35</td>
<td>National</td>
<td>R 7,330</td>
<td>1</td>
</tr>
<tr>
<td>Motor</td>
<td>63</td>
<td>Metro</td>
<td>R 2,957</td>
<td>4</td>
</tr>
<tr>
<td>New Tyre Manufacturing</td>
<td>3371</td>
<td>National</td>
<td>R 11,069</td>
<td>1</td>
</tr>
<tr>
<td>Public Service Co-ordinating</td>
<td>912</td>
<td>National</td>
<td>R 7,685</td>
<td>1</td>
</tr>
<tr>
<td>Restaurant, Catering and Allied</td>
<td>6211</td>
<td>Province</td>
<td>R 3,320</td>
<td>12</td>
</tr>
<tr>
<td>Road Freight and Logistics</td>
<td>7412</td>
<td>National</td>
<td>R 4,919</td>
<td>1</td>
</tr>
<tr>
<td>Road Passenger</td>
<td>7122</td>
<td>National</td>
<td>R 5,894</td>
<td>1</td>
</tr>
<tr>
<td>Textile</td>
<td>312</td>
<td>National</td>
<td>R 5,348</td>
<td>1</td>
</tr>
<tr>
<td>Transnet</td>
<td>711</td>
<td>National</td>
<td>R 7,478</td>
<td>1</td>
</tr>
<tr>
<td>Wood and Paper Sector</td>
<td>3231</td>
<td>National</td>
<td>R 6,803</td>
<td>1</td>
</tr>
</tbody>
</table>

Notes: Bargaining council names are shortened for presentation. Province abbreviations are Eastern Cape (EC), KwaZulu-Natal (KZN) and Western Cape (WC). The SIC5 code is an industry code following the SIC5 classification system. Location indicate the geographic level of location assignment, i.e. national, provincial (9 in South Africa) or district council (52 in South Africa). Clusters refer to location specific units within each bargaining council. Source: Gazetted bargaining council documents published by the South African government.
Table C.12: **Summary stats on tax panel – individuals**

<table>
<thead>
<tr>
<th>Year</th>
<th>Workers</th>
<th>Selected</th>
<th>Age</th>
<th>Female</th>
<th>Duration</th>
<th>Wage_p25</th>
<th>Wage_p90</th>
</tr>
</thead>
<tbody>
<tr>
<td>2008</td>
<td>6696607</td>
<td>6184007</td>
<td>37.0</td>
<td>.39152604</td>
<td>.77653128</td>
<td>22730.393</td>
<td>276277.78</td>
</tr>
<tr>
<td>2009</td>
<td>7000445</td>
<td>6480139</td>
<td>36.8</td>
<td>.40828803</td>
<td>.78436452</td>
<td>23710.016</td>
<td>287943.19</td>
</tr>
<tr>
<td>2010</td>
<td>7667358</td>
<td>7161807</td>
<td>37.3</td>
<td>.41605818</td>
<td>.80991548</td>
<td>26688.479</td>
<td>311551.91</td>
</tr>
<tr>
<td>2011</td>
<td>8226735</td>
<td>7694692</td>
<td>37.3</td>
<td>.41411373</td>
<td>.80026042</td>
<td>26422.131</td>
<td>335109.28</td>
</tr>
<tr>
<td>2012</td>
<td>8518927</td>
<td>7981393</td>
<td>37.2</td>
<td>.41719598</td>
<td>.79825377</td>
<td>25371.135</td>
<td>337349.22</td>
</tr>
<tr>
<td>2013</td>
<td>8696944</td>
<td>8152934</td>
<td>37.2</td>
<td>.41669118</td>
<td>.79896146</td>
<td>26216.545</td>
<td>344890.5</td>
</tr>
<tr>
<td>2014</td>
<td>8805778</td>
<td>8256069</td>
<td>37.2</td>
<td>.41803619</td>
<td>.7965492</td>
<td>27521.789</td>
<td>347771.78</td>
</tr>
<tr>
<td>2015</td>
<td>9014434</td>
<td>8458650</td>
<td>37.4</td>
<td>.45647269</td>
<td>.79661143</td>
<td>28015.436</td>
<td>357910.69</td>
</tr>
<tr>
<td>2016</td>
<td>8956599</td>
<td>8393770</td>
<td>37.4</td>
<td>.46068534</td>
<td>.79065305</td>
<td>27712.656</td>
<td>359345.44</td>
</tr>
<tr>
<td>2017</td>
<td>9028088</td>
<td>8460710</td>
<td>37.5</td>
<td>.46368578</td>
<td>.79489625</td>
<td>28488.758</td>
<td>361228.75</td>
</tr>
<tr>
<td>2018</td>
<td>9911545</td>
<td>9332113</td>
<td>37.8</td>
<td>.4736971</td>
<td>.82527351</td>
<td>33592.277</td>
<td>386959.5</td>
</tr>
</tbody>
</table>

Notes. Wage is defined as the amount recorded under wage code 3601, or wage without benefits. For reference, the 2017 log wages per year at the mean of 11.3 and at the 25th percentile of 10.5 are equal to ZARR81,000 and R36,000 respectively. Selected sample of workers are those in firms with more than 10 workers. Source: SARS tax data panel.

Table C.13: **Summary stats on tax panel – firms**

<table>
<thead>
<tr>
<th>Year</th>
<th>lnWage (mean)</th>
<th>lnWage (p25)</th>
<th>Sep (%)</th>
<th>E-E sep (%)</th>
<th>lnProfit (mean)</th>
<th>lnSales (mean)</th>
<th>Exit firms (%)</th>
<th>Outsource firms (%)</th>
</tr>
</thead>
<tbody>
<tr>
<td>2008</td>
<td>11.14</td>
<td>10.44</td>
<td>52%</td>
<td>41%</td>
<td>12.25</td>
<td>13.77</td>
<td>17%</td>
<td>15%</td>
</tr>
<tr>
<td>2009</td>
<td>11.15</td>
<td>10.44</td>
<td>47%</td>
<td>40%</td>
<td>12.25</td>
<td>13.77</td>
<td>17%</td>
<td>13%</td>
</tr>
<tr>
<td>2010</td>
<td>11.20</td>
<td>10.47</td>
<td>40%</td>
<td>44%</td>
<td>12.22</td>
<td>13.65</td>
<td>13%</td>
<td>12%</td>
</tr>
<tr>
<td>2011</td>
<td>11.19</td>
<td>10.44</td>
<td>39%</td>
<td>44%</td>
<td>12.18</td>
<td>13.60</td>
<td>12%</td>
<td>11%</td>
</tr>
<tr>
<td>2012</td>
<td>11.19</td>
<td>10.44</td>
<td>38%</td>
<td>44%</td>
<td>12.16</td>
<td>13.58</td>
<td>12%</td>
<td>11%</td>
</tr>
<tr>
<td>2013</td>
<td>11.21</td>
<td>10.45</td>
<td>38%</td>
<td>44%</td>
<td>12.17</td>
<td>13.59</td>
<td>11%</td>
<td>11%</td>
</tr>
<tr>
<td>2014</td>
<td>11.22</td>
<td>10.47</td>
<td>37%</td>
<td>44%</td>
<td>12.17</td>
<td>13.60</td>
<td>11%</td>
<td>11%</td>
</tr>
<tr>
<td>2015</td>
<td>11.25</td>
<td>10.49</td>
<td>38%</td>
<td>43%</td>
<td>12.20</td>
<td>13.62</td>
<td>12%</td>
<td>11%</td>
</tr>
<tr>
<td>2016</td>
<td>11.26</td>
<td>10.51</td>
<td>38%</td>
<td>43%</td>
<td>12.19</td>
<td>13.61</td>
<td>13%</td>
<td>11%</td>
</tr>
<tr>
<td>2017</td>
<td>11.27</td>
<td>10.53</td>
<td>38%</td>
<td>47%</td>
<td>12.17</td>
<td>13.60</td>
<td>20%</td>
<td>11%</td>
</tr>
</tbody>
</table>

Notes. Wage is defined as the amount recorded under wage code 3601, or wage without benefits. For reference, the 2017 log wages per year at the mean of 11.3 and at the 25th percentile of 10.5 are equal to ZARR81,000 and R36,000 respectively. Separations are identified from changes in the firm identification of a worker level record across years. E-E separations indicates a change from one firm to another. Profit is the net profit declared by companies. Outsource indicates that at least a third of all workers or 500 workers in a firm switch to another firm. Note that the years 2008 and 2009 are incomplete. Source: SARS tax data panel.
235
4,184
12,949
1,498
175
3,808
535
1,351
315
704
1,498
959
119
73
708
112
16,041
9,678
3,307
5,835
516
2,003
231
543

14
53
46
31
199
38
70
33
58
30
30
9
22
32
36
24
52
30
43
62
88
31
69
99

56
45

Firm
size
(mean)

2.2
2.7
2.9
2.4
2.1
2.8
2.9
2.6
2.8
2.3
2.7
2.0
2.0
2.1
2.5
3.0
2.7
2.4
2.7
2.3
2.0
2.5
2.6
2.8

2.5
2.6

(p90/50)

Inequality

197,832
399,641
286,646
267,544
131,987
252,519
276,013
237,683
230,770
165,112
182,627
115,545
111,535
78,190
274,409
618,837
280,930
209,629
239,951
556,369
431,652
237,845
231,778
285,351

297,584
291,245

Profit
(mean)

370,867
647,299
493,997
449,326
238,008
474,142
446,932
371,351
374,223
308,727
345,990
242,726
197,121
167,806
456,064
1,006,669
520,657
375,959
372,997
764,644
585,981
414,127
415,325
446,234

534,753
492,254

Value
(mean)

65,638
99,747
82,251
73,227
42,056
93,135
73,713
50,103
62,374
64,145
69,007
55,626
44,811
46,733
75,200
131,070
103,191
72,278
57,332
89,418
75,458
76,531
89,347
77,287

103,016
84,537

(median)

Wage

51083
71281
63630
47994
33564
44034
38427
33990
26237
28754
26952
31501
42600
52262
57845
31708
76733
32923
33995
54161
62481
60229
120743
60519

56253

Min.
Wage
(mean)

37%
34%
41%
37%
41%
37%
43%
41%
33%
35%
37%
40%
39%
36%
35%
30%
35%
35%
42%
40%
40%
34%
35%
37%

37%
38%

(mean)

Sep.

30%
37%
36%
32%
44%
35%
54%
46%
35%
37%
17%
36%
45%
44%
34%
35%
35%
39%
41%
37%
40%
33%
30%
38%

36%
36%

(mean)

Churn

Table C.14: Description of individual bargaining councils

17%
38%
23%
68%
48%
23%
24%
49%
27%
23%
26%
88%
63%
64%
54%
46%
24%
28%
44%
18%
22%
54%
21%
32%

53%
30%

(mean)

Female

-0.06
0.06
0.00
-0.02
-0.33
0.10
0.04
-0.19
-0.08
-0.08
-0.04
0.04
-0.29
-0.32
-0.03
0.25
0.15
-0.02
-0.15
0.04
-0.05
-0.07
0.06
-0.06

0.14
0.03

Worker
FE
(mean)

-0.33
-0.09
-0.27
-0.31
-0.55
-0.07
-0.45
-0.36
-0.36
-0.25
-0.25
-0.15
-0.50
-0.56
-0.24
-0.28
0.03
-0.12
-0.37
-0.08
-0.25
-0.31
0.02
-0.40

-0.25
-0.15

Firm
FE
(mean)

0.17
0.24
0.22
0.09
0.00
0.14
0.46
0.28
0.12
0.13
0.17
0.06
0.11
0.30
0.14
0.19
0.19
0.17
0.11
0.09
0.21
0.13
0.34
0.20

.26
0.18

Rent
sharing
(elast.)

Notes. Of the 38 non-government bargaining councils, 13 are not shown due to poor matching in the tax data or no associated wage event: Amanzi
(water); Building (except for the Cape); Canvas Goods; Diamond Cutting; Restaurant, Catering and Allied Trades; Furniture in the Eastern Cape
and South Western Districts; Grain; Motor Ferry; Sugar Manufacturing. KZN refers to KwaZulu-Natal province and WC refers to Western Cape
province. In the columns, inequality refers to mean within-firm inequality, Min Wage refers to the average bargaining council negotiated minimum
wage over the period, churn refers to the sum of separations and hires as a proportion of firm size (subtracting the change in firm size), worker and
firm FE are the average respective components from an AKM regression, and rent-sharing and labor supply elasticities are estimated across all firms
within each bargaining council. The sample is all formal sector firms from 2008 to 2018 using the SARS tax data.

3,227
223,236
601,304
46,825
34,773
145,218
37,346
44,332
18,213
20,813
44,251
8,638
2,621
2,359
25,556
2,654
835,297
285,921
141,516
361,235
45,256
62,435
15,942
53,614

Building (Cape)
Chemical
Civil engineering
Clothing manuf.
Contract cleaning
Electrical
Fishing
Food and rest.
Furniture (KZN)
Furniture (WC)
Furniture (national)
Hairdressing
Laundry (Cape)
Laundry (KZN)
Leather
Meat trade
Metal & Eng.
Motor industry
Restaurant catering
Road Freight & Log.
Road passenger
Textile
Tyre
Wood and paper

149,555
67,377

(number)

(number)

8,370,023
3,062,582

Firms

Workers

Other firms
Ave. private BC

Name

305

0.78
0.76
0.61
0.86
-0.12
0.80
0.57
0.66
0.87
1.12
1.03
0.43
0.73
0.89
0.77
0.51
0.76
0.63
0.42
0.71
0.93
0.69
0.97
0.78

.67
0.70

Labor
supply
(elast.)


C.8 Institutional detail on bargaining councils in South Africa

Bargaining councils were originally formed as clusters of employers and trade union representatives, which met under “industrial councils”, and were introduced in 1924 (and excluded African Black workers). They opened to all workers in 1981, and finally turned into bargaining councils along with the amendments of key labor legislation in the democratic era. All existing industrial councils were deemed bargaining councils; [34] provide an historical overview of the legislation and institutions leading up to bargaining councils today.

Regulatory structure. We may broadly think of regulation in the labor market in the following way, from least organized to most [126]. Firstly, about a third of all workers are informally employed, typically without adhering to minimum conditions such as a written contract [22]. Secondly, the Basic Conditions of Employment and labor Relations Acts form the minimum conditions of employment and are applicable to all employment relationships – in reality, covering formal sector workers. The national minimum wage, set in 2019 and so not relevant to this study, also applies to all workers. Thirdly, wage floors are set unilaterally by the government for selected industries, mostly made up of low wage workers. [31] Fourthly, any workers can become union members, and workers can seek a union recognition agreement if at least 30% of the workplace belongs to the union. Fifthly, and most relevantly for this study, when unions collectively cover 30 per cent of workers in an industry-location (idiosyncratically defined), they can apply with employers to be recognized by the government as a collective bargaining council. There are currently 39 legally recognized private sector bargaining councils in South Africa, each covering a specific industry-region [73]. These regimes overlap: for example, the wholesale and retail industry is covered

[31] There are 11 sectoral determinations, and 8 of them set formal sector minimum wages: contract cleaning, civil engineering, learnerships, private security, wholesale and retail, forestry, farm workers, and hospitality.
by a sectoral determination, with subsets of the industry unionized with workplace bargaining, and other subsets covered under bargaining council agreements.

In terms of bargaining council wage agreements, increases are most often specified as percentage increases applicable to all workers. For example, the Road Passenger agreements for the years 2012-2016 each stipulate the same percentage increase to the existing occupation-specific minimum wages. Increases are often also very large, though most are centred around a real wage increase of just above 0%. Examples of the largest wage increases include a 12-16% increase in the Motor industry in 2011, and a 14% increase in the Road Passenger industry 2011.

**Description of unions.** The unions which represent workers on these bargaining councils are organized under federations, where the main ones are the Congress of South African Trade Unions (COSATU), the South African Federation of Trade Unions (SAFTU), and the Federation of Union South Africa (FEDUSA). COSATU represents about 2 million workers out of about 10 million formal sector workers in the country, and dominates representation in the public sector, including health (NEHAWU), transport (SATAWU), teachers (SADTU), police (POPCRU), and municipal (SAMWU) workers. SAFTU represents nearly a million workers, and dominates representation in manufacturing, most prominently metalworkers (NUMSA) and food (FAWU). FEDUSA was historically comprised mainly of white-collar workers, with a current estimated membership of half a million workers. Among the bargaining councils, the largest led by COSATU-affiliated unions are the Public Service (through various unions), Civil Engineering (through NUM), Road Freight and

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32 In general, unions under the same federation are coordinated through mass conference resolutions, non-poaching agreements (across industries), and administrative support; then within each union there are national, local and establishment structures that form the hierarchy representing workers belonging to that union.

33 Its most prominent private sector unions are in mining (NUM), retail (SACCAWU), and clothing (SACTWU). COSATU is formally in alliance with the ruling political party (the African National Congress or ANC), which was perhaps the core issue resulting in the splitting of dissatisfied and arguably more militant unions that formed SAFTU.
Logistics (through SATAWU); and the largest led by SAFTU affiliated unions are the Metals and Engineering Industry (through NUMSA), and Motor Industry (also NUMSA).

For some quantitative description, I match my compilation of bargaining council agreements to the dominant unions in the relevant bargaining councils. SAFTU-affiliated bargaining councils tend to have higher wage increases than COSATU, and there is some increasing relationship between larger wage increases and the age of the union as well as the number of members in the union. These are not well-powered however, and should only be taken as descriptive patterns rather than causal.

**Further review of South African literature on unions.** This adds to the main review above in section 3.3. The research wing of COSATU, [143], conducted a survey of union workers in 2005. While much has changed since then, it is a rare glimpse into union-specific information and preferences from workers. When asked what were the most important issues for bargaining, by far the biggest response was on wage increases (80%), with secondary mention of security, health benefits and training (20% each). My focus on wage changes rather than non-wage perks does seem to pick up the most important issue. The survey also suggests that, of the workers who leave the union due to separations from the job, two thirds are due to job changes and only one third is due to retrenchments – suggesting a high poaching rate, consistent with employers who compete over workers rather than simply draw from unemployment.

[138] focuses on the process followed whereby a core group of employers and trade union representatives first come to a wage agreement, and then this is extended to non-parties, i.e. the rest of the relevant sector-location (after government validation).[34] He argues that the core group may advocate for higher wages, to the detriment of

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34Moll considers the precursor to bargaining council, industrial councils, though this aspect of the process remained similar.
non-parties who are disproportionately lower wage firms which are then squeezed out and leave more of the product market for the core group. For my analysis below, I cannot distinguish between these core and non-party firms – this does not affect the spillover results. I provide evidence that large wage increases in bargaining councils are not on average correlated with bumps in profits.

While I do not focus on the level of the bargaining council wage premium, I do provide an estimate below of 15% in addition to the union premium. This compares to [32], who combine survey data from 2005 with gazetted bargaining council documents to estimate a bargaining council premium of about 9% above the union premium. However, their instrument of union membership by union membership of others in the household, is persuasively critiqued by [170] who notes this requires a logically inconsistent data generating process.

**Further review of South African literature on bargaining councils.** [85] also consider employment effects in three bargaining councils, the Metals and Engineering Industry, Clothing, and Textiles Bargaining Councils, similarly matching agreements into household survey data between 2010 and 2014. They find a negative employment effect of 8%. Their results are difficult to assess without indications of the quality of the control, pre-trends, or the first stage wage effect. For example, they note the largest negative effects are for the Clothing Bargaining Council, a sector in decline.

**Further review of South African literature on spillovers.** Subsequent to the working paper release of my analysis, [115] considered wage spillovers from mining industry shocks, finding wage effects of the same sign. In the product market, [162] found negative effects up the value chain following an increase in the agricultural minimum wage.

There are some historical discussions about dynamics related to my study. For example, an economic report regarding one of the major retailers in the country
suggests spillover dynamics, “During negotiations in 1987, the company agreed to an unusually high wage increase while also becoming a trendsetter by being the first retail company to recognise May Day and June 16 as paid public holiday” (emphasis added); and later notes for the same company, it is “perceived to be the market leader in the retail sector in relation to wages and employment conditions” [120].

Wage spillovers also featured in the horrific massacre of Lonmin’s platinum mine striking workers in Marikana in 2012. [153] record that the strike was sparked by a nearby wage increase, “Six months before the 2012 massacre, [...] AMCU managed to negotiate a 125 per cent increase in wages, which caught the attention of workers at other platinum mines, including Lonmin.” The Marikana strike in turn led to large wage increases in agriculture in 2013 and mining in 2014. While my model in section 3.2 focuses on the mechanism of employer competition, worker-motivated fairness concerns could also lead to wage spillovers as in these examples, which are also spread along worker flow networks through word of mouth.


[52] Caldwell, Sydnee, and Oehlsen, Emily. Monopsony and the gender wage gap: experimental evidence from the gig economy.


[84] Flinn, Christopher, and Mullins, Joseph. Firms choices of wage-setting protocols in the presence of minimum wages.


[86] Fogel, Jamie, and Modenesi, Bernardo. What is a labor market? classifying workers and jobs using network theory. [https://drive.google.com/file/d/1_5CrJHTDSiAujA0ViSIFIZWjwGfQl0ZKK/view](https://drive.google.com/file/d/1_5CrJHTDSiAujA0ViSIFIZWjwGfQl0ZKK/view) 2021. Job Market Paper.


