Collective Bargaining and Spillovers in Local Labour Markets

Ihsaan Bassier*

Click here for latest version

January 15, 2024

Abstract

How does collective bargaining affect the broader wage structure? How are such spillovers transmitted? In imperfectly competitive labour markets, a rise in wages in the covered sector can improve the outside options of workers in the noncovered sector. I use a decade of wage agreements matched with worker-level data in South Africa to study the effects of sharp changes in collectively bargained wages in an event-study framework. Observed wages in covered firms rise sharply, and within-firm wage inequality declines. I use interfirm worker flows as a measure of distance to test for spillovers, which I motivate with a model where wage changes are transmitted via outside options to nearby firms. Bilateral worker flows correlate with a wide range of firm characteristics, capturing firm links which are poorly predicted by industry and location. I show that firms with higher flows to covered firms differentially increase wages more, with an implied cross-wage elasticity of about 0.8. This is higher than comparable estimates in the literature because I am able to identify the labour market segments empirically relevant to wage spillovers. Firm profit margins decline, as predicted by the model. A micro-data simulation suggests that spillovers double the effects of collective bargaining agreements on the full wage distribution.

Keywords: Collective bargaining; Unions; Spillovers; Worker flows; Monopsony.

JEL codes: J31, J42, J51, L1

*Centre for Economic Performance (CEP), London School of Economics and Political Science (LSE), and Southern Africa Labour and Development Research Unit (SALDRU), University of Cape Town. Address: CEP, LSE, Houghton Street, London, WC2A 2AE UK. Contact: i.bassier@lse.ac.uk. Most of the research for this paper was conducted as part of my dissertation at the University of Massachusetts, Amherst. Many thanks to the UNU-WIDER SA-TIED program for data access and financial support, and to the data team at the SARS-NT Secure Data Facility. This research was also funded through an award by the Washington Center for Equitable Growth. I am especially grateful for extensive comments from Arindrajit Dube, as well as from Joshua Budlender and Suresh Naidu. This work benefited from helpful comments from Debbie Budlender, Deepankar Basu, Ellora Derenoncourt, Taryn Dinkelman, Leila Gautham, Andrew Kerr, Jasmine Kerrissey, Surbhi Kesar, Attila Lindner, Murray Leibbrandt, Alan Manning, and many others; as well as from seminars hosted by the Southern Centre for Inequality Studies, SALDRU, CEP, and the Paris School of Economics. I am grateful to Noreen Kajugusi for excellent recent assistance. All errors are my own.

1 Introduction

A classical question about unions is their impact on wages at jobs that are not covered by collective bargaining. Traditionally, this spillover impact was attributed to union threat effects or the impact of displaced workers' labour supply in the noncovered sector (Freeman and Medoff, 1981; Lewis, 1986). In this paper, I focus on a third mechanism: how changes in unionized jobs affect the outside options for workers in the noncovered sector. Using sharp changes in union collective bargaining wage agreements in South Africa, I assess wage gains for workers in covered firms as well as noncovered firms connected to them through worker flows. I present a theoretical framework of wage transmissions from covered to noncovered firms, and show that worker flows constitute a model-based empirical measure of spillovers through nearby outside options.

I collect 11 years of wage agreements across 39 collective bargaining councils in South Africa and merge this into matched employer-employee administrative data covering the universe of formal sector jobs. My stacked event-study approach compares treated firms in bargaining councils experiencing large changes in prescribed wages with matched control firms. I first find a sharp wage increase of about 4% among covered firms, which is close to the change in prescribed wages, with an accompanying strong decline of within-firm wage inequality. Lack of pre-existing trends in wages, profits, and value added suggest these are not due to profit trends confounding the collectively bargaining agreements. To further account for simultaneous shocks, I find similar effects using national industry agreements and a specification which compares responses among covered firms that are more versus less bound by the prescribed wage increase. I complement the event-studies with fixed effect approaches containing most changes in agreements.

Turning to the spillovers from these direct effects of collective bargaining, I consider noncovered firms that are more closely connected to covered firms, as measured by the worker flows between them. I show that firms with higher bilateral worker flows are geographically closer, as expected, but are also similar in 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, bilateral worker flows are typically concentrated among a few firms. They reveal workers' relevant labour market segment, or outside options, by incorporating information far beyond what the econometrician usually observes.

Using the stacked event-study, but focusing on noncovered firms, I find that noncovered firms with higher pre-event worker flows to covered firms have larger wage increases. The cross-firm wage elasticity is about 0.8. In addition, I find that profit margins and within-firm wage inequality decrease substantially. These estimates show no evidence of pre-existing trends, and are robust to alternative empirical strategies that do not rely on large wage events. My preferred estimates instrument flows in the post-period with the pre-period, and exclude firms in the immediate industry of the covered firms to guard against picking up direct effects. Estimates are similar when controlling for a rich set of time-varying fixed effects, such as for industries (addressing any correlated industry shocks), trade links (highlighting the mechanism of worker flows), or even firm observables predictive of higher flows (addressing various other correlated shocks). The wage effects appear for both incumbent and newly hired workers.

How can we understand these spillover effects? Recent work by Berger, Herkenhoff, and Mongey (2022b) models the static case of a state-wide minimum wage under monopsonistic competition. I show spillovers are more pronounced in my setting with a relatively large group of treated firms and workers. I then extend the static case to a dynamic flows-based setting, following Langella and Manning (2021), with the novel result that the spillovers are mediated by worker flows, thus theoretically motivating my empirical strategy. My framework based on outside options explains the wage transmission effects, the decline in firm profits, and why these effects are increasing in worker flows with treated firms. I discuss how the framework may be complemented by other important mechanisms such as fairness norms.

This picture of the labour market may be surprising given South Africa's high unemployment. While in other work I discuss monopsonistic competition in this context (Bassier, 2023), I show here that firms tend to hire certain types of workers, partially disregarding the large queue of unemployed in South Africa.

My study contributes two sets of insights to our understanding of cross-firm wage spillovers in an imperfectly competitive market. Firstly, I show how such spillovers may arise in the presence of collective bargaining. The question of union wage spillovers is, of course, a classic one, as seminally discussed by Freeman and Medoff (1981).[1] However, the older literature was focused mostly on perfectly competitive labour markets, while this paper focuses on what is potentially a much more direct mechanism of outside options, which arises naturally in markets with monopsony power. Moreover, the prior literature was substantially data-constrained, while I am able to revisit these questions using high-quality administrative data. There is more recent work on cross-firm wage spillovers, but this literature has not considered how spillovers are mediated by collective bargaining arrangements, especially at the sectoral level.[2] This is a striking gap in the literature given the prevalence of sectoral agreements (Bhuller et al., 2022). Significantly, Lewis (1986) commented that "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." Using a microdata simulation, I show that spillovers roughly double the effect of collective bargaining wage increases on the overall wage distribution, and affect the majority of workers.

A second contribution to the literature on spillovers is addressing the key question of how to define the relevant labour market. This is a critical question since spillovers are likely to occur in firms that are "close" in terms of inhabiting the same labour market segment recruiting from a common pool of employees. A few recent papers have used a worker flows measure to isolate

[1] Aside from union threat effects and shifts in wage norms, they also discuss demand effects that offset higher noncovered labour supply. Typically, if employment in the covered sector decreases, this would increase labour supply in the noncovered sector, which in turn would decrease wages. This may be offset if covered sector production declines, and demand for noncovered firms' products increases. This may increase wages in the noncovered sector not sharing the same geographic labour market.

[2] Fortin, Lemieux, and Lloyd (2021) consider union threat effects on the US wage distribution, using union density as a proxy; although they are unable to estimate cross-firm spillovers, the implied cross-wage elasticities are similar to those found in this paper. A few studies find cross-firm wage spillovers in other settings, such as the classic paper of Staiger, Spetz, and Phibbs (2010) in the hospital sector. Aside from cross-firm spillovers, several studies show wage spillovers from state-wide minimum wages, for example Cengiz et al. (2019), Engbom and Moser (2022), and Fortin, Lemieux, and Lloyd (2021) find shifts well into the wage distribution, and Dustmann et al. (2021) and Gopalan et al. (2021) show within-workplace spillovers.

where spillovers may occur.[3] In addition to the novel setting of collective bargaining, my paper contributes by using this flows measure to focus on the transmission of wages, and estimate the corresponding cross-firm wage elasticity.[4] Importantly, I provide a formal basis for a worker flows measure to mediate spillovers within a model which incorporates firm monopsony power, and workers' limited information about alternative jobs. I show that the interfirm worker flows measure enables precise estimation of spillovers because it isolates the empirically relevant labour market segment. This is exactly the outside options mechanism of spillovers. Indeed, my cross-wage elasticity is generally higher than other studies (e.g. 0.35 in Staiger, Spetz, and Phibbs (2010) using geographic distance), since these estimates are likely attenuated by firms which are not candidates for spillovers. Moreover, if spillovers do in fact spread through outside options, 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 design-based estimates of the effects of unions on covered firm outcomes. The union effects literature is perhaps undersized given its importance (Card and Cardoso, 2022; Fanfani, 2022; Magruder, 2012), though some do study unions as a countervailing force to employer monopsony power as I do (Dodini, Salvanes, and Willén, 2021; Dodini, Stansbury, and Willén, 2023). I show that it is important in such studies to exclude potential spillover firms from the control group, to avoid violations of the Stable Unit Treatment Value Assumption (SUTVA) required for credible difference in differences evaluations. Here I am able to identify and exclude violations from spillovers through the labour market. I also present evidence that a negotiated increase in the sectoral wage floor leads to greater growth among covered firms with higher productivity, a re-allocation effect consistent with monopsonistic

[3] Greenstone, Hornbeck, and Moretti (2010), Helm (2020), and Nimczik (2020) consider the transmission of employment shocks to nearby industries (based on worker flows). Caldwell and Harmon (2019) and Jäger et al. (2022) study how workers' information or beliefs about outside options (based on worker flows) affect job search and wages. Schubert, Stansbury, and Taska (2020) study how employer concentration affects wages in nearby occupational outside options (based on worker flows).

[4] A few papers estimate cross-wage elasticities using other measures (not worker flows), such as Derenoncourt et al. (2021) and Staiger, Spetz, and Phibbs (2010). A concurrent paper, Demir (2022), uses worker flows to study wage spillovers from the introduction of a sectoral minimum wage, but does not estimate the cross-wage elasticity. Caldwell and Danieli (2023) study the effect of outside options on wages using a concentration measure, but do not directly assess cross-wage transmission.

competition (Dustmann et al., 2021).

More broadly, these results on collective bargaining and spillovers elucidate why and when firms respond on wages, which is informative about how the labour market functions. While there is ample recent evidence that firms have wage-setting power, this literature has focused on firm rents as determinants of the wage (Card et al., 2018; Card, 2022). The spillover findings in particular show that firms adjust wages dynamically to firm-specific external pressures, but that these pressures are only relevant when workers' limited outside options are improved.

In the next section, I provide institutional context, and describe the data on bargaining council firms. I consider the direct effects on covered firms in section 3, and the spillover effects on noncovered firms in section 4. Section 5 presents a theoretical framework for the spillover effects. In section 6, I discuss the estimates in light of the model, and conclude.

2 Context and data

2.1 Institutional context of South African bargaining councils

Bargaining councils have been a central institutional feature of the South African labour market since at least 1981 when Apartheid restrictions on Black worker unionization were significantly repealed (see Bhorat, Westhuizen, and Goga (2009) and Budlender and Sadeck (2007) for overviews). A bargaining council refers to a collection of employer and employee unions from a particular industrial and geographic area which meets over workplace issues. It is established by applying to the country's Department of Labour under the main criterion that the applicant unions collectively represent 30% of employees in the covered area. Firms falling within the scope of a bargaining council are legally obliged to register with them, including those not part of the negotiating parties; the subset of firms party to the negotiation is determined as those covered by the relevant bargaining council union. As of 2018, there were 39 legally recognized private sector bargaining councils in South Africa (DoL, 2018), of which 21 are national in scope, and most are defined at the 3-digit industry level. They cover about 40% of formal sector workers, which is similar to the national union density of about 30% (note many unions operate outside of bargaining councils). I provide further details on regulatory structure and unions in Supplement F. The institutional structure is similar to much of Scandinavia and Continental Europe (Bhuller et al., 2022; Jäger, Noy, and Schoefer, 2022), with one key difference being the high level of unemployment (about a quarter of the labour force) and informality (about a third of all workers) in South Africa.

One of the most important functions of bargaining councils is to negotiate collective bargaining agreements, which set minimum wages by occupation, though supplementary wages can be establishment-specific. The collective bargaining agreements vary in period, often three years, but sometimes shorter and renewed annually or even longer. As in other industrial relations settings, there is a set procedure for negotiations between parties, beginning with consultations with members. While strikes during negotiations may occur, final agreements often preclude strikes and lockouts for the duration of the contract. Prescribed wage increases are most often indexed to inflation and specified as percentage increases applicable to all workers. Working conditions are also negotiated, though union surveys suggest wages are by far the most important item (NALEDI, 2006). After an agreement is reached, the bargaining council may apply for extension (routinely granted) by government mandate to non-party firms falling within its scope. In this paper, I focus on such agreements that have been extended, for bargaining councils with changes to existing minimum wages.[5] Finally, enforcement is monitored by the bargaining councils which employ inspectors and to which unions may report noncompliant employers.

There has been a small number of studies on collective bargaining in South Africa (e.g. Moll, 1996; Butcher and Rouse, 1999), which use household survey data to approximate bargaining council membership. A contribution of this paper is to compile a publicly available dataset classifying industries and regions as belonging to a bargaining council, with wages for each bargaining council documented separately by year. Perhaps the key prior work is by Magruder (2012), who finds negative employment effects concentrated among smaller firms (and insignificant for larger

[5]The scope of a bargaining council may be changed and new bargaining councils formed through the procedure above with government approval, and so some smaller bargaining councils have merged with others in the same industry over time.

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.[6] I find similar indications that there are employment losses at smaller firms – but, using the firm characteristics in the data, I also find employment growth at higher productivity firms, suggestive of re-allocation effects (see section 6.1). 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. There has been no prior work on spillovers of collective bargaining in South Africa.

2.2 Construction of matched panel data

I provide details on how I construct my main matched firm panel in the Data Supplement E. Here I summarize those descriptions.

I collect bargaining council agreements (published in official government gazettes) from 2008 to 2018, record the industry, location, and wage by year for each bargaining council, and match these to firms as demarcated by industry and location in the tax data. I record the annual wages in these agreements, and cross-check these against a subset provided to me by the Labour 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). Out of a total of 1595 total potential collective bargaining agreement records (11 years by 145 bargaining council clusters), I am able to match 90% with wage records.

There are many sources of measurement error in this data construction. Matching the tax data

[6] 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 discussed, it remains unclear the extent to which the endogeneity of bargaining council spatial location biases the results in Magruder (2012), a key assumption in this design.

to bargaining council agreements is imperfect since there is no direct correspondence between industry and location codes in the two sources. Secondly, since the tax data have no occupational classifications, I use prescribed wages from the "general labour" occupational category of agreements; given that prescribed percentage wage changes across occupations are typically similar, this should not severely affect my analysis of the effects of minimum wage changes. Thirdly, while my definition of the firm is at the area-establishment level, i.e. firm tax reference number unique to each reporting location, some firms may centralize their reporting to a head office or mis-report location.

Despite these measurement shortcomings, 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 in the accuracy of classification of bargaining council firms. As an added step to minimize the impact of any classification error, when I conduct my analysis of spillovers, I exclude firms from the broader industry of the bargaining council. This helps to guard against contamination from matching error, whereby mistakenly categorized firms' actual direct effects are attributed to spillovers.

Finally, my analysis is restricted to private sector bargaining councils. 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%). However, public sector balance sheet data is not recorded in the tax dataset. In addition, the theoretical mechanisms of wage spillovers that I focus on are grounded in profit-maximization—and it is unclear how much that logic applies to the public sector. For these reasons, I focus on wage determination in the private sector workforce only.

2.3 Descriptive statistics

Table 1 shows summary statistics for the cross-sectional panel of firms in columns 1 and 2, restricted to firms with more than 10 workers in the previous period. There are about 30,000 unique firms (6 million workers) covered by bargaining councils, compared to about 50,000 noncovered firms (11 millions workers). Covered and noncovered firms are similar in terms of the distribution of firm size, worker transition rates, and profit or value added per worker. However, covered firms have substantially lower within-firm wage inequality, with higher AKM firm wage premia (estimated following Abowd, Kramarz, and Margolis, 1999). The next section provides causal evidence on these higher wages in covered firms. Covered firms have a larger share of workers in the upper-middle parts of the earnings distribution, e.g. 40% in deciles 6-8 compared to 18% for noncovered firms, which underscores how different the collective bargaining setting is compared to other studies considering coverage from state-wide minimum wages.

The Data Supplement provides several sets of further descriptive statistics. Tables E2 and E3 provide descriptive statistics on the underlying worker and firm panels. Table E4 provides a detailed breakdown of the characteristics of each matched bargaining council, with substantial variation across most characteristics including the number of workers covered and the level of the minimum wage. Because collectively bargained minimum wages are set at the industry-location level, most prescribed "minimum" wages are in the middle parts of the unconditional wage distribution. Following the decomposition in Card and Cardoso (2022), figure E1 shows that bargaining council minimum wages constitute a large part of firm average wages. Minimum wages constitute the full wage in the lowest value added firms, and about half the firm wage in the highest value added firms. This reflects supplementary wage differences which indicate these firms still have wage-setting discretion (as assumed in the model section 5).

3 Direct effects of collectively bargained wages

3.1 Event-study design

I begin my empirical analysis by estimating the "direct" causal effects of prescribed minimum wages on outcomes of bargaining council firms. I follow a stacked event-study design, with careful attention to constructing a clean set of controls. In addition to being of direct interest, these wage results constitute a benchmark for comparing the spillover effects investigated in the next section.

Event sample. I define treatment events as "large" real minimum wage increases of greater than 3% as prescribed by bargaining council contracts. I exclude similarly large increases across the preceding 3 years (to ensure a "clean" pre-treatment period).[7] This yields 47 different events, with 33 unique wage increases (some bargaining councils have multiple separately bargained industry-regions). I combine these 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 A1 shows the distribution of all real bargained wage increases, concentrated just above 0, along with the subset of selected event-wage increases.

The treatment-group sample includes any firm covered by the bargaining council relevant to the agreement event, including those firms to which agreements are extended. The control-group sample contains all firms not covered by bargaining council agreements, in the same calendar year, and in the same larger region or industry as the relevant bargaining council.[8] To avoid bias from spillovers, I exclude noncovered firms that are closely linked to the treated firms in the labour market from the control group. These excluded firms are those with more than 1% of worker flows to bargaining councils from the regression sample (we return to these firms in the next section). 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 in the pre-period.

Table 1 column 3 shows that the stacked event-study sample includes over half of the full panel's covered firms and workers. Covered firms in these two datasets are very similar, though firms in the event sample have slightly higher wages. In terms of noncovered firms, table 1 column 4 describes firms with non-negligible worker flows to covered firms that are excluded from the

[7]These criteria restrict the number of events as follows. Beginning with 1348 contracted wage increases, 811 are in the period 2011 to 2016 to allow for sufficient pre and post years, of which 107 are wage increases of at least 3%, of which 47 meet the additional criteria such as clean pre-period. The 3% threshold is arbitrary, and I check sensitivity to it. The method of converting a continuous underlying variable (here, the prescribed wage changes) into discrete events with an arbitrary threshold follows prior studies, e.g. Cengiz et al. (2019) for minimum wages, Lamadon, Mogstad, and Setzler (2022) for firm productivity shocks, as well as much of the mass layoffs literature (such as Gathmann, Helm, and Schönberg, 2020).

[8]Control-group firms are taken from the same geographic province, which may be thought of as a state in the US, and from the broader one-digit industry. Recall that control-group firms from the broader industry as the covered sector are excluded to guard against classification error; for example, in the case of a 3-digit bargaining council, control-group firms come from the same 1-digit industry, but exclude firms in the same 2-digit industry.

regression sample. Column 5 describes the regression sample control firms, which compared to covered firms have similar firm size distribution and worker transition rates, though higher firm profit per worker and lower AKM firm wage premia.

Specification. My main specification includes fixed effects for each firm (ϕ_j), along with an event-specific location by time effect ($\theta_{event \times loc. \times I_t}$) capturing geographically heterogeneous shocks. In addition, the set of controls includes time effects interacted with a rich set of pre-treatment firm characteristics. These additional characteristics are pre-treatment (continuous) log firm size and log wage by year ($\gamma_{lnfirmsize_{t=-2} \times I_t}$ and $\alpha_{lnwage_{t=-2} \times I_t}$), as well as the pre-event (continuous) change in log firm size ($\beta_{\Delta lnfirmsize_{t<-1} \times I_t}$) and log mean firm wage ($\psi_{\Delta lnwage_{t<-1} \times I_t}$). All regressions are unweighted, and are estimated at the firm level. Standard errors are clustered at the level of bargaining council by event.

$$\begin{aligned} y_{j,t} \quad = \quad & \underbrace{\sum_{t=-3}^{-2} \delta_t (I_t \times treat_j)}_{\text{test of pre-trends}} + \underbrace{\sum_{t=0}^{2} \delta_t (I_t \times treat_j)}_{\text{main effects}} + \phi_j + \theta_{event \times loc. \times I_t} \\ & + \gamma_{lnfirmsize_{t=-2} \times I_t} + \alpha_{lnwage_{t=-2} \times I_t} + \beta_{\Delta lnfirmsize_{t<-1} \times I_t} + \psi_{\Delta lnwage_{t<-1} \times I_t} + e_{j,t} \end{aligned} \tag{1}$$

For intuition, identification of the main coefficients of interest δ_t arises from comparing changes in bargaining council firm outcomes to changes in similar firms (in terms of size and wage) within the same location at the same time but different industries. Note that the coefficients δ_t are normalized relative to the outcome in the period just before treatment, i.e. $t = -1$. This means that the coefficients for δ_t for $t < -1$ provide a test of pre-existing trends up to three years prior to treatment. I divide the outcomes $y_{j,t}$ by the change in log prescribed wages, such that the treatment coefficients are interpretable as elasticities with respect to the prescribed wage change. This has the added benefit that variation in the magnitude of the prescribed wage changes across events contributes to identification of the treatment effects δ_t.[9]

[9]This is equivalent to using the change in log prescribed wages (0 for noncovered firms) in place of the $treat_j$ indicator.

It is worth remarking on the importance of conditioning on a rich set of firm-level characteristics as I do here. Without such conditioning, treated firms exhibit different pre-treatment trends in firm size: while the wage event-study (which is the key topic of this paper) is not sensitive to the exclusion of these controls, the firm size event-study is (see event-studies in Supplementary figure A2). 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. 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 vary freely.

Identification concerns. An immediate concern is the endogeneity of the large prescribed wage increases. For example, such wage increases may be confounded by industry profit booms, which would upwardly bias wage and firm size effects.[10] A number of tests provide re-assurance. Firstly, within the event studies, there do not seem to be any systematic pre-trends in value added or profit margin per worker which would be suggestive of such confounders. Supplementary table A1 shows directly that the coefficients on the lagged change in wages, firm size, profits and value added are insignificant predictors of the change in prescribed wages (conditional on controls), for both the firm panel and event-study samples. While it is possible industry shocks are exactly coincident with the prescribed wage change, this seems unlikely given that agreements are negotiated during the year before and are based on firm financials before that. Secondly, I estimate a more demanding version of equation 1: I first restrict to national agreements, noting these may be less endogenous to confounding local economy shocks; then, following the main specification, I compare (within each event) regions that are bound more by the prescribed wages, i.e. lower wage areas, versus regions bound less, i.e. higher wage areas. This design has been used in the context of national minimum wages, for example by Derenoncourt and Montialoux (2021) and Cribb et al. (2021), and partials out shocks common to the event, but assumes homogeneity in treatment effects.

[10] As Moll (1996) discusses, core employers that negotiate the contracts may be more accommodating of wage increases than non-party firms to which the agreement is extended. While I focus on contracts extended to non-parties of the collective bargaining agreements, I cannot distinguish between core and non-party firms.

Another concern is the external validity of the event-study design, since the event-study sample is small compared to the full set of possible events and these firms may react differently to most firms and to large (as opposed to smaller) prescribed wage increases. One re-assurance is the sample still includes half of all covered firms (table 1); moreover, I show that estimates are very similar when propensity-score re-weighted to resemble the full panel. My event-study approach follows a large recent literature which highlights problems with classical two-way fixed effects estimators, particularly in terms of appropriate control units in settings like mine with staggered and heterogeneous treatment (e.g. Borusyak, Jaravel, and Spiess, 2023; Miller, 2023). Nevertheless, I also provide estimates using this classical approach, which has the benefit of including more events. Finally, I provide a number of other robustness checks, including on the sensitivity to the exact prescribed wage threshold used to define the event.

3.2 Event-study results

Table 2 presents the event-study estimates. The pre-period coefficients are economically small and statistically insignificant, which supports the identification assumption of parallel trends with control firms. There are economically substantial and strongly statistically significant treatment effects on wages, with an elasticity of wages to prescribed wages of 0.9 for low paid workers at firms (25th percentile), and 0.75 for median wages at firms. Within-firm wage inequality therefore declines strongly, with an elasticity of -0.57. There are no significant effects on firm size or profits.[11] Note that eventyear 0 indicates a partially treated year, as tax year records do not correspond exactly in timing to bargaining council agreements.

Figure 1 shows the event studies corresponding to these estimates. Wages have relatively flat pre trends, and a clear, sharp increase at the event-date, then they stabilize. Panel C shows that the within-firm wage gap declines strongly. Supplementary figure A3 shows further wage outcomes, with similar event-study patterns of flat pre-trends along with a sharp uptick. Annualized wages

[11] A large proportion of observations are missing for the profit variable, partly due to small firms not having to report it. Recall firms are only included if they have non-missing outcomes in all event years for the relevant regression, meaning composition effects due to endogenous reporting within firms are ruled out as a source of bias (though there may be selection).

(i.e. adjusted for fraction of the year employed) increase slightly less. New hires are also paid more after the event, and the effects are similar when looking at absolute changes in wages rather than as a percentage of the agreement increases. The decline in within-firm wage inequality is driven purely by greater wage increases for lower paid workers in firms, with little change for higher paid workers. The wage increases vary by the pre-treatment wage of the firm, with higher increases for middle-waged firms (40-70th percentiles), as well as by firm size, with larger effects for mid-sized firms.[12]

Figure 1 panel D shows some tendency for firm size to decline, but this is not significant. The own wage elasticity, i.e. the firm size effect divided by the wage effect, is -0.3 (standard error of 0.26), within the range found elsewhere for minimum wages (Dube, 2019). However, in Supplementary figure A4 panels A and B there is some evidence of an increase in firm exit (though with pre-treatment effects) and no corresponding change in firm entry, which is consistent with some low productivity firms being forced out by the higher minimum wage. Insofar as there is an increase in firm exit, all results are therefore conditional on survival.[13] I show in section 6.1 that firm employment effects vary by firm productivity. The Supplementary figure also shows average separations decrease sharply at the event year with an elasticity of -0.5 (panel C), which suggests a continued relevance of "worker exit" amid the bargaining contract's incorporation of "worker voice".[14] Unemployment insurance payments increase strongly (panel D), though this is partly mechanical as UI payments are a percentage of wages subject to a ceiling. The pre-period coefficients on value added and the profit margin per worker are close to zero (panels E and

[12]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.

[13]For example, $\partial E[ln(emp)]/\partial ln(w) = \partial E[lnemp|s=1]/\partial ln(w) * pr(s=1) + E(lnemp|s=1) * \partial pr(s=1)/dw$, such that from the data $\partial E[ln(emp)]/\partial ln(w) = -0.23 * 0.0185 + 3.385 * -.2 = -.68$ which is a high negative employment effect, though again the firm exit event study does not appear credible.

[14]Interpreted directly, this implies a firm labour supply elasticity of about 1 which suggests considerable monopsony power in line with Bassier (2023). However, this is very likely biased as an estimate of a reduction in turnover since I cannot differentiate between voluntary quits and involuntary fires. And importantly, this is a response to a wage increase at a set of firms (and not a single firm) in the local labour market, making it an imperfect comparison to the response to a firm-level change needed for estimating the firm labour supply elasticity.

F), which is re-assurance that these estimates do not carry substantial bias from endogeneity of bargaining agreements to prior firm performance.

3.3 Robustness

Figure 2 presents various robustness checks on the main event-study wage estimates above (shown in blue; I return later to the spillover estimate checks, shown in green). I begin with alternative samples. The estimates are similar when (a) restricting to events without large prior increases in wage agreements, which otherwise may bias coefficients due to lagged dynamic effects; and (b) testing sensitivity to the exact threshold in prescribed wage changes used for the event definition (3%), by using a slightly higher threshold (3.5%). I also restrict to firms with a higher proportion of lower-paid workers who are more likely bound by the prescribed wage increases; as expected, this gives a higher elasticity of wages to prescribed wage agreements.

The approach based on national agreements gives further confidence that these results are not driven by confounding industry shocks. The average wage effect for these national events is slightly higher. Comparing regions more bound by the prescribed wages to regions less bound within the same collective bargaining agreement (thereby partialling out possible industry-wide shocks), the estimated wage effect is slightly higher than the main specification with an elasticity of 0.99. Supplementary figure A5 shows the corresponding event-studies for the 25th and 50th percentiles of wages in firms, separately for high- and low-wage regions (the difference is the treatment effect).

In terms of alternative specifications, results are similar across three different types of weightings. Firstly, weighting by firm size, the confidence intervals are larger but the point estimate is similar. Secondly, using the double-robust propensity score weighting, which follows Arkhangelsky and Imbens (2022) who show that effects are identified if either the assignment model (propensity score weighting) or the outcome model (regression approach) are correct – in this case, confidence intervals are again larger but still significant with similar point estimates. And thirdly, re-weighting using a propensity score for whether a covered firm is in the event-study sample (based on a rich set of characteristics including industry and location), such that estimates are more representative of the full firm panel. The last row of figure 2 indicates a specification that interacts all controls with separate event indicators, which is just a much richer set of controls, again with very similar results.

Supplementary table C1 shows the other outcomes for these alternative samples and specifications. In a few variations, firm size is significantly negative, such as when restricting to firms with high proportions of low-wage workers, or comparing within bargaining councils. As I show in section 6.1, the firm size effect is heterogeneous in firm productivity, which may be suggestive of re-allocation effects. Profits are also negative in a few specifications, such as when weighted by employment or using the double robust propensity score weights.

Finally, I check the extent to which the pre-period coefficients preclude pre-trends by following the "honest pre-trends" sensitivity checks from Rambachan and Roth (2019); Supplementary figure C1 shows the treatment effects are robust against even a pre-trend that has an 80% probability of being rejected. One may also be concerned about Nickell bias for the outcome of wages given pre-treatment wages is a regressor, but I find very similar results with a split sample correction following Chen, Chernozhukov, and Fernández-Val (2019).[15]

As discussed above, a very different approach from my preferred event-study specification is a classic two-way fixed effects estimator on the full firm panel. For this approach, I follow much of the preferred specification 1 above, but include any prescribed wage changes and do not require clean pre-periods.[16] Consequently, more changes in prescribed wages are admitted (296 versus 47), but these estimates are biased by well-known issues of invalid control group comparisons as discussed above. Supplementary table A2 shows the results. The lagged effects are not significant, and have much smaller magnitudes than the main effects, which is similar to the pre-treatment coefficient test in event-studies and provides some re-assurance. The main effects

[15] In any case, the bias incurred for the non-lag coefficient is small in simulations shown by Hausman and Pinkovskiy (2017), except in cases of extremely high auto-correlation.

[16] Specifically, the sample is restricted to firms with greater than 10 workers in the lagged period, and non-missing outcomes in surrounding periods. I implement equation 1 on this sample by following the linear projections approach of Dube et al. (2022), which is numerically equivalent to the levels approach in a stacked event data structure (making these more comparable). The main regressor is the change in logged prescribed wage. The controls are fixed effects for location, as well as lagged log wage, lagged log firm size, lagged growth in log wage, and lagged growth in firm size.

on wages are strongly significant, with a larger elasticity of wages to prescribed wages for 25th percentile workers in the firm (0.32) compared to median workers (0.22). However, these wage effects are smaller than the event-study elasticities, perhaps unsurprisingly given that (invalid) comparisons with firms experiencing dynamic lagged effects would tend to attenuate the estimates. The lower elasticity could also reflect the smaller prescribed wage changes, for example if these are less enforced. The estimates for firm size and profits are negative and significant.

In sum, the evidence on wage responses indicates a positive causal effect of bargained minimums on wages, which is expected though not clear in other settings (Blandhol et al., 2020; DiNardo and Lee, 2004). Results based on alternative event-study samples and specifications, as well as on a firm panel approach (at least qualitatively), are consistent with the preferred event-study estimates. There are significantly negative effects on within-firm wage inequality, though less clear (sometimes negative) effects on firm size and profits.

4 Spillover effects of collectively bargained wages

4.1 Descriptives of worker flows

In order to measure spillovers from collectively bargained wages, I need a concept of distance from covered firms. I use the proportion of worker flows between each noncovered firm and the relevant set of covered firms as this measure of distance. Intuitively, if the same workers are employable at different firms, these firms define viable outside options. This mechanism is in line with previous work (e.g. Greenstone, Hornbeck, and Moretti, 2010; Helm, 2020), and I show in section 5 that it can be formally motivated in a dynamic model of imperfect competition.

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 bilateral "distance" in several firm characteristics such as spatial distance or firm wages, while including firm fixed effects so that the relationships are relative within firms. I find strongly 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 B1). Indeed, Supplementary figure B2 shows that although sharing the same industry or location are important predictors of flows between firms, simultaneously sharing many other firm characteristics such as size, AKM wage premia, and the proportion of women are comparably important. The figure is very similar considering firm separations or hires rather than gross worker flows, and these predictors explain about half of all firm-to-firm flows.[17]

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 avoids subjective judgements on where to draw an arbitrary industry or geographic boundary around the labour market, and therefore it enables more precise identification than previous work on spillovers. Following a similar argument, several recent papers have used worker flows to define labour markets (Arnold, 2020; Jarosch, Nimcsik, and Sorkin, 2019; Schubert, Stansbury, and Taska, 2020). Put another way, only 10% of firms in the top decile of worker flows to bargaining council firms share the same location and industry, meaning that such an alternative concept of distance would introduce substantial noise into spillover estimates.

[17]Figure B2 panel B compares the predictive power of different firm characteristics to classify spillover firms against the metric of worker flows. Using just location or location and industry (e.g. Berger, Herkenhoff, and Mongey, 2022b) are poor predictors of worker flows: the probability that a firm sharing industry/location actually has high flows (precision) is low, leading to attenuation bias; and the coverage of firms with high flows (recall) is low, making the estimates less representative of the affected sample. Using more predictors increases both precision and recall considerably. Using flows directly as I do implies a horizontal line at 100% precision for any level of recall.

4.2 Event-study design

Event sample. I use the same stacked event-study sample described in section 3.1 above, but crucially exclude covered firms and instead focus on the candidate spillover firms, i.e. noncovered firms with high worker flows to covered firms. To avoid contamination, I take a conservative approach against mismeasurement of bargaining council coverage by excluding all firms that are in a similar industry to the bargaining council.[18] I also restrict focus to the bargaining council affected by the event by excluding a small number of noncovered firms with high connectivity to multiple bargaining councils. Table 1 columns 4 and 5 describe this regression sample, with 11,000 candidate spillover firms and 32,000 unconnected firms with negligible worker flows (together constituting 80% of all noncovered firms). The characteristics of candidate spillover firms are typically between the covered and unconnected firms, except they have a larger mass of workers with below-median earnings.

Specification. I follow much of equation 1 used for bargaining council effects, except I replace the main variable of interest (previously the bargaining council treatment indicators) by the estimated worker flows $\hat{F}_{j(c)}$ for every firm $j(c)$ where c is the most detailed industry-location cell. That is, for each industry-location cell, I take the average proportion of worker flows between each noncovered firm and the relevant covered firms in the event-study pre-treatment period.[19] For interpretability as a benchmark, I divide these flows by the top decile of the highest flow firms (the average flows for this top decile is about 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), Chaisemartin et al. (2022), and Dube et al. (2022).

[18]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.

[19]Note that in the model of section 5 below, firm pre-period flows F_j is one sample realization of the latent attractiveness of treated 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.

$$
\begin{aligned}
y_{j,t} \quad = \quad & \underbrace{\sum_{t=-3}^{-2} \delta_t (I_t \times \hat{F}_{j(c)})}_{\text{test of pre-trends}} + \underbrace{\sum_{t=0}^{2} \delta_t (I_t \times \hat{F}_{j(c)})}_{\text{main effects}} + \phi_j + \theta_{event \times loc. \times I_t} \\
& + \gamma_{lnfirmsize_{t=-2} \times I_t} + \alpha_{lnwage_{t=-2} \times I_t} + \beta_{\Delta lnfirmsize_{t<-1} \times I_t} + \psi_{\Delta lnwage_{t<-1} \times I_t} + e_{j,t} \qquad (2)
\end{aligned}
$$

Identification now arises from variation in pre-treatment connectivity: comparing noncovered firms of varying degrees of connectivity to covered firms, but within the same location and of similar firm size. That is, do noncovered firms that are more strongly connected to treated covered firms exhibit stronger outcome responses to the prescribed wage events? The main outcomes $y_{j,t}$ are the 50th and 25th percentiles of log within-firm wages, log firm size, and log firm profit margin. Each of these outcomes is divided by the relevant bargaining council prescribed wage increase, such that the coefficients δ_t are interpreted as spillover elasticities. As in section 3.1, this has the additional benefit of using variation in the magnitude of the prescribed wage changes across events for identification.

In practice, the regressor regressor $\hat{F}_{j(c)}$ in equation 2 is subject to measurement error. I therefore instrument the average post-period flows with the average pre-period flows (first stage coefficient of 0.77, standard error of 0.0005). Measurement in the pre-period also helps ensure the flows are not endogenous to the treatment effects. For computational speed, I implement equation 2 by following the linear projections approach of Dube et al. (2022), which is numerically equivalent to the levels approach in the stacked event design.[20]

Identification concerns. The concerns regarding the direct effects in section 3.1 above apply to some extent, and I implement the same robustness checks: to address endogeneity, checking pre-treatment trends on value added and profit margin per worker, and comparing within-bargaining council high vs low wage regions; and to address external validity, reweighting the sample and using the classical panel fixed effects approach.

[20]Compared to equation 2, separate regressions are run for each event year, and outcomes are in changes relative to event year -1. The regressor is just the instrumented flows variable $\hat{F}_{j(c)}$, and there is no firm fixed effect since outcomes are in changes. I check in the data that the specifications are numerically equivalent.

However, as mentioned, endogeneity may be less of a concern for noncovered firms given that noncovered firms are never party to negotiations over collective bargaining agreements. Moreover, recall that firms from the same broad industry as the bargaining council are already excluded from the sample, mitigating the risk of common industry shocks. In fact, because the main regressor of flows varies by detailed industry-region, as robustness I include a rich set of time-varying fixed effects in equation 2 to address a range of confounding shocks. For example, industry effects ensure that identification is from the extent of worker flows within each industry.[21]

4.3 Event-study results

Table 3 presents the event-study estimates. The pre-period coefficients are economically small and statistically insignificant, which supports the identification assumption of parallel trends between firms with high and low firms to bargaining councils. The cross-firm wage elasticity is between 0.81 and 0.87. This high cross-firm elasticity may be surprising, suggesting a similar elasticity to prescribed wage changes as for covered firms themselves. However, this applies to a very small fraction of firms with high worker flows; more generally, for example, firms in locations with more than 10% covered firms have on average half as many flows to covered firms, implying a cross-firm wage elasticity of about 0.4.[22] I return to the question of what magnitude we may expect for the cross-wage elasticity in the model section 5. Table 3 also shows that within-firm wage inequality decreases strongly. The point estimate on firm size is negative though not significant, and the

[21]Note that there is no claim that these flows are exogenous; indeed, flows are shown in the section above to be systematically related to firm observables, such as prior wages, industry and location. The identification instead relies on the event-study timing, the magnitude of the increase, and the interaction with this flows measure. Where flows are fully persistent, fixed characteristics are directly accounted for by firm fixed effects; any potential lagged effects causing bias are considered in the robustness check excluding events with substantial prior wage increases.

[22]Another consideration is that spillover firms in the estimation sample have on average 10% lower wages than covered firms, implying that the absolute amount is smaller (i.e. a R1,000 increase in agreements translates to about a R750 increase for spillover firms). Finally, such lower-wage spillover firms are more likely connected to covered firms more bound by prescribed wages, with a higher corresponding direct effect (see figure 2 "low-wage" specification). A puzzle is that given these spillovers are relative to firms with flows of about 0.1 to covered firms as described above, this may imply implausibly large spillovers for firms with much higher flows. It is worth emphasizing that (a) very few firms have such high flows to bargaining councils, and (c) a linear extrapolation to very high flow firms would likely not be appropriate.

estimate on firm profits is economically large and strongly significant.

Figure 3 shows the event-studies of firm median wage effects for the specifications based on OLS, the main specification, and the binary regressor. All show flat pre-trends, with wages rising in the two years after the prescribed wage increase. The estimates are similar, with the OLS estimate lower likely due to measurement error. As in the case of directly covered firms, panel D shows how the two-year-out wage spillovers are lower for higher quantiles of within-firm wages. Consequently, there is a sharp decrease in the p80-p20 within-firm wage gap, consistent with earlier studies on union spillovers (Freeman, 1982; Kahn and Curme, 1987). Supplementary figure B3 shows that similar wage spillovers appear for both incumbent workers and new hires, indicating that these spillovers are not purely driven by individual-level bargaining effects for new hires.

Figure 4 shows other outcomes. Firm size shows no statistically detectable change, with a negative point estimate and an own wage elasticity, i.e. the firm size effect divided by the wage effect, of -0.17 (standard error of 0.27). This is the same as 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 -0.7 or lower (Dube, 2019). Once again, the event-study of the profit margin does not exhibit pre-trends, which is reassurance against differential prior firm performance driving these results. There is a clear post-period decline in profit margin for firms, which highlights a potentially sharp trade-off between profits and wages for spillover firms, as found in an earlier study (Bronars and Deere, 1994).[23] Supplementary figure B4 shows event studies of worker transitions. There are no clear effects on hires from covered firms or separations to covered firms. On the other hand, consistent with the increase in wages, hires from other firms (poaching) increase, and hires from non-employment decrease.

The main regressor implicitly tests for the linear relationship between firm responses and the pre-event flows or connectivity to bargaining council firms. Supplementary figure B5 shows this

[23] 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%.

directly, where spillover wage effects are gradually increasing by quantile of flow connectivity, such that wage changes rise with connectivity (whereas for firm employment, coefficients are close to zero for all quantiles). 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.

4.4 Robustness

Moving to robustness on the event-study estimates, figure 2 shows the wage estimates are robust to the same checks as for the estimated direct effects on covered firms. In terms of alternative samples, the estimates are similar when restricting to events without large prior wage increases (in case estimates are contaminated by lagged dynamics effects), and when using a higher prescribed wage change threshold to test for sensitivity to the event definition. As expected, estimates are higher when restricting to firms with more low-paid workers. For the approach using national agreements, estimates are similar for this set of events, and then estimates are larger (though standard errors include the main estimate) when comparing high- to low-wage regions, perhaps because linked covered firms are all bound strongly by the prescribed wage (event studies in Supplementary figure B6). Estimates are similar for the different weightings: weighting by firm size, using the double-robust propensity score weighting, and re-weighting to be representative of the panel. As before, Supplementary figure C1 shows the effects are robust against a pre-treatment trend that has an 80% probability of being rejected; the split sample correction for Nickell bias also gives similar results. Supplementary table C1 provides the corresponding estimates for other firm outcomes, with substantial negative effects on profits in most specifications and significant negative effects on firm size in only a few cases.

There are a number of concerns more specific to these spillover estimates, with robustness checks presented in figure 5. As discussed, the specification allows for a rich set of time-varying fixed effect controls, and estimates are similar when including controls for industry, aggregate firm worker flows, and average bargaining council network characteristics, each of which may be possible confounders. More generally, one may be concerned that firms with high worker flows to covered firms may experience similar confounding factors as covered firms (whether industry or something else), so I include time-varying controls for the predicted flow of workers based on firm observables. This is a demanding specification, since it removes much of the identifying variation such that the economic distance is purely due to unobserved idiosyncratic factors; the resulting confidence interval is therefore wider, but the point estimate is just a bit lower than the main specification.

Figure 5 also shows that estimates are robust to alternative regressors: the estimate is very similar using a split-sample instrument, following Goldschmidt and Schmieder (2017) (average pre-period flows from firms randomly split into two groups), instead of the post-treatment flows instrument; or just directly using OLS with pre-treatment flows as the regressor, giving a lower estimate as expected since this does not correct for the measurement error in the generated regressor. Using trade links from national industry input-output tables does give a positive and statistically significant spillover, but the estimate is lower at about 0.36. In Supplementary table B1 I show that the main estimates are robust to time-varying control for these trade links, which addresses the concern that the actual underlying mechanism is the product market linkages; Helm (2020) and Greenstone, Hornbeck, and Moretti (2010) do a similar test in the setting of agglomeration spillovers, and also find flows are most relevant. Finally, I implement a placebo check on the specification by randomly re-assigning the regressor $\hat{F}_{j(c)}$ to firms. The effect on the placebo regressor is reassuringly close to zero. Supplementary table B1 shows the effects on firm size and profit margins are in line with the main estimates, with significant negative effects on firm size in a few cases and significant negative effects on profits in almost all cases.

Aside from these robustness checks, a simple sanity-check on the size of the cross-wage elasticity is to recall that these spillover firms were excluded from the regression sample for direct wage effects in section 3, because they are contaminated controls. If instead I include these controls, the estimated direct wage effect attenuates from around 4% to 3%. This reduction implies an indirect estimate of spillovers of about 1.6%. This rough exercise yields a cross-wage elasticity not too far from the preferred estimate above.

Finally, as in the case of covered firms, one approach to address concerns with external validity is to estimate a similar specification using classical two-way fixed effects on the firm panel. Recall that this approach is biased by invalid control group comparisons, but includes a larger proportion of the spillover firms. For each noncovered firm j, I estimate the relevant bargaining council wage as the average annual prescribed wage of bargaining council firms, weighted by the proportion of worker flows between j and each bargaining council. The spillover effects are therefore identified from changes in the flow-weighted magnitude of prescribed covered sector wages. Supplementary table B2 shows the estimates based on the firm panel. The lagged coefficients are all insignificant, which is reassuring. The main effects on wages are significant and positive, with a larger effect on lower percentile workers as expected. Given that the covered wage effects from the panel approach were substantially smaller than 1, I divide through by the relevant covered firm wage change in table A2 to find cross-wage elasticities of 0.59 for the 25th percentile, and 0.66 for the median percentile. These are close to the event-study estimates above, especially the comparable OLS estimate (figure 3 panel A). There are significantly negative effects on within-firm wage inequality, firm size, and firm profits margins.

In sum, these results provide strong evidence that spillovers exist, that they operate through local labour market networks of worker flows, and that they are substantial in magnitude for "nearby" firms. The preferred event-study results are robust to alternative event-study samples, specifications and controls, as well as an approach based on the larger sample of prescribed wage changes from the firm panel.

5 Theoretical framework

What is the mechanism through which we can theoretically understand the spillover results above? The key findings to explain are the increase in wages, decline in profits, and perhaps a decline in firm size. Crucially, these effects are proportional to the flow of workers with covered firms.

Models of monopsonistic competition offer a promising direction, with recent work by Berger, Herkenhoff, and Mongey (2022b) modeling the static case of a state-wide minimum wage, and so I begin by presenting a static model illustrating such spillovers in my setting of firms covered by collective bargaining. This is important because, as Berger, Herkenhoff, and Mongey (2022a) point out, spillovers are small in the state-wide minimum wage case where targeted workers are a relatively small low-wage group. I show spillovers are much larger when a substantial proportion of firms are treated.

However, the static model does not deliver the crucial relationship between spillovers and worker flows with covered firms. I therefore extend the static logit to a dynamic flows-based setting, by assuming a proportion of workers can switch firms every period as in Langella and Manning (2021). Workers switch after receiving job offers from a firm-specific labour market segment or "consideration set". This approach dispenses with arbitrary assumptions of industrial or regional boundaries, and is more in line with the flows evidence presented above in section 4.1. A novel result of this dynamic model is that the spillovers are indeed mediated by such worker flows, which theoretically motivates my empirical strategy in section 4.2. This result also helps motivate several recent papers using worker flows to define outside options (e.g. Caldwell and Harmon, 2019; Jäger et al., 2022; Manning and Petrongolo, 2017), to define labour markets (Arnold, 2020; Jarosch, Nimcsik, and Sorkin, 2019; Felix, 2021; Schubert, Stansbury, and Taska, 2020), and to measure spillovers in other settings (Greenstone, Hornbeck, and Moretti, 2010; Helm, 2020; Nimczik, 2020).

The contributions of this model are thus firstly to illustrate how spillovers may occur in my setting, and secondly to show the link between worker flows with treated firms and spillover responses. In section 6.1 I return to the question of how well this model explains my findings above.

5.1 Static logit

I assume workers have heterogeneous preferences, and 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, 2022; McFadden et al., 1973).[24] Let the utility of workers be expressed as $V_{ij} = \beta ln(w_j) + \theta ln(w_i) + \nu_{ij}$ for firm j and worker i, where w_j is firm j's wage policy, β parameterizes the responsiveness of worker utility to the firm wage, w_i is a worker-specific log-additive wage (which drops out below), and ν_{ij} follows a Gumbel distribution indicating idiosyncratic preferences for firm j of worker i. The corresponding probability a worker is employed at firm j, or equivalently the firm share of j, is in log terms $lnp_j = \beta ln(w_j) - ln(\sum_l^J w_l^\beta)$, for the set J of firms in the firm's relevant labour market.[25] The elasticity of firm j's labour supply to its own wage is $\varepsilon_{jj}^n = \frac{\partial lnp_j}{\partial lnw_j} = \beta(1-p_j) > 0$, which is finite for any finite β as is central to any model of monopsonistic competition.

Spillovers arise because firms are affected by the actions of other firms. Firm j's employment decreases when another firm k raises its wage, with $\varepsilon_{jk}^n = \frac{\partial lnp_j}{\partial lnw_k} = -\beta p_k < 0$. If a set of firms $k \in K$ are treated by a prescribed minimum wage such that they raise their wages, firm $j \notin K$ must trade off raising its own wages or losing workers to these competitors. Along this trade-off, the optimal wage response of firm j is pinned down by its wage setting function.

Firm j sets wages 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 A_j is a firm-specific productivity factor, η is the slope of the marginal revenue product of labour curve, N is the aggregate labour supply constraint, and $p_j(w_j)$ is the firm share as above. This

[24]The evidence in section 4.1, that multiple characteristics affect firm flows, supports the use of a model where non-wage characteristics are relevant for worker utility.

[25]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 generating spillovers. The plausibility of non-atomistic competition is motivated by my discussion on consideration sets below, as well as evidence on market concentration (e.g. Azar, Marinescu, and Steinbaum, 2022). The relevant labour market may for example be all firms in the commuting zone, or (as discussed later) firms relevant to workers' outside options.

yields the optimal own-wage w_j (3), and the cross-firm wage elasticity ε^w_{jk} (4) with respect to a wage increase in firm k:

$$lnw_j = lnA_j - \eta ln(p_j N) + ln(\frac{\varepsilon^n_{jj}}{1+\varepsilon^n_{jj}}) \tag{3}$$

$$\varepsilon^w_{jk} = \frac{dlnw_j}{dlnw_k} = \underbrace{\frac{1+\eta\beta}{1+\eta\beta p_k}}_{\text{multiplier}}(\underbrace{\frac{\eta\beta p_k}{1+\eta\beta}}_{\text{shift in Residual LS}} + \underbrace{\frac{\beta p_k \beta p_j}{\varepsilon^n_{jj}(1+\varepsilon^n_{jj})(1+\eta\beta)}}_{\text{change due to effect on } \varepsilon^n_{jj}}) > 0 \tag{4}$$

Diagrammatic representation. The setup above can be represented by the usual diagram for wage-setting of monopsonistic firms (figure 6). The Residual Labour Supply (LS) curve is given by $lnw^{LS}_j = 1/\beta(ln(p_j) + ln(\sum^J_l w^\beta_l))$, with the corresponding Marginal Cost of Labour (MCL) curve equal to the Residual LS curve plus the markdown $ln(\frac{1+\varepsilon}{\varepsilon})$. Panel A shows the initial effect on a supply-constrained firm k treated by an incremental wage floor, which increases k's wage and employment, as is well-known (e.g. Manning, 2003). Since the increase in firm k's wage increases the term $ln(\sum w^\beta_l)$, panel B shows the shift up in untreated firm j's Residual LS curve, and correspondingly in its MCL curve. This represents the increase in outside options for workers at firm j. There is a small second-order effect on ε^n_{jj}, and there are also second-round or multiplier effects as firms respond to adjustments of other firms. The cumulative increase in firm j's wage is given by Equation 4, and employment decreases.[26]

Treated firms off their supply curves. I implicitly assume above that firms are supply constrained, in particular that an increase in wages increases employment share. There is growing empirical support for this in the monopsony literature (Card, 2022; Manning, 2021). However, if the treated firm k is off its supply curve, as for demand-constrained firms, then its employment

[26]The more general expression representing the two channels for the cross-wage elasticity is $\varepsilon^w_{jk} = -\eta\frac{dlnp_j}{dlnw_k} + \frac{d\varepsilon_{jj}/dlnw_k}{\varepsilon_{jj}(1+\varepsilon_{jj})}$. The first term represents the upward shift of the Residual Labour Supply curve, and the second term represents the decrease in the markdown. The markdown decreases because the firm labour supply elasticity ε^n_{jj} increases, which correspondingly decreases the term $ln(\frac{\varepsilon^n_{jj}}{1+\varepsilon^n_{jj}})$; this indicates that the gap between the new Residual LS and MCL curves *narrows*, and so the wage increases. This decrease in the markdown will tend to dampen the employment decline. The figure also highlights that *some* inelastic product demand elasticity is necessary for a sizable cross-wage elasticity.

response will be lower, and the cross-wage elasticity for firm j will be smaller; see Supplement D.1 for this more general analysis of spillovers. In the case of a negative net employment effect on the treated firms, wage spillovers may still be positive if there is queuing for rationed jobs. In Supplement D.1.2, I show this extension of the model in the spirit of Harris and Todaro (1970), where high wages in treated firms incentivize workers to queue – at risk of unemployment – which increases labour *allocated* to treated firms, in turn reducing the residual labour supply to spillover firms. Alternatively, the monopsony search model of Burdett and Mortensen (1998) can similarly deliver positive wage spillovers with no increase in covered sector employment if one assumes the covered sector is towards the top of the relevant wage distribution (see Supplement D.1.3). With this in mind, I continue below to assume firms are supply-constrained for simplicity.

Comparative statics. I assume $\beta > 0$ and $\eta > 0$, which correspond to a positive labour supply curve and negative marginal revenue curve respectively in figure 6, and imply $\varepsilon^w_{jk} > 0$ in equation 4. Using the parameter values $\beta = 6$, $p_k = 0.5$, $p_j = 0.1$, and $\eta = 0.8$ as a benchmark, the cross-wage elasticity is $\varepsilon^w_{jk} = 0.72$.[27] This suggests that in response to firm k raising its wages by 4%, firm j increases its wage by 3%. The cross-wage elasticity is very similar when the spillover firm's share is negligible, e.g. $p_j = 0.001$ implies $\varepsilon^w_{jk} = 0.71$; or when the change due to the effect on ε^n_{jj} is ignored, i.e. without this term in equation 4, $\varepsilon^w_{jk} = 0.71$. The treated firm share is much more salient: a decrease of p_k from 0.5 to 0.1 decreases the cross-wage elasticity by half to $\varepsilon^w_{jk} = 0.33$. If both firm shares are small, $\varepsilon^w_{jk} \to 0$, though with higher β (noting k is supply-constrained), $\varepsilon^w_{jk} \to 1$.

Profits. As wage-setters, the increase in w_k on firm k's profits π_k are only second-order by the envelope theorem, since adjustments in employment mitigate the first order effects. 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 treated firms. Note that insofar as labour

[27] These parameter choices are motivated as illustrative benchmarks, and then varied below. $\beta = 6$ is within best-practice range of experimental estimates, the shares represent a labour market with a large bargaining council sector, and η is in the range used in other texts (e.g. Manning, 2003). Finding precise estimates of each of these parameters is out of scope of the paper.

is inflexible and so cannot adjust optimally, for both covered and noncovered firms the increase in wages will negatively bite into profits.

Model sensitivity. The large cross-wage elasticity is robust to alternative parametrizations (see Supplement D.1.3). Market-level employment responses to the average wage, which may be important in high-unemployment South Africa, can be accounted for much like in the nested logit formulation of Berger, Herkenhoff, and Mongey (2022b); this dampens the spillover responses as it relieves pressure from the aggregate labour supply constraint. If there are fair wage considerations, the same spillover mechanism applies, and may even be bolstered if the wage norm for non-treated firms shifts with the new treated firm wage. The cross-wage elasticity is similar for alternative functional forms, and the same when including additional non-wage utility factors for workers. One can rationalize positive wage spillovers in alternative models too, such as for firms along a unit circle as in Bhaskar, Manning, and To (2002), the classic monopsony search model of Burdett and Mortensen (1998), or individual bargaining with renegotiation (Caldwell and Harmon, 2019).

5.2 Dynamic logit

The static logit can be adapted to a dynamic setting with worker flows. I extend existing models (see Caldwell, Dube, and Naidu, forthcoming; Langella and Manning, 2021) by focusing on the cross-firm wage elasticity and its connection with worker flows. My purpose is to show firstly that the spillovers result above holds in the dynamic case, and secondly that these spillovers are proportional to a firm's worker flows with treated firms.

Intuition of flows result. If workers draw fresh idiosyncratic preferences every period, and have no stickiness in staying at their current firm, then each period is just a repeated draw of the static model above. Equation 4 applies; note that the cross-wage elasticity is proportional to the share allocated to the treated firm, p_k. The share p_k relevant to firm j can be decomposed into quits and hires between firms j and k, and estimated as their sample measures. Then the cross-wage elasticity is proportional to the average bilateral hire and quit flows with firm k, as a share of firm

j's flows:

$$\varepsilon_{jk}^{w} \propto h_k + q_k = \frac{h_j h_k}{h_j} + \frac{q_j q_k}{q_j} \tag{5}$$

I next go through this logic more carefully, while relaxing the assumption of no stickiness, and incorporating forward-looking behaviour.

Consideration sets. Before proceeding, I briefly note that the share p_k "relevant to firm j" allows for a firm-specific consideration set of workers' outside options. While it is not necessary for the model, it helps motivate why the treated firm shares p_k can be large and thereby generate large spillovers; it is also consistent with my descriptive evidence on inter-firm worker flows above which suggests hires and quits tend to be more firm-specific. An analogous issue exists for product markets, discussed for example by Eliaz and Spiegler (2011) and Matějka and McKay (2015), where it is assumed most products are ignored due to rational inattention, search costs, and constraints. Following much of this literature, I assume these consideration sets are exogenous (see Supplement D.2.1 for discussion), which is also supported by the evidence in Lopez, Guerrero, and Axtell (2015) that bilateral worker flow networks are persistent.

Dynamic quit and hire elasticities. To incorporate dynamics into the static model above, the key additional feature 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 ν_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).[28]

Following much of Langella and Manning (2021), quit choices follow a logit over firm values. Leaving details to Supplement D.2.2, the elasticity of quits from firm j to wages of firm k is then:

$$\varepsilon_{jk}^{q} = \frac{\lambda(\beta/2)\tilde{q}_j\tilde{q}_k}{q_j(1-\rho_w(1-q_k'))} \tag{6}$$

[28] 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.

Where $\tilde{q}_j$ is the logit-probability the worker stays at j; quits are given by $q_j = \lambda(1-\tilde{q}_j)$; ρ_w is workers' discount factor on firm value; and β is as in the static setup above.[29] The elasticity of hires j to wages k follows a similar logic, denoting $\tilde{h}_j$ as the logit-probability utility is maximized at j, and $h_j = \lambda \tilde{h}_j$ as the probability of being hired at j:

$$\varepsilon^h_{jk} = -\frac{\lambda(\beta/2)\tilde{h}_j\tilde{h}_k}{h_j(1-\rho_w(1-q'_k))} \tag{7}$$

For intuition, these equations say that the quits and hires elasticities increase in magnitude with $\tilde{q}_k$ and $\tilde{h}_k$ (analogous to firm share p_k), and with β as in the static framework. They additionally increase in magnitude with ρ_w (workers are more sensitive to others' wages when the future matters more), and decrease in magnitude with the probability of quitting firm k (since workers who switch to k enjoy the higher wage for a shorter period).

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 to wages, including wages of other firms. The steady state optimal wage is very similar to the static framework equation 3, with $p_j = h_j/q_j$ as expected (see Supplement D.2.3):

$$ln(w_j) = ln(A_j) - \eta ln(\frac{h_j}{q_j}N) + ln(\frac{\varepsilon^h_j}{\varepsilon^h_j + 1 - \psi}) \tag{8}$$

Where $\psi = \frac{\rho q_j(1-q_j)}{q_j^2+\rho(1-q_j)} \in [0,1/3]$, and ε^h_j refers to the hiring labour supply elasticity. Firms mark down wages less when the future matters more to them, since employment is persistent; the equation reduces to the static case when $\rho = 0$.

Dynamic cross-wage elasticity. Given the similarities between the wage-setting dynamic equation 3 and the static equation 8, it is unsurprising that the cross-wage elasticity ε^w_{jk} follows suit. As an approximation, ignore the effect on the elasticity (due to its small magnitude; see static model). Using the optimal wage equation 8, and substituting in the quit and hiring elasticities

[29] The use of $\beta/2$ makes the shorthand simplification that the β^q relevant to quits and β^h relevant to hires are equal. I also use the slightly different notation q'_k for quits q_k to indicate that the inclusive value over which this is calculated differs if the quits market considerations sets for firms j and k are different.

derived above in Equations 6 and 7 (see Supplement D.2.4):

$$\varepsilon^{w}_{jk} \approx \frac{\eta(\beta/2)\alpha_k}{(1-\rho_w(1-q'_k))}\Big(\underbrace{\frac{\lambda\tilde{h}_j\tilde{h}_k}{h_j}}_{\text{Prop. j's hires from k}} + \underbrace{\frac{\lambda\tilde{q}_j\tilde{q}_k}{q_j}}_{\text{Prop. j's quits to k}}\Big) > 0 \tag{9}$$

where $\alpha_k = \frac{1-\rho_w}{1-\rho_w(1-\lambda+q'_k)}$ accounts for other firms' spillover responses. Equation 9 holds in steady state, with an iterative adjustment period over the short run. Compared to the static equation 4, this dynamic cross-wage elasticity is positive for $\beta > 0$ and $\eta > 0$, and responds similarly in sign to changes in β, η, $\tilde{q}_k$ and $\tilde{h}_k$ (analogous to p_k), and $\tilde{q}_j$ and $\tilde{h}_j$ (analogous to p_j). Using similar benchmark parameters ($\beta = 6$, $\eta = 0.8$, $\tilde{q}_k = \tilde{h}_k = 0.5$, and $\tilde{q}_j = \tilde{h}_j = 0.1$), along with offer rate $\lambda = .5$ and discounting $\rho = .95$, the cross-wage elasticity is slightly higher than the static case at $\varepsilon^{w}_{jk} = 0.81$ (as found above in section 4). In terms of the new parameters, the cross-wage elasticity increases for lower λ (e.g. $\varepsilon^{w}_{jk} = 1.16$ for $\lambda = .4$), and varies non-linearly with ρ_w. Note ε^{w}_{jk} may be greater than 1, depending on parameter values, as found in some of the empirical estimates of section 4.4.

Equation 9 contains the average hires from and quits to firm k, that is, the flow of workers between firms j and k, $f_{jk} = \frac{\lambda\tilde{h}_{jk}}{h_j} + \frac{\lambda\tilde{q}_{jk}}{q_j}$. Hires and quits enter additively because their respective elasticities 7 and 6 are opposite-signed.[30] Equation 9 thus retains the intuition of the stylized model equation 5 under more plausible assumptions of the dynamics.

5.3 Measure of collective bargaining spillovers

How does this framework apply to my setting of collective bargaining? A wage increase is prescribed for a "covered" group of firms, corresponding to the treated firms K above. Note that the model abstracts from the endogeneity of the prescribed wage itself; as discussed in section 3.1,

[30] Note that if net rather than gross flows were relevant, then this would imply firms that are very strongly connected and similarly ranked with equal hires and separations to each other would have negligible ε^{w}_{jk} (an implausible and undesirable implication of the model).

there is evidence in support of this assumption, and moreover section 4.2 discusses how, even if the prescribed wages are partly endogenous to covered firms, they are likely exogenous to spillover firms.

To draw a closer link between the empirical results in section 4 and the model equation 9, let $\Delta ln(w_{B(k)})$ indicate the prescribed increase in wages for a covered firm k in bargaining council event B. An advantage of a flows measure of spillovers, compared to for example geographical distance, is that interfirm worker flows are additive; this allows the primary flows measure below, F_{jB}, to aggregate hires and quits between the noncovered firm j and any covered firm k in the relevant bargaining council event B. Thus for this setting, equation 9 implies the estimating equation:

$$\frac{\Delta ln(w_j)}{\Delta ln(w_{B(k)})} = \delta F_{jB} \tag{10}$$

$$F_{jB} = \underbrace{\frac{\lambda \sum_k^{k \in B} \tilde{h_{jk}}}{h_j}}_{\text{Prop. j's hires from firms in B}} + \underbrace{\frac{\lambda \sum_k^{k \in B} \tilde{q_{jk}}}{q_j}}_{\text{Prop. j's quits to firms in B}}$$

Equation 10 motivates the empirical specification 2 for the spillovers estimation above: the cross-firm wage elasticity ε_{jk}^w is the ratio of firm j's response $\Delta ln(w_j)$ to the fixed change $\Delta ln(w_{B(k)})$, and this ratio is equal to the flows with covered firms F_{jB} multiplied by δ.[31] δ denotes the cross-firm wage elasticity when $F_{jB} = 1$, and corresponds to a function of $\alpha_{B(k)}$, ρ_w, β and η in equation 9: in the estimation, with the relevant fixed effects, these parameters can vary across industry and location without causing bias.[32] My empirical strategy in section 4 therefore uses the covariance between $\Delta ln(w_j)$ and F_{jB} (conditional on controls) to estimate δ, which is the cross-firm wage elasticity.

This theoretical link between a firm's responses in the dynamic logit and its flows F_{jB} is a

[31]Note this does not account directly for variation in wage responses of individual treated firms k, and so the estimation assumes that covered firm wage responses are uncorrelated with F_{jB} (conditional on controls). Ideally, one would account for each covered firm's wage response as a margin of treatment. In practice, it is not obvious how to identify a precise firm-specific treatment effect.

[32]In the discrete case with an adjustment path to the new steady state, 10 is not exact since F_{jB} in equation 10 is a function of intermediate wages; in the estimation above I account for this by instrumenting post- by pre-treatment flows, and tracing the response 3 years out.

novel contribution of this paper, and motivates its empirical use in this study and related measures by several others. And while this framework presents a particular mechanism for spillovers, other approaches are embedded insofar as they are proportional to F_{jB}. For example, noncovered firms aware of spillovers patterns may respond immediately after the announcement of a wage agreement, which may appear as union threat effects, wage norm effects or pattern bargaining; and these pressures may similarly be mediated through worker flows.

6 Discussion

6.1 Evidence and theory

How do the estimates presented in sections 3 and 4 match up with the theoretical framework in section 5? The positive wage effects on covered firms show that prescribed wage changes did induce direct wage effects on covered firms, as expected. The corresponding estimated positive wage effects on noncovered firms are significant and, crucially, proportional to worker flows with covered firms as predicted by the framework. The framework also provides an understanding of the negative estimated effects on noncovered firm profits, as well as the smaller effects on covered firm profits. The worker transition responses are broadly consistent with the firm wage increases, with negative effects on worker separations for covered firms, and increases in hires for non-covered firms from only other non-covered firms (Supplementary figure B4).

One potential mismatch is the link between the quantity effect on covered firms and the wage response for noncovered firms. While the baseline framework assumes that covered firms increase employment, I do not find such a statistically significant increase empirically.[33] This may be

[33] Recall that the framework of section 5 predicts that an increase in covered firm wages raises employment, which decreases the residual labour supply of noncovered firms. While the resulting spillover wage response mitigates some of the counterfactual employment increase in covered firms, there should still be some increase in covered firm employment. Section 3 discusses the employment results, which vary by specification, but are mostly small. A useful test of the relevance of the competitive mechanism of spillovers would be the estimated effects on worker flows between covered and noncovered firms; unfortunately, these estimates are too imprecise (see Supplementary figure

reconciled in the model in three ways already mentioned. Firstly, workers may queue for jobs at covered sector firms, such that the increase in covered firm wages would not increase covered firm employment, but would decrease the residual labour supply of noncovered firms (see Supplement D.1.2). In this case, one may expect an increase in informal sector workers or unemployment; unfortunately the administrative data are limited to the formal sector, and results based on merged data from household surveys are inconclusive (see Supplementary figure C4).

Secondly, if the increase in prescribed wages raises the fair wage norm, which functions as a reservation wage, then the covered firms' rise in employment would be partially offset while potentially bolstering the spillover effects (see Supplement D.1.3). Recent work has highlighted the importance of such wage norms or "benchmarking" in firm wage-setting (Cullen, Li, and Perez-Truglia, 2022). Thirdly, departing from the logit model, the classic Burdett and Mortensen (1998) model can deliver positive wage spillovers without increases in covered firm employment. There are similarities with the above: one needs to assume job offers arrive more for the searching unemployed than the employed (a bit like queuing), and spillovers arise due to an increase in the reservation wage (a bit like a fair wage norm). However in this model there are no non-search frictions, i.e. a firm paying slightly more is infinitely more attractive, and spillovers are not proportional to the worker flows with the covered sector. A combination of these three mechanisms may explain the small estimated covered sector employment effect, or other non-competitive mechanisms such as union threat effects may be relevant.[34]

Aside from these model-based explanations, another explanation is that this negligible estimated *average* firm size effect is consistent with the baseline model if it hides substantial heterogeneity in firm size effects, as found elsewhere (Bustos, 2021; Dodini, Stansbury, and Willén, 2023; Dustmann et al., 2021; Fanfani, 2022).[35] In terms of the model, higher-wage firms are

B4).

[34]If the probability of workers unionizing is increasing in the perceived union wage premium, and information on this premium spreads through outside options (e.g. word of mouth), then an increase in the prescribed wage for covered firms would increase the perceived premium for workers in noncovered firms. Unionization may have longer term effects on profits, so noncovered firms may choose to reduce the perceived union wage premium by increasing wages.

[35]It is also possible that, since the employment effect is estimated relative to the counterfactual, employment may still have increased in absolute terms. However, to explain spillover wage effect after the treatment, this implies that

supply-constrained and so an increase in their wages leads to expansion; while lower-wage firms have low productivity, cannot pay the prescribed wage, and so are forced to downsize (Dickens, Machin, and Manning, 1999). If spillover firms are more connected to the expanding firms, the relevant covered employment effect would be positive.

Supplementary figure C2 shows the results from a regression which uses the main bargaining council specification 1, 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 labour 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. As robustness, I show event-studies of differential wage and firm size effects by pre-event wage in Supplementary figure C3.

Ideally one would test the extent to which spillover firms were connected to the subset of covered firms that expanded, but this setting is underpowered for such a demanding empirical exercise. Overall, the link between the quantity and wage responses in spillovers is an important open question for further research.

6.2 Microsimulation of aggregate wage effects

How important are these bargaining council wage increases for the aggregate earnings distribution, including effects on both covered and noncovered firms? I implement a simple microsimulation using characteristics in the data for workers and firms in the base sample of 2008, then project to 2018 based on the estimates above from the stacked event-study designs. This assumes the estimated treatment effects are comparable for other prescribed wage increases.

For the direct effects, given that the contracted wage changes in section 3 appear to map directly into sustained wage increases, I set the simulated wage increase of each covered worker as the ob-

counterfactual employment for covered firms in the stacked event-study would have grown after the prescribed wage increases, i.e. that these prescribed wage increases are strongly endogenous to industry shocks. As discussed in section 3.1, I show some evidence against this, though my within-CBA estimates allow for this possibility.

served change in that bargaining council's wage agreements over the full period (2008-2018). For the spillover effects, following equation 10, I set the simulated wage increase of each noncovered worker equal to the change in wage agreements over the period for the relevant bargaining council in their industry-location, times by the worker flows to that bargaining council, and times by the estimated cross-wage elasticity of 0.8 from section 4 above. Note this micro-simulation does not take into account other effects, for example on assignment of workers to firms.

Supplementary figure C5 plots these simulated wage distributions relative to the observed distribution in 2008. The x-axis shows the AKM worker quantile as a proxy for worker quality. The figure shows the direct effect of bargaining councils on the aggregate earnings distribution, which is on average about 8%. The largest effects are for the mid-quantile workers, with the smallest effects at the top of the worker distribution. The figure also shows the spillover effects, which add another 8% to the average wages of all worker quantiles.

Note that spillovers increase both the average magnitude and reach of the wage agreements, as more people benefit. Although bargaining council firms are concentrated in higher earnings deciles, spillover firms are more evenly spread, and the aggregate effects on inequality are negligible in this simulation. Supplementary figure C6 panel A shows similar distributional effects when accounting for possible negative employment effects. Panel B shows a counterfactual where noncovered firms have artificially greater flows to bargaining councils, which would increase the aggregate effect of spillovers, suggesting there are substantial gains to lowering labour market frictions.

In sum, this extrapolative exercise suggests bargaining councils increase average wages by about 15% and affects workers across the distribution of earnings. Neglecting to account for spillovers would reduce these bargaining council effects by about half.

6.3 Concluding thoughts

This paper demonstrates the direct and indirect impact of collective bargaining on the labour market. I find that, following a large wage increase prescribed by bargaining council agreements, observed covered firm wages increase. Firms that are strongly connected by worker flows to covered firms see wage increases of a similar magnitude, together with a decrease in profit margins. Firm inequality declines for both sets of firms, and workers are affected throughout the earnings distribution. A simple simulation suggests that such spillover effects double the direct impact of these bargaining council agreements, which highlights the broad-ranging impact of institutional regulation on the aggregate wage structure.

A methodological contribution of this paper is to motivate the flows measure of spillovers in a dynamic framework of monopsonistic competition and localized labour markets, as supported by recent empirical work. While the baseline framework focuses on the competitive mechanism, effects on outside options measured by worker flows may be much broader, with several complementary mechanisms such as norms of fairness or union threat effects. Such mechanisms may be important in rationalizing wage spillovers when there are negative net employment effects from covered firms.

The findings in this paper should be read in the global context of declining private sector union density. The spillovers across industries, and on low-wage workers that are not well-unionized, is an important consideration for assessing the continued impact of unionized workers. More generally, this paper highlights the power of regulation to influence the wage structure. Unions and wage boards are popular policy recommendations to constrain monopsony power (Dube, 2018; Stelzner and Paul, 2020), and South Africa's collective bargaining councils serve as an illuminating example.

References

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

Arkhangelsky, Dmitry and Guido W Imbens (2022). “Doubly robust identification for causal panel data models”. In: *The Econometrics Journal* 25.3, pp. 649–674.

Arnold, David (2020). *Mergers and acquisitions, local labor market concentration, and worker outcomes*. https://scholar.princeton.edu/sites/default/files/dharnold/files/jmp.pdf. Job Market Paper.

Azar, José, Ioana Marinescu, and Marshall Steinbaum (2022). “Labor market concentration”. In: *Journal of Human Resources* 57.S, S167–S199.

Badaoui, Eliane and Frank Walsh (2022). “Productivity, non-compliance and the minimum wage”. In: *Journal of Development Economics* 155, p. 102778.

Bassier, Ihsaan (2023). “Firms and inequality when unemployment is high”. In: *Journal of Development Economics* 161, p. 103029.

Bassier, Ihsaan et al. (2021). “Locked down and locked out: Repurposing social assistance as emergency relief to informal workers”. In: *World Development* 139, p. 105271.

Berger, David, Kyle Herkenhoff, and Simon Mongey (2022a). “Labor market power”. In: *American Economic Review* 112.4, pp. 1147–93.

— (2022b). *Minimum Wages, Efficiency and Welfare*. Tech. rep. National Bureau of Economic Research.

Bhaskar, Venkataraman, Alan Manning, and Ted To (2002). “Oligopsony and monopsonistic competition in labor markets”. In: *Journal of Economic Perspectives* 16.2, pp. 155–174.

Bhaskar, Venkataraman and Ted To (1999). “Minimum wages for Ronald McDonald monopsonies: A theory of monopsonistic competition”. In: *The Economic Journal* 109.455, pp. 190–203.

Bhorat, Haroon, Carlene Van der Westhuizen, and Sumayya Goga (2009). “Analysing wage formation in the South African labour markets: The role of bargaining councils”. In: *Development Policy Research Unit Working Paper* 09/135.

Bhuller, Manudeep et al. (2022). “Facts and Fantasies about Wage Setting and Collective Bargaining”. In: *Journal of Economic Perspectives* 36.4, pp. 29–52.

Blandhol, Christine et al. (2020). *Do Employees Benefit from Worker Representation on Corporate Boards?* Tech. rep. National Bureau of Economic Research.

Borusyak, Kirill, Xavier Jaravel, and Jann Spiess (2023). “Revisiting event study designs: Robust and efficient estimation”. In: *Review of Economic Studies*.

Bronars, Stephen G and Donald R Deere (1994). “Unionization and profitability: Evidence of spillover effects”. In: *Journal of Political Economy* 102.6, pp. 1281–1287.

Budlender, Debbie and Shaheeda Sadeck (2007). "Bargaining council and other benefit schemes". In: *A unpublished report by the Community Agency for Social Enquiry. Prepared for the National Treasury, July*.

Budlender, Joshua and Amina Ebrahim (2020). *Industry classification in the South African tax microdata*. Tech. rep. United Nations University World Institute for Development Economics Research.

Burdett, Kenneth and Dale T Mortensen (1998). "Wage differentials, employer size, and unemployment". In: *International Economic Review*, pp. 257–273.

Bustos, Emil (2021). *The Effect of Centrally Bargained Wages on Firm Growth*. https://www.dropbox.com/s/hnmptmy0o3vwqvz/bustos_cbw_main.pdf?dl=0. Job Market Paper.

Butcher, Kristin F and Elena Cecilia Rouse (1999). *Wage effects of unions and industrial councils in South Africa*. The World Bank.

Caetano, Carolina et al. (2022). "Difference in differences with time-varying covariates". In: *arXiv preprint arXiv:2202.02903*.

Cahuc, Pierre, Fabien Postel-Vinay, and Jean-Marc Robin (2006). "Wage bargaining with on-the-job search: Theory and evidence". In: *Econometrica* 74.2, pp. 323–364.

Caldwell, Sydnee and Oren Danieli (2023). "Outside options in the labor market". In: *The Review of Economic Studies*.

Caldwell, Sydnee, Arindrajit Dube, and Suresh Naidu (forthcoming). *Monopsony Makes It Big: Firm Wage-Setting in Modern Labor Markets*. Tech. rep. mimeo.

Caldwell, Sydnee and Nikolaj Harmon (2019). "Outside options, bargaining, and wages: Evidence from coworker networks". In: *Unpublished manuscript, Univ. Copenhagen*, pp. 203–207.

Callaway, Brantly, Andrew Goodman-Bacon, and Pedro HC Sant'Anna (2021). "Difference-in-differences with a continuous treatment". In: *arXiv preprint arXiv:2107.02637*.

Caplin, Andrew, Mark Dean, and John Leahy (2019). "Rational inattention, optimal consideration sets, and stochastic choice". In: *The Review of Economic Studies* 86.3, pp. 1061–1094.

Card, David (2022). "Who Set Your Wage?" In: *American Economic Review* 112.4, pp. 1075–90.

Card, David and Ana Rute Cardoso (2022). "Wage flexibility under sectoral bargaining". In: *Journal of the European Economic Association* 20.5, pp. 2013–2061.

Card, David et al. (2018). "Firms and labor market inequality: Evidence and some theory". In: *Journal of Labor Economics* 36.S1, S13–S70.

Cengiz, Doruk et al. (2019). "The effect of minimum wages on low-wage jobs". In: *The Quarterly Journal of Economics* 134.3, pp. 1405–1454.

Cengiz, Doruk et al. (2022). "Seeing beyond the trees: Using machine learning to estimate the impact of minimum wages on labor market outcomes". In: *Journal of Labor Economics* 40.S1, S203–S247.

Chaisemartin, Clément de et al. (2022). "Difference-in-Differences Estimators for Treatments Continuously Distributed at Every Period". In: *arXiv preprint arXiv:2201.06898*.

Chen, Shuowen, Victor Chernozhukov, and Iván Fernández-Val (2019). "Mastering panel metrics: causal impact of democracy on growth". In: *AEA Papers and Proceedings*. Vol. 109. American Economic Association 2014 Broadway, Suite 305, Nashville, TN 37203, pp. 77–82.

Cribb, Jonathan et al. (2021). *The distributional and employment impacts of nationwide Minimum Wage changes*. Tech. rep. IFS Working Paper.

Cullen, Zoe B, Shengwu Li, and Ricardo Perez-Truglia (2022). *What's My Employee Worth? The Effects of Salary Benchmarking*. Tech. rep. National Bureau of Economic Research.

De Chaisemartin, Clément and Xavier d'Haultfoeuille (2020). "Two-way fixed effects estimators with heterogeneous treatment effects". In: *American Economic Review* 110.9, pp. 2964–96.

Demir, Gökay (2022). *Labor Market Frictions and Spillover Effects from Publicly Announced Sectoral Minimum Wages*. Working paper 985. Ruhr Economic Papers.

Derenoncourt, Ellora and Claire Montialoux (2021). "Minimum wages and racial inequality". In: *The Quarterly Journal of Economics* 136.1, pp. 169–228.

Derenoncourt, Ellora et al. (2021). *Spillover effects from voluntary employer minimum wages*. Tech. rep. National Bureau of Economic Research.

Dickens, Richard, Stephen Machin, and Alan Manning (1999). "The effects of minimum wages on employment: Theory and evidence from Britain". In: *Journal of labor economics* 17.1, pp. 1–22.

DiNardo, John and David S Lee (2004). "Economic impacts of new unionization on private sector employers: 1984–2001". In: *The Quarterly Journal of Economics* 119.4, pp. 1383–1441.

Dodini, Samuel, Kjell G Salvanes, and Alexander Willén (2021). *The dynamics of power in labor markets: Monopolistic unions versus monopsonistic employers*. Discussion Paper 16834. CEPR.

Dodini, Samuel, Anna Stansbury, and Alexander Willén (2023). *How Do Firms Respond to Unions?* Job Market Paper.

DoL (2018). *Registered private sector bargaining councils in South Africa as for March 2018*. Pretoria: Department of Labour, Republic of South Africa.

Dube, Arindrajit (2018). *Using wage boards to raise pay*. Tech. rep. Economists for Inclusive Prosperity.

— (2019). "Impacts of minimum wages: review of the international evidence". In: *Independent Report. UK Government Publication*, pp. 268–304.

Dube, Arindrajit et al. (2022). "A local projections approach to difference-in-differences event studies". In: *NBER Working Paper*.

Dustmann, Christian et al. (2021). “Reallocation effects of the minimum wage”. In: *The Quarterly Journal of Economics*.

Eliaz, Kfir and Ran Spiegler (2011). “Consideration sets and competitive marketing”. In: *The Review of Economic Studies* 78.1, pp. 235–262.

Engbom, Niklas and Christian Moser (2022). “Earnings inequality and the minimum wage: Evidence from Brazil”. In: *American Economic Review* 112.12, pp. 3803–3847.

Fanfani, Bernardo (2022). *The employment effects of collective wage bargaining*. Laboratorio R. Revelli, Collegio Carlo Alberto.

Felix, Mayara (2021). “Trade, labor market concentration, and wages”. In: *Job Market Paper*.

Fortin, Nicole M, Thomas Lemieux, and Neil Lloyd (2021). “Labor market institutions and the distribution of wages: The role of spillover effects”. In: *Journal of Labor Economics* 39.S2, S369–S412.

Freeman, Richard B (1982). “Union wage practices and wage dispersion within establishments”. In: *ILR Review* 36.1, pp. 3–21.

Freeman, Richard B and James L Medoff (1981). “The impact of the percentage organized on union and nonunion wages”. In: *The Review of Economics and Statistics*, pp. 561–572.

Gathmann, Christina, Ines Helm, and Uta Schönberg (2020). “Spillover effects of mass layoffs”. In: *Journal of the European Economic Association* 18.1, pp. 427–468.

Goldschmidt, Deborah and Johannes F Schmieder (2017). “The rise of domestic outsourcing and the evolution of the German wage structure”. In: *The Quarterly Journal of Economics* 132.3, pp. 1165–1217.

Gopalan, Radhakrishnan et al. (2021). “State minimum wages, employment, and wage spillovers: Evidence from administrative payroll data”. In: *Journal of Labor Economics* 39.3, pp. 673–707.

Greenstone, Michael, Richard Hornbeck, and Enrico Moretti (2010). “Identifying agglomeration spillovers: Evidence from winners and losers of large plant openings”. In: *Journal of Political Economy* 118.3, pp. 536–598.

Harris, John R and Michael P Todaro (1970). “Migration, unemployment and development: a two-sector analysis”. In: *The American Economic Review* 60.1, pp. 126–142.

Hausman, Jerry A and Maxim Pinkovskiy (2017). “Estimating dynamic panel models: backing out the Nickell Bias”. In.

Helm, Ines (2020). “National industry trade shocks, local labour markets, and agglomeration spillovers”. In: *The Review of Economic Studies* 87.3, pp. 1399–1431.

Hsieh, Chang-Tai and Benjamin A Olken (2014). “The missing" missing middle"”. In: *Journal of Economic Perspectives* 28.3, pp. 89–108.

Jäger, Simon, Shakked Noy, and Benjamin Schoefer (2022). "The German Model of Industrial Relations: Balancing Flexibility and Collective Action". In: *Journal of Economic Perspectives* 36.4, pp. 53–80.

Jäger, Simon et al. (2022). *Worker beliefs about outside options*. Tech. rep. National Bureau of Economic Research.

Jarosch, Gregor, JS Nimcsik, and Isaac Sorkin (2019). *Granular Search, Market Structure, and Wages*.

Kahn, Lawrence M and Michael Curme (1987). "Unions and nonunion wage dispersion". In: *The Review of Economics and Statistics*, pp. 600–607.

Lamadon, Thibaut, Magne Mogstad, and Bradley Setzler (2022). "Imperfect competition, compensating differentials, and rent sharing in the US labor market". In: *American Economic Review* 112.1, pp. 169–212.

Langella, Monica and Alan Manning (2021). "The measure of monopsony". In: *Journal of the European Economic Association*.

Levy, R et al. (2014). *Labour relations in South Africa*. 5th ed. Cape Town: Oxford University Press.

Lewis, H Gregg (1986). "Union relative wage effects". In: *Handbook of labor economics* 2, pp. 1139–1181.

Lopez, Eduardo, Omar Guerrero, and Robert L Axtell (2015). "The network picture of labor flow". In: *arXiv preprint arXiv:1507.00248*.

Magruder, Jeremy R (2012). "High unemployment yet few small firms: The role of centralized bargaining in South Africa". In: *American Economic Journal: Applied Economics* 4.3, pp. 138–66.

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

— (2021). "Monopsony in labor markets: A review". In: *ILR Review* 74.1, pp. 3–26.

Manning, Alan and Barbara Petrongolo (2017). "How local are labor markets? Evidence from a spatial job search model". In: *American Economic Review* 107.10, pp. 2877–2907.

Matějka, Filip and Alisdair McKay (2015). "Rational inattention to discrete choices: A new foundation for the multinomial logit model". In: *American Economic Review* 105.1, pp. 272–98.

McFadden, Daniel et al. (1973). "Conditional logit analysis of qualitative choice behavior". In.

Miller, Douglas L (2023). "An Introductory Guide to Event Study Models". In: *Journal of Economic Perspectives* 37.2, pp. 203–230.

Moll, Peter (1996). "Compulsory centralization of collective bargaining in South Africa". In: *The American Economic Review* 86.2, pp. 326–329.

NALEDI (2006). *The workers' survey for COSATU*. http://us-cdn.creamermedia.co.za/assets/articles/attachments/03048_naledi_workers_survey_2006.pdf. National Labour and Economic Development Institute.

National Treasury and UNU-WIDER (2020a). *CIT-IRP5 Firm Panel 2008-2018 [dataset]*. Version 4.0. Pretoria: South African Revenue Service [producer of the original data], 2019. Pretoria: National Treasury and UNU-WIDER [producer and distributor of the harmonized dataset], 2020.

— (2020b). *IRP5 2008-2018 [dataset]*. Version 4.0. Pretoria: South African Revenue Service [producer of the original data], 2019. Pretoria: National Treasury and UNU-WIDER [producer and distributor of the harmonized dataset], 2020.

Nimczik, Jan Sebastian (2020). *Job mobility networks and data-driven labor markets*. Tech. rep. Technical report, Working Paper.

Rambachan, Ashesh and Jonathan Roth (2019). "An honest approach to parallel trends". In: *Unpublished manuscript, Harvard University*.

Rust, John (1987). "Optimal replacement of GMC bus engines: An empirical model of Harold Zurcher". In: *Econometrica: Journal of the Econometric Society*, pp. 999–1033.

Schubert, Gregor, Anna Stansbury, and Bledi Taska (2020). "Monopsony and outside options". In: *Available at SSRN*.

Staiger, Douglas O, Joanne Spetz, and Ciaran S Phibbs (2010). "Is there monopsony in the labor market? Evidence from a natural experiment". In: *Journal of Labor Economics* 28.2, pp. 211–236.

Stelzner, Mark and Mark Paul (2020). "Monopsony and collective action in an institutional context". In: *Review of Social Economy*, pp. 1–21.

Train, Kenneth E (2009). *Discrete choice methods with simulation*. Cambridge university press.

7 Tables and Figures

Table 1: **Descriptive statistics of firm panel and stacked event-study, by coverage**

Statistic	(1) Cross-sectional panel	(2)	(3) Stacked event study	(4)	(5)
	Covered	Uncovered	Covered	Spillover	Unconnected
Num. firms (th.)	29	54	16	11	32
Num. workers (m.)	6.26	10.64	3.29	1.71	5.26
Firm size (% of firms)					
1–10	.07	.07	.07	.07	.07
11–30	.52	.5	.5	.48	.48
31–100	.29	.28	.31	.3	.3
100+	.13	.15	.12	.16	.16
Wages (annual, ZAR th.)					
Prescribed min.	56.1		59.2		
Mean	181	114	112	117	124
Median	79	86	83	86	92
Wage p80-p20 ratio	4.97	7.83	4.97	5.23	6.12
AKM worker FE	-.13	-.05	-.08	-.04	.01
AKM firm FE	.09	-.03	.14	-.01	.02
Earnings distribution (% of workers)					
Decile 1-5	.42	.58	.28	.56	.48
Decile 6-8	.4	.18	.6	.25	.27
Decile 9-10	.18	.24	.12	.19	.25
Transition rates (% firm size)					
Hires	.33	.35	.33	.35	.35
E-E hires	.1	.11	.1	.11	.11
Separations	.32	.34	.32	.34	.34
E-E separations	.1	.12	.1	.11	.12
Firm rents (p.p., th.)					
Profit	227	233	217	239	250
Value added	392	419	392	428	452

Notes. Columns 1-2 provide descriptive statistics for the panel of firms, restricted to at least 10 workers in the previous period. "Covered" firms belong to a bargaining council, and "noncovered" firms are all others. Columns 3-5 provide descriptive statistics for the stacked event-study, restricted to firms in event year -1 that have above 10 workers in the pre-period. See main text for details on event-sample construction. "Covered" firms belong to a bargaining council, "spillover" firms are noncovered firms with high worker flows to covered firms, and "unconnected" firms are noncovered firms with negligible (less than 1%) flows to covered firms. In terms of statistics, the number of firms and workers refer to unique instances, and AKM worker and firm FE are the respective components from a regression of log wages on worker and firm fixed effects (Abowd, Kramarz, and Margolis, 1999). In brackets, "m." refers to millions, "th." refers to thousands, and "p.p." refers to per person (i.e. divided by firm size).

Table 2: **Elasticity of outcomes to prescribed wage increases for covered firms**

	(1) Wage (p25)	(2) Wage (p50)	(3) Wage p80-p20	(4) Firm size	(5) Profit
Pre-period	0.045	-0.076	-0.096	-0.070	0.132
	(0.1238)	(0.1001)	(0.1346)	(0.0692)	(0.5623)
Main effect	0.904***	0.749***	-0.567***	-0.230	-0.290
	(0.3111)	(0.2362)	(0.1889)	(0.1996)	(0.3639)
Obs (m.)	2.4	2.4	2.4	2.4	1.2
Treated firms (th.)	19	19	18	19	12

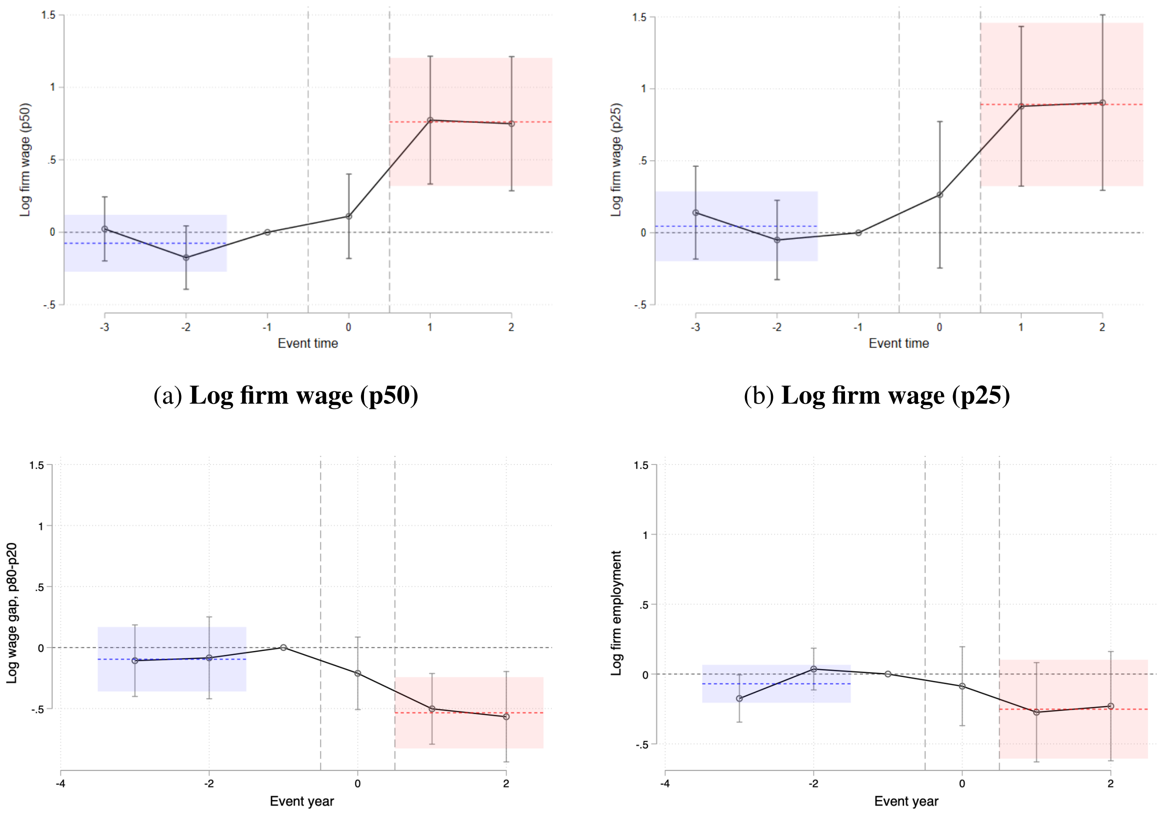
Notes. The table shows estimates based on the main event-study specification assessing direct effects on covered firms from 47 bargaining council wage increases (see section 3.1). The sample is of stacked events, restricted to balanced firms with more than 10 workers in the pre-period, and includes noncovered control firms. The main regressor is an indicator for covered firms, with identification from variation within firms over time and a rich set of time-varying fixed effects. All outcomes are in logs, and scaled by the magnitude of the prescribed wage increase such that coefficients are interpreted as elasticities with respect to the prescribed wage. Regressions are run at the unweighted firm-level, and standard errors are clustered by bargaining council. Row 1 shows the pre-treatment coefficient as a placebo, and row 2 shows the main treatment effect which is interpretable as an elasticity of firm wage changes to prescribed minimum wage changes.

Table 3: **Elasticity of outcomes to prescribed wage increases for noncovered firms**

	(1) Wage (p25)	(2) Wage (p50)	(3) Wage p80-p20	(4) Firm size	(5) Profit
Pre-period	-0.195	0.006	0.114	-0.059	-0.801
	(0.2135)	(0.1474)	(0.2205)	(0.0960)	(0.8669)
Main effect	0.814***	0.874***	-0.765***	-0.149	-2.860***
	(0.2841)	(0.2047)	(0.2418)	(0.2389)	(0.9630)
Obs (th.)	280	280	280	280	52

Notes. The table shows estimates based on the main event-study specification assessing spillover effects from 47 bargaining council wage increases (see section 4.2). The sample is of stacked events, restricted to balanced firms with more than 10 workers in the pre-period, and excludes covered firms. The main regressor is the pre-treatment flow of workers to bargaining council firms, with identification from variation within firms over time and a rich set of time-varying fixed effects. All outcomes are in logs, and scaled by the magnitude of the prescribed wage increase such that coefficients are interpreted as elasticities with respect to the prescribed wage. Regressions are run at the unweighted firm-level, and standard errors are clustered by industry-location. Row 1 shows the pre-treatment coefficient as a placebo, and row 2 shows the main treatment effect which is the cross-firm wage elasticity of firm wage changes to the relevant bargaining council prescribed minimum wage changes.

Figure 1: **Event-study elasticities to prescribed wage increases for covered firms**



(a) **Log firm wage (p50)**

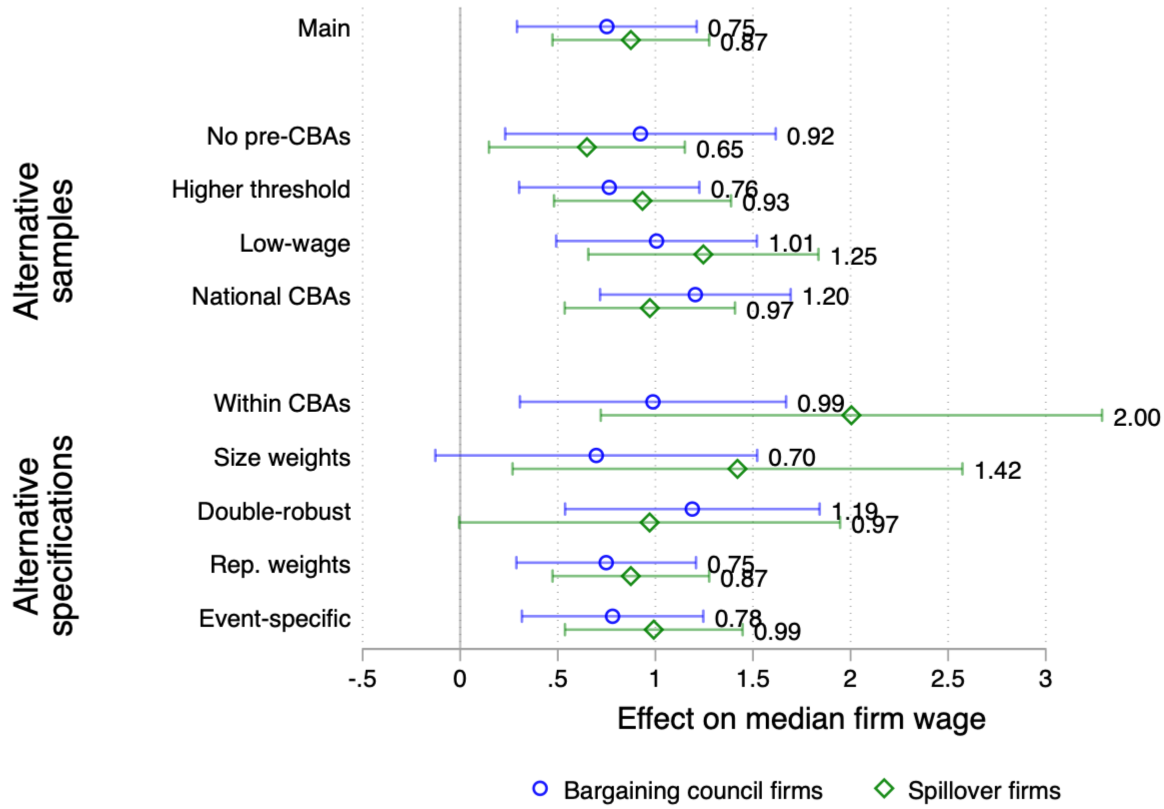
(b) **Log firm wage (p25)**

(c) **Within-firm p80-p20 wage gap**

(d) **Log firm size**

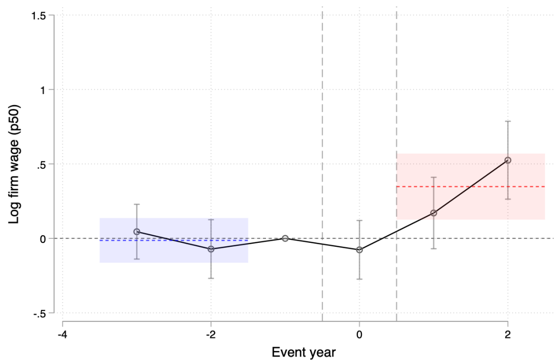
Notes. The figure shows estimates based on the main event-study specification assessing direct effects on covered firms from 47 bargaining council wage increases (see section 3.1), and corresponding to table 2. The sample is of stacked events, restricted to balanced firms with more than 10 workers in the pre-period, and includes noncovered control firms. The main regressor is an indicator for covered firms, with identification from variation within firms over time and a rich set of time-varying fixed effects. All outcomes are in logs, and scaled by the magnitude of the prescribed wage increase such that coefficients are interpreted as elasticities with respect to the prescribed wage. Regressions are run at the unweighted firm-level, and standard errors are clustered by bargaining council. The panels show the estimated effect for each event-period on (A) the 25th percentile of within firm log wages, (b) the 50th percentiles of within-firm log wages, (C) the p80-p20 within-firm wage gap, and (D) log firm size. All y-axes have been set to the same range for comparison.

Figure 2: **Alternative estimates of two-year out wage elasticities for covered and noncovered firms**

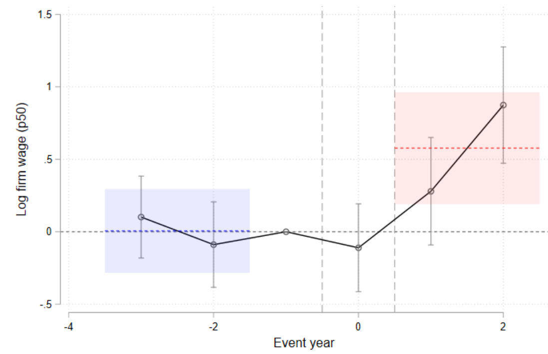


Notes. The figure shows estimates based on the main event-study specification from 47 bargaining council wage increases, assessing direct effects on covered firms (see section 3.1), as well as spillover effects on noncovered firms (see section 4.2). The sample is of stacked events, restricted to balanced firms with more than 10 workers in the pre-period. Identification is from variation within firms over time, and includes a rich set of time-varying fixed effects. Regressions are run at the unweighted firm-level. Each estimate is the final period (two-year-out) effect on log firm median wages for the relevant specification, relative to the relevant bargaining council prescribed wage change. Row headings are as follows, with thousands of treated firms in the covered regression and thousands of firms in the spillover regression shown respectively in brackets. "Main" is the main set of results (17.6 and 46.8). As alternative samples: "No pre-CBAs" excludes events of bargaining councils which had a large change in prescribed wages in event years -4 or -5 (11.3 and 25.3); "Higher threshold" restricts to events of bargaining councils with prescribed wage changes of at least 3.5% (17.1 and 37.2); "Low-wage" restricts to firms with above-median proportion of pre-period workers who were paid below the median (8.6 and 22.8); "National CBAs" restricts to collective bargaining councils covering national industries (13.4 and 34.3). As alternative specifications: "Within CBAs" uses national changes in prescribed wages to compare regions with low vs high median firm wages (13.3 and 20.6); "Size-weight" weights the main specification by firm size (10.4 and 16.8); "Double robust" weights the main specification by the propensity score, estimated on pre-event firm characteristics (15.7 and 13.0); "Rep. weights" re-weights the main specification to be representative of the full firm panel (17.6 and 46.8); and "Event specific cont" interacts all controls in the main specification with each event (17.6 and 37.0).

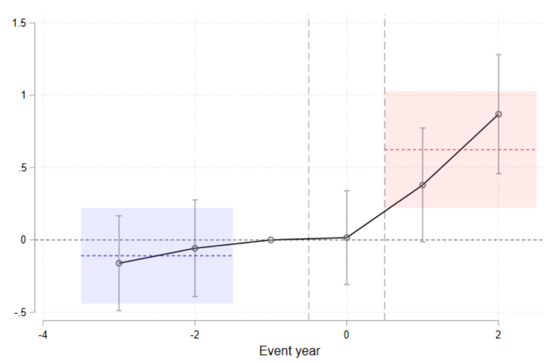
Figure 3: **Event-study elasticities of firm wages to prescribed wage increases for noncovered firms**



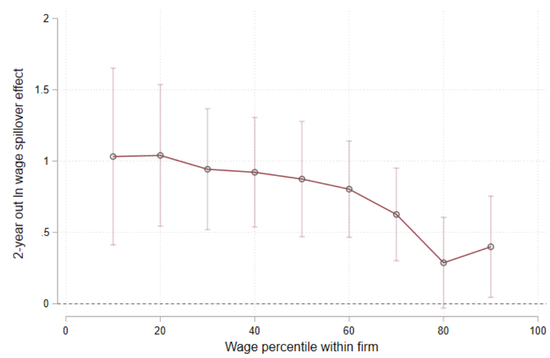
(a) **OLS (p50)**



(b) **IV (p50)**



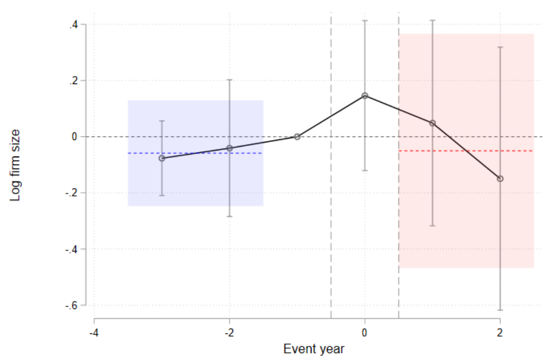
(c) **IV (high vs low)**



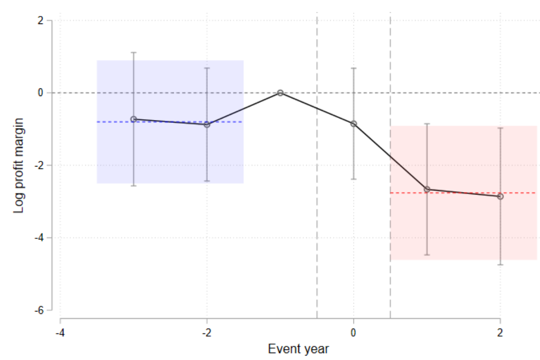
(d) **Two-year out wages, by decile of within-firm wages**

Notes. The figure shows estimates based on the main event-study specification assessing spillover effects from 47 bargaining council wage increases (see section 4.2), and corresponding to table 3. The sample is of stacked events, restricted to balanced firms with more than 10 workers in the pre-period, and excludes covered firms. The main regressor is the pre-treatment flow of workers to bargaining council firms, with identification from variation within firms over time, and a rich set of time-varying fixed effects. All outcomes are in logs, and scaled by the magnitude of the prescribed wage increase such that coefficients are interpreted as elasticities with respect to the prescribed wage. Regressions are run at the unweighted firm-level, and standard errors are clustered by industry-location. Panel A shows the OLS estimates of median firm wages, Panel B shows the IV estimates of median firm wages, Panel C shows the IV estimates of high versus low flow firms, and Panel D shows final year estimates from deciles of within-firm wages.

Figure 4: **Event-study elasticities of other outcomes to prescribed wage increases for noncovered firms**



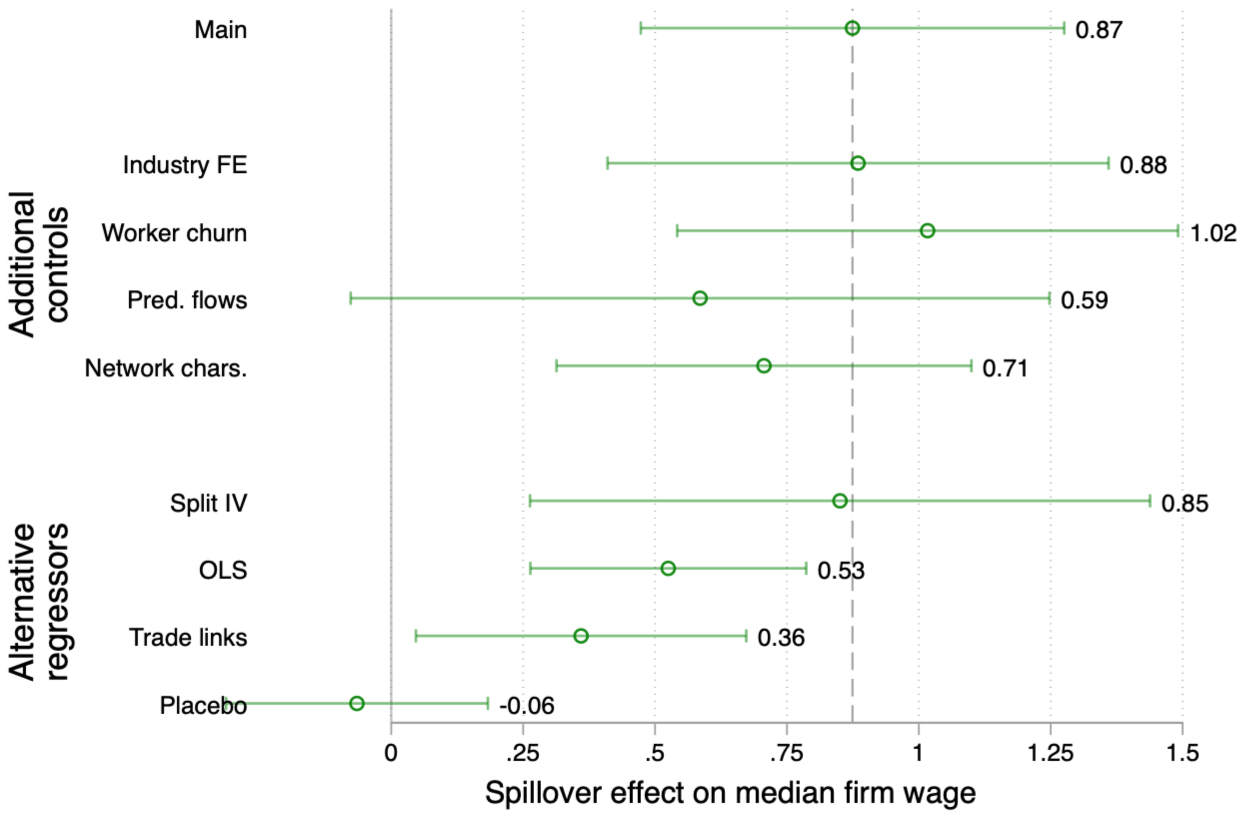
(a) **Firm size**



(b) **Firm profit margin**

Notes. The figure shows estimates based on the main event-study specification assessing spillover effects from 47 bargaining council wage increases (see section 4.2), and corresponding to table 3. The sample is of stacked events, restricted to balanced firms with more than 10 workers in the pre-period, and excludes covered firms. The main regressor is the pre-treatment flow of workers to bargaining council firms, with identification from variation within firms over time, and a rich set of time-varying fixed effects. All outcomes are in logs, and scaled by the magnitude of the prescribed wage increase such that coefficients are interpreted as elasticities with respect to the prescribed wage. Regressions are run at the unweighted firm-level, and standard errors are clustered by industry-location. Panels A shows the the log of the number of workers per firm, and Panel B shows the log of the profit margin (defined as each firm's total profit over their total value added).

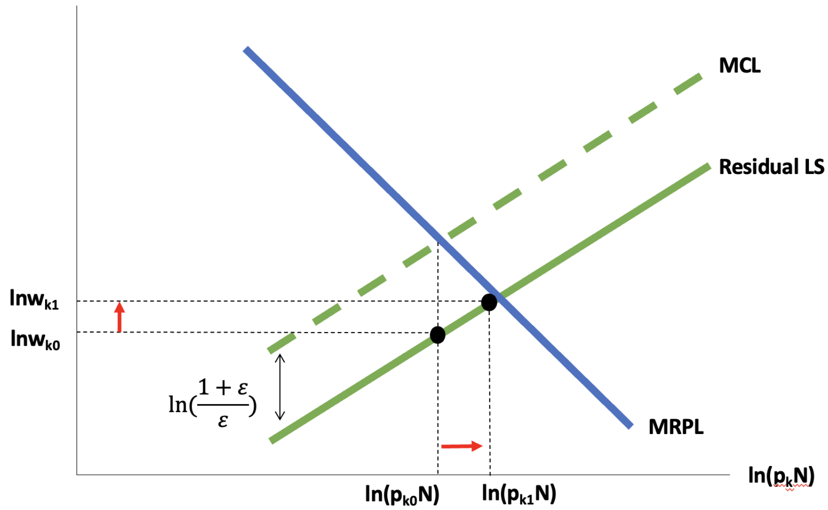
Figure 5: **Alternative estimates of two-year-out spillover wage elasticities**



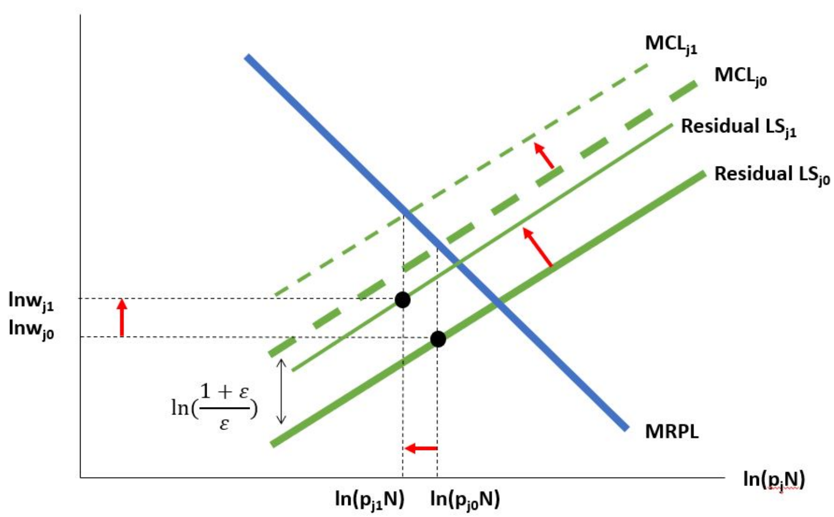
Notes. The figure shows estimates based on the main event-study specification assessing spillover effects from 47 bargaining council wage increases (see section 4.2). The sample is of stacked events, restricted to balanced firms with more than 10 workers in the pre-period, and excludes covered firms. The baseline regressor is the pre-treatment flow of workers to covered firms. Identification is from variation within firms over time, and includes a rich set of time-varying fixed effects. All outcomes are in logs, and scaled by the magnitude of the prescribed wage increase such that coefficients are interpreted as elasticities with respect to the prescribed wage. Regressions are run at the unweighted firm-level, and standard errors are clustered by industry-location. Each estimate is the final period effect on firm median wages for the relevant specification. "Main" is the main set of results (280,000 obs). As alternative controls interacted with event-time in the main specification: "Industry FE" adds industry fixed effects (280,000 obs), "Worker churn" adds the churn or turnover rate of workers i.e. excess transitions (197,000 obs), "Predicted flows" adds the predicted flow of workers to bargaining councils based on observable characteristics (99,000 obs), and "Network characteristics" adds average characteristics from the relevant industry-location such as time-varying wages and firm size (280,000 obs). As alternative regressors: "Split IV" instruments the pre-period worker flows in a randomized sample with the complement sample (278,000 obs); "OLS" omits the instrument, but otherwise has the same specification (279,000 obs); "Trade flows" uses the proportion of trade between firms as given by sectoral Input-Output tables (64,000 obs); and "Scrambled flows (placebo)" randomly re-assigns worker flows to firms in the main specification (275,000 obs).

Figure 6: **Diagram of prescribed wage effects on covered and noncovered firms**

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



(b) **Cumulative spillover effect on noncovered firms**



Notes. The equation for the Marginal Revenue Product of Labour Curve (MRPL) is $ln(mrpl) = lnA_j - \eta ln(p_jN)$ for firm j, aggregate labour supply N, proportion of employment p_j and firm-specific productivity A_j. The equation for the Residual Labour Supply (LS) is $lnw_j^{LS} = 1/\beta(ln(p_jN) - lnN + ln(\sum_l^J w_l^\beta))$, where the summation term includes treated firms' employment (this line is only linear at a first order approximation). The equation for the Marginal Cost of Labour (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 3 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}.

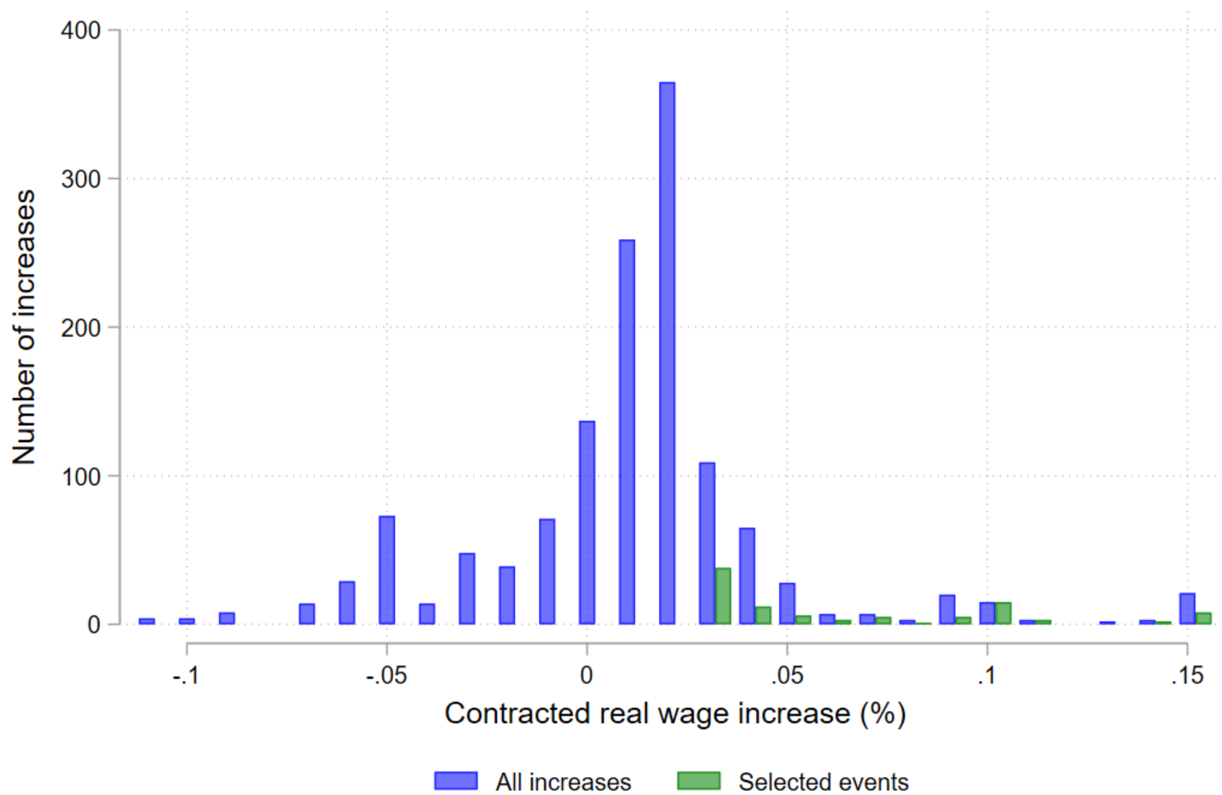
Collective Bargaining and Spillovers in Local Labour Markets: Supplementary Material

Ihsaan Bassier

Contents

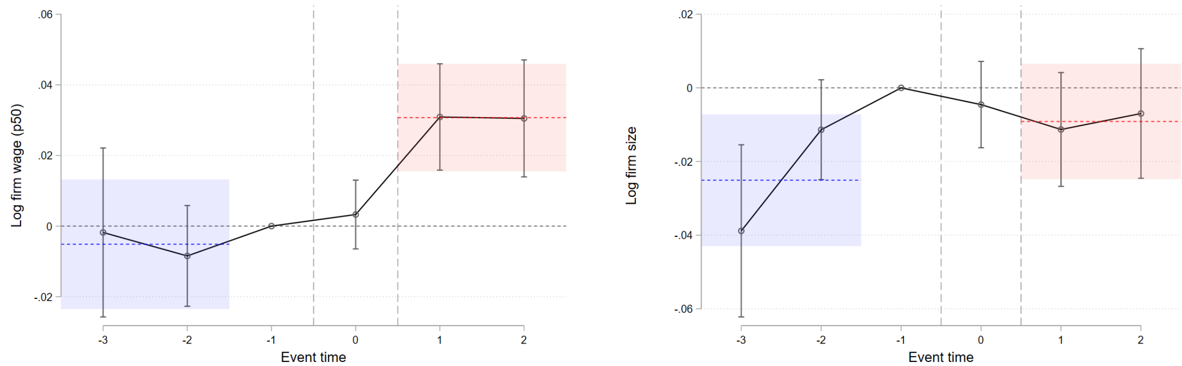
A Appendix: Direct effects

Figure A1: **Bargaining council wage increases and selected events, 2008-2018**



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

Figure A2: **Effect of prescribed wage increases on bargaining council firms: Sparse specification**



(a) **Log firm wage (p50)** (b) **Log firm size**

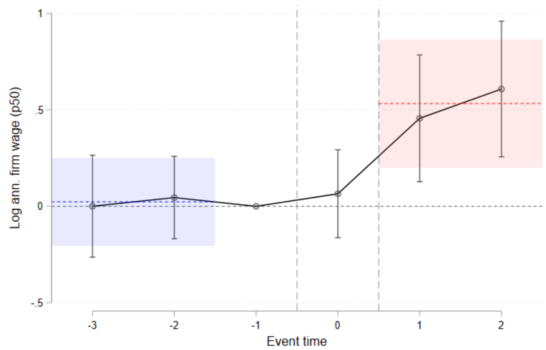
Notes. The figure shows estimates based on the main event-study specification assessing spillover effects from 47 bargaining council wage increases (see section 3.1), with fewer controls. The sample is of stacked events, restricted to balanced firms with more than 10 workers in the pre-period, and excludes covered firms. The main regressor is the pre-treatment flow of workers to bargaining council firms, with identification within-firm, and only fixed effects for event by location by time (no controls for pre-treatment log wage or log firm size or growth). Regressions are run at the unweighted firm-level, and standard errors are clustered by industry-location.

Table A1: **Tests for endogeneity of prescribed wage increases: Lagged coefficients**

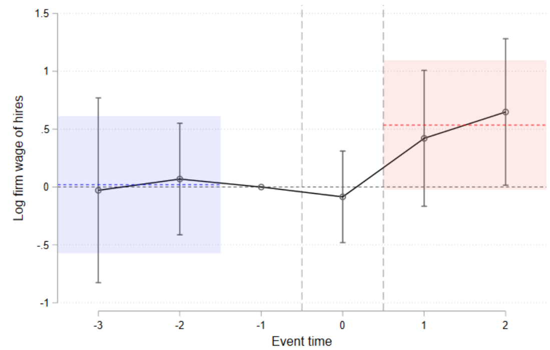
	(1)	(2)	(3)	(4)
Wage (mean)	0.009		-0.117	
	(0.017)		(0.138)	
Firm size	0.002		-0.011	
	(0.080)		(0.093)	
Profit (pp)	0.000	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Value added (pp)	-0.023	-0.019	0.040	0.012
	(0.052)	(0.015)	(0.082)	(0.078)
Obs	128,324	129,609	5,843	5,854
R-squared	.21	.21	.10	.10
Panel	Y	Y		
Event study			Y	Y

Notes. The table shows the coefficients from a regression of lagged changes in the log of each variable in rows, on the outcome of the change in logged prescribed wages. The table shows the variables are not significant predictors of the prescribed wage changes. The samples in columns 1 and 2 are from the firm panel of all prescribed wage changes, and the samples in columns 3 and 4 are from the event study. Both samples are limited to covered firms, all regressions contain fixed effects for industry by location (as in the main regression specifications), and standard errors are clustered by industry-location.

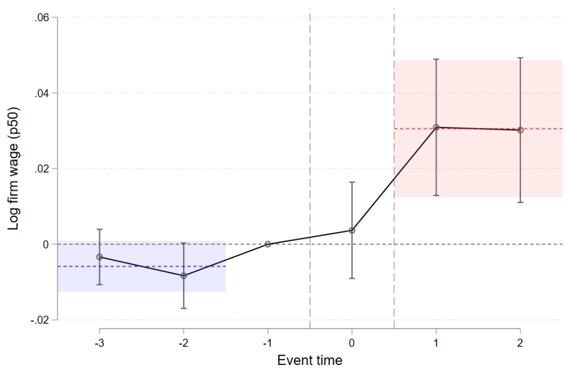
Figure A3: **Effect of prescribed wage increases on wages of bargaining council firms**



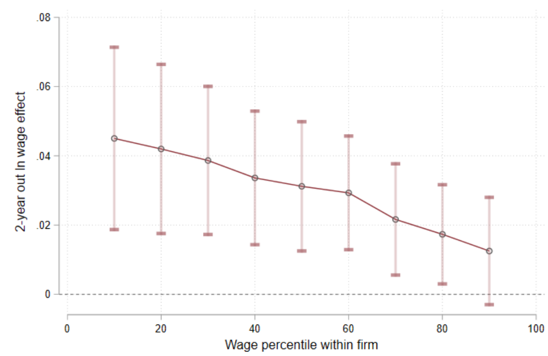
(a) **Annualized wages**



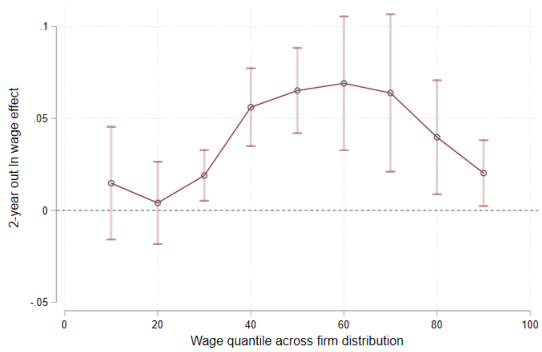
(b) **Wage of new hires**



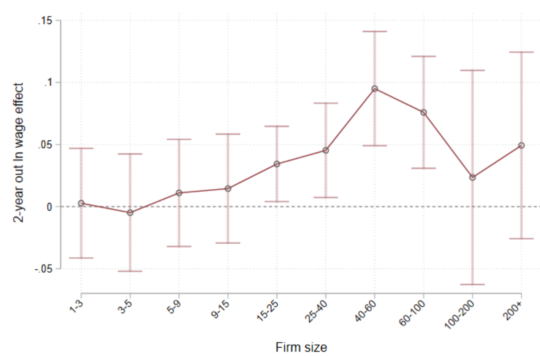
(c) **Absolute wage effect**



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



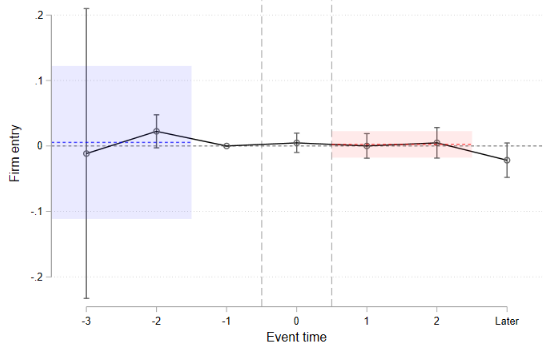
(e) **By quantile across firm wages**



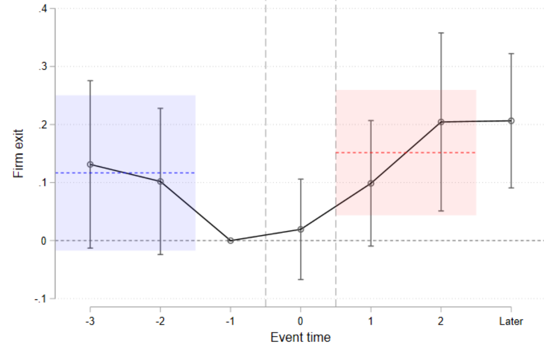
(f) **By quantile across firm size**

Notes. The figure shows estimates based on the main event-study specification assessing direct effects on covered firms from 47 bargaining council wage increases (see section 3.1). The sample is of stacked events, restricted to balanced firms with more than 10 workers in the pre-period, and includes noncovered control firms. The main regressor is an indicator for covered firms, with identification from variation within firms over time and a rich set of time-varying fixed effects. All outcomes are in logs, and scaled by the magnitude of the prescribed wage increase such that coefficients are interpreted as elasticities with respect to the prescribed wage. Regressions are run at the unweighted firm-level, and standard errors are clustered by bargaining council. The event-period outcomes for Panels A to C are respectively: annualized median wages, wages of new hires, and the absolute wage changes (not normalized by the prescribed wage increase). Panels D-F show the event year 2 coefficients for separate regressions by deciles categories of within-firm wages, across-firm wages and firm-size respectively.

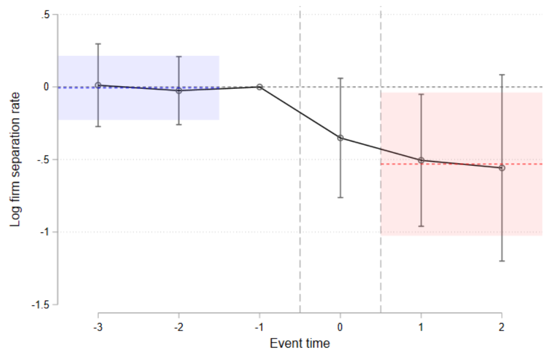
Figure A4: **Effect of prescribed wage increases on other outcomes in bargaining council firms**



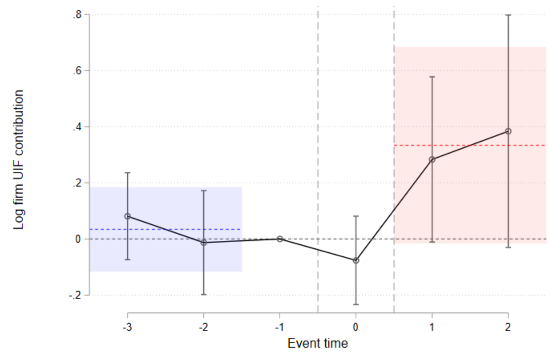
(a) **Firm entry**



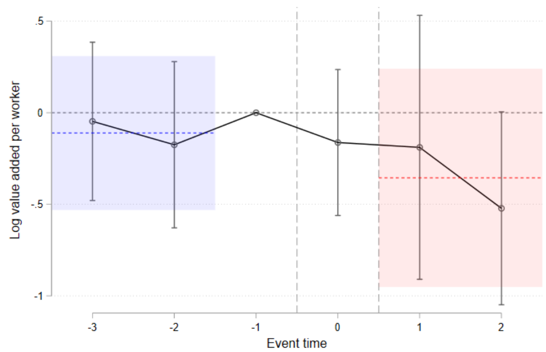
(b) **Firm exit**



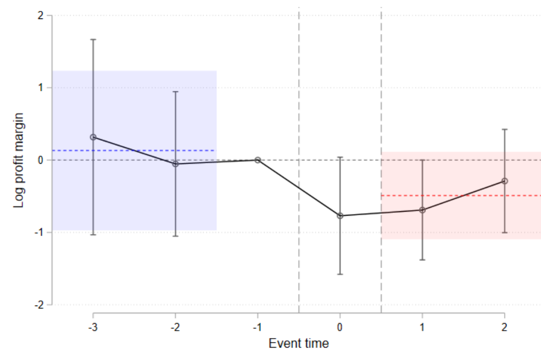
(c) **Separations**



(d) **UI co-payments**



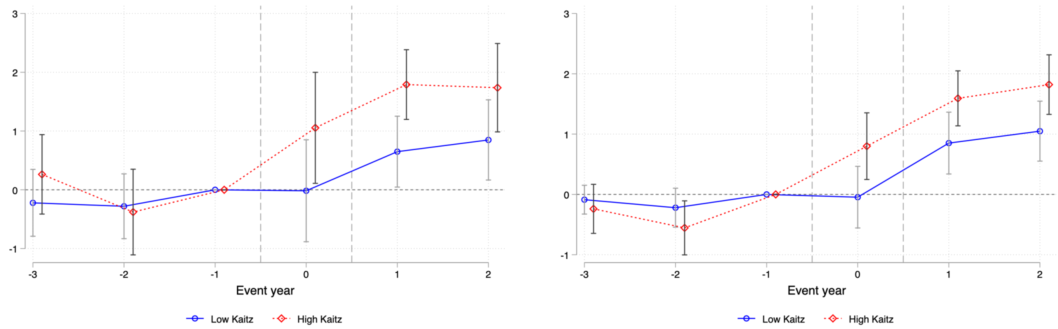
(e) **Value added per worker**



(f) **Profit margin**

Notes. The figure shows estimates based on the main event-study specification assessing direct effects on covered firms from 47 bargaining council wage increases (see section 3.1). The sample is of stacked events, restricted to balanced firms with more than 10 workers in the pre-period, and includes noncovered control firms. The main regressor is an indicator for covered firms, with identification from variation within firms over time and a rich set of time-varying fixed effects. All outcomes are in logs, and scaled by the magnitude of the prescribed wage increase such that coefficients are interpreted as elasticities with respect to the prescribed wage. Regressions are run at the unweighted firm-level, and standard errors are clustered by bargaining council. Panels A-F show the event-study outcomes respectively as follows, all in logs: firm entry, firm exit, proportion of a firm's workers that separate in each year, Unemployment Insurance (UI) co-payments, value added per worker, and net profits over value added. Firm entry is defined as an indicator equal to 1 if the firm is not present in the data in the 2 prior years, and is present in the data in the next 3 years; similarly, firm exit is defined as an indicator equal to 1 if the firm is present in the data for 3 years, but not present for the next 2 years.

Figure A5: **Effect of prescribed wage increases on wages of bargaining council firms: Within-CBA specification**



(a) **Log firm wage (p25)** (b) **Log firm wage (p50)**

Notes. The figure shows estimates from the within-CBA approach to evaluating direct effects on covered firms (see section 3.1). The approach follows the main specification, with additional regressor term as an interaction between covered firms and high Kaitz areas. High Kaitz areas are defined as broad industry-regions with above-median (within each event) ratio between the bargaining council wage and median wage. As in the main specification, identification is within-firm, and includes a rich set of fixed effects interacted with event-time. All outcomes are in logs. The regression is run at the unweighted firm-level, and restricted to balanced firms with more than 10 workers in the pre-period which are part of national bargaining councils (CBAs). Standard errors are clustered at the level of bargaining council treatment by event.

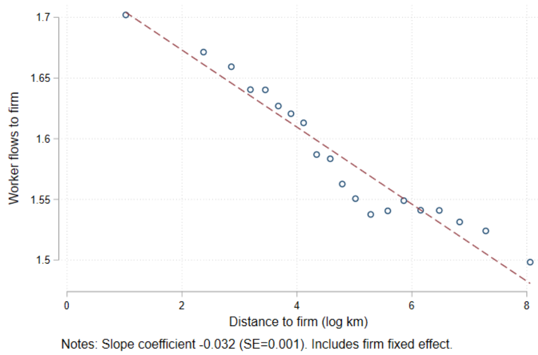
Table A2: **Effects of prescribed wage increases on covered firms: Firm panel**

	(1) Wage (p25)	(2) Wage (p50)	(3) Wage p80-p20	(4) Firm size	(5) Profit
Lagged effect	-0.053	-0.052	0.041	-0.002	0.088
	(0.074)	(0.060)	(0.076)	(0.028)	(0.202)
Main effect	0.319**	0.215**	-0.107	-0.153	-0.401*
	(0.134)	(0.100)	(0.092)	(0.102)	(0.236)
Obs (th.)	120	120	120	120	27
Treated firms (th.)	31	31	31	31	9.9

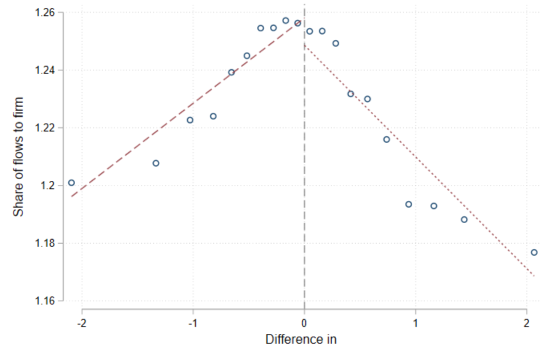
Notes. The table shows estimates of direct effects on covered firms (see section 3.3). The main difference with table 2 is that the sample is of the firm panel (i.e. not the stacked event-study), and so includes more prescribed wage changes (296), but many of which are small with contaminated pre-periods. The sample is restricted to covered and noncovered firms with non-missing outcomes in surrounding years and more than 10 workers in the lagged periods. The main regressor is the change in prescribed wages interacted with an indicator for covered firms, and fixed effects interacted with event time for location, and levels and growth in lagged wages and employment. Regressions are run at the unweighted firm-level, and standard errors are clustered by bargaining council. All outcomes are in logged changes. Row 1 shows the lagged placebo coefficient, and row 2 shows the main effect which is the elasticity of firm wage changes to prescribed minimum wage changes.

B Appendix: Spillover effects

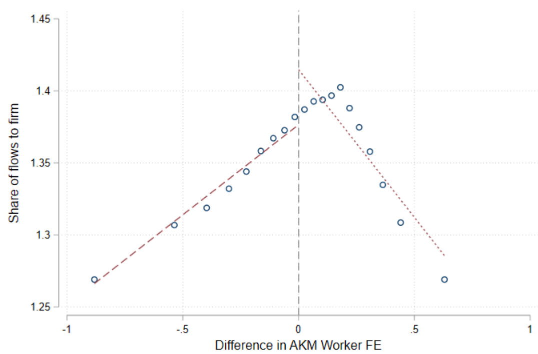
Figure B1: **Firm-to-firm flows and measures of distance**



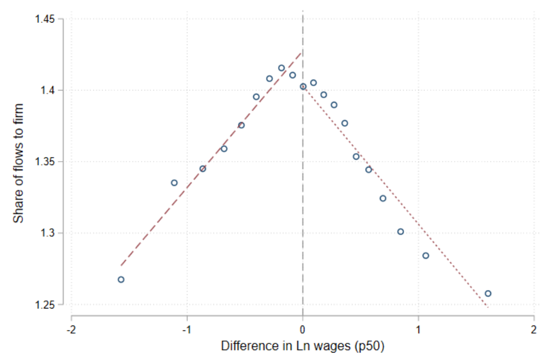
(a) **Spatial distance**



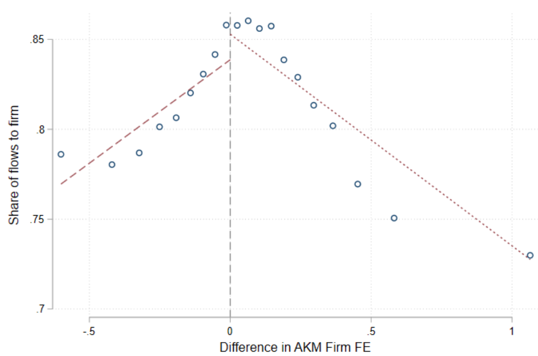
(b) **Value added per worker**



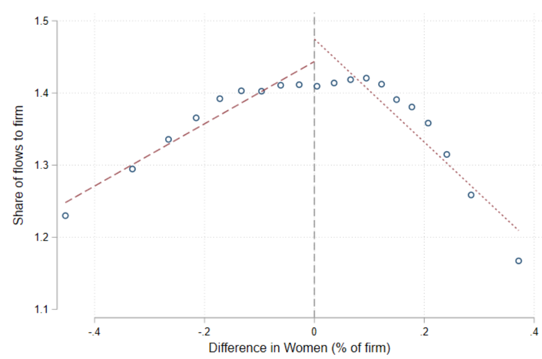
(c) **Worker AKM FE**



(d) **Firm wage (median)**



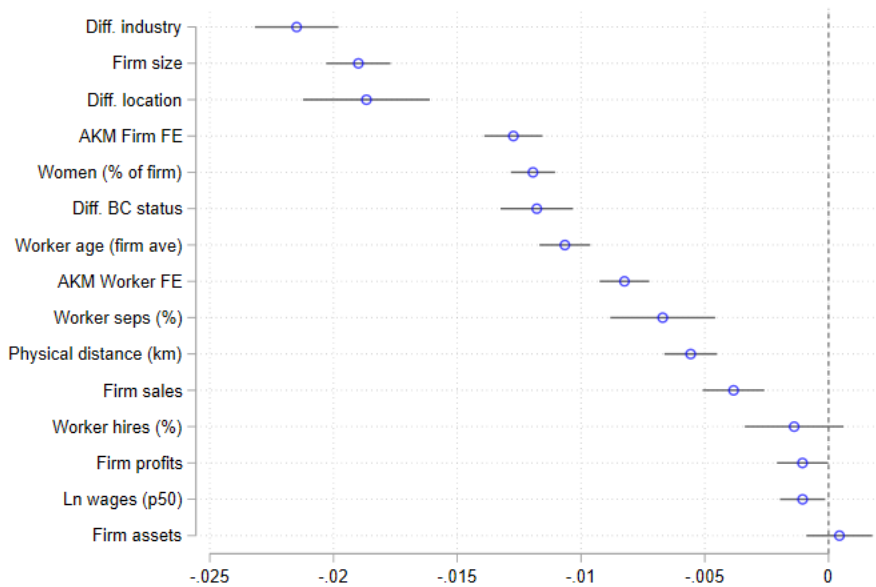
(e) **Firm wage premium**



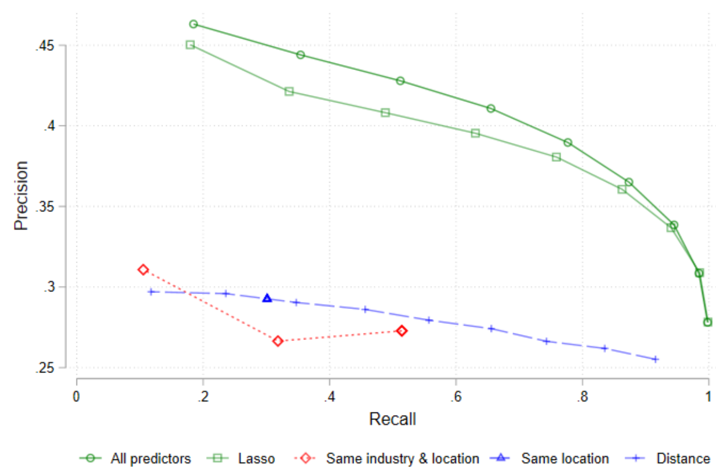
(f) **Proportion of women**

Notes. For each firm, total worker flows from each own firm to each other firm is averaged as a proportion of own firm flows. Fixed effects for each firm are included. Difference is measured as the own-firm characteristic minus other-firm's characteristic. Worker flows between firms are shown by the respective panels for: (a) spatial distance, (b) difference in value added per worker, (c) difference in average AKM worker fixed effects, (d) difference in firm median wage, (e) difference in AKM firm wage premium between firms (this additionally includes controls for the AKM worker premium), and (f) difference in firm proportion of women between firms.

Figure B2: **Predictors of firm-to-firm worker flows**



(a) **Predictor coefficients**



(b) **Precision and recall**

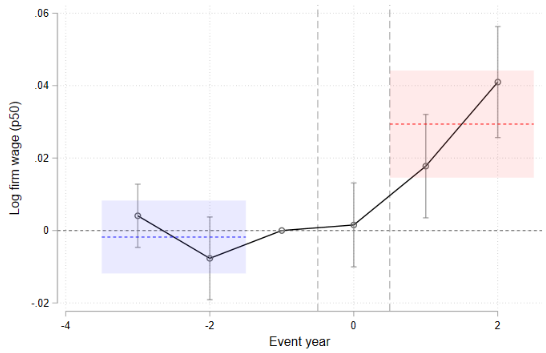
Notes. For each firm, total worker flows to each other firm is averaged as a proportion of all flows. Panel A presents the coefficients from a single regression of firm-to-firm flows on the deviation in firm covariates from the primary firm average. Fixed effects for each firm are included. Distance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

Table B1: **Effects of prescribed wage increases on spillover firms: Additional controls and specifications**

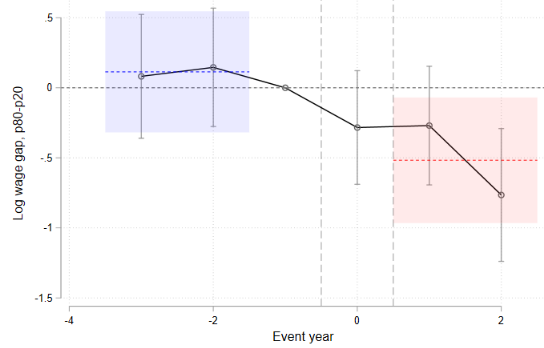
	(1) Wage (p50)	(2) Firm size	(3) Profit
Main effect	0.874***	-0.149	-2.860***
	(0.205)	(0.239)	(0.963)
Add. controls			
Industry	0.885***	0.041	-3.814***
	(0.242)	(0.269)	(1.274)
Churn	1.017***	-0.425*	-2.441**
	(0.242)	(0.254)	(1.070)
Flows	0.585*	-0.745*	-3.018**
	(0.338)	(0.437)	(1.464)
Network	0.706***	0.088	-2.927***
	(0.201)	(0.238)	(0.967)
Trade links	1.896***	0.993	3.934
	(0.624)	(0.607)	(2.407)
Alt. regressors			
Split IV	0.851***	-0.285	-3.727**
	(0.300)	(0.387)	(1.713)
OLS	0.525***	-0.089	-1.711***
	(0.133)	(0.155)	(0.595)
Trade links	0.360**	-0.258	0.760
	(0.160)	(0.188)	(0.789)
Placebo	-0.065	-0.089	1.003*
	(0.127)	(0.144)	(0.532)

Notes. The figure shows estimates based on the main event-study specification assessing spillover effects from 47 bargaining council wage increases (see section 4.2). The sample is of stacked events, restricted to balanced firms with more than 10 workers in the pre-period, and excludes covered firms. The baseline regressor is the pre-treatment flow of workers to covered firms. Identification is within-firm, and the baseline includes a rich set of fixed effects interacted with event-time. All outcomes are in logs, and scaled by the magnitude of the prescribed wage increase such that coefficients are interpreted as elasticities with respect to the prescribed wage. Regressions are run at the unweighted firm-level, and standard errors are clustered by industry-location. Each estimate is the final period effect on firm median wages for the relevant specification. "Main" is the main set of results (280,000 obs). As alternative controls interacted with event-time in the main specification: "Industry FE" adds industry fixed effects (280,000 obs), "Worker churn" adds the churn or turnover rate of workers i.e. excess transitions (197,000 obs), "Predicted flows" adds the predicted flow of workers to bargaining councils based on observable characteristics (99,000 obs), and "Network characteristics" adds average characteristics from the relevant industry-location such as time-varying wages and firm size (280,000 obs). As alternative regressors: "Split IV" instruments the pre-period worker flows in a randomized sample with the complement sample (278,000 obs); "OLS" omits the instrument, but otherwise has the same specification (279,000 obs); "Trade flows" uses the proportion of trade between firms as given by sectoral Input-Output tables (64,000 obs); and "Scrambled flows (placebo)" randomly re-assigns worker flows to firms in the main specification (275,000 obs). The observations are as indicated in brackets for the wage and firm size outcomes, but about a quarter as much for the profit outcomes.

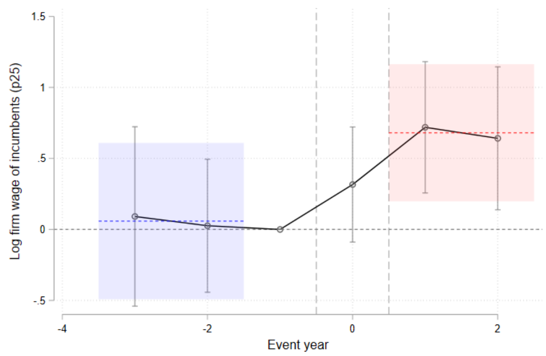
Figure B3: **Additional spillover effects on wages**



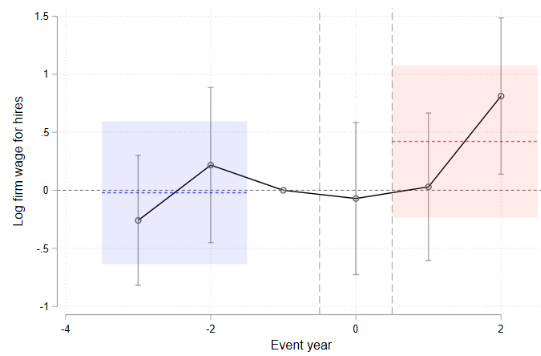
(a) **Raw wages (p50)**



(b) **Within-firm p80-p20 wage gap**



(c) **Incumbent wages**



(d) **New hire wages**

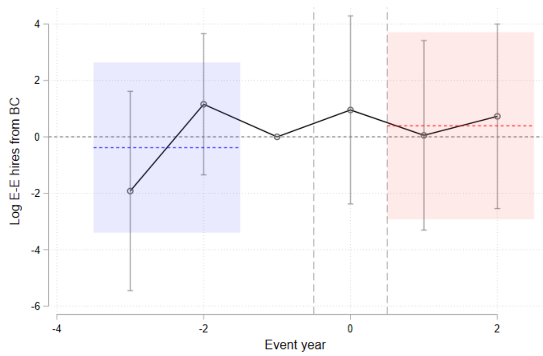
Notes. The figure shows estimates based on the main event-study specification assessing spillover effects from 47 bargaining council wage increases (see section 4.2). The sample is of stacked events, restricted to balanced firms with more than 10 workers in the pre-period, and excludes covered firms. The main regressor is the pre-treatment flow of workers to bargaining council firms, with identification within-firm, and a rich set of fixed effects interacted with event-time. All outcomes are in logs, and scaled by the magnitude of the prescribed wage increase such that coefficients are interpreted as elasticities with respect to the prescribed wage. Regressions are run at the unweighted firm-level, and standard errors are clustered by industry-location. The outcomes are as follows by panel: (a) raw wage effects (i.e. not scaled to the wage agreements), (b) the wage gap between the 80th and 20th percentiles within firms, (c) wages of incumbent workers, and (d) wages of new hires.

Table B2: **Effects of prescribed wage increases on spillover firms: Firm panel**

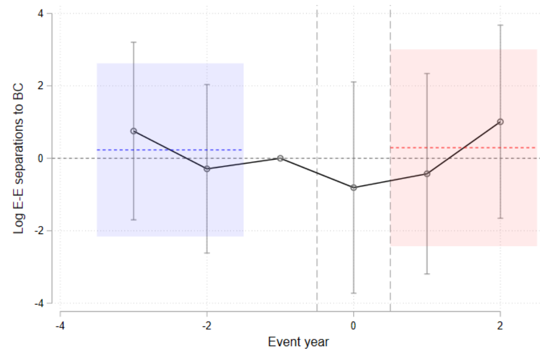
	(1) Wage (p25)	(2) Wage (p50)	(3) Wage p80-p20	(4) Firm size	(5) Profit
Lagged effect	-0.088	-0.064	0.054	0.009	-0.009
	(0.068)	(0.050)	(0.074)	(0.024)	(0.231)
Main effect	0.188*	0.142*	-0.149*	-0.281***	-0.553**
	(0.104)	(0.079)	(0.085)	(0.087)	(0.279)
Cross-wage elasticity	0.590	0.659	-0.692	-1.304*	-2.567
	(0.409)	(0.478)	(0.509)	(0.729)	(1.762)
Obs (th.)	120	120	120	120	24
Firms (th.)	35	35	35	35	10

Notes. The table shows estimates of spillover effects on noncovered firms (see section 4.4). The main difference with table 3 is the sample is of the firm panel (i.e. not the stacked event-study), and so includes more prescribed wage changes, but many of which are small with contaminated pre-periods. The sample is restricted to firms with non-missing outcomes in surrounding years, more than 10 workers in the lagged periods, and excludes covered firms. The main regressor is the flow of workers to bargaining council firms, scaled by the magnitude of the prescribed wage increase, and fixed effects interacted with event time for location, and levels and growth in lagged wages and employment. All outcomes are in logged changes. Row 1 shows the pre-period placebo coefficient, row 2 shows the main effect of firm wage changes to the bargaining council prescribed minimum wage changes, and row 3 shows the cross-wage elasticity as the adjusted effect based on row 2 and the covered sector wage estimates from table A2 (standard errors use delta method). Regressions are run at the unweighted firm-level, and standard errors are clustered by industry-location.

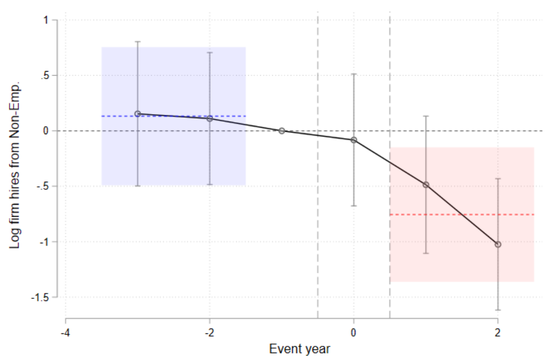
Figure B4: **Spillover effects on other outcomes**



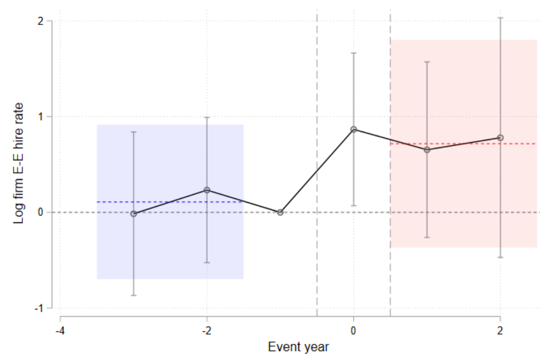
(a) **E-E hires from BC firms**



(b) **E-E separations to BCs**



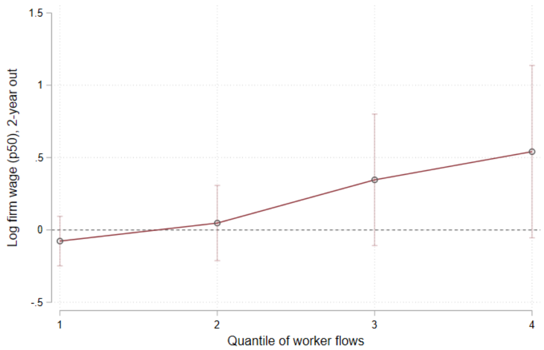
(c) **Firm N-E hires**



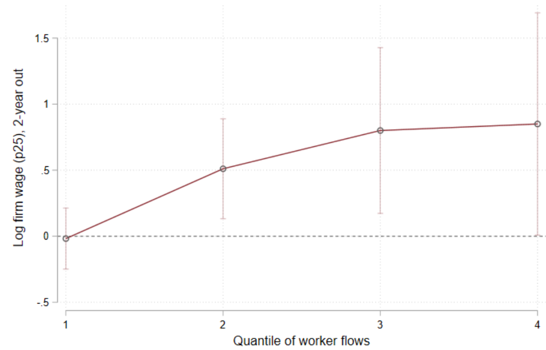
(d) **Firm E-E hires**

Notes. The figure shows estimates based on the main event-study specification assessing spillover effects from 47 bargaining council wage increases (see section 4.2). The sample is of stacked events, restricted to balanced firms with more than 10 workers in the pre-period, and excludes covered firms. The main regressor is the pre-treatment flow of workers to bargaining council firms, with identification within-firm, and a rich set of fixed effects interacted with event-time. All outcomes are in logs, and scaled by the magnitude of the prescribed wage increase such that coefficients are interpreted as elasticities with respect to the prescribed wage. Regressions are run at the unweighted firm-level, and standard errors are clustered by industry-location. The outcomes are (a) log employment-to-employment separations from spillover to covered firms, (b) log employment-to-employment hires from covered firms to spillover firms, (c) log hires from non-employment, and (d) log hires from employment.

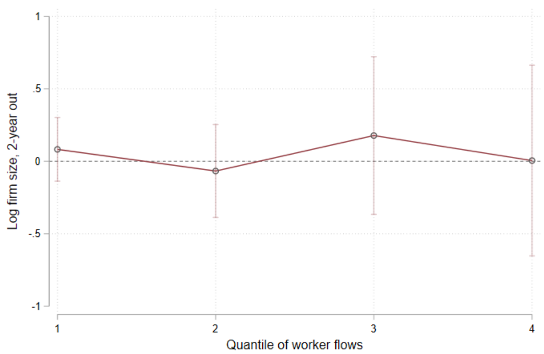
Figure B5: **Spillover wage effects by connectivity to bargaining council**



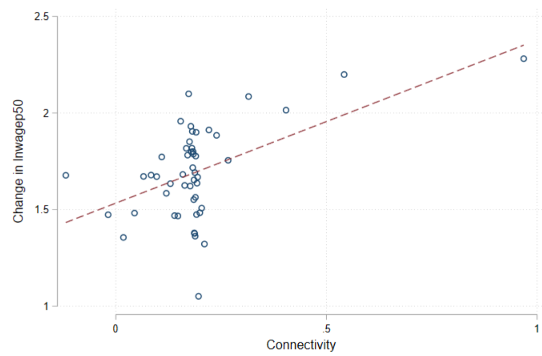
(a) **Spillovers by quantile: Wages (p50)**



(b) **Spillovers by quantile: Wages (p25)**



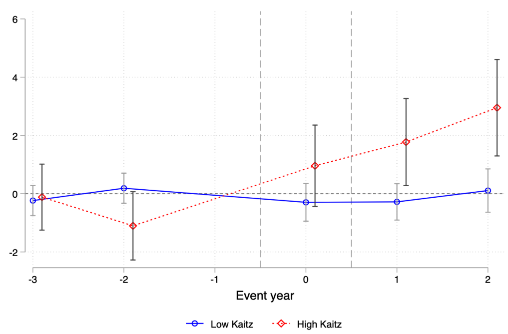
(c) **Spillovers by quantile: Firm employment**



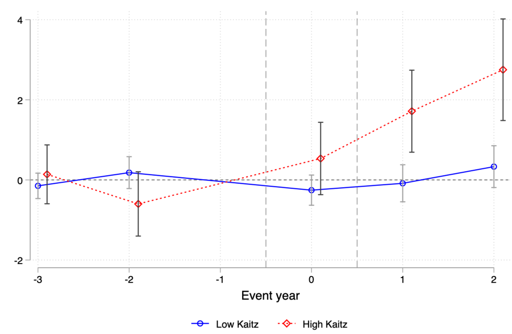
(d) **Binscatter of changes in median wages**

Notes. The data consist of noncovered firms from 47 bargaining council wage increases using the main event-study panel (see section 4.2). Panels A-C show coefficients from the main spillovers specification, except where the regressor is a binary for the quantile of worker flows, for the respective outcomes of wage percentile 50, wage percentile 25 and firm employment. In this specification, the lowest quantile compares low flow firms to other low flow firms, whereas the highest quantile compares high flow firms to low flow firms. Panel D shows a binned scatterplot of changes in wages of noncovered firms (relative to relevant agreements) against the degree of flows with the relevant bargaining council. The residual of a regression of split-sample measures of connectivity is included as a control, as an implementation of the control function approach.

Figure B6: **Effect of prescribed wage increases on wages of noncovered firms: Within-CBA specification**



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



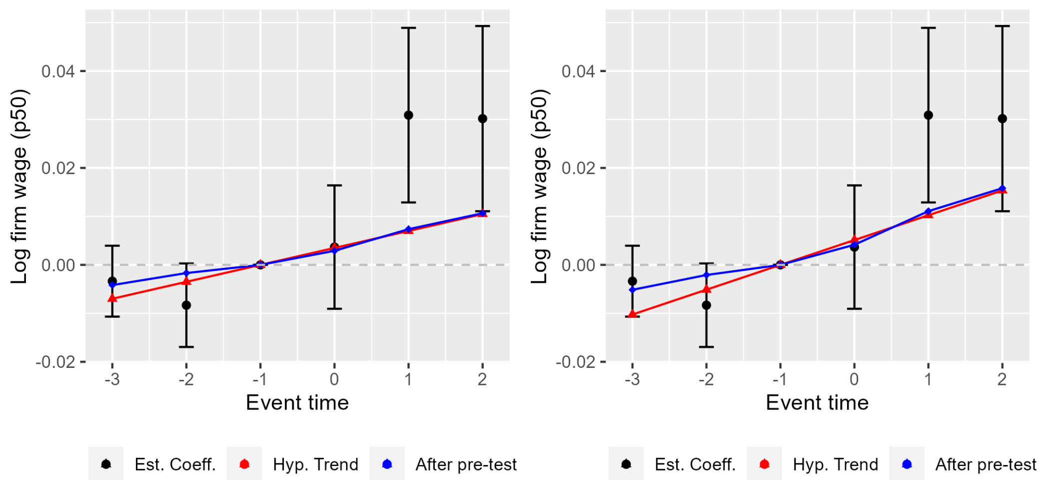
(b) **Log firm wage (p50)**

Notes. The figure shows estimates from the within-CBA approach to evaluating spillover effects on noncovered firms (see section 4.2). The approach follows the main spillovers specification, with additional regressor term as an interaction between noncovered firms and high Kaitz areas. High Kaitz areas are defined as broad industry-regions with above-median (within each event) ratio between the bargaining council wage and median wage. As in the main specification, identification is within-firm, and includes a rich set of fixed effects interacted with event-time. All outcomes are in logs. The regression is run at the unweighted firm-level, and restricted to balanced firms with more than 10 workers in the pre-period which are part of national bargaining councils (CBAs). Standard errors are clustered at the level of bargaining council treatment by event.

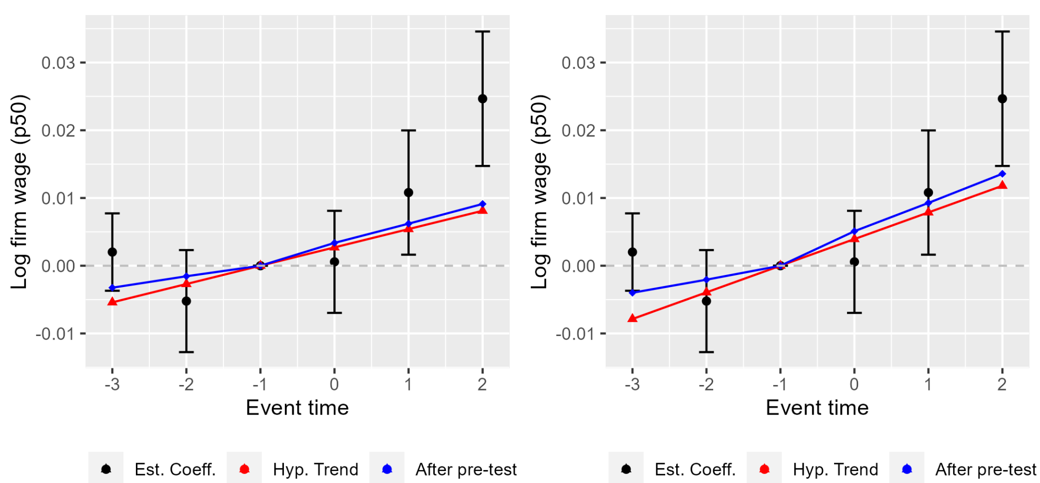
C Appendix: Additional findings

C.1 Robustness

Figure C1: **“Honest pretrends” sensitivity checks**



(a) **Bargaining council firm wages: 50% and 80% power**



(b) **Spillover wages: 50% and 80% power**

Notes. The figure shows sensitivity checks on the main estimates from the event-study evaluating direct treatment effects on bargaining council firms (see section 3.1) as well as spillover effects on noncovered firms (see section 4.2). The estimates correspond to the main specification for the bargaining council estimates, and the OLS specification for the spillover estimates (the Roth package requires the covariance matrix, i.e. simultaneous estimation of the event-period effects rather than separate regressions as in the IV specification). The figures show a red line which represents a hypothetical pre-trend, and a the blue line which represents the hypothetical pre-trend conditional on passing the pre-trend test. These are constructed with 50% and 80% probability of being rejected, respectively. Panel A shows the sensitivity for bargaining council wage estimates. Panel B shows sensitivity for spillover wage estimates.

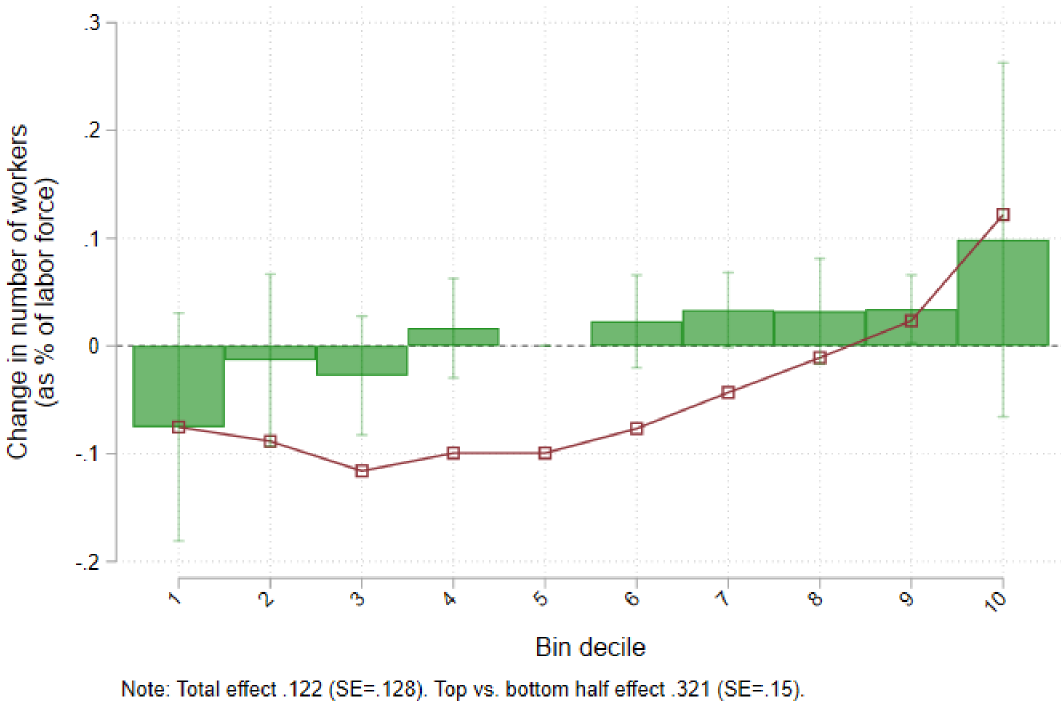
Table C1: **Alternative estimates for effects of prescribed wage agreements**

	(1)	(2)	(3)	(4)	(5)	(6)
	Wage (p50)		Firm size		Profit	
	Direct	Spillover	Direct	Spillover	Direct	Spillover
Main effect	0.751***	0.874***	-0.200	-0.149	-0.338	-2.860***
	(0.235)	(0.205)	(0.202)	(0.239)	(0.566)	(0.963)
Alt. samples						
No pre-CBA	0.924***	0.649**	-0.147	-0.023	-0.603	-3.698***
	(0.354)	(0.256)	(0.318)	(0.287)	(0.489)	(1.218)
Higher threshold	0.764***	0.934***	-0.289	-0.631**	-0.343	-1.990**
	(0.236)	(0.231)	(0.188)	(0.268)	(0.580)	(0.991)
Low-wage firms	1.006***	1.246***	-0.402**	-0.305	0.099	-2.651*
	(0.262)	(0.301)	(0.160)	(0.309)	(0.854)	(1.491)
National CBAs	1.205***	0.972***	-0.384	-0.201	-0.618	-2.729***
	(0.249)	(0.223)	(0.256)	(0.259)	(0.743)	(0.976)
Alt. specifications						
Within CBAs	0.988***	2.004***	-1.436***	-0.942	-1.007	-1.183
	(0.348)	(0.655)	(0.382)	(0.765)	(1.285)	(2.222)
Size weights	0.697*	1.421**	-0.506	-2.464***	-1.694**	-3.056
	(0.421)	(0.588)	(0.496)	(0.754)	(0.786)	(2.262)
Double robust	1.189***	0.971*	-0.346	-1.285**	-1.472**	-7.529**
	(0.333)	(0.498)	(0.243)	(0.576)	(0.656)	(3.208)
Rep. weights	0.748***	0.874***	-0.206	-0.149	-0.342	-2.860***
	(0.235)	(0.205)	(0.201)	(0.239)	(0.568)	(0.963)
Event-specific	0.781***	0.992***	-0.157	0.022	-0.295	-2.437**
	(0.237)	(0.232)	(0.204)	(0.250)	(0.571)	(1.201)

Notes. The table shows estimates based on the main event-study specification from 47 bargaining council wage increases, assessing direct effects on covered firms (see section 3.1), as well as spillover effects on noncovered firms (see section 4.2). The sample is of stacked events, restricted to balanced firms with more than 10 workers in the pre-period. Identification within-firm, and includes a rich set of fixed effects interacted with event-time. Regressions are run at the unweighted firm-level. Each estimate is the final period (two-year-out) effect on log firm median wages for the relevant specification, relative to the relevant bargaining council prescribed wage change. Row headings are as follows, with thousands of treated firms in the covered regression and thousands of firms in the spillover regression shown respectively in brackets. "Main" is the main set of results (17.6 and 46.8). As alternative samples: "No pre-CBAs" excludes events of bargaining councils which had a large change in prescribed wages in event years -4 or -5 (11.3 and 25.3); "Higher threshold" restricts to events of bargaining councils with prescribed wage changes of at least 3.5% (17.1 and 37.2); "Low-wage" restricts to firms with above-median proportion of pre-period workers who were paid below the median (8.6 and 22.8); "National CBAs" restricts to collective bargaining councils covering national industries (13.4 and 34.3). As alternative specifications: "Within CBAs" uses national changes in prescribed wages to compare regions with low vs high median firm wages (13.3 and 20.6); "Size-weight" weights the main specification by firm size (10.4 and 16.8); "Double robust" weights the main specification by the propensity score, estimated on pre-event firm characteristics (15.7 and 13.0); "Rep. weights" re-weights the main specification to be representative of the full firm panel (17.6 and 46.8); and "Event specific cont" interacts all controls in the main specification with each event (17.6 and 37.0). The observations are as indicated in brackets for the wage and firm size outcomes, but about a quarter as much for the profit outcomes.

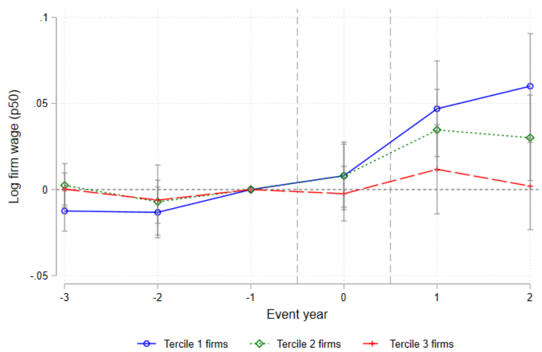
C.2 Re-allocation

Figure C2: **Event-study effects on number of workers, by decile of value added per worker**

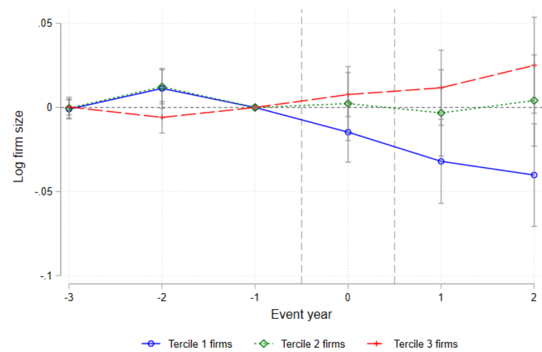


Notes. The main specification and sample for effects on bargaining council firms are used (see equation 1), with the following modifications: Event-periods are collapsed into the pre- vs post-period, and the primary treatment indicator is interacted with an indicator for each decile of pre-period firm 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 labour 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.

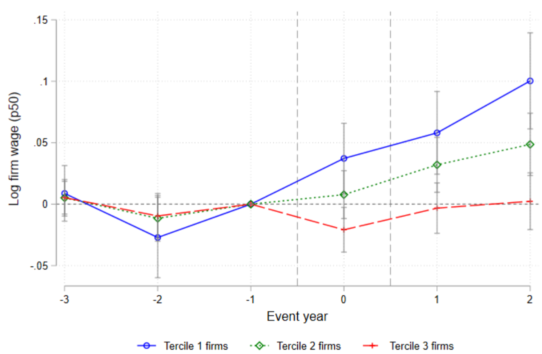
Figure C3: **Re-allocation effects, by pre-event firm wage**



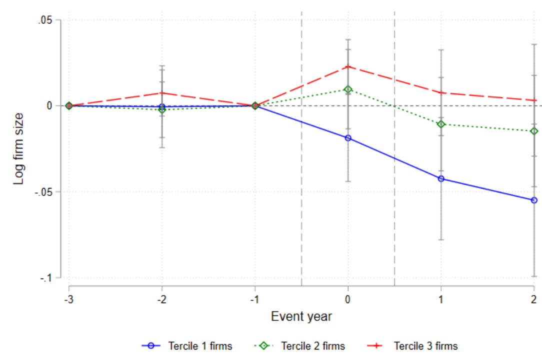
(a) **BC firms: Wages**



(b) **BC firms: Size**



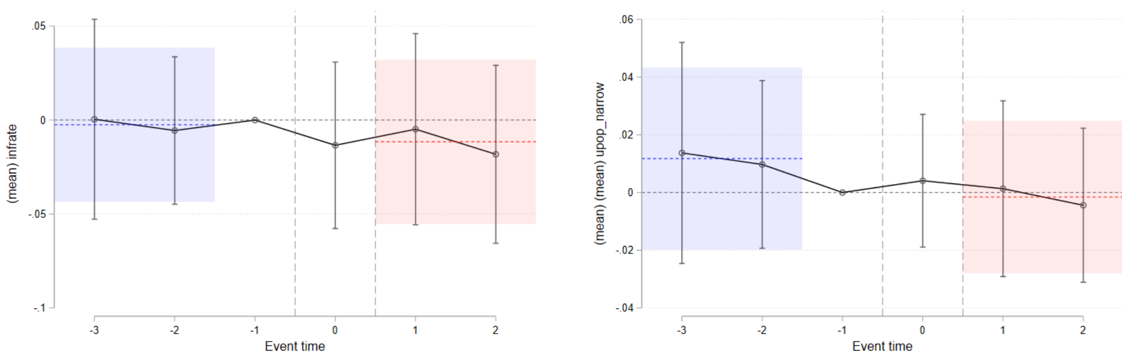
(c) **Spillover firms: Wages**



(d) **Spillover firms: Size**

Notes. The figure shows estimates from the event-study evaluating direct treatment effects on bargaining council firms (see section 3.1) as well as spillover effects on noncovered firms (see section 4.2). In each specification, the treatment coefficients are interacted by tercile of firm pre-event wage. Coefficients plotted are for the each event-period effect. Panel A shows the effects for bargaining council firms on wages and firm size respectively. Panel B shows the effects for spillover firms, on wages and firm size respectively. The own-wage elasticity for the bottom tercile is large and negative at -0.67 compared to the middle tercile which is positive at 0.14. A similar pattern emerges for spillover firms.

Figure C4: **Aggregate employment effects of bargaining council wages increases**

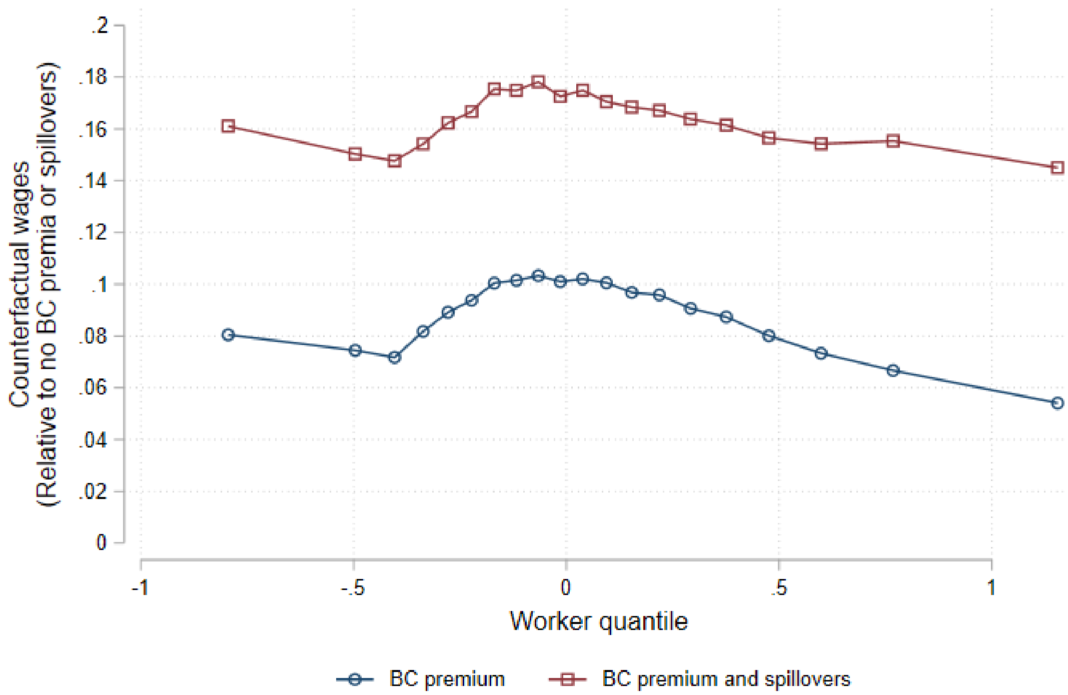


(a) **Informality rate** (b) **Unemployment rate**

Notes. Informality and unemployment are merged in from the Quarterly Labour Force Surveys 2008-2018 to the main event study dataset. Samples are collapsed to the level of the municipality, and the main regression specification for direct effects is used (see section 3.1), with controls at the municipality level and treatment equal to the share of bargaining council firms in the municipality.

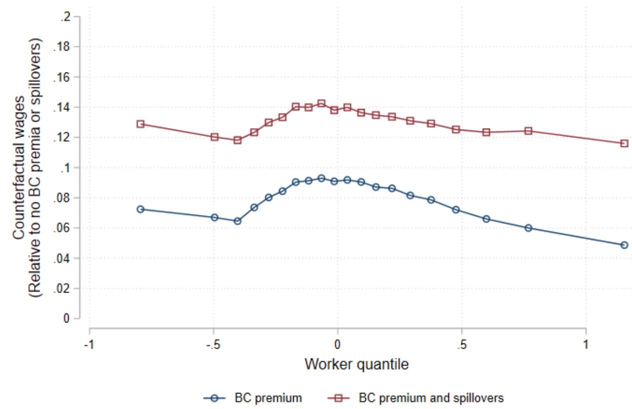
C.3 Microsimulations of aggregate labour market effects

Figure C5: **Microsimulation of prescribed wage effects on wage distribution**

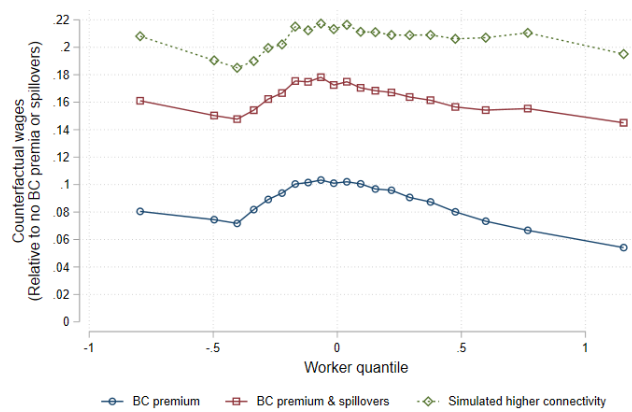


Notes. The figure simulates the wage effects of bargaining council wage agreements on the overall wage distribution, by quantile of the AKM worker fixed effect. The baseline of 0 represents the observed wage distribution in 2008. The blue line shows counterfactual wages based on adding in the bargained wage increases between 2008-2018 relevant to each covered worker, given that the estimated elasticity of wages to prescribed wage increases is close to 1. The red line shows counterfactual wages based on the blue line plus the implied spillovers relevant to noncovered workers, as estimated using the flows from each firm to bargaining councils along with an estimated cross-wage elasticity of 0.8.

Figure C6: **Simulated effect on firm wage distribution, additional variations**



(a) **Including employment effects**



(b) **Counterfactual of more equal flows**

Notes. The figure simulates the effect of bargaining councils by quantile of the AKM worker fixed effect. The baseline is observed wages in 2008. The blue line shows counterfactual wages based on adding in the bargained wage increases between 2008-2018 relevant to each covered worker, given that the estimated elasticity of wages to prescribed wage increases is close to 1. The red line shows counterfactual wages based on the blue line plus the implied spillovers relevant to noncovered workers, as estimated using the flows from each firm to bargaining councils along with an estimated cross-wage elasticity of 0.8. Compared to figure 9, Panel A includes employment effects by subtracting out the point estimates of the disemployment effects: the own-wage elasticities for bargaining council firms of -0.1 and for spillover firms of -0.2 are used, as discussed in section 3.2. Panel B adds a green line which shows a counterfactual as follows: Firms have worker flows to bargaining councils at least equal to the share of bargaining council workers in their own industry-location. This seems to be a reasonable benchmark if flows were drawn equally from firms across one's industry-location cell.

D Appendix: Theory

D.1 Logit static model

D.1.1 Further details of setup

As a more general form of the static setup, I allow the total labour allocated to all the covered set of firms K in the consideration set to be a free parameter n_k, which I assume varies positively with wages.[36] In the main text, I focus on the supply-constrained case, but here I present the more general case. I also explicitly incorporate the consideration set of outside options by restricting to the relevant labour market, denoted by S as in the term $ln(\sum_l^S w_l^\beta)$.[37] The modified probability of employment at firm j is $lnp_j = \beta ln(w_j) - ln(n_k + \sum_{l \neq k}^S w_l^\beta)$.

Taking logs, we can compute the firm labour supply elasticities:

$$\varepsilon_{jj}^n = \frac{\partial lnp_j}{\partial lnw_j} = \beta(1 - \frac{w_j^\beta}{n_k + \sum_{l \neq k}^S w_l^\beta}) = \beta(1 - p_j) \tag{11}$$

Since I assume labour allocated to k varies positively with wages for treated firms k, then $\varepsilon_{jk}^n = \frac{\partial lnp_j}{\partial lnw_k} < 0$. In the notation above, $\varepsilon_{jk}^n = -\frac{\partial n_k / \partial lnw_k}{n_k + \sum_{l \neq k}^S w_l^\beta}$, and $\partial n_k / \partial lnw_k > 0$. In the case where k is supply-constrained, then $\varepsilon_{jk}^n = -\beta(\frac{w_k^\beta}{\sum_l^S w_l^\beta}) = -\beta p_k$. These expressions for ε_{jj}^n and ε_{jk}^n match those in standard derivations of the logit, such as in Train (2009).

The firm's maximand and optimal wage-setting equation are as in the main text, leading to the following cross-firm wage elasticity:

$$\varepsilon_{jk}^w = \frac{dln(w_j)}{dln(w_k)} = \frac{-\varepsilon_{jk}^n \beta p_j}{\varepsilon_{jj}^n (1 + \varepsilon_{jj}^n)} - \varepsilon_{jk}^n \eta - \eta \beta \varepsilon_{jk}^w + \eta \beta \sum_{l \neq k} p_l \varepsilon_{lk}^w$$

$$\varepsilon_{jk}^w = \frac{dlnw_j}{dlnw_k} = \underbrace{\frac{1 + \eta\beta}{1 + \eta\beta p_k}}_{\text{multiplier}} (\underbrace{\frac{-\varepsilon_{jk}^n \eta}{1 + \eta\beta}}_{\text{shift in Residual LS}} + \underbrace{\frac{-\varepsilon_{jk}^n \beta p_j}{\varepsilon_{jj}^n (1 + \varepsilon_{jj}^n)(1 + \eta\beta)}}_{\text{change due to effect on } \varepsilon_{jj}^n}) > 0 \tag{12}$$

This expression for ε_{jk}^w makes the simplifying assumption that $\varepsilon_{jk}^w = \varepsilon_{lk}^w$, i.e. the cross-wage elasticity is similar for non-treated firms. This assumption is purely made for expositional clarity,

[36] This is *not* equal to the total employment in covered firms. Firstly, n_k is still subject to the aggregate labour supply constraint N, as a total share of covered firms would be. Secondly, labour allocated n_k may differ from actual employment at firm k. See below on queuing.

[37] Another way to present this is through an adjacency matrix S, where $S_{jl} = 1$ if the firm l is part of j's consideration set, and $S_{jl} = 0$ otherwise. Then the term $ln(\sum_l^J w_l^\beta)$ becomes $ln(\sum_l^J (w_l^\beta S_{jl}))$. The adjacency coefficients S_{jl} could also represent the *degree* of connectedness, i.e. a continuous measure as discussed in the dynamic context below. For simplicity of exposition, I just index over S here.

and is equivalent to assuming that the changes due to the effects of k's wage increase on the own elasticities are equal. In practice, the impact on ε^w_{jk} of the change due to the effect on ε^n_{jj} is orders of magnitude smaller than the effect due to the shift in the Residual LS. Secondly, for brevity in expression 12, I have omitted the second order effects on ε_{jj}. Again, this makes little difference in terms of magnitudes. The full expression with second order effects, instead of equation 12, is $\varepsilon^w_{jk} = \frac{x}{1+x}$, with $x = (\frac{-\varepsilon^n_{jk}\beta p_j}{\varepsilon_{jj}(1+\varepsilon_{jj})} - \varepsilon^n_{jk}\eta)$. In simulations across a range of value inputs, the difference between the expressions can only be seen at the fourth decimal.

D.1.2 Spillovers when firms are not labour supply constrained

Assumption on labour allocation effect on treated firms. My framework relies on a positive net labour allocation effect for treated firms to yield positive wage spillovers.[38] For an individual treated firm, the employment effect is positive as long as the prescribed wage is below the intersection point of the marginal cost and revenue curves (see figure 6). The cumulative net employment effect will depend on how binding the wage agreement is relative to firm productivity for the mass of firms, which will vary by agreement and firm type. Insofar as low productivity firms face employment losses, they will tend to temper the wage increases through the classic union spillover mechanism, i.e. wages are pushed up in the covered sector but labour is displaced to the noncovered sector, which pushes down wages there (Freeman and Medoff, 1981).

Incorporating job queuing. Positive wage spillovers in the noncovered sector are still possible in the case of a negative net employment effect on treated firms, if we allow for job queuing. We may modify the model as follows. At the beginning of the period, firms post their desired wage and employment (unconstrained or by wage agreement), subject to the setup in section 5 of labour supply constraints and demand constraints, and knowledge of the distribution of workers' idiosyncratic preferences. Next, workers choose a firm, with the knowledge of their own idiosyncratic utility as well as each firm's desired wage and employment. There is no on-the-job search, and a worker may choose to go to a firm where the worker knows there are more workers that jobs, i.e. queuing, such that they face a risk of unemployment. Finally, firms randomly choose workers to fill its desired employment slots. If firms are not on their supply constraint, then while the chosen

[38] This may be surprising, and partly reflects modeling choices. For example, if the employment allocation remains the same, but wages rise in a set of firms, one may still expect spillover wage responses through changes in norms or to prevent dynamic selection on unobserved ability. However, in this model, only quantities matter for competition. We can use the concept of the "shadow wage" Berger, Herkenhoff, and Mongey (2022b), i.e. the wage required to attract a given quantity of workers, which diverges from the actual wage for demand constrained firms. When firm k increases its wage but is demand-constrained, then some workers would be attracted to firm k who would not otherwise have been attracted at the corresponding shadow wage, because of the high offered wage, despite a relatively low idiosyncratic utility for firm k. As a result, after a random draw by firm k from the attracted workers, the residual labour supply of workers has a slightly different draw of idiosyncratic utilities to what it would have been otherwise. However, since idiosyncratic utility is uncorrelated across firms, on average the *distribution* of idiosyncratic utilities is the same as before the wage increase, and firm decisions are unaffected, i.e. remaining quantities are the same.

workers will be paid at the posted wage, the rest of the workers in the queue will be unemployed.

This is illustrated in figure D1, in the case of a minimum wage on a covered sector firm. I focus on the case of a demand-constrained firm, since queuing does not occur at supply constrained firms. Faced with a minimum wage lnw_{k1}, a demand-constrained firm chooses employment $p_{k1}N$. However, observing the worker supply at this wage, many more workers are willing to join the firm but will be rejected. Given the timing of wage-posting followed by labour allocation and then selection, many workers will trade a risk of rejection for the higher wage; in figure D1, this is represented by a point slightly to the right and below of (p_{k1}, w_{k1}) where the expected wage is equal. The curve of equal expected wage is traced out by the Expected Wage curve, $lnw_E = lnw_{k1} + ln(p_{k1}N) - ln(pN)$ where lnw_E is the expected wage at (p_{k1}, w_{k1}). Workers choose the point of intersection between the Residual LS curve and the Expected Wage curve, since further to the right no more workers are willing to work at the corresponding expected wage. The number of workers that choose firm k is therefore p_{kS}, given by $ln(p_{kS}N) = \frac{1}{1+\beta}(\beta lnw_{k1} + \beta ln(p_{k1}N) + ln(N) - ln(\sum_l^S w_l^\beta)$ and the corresponding expected wage on the Residual LS. Once firms randomly choose from this queue of workers, p_k workers are employed at wage w_{k1}, and $p_{kS} - p_{k1}$ workers are unemployed.

The upshot is that even when a prescribed increase in the covered firm wage induces an employment contraction, more workers will queue than jobs available in hopes of getting the higher wage. As in the case for a positive employment effect discussed in the main text, this will cause the residual labour supply for noncovered firms to decline, giving positive wage spillovers. Of course, if the wage floor is extremely high, and the corresponding employment at firm k reduces enough, then the number of workers who choose firm k will be lower than p_{k0}.[39]

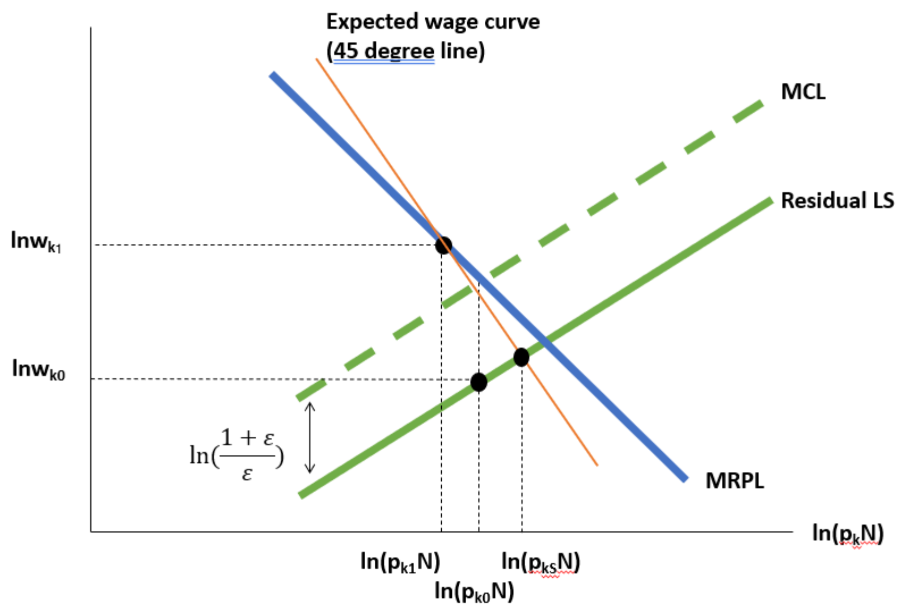
D.1.3 Model sensitivity

Static logit model The baseline static logit model can be extended in many ways.

Nested labour markets. If the local market is responsive to the average wage, then employment need not decline by as much in the spillover firms. We can express this as $n_j = p_j \bar{w}^\theta$, where the average wage is the firm-share weighted geometric mean $\bar{w} = \Pi_{l=1}^S w^{p_l}$. Then $ln(n_j) = \theta ln(\bar{w}) + \beta ln(w_j) - ln(\sum_l^S w_l^\beta)$, $\varepsilon_{jj}^n = \theta p_j + \beta(1-p_j)$, and $\varepsilon_{jk}^n = \theta p_k - \beta p_k$. The cross-wage elasticity also carries the extra θ term, $\varepsilon_{jk}^w = \frac{1}{1+\eta\beta p_k + \eta\theta(1-p_k)}(\frac{(\beta-\theta)^2 p_k p_j}{\varepsilon_{jj}(1+\varepsilon_{jj}^n)} + (\beta-\theta)\eta p_k)$. The cross-firm wage elasticity is slightly lower, though with a smaller employment decline. The intuition is that the aggregate market supply increases in response to the higher average wages, which in turn implies a lower absolute decrease in the residual labour supply. Since firm j's optimal wage is primarily

[39]This threshold wage is well above the intersection of the MCL and MRPL curves; the gap between this threshold wage floor and this intersection is $ln(\frac{1+\varepsilon_{jj}}{\varepsilon_{jj}})\frac{\eta}{1-\eta} > 0$ $(0 < \eta < 1)$, i.e. higher for when the firm labour supply elasticity is lower (steeper Residual LS curve) and when the product demand elasticity is higher (steeper MRPL curve).

Figure D1: **Diagram of wage floor effect on covered firms, with queuing**



Notes: The equation for the Marginal Revenue Product of Labour Curve (MRPL) is $ln(mrpl) = lnA_j - \eta ln(p_j N)$ for firm j, aggregate labour supply N, proportion of employment p_j and firm-specific productivity A_j. The equation for the Residual Labour Supply (LS) is $lnw_j^{LS} = 1/\beta(ln(p_j N) - lnN + ln(n_k + \sum_l^S w_l^\beta))$, where n_k denotes the treated firms' employment. The equation for the Marginal Cost of Labour (MCL) is $ln(mcl) = lnw_j^{LS} + ln(\frac{1+\varepsilon}{\varepsilon})$ or the Residual LS wage 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. The equation for the Expected Wage Curve is given by $lnw_E = lnw_{k1} + ln(p_{k1}N) - ln(pN)$ where lnw_E is the expected wage at lnw_k, $ln(p_k N)$. With queuing, the number of workers allocated to firm k is set where the Expected Wage Curve intersects with the Residual LS curve, such that $p_{k1}N$ workers are employed at k at wage w_{k1}, and $(p_{kS} - p_{k1})N$ workers are unemployed.

responding to changes in this residual labour supply, its wage response is correspondingly smaller (as is the employment decline).

Functional forms. A range of optimal wage-setting curves are possible, which imply other cross-firm wage elasticities. For example, the functional form of the profit function could be different. Dustmann et al. (2021) in their appendix use a similar logit model to demonstrate reallocation, but with a production function which is increasing in $ln(N)$, i.e. $A_j ln(p_j(w_j)N)$ instead of $\frac{1}{1-\eta}A_j(p_j(w_j)N)^{1-\eta}$ above.[40] The cross wage elasticity expression is $\varepsilon^w_{jk} = \frac{\beta p_k}{1+\beta p_k}(\frac{\beta p_j}{\varepsilon_{jj}(1+\varepsilon_{jj})} + 1)$, which is actually identical to the baseline model with $\eta = 1$.For the calibration values above of $\beta = 6$ and $p_k = 0.5$, this implies a larger magnitude of $\varepsilon^w_{jk} = 0.94$.

Additional utility factors. Several other factors, most obviously in this application the geographic distance, may enter into the utility function of workers from their job in addition to the wage. We can pose this as $V(w_j) = \beta ln(w_j) + \theta X + \nu_{ij}$, where θX represents any non-wage utility factors. These may be specific to the person, firm or match. It turns out that following the working above, the expressions remain the same.

Fair wage norms Building on the static model, an alternative mechanism for spillover effects is through fair wage norms. Assume fair wage norms are modeled in the same way as a reservation wage sector, i.e. building on the main model setup above, let there be a "firm" that represents an opting out of the relevant labour market and offers the "fair wage" or "reservation wage" w_*. Then the labour supply function for firm j is given by $lnp_j = \beta ln(w_j) - ln(w_*^\beta + \sum_l^S w_l^\beta)$. The analysis is exactly the same as in the main text for noncovered firms, since to firm j there is no differentiation between a reservation wage sector and a covered sector. Of course, when incorporating consideration sets or possibly differential separation and hiring elasticities as in the dynamic model, the likely low-wage nature of the reservation sector will differentiate it from the covered sector.

This also has implications for the covered sector. If we assume that the wage norm w_* is benchmarked to the prescribed wage w_B, as in a factor $w_* = \alpha w_B$, then as long the cross-firm wage elasticity $\varepsilon^w_{jk} \leq 1$, the covered sector firms will not increase their wages further but their employment will decline. That is, the initial covered sector employment increase will raise wages and employment, but the increase in wage norms will somewhat offset the change in employment. Along with section D.1.2 above, this may explain the negligible change in employment found empirically in this paper for covered sector firms. For spillover firms, the same logic as the main setup applies, except that the relevant proportion p_k of the sector increasing wages is now larger as it additionally includes the reservation wage sector.

An alternative form of this is if reservation wages enter into the worker's utility function as $p_j =$

[40] As is standard for the literature, they assume in their model that all firms have negligible share and so do not consider spillovers. Including spillovers, the wage equation is $lnw_j = \frac{1}{1+\beta}(ln(\frac{\varepsilon_{jj}}{1+\varepsilon_{jj}}) + lnA_j - ln(N) + ln(\sum w_l^\beta))$.

$\frac{(w_j-w_*)^\beta}{\sum_{l=1}^{S}(w_l-w_*)^\beta}$. The cross wage elasticity becomes $\varepsilon_{jk}^{w} = \frac{\beta\phi_k}{1+\eta\beta(1-\sum p_l\phi_l)}(\frac{\beta\phi_j p_j p_k-(1-p_j)w_*/(w_k-w_*)}{\varepsilon_{jj}(1+\varepsilon_{jj})} + \eta p_k)$ where $\phi_j = w_j/(w_j-w_*)$ is simply an additional factor, $\varepsilon_{jj} = \beta\phi_j(1-p_j)$, and $\varepsilon_{jk} = -\beta\phi_k p_k$. For the benchmark values, the cross-firm wage elasticity is similar, but the elasticity is higher for low-wage firms paying closer to w_*.

Monopsony search model of Burdett and Mortensen (1998) I reproduce here the key equations in Burdett and Mortensen (1998) relevant for the analysis of spillovers, and refer the reader to the paper for details. Workers are homogenous with unemployment income b, and firms are homogenous with productivity p. Denote the arrival rate of job offers for employed and unemployed workers as λ_1 and λ_0 respectively. Assuming the value of employment is increasing in the wage, a reservation wage R exists such that the value of a firm offering wage w is greater than the value of unemployment for any $w > R$. Then for the distribution of firms $F(w)$, with discount rate r and exogenous separation rate δ:

$$R-b = (\lambda_0-\lambda_1)\int_R^\infty \frac{1-F(x)}{r+\delta+\lambda_1(1-F(x))}dx \tag{13}$$

The paper then derives the equilibrium wage distribution with equal profits, which is that wages will be equally dispersed between R and p. There are no mass points in the wage distribution, since otherwise a firm offering a wage just above the mass point would be able to poach any worker from the mass point while only paying slightly higher wages – thereby increasing its profits.

Regarding spillovers in the setting of this paper, assume that the set of firms covered by the collective bargaining agreement constitute the top of the relevant wage distribution $F(w)$. Descriptively, this may be plausible since covered firms have a large firm wage premium. Then as covered firms increase wages to comply with a higher prescribed minimum wage, equation 13 implies that R increases as long as $\lambda_0 > \lambda_1$. Given that no firm sets a wage below R, noncovered firms will respond to the increase in R by increasing their own wages — a positive spillover wage effect.

Moreover, in this setup covered firms do not increase employment. To see this more directly, for a covered sector prescribed minimum wage w_B the flow of workers into the covered sector is $\lambda_1 F(w_B)(1-u)+\lambda_0 u$ (where u is unemployment), and the flow of workers out of the covered sector is $\delta(1-G(w_B))(1-u)$ (where $G(w_B)$ is the cumulative distribution of workers paid below w_B). Recall the assumption that covered firms are already at the top of the distribution, so there is no change in any of these parameters when w_B increases, meaning that the total employment in this sector remains the same. Along with the queuing model D.1.2 above, this model therefore provides an alternative rationalization of how small covered sector employment effects (as found empirically) may be consistent with positive wage spillovers.

Ronald McDonald monopsonies The model in Bhaskar and To (1999) delivers very similar results when considering spillovers. N firms are assumed to be equally spaced around a unit circle (think of the line as a one-dimensional propensity score of attractive characteristics), at a distance $1/N$ apart. With a "transportation" cost of t per unit distance, a worker on the cicle will prefer firm j over firm k when they are at most distance x^0 away from firm i, such that $w_j - tx^0 > w_k - t(1/N - x^0)$. This gives a labour supply of $n_j = 1/N + (w_j - w_k)/t$, an own-employment elasticity $\varepsilon^n_{jj} = \frac{\partial n_j}{\partial w_j}\frac{w_j}{n_j} = w_j/(t/n + w_j - w_k)$, and cross-employment elasticity $\varepsilon^n_{jk} = -w_k/(t/n + w_j - w_k)$. Maximizing w.r.t. wage, $w_j = (\phi - t/n + w_k)/2$, and with symmetric wages, $w^* = \phi - t/n$.

In my context of partial coverage of the minimum wage changes, I allow for different firm wages. Using ε_{jj} and n_j for this model, but retaining the production function in my model, the specific cross-wage elasticity is then given by $\varepsilon^w_{jk} = \frac{w_k}{1+\eta w_j/(n_j t)}(\frac{1}{t/n + tw_j - w_k} + \frac{\eta}{n_j t})$. In simulations, setting $w_j = 5$, $w_k = 6$, $\eta = .5$, $n = 10$ and $t = 20$ (such that cost to travel betwen two neighbouring firms is $t/n = 2$), then the labour supply elasticity is $\varepsilon^n_{jj} = 5$ and $\varepsilon^w_{jk} = 1.14$. This is larger than in the main model above, perhaps because the labour supply elasticity is more sensitive to the competitor wage.

Individual bargaining Consider models of search with individual-level re-negotiation. Workers who receive a better offer from another firm may choose not to quit and instead re-negotiate up wages (Cahuc, Postel-Vinay, and Robin, 2006). An increase in the wage of firm k may then lead to an increase in wages of firm j.

For a simple exposition, I follow (Caldwell and Harmon, 2019) and allow for both posting and renegotiating firms. In my setup, bargaining council firms bound by minimum wage agreements post these prescribed wages, while other potential spillover firms renegotiate with workers. For the simple two-period case where workers have no bargaining power, and receive offers at rate λ from bargaining council firms, the expected wage in period 2 for worker i at firm j depends on the firm's productivity p and the last offer w_1 that the worker received:

$$E[w_2(w_1,p)] = (1-\lambda e^{\lambda})w_1 + \lambda e^{\lambda}(\underbrace{\int_{w_1}^{p} x \cdot dF(x)}_{\text{renegotiate wage}} + \underbrace{\int_{p}^{p_{max}} x \cdot dF(x)}_{\text{switch firm}})$$

where $F(x)$ is the distribution of wages in posting firms. The wage w_2 will be higher after an increase in bargaining council prescribed wages (i.e. positive wage spillovers) if the mass of bargaining council firms increases wages within the range (w_1, p). It will also lead to an increase in the probability that the worker switches to another firm.

D.2 Logit dynamic model

D.2.1 Consideration sets

As discussed in the main text, a noteworthy assumption in much of the literature on consideration sets is to treat these consideration sets as exogenous to the relevant choice variables (an exception is Caplin, Dean, and Leahy, 2019). A motivation for this could be through exogenous constraints such as geographic distance. For example, in a simple model, utility net of transport costs is $V_j = \beta ln(w_j - tx_i) - ln(\sum(w_l - tx_i)^{\beta})$, which is exactly as in the reservation wage setup discussed above with $b = tx_i$ representing any matrix of distances x_i for worker i with associated costs t. A worker never accepts a negative net wage (i.e. $w_j - tx_{ij} > 0$), disqualifying such outside options from the consideration set. The exogeneity of consideration sets is also supported by studies of worker employment networks as in this context.

However, I do explicitly incorporate the dynamic aspect of workers' choice over consideration sets. For example, if firm j has a better consideration set than firm k (where "better" implies access to higher wage firms), then the worker may choose firm j even if the current-period idiosyncratic utility at firm k would be higher ($w_j + \varepsilon_{ij} < w_k + \varepsilon_{ik}$). This will not matter in the case of a common consideration set, but insofar as firms are overlapping in consideration sets and job ladders exist in the hiring and quitting markets, this could be important. I assume that workers' consideration sets are firm-specific, but allow for different hiring and quitting consideration sets for each firm.[41]

D.2.2 Dynamic quit and hire elasticities

Following Langella and Manning (2021), consider the utility of staying in the firm after the realization of the utility shock, where the function X_j denotes the expected discounted utility of remaining in the firm (excluding the current idiosyncratic shock ν_j). Both X_j and value V_j^w depend on own wages w_j and other firms' wages denoted w_{-j}, and the discount factor is ρ_w.

$$X_j(w_j, w_{-j}) + \nu_j = (\beta/2)ln(w_j) + \rho_w V_j^w(w_j, w_{-j}) + \nu_j \tag{14}$$

The value to the worker of being in firm j can be written as the weighted sum of the value of staying, and the value of choosing a firm from the consideration set:

$$V^w(w_j) = (1-\lambda)(X_j + E[\nu_j]) + \lambda E \max[X(w_j) + \nu_j, X(w_1) + \nu_1, ..., X(w_{S_j}) + \nu_{S_j}] \tag{15}$$

[41] For example, if firm k is a high wage firm and j is a low wage firm, then firm j has more separations to k than hires from k. When k raises its wage, j will primarily experience the effect through workers who separate more towards k, since j's hires will primarily be affected by a decrease in hires from other lower wage firms (since those hires are more likely to go to k). In the reverse situation, where k has low wages and j has high wages, then j will primarily experience its change in employment through a decrease in direct hires from k.

We can specify quits as the share of j workers receiving offers who do not choose j. Note that the worker only knows her current-period realizations of ν_j. Future realizations, as entering through the discounted value term in 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, and $\rho_w V_j^w$ in Equation 14 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, noting the distribution of ν_j. Then the probability utility is maximized at j can be denoted $\tilde{q}_j = exp(X_j)/\sum exp(X_l)$, and:

$$q_j(w_j, w_{-j}) = \lambda(1-\tilde{q}_j) \tag{16}$$

We are interested in how quits from j change with another firm's wage w_k:

$$\frac{\partial q_j}{\partial w_k} = \lambda \tilde{q}_j \tilde{q}_k \frac{\partial X_k}{\partial w_k} \tag{17}$$

Using Equations 14 and 15 for the first equality, and substituting in 16 for the second:[42]

$$\frac{\partial X_k}{\partial w_k} = \frac{(\beta/2)}{w_k} \frac{1}{1-\rho_w(1-\lambda+\lambda\tilde{q}_k')} = \frac{(\beta/2)}{w_k} \frac{1}{1-\rho_w(1-q_k')} \tag{18}$$

Therefore, combining 17 and 18 for the first equality, and substituting in the derivative of 16 for the second, the elasticity of quits j to wages k is:[43]

$$\varepsilon_{jk}^q = \frac{\partial q_j}{\partial w_k}\frac{w_k}{q_j} = \frac{\lambda(\beta/2)\tilde{q}_j\tilde{q}_k}{q_j(1-\rho_w(1-q_k'))} \tag{19}$$

The elasticity of hires to j with regard to a change in 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 $\tilde{h}_j = exp(X_j)/\sum exp(X_l)$. These are defined separately to allow for different quit and hire market consideration sets.

$$\varepsilon_{jk}^h = -\frac{\lambda(\beta/2)\tilde{h}_j\tilde{h}_k}{h_j(1-\rho_w(1-q_k'))} \tag{20}$$

[42] I use the slightly different notation q_k' to indicate that the inclusive value over which this is calculated differs if the quits market considerations sets for firms j and k are different.

[43] For interest, the own quit wage elasticity is $\varepsilon_j^q = -\frac{\lambda(\beta/2)\tilde{q}_j(1-\tilde{q}_j)}{q_j(1-\rho_w(1-q_k'))}$, which matches the corresponding expression in Langella and Manning (2021), taking into account the appropriate substitutions (they leave the expression at the more general level and allow for quits to unemployment). The own hire wage elasticity is $\varepsilon_j^h = \frac{\lambda(\beta/2)\tilde{h}_j(1-\tilde{h}_j)}{h_j(1-\rho_w(1-q_k'))}$.

D.2.3 Firm wage optimization

The firm's objective is to dynamically maximize profits by setting the wage w^h_j which induces the current period hires $h(w^h_j, w^h_{-j})$, depending also on other firms' wages w^h_{-j} as above in the static model. The firm faces this choice dynamically, conditional on its previous average wage $\bar{w}_j$, and corresponding previous employment $p(\bar{w}_j)$. While the proportion of workers reallocating each period is fixed at λN on aggregate, quits from the firm $q(\bar{w})$ depend on the wage such that $p(\bar{w}_j)(1-q(\bar{w}_j))$ of the previous workers remain as above. The firm's value function is:

$$\begin{aligned} V^f_j = \max_{w^h_j}\{ & \frac{1}{1-\eta} A_j [p^t_j N]^{1-\eta} - \bar{w}_j p(\bar{w}_j)(1-q(\bar{w}_j))N \\ & - w^h_j h(w^h_j, w^h_{-j})N + \rho V^f_j(p^t_j, \bar{w}^t_j)\} \end{aligned} \tag{21}$$

That is, the firm maximizes revenue minus wages for incumbents and new hires, taking into account future profits. Each period workers separate based on the previous wage, and join based on the offered wage. The current period employment and wages evolve as a weighted average of the previous wage and offered wage, determining the next period problem $V^d_j(p^t_j, \bar{w}^t_j)$ mechanically as follows:

$$\begin{aligned} p^t_j &= p(\bar{w}_j)(1-q(\bar{w}_j)) + h(w^h_j, w^h_{-j}) \\ \bar{w}^t_j &= \frac{p(\bar{w}_j)(1-q(\bar{w}_j))\bar{w}_j + h(w^h_j, w^h_{-j})w^h_j}{p(\bar{w}_j)(1-q(\bar{w}_j)) + h(w^h_j, w^h_{-j})} \end{aligned} \tag{22}$$

The first order condition implies

$$\frac{\partial V^f_j}{\partial w^h_j} = A_j(p^t_j N)^{-\eta} h_1 N - hN - w^h_j h_1 N + \rho \frac{\partial V^f_j}{\partial p^t_j}\frac{\partial p^t_j}{\partial w^h_j} + \rho \frac{\partial V^f_j}{\partial \bar{w}^t_j}\frac{\partial \bar{w}^t_j}{\partial w^h_j} = 0$$

And using the envelope theorem,

$$\begin{aligned} \frac{\partial V^f_j}{\partial p_j} &= A_j(p^t_j N)^{-\eta}(1-q(\bar{w}_j))N - \bar{w}_j(1-q(\bar{w}_j))N + \rho \frac{\partial V_j}{\partial p^t_j}\frac{\partial p^t_j}{\partial p_j} + \rho \frac{\partial V_j}{\partial \bar{w}^t_j}\frac{\partial \bar{w}^t_j}{\partial p_j} \\ \frac{\partial V^f_j}{\partial \bar{w}_j} &= -p_j(1-q_j)N + \rho \frac{\partial V_j}{\partial \bar{w}^t_j}\frac{\partial \bar{w}^t_j}{\partial \bar{w}_j} \end{aligned}$$

In steady state, firms set the same hiring wage as the previous wage, $\bar{w}_j = w^h_j$, and correspondingly the previous and current employment proportions are stable, $p_j = p^t_j$. Substituting in derivatives, as well as the steady state, $\partial p^t_j/\partial p_j = 1-q_j$, and $p_j = h_j/q_j$, then $\partial \bar{w}^t_j/\partial \bar{w} = 1-q_j$, and $\partial \bar{w}^t_j/\partial p_j = 0$. This implies for the equations above, $\frac{\partial V_j}{\partial p_j} = \rho\frac{1-q_j}{q_j}(A_j(p^t_jN)^{-\eta} - \bar{w}_j)N$ and $\frac{\partial V_j}{\partial \bar{w}_j} = \rho\frac{1-q_j}{q_j}(-\frac{h_j}{q_j})N$. Finally, substituting back into the first order condition,

$$(A_j(\frac{h_j}{q_j}N)^{-\eta}h_1 - w_jh_1)(\lambda+\rho\frac{1-q_j}{q_j}) - h_j(1+\rho\frac{(1-q_j)^2}{q_j^2}) = 0$$

Rearranging, the wage w_j is optimized as

$$ln(w_j) = ln(A_j) - \eta ln(\frac{h_j}{q_j}N) + ln(\frac{\varepsilon_j}{\varepsilon_j+1-\psi}) \tag{23}$$

Where $\psi = \frac{\rho q_j(1-q_j)}{q_j^2+\rho(1-q_j)}$ and ε_j refers to the hiring labour supply elasticity. This is extremely similar to the static equation above, with $p_j = h_j/q_j$ in steady state as expected, and $\psi \in [0,1/3]$ for the range of values (with full discounting, i.e. $\rho = 0$, this reduces to the static equation expression).

D.2.4 Dynamic cross-wage elasticity

Using the quit and hiring elasticities derived above in Equations 19 and 20:

$$\varepsilon^w_{jk} \approx -\eta(\varepsilon^h_{jk} - \varepsilon^q_{jk}) = \frac{\eta(\beta/2)}{(1-\rho_w(1-q'_k))}(\frac{\lambda\tilde{h}_j\tilde{h}_k}{h_j} + \frac{\lambda\tilde{q}_j\tilde{q}_k}{q_j}) \tag{24}$$

To understand the steady state effects, Equation 24 needs to account for other firms' wage responses, which (after some work) appears as an extra multiplicative term $\alpha = \frac{1-\rho_w}{1-\rho_w(1-\lambda+q'_k)}$. The analog to the assumption in 12 above that $\varepsilon^w_{jk} = \varepsilon^w_{lk}$ for other non-treated firms l, is here that $dX_j/dw_k = dX_l/dw_k$.[44] And to translate this 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:

$$\varepsilon^w_{jk} \approx \frac{\eta(\beta/2)\alpha}{(1-\rho_w(1-q'_k))}(\underbrace{\frac{\lambda\tilde{h}_{jk}}{h_j}}_{\text{Prop. j's hires from k}} + \underbrace{\frac{\lambda\tilde{q_{jk}}}{q_j}}_{\text{Prop. j's quits to k}}) \tag{25}$$

The term in brackets is the average hires from and quits to firm k, that is, the flow of workers f_{jk} between firms j and k, $f_{jk} = \frac{\lambda\tilde{h}_{jk}}{h_j} + \frac{\lambda\tilde{q_{jk}}}{q_j}$. To produce the cross-wage elasticity ε^w_{jk}, these flows are multiplied by the term $\eta\beta$ as in the static model, the multiplier term α to account for other firms' responses, and $1/(1-\rho_w(1-q'_k))$ to account for workers' discounted value of the probability she

[44] That is, the responsiveness of the non-stochastic part of firm value to a change in w_k is similar. As in the static case, this is not necessary to derive the cross-wage elasticity, but does simplify the expression considerably.

will stay in firm k if she moves there (a lower wage response is required if quits from k are anyway high).

Note equation 25 holds in steady state. Over the short run, there is an adjustment period to reach steady state. The second-round wage responses from other firms reacting in turn to the new wage increases may similarly require an adjustment period, further attenuating the initial response relative to the longer run cross-wage elasticity. λ determines the speed of the adjustment to the new firm steady state: the replacement rate r of the firm's workers as governed by the adjusted flows is given by $r = 1-(1-\lambda)^t$. For example, if $\lambda = 1/3$, $r = 1/3$ of the firm's workers would be replaced in one period, and $r = 2/3$ would only be replaced in $t = 2.7$ periods.

E Appendix: Data

The SARS administrative tax dataset provides a near-universe of formal sector individual labour market wage outcomes and firm balance sheet information. It is periodically updated, with available years extending from financial year 2008 to 2018.[45] It is easily one of the richest sources of economic data for South Africa's formal sector economy. However, a key limitation (similarly to other administrative data) is that the data are collected for the purposes of taxation only, thus by design misses out on key covariates essential to the analysis of many important economic questions. For example, there are no data on worker occupation, race or education; on the outcomes of non-workers pertaining to unemployment; or on whether a given worker or firm is covered by a collective bargaining agreement. The purpose of this Data Supplement is to outline how I use bargaining council (BC) data, which I compile externally to the tax data and then match them into the tax data.

E.1 Compiling bargaining council agreements

The government gazette publishes bargaining council agreements, which may be found online at `https://www.greengazette.co.za`. By going through at least one agreement in detail per bargaining council, I record the industry and location of each. I supplement this with the compilation of wages provided to me by the Labour Research Service, and check each wage against the actual agreements as published in the government gazette.

I match 34 bargaining councils, which correspond to 32 of the 38 private sector bargaining councils and 2 public sector bargaining councils.[46] For each bargaining agreement, I select the SIC 5 classification code that best matches the wording in the agreement. This may be at the 3, 4 or 5 digit level depending on the industry descriptions. Similarly for location, I select area based on the description in each bargaining agreement, at the national (all locations), province or district council level. Note that some bargaining councils are defined at the municipal level, but I use district council as the lowest level for simplicity.

Each bargaining council agreement may have many clusters of locations, where each cluster may have a different set of bargained wages or conditions. There are 145 clusters in total. As a consequence of the clusters, the majority of the clusters are defined by district council locations

[45] Financial year 2008 corresponds to calendar time March 2007 to February 2008. Since the writing of this paper, further years have become available.

[46] The following three bargaining councils were not merged because I could not find the corresponding gazetted agreements: Amanzi BC, Grain BC, and Sugar Manufacturing and Refining BC. The Building BC (East London) and the Motor Ferry BC were excluded because they overlap directly on industry-location with other BCs (Building of Southern and Eastern Cape, and Motor respectively). The Furniture BC (South Western Districts) had no firm level matches in the tax data. The public sector bargaining councils are the Public Service Coordinating BC (PSCBC) and the South African Local Government BC (SALGBC).

even though nearly half of the bargaining councils are nationally based. In total, I consolidate nearly 1600 records of wages from different bargaining council clusters by industry and location.

Table E1 provides these details on industry and location for each bargaining council. The average wage for all bargaining councils is about R4,500, provided in the table for 2018 (adjusted to 2016 ZAR). The wage selected is the minimum wage bargained in the agreement, which usually covers occupations such as "labourer" (41% of listed wages) or "general worker" (30% of listed wages). I cross-check these categories with the QLFS data, though this is a limited check given that the QLFS data do not have as much detail on industry, and their correspondence with occupational categories in the agreements is not straight-forward. The minimum wages are highest in the New Tyre Manufacturing, Transnet, and Public Service Coordinating bargaining councils, and lowest in Hairdressing and Furniture Manufacturing for Kwa-Zulu Natal.

E.2 Matching bargaining council agreements to tax data

I construct the tax panel by combining all available worker-level variables for tax years, restricting to workers of ages 20 to 60 years old, with one job per worker, and merging in firm-level variables. The wage variable is just the wage-code "3601". Table E2 briefly summarizes the tax data individual-level panel between 2008 and 2018. The number of workers in the panel ranges from 6.7 million to 10 million, with 5-10% of the sample cut out of the main event-study specification due to restricting to firms with at least 10 workers in the pre-period. About 40% of workers are women on average, and workers are middle-aged at 37 years old. Most job durations are for 80% of the year, though this is clustered around full time jobs. The 90th percentile of wages is about 13 times the 25th percentile of wages, indicating the vast degree of inequality in the South African formal sector economy.

Table E3 briefly summarizes the tax data firm panel between 2008 and 2018. From the firm balance sheet side, the profit and turnover are slightly higher in 2009, but are otherwise stable. The firm exit rate (defined as the firm not observed in the next year) and outsource probability (defined when a third or more than 500 workers switch to the same other firm at the same time) are similarly higher for the incomplete years.

There are a few caveats to the matching process. Firstly, the definition of the industry variable is important, and I follow the best practice as laid out in Budlender and Ebrahim (2020). Within this industry code, I select the SIC 5 industry classification system. Secondly, the location variable is just as crucial for the matching. I focus on the IRP5 individual level business location variable, since the bargaining councils are defined by the location of the firm not the worker. A key limitation is that this variable is largely missing for the earlier years from 2008 to 2012. To impute location, I aggregate workers by payroll identification number (payereferenceno) and select the modal district council as the preferred value of the location. Within the same firm, under the assumption that firms do not change location, I assign the location of later years for each firm for records of earlier years. Given this near-complete location variable for district council, I then assign province based on the district council. A key problem with this approach is that within a payroll number, worker-level records suggest several associated locations. Perhaps some plants file taxes only at a head office, or payroll identification is itself an aggregation of many plants.

Out of a total of 1595 total potential records (11 years by 145 bargaining council clusters), I have matched 90% with wage records. In terms of number of workers over the entire panel, about 30% appear to be covered by bargaining councils and 70% noncovered.

E.3 Descriptives of matched data

In table E4, I show characteristics by bargaining council focusing on those with an event or large wage increase at some point, as highlighted in my main analysis. There is considerable variation across the different bargaining councils for each characteristic. The largest non-government bargaining council is the Metals and Engineering Industry (MEIBC), both in terms of workers (over 800,000) and firms (16,000). There are several other large bargaining councils with hundreds of thousands of workers, such as Civil Engineering, Road Freight and Logistics, Motor Industry, and Chemical. There are also several small bargaining councils, which are more locally defined and in narrower industries, such as Laundry in KwaZulu-Natal, Meat Trade in Gauteng or Hairdressing.

The most profitable BCs on a per-person basis are the Meat Trade, Road Freight and Logistics, and Road Passenger bargaining councils. However, all of the bargaining councils have a high average per person profit that is far above the average wage. The bargaining council minimum wages go as low as around R30,000 per year or R2,500 per month; incidentally, this is substantially below the 2019 national minimum wage of R3,120 (all 2016 inflation-adjusted). There are also higher minimums, such as in the Tyre BC (R120,000 per year or R10,000 per month) and MEIBC (R76,000 per year or R6,300 per month). It is worth noting the low proportion of women in bargaining councils generally, with an average of 30% compared to over 50% for other firms. Indeed, the large bargaining councils listed above all have less than a quarter women, except for Chemical. Lastly, regarding labour market parameters, the firm level rent-sharing is generally *lower* than in other firms, perhaps because wages are set more sectorally, though some industries have high rent sharing elasticities (such the Tyre BC). The firm labour supply elasticity is closer to the average, though again with substantial variation.

Overall, the matched data provide a rich picture of the variation across bargaining councils across several characteristics (e.g. minimum wages or number of firms), as well as some common features (e.g. a low proportion of women or increasing minima with value added per worker).

One concern for this paper in the South African context may be the relevance of wage competition when unemployment is high. As one indicator, figure E2 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.[47] In addition, the classic job ladder model with search frictions only requires *some* responsiveness of labour supply to the wage for monopsonistic wage competition to be relevant.

Figure E1: **Decomposition of wages into prescribed wages and firm wage premia**

Notes. The analysis follows the decompositions suggested in Card and Cardoso (2021). I decompose the wage into a baseline (percentile 1 of wages), the gap between the baseline and the bargaining council wage floor, the gap between the wage floor and the firm average wage ("firm cushion"), and the gap between the firm average wage and the worker's own wage. The figure compares the floors and firm cushions at firms in bargaining councils, firms in sectoral determinations (sectors with government set minimum wages), and firms that are not covered. The sample is all formal sector firms from 2008 to 2018 using the SARS tax data.

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

Figure E2: **Firm wages and share of hires from non-employment**

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

Table E1: **Bargaining council industry and location**

Bargaining Council	SIC5 code	Location	Wage	Clusters
Building (Bloemfontein)	504	District Council	R 4,053	2
Building (Cape of Good Hope)	504	District Council	R 4,670	2
Building (Kimberley)	504	District Council	R 2,272	4
Building (NW Boland)	504	District Council	R 3,470	4
Building (SE Cape)	504	District Council	R 3,190	2
Canvas Goods	312	Province	R 5,325	1
Chemical	3354	National	R 5,995	1
Civil Engineering	502	National	R 6,055	1
Clothing Manufacturing	314	District Council	R 4,382	33
Contract Cleaning Services	8893	Province	R 3,178	1
Diamond Cutting	3426	National	R 1,975	1
Electrical	5032	District Council	R 3,516	16
Fishing	13	National	R 3,450	1
Food Retail and Restaurant	6211	District Council	R 2,997	9
Furniture	391	Province	R 2,635	10
Furniture Manufacturing (EC)	391	District Council	R 2,220	2
Furniture Manufacturing (KZN)	391	District Council	R 1,343	4
Furniture Manufacturing (WC)	391	District Council	R 2,634	2
Hairdressing	9902	Province	R 1,340	7
Laundry (Cape)	9901	District Council	R 3,626	1
Laundry (KZN)	9901	Province	R 4,227	1
Leather	3162	National	R 3,997	1
Local Government	913	National	R 6,715	1
Meat Trade (Gauteng)	6121	Province	R 3,185	1
Metal and Engineering	35	National	R 7,330	1
Motor	63	Metro	R 2,957	4
New Tyre Manufacturing	3371	National	R 11,069	1
Public Service Co-ordinating	912	National	R 7,685	1
Restaurant, Catering and Allied	6211	Province	R 3,320	12
Road Freight and Logistics	7412	National	R 4,919	1
Road Passenger	7122	National	R 5,894	1
Textile	312	National	R 5,348	1
Transnet	711	National	R 7,478	1
Wood and Paper Sector	3231	National	R 6,803	1

Notes. Bargaining council names are shortened for presentation. Province abbreviations are Eastern Cape (EC), KwaZulu-Natal (KZN) and Western Cape (WC). The SIC5 code is an industry code following the SIC-5 classification system. Location indicate the geographic level of location assignment, i.e. national, provincial (9 in South Africa) or district council (52 in South Africa). Clusters refer to location specific units within each bargaining council. Source: Own compilation from Gazetted bargaining council documents published by the South African government.

Table E2: **Summary stats on tax panel – individuals**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Year	Workers	Selected	Age	Female	Duration	Wage_p25	Wage_p90
2008	6696607	6184007	37.078545	.39152604	.77653128	22730.393	276277.78
2009	7000445	6480139	36.867508	.40828803	.78436452	23710.016	287943.19
2010	7667358	7161807	37.36937	.41605818	.80991548	26688.479	311551.91
2011	8226735	7694692	37.309708	.41411373	.80026042	26422.131	335109.28
2012	8518927	7981393	37.244785	.41719598	.79825377	25371.135	337349.22
2013	8696944	8152934	37.25687	.41669118	.79896146	26216.545	344890.5
2014	8805778	8256069	37.256012	.41803619	.7965492	27521.789	347771.78
2015	9014434	8458650	37.401371	.45647269	.79661143	28015.436	357910.69
2016	8956599	8393770	37.471569	.46068534	.79065305	27712.656	359345.44
2017	9028088	8460710	37.570023	.46368578	.79489625	28488.758	361228.75
2018	9911545	9332113	37.837261	.4736971	.82527351	33592.277	386959.5

Notes. Wage is defined as the amount recorded under wage-code "3601", i.e. base wages without benefits. For reference, the 2017 log wages per year at the mean of 11.3 and at the 25th percentile of 10.5 are equal to ZAR 81,000 and 36,000 respectively. The "selected sample" of workers are those in firms with more than 10 workers. Source: SARS tax data panel.

Table E3: **Summary stats on tax panel – firms**

	lnWage (mean)	lnWage (p25)	Sep (%)	E-E sep (%)	lnProfit (mean)	lnSales (mean)	Exit (% firms)	Outsource (% firms)
2008	11.14	10.44	52%	41%			17%	15%
2009	11.15	10.44	47%	40%	12.25	13.77	17%	13%
2010	11.20	10.47	40%	44%	12.22	13.65	13%	12%
2011	11.19	10.44	39%	44%	12.18	13.60	12%	11%
2012	11.19	10.44	38%	44%	12.16	13.58	12%	11%
2013	11.21	10.45	38%	44%	12.17	13.59	11%	11%
2014	11.22	10.47	37%	44%	12.17	13.60	11%	11%
2015	11.25	10.49	38%	43%	12.20	13.62	12%	11%
2016	11.26	10.51	38%	43%	12.19	13.61	13%	11%
2017	11.27	10.53	38%	47%	12.17	13.60	20%	11%

Notes. Wage is defined as the amount recorded under wage code 3601, or wage without benefits. For reference, the 2017 log wages per year at the mean of 11.3 and at the 25th percentile of 10.5 are equal to ZARR81,000 and R36,000 respectively. Separations are identified from changes in the firm identification of a worker level record across years. E-E separations indicates a change from one firm to another. Profit is the net profit declared by companies. Outsource indicates that at least a third of all workers or 500 workers in a firm switch to another firm. Note that the years 2008 and 2009 are incomplete. Source: SARS tax data panel.

Table E4: **Description of individual bargaining councils**

Name	Workers (number)	Firms (number)	Firm size (mean)	Inequality (p90/50)	Profit p.p. (mean)	Value Add p.p. (mean)	Wage (median)	Min. Wage (mean)	Sep. (mean)	Churn (mean)	Female (mean)	Worker FE (mean)	Firm FE (mean)	Rent sharing (elast.)	Labor supply (elast.)
Other firms	8,370,023	149,555	56	2.5	297,584	534,753	103,016		37%	36%	53%	0.14	-0.25	.26	.67
Ave. private BC	3,062,582	67,377	45	2.6	291,245	492,254	84,537	56253	38%	36%	30%	0.03	-0.15	0.18	0.70
Building (Cape)	3,227	235	14	2.2	197,832	370,867	65,638	51083	37%	30%	17%	-0.06	-0.33	0.17	0.78
Chemical	223,236	4,184	53	2.7	399,641	647,299	99,747	71281	34%	37%	38%	0.06	-0.09	0.24	0.76
Civil engineering	601,304	12,949	46	2.9	286,646	493,997	82,251	63630	41%	36%	23%	0.00	-0.27	0.22	0.61
Clothing manuf.	46,825	1,498	31	2.4	267,544	449,326	73,227	47994	37%	32%	68%	-0.02	-0.31	0.09	0.86
Contract cleaning	34,773	175	199	2.1	131,987	238,008	42,056	33564	41%	44%	48%	-0.33	-0.55	0.00	-0.12
Electrical	145,218	3,808	38	2.8	252,519	474,142	93,135	44034	37%	35%	23%	0.10	-0.07	0.14	0.80
Fishing	37,346	535	70	2.9	276,013	446,932	73,713	38427	43%	54%	24%	0.04	-0.45	0.46	0.57
Food and rest.	44,332	1,351	33	2.6	237,683	371,351	50,103	33990	41%	46%	49%	-0.19	-0.36	0.28	0.66
Furniture (KZN)	18,213	315	58	2.8	230,770	374,223	62,374	26237	33%	35%	27%	-0.08	-0.36	0.12	0.87
Furniture (WC)	20,813	704	30	2.3	165,112	308,727	64,145	28754	35%	37%	23%	-0.08	-0.25	0.13	1.12
Furniture (national)	44,251	1,498	30	2.7	182,627	345,990	69,007	26952	37%	17%	26%	-0.04	-0.25	0.17	1.03
Hairdressing	8,638	959	9	2.0	115,545	242,726	55,626	31501	40%	36%	88%	0.04	-0.15	0.06	0.43
Laundry (Cape)	2,621	119	22	2.0	111,535	197,121	44,811	42600	39%	45%	63%	-0.29	-0.50	0.11	0.73
Laundry (KZN)	2,359	73	32	2.1	78,190	167,806	46,733	52262	36%	44%	64%	-0.32	-0.56	0.30	0.89
Leather	25,556	708	36	2.5	274,409	456,064	75,200	57845	35%	34%	54%	-0.03	-0.24	0.14	0.77
Meat trade	2,654	112	24	3.0	618,837	1,006,669	131,070	31708	30%	35%	46%	0.25	-0.28	0.19	0.51
Metal & Eng.	835,297	16,041	52	2.7	280,930	520,657	103,191	76733	35%	35%	24%	0.15	0.03	0.19	0.76
Motor industry	285,921	9,678	30	2.4	209,629	375,959	72,278	32923	35%	39%	28%	-0.02	-0.12	0.17	0.63
Restaurant catering	141,516	3,307	43	2.7	239,951	372,997	57,332	33995	42%	41%	44%	-0.15	-0.37	0.11	0.42
Road Freight & Log.	361,235	5,835	62	2.3	556,369	764,644	89,418	54161	40%	37%	18%	0.04	-0.08	0.09	0.71
Road passenger	45,256	516	88	2.0	431,652	585,981	75,458	62481	40%	40%	22%	-0.05	-0.25	0.21	0.93
Textile	62,435	2,003	31	2.5	237,845	414,127	76,531	60229	34%	33%	54%	-0.07	-0.31	0.13	0.69
Tyre	15,942	231	69	2.6	231,778	415,325	89,347	120743	35%	30%	21%	0.06	0.02	0.34	0.97
Wood and paper	53,614	543	99	2.8	285,351	446,234	77,287	60519	37%	38%	32%	-0.06	-0.40	0.20	0.78

Notes. Of the 38 non-government bargaining councils, 13 are not shown due to poor matching in the tax data or no associated wage event: Amanzi (water); Building (except for the Cape); Canvas Goods; Diamond Cutting; Restaurant, Catering and Allied Trades; Furniture in the Eastern Cape and South Western Districts; Grain; Motor Ferry; Sugar Manufacturing. KZN refers to KwaZulu-Natal province and WC refers to Western Cape province. In the columns, inequality refers to mean within-firm inequality, Min Wage refers to the average bargaining council negotiated minimum wage over the period, churn refers to the sum of separations and hires as a proportion of firm size (subtracting the change in firm size), worker and firm FE are the average respective components from an AKM regression, and rent-sharing and labour supply elasticities are estimated across all firms within each bargaining council. The sample is all formal sector firms from 2008 to 2018 using the SARS tax data. The PPP exchange rate from South African ZAR to US Dollar was about 7 in 2020, and the nominal exchange rate was about R15. The equivalent PPP-adjusted minimum wage range for these bargaining councils is between about $350 and $1,500 per full time month, or between $2 and $8.50 per hour.

F Appendix: Institutional details

Regulatory structure. We may broadly think of labour market regulation in South Africa in the following way, from least organized to most (Levy et al., 2014). Firstly, about a third of all workers are informally employed, typically without adhering to minimum conditions such as a written contract (Bassier et al., 2021). Secondly, the Basic Conditions of Employment Act and Labour Relations Act form the minimum conditions of employment and are applicable to all employment relationships – in reality, applying to formal sector workers. Thirdly, wage floors are set unilaterally by the government for selected industries, mostly made up of low-wage workers.[48] Fourthly, any workers can become union members, and workers can seek a union recognition agreement if at least 30% of the workplace belongs to the union. Fifthly, and most relevant for this study, when unions collectively cover 30% of workers in an industry-location (idiosyncratically defined), they can apply with employers to be recognized by the government as a collective bargaining council. These regimes overlap: for example, the wholesale and retail industry is covered by a sectoral determination, with subsets of the industry unionized with workplace bargaining, and other subsets covered under bargaining council agreements.

Description of unions. The unions which represent workers on these bargaining councils are typically organized under federations. The main federations are the Congress of South African Trade Unions (COSATU), the South African Federation of Trade Unions (SAFTU), and the Federation of Union South Africa (FEDUSA). In general, unions under the same federation are coordinated through mass conference resolutions, non-poaching agreements (across industries), and administrative support; then within each union there are national, local and establishment structures that form the hierarchy representing workers belonging to that union.

COSATU represents about 2 million workers out of about 10 million formal sector workers in the country, and dominates representation in the public sector, including health (NEHAWU), transport (SATAWU), teachers (SADTU), police (POPCRU), and municipal (SAMWU) workers. Its most prominent private sector unions are in mining (NUM), retail (SACCAWU), and clothing (SACTWU). COSATU is formally in alliance with the ruling political party (the African National Congress or ANC), which was perhaps the core issue resulting in the splitting of dis-satisfied and arguably more militant unions that formed SAFTU.

SAFTU represents nearly a million workers, and dominates representation in manufacturing, most prominently metalworkers (NUMSA) and food (FAWU). FEDUSA was historically comprised mainly of white-collar workers, with a current estimated membership of half a million workers. Among the bargaining councils, the largest led by COSATU-affiliated unions are the

[48] There are 11 sectoral determinations. 8 of them set formal sector minimum wages: contract cleaning, civil engineering, learnerships, private security, wholesale and retail, forestry, farm workers, and hospitality.

Public Service (through various unions), Civil Engineering (through NUM), and Road Freight and Logistics (through SATAWU); and the largest led by SAFTU affiliated unions are the Metals and Engineering Industry (through NUMSA), and Motor Industry (also NUMSA).

In my compilation of wage agreements, SAFTU-affiliated bargaining councils tend to have higher wage increases than COSATU, and there is a positive correlation between larger wage increases and the age of the union as well as the number of members in the union.