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Collective bargaining and spillovers in local labor markets

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Abstract
How does collective bargaining affect the broader wage structure? How are such spillovers transmitted? I present a model where firms with wage-setting power that are not covered by collective bargaining agreements, but are close to collective bargaining firms, are incentivized to increase their wages alongside these wage agreements. My model suggests an empirically rich measure of closely connected firms, the flow of workers between them. I test my hypotheses across a decade of wage agreements matched with worker-level data in South Africa. I show that bilateral worker flows reflect a wide range of firm characteristics, capturing firm links which are poorly predicted by industry and location. Observed wages in collective bargaining firms follow sharp increases in prescribed wages, and indeed firms more closely connected by worker flows to covered firms differentially increase wages more. My implied cross-wage elasticity is higher than comparable estimates in the literature because I am able to pin down the labor market segments empirically relevant to wage spillovers. A microdata simulation suggests that spillovers double the intensive and extensive margin effects of collective bargaining agreements on the full wage distribution.

1 Introduction

A classical question concerning collective bargaining institutions is their impact on non-covered wages. This impact is usually attributed to union threat or displaced labor supply effects (Freeman and Medoff, 1981; Lewis, 1986). 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.

Although wage spillovers may follow a variety of channels, I begin by sketching a possible framework. I start with a static model featuring preference heterogeneity which gives employers wage-setting power. I show that when a large part of the market (such as a set of collectively bargaining firms) is treated by an increase in wages, there is an upward shift in other firms' residual labor supply curves. This 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, but which accounts for workers' discounted valuation of job offers. Importantly, I allow offers to be drawn from firm-specific labor market segments. This leads to a key insight, that spillovers at a non-treated firm are proportional to the flow of workers with treated firms.

My matched employer-employee data allow me to trace these worker flows. Firms with higher bilateral worker flows are geographically closer, as expected, but are also similar along a number of other characteristics such as the 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' relevant labor market segment by incorporating information far beyond what the econometrician usually observes.

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. I then consider non-covered firms where flows to covered firms are high (i.e. in the same labor market segment), and compare them with firms where flows are negligible. I find similar wage increases of about 3.5%. 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 wage 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 non-covered 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 (Bassier, 2022), I show here that firms tend to hire certain types of workers, partially disregarding the large queue of unemployed in South Africa. Compared to other contexts, firms may be more monopsonistic in South Africa, and so wage spillovers may actually be relatively understated. I estimate small own-wage employment elasticities, and rule out large negative values.

My study contributes two sets of insights to our understanding of spillovers. Firstly, the specific institution of collective bargaining deserves attention as a pervasive labor market feature. In a recent *Journal of Economic Perspectives* symposium on labor market institutions, Bhuller et al. (2022) write that while employers negotiate wages with labor unions in most OECD countries, "the textbook models of competitive labor markets, monopsony, and search and matching all assume a decentralized wage setting where individual firms and workers determine wages." Moreover, they highlight the importance of studying sectoral bargaining institutions as opposed to establishment-level unionization (and Lewis (1986) makes a similar comment), since "a study focused on changing an individual determinant of wages, while holding the overall system of wage setting fixed, cannot tell us about the systemic effects of broader changes in the wage setting system." The focus on spillovers in this paper illustrates this point. An older, more data-constrained literature debated the question of union wage spillovers. Freeman and Medoff (1981) discuss how positive wage spillovers may arise from union threat effects, shifts in wage norms, or demand effects that offset higher non-covered labor supply.[1] My study focuses on what is potentially a much more direct mechanism of outside options.

Recent studies on spillovers have focused on minimum wages. Following a minimum wage increase, Cengiz et al. (2019), Engbom and Moser (2021), and Fortin, Lemieux, and Lloyd (2021) find shifts well into the overall wage distribution, Dustmann et al. (2021) and Gopalan et al. (2021) show within-firm spillovers, and Staiger, Spetz, and Phibbs (2010) show cross-workplace spillovers in the hospital sector. Yet Berger, Herkenhoff, and Mongey (2022a) 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

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[1]If employment in the covered sector decreases, and this leads to a decline in production, this would increase labor supply in the non-covered sector which in turn would decrease wages. The suggestion is that this may be offset by other factors. For example, demand for non-covered firms' products may increase, especially for firms in the same industry but not sharing the same geographic labor market. Another example is if covered firms have a high capital-labor ratio, and therefore release sufficient capital when decreasing employment, then non-covered firms may undergo capital deepening and raise wages.

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%).

A second contribution to the literature on spillovers is 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 worker flows between treated and non-treated firms. I view my model as complementary to Berger, Herkenhoff, and Mongey (2022b), since I rely on similar features of the labor market, but provide a flows-based framework amenable to clean identification.[2] 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.[3] Moreover, if spillovers do in fact spread through 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 reallocation effect consistent with monopsonistic competition (Dustmann et al., 2021). Thirdly, my event-studies show sharp declines in within-firm wage inequality, though small effects on overall inequality.

In the next section, I outline a theoretical framework of wage increases and spillovers. I provide institutional context in section 3, and profile in my data bargaining council firms and interfirm worker flows in section 4. I evaluate the effects of large changes in prescribed wages on bargaining council firms in section 5, and on spillover firms in section 6. I discuss additional findings on

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[2]My framework makes progress on both strategic interaction and the limited information of alternative jobs available to searching workers, two aspects which Card (2022) previously highlighted as needing further work in his review of the monopsony literature. My model also provides a motivation for related flows-based measures in concurrent studies of outside options by Demir (2022) and Jäger et al. (2022), as well as a study by Caldwell and Harmon (2019) which considers co-worker links.

[3]My cross-wage elasticity is approximately 0.8, compared to about 0.35 in the firm study of Staiger, Spetz, and Phibbs (2010), or 0.36 in the sector study of Willén (2021). My estimates are consistent with those *implied* by Fortin, Lemieux, and Lloyd (2021), who use the share unionized in a labor market to find a "threat effect" spillover on wages of non-union workers close to the union premium for men (though smaller and less clear for women).

robustness, heterogeneity and re-allocation in section 7, and conclude in section 8 with a simulation of aggregate effects.

2 Theoretical framework

2.1 Model features

My theoretical framework combines monopsonistic competition, strategic interaction, and firm-specific labor market segments. The contributions of this model are firstly to illustrate why and how spillovers may occur, and secondly to show the link between worker flows and 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 (Card et al., 2018; Lamadon, Mogstad, and Setzler, 2019). Firms are aware that there is a distribution of such idiosyncratic utilities, and so optimize wages with the knowledge that cuts to the wage have little impact on the (unobserved) utility of workers with high idiosyncratic preferences. The logit model provides a classic representation of this setup (Manning and Petrongolo, 2022; McFadden et al., 1973), and delivers the core intuition driving spillover wage responses: a firm's optimal wage depends on other firms' decisions.

I incorporate job search to deliver the link between worker flows and spillover wage responses. I assume a proportion of workers can switch firms every period, and receive job offers from a firm-specific labor market segment or "consideration set". Persistent consideration sets can be proxied by historical flows of workers to other firms, which is similar to the measure of outside options as co-worker outcomes explored by Caldwell and Harmon (2019) and Jäger et al. (2022). Labor markets defined by worker flows have been studied by Arnold (2020), Jarosch, Nimcsik, and Sorkin (2019), and Schubert, Stansbury, and Taska (2020), and dispense with arbitrary assumptions of industrial or regional boundaries; considering firm-specific flows allows markets to overlap. A key result of the dynamic model is that firm strategic interaction is driven along such worker flows.

My static model is perhaps closest to Berger, Herkenhoff, and Mongey (2022b).[4] I make the extension to a dynamic flows-based setting with consideration sets, and a corresponding empirical measure of spillovers. I also focus on prescribed wages covering firms in the middle 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.

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[4]They also rely on monopsonistic competition between firms with non-negligible market share, which induces strategic interaction. Their CES production function is closely linked to my logit framing (Anderson, De Palma, and Thisse, 1988),

I provide a detailed discussion of the model in Supplement F. Below I first summarize the static logit model, then take it to a dynamic context, and end with the application to my case of collective bargaining.

2.2 Static logit

Let the utility of workers be expressed as $V(w_j) = \beta ln(w_j) + v_{ij}$, where β parameterizes the responsiveness of worker utility to wages, and v_{ij} follows a Gumbel distribution indicating idiosyncratic preferences for the firm. The corresponding probability a worker is employed at firm j, or equivalently the firm share of j, is in log terms $ln p_j = \beta ln(w_j) - ln(\sum_l^J w_l^\beta)$. While an assumption of atomistic competition is usually made at this point in standard logit setups (e.g. Card et al., 2018), enabling one to treat the term $ln(\sum_l^J w_l^\beta)$ as a constant, I retain this term as it is essential for the strategic interaction that generates spillovers. The plausibility of non-atomistic competition is motivated by my discussion on consideration sets below, as well as empirical evidence (e.g. Azar, Marinescu, and Steinbaum, 2022).

The firm labor supply elasticity is $\varepsilon_{jj}^n = \frac{\partial ln p_j}{\partial ln w_j} = \beta(1 - p_j) > 0$. Employment decreases when another firm raises its wage, with $\varepsilon_{jk}^n = -\beta p_k < 0$. When treated firms k raise their wages, firm j must trade off raising its own wages or losing workers to these treated competitors. The optimal wage response of firm j is pinned down by its wage setting function. Assume 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 η parametrizes the downwards-sloping firm demand, N is the aggregate labor supply constraint, and $p_j(w_j)$ is the firm share as above. This yields the optimal own-wage w_j, and the cross-wage elasticity ε_{jk}^w:

$$ln w_j = ln A_j - \eta ln(p_j N) + ln(\frac{\varepsilon_{jj}^n}{1 + \varepsilon_{jj}^n}) \tag{1}$$

$$\varepsilon_{jk}^w = \frac{d ln w_j}{d ln w_k} = \underbrace{\frac{1 + \eta\beta}{1 + \eta\beta p_k}}_{\text{multiplier}} (\underbrace{\frac{\beta p_k \eta}{1 + \eta\beta}}_{\text{shift in Residual LS}} + \underbrace{\frac{\beta p_k \beta p_j}{\varepsilon_{jj}(1 + \varepsilon_{jj})(1 + \eta\beta)}}_{\text{change due to effect on } \varepsilon_{jj}^n}) > 0 \tag{2}$$

Diagrammatic representation. The setup above can be represented by the usual diagram for wage-setting of monopsonistic firms (figure 1). The Residual Labor Supply (LS) curve is given by $ln w_j^{LS} = 1/\beta(ln(p_j N) - ln N + ln(\sum_l^J w_l^\beta))$, with corresponding Marginal Cost of Labor curve equal to the Residual LS curve plus $ln(\frac{1+\varepsilon}{\varepsilon})$. Panel A shows the initial effect on a monopsonistic firm k treated by an incremental wage floor, which increases its wage and employment, as may be familiar to readers (e.g. Manning, 2003). Panel B shows that the increase in firm k's wage *shifts up the uncovered firm j's Residual LS curve* through the term $ln(\sum w_l^\beta)$. This represents the increase in outside options for workers at firm j. In addition, the gap $ln(\frac{\varepsilon_{jj}^n}{1+\varepsilon_{jj}^n})$ between the

new Residual LS and MCL curves *narrows*, because the firm labor supply elasticity ε_{jj}^n increases, and this increases the wage through decreasing the markdown. Finally, there are second-round or multiplier effects as firms respond to adjustments of other firms (including for treated firms k). The cumulative increase in firm j's wage is given by Equation 2, with a decrease in employment.[5]

Treated firms off the supply curve. I have assumed above that firms are supply constrained, and in particular that – holding other wages constant – a wage increase in treated firms will increase their employment share (leading to a decrease in the residual labor supply to spillover firms). There is growing support in the empirical monopsony literature that most firms are indeed labour constrained (Card, 2022; Manning, 2021). However, firm employment may be off their supply curves, for example demand-constrained or determined through efficient bargaining. Then the cross-wage elasticities above will be attenuated insofar as the treated firm employment response is lower (relative to the supply-constrained case); see Supplement F.1 for this more general analysis of spillovers. And, by incorporating queuing for rationed jobs into the model, wage spillovers would still be positive even if the net employment effect on treated firms is negative. In Supplement F.3, I show this extension of the model in the spirit of Harris and Todaro (1970), where the high wage in treated firms incentivizes workers to queue – at risk of unemployment – which would increase labor *allocated* to treated firms, in turn reducing the residual labor supply to spillover firms.

Comparative statics. Using the parameter values $\beta = 6$, $p_k = 0.5$, $p_j = 0.1$, and $\eta = 0.5$, the cross-wage elasticity is $\varepsilon_{jk}^w = 0.62$.[6] 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 the spillover firm's share is negligible, e.g. $p_j = 0.001$ implies $\varepsilon_{jk}^w = 0.6$. The treated firm share is much more salient: a decrease of p_k to 0.1 decreases the cross-wage elasticity to $\varepsilon_{jk}^w = 0.23$. If both firm shares are small, the cross-wage elasticity is close to 0 (holding β constant), though under perfect competition with $\beta \to \infty$, $\varepsilon_{jk}^w \to 1$ (see Supplement F.1.2 for further comparative statics).

Profits. As wage-setters, the first-order effects of an increase in w_k on k's profits π_k are zero by the envelope theorem if supply-constrained. However, as noted by Bhaskar and To (1999), 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 π_j are negative, 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. The large cross-wage elasticity is robust to alternative parametrizations. We can account for market-level employment responses to the average wage (which may be

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[5]The more general expression representing the two channels for the cross wage elasticity is $\varepsilon_{jk}^w = -\eta \frac{dlnp_j}{dlnw_k} + \frac{d\varepsilon_{jj}/dlnw_k}{\varepsilon_{jj}(1+\varepsilon_{jj})}$. The first term represents the upward shift of the Residual Labor Supply curve, and the second term represents the decrease in the markdown. The decrease in the markdown will tend to dampen the employment decline.

[6]These parameter values are difficult to measure, and so illustrative although consistent with rough estimates from the firm data.

important in high-unemployment South Africa). This is much like a nested logit function, closer to Berger, Herkenhoff, and Mongey (2022b), which dampens the spillover responses as it relieves pressure from the aggregate labor supply constraint. If there are fair or efficiency wage considerations, non-covered firms are further incentivized to raise wages, which increases ε_{jk}^{w}. The entire labor supply curve may be different, such as for firms along a unit circle as in Bhaskar and To (1999) or Bhaskar, Manning, and To (2002) (see Supplement F.4 for further model comparisons such as individual bargaining).

2.3 Dynamic logit

The static logit can be adapted to a dynamic setting with worker flows. Dynamic monopsony models have been presented by Berger, Herkenhoff, and Mongey (2022a), Caldwell, Dube, and Naidu (forthcoming), and Langella and Manning (2021). I build on these models by incorporating firm consideration sets, applying these models to the logit case, and drawing out the expressions for the cross-wage elasticity. My purpose is to show firstly that the spillovers result above holds in the dynamic case, and secondly that these spillovers are proportional to worker flows with treated firms. I illustrate this in a stylized case for intuition, and then summarize the full model.

Intuition of flows result. If workers draw fresh idiosyncratic preferences every period, and have no stickiness in staying in their current firm (this will be relaxed), then each period is just a repeated draw of the static model above.

Since equation 2 applies, the cross-wage elasticity is proportional to the share allocated to the treated firm, p_k. To incorporate firm-specific consideration sets, assume these are unobserved by the worker (this will be relaxed), then the p_k relevant to firm j can be measured as the proportion of workers in firm j in one period that go to firm k in the next period. Applying a similar logic to hires, the cross-wage elasticity is proportional to the average hires to and quits from firm k, i.e. the worker flows with treated firms:

$$\varepsilon_{jk}^{w} \propto h_k + q_k = \frac{h_j h_k}{h_j} + \frac{q_j q_k}{q_j} \tag{3}$$

Consideration sets. As motivation for incorporating consideration sets, note that standard search models allow for offers from *any* firm in the labor market. This seems an extreme assumption, and my descriptive evidence on inter-firm worker flows suggests hires and quits tend to be more firm-specific. An analogous issue in the product market is discussed in the marketing and recent industrial organization literatures (Abaluck and Adams-Prassl, 2021; Eliaz and Spiegler, 2011; Manzini and Mariotti, 2014; Matějka and McKay, 2015). In this literature, most products are ignored due to rational inattention, search costs, and constraints. Considerations sets in these papers have been modeled in a logit setup with some λ probability of switching to any unit in the

consideration set (while $1 - \lambda$ stay with the default), as I do for the dynamic model below.

A noteworthy assumption in much of this literature is to treat consideration sets as exogenous to the relevant choice variables. A motivation for this could be through exogenous constraints such as geographic distance (see Supplement F.2.2). The exogeneity assumption is supported by Lopez, Guerrero, and Axtell ([2015]), who show that bilateral worker flow networks are persistent, representing static edges which act as a "scaffolding" of worker mobility, in Finland and Mexico. In my empirical implementation, to alleviate remaining concerns, I also instrument post-period worker flows with pre-period flows (and find high persistence).

Dynamic quit and hire elasticities. The key additional feature of dynamic labor supply elasticities, compared to the static case, is that workers' job choice includes the value of switching to other firms and consideration sets. Assume that every period a worker takes a fresh draw of idiosyncratic preferences v_j for firm j, with Gumbel distribution as above (suppressing i-subscripts). With probability $1 - \lambda$ the worker receives no offers, and with probability λ the worker receives offers from a firm-specific consideration set S_j (always including itself).[7]

As in the static case above, workers derive present utility of the firm equal to $\beta ln(w_j) + v_j$, but the value of the firm now includes the discounted future value $\rho_w V_j^w$. Following Langella and Manning ([2021]), consider the utility of staying in the firm excluding the current v_j, $X_j(w_j, w_{-j}) = \beta ln(w_j) + \rho_w V_j^w(w_j, w_{-j})$, where other firms' wages are denoted w_{-j}. Then the value to the worker of being in firm j can be written as the weighted sum of the value of staying plus the value of choosing a firm from the consideration set:

$$V_j^w(w_j, w_{-j}) = (1 - \lambda)(X_j + E[v_j]) + \lambda E \max[X_j + v_j, X_1 + v_1, ..., X_{S_j} + v_{S_j}] \tag{4}$$

We can specify quits as the share of j workers who receive offers and choose not to stay at j. Note that future realizations of v_j are random to the worker and uncorrelated with previous realizations (by assumption, see Rust ([1987]) for a discussion). In particular, the expected value of being able to switch is the inclusive value, such that $\rho_w V_j^w$ takes the form $\rho_w((1 - \lambda)X_j + \lambda ln(\sum_l^{S_j} exp(X_l)))$. The probability the worker chooses j can therefore be specified in logit terms, as $\tilde{q}_j = exp(X_j) / \sum exp(X_l)$, and so quits $q_j(w_j, w_{-j}) = \lambda(1 - \tilde{q}_j)$. The elasticity of quits j to wages k is (see Supplement F.2.3):[8]

$$\varepsilon_{jk}^q = \frac{\lambda \beta \tilde{q}_j \tilde{q}_k}{q_j(1 - \rho_w(1 - q_k'))} \tag{5}$$

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[7]The assumption that all workers redraw idiosyncratic preferences can be relaxed, without loss of generality. λ is redefined as the product of the proportion redrawing preferences and the proportion receiving job offers.

[8]For interest, the own quit wage elasticity is $\varepsilon_j^q = -\frac{\lambda \beta \tilde{q}_j(1 - \tilde{q}_j)}{q_j(1 - \rho_w(1 - q_k'))}$, which matches the corresponding expression in Langella and Manning ([2021]), taking into account the appropriate substitutions. The own hire wage elasticity is $\varepsilon_j^h = \frac{\lambda \beta \tilde{h}_j(1 - \tilde{h}_j)}{h_j(1 - \rho_w(1 - q_k'))}$.

The elasticity of hires j to wages k follows a similar logic, with the probability of being hired at j equal to $h_j = \lambda \frac{exp(X_j)}{\sum_l^{S_j} exp(X_l)}$, and the probability utility is maximized at j denoted $\tilde{h}_j = exp(X_j)/\sum exp(X_l)$:

$$\varepsilon_{jk}^h = -\frac{\lambda \beta \tilde{h}_j \tilde{h}_k}{h_j(1 - \rho_w(1 - q_k'))} \tag{6}$$

For intuition, this elasticity increases with the probability of quitting k, and reduces to the static case when there is full discounting $\rho_w = 0$. Note the quit and hire market consideration sets may differ.

Firm wage optimization. The firm dynamically maximizes profits by setting the wage in each period, conditional on its previous wage and employment. Firm hires and quits respond dynamically to the wage, and also depend on other firms' wages as in the static model. The steady state optimal wage is very similar to the static equation 1, with $p_j = h_j/q_j$ as expected (see Supplement F.2.4):

$$ln(w_j) = ln(A_j) - \eta ln(\frac{h_j}{q_j}N) + ln(\frac{\varepsilon_j^h}{\varepsilon_j^h + 1 - \psi}) \tag{7}$$

Where $\psi = \frac{\rho q_j(1-q_j)}{q_j^2 + \rho(1-q_j)} \in [0, 1/3]$ and ε_j^h refers to the hiring labor supply elasticity. As above, this reduces to the static case with full discounting $\rho = 0$.

Dynamic cross-wage elasticity. Given the similarities between the static and dynamic wage-setting equations, the cross-wage elasticity ε_{jk}^w also follows a similar form. As an approximation, ignore the effect on the elasticity (due to its small magnitude, see static model). To translate into worker flows, let $\tilde{h}_{jk} = \tilde{h}_j \tilde{h}_k$ represent the proportion of $j's$ hires that instead go to k, and similarly $\tilde{q}_{jk} = \tilde{q}_j \tilde{q}_k$ the proportion of retained j's quits that instead go to k. Then using the quit and hiring elasticities derived above in Equations 5 and 6 (see Supplement F.2.5):

$$\varepsilon_{jk}^w \approx \frac{\eta \beta \alpha}{(1 - \rho_w(1 - q_k'))} \Big(\underbrace{\frac{\lambda \tilde{h}_{jk}}{h_j}}_{\text{Prop. } j\text{'s hires from } k} + \underbrace{\frac{\lambda \tilde{q}_{jk}}{q_j}}_{\text{Prop. } j\text{'s quits to } k} \Big) \tag{8}$$

where $\alpha = \frac{1 - \rho_w}{1 - \rho_w(1 - \lambda + q_k')}$ accounts for other spillover responses. The term in brackets is the average hires from and quits to firm k, that is, the flow of workers between firms j and k, $f_{jk} = \frac{\lambda \tilde{h}_{jk}}{h_j} + \frac{\lambda \tilde{q}_{jk}}{q_j}$.[9] Equation 8 thus retains the intuition of the stylized model equation 3, and suggests an estimating equation which relates the change in firm j's wage to the change in firm k's wage, as mediated by the flows f_{jk}: $\Delta ln(w_j) \propto f_{jk}\Delta ln(w_k)$.

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[9]Equation 8 holds in steady state, and over the short run there is an adjustment period which attenuates the cross-wage elasticity.

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. I largely abstract from the endogeneity of the minimum wage in the main analysis, though I discuss this possibility. My setting of wage agreements is actually advantageous for studying spillover responses, since in my framework above productivity-driven initial wage increases are complicated by second-round own-wage responses.

As discussed earlier, my framework relies on a positive net effect on labor allocated to covered firms to yield positive wage spillovers. Insofar as firms are not on their labor supply curves, the net employment effect will depend on how binding the wage agreement is relative to firm productivity for the mass of firms. Insofar as low productivity firms face employment losses, they will tend to temper 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 (Freeman and Medoff, 1981). In this case, as long as labor allocations change along the previous hiring and separations patterns, f_{jk} will still be proportional to the spillover response.

Measuring flows to covered firms. Firms not bound by collective bargaining wage agreements 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}:

$$f_{jBC} = \underbrace{\frac{\lambda \sum_k^{BC \in S_j} \tilde{h}_{jk}}{h_j}}_{\text{Prop. j's hires from BC firms}} + \underbrace{\frac{\lambda \sum_k^{BC \in S_j} \tilde{q}_{jk}}{q_j}}_{\text{Prop. j's quits to BC firms}} \tag{9}$$

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, as revealed by previous patterns of worker switches and which allows for any patterns of industry and geo-location mobility. This enables a more precise identification of the magnitude and mechanisms than previous work on spillovers.

3 Context and data construction

3.1 South African bargaining council context and literature

Bargaining councils have been perhaps 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 (DoL, 2018). I provide institutional details in Supplement H. The institutional arrangements are similar to much of Scandinavia and Continental Europe (Bhuller et al., 2022; Jäger, Noy, and Schoefer, 2022). Unions are mostly organized by sector, and belong to a small number of confederations. There is two-tier bargaining, where minimum wages are centralized, but supplementary wages can be negotiated at the establishment level.[10] A key difference regarding South Africa is the high levels of unemployment (about a quarter of the labor force), and informality (about a third of all workers). Bargaining council agreements are routinely extended by the government to workers who are not unionized, which contrasts with for example Germany, where firms opt-in to centralized bargaining.

There have been over a dozen studies of union and collective bargaining premia in South Africa, with the earliest by Moll (1996), Schultz and Mwabu (1998), and Butcher and Rouse (1999). Wittenberg and Kerr (2019) 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, Kerr and Wittenberg (2021) 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. 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. 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 Magruder (2012), who finds negative employment effects which are concentrated among smaller firms (and

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[10]Because minimum wages are set at the industry-location level most "minima" are in the middle parts of the unconditional wage distribution. They are therefore unlike a national minimum wage. One can think of the effects on the wage distribution as pushing up the bottom in the case of a national minimum, and pulling up the middle parts in the case of bargaining councils.

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.[11] 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 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 Hsieh and Olken (2014) 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.

3.2 Construction of matched panel data

I provide detail on how I construct my main matched firm panel in Supplement G, including summary statistics on the firm and individual panels. I summarize 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.[12] 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 dataset I have constructed from worker and firm tax records (National Treasury and UNU-WIDER, 2020a; National Treasury and UNU-WIDER, 2020b).

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%).

There are many sources of measurement error, such as in matching the tax data to bargaining council agreements (since there is no direct correspondence between industry and location codes in the two sources) and in assigning occupation-specific wages when the tax data have no occu-

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[11]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.

[12]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.

pational classifications (I use the "general labor" wage). However, as will be shown later in an event-study setup, observed wages track large sharp changes in 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.

4   Descriptive data

4.1   Descriptives on bargaining council firms

I profile bargaining council firms in my data here, with further details in Supplement A.1. 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% (controlling for worker effects), and have much less within-firm wage inequality than uncovered firms. These characteristics are consistent with the literature (Cahuc, Carcillo, and Zylberberg, 2014), 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 high. As one indicator, Supplementary figure A3 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.[13] 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.

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. While for others use flows to define labor markets (e.g.

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[13]A similar point was made by a bargaining council union official in informal discussions.

Arnold, 2020), I use flows as a measure of *distance* between individual firms, where firms "further away" are less suitable outside options.

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 the "distance" in several firm characteristics, including 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 – indicating that *deviation* in each firm's characteristic is appropriate (Supplementary figure A5). 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.

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 Supplementary figure A4, I show that using typical characteristics such as location and industry are poor *predictors* of worker flows, a finding shared by Guerrero and López (2015).

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 (as I model above). 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 Supplementary figure A5, 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. There are large flows across firm earnings classes as well as industries — only 30% of switches are within the same 1-digit industry.

Figure 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 Supplement A.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.

5 Treatment effects of prescribed wage increases

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. 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%.[14] I exclude similarly large increases across the preceding 3 years (to ensure a clean pre-period). There are 47 different events, with 33 unique wage increases (some bargaining councils have multiple separately bargained industry-regions), which I combine in a stacked event-study design to address issues with heterogeneous effects and staggered treatment (Cengiz et al., 2019; De Chaisemartin and d'Haultfoeuille, 2020). Supplementary figure B1 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 or industry as the relevant bargaining council. To avoid bias from spillovers (see next section), I exclude non-covered firms with more than 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 (ϕ_j), event by calendar year (τ_t), location by year ($\theta_{location \times t}$), pre-event firm size and wage by year ($\gamma_{firmsize \times t}$ and $\alpha_{wage \times t}$), as well as the pre-event change in log firm size ($\beta_{\Delta ln firmsize_{t<-1} \times t}$) and log mean firm wage ($\psi_{\Delta lnwage_{t<-1} \times t}$). 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).

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[14]The 3% threshold is arbitrary, though other cuts associated with heterogeneity analysis yield similar results.

$$y_{j,t} = \underbrace{\sum_{t=-3}^{-2} \delta_t(\tau_t \times treat_j)}_{\text{test of pre-trends}} + \underbrace{\sum_{t=0}^{2} \delta_t(\tau_t \times treat_j)}_{\text{main effects}} + \phi_j + \theta_{event \times loc. \times t}$$

$$+ \gamma_{firmsize_{t=-2} \times t} + \alpha_{wage_{t=-2} \times t} + \beta_{\Delta ln firmsize_{t<-1} \times t} + \psi_{\Delta lnwage_{t<-1} \times t} + e_{j,t} \quad (10)$$

For intuition, identification of the main coefficients of interest δ_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 δ_t are 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. Caetano et al. (2022) 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 7.1, including a doubly robust strategy. Note that my controls for pre-treatment change in size and mean wage weaken the pretrend test for those particular outcomes, such that only one of the three pre-treatment coefficients δ are free.

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.[15]

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[15]Moll (1996) 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.

5.2 Results

Wages. Figure 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 one considers higher percentiles of within-firm wages (though at the 80th percentile, there is still a 2% increase). Supplementary figure B2 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 (Blandhol et al., 2020; DiNardo and Lee, 2004).

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.[16]

Other outcomes. I show other firm responses to bargained wage increases in Supplementary figure B3. 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 Bassier (2022).[17] 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 $-.02$ and $.01$. In addition, as pointed out by Berger, Herkenhoff, and Mongey (2022a), 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), which is suggestive of negligible net within-firm

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[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. Badaoui and Walsh (2022) motivate this type of endogenous compliance for developing countries, where worker are relied on to detect contract violations, but workers refrain from reporting such violations if they know their low-productivity firm will downsize as a result.

[17]Using the 3% wage effect, yielding a separations elasticity of $-.75$, and the formula in Manning (2003) that the firm labor supply elasticity is -2 times the separations elasticity.

employment effects.

What does the muted covered firm employment effect suggest about spillovers, given a model where spillovers are responses to shifts in the residual labor supply? A few notes caution against strong priors. Firstly, observed effects may include responses to non-covered wage increases, meaning that absent the spillovers the employment effect may have been positive. Secondly, large heterogeneity in employment effects may drive spillovers in relevant connected firms; section 7.3 gives evidence for positive employment effects in high productivity and high wage firms. Thirdly, as discussed in the model, the residual labor supply may still contract for spillovers if workers queue for jobs at high wage covered firms, which is particularly plausible in South Africa.

Regarding further outcomes, 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 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 is only suggestive.

6 Spillover effects of prescribed wage increases

6.1 Empirical design

To quantify the non-covered 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 2 above, and specifically the term f_{jBC} in equation 9. Intuitively, if the same workers are employable at different firms, these firms define viable outside options. Labor constrained firms compete over these same workers, meaning that wage spillovers should transmit through this channel of worker flows.

In terms of specification, I follow much of equation 10 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 detailed 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 $f_{j(c)BC}$.[18] For interpretability,

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[18]Note that in the model, firm pre-period flows f_{jBC} is one sample realization of the latent attractiveness of treated

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 a treatment dosage, which is discussed in the context of difference in differences by Callaway, Goodman-Bacon, and Sant'Anna (2021) and Chaisemartin et al. (2022).

$$y_{j,t} = \underbrace{\sum_{t=-3}^{-2} \delta_t(\tau_t \times \tilde{f}_{j(c)BC})}_{\text{test of pre-trends}} + \underbrace{\sum_{t=0}^{2} \delta_t(\tau_t \times \tilde{f}_{j(c)BC})}_{\text{main effects}} + \phi_j + \theta_{event \times loc. \times t}$$
$$+ \gamma_{firmsize_{t=-2} \times t} + \alpha_{wage_{t=-2} \times t} + \beta_{\Delta ln firmsize_{t<-1} \times t} + \psi_{\Delta ln wage_{t<-1} \times t} + e_{j,t} \qquad (11)$$

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 11, 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). Huber (2021) 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.[19] Secondly, Huber (2021) 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 Supplementary figure C3).

I use a split-sample IV strategy for the main estimates to reduce measurement error in the $\tilde{f}_{j(c)BC}$ 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 by the average flow for the complement sample within each industry-

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firms. Averaging over detailed industry-location cells increases the signal to noise ratio (as does instrumenting, see below), while preserving the variation in connectedness to treated firms.

[19]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.

location. The split sample instrument has been used for example by Bassier, Dube, and Naidu (2021) and Goldschmidt and Schmieder (2017).

6.2 Results

Wages. Figure 4 shows large spillover effects from prescribed bargaining council wage increases on uncovered firm average wages. Panel A shows the continuous flows regressor, where one unit corresponds to industry-locations with an average of about 10% pre-event worker flows to the relevant bargaining council. There is a sharp rise in wages one and two years after the bargaining council wage increase, with insignificant wage effects in the years prior to that. The flows regressor is especially informative as it shows both the existence of the wage effect, and consistency with the mechanism that it increases with flows. Panel B shows the binary regressor, where treated units correspond to high flow firms. There is a sharp rise in wages, this time in the final event period, with flat and insignificant effects prior to that.

The interpretation of these wage effects takes some care, as the flows variable is not in wage units. Panel A shows a wage increase comparable to the wage increase for directly treated firms shown in figure 3, of about 4%, and panel B shows an even higher (though not statistically distinguishable) effect. These wage spillovers may seem high, but note that figure 3 represents an average. There is wide heterogeneity in bargaining council wage increases, for example up to 10% for mid-size firms (see Supplementary figure B2), and the spillover firm wage effect should be compared to the relevant flows-weighted average bargaining council wage increases. I will implement this normalization in revisions to this paper.

Figure 4 panel C shows 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 (Freeman, 1982; Kahn and Curme, 1987) – see Supplementary figure C1 for this and other wage outcomes. Panel D shows 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-wage 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 5 shows other firm outcomes, with further outcomes shown in Supplementary figure C2. 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). Although larger than the treated firm employment

effect, this is close to the median estimate of -0.17 from a recent review of US minimum wage studies, and the 95% confidence interval still rules out large negative employment elasticities of -0.8 or lower (Dube, 2019). As for bargaining council firms, unemployment insurance payments increase strongly. 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 (Bronars and Deere, 1994). 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.[20]

Supplementary figure C2 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 monopsonistic local environment with some degree of wage competition over employed workers. Although general unemployment in South Africa is extremely high, unemployment relevant to these 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 (Supplementary figure C3). This serves as a placebo or falsification test similar to Cengiz et al. (2022), 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.

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[20]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. 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 5 shows the profit *margin*, not profit itself.

7 Additional findings

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 Supplement D.

Bargaining council firm results. 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.[21] Thirdly, 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 firm results. I provide further discussion and tests in Supplement D.2. Figure 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, by controlling 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 a small, insignificant estimate of $-.004$ (SE of $-.008$).

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.

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

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[21]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.

industries and locations, comparable to previous work, I find no wage effects – which highlights the value of using my worker flows measure. 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 Supplement D.3. Figure 7 shows noteworthy wage estimates. 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.

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 Arkhangelsky and Imbens (2021) 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 Rambachan and Roth (2019).

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 Supplement D.4. I highlight two sets of result here.[22]

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[22]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.

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 2.4). 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 connectivity of the consideration set of the firm (see section 2) is to look at the top share of workers flowing from a firm to another. I find that firms with a low such top share (indicating less concentration) 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.

7.3 Re-allocation

In the modeling section 2, I treated the effect on total bargaining council employment p_k as largely homogenous, with some discussion of differential effects by firm productivity. Although the average effect on bargaining council firm size is close to zero, I show that there are significant decreases in employment for low productivity firms, and suggest that these may be offset by increases in firm size for higher productivity 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 (Dustmann et al., 2021) and Sweden (Bustos, 2021).

Figure 8 shows the results from a regression which uses the main bargaining council specification 10, 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 in Supplement D.5.

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 Supplementary figure D12 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.

8 Discussion

8.1 Aggregate labor market effects

How important are bargaining council wage increases for the aggregate earnings distribution, considering both the direct and spillover wage effects? As Supplementary table A2 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 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. Fortin, Lemieux, and Lloyd (2021) 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 2. 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. Supplement E 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.

What would the wage spillovers look like if there were fewer frictions limiting workers' consideration sets? Supplementary figure E3 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.

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 firm wage-setting power 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 (Goos, Manning, and Salomons, 2014), and as for South Africa, Crankshaw (2022) 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 unionized 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, this paper highlights the potential power of regulation, whether in the form of minimum wages or collective bargaining, to influence the wage structure. Collective bargaining councils or wage boards are a popular policy recommendation to constrain monopsony power (Dube, 2018; Stelzner and Paul, 2020), 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 (Card, Heining, and Kline, 2013; Song et al., 2018).

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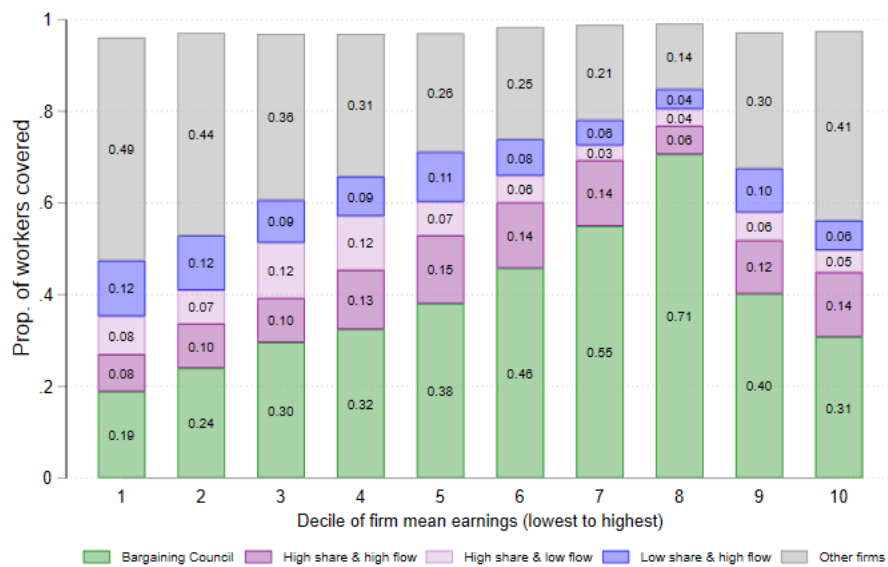
9 Figures

Figure 1: **Diagram of wage floor effects on covered and non-covered firms**

(a) **Initial treatment effect on covered firms**

### (b) **Cumulative spillover effect on non-covered firms**
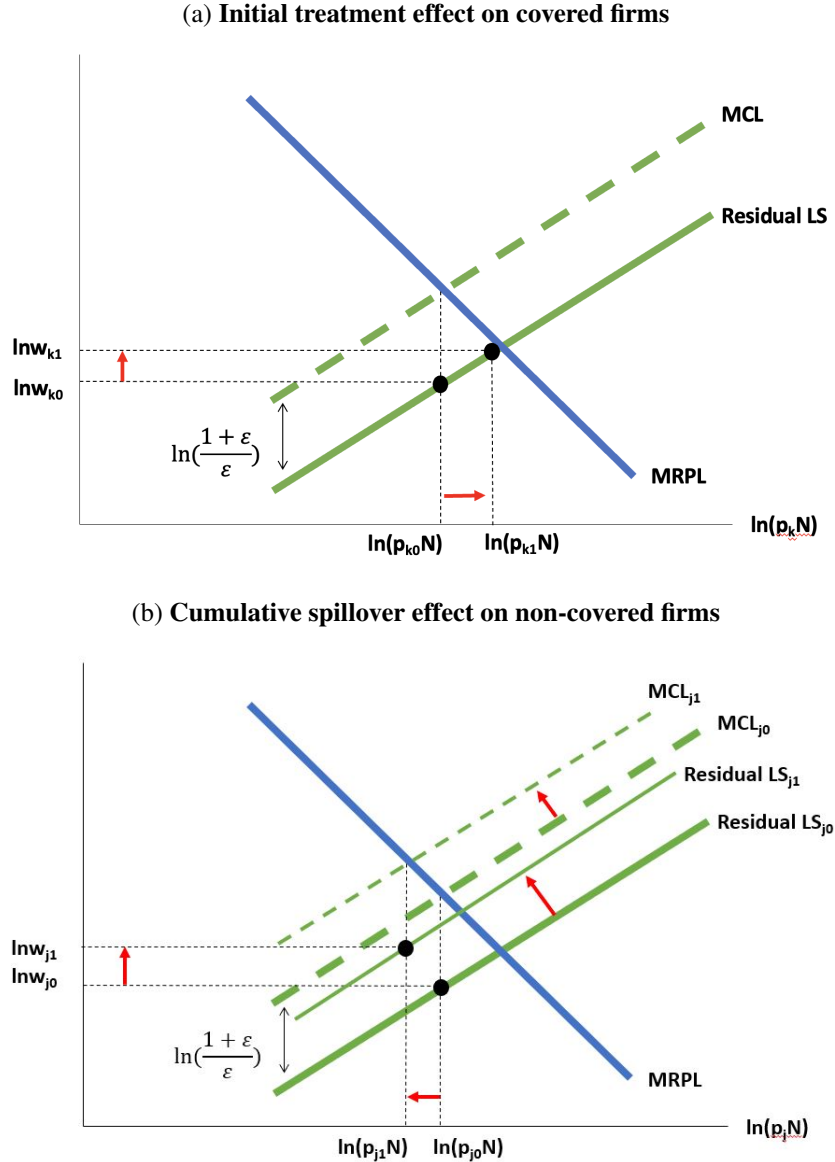
Notes: The equation for the Marginal Revenue Product of Labor Curve (MRPL) is $ln(mrpl) = lnA_j - \eta ln(p_j N)$ for firm j, aggregate labor supply N, proportion of employment p_j and firm-specific productivity A_j. The equation for the Residual Labor Supply (LS) is $lnw_j^{LS} = 1/\beta (ln(p_j N) - lnN + ln(n_k + \sum_l^J w_l^\beta))$, where n_k denotes the treated firms' employment (this line is only linear at a first order approximation). The equation for the Marginal Cost of Labor (MCL) is $ln(mcl) = lnw_j^{LS} + ln(\frac{1+\varepsilon}{\varepsilon})$ or the Residual LS plus $ln(\frac{1+\varepsilon}{\varepsilon})$. 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 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}.

Figure 2: **Distribution of bargaining council, spillover and other workers, by decile of earnings**

Notes. Share in the legend 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.

Figure 3: **Effect of prescribed wage increases on wages of bargaining council firms**
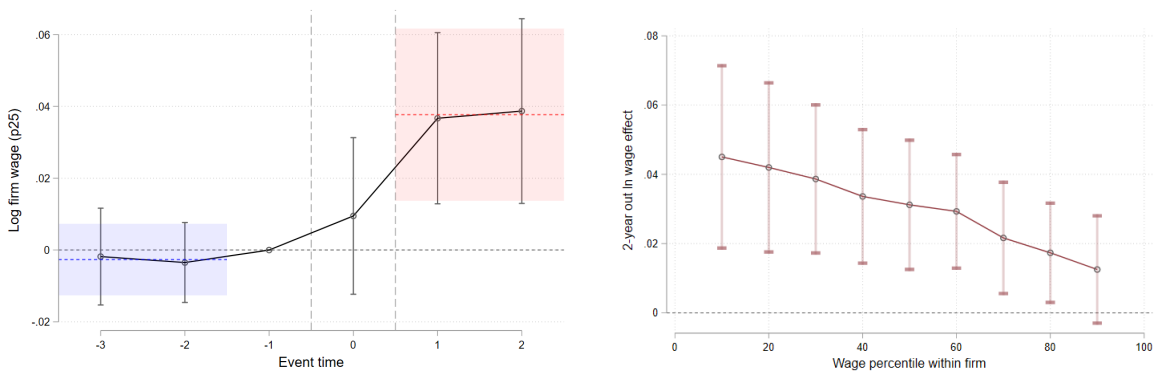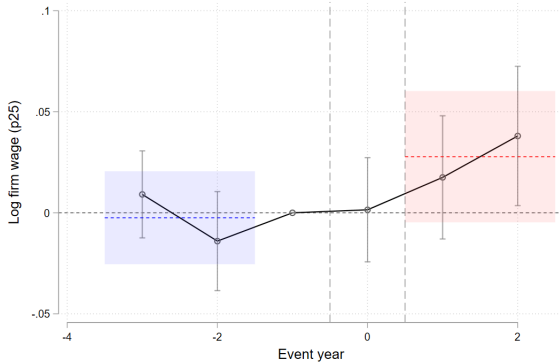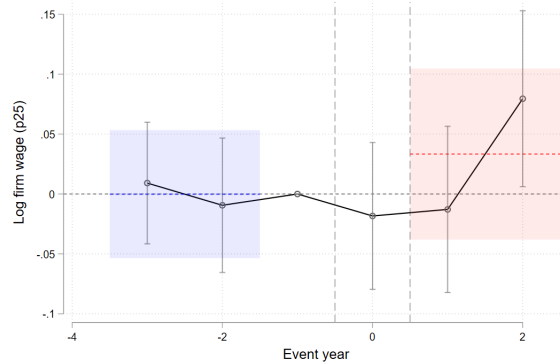

(a) **Log firm wage (p25)**

(b) **Log firm wage,
by within-firm wage decile**

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 10). 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.
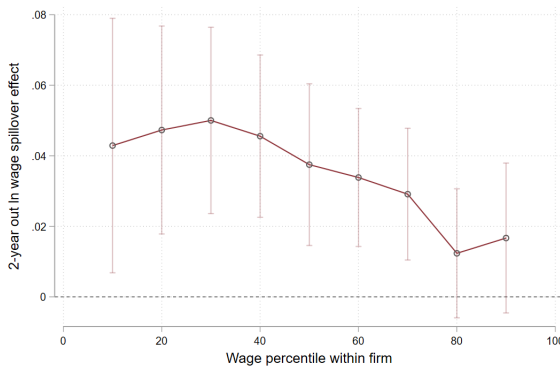
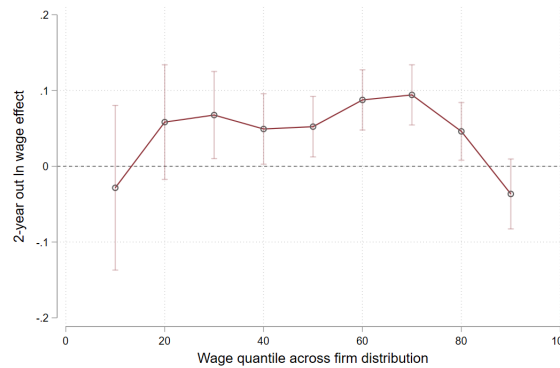Figure 4: **Effect of prescribed wage increases on wages of non-covered firms**

(a) **Log firm wage (continuous regressor)**


(b) **Log firm wage (binary regressor)**

(c) **Two-year out wages,**
by decile of within-firm wages

(d) **Two-year out wages,**
by decile of across-firm wages

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 11). 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 D shows the final-period estimated effects on log wages, by decile of wages across different firm using a RIF regression.

Figure 5: **Effect of prescribed wage increases on other outcomes of non-covered firms**
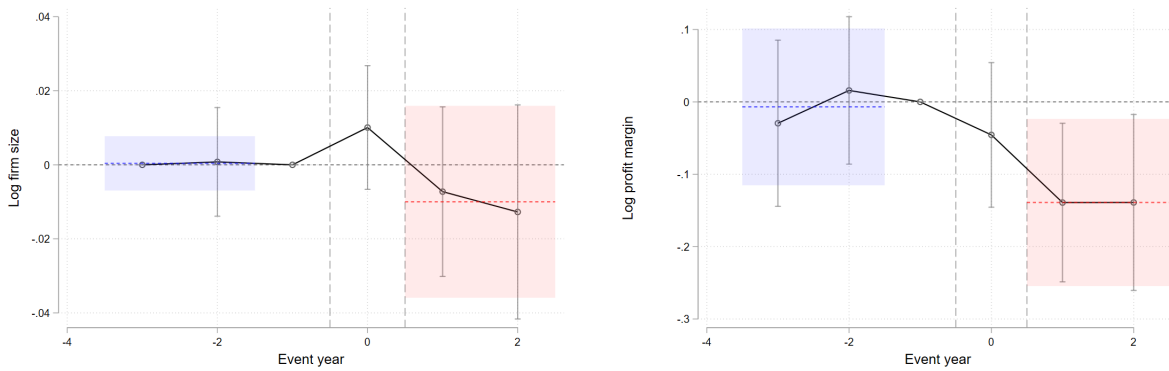
(a) **Log firm size**

(b) **Log firm profit margin**

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 11). 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.

Figure 6: **Alternative estimates of two-year out spillover wage effects**
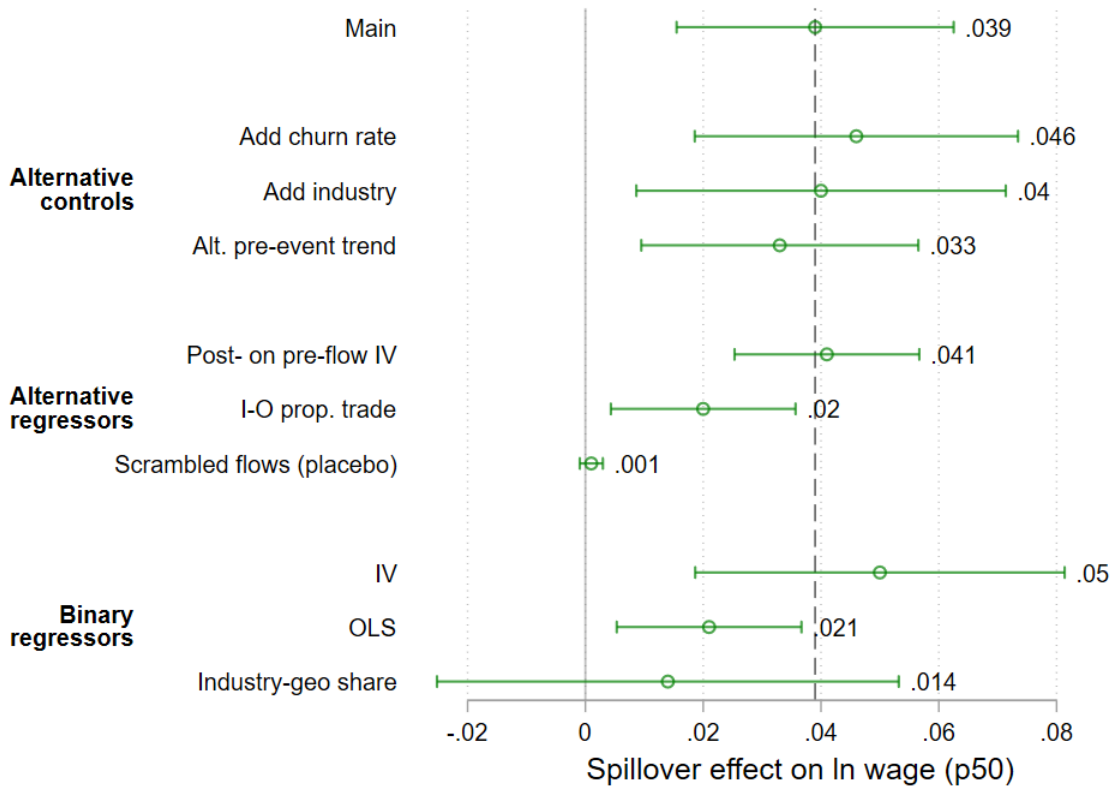
Notes. The figure shows alternative estimates from the event-study evaluating spillover effects from 47 bargaining council wage increases between 2011 and 2016 (see Equation 11). Each estimate is the final period effect on firm median wages for the relevant specification. "Main" is the main set of results. 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.

Figure 7: **Alternative estimates of two-year out wage effects on covered and uncovered firms**
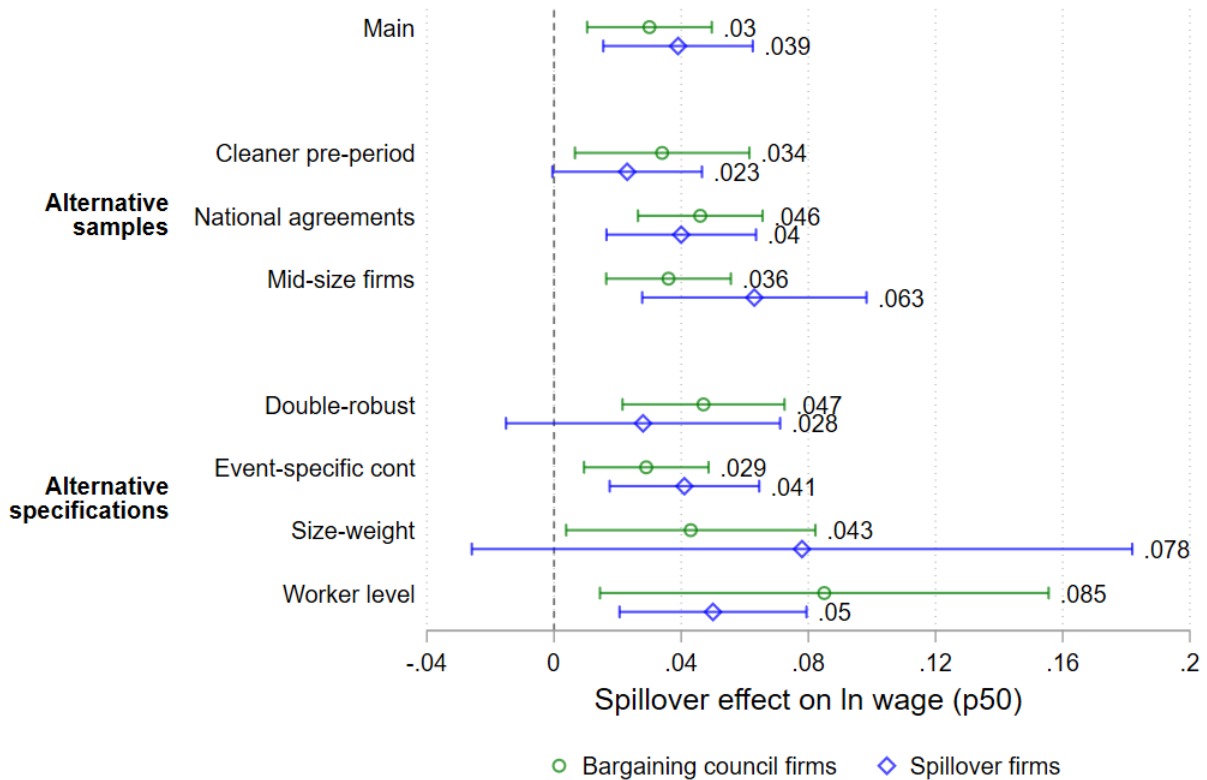
Notes. The figure shows alternative estimates from the event-study evaluating direct treatment effects on bargaining council firms (see Equation 10) as well as spillover effects on uncovered firms (see Equation 11). Each estimate is the final period effect on firm median wages for the relevant specification. "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 regressesors, restricting to workers with bottom-tercile wages in the pre-period, and controlling only for fixed effects by individuals, event-year demographics and location.

Figure 8: **Event-study effects on number of workers, by decile of value added per worker**
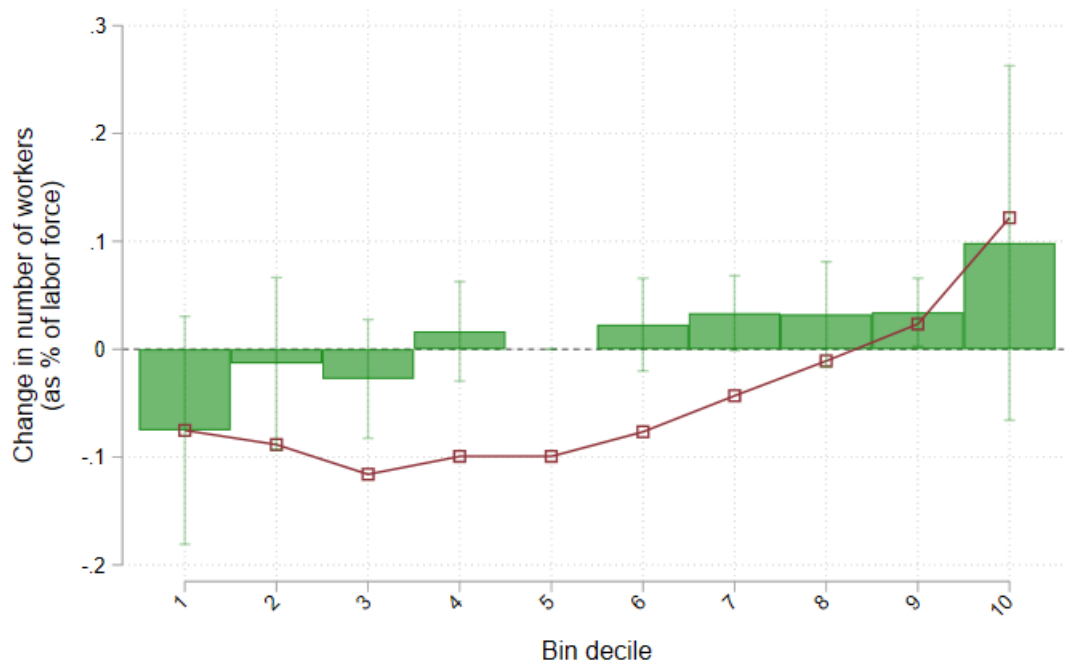
Note: Total effect .122 (SE=.128). Top vs. bottom half effect .321 (SE=.15).

Notes. The main specification and sample for effects on bargaining council firms are used (see equation 10), 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.

Figure 9: **Microsimulation of prescribed wage effects on wage distribution**
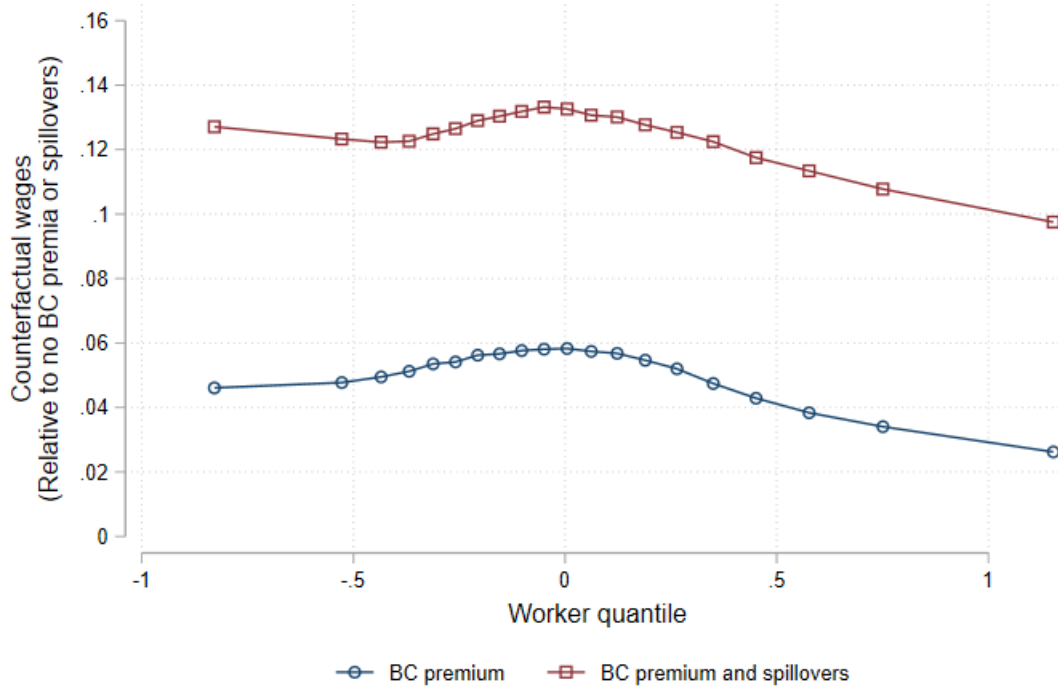
Notes. The figure simulates the wage effects of bargaining council wage agreements on the overall wage distribution, by quantile of the AKM worker fixed effect. The baseline of 0 represents the observed wage distribution. The blue line shows counterfactual wages based on adding in the bargained wage 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.

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