Central bargaining and spillovers in local labor markets

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Abstract

How does centralized bargaining affect the broader wage structure? And what does this tell us about the mechanisms that govern the wages and flows of labor? Using bargaining council contracts matched with firm and worker level data in South Africa, I show wages correspond sharply to large contracted wage increases especially in mid-wage and mid-size firms. A simple model of monopsonistic competition and strategic interaction predicts sizable cross-firm wage spillovers from such partially treated labor markets. To account for the overlapping and localized nature of labor markets, I show that a model-based measure of wage spillovers is proportional to worker flows between firms. Using observable worker-level flows, I isolate the empirically relevant labor market segment, and find a cross wage elasticity of about 0.8. Previous estimates are lower, which is consistent with being based on less precise measures of spillovers. A microdata simulation suggests that accounting for these spillovers doubles the effect of contracted wage increases on the full wage distribution.

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1 Introduction

Centralized bargaining institutions that cover only parts of the labor market are pervasive across the world, with at least 40 countries recording over 30% coverage (ILOstat, 2020). Yet our understanding of the effect of centralized bargaining agreements on the broader wage structure has lagged behind recent work showing that labor markets feature employer wage-setting power, strategic interaction, and localization. I argue that such an empirically grounded view is crucial for understanding the influence of institutional regulation on the wage structure. I study bargaining council agreements between 2008 and 2018 in South Africa, matching them with worker and firm-level data, and find that these agreements result in observable wage gains for workers in both contractually bound firms *and* firms connected to them through worker flows.

One way to think about the broader effects of bargaining councils is in terms of employer wage-setting power. Though once considered limited to company towns, monopsony power – one form of employer power – is increasingly recognized as pervasive (Dube, Giuliano, and Leonard, 2019; Sokolova and Sorensen, 2021). Monopsony power may be constrained through minimum wages, set through unions of organized workers (Manning, 2003a). While there is a large literature on collectively bargained minimum wages, the underlying model of the labor market is usually treated as competitive especially regarding uncovered workers (Cahuc, Carcillo, and Zylberberg, 2014).

A particular prediction from models of monopsonistic competition with strategic interaction concerns spillovers. Following a minimum wage on treated firms, nearby non-treated firms are no longer as attractive as before, meaning they have to increase wages to retain workers (Manning, 2003b). The effects of this minimum wage on firm size depends on the firm's productivity: low productivity firms are priced out by the minimum wage and are forced to downsize, while mid productivity firms may increase size as the cost of the marginal worker decreases (Dickens, Machin, and Manning, 1999). The net employment effect can vary, but the structure of jobs concentrates more towards higher wage, more productive firms.

I investigate these predictions relating collective bargaining to broader monopsonistic labor markets. Does centralized bargaining actually affect wage-setting, thereby constraining the markdowns associated with employer power? Do these effects spill over onto nearby firms? And how does this all influence the structure of jobs in the broader labor market? In particular, I study sharp changes in bargained contracts, tracing their effect on wages, worker flows, firm size, and firm profits for bargaining council and nearby firms. I measure nearby firms as firms that are not contractually bound by bargaining councils, but draw from the same local labor market as measured by the network of worker flows.

This study contributes mainly to our understanding of spillovers from institutional wage increases. There are several studies providing support for the existence of spillovers following institutional wage increases. Cengiz et al. (2019), Engbom and Moser (2021), Fortin, Lemieux, and Lloyd (2021), and Lee (1999) show shifts well into the distribution of wages following increases in the minimum wage. Muralidharan, Niehaus, and Sukhtankar (2017) show that experimentally increased wages in India's NREGS wages led to large increases in wages *and* employment in nearby private markets. Of the studies considering firm-level effects, perhaps the most well-known is Staiger, Spetz, and Phibbs (2010), who find that non-treated hospitals raised their wages in response to a wage increase at treated hospitals, with a cross-wage elasticity of about 0.35. More recently, Derenoncourt et al. (2021) observe that large employer voluntary wage increases led to increases in wages of similar jobs, with an elasticity of 0.23.

My study brings two sets of insights to our understanding of spillovers. Firstly, the specific institution of central bargaining deserves attention as a pervasive feature of labor markets. Berger, Herkenhoff, and Mongey (2019) point out that spillovers may be relatively low in the case of minimum wages, since directly bound firms are likely low-wage and small, suggesting lower interactions. Central bargaining on the other hand, as in the case of South Africa, concerns a large proportion of firms in the middle and upper parts of the earnings distribution. Since I study the universe of formal sector firms in South Africa, I also directly evaluate the importance of central bargaining and spillovers for the overall distribution. Accounting for spillovers doubles the simulated effect of bargaining council contract wage increases on the overall wage distribution from 5% to 10%. The spillover wage effects are also much more evenly spread across the overall wage distribution, in contrast to bargaining council firms.

Secondly, I show that these spillovers occur through local connected sets of firms as determined by worker flows. I motivate this insight through a model of monopsonistic competition and strategic interaction with atomistic firms, and derive this relationship between spillovers and each firm's worker flows to the treated firms. This highlights the mechanism by which these spillovers occur, and also enables more precise measurement through isolating the empirically relevant labor market segment.[1]

I find that workers frequently switch to firms across industrial and geographic boundaries, creating dependencies between firms in seemingly disparate industry-by-location cells. While bargaining councils cover about 40% of formal sector workers in South Africa, firms that are closely connected through overlapping local labor markets account for at least another 30%. My matched employer-employee data allow me to trace the spillovers through these worker transitions between bargaining council and other firms. I observe wage spillovers from bargaining council contract changes that are similar in magnitude to the observed wage changes in bargaining council firms. In addition I find that profit margins decrease by a magnitude that plausibly indicates a wage-profit trade-off for these firms, as found elsewhere (Draca, Machin, and Van Reenen, 2011).

Beyond the literature on spillovers, this study contributes to our understanding of firms covered by collective bargaining. I provide one of the first comprehensive firm profiles of South Africa's bargaining council system, since previous studies have relied on household survey data (Bhorat, Goga, and Van der Westhuizen, 2012; Magruder, 2012). I also present evidence that higher productivity treated firms grow in response to the minimum wage, a re-allocation effect consistent with monopsonistic competition (Dustmann et al., 2021). This builds on Dobbelaere et al. (2020) and Lagos (2019) who show that centralized bargaining in Germany

[1] This is actually consistent with a previous study by Poole (2013), who finds that domestic firm incumbent workers with more hires from multinational corporations have higher wages. She interprets this as evidence of productivity gains from knowledge transfer, but given the wage premium of multinational corporations, this could just as easily be interpreted as in my context as competitive wage responses of firms. Indeed, the study notes, "What exactly occurs inside the domestic worker's establishment is beyond the scope of this paper."

and Brazil are subject to monopsonistic competition, allowing for wage markdowns which may be eroded through these wage contracts. Recently, Card and Cardoso (2021) decompose wages in Portugal into parts attributable to contracted wages, voluntary firm wage "cushions", and a residual. I perform a similar decomposition, which reveals that bargaining council minimum wages explain over a *third* of the overall gender wage gap in formal sector wages, primarily a result of disproportionately fewer women in bargaining council firms.

In the next section, I outline a theoretical framework of firm-level minimum wages and spillovers. I provide institutional context and describe South Africa's bargaining councils in section 3. I evaluate the effects of large contract changes on bargaining council firms in section 4, and on spillover firms in section 5. I discuss evidence on the aggregate effects, re-allocation, robustness, and heterogeneity in section 6, and then conclude.

2 Theoretical framework

2.1 Motivation of model features

My theoretical framework combines monopsonistic competition, strategic competition, and localized labor markets. Each of these features are supported by recent empirical work, and are necessary to rationalize sizable wage spillovers, though the model nests competitive, non-atomistic, and fully connected markets. While none of these individual features are new, the contribution of this model is to provide a tight theoretical and empirical link between monopsonistic competition and the local-ness of firms.

Monopsonistic competition is widely modeled through preference heterogeneity, where the characteristics of a firm (such as location or coworkers) provide idiosyncratic utility in addition to the wage offered (Card, Cardoso, et al., 2018; Langella and Manning, 2021; Lamadon, Mogstad, and Setzler, 2019; Dustmann et al., 2021). Firms are aware that there is a distribution of such idiosyncratic utilities, even though they cannot assign them to individual workers, and so optimize wages with the knowledge that cuts to the wage have little impact on the utility of workers with high idiosyncratic preferences. The logit model provides a classical representation of this setup (McFadden et al., 1973). Strategic competition comes out naturally from the logit model as long as firms are atomistic, that is, have sizable employment share in the labor market. Arnold (2020), Berger, Herkenhoff, and Mongey (2019), and Jarosch, Nimcsik, and Sorkin (2019), amongst others, have motivated such atomistic firms resulting in concentrated labor markets.

Secondly, local labor markets are associated with a vast literature exploring the idea that not all firms are available to workers as outside options. Most obviously, the search literature typically includes a probability that a worker receives a job offer, well below unity (Burdett and Mortensen, 1998; Cahuc, Postel-Vinay, and Robin, 2006). I link these outside options to worker flows, a generalization of Caldwell and Harmon (2019) who consider the wage effects of co-worker networks. Labor markets that are constituted by worker flows have been studied by Arnold (2020), Jarosch, Nimcsik, and Sorkin (2019), and Schubert, Stansbury, and Taska (2020), and provide an intuitive measure of the overlapping local-ness of labor markets that are not confined to industrial or regional boundaries (Fogel and Modenesi, 2021; Manning and Petrongolo, 2017).

These two features of the model complement each other. The local-ness of labor markets, measured precisely by worker flows, motivates the atomistic nature of firms relative to "connected" firms. The main result of the model is that strategic interaction takes place along these connections. The next section summarizes the model by first outlining the static logit mode with strategic interaction, then takes that to a dynamic context with local markets, and ends with the empirical implementation. The full model is in Appendix E.

2.2 Summary of theoretical framework

Static logit

Let the utility of workers be expressed as $V(w_j) = \beta ln(w_j) + \nu_{ij}$, where β parameterizes the latent monopsony power (i.e. the responsiveness of worker utility to wages), and ν_{ij} follows a

Gumbel distribution indicating idiosyncratic preferences for the firm. The distribution yields the probability a worker is employed at firm j, or equivalently the firm share of j, in log terms $lnp_j = \beta ln(w_j) - ln(\sum_k^J w_k^\beta)$. The firm labor supply elasticity is $\varepsilon_{jj} = \frac{\partial lnp_j}{\partial lnw_j} = \beta(1-p_j)$, and the cross-employment elasticity $\varepsilon_{jk} = \frac{\partial lnp_j}{\partial lnw_k} = -\beta p_k$.

The expressions for ε_{jj} and ε_{jk} demonstrate that a firm k which raises its own wage will see an increase in its own employment. The employment share of other firms j will consequently decrease. Firm j, faced with this wage increase from firm k, can trade-off raising its own wages or losing its workers. Firm j faces a wage-employment locus of options, depicted in figure E1.

The optimal wage response is pinned down by the firm's wage setting function. Firm's maximize profits $\pi_j = \max_{w_j} \frac{1}{1-\eta} A_j(p_j(w_j)N)^{1-\eta} - w_j \cdot p_j(w_j)N$, which yields the associated wage and wage-cross-wage elasticity, denoted as ε_{jk}^w:

$$lnw_j = \frac{1}{1+\eta\beta}(ln(\frac{\varepsilon_{jj}}{1+\varepsilon_{jj}}) + lnA_j - \eta ln(N) + \eta ln(\sum w_l^\beta)) \tag{1}$$

$$\varepsilon_{jk}^w = \frac{dlnw_j}{dlnw_k} = \frac{\beta p_k}{1+\eta\beta p_k}(\frac{\beta p_j}{\varepsilon_{jj}(1+\varepsilon_{jj})} + \eta) \tag{2}$$

The intuition for the positive cross wage elasticity ε_{jk}^w is illustrated by the two terms in the bracket of equation 2, representing two channels. The rise in firm k's wage primarily increases firm k's own employment share, which decreases firm j's employment share. The first term shows its effect through the firm labor supply elasticity, which increases when p_j is smaller and therefore raises the wage slightly. The second term shows its effect through the (decreasing) returns to scale parameter η, since lower employment implies higher marginal returns which for a constant markdown corresponds to higher wages.

Figure E1 illustrates the intersection of the optimal wage equation and the wage-employment locus. Using the parameter values $\beta = 6$, $p_k = 0.5$, $p_j = 0.1$, and $\eta = 0.5$, the cross wage elasticity pinned down is $\varepsilon_{jk}^w = 0.62$. This suggests that in response to firm k raising its wages by 5%, firm j increases its wage by 3% and loses 6% employment. The cross-wage elasticity is very similar even if firm j's share is negligible, e.g. $p_j = 0.001$ implies $\varepsilon_{jk}^w = 0.6$.

If the primary firm share p_k decreases, the cross-wage elasticity also decreases but remains substantial: $p_k = 0.1$ with $p_j = 0.001$ replicates the cross-wage elasticity of $\varepsilon^w_{jk} = 0.23$ from large employer wage increases found in Derenoncourt et al. (2021). If both firm shares are small, the cross-wage elasticity is close to 0 (holding β constant), and, under perfect competition with $\beta \to \infty$, $\varepsilon^w_{jk} \to 1$.

Finally, a range of optimal wage-setting curves are possible. For example, Dustmann et al. (2021) use a production function $A_j ln(p_j(w_j)N)$. Keeping the rest of the analysis the same, this implies a much larger magnitude of $\varepsilon^w_{jk} = 0.94$. Many other variants are possible, such as including fair wage considerations or allowing the local market employment to respond to the average wage. The upshot is that the wage-employment locus faced by firm j provides a range of feasible cross-wage elasticities which under plausible parametrizations are large.

Dynamic logit

The standard logit can be adapted to a dynamic setting allowing for worker flows, following a simplified version of the models presented by Caldwell, Dube, and Naidu (forthcoming) and Langella and Manning (2021).

Every period workers take a fresh draw of idiosyncratic preferences ϵ_{ij}. With probability $1 - \lambda$ the worker receives no offers, and with probability λ the worker receives offers from a choice set S of connected firms (always including itself). Referring to the static logit setup above, let firms $k = 1, ..., J$ represent the choices available to the worker between firms in the "consideration set" or *connected set* $S.p_{j,S}$ represents the probably of employment in firm j as before. Then quits and recruits are given by:

$$q_{j,S}(w_j) = \lambda(1 - p_{j,S})$$

$$R_{j,S}(w_j) = \sum_{l \neq j}^{S} \lambda p_{j,S'} \cdot p_{l,S'} N_{l,S'}$$

Where $N_{l,S'}$ indicates the total number of workers in firm l's connected set S'. These expressions are related to the baseline logit through $n_{j,S} = R_{j,S}(w_j)/q_{j,S}(w_j)$. Taking logs, and the derivative of firm j with respect to a wage change in firm k:

$$\begin{aligned}
\frac{\partial ln(n_{j,S})}{\partial ln w_k} &= \frac{\partial ln(R_{j,S})}{\partial ln w_k} - \frac{\partial ln(q_{j,S})}{\partial ln w_k} \\
&= -\beta \cdot (\underbrace{\frac{p_k \cdot \lambda p_j \sum_{l \neq j}^{S}(p_l N_{l,S'})}{R_{j,S}}}_{\text{Prop. j's hires from k}} + \underbrace{\frac{p_k \cdot \lambda p_j}{q_{j,S}}}_{\text{Prop. j's quits to k}}) \\
&= -\beta \cdot f_{jk}
\end{aligned} \tag{3}$$

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

Empirical implementation

In this paper, I study the particular case of collective bargaining, where a minimum wage is agreed to by a group of firms that are part of a larger (connected) labor market. I largely abstract from the endogeneity of the minimum wage in the main analysis, though I discuss this possibility. I model the minimum wage following Dickens, Machin, and Manning (1999), who classify firms into three regimes. Firstly, firms with first-period wages above the new minimum wage are directly unaffected, though will be affected through feedback similarly to connected spillover firms. Secondly, firms with wages below the new minimum, but high enough productivity, will raise their second-period wage to the new minimum, and their employment will increase. Thirdly, firms with wages below the new minimum, but productivity not high enough, will meet their contractual obligation by raising their second-period wage to the new minimum, but their employment will decrease. The lowest productivity firms will exit, altogether leading to re-allocation as in Dustmann et al. (2021).

The connected firms are not contractually bound by these minimum wages, and are modeled as above with spillovers proportional to the average quits and hires f_{jk} to the treated firms. In the empirical implementation that follows, I measure f_{jk} in the pre-period. In my event study of collective bargaining wage increases, multiple firms raise their wages. Since their proportions are additive, I aggregate hires from and quits to any firm k part of a bargaining council BC. My primary measure of connectivity is therefore f_{jBC}, which is a revealed choice set of workers in firm j. What are plausible values of f_{jBC}? Smaller firms in particular have small sets of connected firms, for example firms with between 10 and 50 workers have on average less than 4 distinct firms which workers separate to in a given year, and other firms hire an average maximum share of 45%. This justifies the calibration values for the cross wage elasticities above.

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

3 Context and descriptive data

3.1 Institutional context of South African bargaining councils

Bargaining councils have perhaps been the central institutional feature of the South African labor market since the early 1980s when Apartheid restrictions on Black worker unionization were largely repealed. Today, South Africa operates on a multi-tiered workplace legislative structure, constituted by overlapping organizational structures.

We may broadly think of regulation in the labor market 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, J. Budlender, et al., 2021). Secondly, the Basic Conditions of Employment and labor Relations Acts form the minimum conditions of employment and are applicable to all employment relationships – in reality, covering formal sector workers. Thirdly, wage floors are set unilaterally by the government for selected industries, mostly made up of low wage workers.[2] Fourthly, any workers can become union members, and workers can seek a union recognition agreement if at least 30% of the workplace belongs to the union. Fifthly, and most relevantly for this study, when unions collectively cover 30 per cent of workers in an industry-location (idiosyncratically defined), they can apply with employers to be recognized by the government as a centralized bargaining council.. There are currently 39 legally recognized private sector bargaining councils in South Africa, each covering a specific industry-region (DoL, 2018). 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.

There have been over a dozen studies of union and collective bargaining premia in South Africa, with the earliest by Moll (1996), Schultz and Mwabu (1998), and Butcher and Rouse (1999). Wittenberg and Kerr (2019) provide an excellent review of the *union* premium literature, focusing on union wage premia and providing updated estimates.[3] They document a unionization rate of about 30% in the South African labor market, and estimate a union wage premium of 25-30%, which aligns with much of the literature and is generally higher than other countries. In a related paper, Kerr and Wittenberg (2021) argue that while unions tend to increase wages more for lower wage union members, most union members are in the upper middle parts of the wage distribution and this results in an inequality-increasing effect from union wage premia. As I discuss below, I find a very similar pattern for bargaining council workers, though a key result is that spillovers temper this disequalizing effect.

The literature on bargaining councils has been more limited than the union literature because it is difficult to cleanly identify bargaining councils. A contribution of this paper is to compile a publicly available dataset classifying industries and regions into each bargaining

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

[3]Recall that there is an overlapping relationship between bargaining councils and unionization: a large portion of bargaining councils are unionized, but many bargaining council workers are not unionized and many union workers are outside of bargaining councils.

council, with wages for each bargaining council separately by year. In addition, the existing papers use household survey data, which are limited by non-representativeness at the bargaining council level (as opposed to the tax data where I observe the full population of firms). While I do not focus on the level of the bargaining council wage premium, I do provide an estimate below of 15% in addition to the union premium.[4]

A major study in this literature on South African bargaining councils is written by Magruder (2012), who finds negative employment effects which are concentrated among smaller firms.[5] He uses a spatial regression discontinuity design, identifying the employment effects from either side of the boundaries of bargaining councils. There are a few concerns with this approach, for example the quality of household survey data used,[6] and the endogeneity of boundaries defined by the bargaining councils.[7] I find similar indications that there are employment losses at smaller firms – but this coincides with employment growth at higher productivity firms, suggestive of re-allocation effects. The policy conclusions from such a re-allocation are unclear, for example if workers are mostly re-allocating to more productive firms this would increase total production as well as wages. The extent to which this is countered by employment losses in small firms is an empirical question.

To date, it is my understanding that no paper in the South African literature has considered the effects of bargaining councils at the firm level, their spillovers on other firms, or their overall impact on the labor market structure. My analysis on spillovers also contributes more generally

[4] This compares to Bhorat, Goga, and Van der Westhuizen (2012), who combine survey data from 2005 with gazetted bargaining council documents to estimate a bargaining council premium of about 9% above the union premium. However, their instrument of union membership by union membership of others in the household, is persuasively critiqued by Wittenberg (2014) who notes this requires a logically inconsistent data generating process.

[5] Flowerday, Rankin, and Schöer (2017) also consider employment effects in three bargaining councils, the Metals and Engineering Industry, Clothing, and Textiles Bargaining Councils, similarly matching agreements into household survey data between 2010 and 2014. They find a negative employment effect of 8%. Their results are difficult to assess without indications of the quality of the control, pre-trends, or the first stage wage effect. For example, they note the largest negative effects are for the Clothing Bargaining Council, a sector in decline.

[6] The location of households is measured, not firms; and the industry is only measured at the 2-digit level, whereas many bargaining councils are more accurately defined at the 3 or 4 digit level. Household data are also not representative at the unit of analysis used (magisterial district by industry), with very few observations in some of the cells.

[7] Magruder does consider this, but finds some puzzling results when performing robustness tests, for example that firms bunch on both sides of the border (i.e. on the bargaining council side too).

to the literature on regulatory policies in the South Africa, such as state-set minimum wage effects where spillovers have only been considered in passing (Bhorat, Kanbur, and Mayet, 2013; Dinkelman and Ranchhod, 2012).

3.2 Construction of matched panel data

I provide detail on how I construct my main matched firm panel in the Data Appendix (section F). I provide a brief summary of the data construction here, and then describe the matched firm panel.

I collect bargaining council agreements from 2008 to 2018, record the industry, location and wage by year for each bargaining council, and match these to firms as demarcated by industry and location in the tax data.[8] I record the annual wages in these contracts using the actual bargaining council wage agreements published in official government gazettes, and also cross-check these wages against a tabulation of wages by bargaining council and year provided to me by the labor Research Service. I then match these industry-by-location wages to a matched employer--employee data set I have constructed from worker and firm tax records (National Treasury and UNU-WIDER, 2020a; National Treasury and UNU-WIDER, 2020b).

The matching between the tax data and the bargaining council agreements is imperfect since there is no direct correspondence between industry and location codes in the two sources. Another source of measurement error is that the tax data lack occupational classification (wages are bargained by occupation), meaning that only one wage can be used for each year in the bargaining council. I choose the occupation that corresponds to the "general" worker. As will be shown later in an event-study setup, observed wages track large sharp changes in the contracted bargaining council wages, which gives confidence that the identification of bargaining council firms as described above is not too noisy. In my analysis of spillovers, I exclude firms from the broader industry of the bargaining council to guard against contamination from identification error.

[8] Regions are defined at different levels, where 21 councils are national in scope, 5 are provincial and 13 are based on districts. In terms of industry, most bargaining councils are defined at the 3-digit industry level, though some are defined at the 2 or 4-digit levels.

3.3 Descriptive data on bargaining councils

Overall, bargaining council firms cover 40% of formal sector workers. Table A1 presents some comparisons between bargaining council firms, sectoral determination firms, and uncovered firms (as the omitted category).[9] I begin by describing the firm wage effects, which isolate the wage premium associated with the firm including bargaining council or union effects.[10] On average, bargaining council firms are higher-wage firms. Controlling for unionization, the bargaining council specific premium is about 15% (column 3). Bargaining council firms have much less within-firm wage inequality than uncovered firms (columns 4 and 5), a characteristic which is consistent with the literature (Cahuc, Carcillo, and Zylberberg, 2014), and which I show later is causally linked to responses to wage contract changes. Interestingly, bargaining council firms do not have conditionally different value added per worker than uncovered firms (column 6). This is entirely accounted for by industry controls, since bargaining councils *do* have unconditionally higher value added per worker.

Figure A1 shows that the sorting of workers by firm wage premia also differs for bargaining council versus other firms. Worker quality increases steeply with firm wage premia in non-bargaining council firms, as observed in several countries (Engbom and Moser, 2021; Song et al., 2018). However, such sorting is substantially lower for bargaining council firms, particularly above the median of AKM firm effects, suggesting that the higher firm wage premia are driven by the regulatory differences rather than for example firm production choices. Panel B further highlights that there is considerable overlap in the distributions of firm wage premia between bargaining council and other firms. These cross-sectional differences highlight the potential

[9] I use my constructed dataset to profile the South African formal labor market into its constituent regulatory regimes as described above. By merging union density at the municipality by industry level using the Quarterly labor Force Survey (QLFS, StatsSA 2020) for the corresponding years, we can have a more complete picture of the labor market in the data. About 20% of workers are part of high-union density industry-locations, but are not part of bargaining councils. A further 15% of formal sector workers are neither in bargaining councils nor in high union density cells, and the balance – about 25% – are uncovered. We can also factor in the 33% of workers in the informal sector in order to have a profile of the entire labor market, and in this case approximately 30% of workers are part of bargaining councils, 15% are in the union category, 10% are only covered by sectoral determinations, and 20% are uncovered.

[10] This follows Abowd, Kramarz, and Margolis (1999), where the firm wage premium is the firm component of a two way fixed effects regression of log wages on firm and worker fixed effects. See Bassier (2021) for more details and an application in the context of these South African tax data.

effects of bargaining councils, but also emphasize their partial and overlapping coverage.

Where are bargaining councils located? Figure A2 shows the proportion of bargaining council workers by industry and earnings decile. Bargaining councils are concentrated mainly in the manufacturing, construction, trade and transport industries in addition to covering the public sector (the major part of social services). Bargaining council workers are mostly in the upper middle parts of the firm earnings distribution, increasing in proportion from about 20% in the lowest decile up to 70% in the 8th decile and then dropping off to 30% in the uppermost decile. Part of this is endogenous: bargaining council firms are higher in the firm earnings distribution *because* they are bargaining council firms, i.e. wages are higher from negotiated contracts. But part of this is also the types of firms, as the *unconditional* value added for bargaining council firms is higher. Either way, this figure shows that *marginal* changes in the wage premium are likely to affect upper middle parts of the firm earnings distribution more.

Table F3 in the Data Appendix (section F) provides a detailed breakdown of the characteristics of each bargaining council used in my analysis. The largest is the Metals and Engineering Industry Bargaining Council with nearly a million workers, and there are several smaller bargaining councils with only a few thousand workers. There is substantial variation across most characteristics, with minimum wages as low as R2,500 per month or as high as R10,000 per month. In general, higher value added bargaining councils appear to have higher minimum wages, though profits are not strongly related.

In figure A3, I follow Card and Cardoso (2021) in decomposing log wages into the sum of a baseline wage (proxied by the first percentile of wages), the gap between the baseline and the relevant firm's minimum wage ("minimum wage gap"), the gap between the firm's minimum wage and the firm's average wage ("firm wage cushion"), and the gap between the firm's average wage and the worker's wage ("own wage gap"). I restrict firms to the private sector. The first panel shows that the minimum wage gap accounts for the majority of the firm average wage in bargaining council firms, ranging from the full average wage for the lowest value add firms to about half the firm wage for the highest value added firms.

The second panel of figure A3 displays the cross-sectional profile of workers as they age.

There is a steep initial slope in the firm wage cushion and own wage gap for youth, approximately until age 35, before leveling off. The minimum wage gap, however, has a much flatter incline across the age cohorts, suggesting perhaps more limited upward mobility in these bargaining council minima even if they do constitute a substantial component of the wage.

One stark characteristic common to private sector bargaining councils is the low proportion of women: on average, 30% compared to over 50% for the rest of the labor market. I estimate here that nearly half of the gender wage gap is accounted for by net differences in firm average wages as opposed to net differences in the own wage gap.[11] I then decompose this gender gap in firm average wages and find that over 80% of the gender gap in firm average wages is accounted for by differences in minimum wage gaps (figure A3). Thus the minimum wage gaps explain over a third of the overall gender wage gap for formal sector workers, and this is a result of the combination of bargaining council firms being high on the firm earnings distribution, along with bargaining councils having such a disproportionately low number of women.

There are differences even within bargaining council firms (bottom panel of figure A3). Women tend to be towards the lower minimum wage bargaining councils: 20% of the gender wage gap for bargaining council workers (or half of the gap in firm average wages for these workers) is accounted for by differences in minimum wage gaps. Finally, a notable exception is the public sector bargaining council which has a completely different gender impact to the private sector: there are more women than men in the public sector, and minimum wages actually favor women here.

Overall, the matching of agreements into the tax data reveals a profile of bargaining councils that shows higher firm wages, with wide variation across value added, minimum wages, and other characteristics. Bargaining council minimum wages constitute a large part of firm average wages. The next sections investigate the causal link between minimum and firm average wages, as well as their impact on other non-covered firms across the distribution of earnings.

[11] This is consistent with my previous analysis in Bassier (2021), where I similarly estimated that nearly half of the gender wage gap is accounted for by differences in the types of firms women are at compared to men (as measured by AKM firm wage premia).

4 Treatment effects of contracted wage increases

4.1 Empirical design

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

Bargaining council agreements are generally formed by core members, then extended to the rest of the industry-location, idiosyncratically defined. Though each agreement typically specifies wages by ad hoc region-by-occupation cells, wage increases are often "across the board": for example, the Road Passenger agreements for the years 2012-2016 each stipulate general percentage increases to the existing minimum wages, even though minimum wages are defined by occupation. Wage agreements are typically indexed to inflation, and set for three years ahead.

I identify events as large real minimum wage increases in the equivalent of a "general labor" occupational category (usually the lowest), where "large" is defined as greater than 3%. I exclude similarly large increases across the preceding 3 years (to ensure a clean pre-period). I combine approximately 50 of these increases across different bargaining councils in a stacked event-study design like Cengiz et al. (2019).[12] Figure B1 shows the distribution of all real bargained wage increases, concentrated just above 0, as well as the selected event-wage increases.

The control sample contains all non-bargaining council firms from the same calendar years that are in the larger region and industry of the bargaining council. For example, the restaurant bargaining council firms located in district councils in Gauteng province are compared to all firms in both Gauteng province and the trade industry. Given that spillovers may increase wages in the non-treated firms (see next section), I exclude connected non-treated firms from

[12]There are 47 different events, defined as a large wage increase within a separately bargained bargaining council industry-region. Note that some bargaining councils have multiple separately bargained industry-regions. There are 33 unique wage increases across these 47 events.

the regression sample, where connected is defined as firms with more than 1% of worker flows to bargaining councils. Figure B4 shows how much the wage effect is attenuated when including contaminated controls, from about 4% to 3%, highlighting the importance of choosing clean controls for the event study. I also restrict the sample of bargaining council and control firms to be balanced across the eventyears, and for firms to have at least 10 workers.

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

$$\begin{aligned} y_{j,t} \;&=\; \sum_{t=-3}^{-2} \delta_t(\tau_t \times treat_j) + \sum_{t=0}^{2} \delta_t(\tau_t \times treat_j) + \phi_j + \theta_{event \times loc. \times t} \\ &\quad + \gamma_{firmsize_{t=-2} \times t} + \alpha_{wage_{t=-2} \times t} + \beta_{\Delta lnfirmsize_{t<-1} \times t} + \psi_{\Delta lnwage_{t<-1} \times t} + e_{j,t} \end{aligned} \tag{4}$$

For intuition, identification of the main coefficients of interest δ_t arises from comparing changes in bargaining council firms to changes in similar firms (in terms of size and wage) within the same location at the same time. Pre-event $\delta_{t<0}$ is a test of pre-trends up to three years prior, and δ_t are all interpreted level to the outcome in $t = -1$.

The reason for the dense set of controls is that it is difficult to find control firms with similar pre-trends in firm size, without explicitly conditioning on them. I show in figure B4 that the bargaining council wage effects are nearly identical, with flat pre-trends, using a sparser specification which only includes firm and location by time fixed effects. I use this sparser specification with propensity score matching on dozens of pre period firm characteristics as a robustness check. Figure D5 shows clean pre-trends for the outcomes in this case, except again for firm size.

4.2 Results

Figure B2 shows large average wage increases following the contract changes. The 25th percentile of within-firm wages increases by 4% post implementation, with flat pre trends. The increase is slightly lower for median within-firm wages (3%), and in general decreases as I consider higher percentiles of within-firm wages (though at the 80th percentile, there is still a 2% increase). New hires are also paid 2-3% more after the event, again with reassuringly flat pre-trends. While these wage responses are largely expected, this is the first dynamic evidence of such effects across the firm distribution for South African bargaining councils. More generally, this shows a positive causal effect of bargained minimums on average wages, which has been challenged in other settings (Blandhol et al., 2020; DiNardo and Lee, 2004).

These wage increases vary substantially by the pre-event wage of the firm: at lower quantiles of average firm wages, there is little impact of the contracted wage increases, but the effect is higher than average for mid-waged firms (40-70th percentiles) showing point estimates of 5-7% wage increases.[13] The wage increases are lower for the top quantiles, perhaps because the contracted minimum wage increases are less binding for these firms. There is a very similar pattern for wage increases by firm size: for the smaller firms, up until about 15 workers, there are no statistically detectable wage effects, whereas for mid-sized firms between 15 and 100 workers, there appear to be large effects. The increases are once again lower and not statistically significant for the largest firms. The low response for low-wage and small firms may be due to exemption clauses in several bargained wage contracts 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.

I show other firm responses to bargained wage increases in figure B3. Average separations decrease by about 2% at the event year (statistically significant in event years 0 and 1), after flat pre-trends. Interpreted directly, this implies a firm labor supply elasticity of about 1.5 which suggests considerable monopsony power in line with Bassier (2021).[14] However, this is very likely

[13] On average for firms with more than 50 workers, the increase in the 25th percentile of within-firm wages is over 5% (with flat pre-trends, event study available on request).

[14] Using the 3% wage effect, yielding a separations elasticity of $-.75$, and the formula in Manning (2003a)

biased as an estimate of a reduction in turnover since I cannot differentiate between voluntary quits and involuntary fires. Indeed, average firm size does not show detectable changes, with a confidence interval between $-.02$ and $.01$. Unemployment insurance payments increase strongly, with flat pre-trends and an increase of about 1.5% – implying a positive co-variance between wages and amenities in these contracts. As we noted in figure B2 above, wages increase most at the bottom of each firm, and this figure shows the resulting decrease in within-firm wage inequality. Finally, there do not seem to be any systematic pre-trends in value added or the profit margin per worker, which is re-assurance that these estimates do not carry substantial bias from possible endogeneity of bargaining contracts to prior firm performance.

The main specification given by equation 4 includes pre-period controls of wages and firm size in both levels and trends. Figure B4 shows that these controls are not necessary for the wage effects, where using only firm and location by time fixed effects still results in flat pre-trends and a 4% increase in wages as in the main results above. However, using this sparser specification, firm size exhibits a pretrend which disappears with the additional controls. Figure B4 also shows what happens when we fail to exclude potentially "contaminated" controls: the wage effect on is about 3% instead of 4%. This hints strongly at the spillover results presented in the following section.[15]

Although these results show a sharp impact of the contracted wages, the average change in *contracted* wages can be above (due to weak enforcement) or below (due to higher wage firms also responding) the actual change in firm average wages. As a bounding exercise for this direct effect of contracted wages, I consider a counterfactual simulation of perfect compliance where I set a worker's wage equal to the relevant legal wage if below it, or leave the wage as is otherwise. Using the primary specification above with this simulated outcome, the post-period effect on median within-firm wages is only slightly higher (3.5%) than when using observed wages (3%). While the level of wages may exhibit substantial non-compliance across these firms, this implies

that the firm labor supply elasticity is -2 times the separations elasticity.

[15] We can actually use these estimates to back out a rough indirect estimate of spillovers. Noting that there are about 20,000 bargaining council firms, and a further 25,000 high spillover firms, the decrease in wage effect from 4% to 3% implies a spillover wage effect of about 2.2%. This is very close to several of the direct estimates of wage spillovers in the next section.

that the dynamic changes in wages follow the contracted wages quite closely.

5 Spillover effects of contracted wage increases

5.1 Empirical design

In a monopsonistic market, firms compete with each other for workers by setting higher wages. An increase in a treated firm's wage will elicit a response from "closely connected" firms, that is, firms that compete over the same pool of workers. Workers may leave connected untreated firms and move towards the treated firms. The connected untreated firms may also increase wages to incentivise workers against leaving towards the treated firms.

To quantify these wage responses, or spillovers, from connected untreated firms, a key question is how the treatment dosage is defined. Following the model-based measure of spillovers in section 2 above, and specifically the term f_{jk} in equation 3, I measure the average proportion of worker flows between each industry-location of non-treated firms with firms in the bargaining council in the event-study pre-period. The idea is that if the same workers are employable at different firms, this set of firms defines a fluid local labor market. labor constrained firms are competing strategically over this same labor pool, meaning that wage spillovers operating through the labor market should transmit through this channel of worker flows. This average flow measure is used to measure spillovers in the product market space by Bloom, Schankerman, and Van Reenen (2013).

My flow measure is also very close to one used by Arnold (2020) in defining labor markets. In deriving a measure of labor market concentration (akin to the Herfindahl–Hirschman Index or HHI) that is not dependent on discontinuous industry and location boundaries, Arnold (2020) defines the *value* of a job in industry-location A relative to a job in industry-location B as the flow of workers from B to A divided by the total number of workers in A. In my case, industry-location A can be considered the bargaining council. The major difference is then dividing by the size of A, which I show does not change the results substantially.[16] In my data,

[16]Theoretically, the division by size is motivated by that idea that the option value of a job in A for a worker

the coefficient in a regression of log wages on log value of the firm (defined following Arnold (2020)) is 0.1.

In terms of specification, I follow much of equation 4 used for bargaining council effects, except I replace the main variable of interest (previously the event-year treatment indicators) by the average flow measure for every firm $j(c)$ where c is the local labor market cell. This flow measure represents a treatment dosage. I exclude non-treated firms within the same industry-location as bargaining councils, in case these are purely errors in the treatment identification.

$$\begin{aligned} y_{j,t} \quad = \quad & \sum_{t=-3}^{-2} \delta_t(\tau_t \times f_{j(c)BC}) + \sum_{t=0}^{2} \delta_t(\tau_t \times f_{j(c)BC}) + \phi_j + \theta_{event \times loc. \times t} \\ & + \gamma_{firmsize_{t=-2} \times t} + \alpha_{wage_{t=-2} \times t} + \beta_{\Delta lnfirmsize_{t<-1} \times t} + \psi_{\Delta lnwage_{t<-1} \times t} + e_{j,t} \qquad (5) \end{aligned}$$

Identification now arises from variation in pre-event connectivity: comparing non-treated firms of varying degrees of connectivity to bargaining councils but within the same location and of similar firm size. That is, do untreated firms that are more strongly connected to treated firms exhibit outcome responses to the contracted wage events?

As in the earlier specification, I make sure to exclude contaminated controls: here, these are firms that have low connectivity to the local bargaining council, but high connectivity to another bargaining council (perhaps a different industry in the same location). In addition to firms in the same location as treated firms, I include in the non-treated firm sample the non-treated firms that are in the same *flow-estimated* labor market. I cluster firms by network, and include a network if any treated firm is a part of it (except for the five largest networks, which are less meaningful). The aim is to avoid excluding firms that are connected to treated firms but outside of the immediate geographical location.

I use a split-sample IV strategy for the main estimates to reduce measurement error in the $f_{j(c)BC}$ variable. As a generated regressor there will be noise in this variable compared to the true value of the firm connectivity, and this will attenuate the coefficient towards 0. I avoid this

in B may decrease with A's size since flows are more likely to a larger A, conditional on the same value.

attenuation bias by randomly splitting firms in each industry-location, and instrumenting the average flow for the firm's own sample by the average flow for the complement sample within each industry-location. The split sample instrument has been used for example by Bassier, Dube, and Naidu (2021) and Goldschmidt and Schmieder (2017).

As separate specifications, I estimate a inverse-propensity score weighted regression analogously to the bargaining council firm regressions. I also estimate a binary version of equation 5, where I compare highly connected firms (greater than 5% flows) with unconnected firms. Finally, I run an individual level specification as I did for bargaining council workers, with treatment here defined as workers in highly connected firms.

A brief comment on the constructed regressor, the flow variable. Why not simply compare firms in the same industry-location as bargaining council firms? Firstly, industry and location are in fact not good indicators of whether firms draw from the same labor market. One way to see this is to compare the flow of employment-employment firm switches across the full sample of workers. Only 30% of switches are within the same Standard Industrial Classification 1-digit industry! Fully one third of switches are to a different province entirely. Using industry and location is a weak proxy for flows, which we can rather directly observe through worker transitions as I have done. A second reason is more practical: since the bargaining council identification is fuzzy, choosing spillover firms only in the same industry-location may pick up *actually-treated* bargaining council firms, thereby estimating spurious spillover effects.

5.2 Descriptives of spillover firms

What do these spillover firms look like? Table C1 shows the characteristics of firms, by the proportion of its flows to bargaining councils (as explained above). I categorize firms into bargaining council firms, and then firms with high, medium, low and negligible flows to these bargaining council firms.[17] In this stacked event firm dataset used in the main specification (restricted to event year -1 and firms with more than 10 workers), nearly a third of bargaining

[17] The categories are arbitrarily divided as follows: high is above 0.1, medium is between 0.05 and 0.1, low is between 0.01 and 0.05, and negligible is below 0.01.

council firm flows are to other firms in the same bargaining council. The high and medium categories together have close to the number of firms in the relevant bargaining councils (25,000 compared to 19,000), as well as the number of workers (4 million compared to 5 million). The firm size is considerably larger in bargaining councils than in spillover or other firms (270 compared to 170). Within firm size categories, there are slightly more bargaining council firms in higher categories, but about two-thirds of (unweighted) firms for both have between 10 and 40 workers.

As described in section 3, bargaining councils have a very low proportion of women — but the proportion of women in connected spillover firms is much larger. This highlights the importance of accounting for spillover effects when evaluating the aggregate causal effects of bargaining councils, in this case on gender wage inequality. Similarly, the AKM firm wage effects are 10-15% higher in bargaining councils, again highlighting the equalizing effects of spillovers on the aggregate distribution. In general, the other characteristics such as wages, churn, AKM worker fixed effects (as a proxy for worker quality), and profit per worker are similar.

Where are the high spillover firms located compared to the bargaining council firms? Figure C1 shows the geographic location of Metals and Engineering Industry Bargaining Council firms as an example. Bargaining council firms are located in the urban centers (Gauteng region features most prominently), and this is replicated in the map of spillover firms. Even the region straddling the Northern Cape and North West, which is not a major urban center, also shows spillover firms in the same area. Indeed, it would be strange otherwise, if workers were switching between firms that were geographically distant.

Figure C2 provides some intuition about the connectivity of firms in general. As discussed in section 2, connectivity depends on the *connected set* of any firm. As an illustration of this, I find for each firm the share of workers that go to other firms.[18] On average, the number of connected firms is about a tenth of the size of the firm. The figure plots the maximum share

[18] If a firm is in the connected set and offers wages, but is rejected by all workers, then I will not observe this firm as part of the connected set.

of these connected firms, as well as the concentration of those shares. In both cases, the plots show that connectivity increases tightly with firm size.

Finally, figure C3 compares the proportions of bargaining council, spillover and other firms by industry and earnings decile. Spillover firms are distributed much more evenly across both industries and earnings deciles than bargaining council firms. The implications are that there is a lot of movement across industries as well as firm earnings classes — as noted above, only 30% of switches are within the same 1-digit industry. The spillovers observed below therefore propagate diffusely through the economy.

5.3 Results

Figure C4 shows large spillover effects from contracted bargaining council wages on connected firm average wages. For an indication of magnitude, I scale the outcomes by the average flow in approximately the top 24,000 firms.[19] To interpret this, the figure shows that average wages in industry-locations with greater than 5% of pre-event worker flows to the relevant bargaining council experienced an average increase of nearly 4% at the 25th percentile of within-firm wages and 3.5% at the 50th percentile of within firm wages. This is as large as the comparable wage effects on directly treated firms.

The two-year out wage spillovers are substantial and significant across the distribution of within-firm wages, with smaller effects for better paid workers within these firms. There are also substantial effects for spillover firms across quantiles of the between-firm wage distribution, with similar patterns to what I found for bargaining council firms: the effects are not statistically significant for the lowest or highest wage firms, but the wage effects reach as high as 5% in the middle waged firms.[20]

[19]This is an arbitrary scaling, since every firm in the entire economy has a measure of flows, even if most would be 0. 24,000 is chosen because it is the number of firms with more than 5% of its flows towards bargaining councils. It is also a comparable figure to the number of firms in bargaining councils. The average flow for these firms is about .09.

[20]Recall that I am careful to exclude potential bargaining council firms in these regressions, by excluding all firms from the spillover regression that are in a similar industry to the bargaining council. 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 contracts do not enter the regression. This also means that the spillovers potentially *underestimated*, since these firms that are excluded

Figure C5 shows other firm outcomes. Separations decrease strongly in the post-period, equal to about a 4% decrease in separations.[21] Firm size may decrease, with a point estimate of -2%, but is not statistically significant. As for bargaining council firms, unemployment insurance payments increase strongly, up by 2% by the final event year. For each of these outcomes, including the wages, the effect appears strongest in the final event year. This implies some kind of lagged response, as wages increase by the first year after the event in bargaining council firms, and may take a year to propagate outwards through worker flows.

As in the case of bargaining council firms, there is a lower wage effect for higher percentiles of within-firm wages, and this results in a decrease in within-firm wage inequality. The increase in wages for spillover firms also comes out when looking at individual level workers, especially when considering low-wage workers as well as stayers. That is, the wage effects on spillovers are not purely about worker composition or differential wages for new hires.

Once again, profits margin per worker do not exhibit pre-trends, which is reassurance against differential prior firm performance driving these results. The post-period decline in profits for these firms highlights a potentially sharper trade-off between profits and wages for spillover firms than for bargaining council firms. Such a trade-off between profits and wages is consistent with contracted wage increases which tend to be about splitting rent rather than choosing a point on a wage-employment locus (Draca, Machin, and Van Reenen, 2011). How plausible is this trade-off in terms of the observed reduction in profit and increase in wages? I perform a counterfactual simulation where I increase each firm's wage-bill 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, effort effects, or the non-linearity of the marginal profit per worker, it is re-assuring that the average reduction in profit per worker implied is 3.3%. This is not far from the decrease in profit per worker estimated for spillover firms, which ranges from 3 to 7%.

In table C2, I provide the point estimates for the final period as well as for alternative

are high-flow firms (depending on the linearity of the relationship of spillovers to the flow measure).

[21] This does cannot directly be interpreted as a firm labor supply elasticity, as the model in section 2 illustrates.

specifications of these spillover regressions. Column 1, the main specification, shows responses for the 25th and 50th percentiles of within-firm wages, as well as firm size and profit margin.[22] Column 2 shows the estimates using OLS, demonstrating that there is substantial attenuation associated with the generated flow regressor and corrected by the IV split-sample strategy. However, as expected relative to an IV regression, the OLS standard errors are much smaller. Column 3 presents the results when using a binary indicator for treatment, as opposed to a continuous flow treatment regressor, with similar results to the main specification.

Columns 4-7 add various controls. Column 4 adds industry by time fixed effects, to account for any trends in industries that may be shared with the bargaining council and driving the results, for example a booming export industry which enabled the contracted wage increases to begin with. While the magnitudes are smaller, the qualitative results are similar; note that some shared industry-by-time effects may be "true" causal effects but will be subtracted from this regression. Column 5 controls for the churn of a firm, interacted with time, to address concerns that these spillover effects are simply picking up wage growth in firms with high churn. The effects are again comparable to the main specification. Column 6 estimates the flows directly between industry-locations (rather than between firms, which are then aggregated to industry-locations). Finally, column 7 divides the regressor by the churn in the firm as in the value measure used by Arnold (2020). The results are similar across these specifications, ranging from 1 to 4% for the wage spillovers (all strongly significant with relatively flat pre-trends), and from 0 to -2% for the firm size effects (some significant, some not).

These results provide strong evidence that spillovers exist, that their operate through local labor market connected sets, and that they are substantial for the connected spillover firms.

[22] The event year 2 effect is not statistically significant for the profit margin, but as shown in figure C5, it is significant with comparable magnitude in event year 1. It is also significant in the final year for several alternative specifications.

6 Discussion

In my analysis of the impact of bargaining council wage contracts, I presented evidence of large effects on bargaining council firms, including increases in wages and negligible effects on firm size. Importantly, I show that firms that are not part of bargaining councils, but that are strongly connected to them via worker flow and substitution patterns as observed in the data, also increase their wages after the bargaining contract wage increase in what I interpret as a spillover effect. I discuss these results further below, showing the aggregate effects on the labor market, suggestive evidence of re-allocation, considering robustness of these results to various specification concerns, and highlighting important aspects of heterogeneity.

6.1 Aggregate labor market effects

How important are the bargaining council wage increases for the aggregate earnings distribution? Given the evidence above that spillovers exist and are substantial for the affected firms, how important are they compared to the direct bargaining council effects? As table C1 shows, the number of workers and firms in bargaining councils and their highly connected sets are similar; together with the similar cross-wage effects, this suggests that spillovers may be as important for the earnings distribution as the direct effects on bargaining councils.

I illustrate this more precisely in figure D1, where I use the full firm by year panel in the tax data, and simulate the effects on the distribution using the actual characteristics of firms. This micro-simulation exercise just uses the incidence of bargaining council status and flows, and does not take into account other effects for example on assignment of workers to firms. I add on the bargaining council premium to bargaining council firms, estimate the flows from each non-covered firm to bargaining council firms, then use the causally estimated cross wage elasticity from section 5 above to add on an associated spillover effect.

What is a meaningful bargaining council wage premium? In section 4 above I show the wage effects of *particular* wage contracts (e.g. large increases with clean event study pre-periods), which leave out much of the total effect. One estimate of the premium across the labor market is to use the actual wage contracts over the period 2008-2018. On average, the bargaining council agreements increases real wages by 15% over the 11 year span. Average non-covered firm growth is close to zero over this period, and recall that the pass-through of contracted wage to actual wages is close to 1. It turns out the OLS estimate of the bargaining council premium from a regression of AKM firm fixed effects on a bargaining council indicator is very close to 15% (table A1).

Figure D1 plots the simulated counterfactual wages by AKM worker quantile. The figure shows in blue the direct effect of bargaining councils on the aggregate earnings distribution, on average about 5%. The largest effects are for the mid-quantile workers, with the smallest effects at the top of the worker distribution. The spillover effects more than double the total impact of these bargaining council wages, reflecting the local connections from bargaining councils that flow through the entire labor market. These spillover effects are more evenly spread through the distribution, adding about 7% to the wages of all worker quantiles. These distributional effects reflect the location of spillover firms, as shown in figure C3. Similarly, due to the mix in locations of bargaining council and spillover firms along the firm earnings distribution, the effects on inequality are negligible in this simulation.

In summary, this simulation — based on the actual wage contracts and evidence of causal spillover effects above — suggests bargaining councils increase average wages by about 10% across the distribution, and by even more for actual bargaining council firms. Neglecting to account for spillovers would make the bargaining council effects appear to be much smaller, as well as potentially disequalizing.

6.2 Re-allocation

Although the aggregate effect on bargaining council firm size is negligible, I show that there are significant decreases in employment for low wage firms in conjunction and there is suggestive evidence of increases in firm size for higher wage firms. Theoretically, section E.3 lays out how re-allocation of workers from low wage to higher wage firms can occur when high-wage firms are labor constrained and so an increase in their wages leads to expansion, and low-wage firms have low productivity, cannot pay the minimum wage, and so are forced to downsize. These need not be exactly the same workers who transfer from low-wage to higher wage firms, especially if the connected sets of these respective firms do not overlap. These dynamics are recorded elsewhere in response to minimum wages, for example Germany (Dustmann et al., 2021) and Sweden (Bustos, 2021).

Even in a high unemployment environment such as South Africa, bargaining council firms could plausibly be labor constrained if the jobs require skills and experience that are not easily found at the prevailing wage and if workers do no not easily switch firms. On the other hand, if firms are marking down wages for other reasons, a minimum wage could still force out the lower wage firms while raising wages without affecting employment in higher wage firms.

In figure D2, I show the coefficients of the main bargaining council firm specification 4, with additional interacted indicators for tercile of the bargaining council wage distribution. As before, I show the outcomes of firm median wage, firm separations, and firm size. The bottom tercile shows a statistically significant decrease in firm size, along with very little change in separations despite the increase in wages.[23] In contrast, the middle tercile shows a positive but not statistically detectable change in firm size, as well as a marginally significant decrease in separations. Adjusting for the different wage increases by dividing these firm size changes by the wage coefficient, the own-wage elasticity for the bottom tercile is large and negative at -0.7 compared to the middle tercile which is positive at 0.3. As another indication, figure **??** shows the results from a similar regression, this time considering deciles of value added per worker. The figure shows that the lowest value added firms decrease in firm size relative to the highest value added firms.

As explained above, these results should be interpreted with caution. A key question is whether there was an increase in firm size for higher wage firms. Here, the firm size effect is not significant, but as I show below in table D2 the coefficient is positive and significant using other

[23]Note these bottom tercile firms are not the same as the low-wage firms shown in figure B2. The former, discussed here, are the bottom tercile of *bargaining council firms* which, are actually in the middle part of the overall distribution.

measures such as for higher AKM firm fixed effects. Ultimately, this question relates to what the aggregate effect on employment was: the mean point estimates are negative but neither statistically significant nor robust. In the case of neutral aggregate employment effects, there could be substantial churn effects if the workers who lost jobs are not the same as workers who gained jobs, even if this does mean the mass of workers in bargaining councils moved towards higher wage firms. Lastly, there are aggregate productivity effects, as workers move towards higher value added firms.

6.3 Robustness

My main results are robust to a number of concerns, with alternative specification results shown in table D1. Column 1 shows the main OLS regression results, for bargaining council firms (top panel) and for spillover firms (bottom panel). I show the cross-wage elasticity (CWE) as the log wage effect on median firm wages for spillover firms relative to bargaining council firms. In column 1, the cross wage elasticity is about 0.8.

Column 2 presents the results from a specification which includes firm-specific linear pre-trends, constructed over the pre-period. This renders the pre-trend test meaningless (since there is no pre-trend by construction), but partials out any pre-trend if that was a concern in any of the previous event studies. The estimates are extremely similar as expected.

One weakness of the bargaining council event study is that the number of years before the event is chosen arbitrarily, in my case as a balance between having enough years to judge a pre-trend with retaining enough events. Bargaining councils generally re-negotiate wages annually, with major rounds in 3-year intervals. In column 2, I therefore exclude major events across event years -4 to -3, which is just outside the primary event study period but would adjust for the previous bargaining round. For example, a concern is that lagged dynamic effects of the contract changes in the previous round would show up as pre-trends in my pre-period. While about half of the number of events are lost in this cut, the results for bargaining council and spillover firms are very similar. Figure D4 shows the event studies, notably a flatter pre-trend for the wage spillovers.

Column 4 weights the main specification by pre-period firm size, for an indication of worker-level effects. The point estimate of the firm size effect is more negative, though the 95% confidence interval is large and includes up to a 0.07 increase in log size for both bargaining council and spillover firms. The change in profit margin is *positive*, which suggests that the negative profit margin effects are concentrated among smaller firms that face the wage-profit trade-off most sharply.

In column 5, I use an alternative specification based on propensity score re-weighting instead of regression matching. I regress the bargaining council firm indicator on several pre-period characteristics to produce a propensity score, and analogously for high spillover firms. I then use the primary specification in (equation 4), without the fixed effects for pre-period levels and growth in wages and employment. In this regression, I am only controlling for firm fixed effects and location by time fixed effects, but I am additionally matching on pre-period characteristics. That is, I investigate wages in bargaining council firms compared to similar firms in the same location, by period. Regression matching allows potentially less comparable firms to act as controls, which has the advantage of including more controls (where valid) and disadvantage of including less comparable controls (where invalid). Figure D5 shows the event-studies, with relatively flat pre-trends aside from the bargaining council firm size effects. The estimates are qualitatively similar but quantitatively larger for bargaining council firms. The wage effect is smaller and not statistically significant for spillover firms, but is statistically significant using other measures such as annualized firm wages. I also run a "double robust" specification, that is, running the main specification (equation 4) with the full set of fixed effects as specified there, and with the additional weighting of firms by the propensity score weights.[24] The advantage is that this specification is robust to concerns with both propensity score and regression match (Arkhangelsky and Imbens, 2021). Results are once again similar, including significant effects on the log wage spillover outcome (0.025, with standard error of 0.012).

Lastly, I address the concern that bargaining council contracts may be endogenous to local

[24]Results available on request.

economy trends. In column 6, I restrict events to bargaining council contracts that are negotiated at the national level, thereby excluding more local-level bargaining. This check has the additional advantage of addressing measurement concerns, since location for multi-branch firms may not always be accurately recorded, meaning that my classifications of firms in local-level bargaining councils may miss out this important group.[25] Results are similar to the main specification.

6.4 Heterogeneity

I discuss heterogeneity in these main results across four dimensions. First, I run an AKM regression on all firms in rolling 3-year periods, which allows me to divide firms into high and low firm fixed effects based on event pre-periods.[26] Columns 1 and 2 refer to below and above median firm fixed effects respectively. While wage effects are statistically significant in both cases of bargaining council firms, the firm size and separations effects are significant and of opposite signs – consistent with the re-allocation effects I presented earlier (subsection 6.2), where lower wage firms decrease in firm size and higher wage firms expand. A similar contrast is observed for spillover firms. The magnitude of the wage spillovers are much larger for low firm effect firms, but the own-wage elasticities driven by the firm size effects are of opposite signs.

Next, I consider heterogeneity by Kaitz index, that is, the minimum wage to local median wage ratio. One advantage of centralized bargaining councils is that locally negotiated wages may be set in a way that is more optimal for the local labor market. We can test this directly by considering national bargaining councils, where wages are set across regions, and testing for differential effects where minimum wages were set relatively higher or lower than the local labor market median. Columns 3 and 4 stratify within each event the industry-regions with

[25] Firms are legally required to input the address of the branch of the worker. However, the data suggest that some firms are simply putting the address of headquarters instead of each branch. Data administrators at the tax data facility are continuing to investigate this issue.

[26] This follows Abowd, Kramarz, and Margolis (1999), where the firm wage premium is the firm component of a two way fixed effects regression of log wages on firm and worker fixed effects. See Bassier (2021) for more details and an application in the context of these South African tax data.

low versus high Kaitz ratios respectively. For firms with a low minimum wage relative to the median local wage, effects are generally more muted: the bargaining council wages only increase by 3%, and spillover firm wages increase by 1.5%, with little change in firm size in either case (narrow confidence intervals). On the other hand, where the minimum wage is high relative to the median local wage, the wage effects are much larger (as expected), but so are the decreases in firm size. *Bargaining council* firms in this case see a large decrease in profit margins.

Measuring monopsony power as the firm labor supply elasticity within each bargaining councils, columns 5 and 6 show bargaining council events with above and below median monopsony power (low and high labor supply elasticities respectively). The low monopsony power firms show much stronger responses on wages and a much stronger response in separations, while spillovers appear substantially stronger for high monopsony power events.

Finally, I compare effects on men and women. I restrict each of the outcomes, by firm, to men (column 7) and women (column 8) separately and run the primary specifications as for the main results. Wage effects in bargaining council firms appear stronger for women, with greater increases in firm size. The wage spillovers are similar across men and women. These differential results may be driven by a larger proportion of women being located in the lower wage bargaining council firms, as described in section 3.

7 Conclusion

This paper demonstrates the direct and indirect impact of centralized collective bargaining on the labor market. I find that, following a large wage increase mandated in bargaining council contracts, observed bargaining council firm wages increase, and there is no statistically detectable effect on firm size on average. Firms that are strongly connected to the same local labor market as these bargaining council firms see wage increases of a similar magnitude, together with a decrease in profit margins. A simple simulation suggests that such spillover effects double the direct impact of these bargaining council contracts. Together with the evidence of re-allocation effects, this highlights the broad ranging impact of such institutional regulation on the aggregate wage structure.

The methodological contribution of this paper is to ground the measure of spillovers in a view of the labor market that has monopsonistic competition and localized labor markets. This measure is motivated by a simple model featuring these characteristics, and supported by a recent empirical literature. While this spillover mechanism focuses on the flow of workers connecting firms through overlapping local labor markets, there are several complementary mechanisms such as norms of fairness (workers in spillover firms may benchmark their level of the fair wage on the bargaining council wage), or union threat effects (spillover firms choose to provide wage increases rather than risk their workers unionizing and thereby demanding a full non-wage benefits package) that may be explored in future work.

The labor market dynamics discussed in this paper highlight the potential power of regulation, whether in the form of minimum wages or collective bargaining, to influence the wage structure in a monopsonistically competitive labor market. Centralized bargaining councils or wage boards are a popular policy recommendation to constrain monopsony power (Dube, 2018; Stelzner and Paul, 2020), and South Africa's centralized bargaining councils are thus an illuminating example. Altogether the propagation of centralized wage regulation through connected labor markets provides an institutional lever for reversing the trend of rising between-firm inequality in several countries (Card, Heining, and Kline, 2013; Song et al., 2018).

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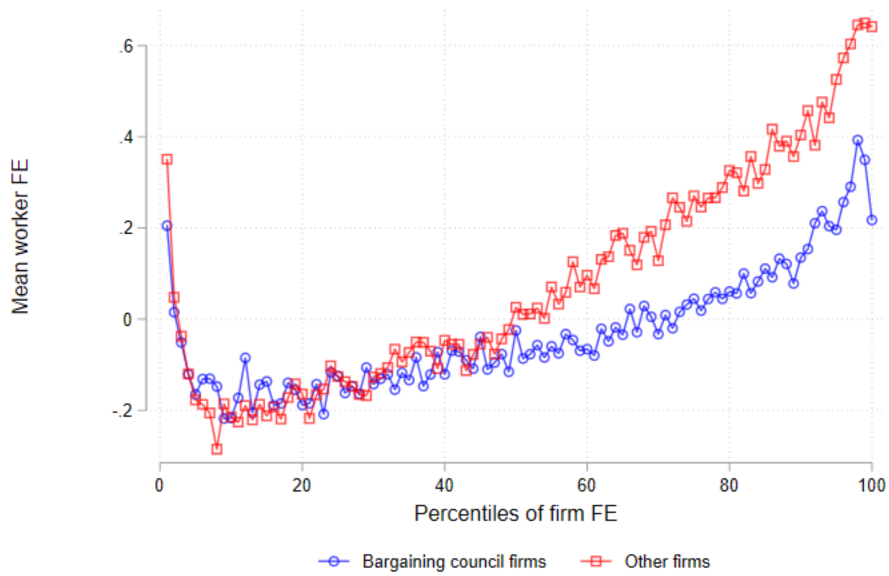
A Figures: Description of Bargaining Councils

Table A1: **Bargaining council premia**

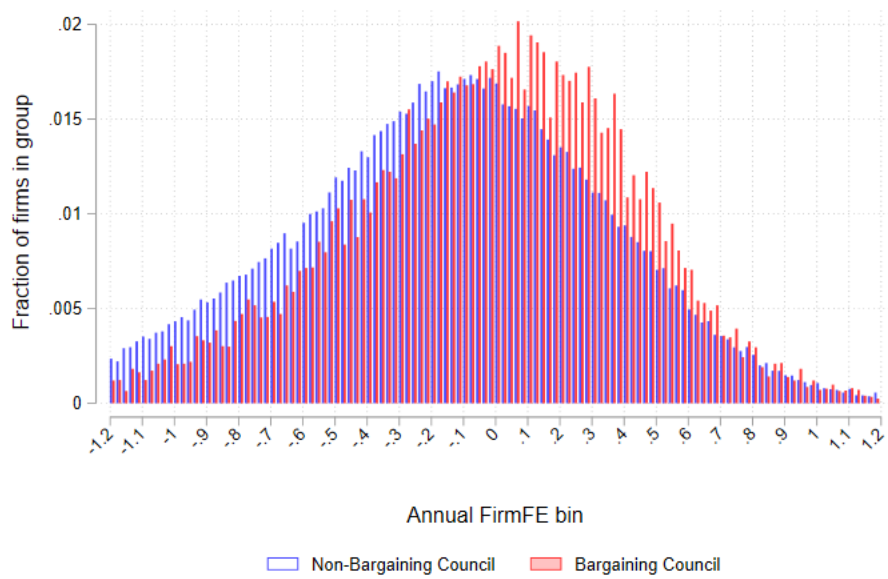
	AKM Firm FE			Within-firm wage inequality		Value Added pp
				p90-p50	p50-p10	
	(1)	(2)	(3)	(4)	(5)	(6)
Bargaining council	0.297***	0.315***	0.148**	-0.153***	-0.111**	-0.010
	(0.060)	(0.041)	(0.061)	(0.027)	(0.048)	(0.051)
Sect. Determination	-0.198***	0.098	0.003	-0.155***	0.067	-0.079
	(0.057)	(0.060)	(0.069)	(0.033)	(0.042)	(0.053)
Obs	644789	639710	544864	1678578	1678550	1029023
Outcome	ffe	ffe	ffe	lnwagep90p50	lnwagep50p10	lnvap
Controls						
Worker quality	Y	Y	Y	Y	Y	Y
Indus + Loc FE		Y	Y	Y	Y	Y
Union			Y			

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

Figure A1: **Bargaining councils, AKM Firm FE and sorting**



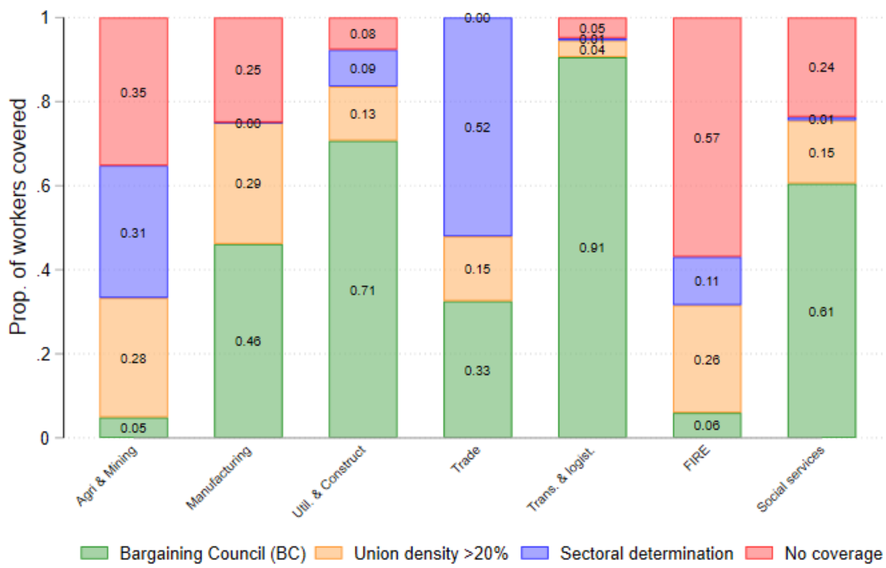
(a) **Sorting of firm and worker FE**



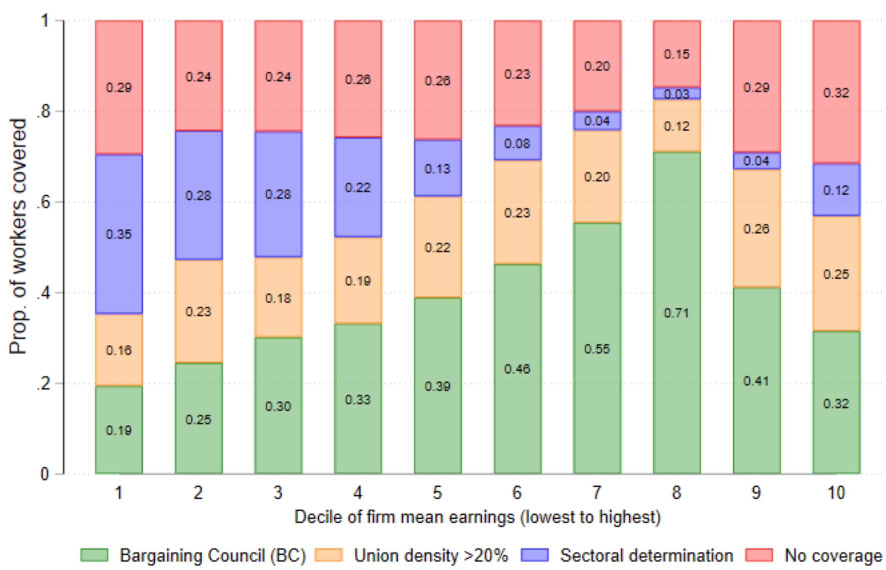
(b) **Distribution of firm FE**

Notes. AKM firm and worker FE refer to the respective components from a regression of log annualized wages on two-way fixed effects for workers and firms, estimated over the full sample from 2008-2018. The figures are plotted at the unweighted firm level. The percentiles of firm FE are calculated on the full distribution, i.e. the values of firm FE for the bargaining council and other firms are comparable at each percentile. The sample is all formal sector firms from 2008 to 2018 using the SARS tax data, with average firm size greater than 20 workers (for estimation by AKM).

Figure A2: **Distribution of workers by regulatory regime**



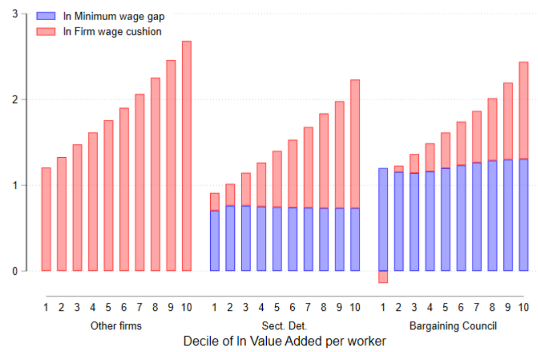
(a) **By industry**



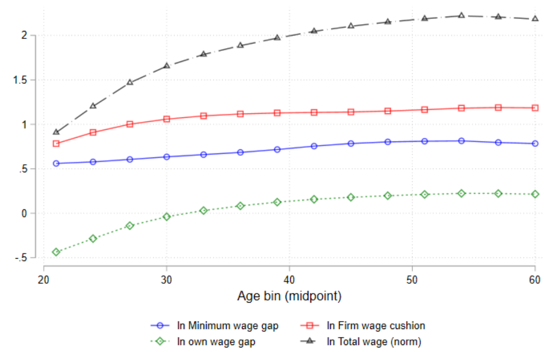
(b) **By firm average earnings decile**

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

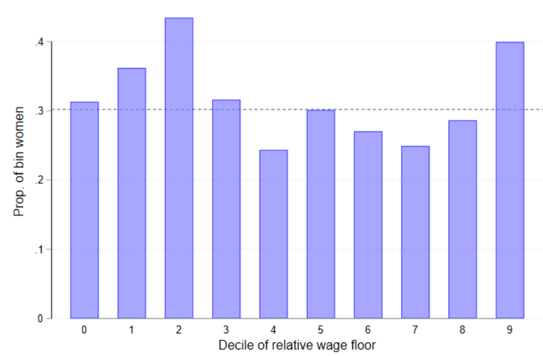
Figure A3: **Decomposition of wages: Floors and cushions**



(a) **Comparison of regimes**



(b) **Age profile**

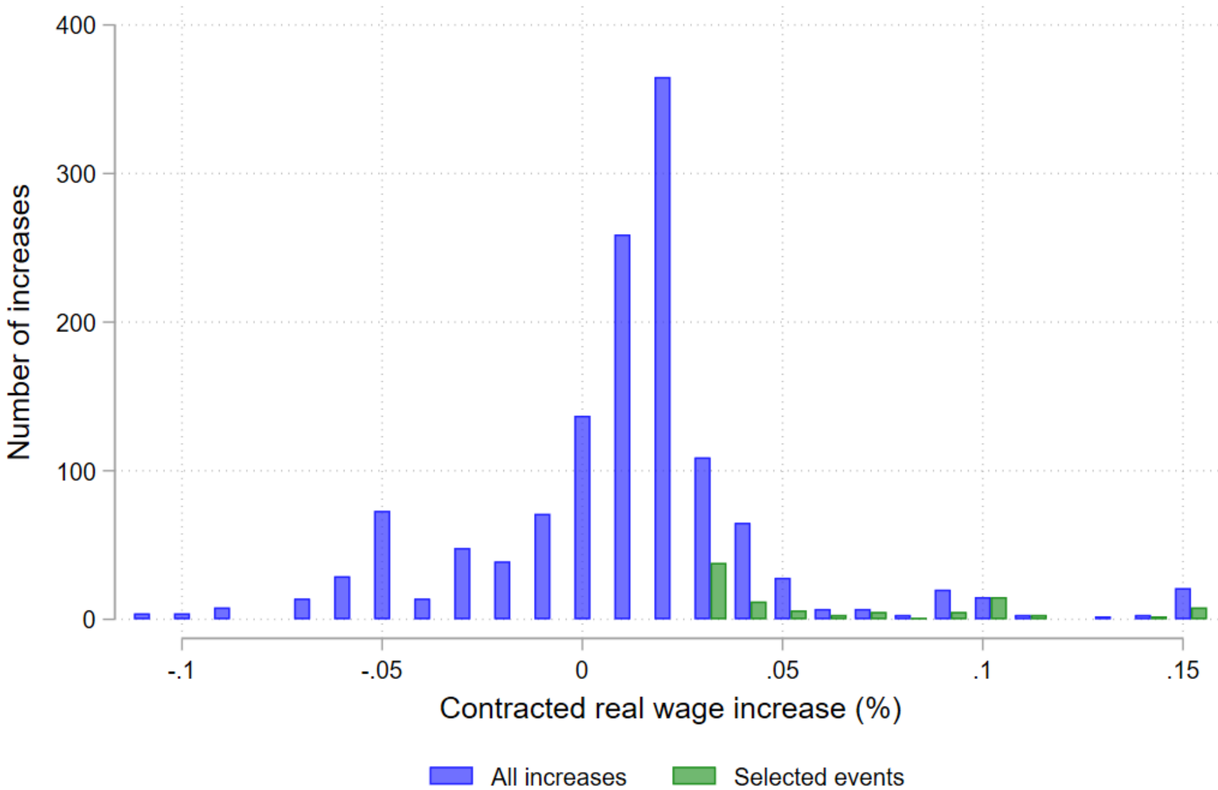


(c) **Density of women among bargaining council wage floors**

Notes. The analysis follows the decompositions suggested in Card and Cardoso (2021). I decompose the wage into a baseline (percentile 1 of wages), the gap between the baseline and the bargaining council wage floor, the gap between the wage floor and the firm average wage ("firm cushion"), and the gap between the firm average wage and the worker's own wage. The top figure compares the floors and firm cushions at firms in bargaining councils, sectoral determinations and that are not covered. The middle figure shows the density of women, by decile of wage floor and firm cushion across bargaining councils only, where the average proportion of women in bargaining councils is shown as a horizontal line (30%). The bottom figure shows the worker-level cross-section of each component by age. The sample is all formal sector firms from 2008 to 2018 using the SARS tax data.

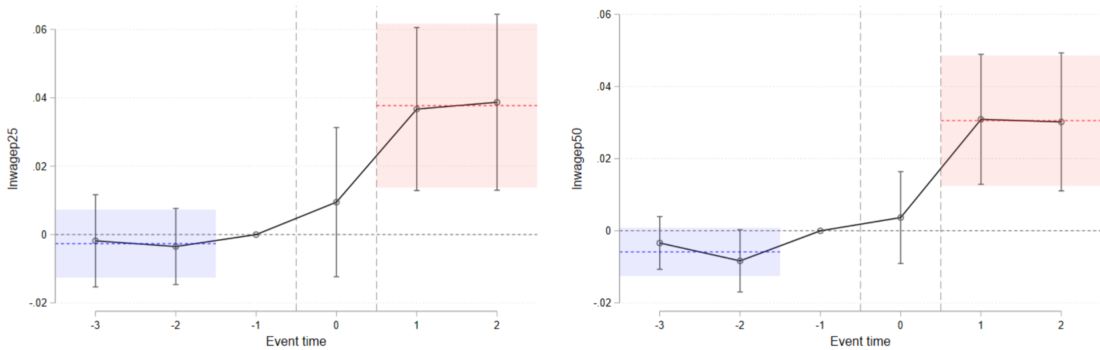
B Figures: Effects of Contract Changes on Bargaining Council Firms

Figure B1: **Bargaining council wage increases and selected events**, 2008-2018

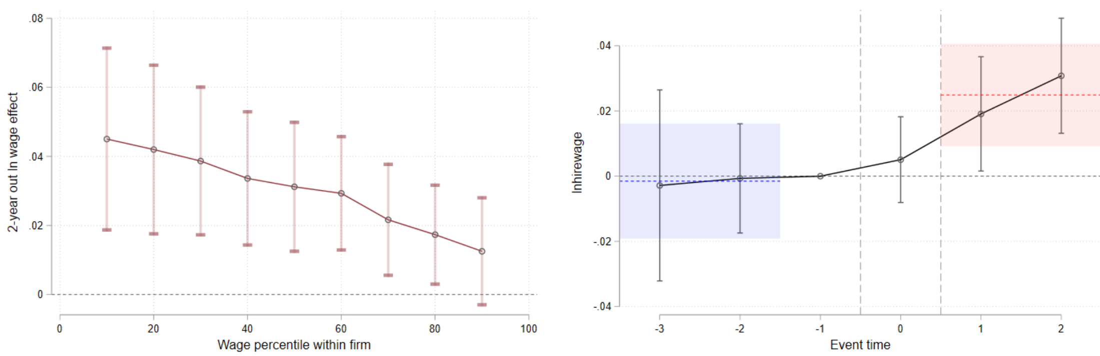


Notes. Out of all annual bargaining council wage increases, events are selected based on (i) A minimum of a 3% real wage increase, (ii) At least 3 pre and 3 post periods (implying only 2011-2016 admitted), and (iii) No real wage increases greater than 3% in the pre-period. The final bar in the figure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

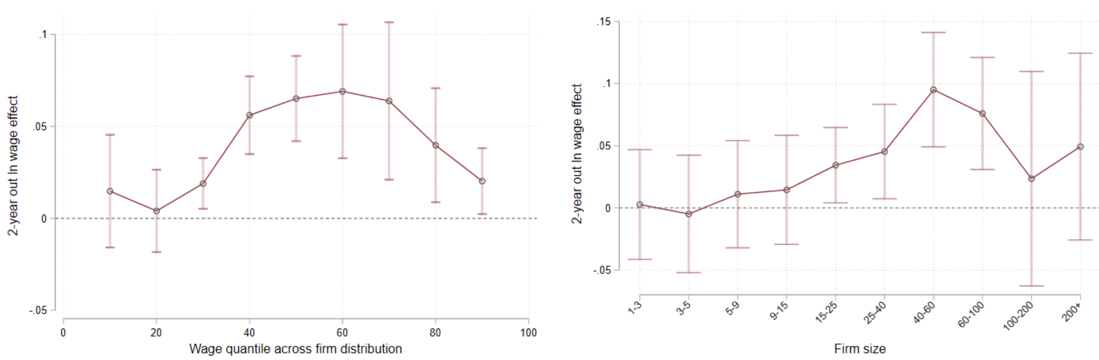
Figure B2: **Effect of contracted wage increases on wages of bargaining council firms**



(a) **Within firms: p25 and p50**



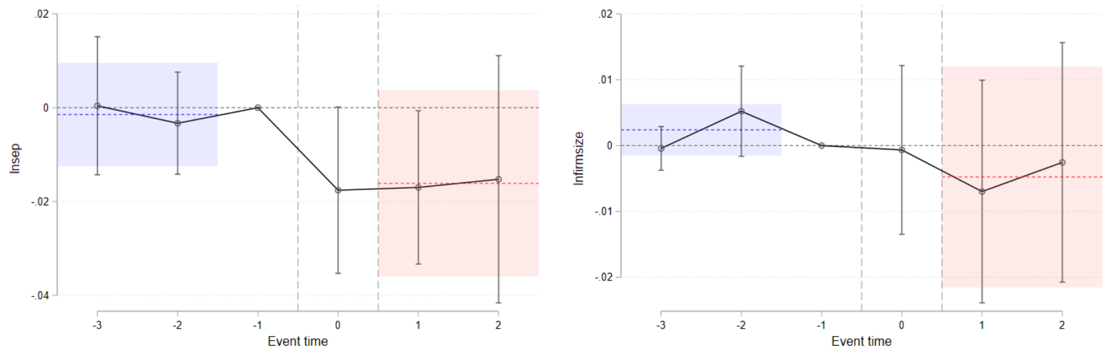
(b) **By quantile within firms and new hires**



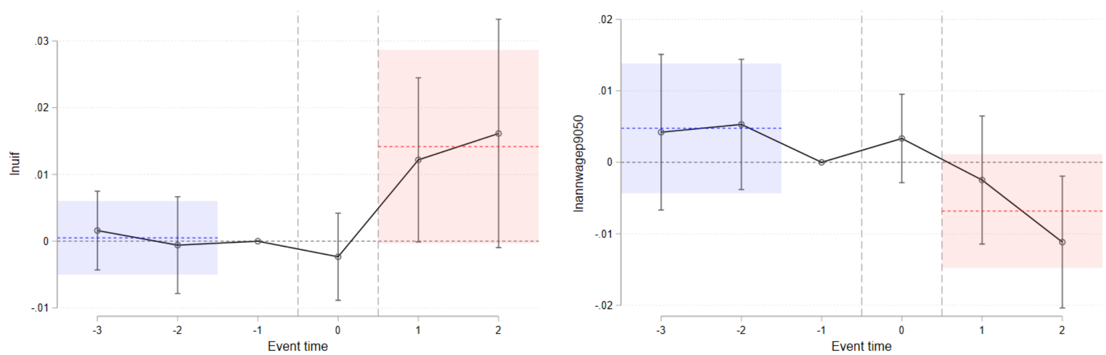
(c) **By quantile across firm wages and firm size**

Notes. Each event contains non-treated control firms, and are stacked in a panel unique on event, firm and year combinations. Firms with more than 1% of worker flows to the bargaining council are excluded from the control. The regression is run at the unweighted firm-level, is restricted to balanced firms with more than 10 workers in the pre-period, and includes fixed effects for firm, location by year, firm size by year, and growth in pre-period wage and employment by year. Standard errors are clustered at the level of bargaining council treatment by event. The top panel outcomes are the 25th and 50th percentiles of within firm wages. The middle panel plots the coefficients at event-year 2 for separate regressions by quantiles of with firm wages and quantile of firm in the firm distribution of 25th percentile within-firm wages. The bottom panel shows the outcome of mean wages of new hires at each firm, as well as event year 2 coefficients for separate regressions by firm size category.

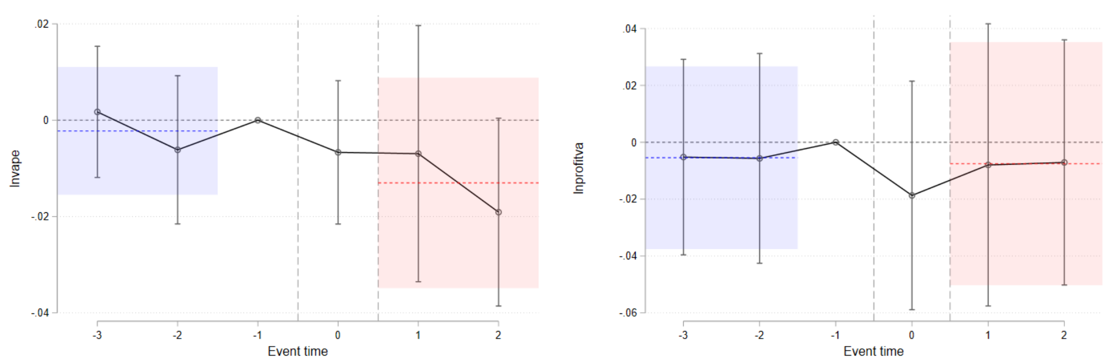
Figure B3: **Effect of contracted wage increases on other outcomes in bargaining council firms**



(a) **Separations and firm size**



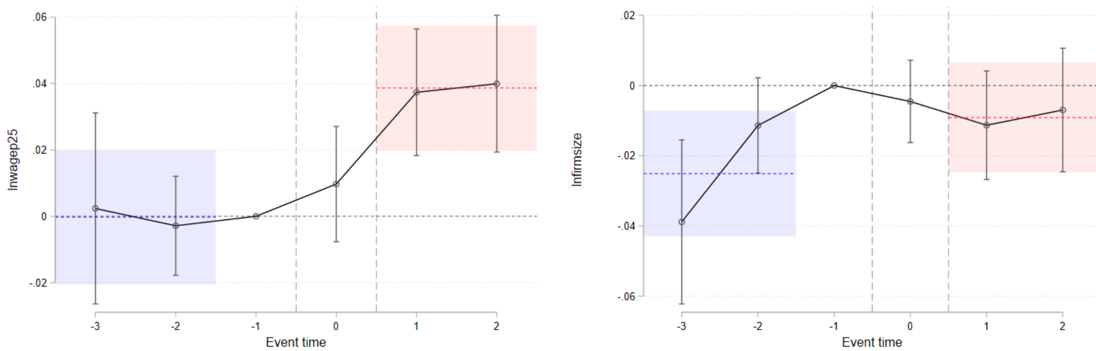
(b) **UI co-payments and within-firm p90-p50 wage gap**



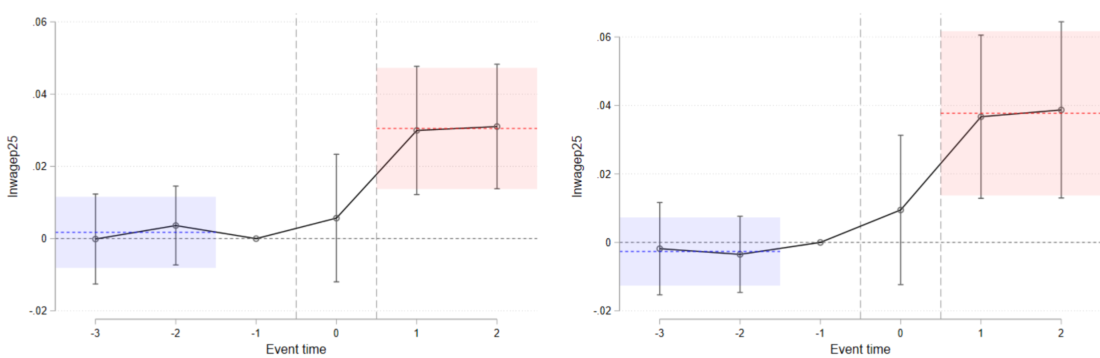
(c) **Value added per worker and profit margin**

Notes. See text or previous figure for specification details. All outcomes are logged. The top panel outcomes are the log of the proportion of a firm's workers that separate in each year, and the log of the number of workers per firm. Value added per worker is defined as firm sales minus capital and intermediate materials costs, all divided by firm size. Profit margin is defined as the firm's total profit over the firm's total value added. Unemployment Insurance (UI) co-payments are amounts paid towards a worker's UI fund to be paid out in the case of retrenchment.

Figure B4: **Effect of contracted wage increases: Rejected specifications**



(a) **Only location-time fixed effects**: Log wage and firm size



(b) **Including versus excluding high spillover firms in control**

Notes. The panels follow the main sample and specification, except for the following adjustments. The top panel includes only firm and event-year fixed effects, i.e. it does not have fixed effects for firm size by year, and growth in pre-period wage and employment by year. The bottom panel *includes* high spillover firms in the control (left), with the main specification excluding the control shown for comparison (right).

C Figures: Spillovers of Contract Changes

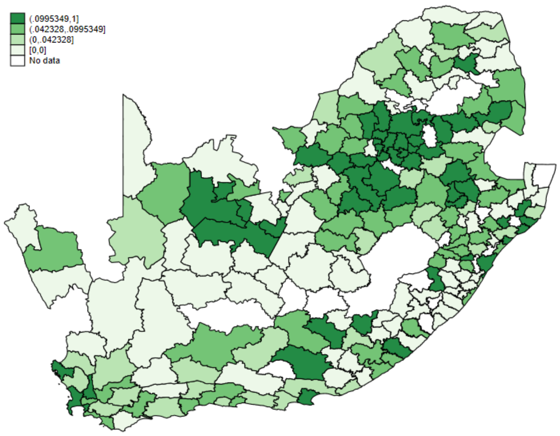
C.1 Descriptives of Spillover Firms

Table C1: **Characteristics of firms, by proportion of flows to bargaining council**

	(1)	(2)	(3)	(4)	(5)
statistic	treated	high	medium	low	unconnected
flow to BC firms	.32	.15	.07	.02	0
workers	5254124	730481	3283394	1.74e+07	6.07e+07
numfirms	19345	6473	18622	97527	345325
firmsize	272	113	176	179	176
wage	112157	104893	111151	118888	123760
wage p20	44597	39732	42202	43783	45911
wage p50	83346	78529	82257	87651	92441
wage p90p50	3.1	3.1	3.12	3.09	3.11
growth emp	.05	.04	.06	.06	.06
growth wage	.1	.1	.08	.08	.08
profitpe	216582	244141	256005	235529	250281
value added pp	392225	412029	438344	427073	452178
EEsep	.1	.09	.1	.12	.12
EEhire	.1	.09	.1	.11	.11
churn	.48	.49	.5	.53	.52
female	.24	.41	.4	.48	.48
worker FE	.05	.06	.05	.1	.13
firm FE	-.07	-.21	-.18	-.22	-.18

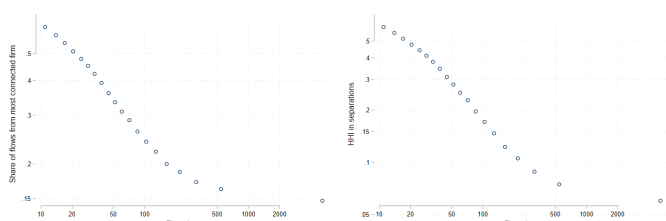
Notes. The sample is event year -1 of the main stacked event by firm by year balanced panel regression sample (restricted to firms above 10 workers as in the main specification). Treated indicates bargaining council firms, and high, medium, low and unconnected indicate decreasing degrees of connectedness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

Figure C1: **Location of MEIBC firms and spillovers**



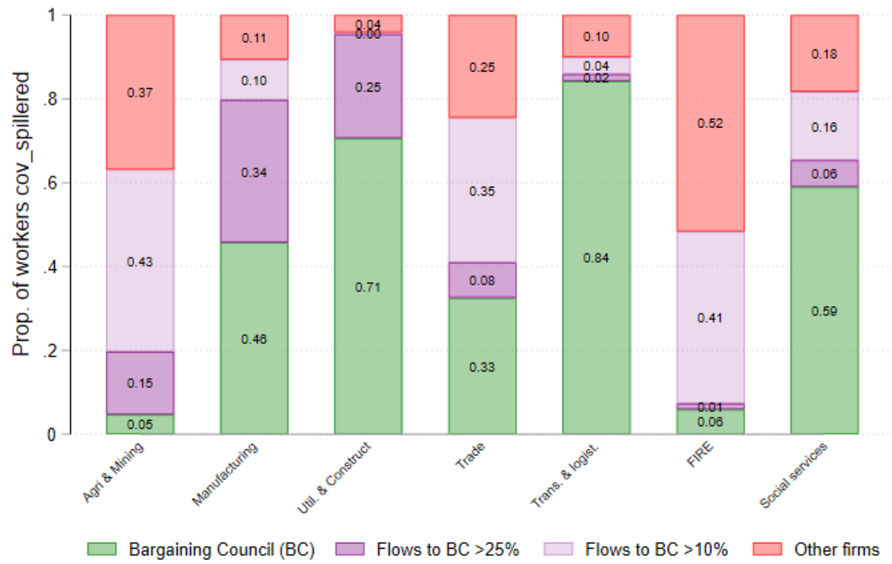
Notes. The maps show municipalities in South Africa (approximately 232 distinct areas). The average proportion of bargaining council (left) and spillover (right) is plotted, with darker shades corresponding to higher proportions. Firms are classified as high flow firms if they have more than 5% of flows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

Figure C2: **Connectivity and firm size**

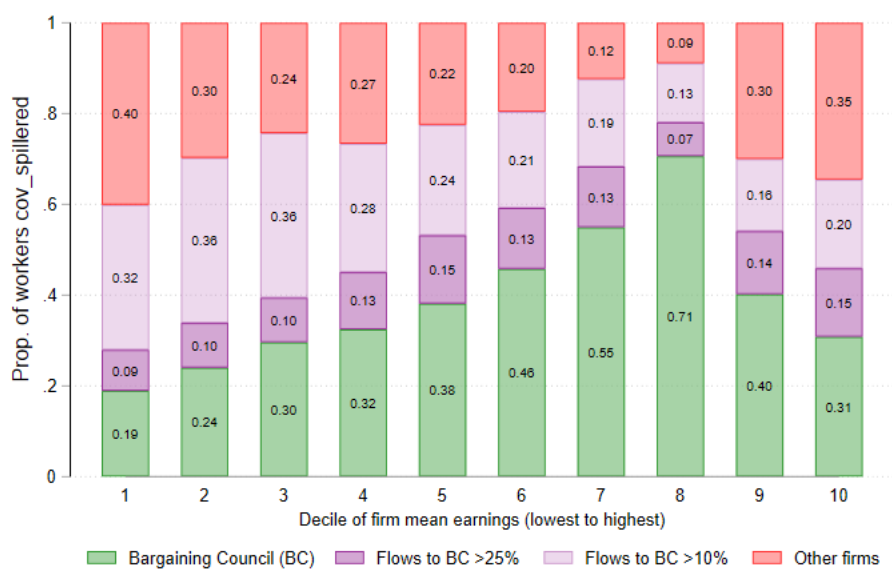


Notes. The figures illustrate how connectivity to other firms varies with firm size. The left shows the average maximum share that another firm k hires from a given firm j, by firm j's size. The right shows the concentration (as measured by the HHI) in the share firms that hire from firm j, by firm j's size. The axes are in log scale.

Figure C3: **Distribution of bargaining council, spillover and other workers**



(a) **By industry**



(b) **By firm average earnings decile**

Notes. Flows to BC refer to average firm separations to and hires from bargaining council firms. Other firms include all other formal sector firms in the sample. The sample is all formal sector firms from 2008 to 2018 using the SARS tax data.

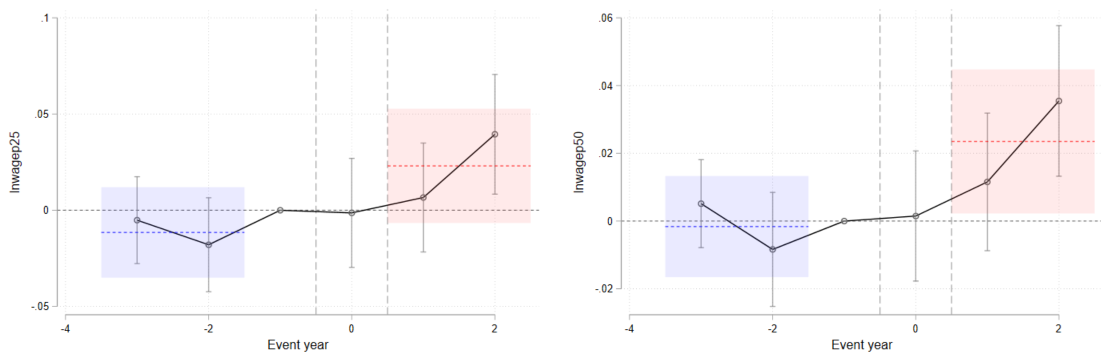
C.2 Effects of Wage Contract Changes on Spillover Firms

Table C2: **Spillover effects from bargaining council wage contracts, event year 2 coefficients from alternative specifications**

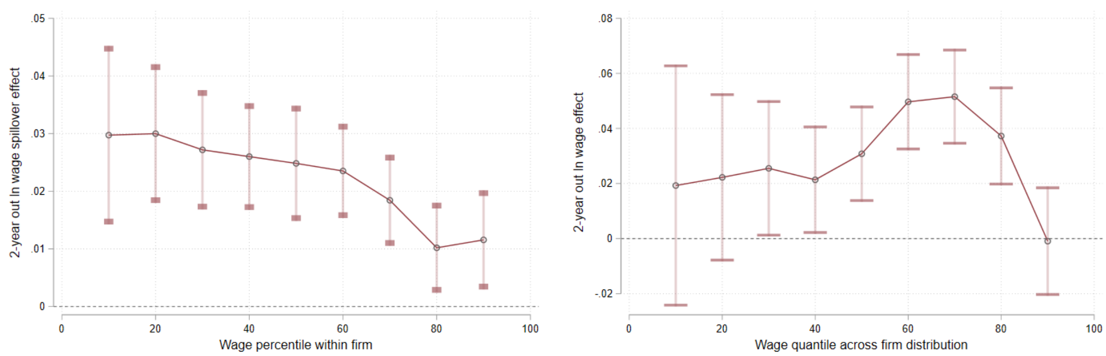
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
lnwagep25	0.039	0.024	0.044	0.014	0.043	0.026	0.011
	(0.016)	(0.007)	(0.016)	(0.007)	(0.009)	(0.006)	(0.006)
lnwagep50	0.035	0.025	0.031	0.018	0.039	0.026	0.011
	(0.011)	(0.005)	(0.013)	(0.005)	(0.006)	(0.004)	(0.004)
lnfirmsize	–0.019	–0.006	–0.013	0.001	–0.016	–0.011	–0.007
	(0.014)	(0.006)	(0.015)	(0.006)	(0.007)	(0.005)	(0.004)
lnprofitva	–0.057	–0.061	–0.050	–0.073	–0.058	–0.028	–0.016
	(0.074)	(0.022)	(0.044)	(0.024)	(0.026)	(0.016)	(0.015)
Spec			binary	indus FE	churn FE	LM flow	Arnold
IV	Y						
OLS		Y	Y	Y	Y	Y	Y

Notes. Each cell refers to the coefficient in event year 2 of a separate regression. The first column refers to the split-sample strategy for the regression, the second is OLS with the same specification, the third uses a binary indicator for high vs low flows instead of the continuous flow regressor, the fourth and fifth add firm industry and churn fixed effects respectively, the sixth calculates the flow directly on the average industry by location level (not first at the firm level), and the sixth column divides the flow measure by the industry-location size as in Arnold (2020).

Figure C4: **Spillover effects on wages by average flow in local market**



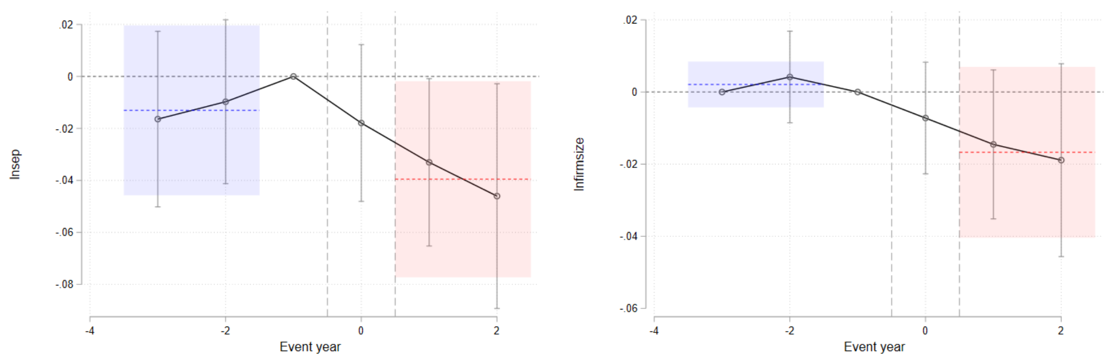
(a) **Within firm wage effect, p25 and p50**



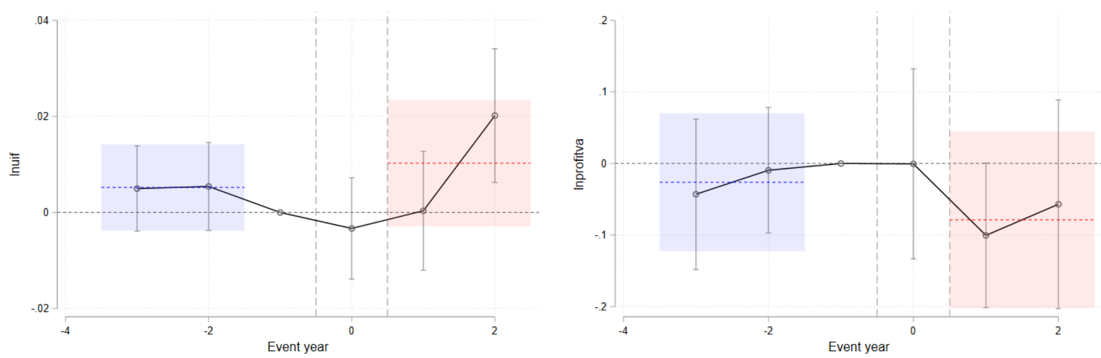
(b) **By quantile of within firm wages and across firm wages**

Notes. The regression specification largely follows the specification used in the previous section for the effect of wage contracts on bargaining council firm outcomes. Each event contains non-treated control firms, and are stacked in a panel unique on event, firm and year combinations. Firms with more than 1% of worker flows to the bargaining council are excluded from the control. The regression is run at the unweighted firm-level, is restricted to balanced firms with more than 10 workers in the pre-period, and includes fixed effects for firm, location by year, firm size by year, and growth in pre-period wage and employment by year. Standard errors are clustered at the level of bargaining council treatment by event. The main difference is that the primary regressor is the average pre-period flow between industry by location clusters of firms and the bargaining council. I use a split sample approach to reduce measurement error, where the average flow of randomized firms within local labor markets is instrumented by the complement set of firms. For event-period effects, I run a separate regression for each pairwise time period relative to event year -1. Standard errors are clustered at the level of industry by location by event. The regression is restricted to non-treated firms, and firms in the same industry as the bargaining council are excluded. The top panel outcomes are the within-firm wage percentiles, and the bottom panel shows the event year 2 coefficients for separate regressions by quantiles of within firm wages and quantiles of firms across the firm wage distribution.

Figure C5: **Spillover effects on other outcomes by average flow in local market**



(a) **Separations and firm size**



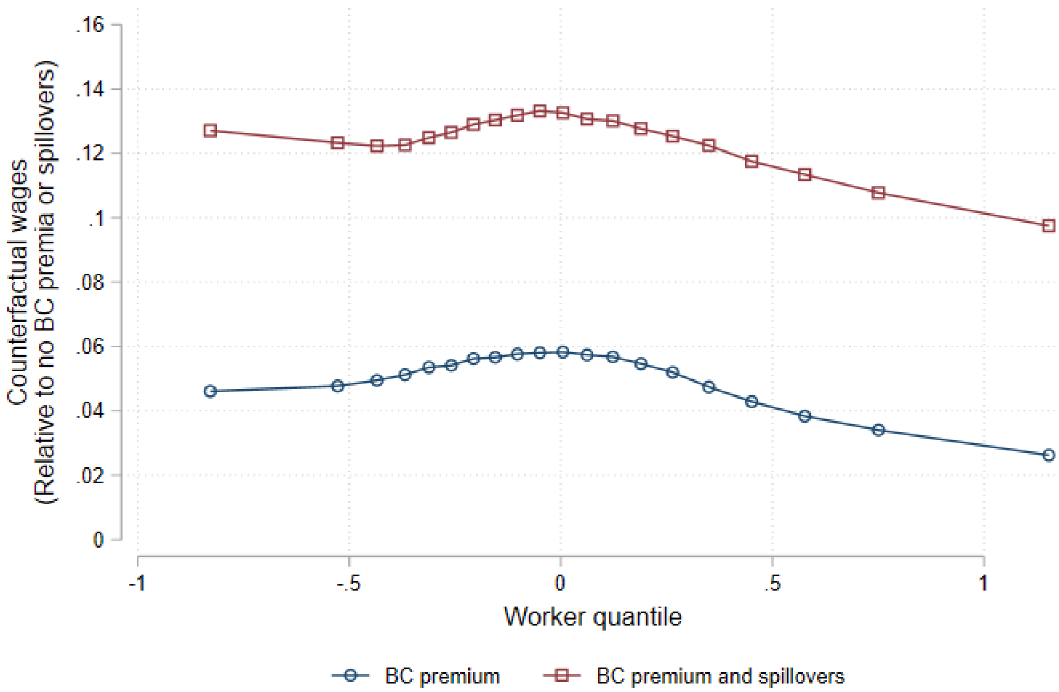
(b) **Unemployment Insurance co-payments and profit margin**

Notes. See previous figure or text for specification. All outcomes are logged. The top panel outcomes are the log of the proportion of a firm's workers that separate in each year, and the log of the number of workers per firm. Value added per worker is defined as firm sales minus capital and intermediate materials costs, all divided by firm size; and then profit margin is defined as the firm's total profit over the firm's total value added. Unemployment Insurance (UI) co-payments are amounts paid towards a worker's UI fund to be paid out in the case of retrenchment.

D Other tables and figures

D.1 Aggregate labor market effects

Figure D1: **Simulated effect on firm wage distribution**



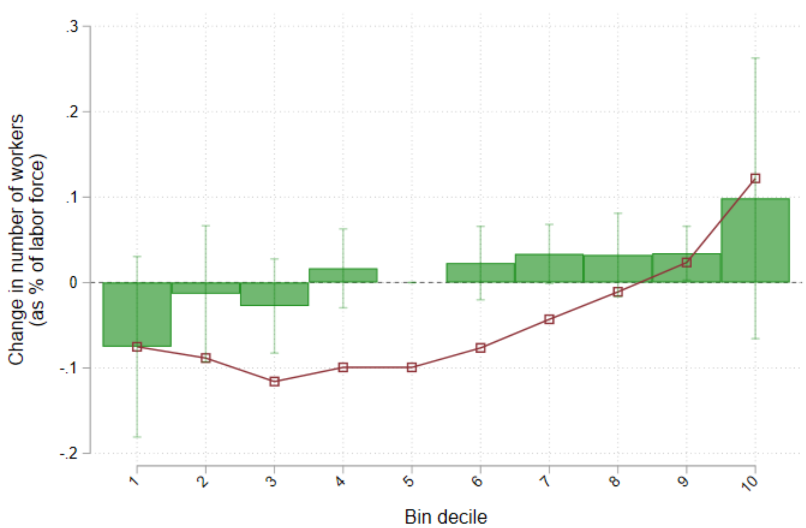
Notes. The figure simulated the effect of bargaining councils by quantile of the AKM worker fixed effect. The baseline is observed wages. The blue shows counterfactual wages which add in the relevant bargained wage increases between 2008-2018. The red bars further add in the spillovers implied by the flows to bargaining councils and the cross-wage elasticity above.

D.2 Re-allocation

Figure D2: **Summary of re-allocation effects, by pre-event wage**

Notes. The main specification and sample are used for bargaining council firms, with an indicator for each tercile of the pre-period firm wage distribution. Coefficients plotted are for the post period effect, and the own-wage elasticity (OWE) defined as the firm size coefficient divided by the wage coefficient is displayed above. Where omitted, the wage coefficient is not significant.

Figure D3: **Difference-in-Differences histograms by bin of value added per worker**



Notes. The main specification and sample are used for bargaining council firms, collapsed to pre vs post period, and with an indicator for each decile of pre-period of value added per worker (the omitted category is the 5th decile). The main outcome is the count of workers in each firm.

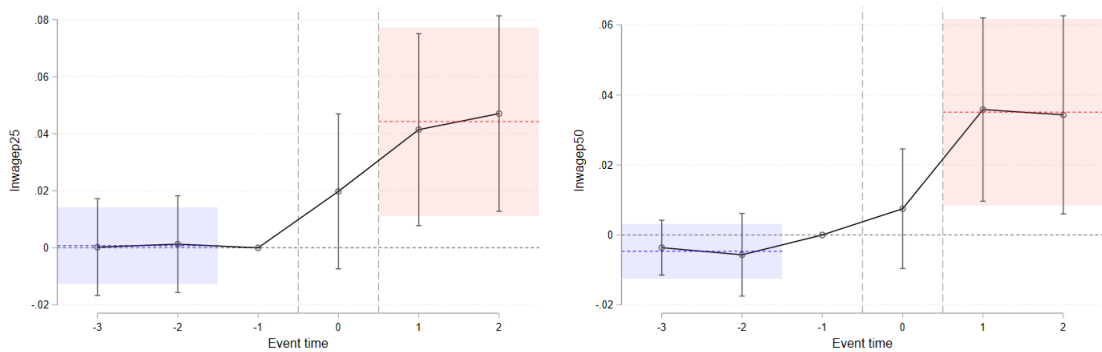
D.3 Robustness

Table D1: **Alternative specifications for bargaining council and spillover effects**

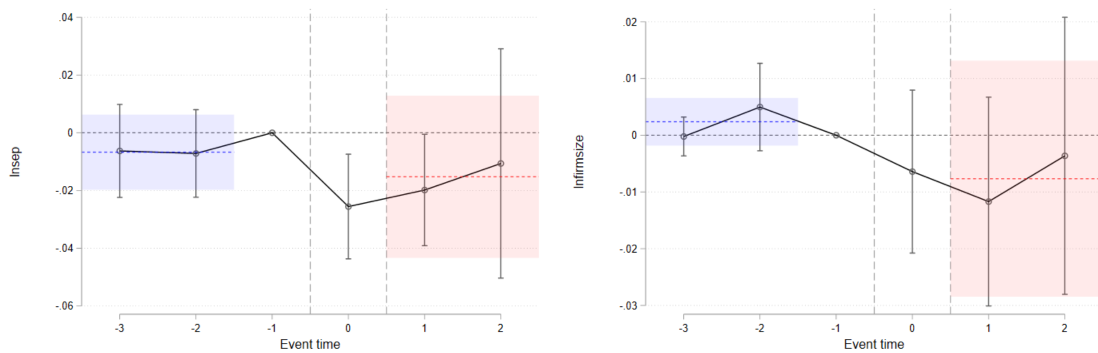
	(1)	(2)	(3)	(4)	(5)	(6)
y	main	pretrendFE	nopreBC	sizewgt	pwgt	nation1
lnwagep50	0.030	0.025	0.034	0.040	0.061	0.046
	(0.010)	(0.012)	(0.014)	(0.016)	(0.011)	(0.010)
lnfirmsize	–0.003	–0.003	–0.004	–0.050	–0.034	–0.011
	(0.009)	(0.010)	(0.012)	(0.058)	(0.010)	(0.010)
lnsep	–0.015	–0.015	–0.011	–0.021	–0.032	–0.037
	(0.013)	(0.022)	(0.020)	(0.021)	(0.014)	(0.015)
lnprofitva	–0.007	–0.015	–0.017	0.111	–0.066	–0.022
	(0.022)	(0.040)	(0.020)	(0.080)	(0.027)	(0.028)
lnwagep50	0.025	0.024	0.016	0.081	0.011	0.024
	(0.005)	(0.005)	(0.006)	(0.029)	(0.010)	(0.005)
lnfirmsize	–0.006	–0.006	–0.002	–0.045	–0.019	–0.006
	(0.006)	(0.006)	(0.007)	(0.056)	(0.011)	(0.006)
lnsep	–0.009	–0.004	–0.010	–0.012	0.001	–0.006
	(0.009)	(0.009)	(0.012)	(0.027)	(0.021)	(0.009)
lnprofitva	–0.061	–0.050	–0.078	0.168	–0.121	–0.057
CWE	0.817	0.941	0.467	2.014	.	0.528

Notes. Column 1 (main) presents the OLS results, column 2 (pretrendFE) includes firm by time linear trend fixed effects based on the pre-period, column 3 (nopreBC) excludes events with large wage increases in the period just before the event study, i.e. event year -4, column 4 (sizewgt) weights the regression by firm size, column 5 (pwgt) presents the propensity score specification, and column 6 (nation1) restricts events to bargaining council events where wages are set nation-wide. The propensity score for column 5 is based on a regression of treatment status on pre-period control variables, and the column 5 regression only controls for firm fixed effects and location by time fixed effects. The top panel presents results for bargaining council firms, and the bottom panel gives results for spillover firms. The cross-wage elasticity (CWE) divides the spillover wage coefficient by the bargaining council wage coefficient. The lnwagep50 outcome is the median within-firm wage. The CWE is missing where the wage effects are not significant.

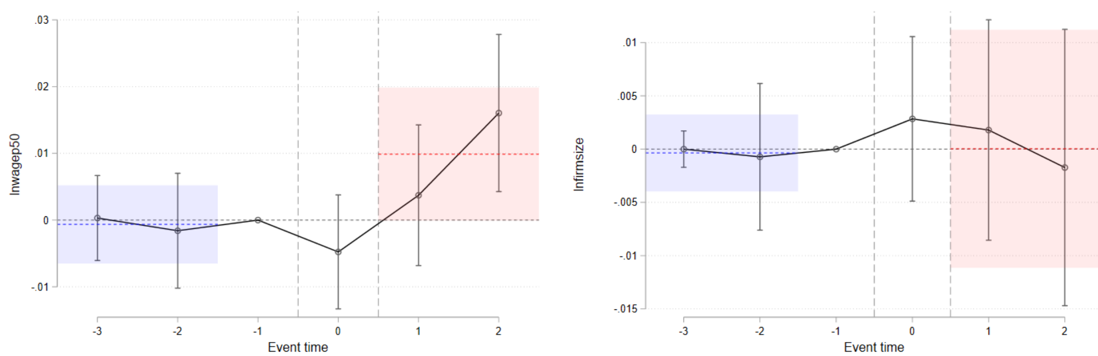
Figure D4: **No prior large contracted wage increase**



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



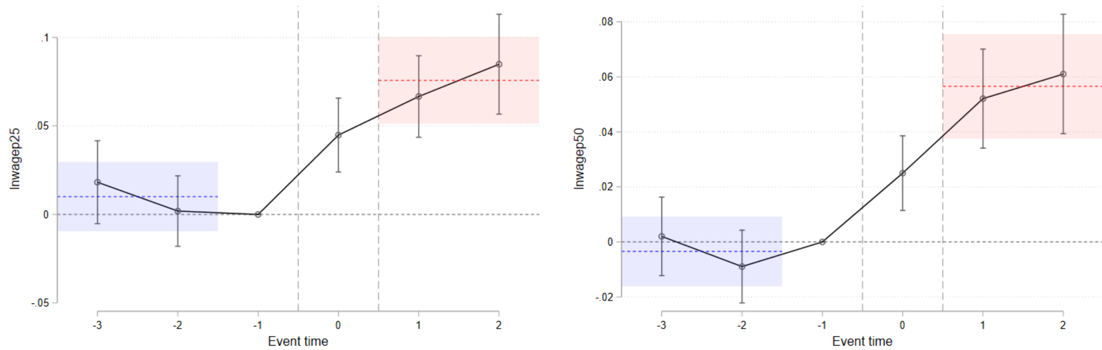
(b) **Bargaining council firm separations and size**



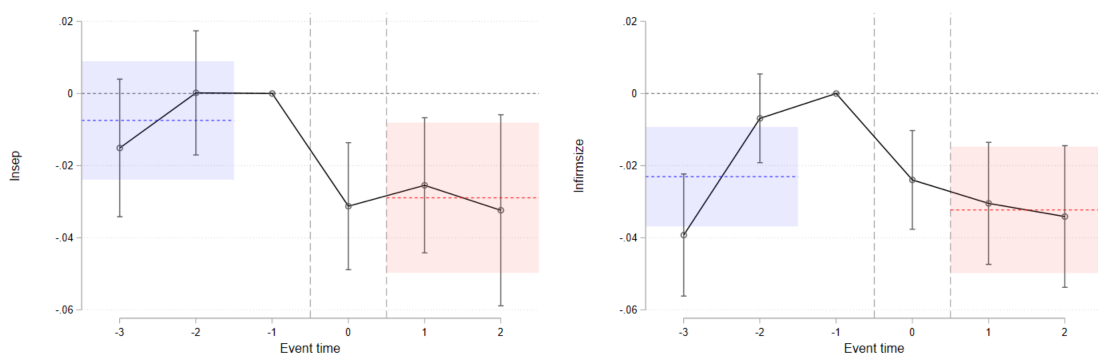
(c) **Spillover firm wages and size**

Notes. The specification follows the primary specifications, except events with large minimum wage increases just before the pre-period window are excluded. This leaves about half the number of events.

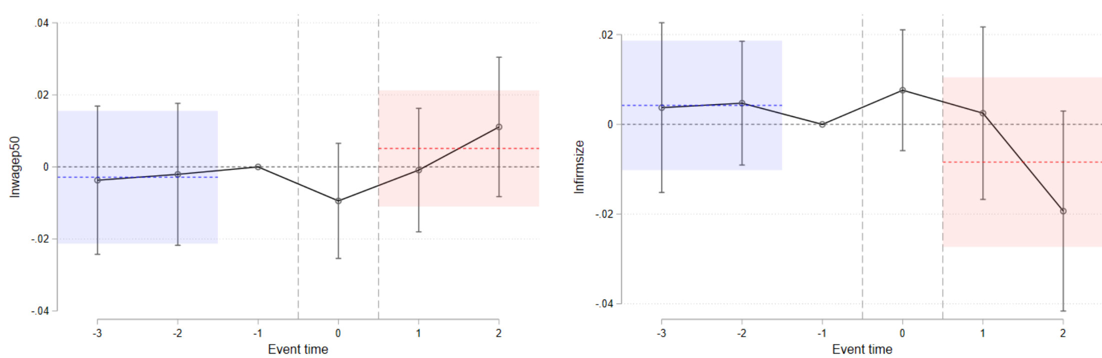
Figure D5: **Propensity score weighting**



(a) **Bargaining council firm wages**



(b) **Bargaining council firm separations and size**



(c) **Spillover firm wages and size**

Notes. Regressions are propensity score weighted, based on pre-period characteristics. The primary regression specification is followed, except with fixed effects only for firm and event-by-location-by-time and weighted by the propensity score.

D.4 Heterogeneity

Table D2: **Spillover effects from bargaining council wage contracts, heterogeneity**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
y	ffeq0	ffeq1	kaitz0	kaitz1	lseq0	lseq1	fem0	fem1
lnwagep50	0.036	0.017	0.027	0.085	0.018	0.040	0.014	0.032
	(0.014)	(0.008)	(0.011)	(0.024)	(0.013)	(0.013)	(0.012)	(0.014)
lnfirmsize	–0.025	0.029	0.008	–0.083	0.003	–0.006	0.013	0.039
	(0.011)	(0.010)	(0.010)	(0.025)	(0.012)	(0.013)	(0.015)	(0.022)
lnsep	0.009	–0.037	–0.029	–0.034	0.004	–0.029	0.003	0.008
	(0.014)	(0.018)	(0.019)	(0.019)	(0.013)	(0.018)	(0.012)	(0.013)
lnprofitva	–0.019	0.010	0.013	–0.070	–0.008	–0.006	.	.
	(0.032)	(0.034)	(0.041)	(0.028)	(0.043)	(0.024)	(.)	(.)
lnwagep50	0.045	0.010	0.015	0.064	0.047	0.018	0.022	0.026
	(0.007)	(0.007)	(0.008)	(0.009)	(0.010)	(0.006)	(0.006)	(0.006)
lnfirmsize	–0.024	0.020	–0.001	–0.032	–0.014	–0.003	0.006	0.008
	(0.008)	(0.008)	(0.009)	(0.011)	(0.012)	(0.007)	(0.007)	(0.007)
lnsep	–0.011	–0.007	–0.015	–0.013	–0.010	–0.008	0.006	0.037
	(0.012)	(0.012)	(0.013)	(0.016)	(0.012)	(0.012)	(0.012)	(0.013)
lnprofitva	–0.069	–0.050	–0.043	–0.037	–0.033	–0.071	.	.
	(0.035)	(0.029)	(0.031)	(0.044)	(0.040)	(0.026)	(.)	(.)
CWE	1.253	.	0.537	0.752	2.555	0.451	1.585	0.812

Notes. The divisions are made below and above the median of each measure. Columns 1 and 2 (ffeq) divides the sample below and above the median AKM firm fixed effect; columns 3 and 4 (kaitz, i.e. the minimum wage to local median wage ratio) are restricted to events with nationally set wages, and divide within each event firm in areas where the Kaitz ratio is low (kaitz0, i.e. relatively high median wages) and where the Kaitz ratio is high (kaitz1, i.e. relatively low median wages); columns 4 and 5 divide events into bargaining councils with low firm labor supply elasticities (more monopsonistic market) and bargaining councils with high firm labor supply elasticities (more competitive market); columns 7 and 8 (fem) restrict the firm-level outcomes to men and women workers at each firm respectively. Profit margin outcome is missing for the sex regression because this is a firm-level outcome and cannot be disaggregated by sex. The top panel presents results for bargaining council firms, and the bottom panel gives results for spillover firms. The cross-wage elasticity (CWE) divides the spillover wage coefficient by the bargaining council wage coefficient. The CWE is missing where the wage effects are not significant.

E Derivations of theoretical framework

E.1 Logit static model

The logit model is widely used to model monopsony (Card, Cardoso, et al., 2018; Dustmann et al., 2021). Utility of a worker may be expressed as $V(w_j) = \beta ln(w_j) + \nu_{ij}$, where β parameterized the latent monopsony power (i.e. the responsiveness of worker utility to wages), and ν_{ij} follows a Gumbel distribution indicating idiosyncratic preferences for the firm. The source of monopsony power is a firm's knowledge of the distribution of ν_{ij}, as cuts to the wage have little impact on the utility of workers with high idiosyncratic preferences.

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

$$p_j = \frac{w_j^{\beta}}{\sum_{k=1}^{J} w_k^{\beta}} \tag{6}$$

Taking logs, we can compute the firm labor supply elasticity ε_j.

$$lnp_j = \beta ln(w_j) - ln(\sum_{k}^{J} w_k^{\beta})$$

$$\varepsilon_{jj} = \frac{\partial lnp_j}{\partial lnw_j} = \beta(1 - \frac{w_j^{\beta}}{\sum w_k^{\beta}}) = \beta(1 - p_j) \tag{7}$$

$$\varepsilon_{jk} = \frac{\partial lnp_j}{\partial lnw_k} = -\beta(\frac{w_k^{\beta}}{\sum w_k^{\beta}}) = -\beta p_k \tag{8}$$

Note the potential for spillover effects: $\varepsilon_{jk} < 0$, i.e. an increase in the wage of a connected firm k (holding w_j constant) decreases the employment in firm j.

The expressions for ε_{jj} and ε_{jk} match those in standard derivations of the logit, such as in (Train, 2009). It demonstrates that a firm k which raises its own wage will see an increase in own employment. Another firm j, faced with this wage increase from firm k, can trade-off raising its own wages or losing its workers. Given a change in firm k's wage Δlnw_k, firm j's share will change by $\Delta lnp_j = \varepsilon_{jk}\Delta lnw_k = -\beta p_k \Delta lnw_k$ while firm j's wage w_j remains the same. Alternatively, firm j may seek to maintain its previous employment share, through raising its wage by an equivalent amount, $\Delta lnw_j = \Delta lnw_k$.[27]

The wage response chosen by j is determined by the firm's wage setting function. Abstracting from product market competition and normalizing the price to 1, the profit optimization equation and optimal wage are:

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

$$lnw_j = ln(\frac{\varepsilon_{jj}}{1+\varepsilon_{jj}}) + lnA_j - \eta ln(p_j N) \tag{9}$$

Where $\varepsilon_{jj} = \beta(1-p_j)$ is given above. The final term includes p_j which is a function of *all* wages of firms in the local labor market.[28] This wage-setting equation nests the constant returns production function ($\eta = 0$) yields the standard markdown formula $W_j = A_j \frac{\varepsilon_{jj}}{1+\varepsilon_{jj}}$. We can derive the wage cross wage elasticity, denoted as ε^w_{jk}, by using equations 9 and 8:

$$\varepsilon^w_{jk} = \frac{dln(w_j)}{dln(w_k)} = \frac{\beta p_k \beta p_j}{\beta(1-p_j)(1+\beta(1-p_j))} + \eta\beta p_k - \eta\beta\varepsilon^w_{jk} + \eta\beta\sum_{l\neq k} p_l \varepsilon^w_{jk}$$

$$\varepsilon^w_{jk} = \frac{\beta p_k}{1+\eta\beta p_k}(\frac{\beta p_j}{\varepsilon_{jj}(1+\varepsilon_{jj})} + \eta) \tag{10}$$

Where the final expression for ε^w_{jk} makes the non-consequential simplifying assumption that $\varepsilon^w_{jk} = \varepsilon^w_{lk}$.[29] The wage-cross-wage elasticity ε^w_{jk} is positive, and sensitive to increases in other's firm share p_k, the extent of decreasing returns to scale η, and higher competitiveness β (it

[27] See this from $\Delta lnp_j = \beta ln(w_j \cdot (1+\Delta w_j)) - ln(\sum_k^J (w_k \cdot (1+\Delta w_j))^\beta) - lnp_j = \beta ln(w_j) + \beta ln((1+\Delta w_j) - ln(\sum_k^J w_k^\beta) - \beta ln((1+\Delta w_j) - lnp_j = 0$.

[28] Equivalently, $lnw_j = \frac{1}{1+\beta\eta}(ln(\frac{\varepsilon_{jj}}{1+\varepsilon_{jj}}) + lnA_j - \eta ln(N) + \eta ln(\sum w_l^\beta))$, where the w_l are still a function of other wages in the local labor market.

[29] This is equivalent to assuming the employment shares of the other firms (excluding firm k) are the same in the ε^w_{jk} expression. This affects the final expression only through deviations in the $\eta\beta p_k$ term of the denominator in the first fraction.

has low sensitivity to changes in p_j). The intuition for these positive cross wage elasticities is illustrated by the two terms in the bracket, representing two channels. The rise in firm k's wage primarily increases firm k's own employment share, which decreases firm j's employment share. The first term shows its effect through the firm labor supply elasticity, which increases when p_j is smaller and therefore raises the wage slightly. The second term shows its effect through the decreasing returns to scale, since lower employment implies higher marginal returns which for a constant markdown corresponds to higher wages.[30][31]

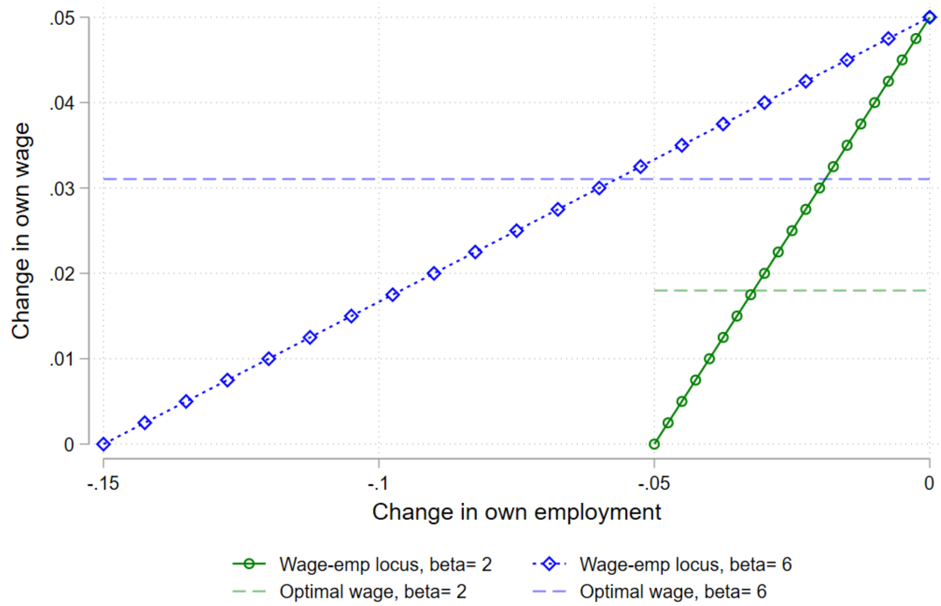
Figure E1 illustrates the positively sloped locus which gives the trade-off that connected firms face between losing employment and raising its wage. The optimal wage-setting response is plotted from equation 10 as a negatively sloped dashed line, and intersects with the locus to pin down the optimal wage. The blue lines use the parameter values $\beta = 6$, $p_k = 0.5$, $p_j = 0.1$, and $\eta = 0.5$. Then, following firm k raising its wage by 5%, firm j can lose up to 15% of its own employment (if it does not respond by raising its wage at all), or retain its employment share and instead match the wage increase at 5%. The cross wage elasticity pinned down by the optimal wage-setting equation is $\varepsilon^w_{jk} = 0.62$, suggesting that firm j increases its wage by about 3% and loses just about 6% employment. I contrast this with firms facing a low labor supply elasticity ($\beta = 2$, plotted in green). The slope becomes steeper, and the optimal cross wage elasticity becomes $\varepsilon^w_{jk} = 0.36$, implying that firm j raises its wage by 2% and loses 3% of its employment. As usual, the calibration magnitudes in this simple model should be interpreted only illustratively.

ε^w_{jk} is very similar even if firm j's share is negligible, e.g. $p_j = 0.001$ implies $\varepsilon^w_{jk} = 0.6$ (compared to $\varepsilon^w_{jk} = 0.62$ above). If the primary firm share p_k decreases, the cross wage elasticity also decreases but remains substantial even at low shares. For example, $p_k = 0.1$ with $p_j = 0.001$ replicates the cross wage elasticity of $\varepsilon^w_{jk} = 0.23$ from large employer wage increases found in

[30] This highlights that *some* decreasing returns to scale is necessary for a sizeable cross wage elasticity. In the simple case of $\eta = 0$, the implied cross wag elasticity is much smaller.

[31] The possibility of second round wage responses is not concerning, since the wage increase we consider here for firm k is imposed and takes firm k off its optimal wage setting curve. For firm k's optimal wage to be higher than its new minimum wage once feedbacks are accounted for, the cross wage elasticity needs to be greater than 1. In this case, equating the response functions of the firms as in Bertrand competition would imply that the initial cross wage elasticities are multiplied by small factor.

Figure E1: **Wage and employment responses to another firm's wage in logit model**



Notes: In this illustration, the other firm increases its wage by 5%. The other firm has share of 0.5, and this firm's own share is 0.1. The optimal wage line follows from the firm's profit-maximizing function ($\eta = 1$). The slope is $1/(\beta p_k)$ and the y-intercept is the other firm's wage increase (here 5%).

Derenoncourt et al. (2021). If both firm shares are small, however, the cross wage elasticity is close to 0 as one expects (holding β constant). Finally, under perfect competition with $beta \to \infty$, then $\varepsilon^w_{jk} \to 1$ even if all firms have negligible share. In all cases, the total profit of the firm decreases.

In the figure, the intersection point depicting the optimal response is dependent on the particular wage-setting curve chosen. A range of optimal wage-setting curves are possible, which may imply other points on this locus. For example, the functional form of the profit function could be different. Dustmann et al. (2021) in their appendix show a logit model to demonstrate re-allocation, and use a production function which is increasing in $ln(N)$, $A_j ln(p_j(w_j)N)$ instead of $\frac{1}{1-\eta}A_j(p_j(w_j)N)^{1-\eta}$ above.[32] For the calibration values above of $\beta = 6$ and $p_k = 0.5$,

[32] 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 $lnw_j = \frac{1}{1+\beta}(ln(\frac{\varepsilon_{jj}}{1+\varepsilon_{jj}}) + lnA_j - ln(N) + ln(\sum w_l^\beta))$. The cross wage elasticity expression is $\varepsilon^w_{jk} = \frac{\beta p_k}{1+\beta p_k}(\frac{\beta p_j}{\varepsilon_{jj}(1+\varepsilon_{jj})} + 1)$. Both of these expressions are identical to above with $\eta = 1$. Note the parameterization value used above is $\eta = .5$. While the production function $\frac{1}{1-\eta}A_j(p_j(w_j)N)^{1-\eta}$ with $\eta = 1$ is strictly not defined, $\eta \to 1$ implies for both functions that production increases with employment like $1/N$. My main functional specification, $\eta = 0.5$, allows for a greater sensitivity $(1/\sqrt{N})$.

this implies a much larger magnitude of $\varepsilon^w_{jk} = 0.94$. Alternatively, the profit function could include other considerations. If there are fair or reservation wage considerations, the production function may follow instead $\frac{1}{1-\eta}A_j(\frac{w_j}{w_*}p(w_j)N)^{1-\eta}$, a variant of Akerlof and Yellen (1990) where w_* is a reference wage.[33] Again for the calibration values above, this implies a magnitude of $\varepsilon^w_{jk} = 0.75$. Many other wage-setting equation variants are possible, and the upshot is that the locus provides a range of feasible cross-wage elasticities which under plausible parametrizations are large.

This baseline model of logit with strategic interactions can be extended in many ways. If the local market is responsive to the average wage, then employment need not decline by as much in the spillover firms. We can express this as $n_j = p_j\bar{w}^\theta$, where the average wage is the firm-share weighted geometric mean $\bar{w} = \Pi^J_{k=1}w^{p_k}$. Then $ln(n_j) = \theta ln(\bar{w}) + \beta ln(w_j) - ln(\sum^J_k w^\beta_k)$, $\varepsilon_{jj} = \theta p_j + \beta(1 - p_j)$, and $\varepsilon_{jk} = \theta p_k - \beta p_k$. The cross-wage elasticity, using the wage setting equation 9 also carries the extra θ term, $\varepsilon^w_{jk} = \frac{1}{1+\eta\beta p_k+\eta\theta(1-p_k)}(\frac{(\beta-\theta)^2 p_k p_j}{\varepsilon_{jj}(1+\varepsilon_{jj})} + (\beta-\theta)\eta p_k)$. The cross wage elasticity is slightly lower, though with a smaller employment decline. The intuition is that the aggregate market supply increases in response to the higher average wages, which in turn implies a lower absolute decrease in firm j's employment. Since firm j's optimal wage is primarily responding to changes in its own employment, its wage response is correspondingly smaller.

E.2 Logit dynamic model

The standard logit can be adapted to a dynamic setting allowing for worker flows, following a simplified version of the models presented by Caldwell, Dube, and Naidu (forthcoming) and Langella and Manning (2021). Every period workers take a fresh draw of idiosyncratic preferences ϵ_{ij}.[34] With probability $1 - \lambda$ the worker receives no offers, and with probability λ the

[33] The wage equation becomes $lnw_j = \frac{1}{1+\beta}(\frac{1}{\eta}lnA_j - \frac{1-\eta}{\eta}ln(w_*) - ln(N) + ln(\sum w^\beta_l))$. The cross wage elasticity expression is $\varepsilon^w_{jk} = \frac{\beta p_k}{1+\beta p_k}$. Interestingly, this expression is identical to above with no own-elasticity terms and $\eta = 1$.

[34] This is an extreme assumption but may be reframed in several ways. Instead of λ as offers, this could be the proportion whose preferences are redrawn. Or the ϵ_{ij} can follow a random walk, with small shocks every period, which would result in small proportion of switching.

worker receives offers from a choice set S of connected firms (always including itself).

Referring to equation 6[35] where firms $k = 1, ..., J$ represent the choices available to the worker, let $p_{j,S}$ represent the probably of employment in firm j given then choice between firms in "consideration set" or *connected set* S.[36] The connected set S of firm j need not be the same as the connected set S' of the firms from which firm j recruits.

$$q_{j,S}(w_j) = \lambda(1 - p_{j,S})$$

$$R_{j,S}(w_j) = \sum_{l \neq j}^{S} \lambda p_{j,S'} \cdot p_{l,S'} N_{l,S'}$$

Where $N_{l,S'}$ indicates the total number of workers in firm l's connected set S'. These expressions are related to the baseline logit through $n_{j,S} = R_{j,S}(w_j)/q_{j,S}(w_j)$. Taking logs, and the derivative of firm j with respect to a wage change in firm k:

$$\begin{aligned}
\frac{\partial ln(n_{j,S})}{\partial ln w_k} &= \frac{\partial ln(R_{j,S})}{\partial ln w_k} - \frac{\partial ln(q_{j,S})}{\partial ln w_k} \\
&= \left(\frac{\lambda \varepsilon_{jk} p_j \sum_{l \neq j}^{S}(p_l N_{l,S'})}{R_{j,S}} - \left(-\frac{\varepsilon_{jk} \lambda p_j}{q_{j,S}}\right)\right) \\
&= -\beta \cdot \left(\underbrace{\frac{p_k \cdot \lambda p_j \sum_{l \neq j}^{S}(p_l N_{l,S'})}{R_{j,S}}}_{\text{Prop. j's hires from k}} + \underbrace{\frac{p_k \cdot \lambda p_j}{q_{j,S}}}_{\text{Prop. j's quits to k}}\right) \\
&= -\beta \cdot f_{jk}
\end{aligned} \tag{11}$$

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

[36] The assumption that the worker's choice follows equation 6, even under Logit preferences, ignores the dynamic aspect of the worker's choice. For example, if firm j has a better consideration set that firm k (where "better" implies access to higher wage firms), then the worker may choose firm j even if $w_j + \epsilon_{ij} < w_k + \epsilon_{ik}$. We can justify this assumption by requiring the consideration set to be exogenous to the firm. That is, the worker values firm j as $V_j = \frac{1}{\rho + \lambda_{j,S}}[\beta ln(w_j) + \epsilon_{ij} + \lambda_{j,S} A_j]$, where $A_j = ln(\sum_{k=1}^{S} exp(V_k))$ is the maximum value of Gumbel-distributed firm choices evaluated across consideration set S. If firm consideration sets are all identical, either because they share the same labor market or because the worker does not have knowledge of it and so treats all consideration sets as identical in expectation, then this term cancels out and we return to the expression 6. If consideration sets differ by firm but are exogeneous, then firm values are augmented by this term, $p_j = \frac{A_j w_j^{\beta}}{\sum_{k=1}^{J} A_k w_k^{\beta}}$ and the remainder of the analysis is the same.

Where the elasticity ε_{jk} is from equation 8 above, and f_{jk} represents the firm j's average hires from and quits (flows) to firm k.[37] Firm j's employment response to firm k's wage increase is therefore proportional to f_{jk}, the flows between firm j and firm k. Given that any wage response from firm j is determined by the impact on its own employment (using the static model of section E above), the wage response too is a function of f_{jk}.[38]

How large is f_{jk}? The consideration set this depends on can be small or concentrated, allowing for large values. For example, if S only includes the firms from which a worker receives an offer, the worker could face a choice similar to a 2 or 3 firm market in the static logit above. This relates to the values assigned to firms in the calibration of plausible cross wage elasticities above, justifying firms with substantial market shares.[39]

While λ does not appear explicitly in the final expression of equation 8, it is potentially an important part of the firm's response.[40] It determines the speed of the adjustment to the new firm steady state: the replacement rate r of the firm's workers as governed by the adjusted flows is given by $r = 1 - (1-\lambda)^t$. For example, if $\lambda = 1/3$, $r = 1/3$ of the firm's workers would be replaced in one period, and $r = 2/3$ would only be replaced in $t = 2.7$ periods. Measured over a discrete period of time then, firm j's responses are also initially proportional to λ. Moreover, λ may vary by firm. In a firm with a larger connected set S, we may expect the probability of receiving an offer λ to be larger. Let $\lambda_{j,S} = \lambda(S)$, where $\lambda'(S) > 0$ leaves the offer function

[37] Equation 11 assumes in the derivative with respect to recruits that other firm l size is constant. Taking changes in firm l size into consideration (as would be the case in the new steady state equilibrium) only changes the expression slightly: replace $\sum_{l \neq j}^{S}(p_l N_{l,S'})$, with $(2\sum_{l \neq j}^{S}(p_l N_{l,S'})) - N_{k,S'}$. In particular, if the connected sets S are the same across all firms, noting k is a part of S too, this expression is identical to the original in equation 11. The intuition is that any decreases in other firm's sizes are just transitions towards increasing firm k's size, which does not affect firm j's net recruitment.

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

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

[40] If we allow exogenous separations, and re-define $q_{j,S}(w_j)$=λ(1-$p_{j,S}$)+$\delta$and $R_{j,S}(w_j) = \sum_{l \neq j}^{S} \lambda p_{j,S'} \cdot p_{l,S'} N_{l,S'} + \lambda_u N_u p_j$, where $N_u = \delta/(\delta + \lambda_u)$, then $\partial ln(n_{j,S})/\partial ln(w_k) = -\beta(f_{jk} + p_k p_j \lambda_u \delta/(\delta + \lambda_u))$. That is, λ does appear, but only for the offer rate to the unemployed and as an additive term.

general besides that it increases with the number of firms in firm j's set S. Then this provides a direct relationship between the firm's responses (including its labor supply elasticity) and the firm's connectivity.

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

E.3 Application to collective bargaining and minimum wages

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

I model the minimum wage following Dickens, Machin, and Manning (1999), with the wage setting equation 9 above. Firms fall into one of three regimes. Firstly, firms with first-period wages above the new minimum wage ($w_{j1} > w_{min}$) are directly unaffected, though will be affected through feedbacks similarly to connected spillover firms. Secondly, firms with wages below the new minimum ($w_{j1} < w_{min}$), but high enough productivity ($ln(A_j) - \eta n_{j1} > w_{min}$), will raise their second-period wage to the new minimum ($\Delta w_j = w_{min} - w_{j1}$), and their employment will increase. Thirdly, firms with wages below the new minimum ($w_{j1} < w_{min}$), but productivity not high enough ($ln(A_j) - \eta n_{j1} < w_{min}$), will meet their contactual obligation by raising their second-period wage to the new minimum ($\Delta w_j = w_{min} - w_{j1}$), but their employment will be demand-constrained and decrease ($\Delta n_j = n_{j1} - 1/\eta(ln(A_j) - w_{min})$). The lowest productivity firms (with the lowest wages) will exit.

The net effect depends on the distribution of contractually bound firm productivities and wages. A sizeable labor market effect of a minimum wage depends first of all on a substantial share of contractually bound collective bargaining firms having wages initially below the new minimum. To the extent that a large proportion of firms within this subset have low productivities, falling into the third regime above, the net employment change will be lower. Endogenous minimum wage bargaining may ensure this category is negligible (given the interests of both firms and workers), though there are counter-tendencies (an example outlined in Moll (1996)). Note that even with no net employment change, there can still be substantial firm size and worker re-allocation effects for bargaining council workers. The scope for second regime firms, whose employment increases following adherence to a higher minimum wage, is larger in more monopsonistic firms where the markdown of the wage from productivity is higher.

The connected spillover firms are not contractually bound by these minimum wages, and are modelled as in sections E and E.2 above. Figure E1 works well as an illustration of my setup. Bargaining council firms plausibly account for 50% of a firm's labor market, implying a cross wage elasticity of 0.6 for even small connected firms. Profits decrease for these spillover firms.

F Data construction

The SARS administrative tax dataset provides a near-universe of formal sector individual labor market wage outcomes and firm balance sheet information. It is periodically updated, with the latest around of available years extending from financial year 2008 to 2018.[41] It is easily one of the richest sources of economic data for South Africa's formal sector economy. However, a key limitation is that the data were collected for the purposes of taxation only and by design misses out on key covariates essential to the analysis of many important economic questions. For example, there are no data on worker occupation, race or education; on the outcomes of non-workers pertaining to unemployment; or on whether a policy applies to a given worker or firm (e.g. individual grants or investment subsidies).

The purpose of this data appendix is to outline how I use bargaining council (BC) data which I compile and which are external to the tax data, and match them into the tax data. More generally for policy questions, the limits in the tax data may be partially mitigated by matching in information available from other sources, such as Statistics South Africa's Quarterly labor Force Survey (QLFS) or government gazettes. This makes it possible to attain second-best estimates on a much wider range of key policy questions.

The matched firm-level dataset is available to any researcher at the National Treasury Secure Data Facility in Pretoria. Please contact me with any questions regarding where to find the data or associated code. The most important caveat is that the matching is imperfect. It relies particularly on the quality of the location and industry data. There has been some work on the industry codes and locations of firms, but some imprecision remains.

F.1 Compiling bargaining council agreements

The government gazette publishes bargaining council agreements, which may be found online at https://www.greengazette.co.za/. Bargaining council contracts are generally set in 3-year terms, and pegged to inflation (plus a negotiated amount). By going through at least

[41] Financial year 2008 corresponds to calendar time March 2007 to February 2008.

one contract in detail per bargaining council, I record the industry and location of each. I supplement this with the compilation of wages provided to me by the labor Research Service, and check each wage against the actual 3-year contracts 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.[42] For each bargaining contract, I select the SIC 5 classification code that best matches the wording in the contract. 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 contract, 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 contract may have many clusters of locations, where each cluster may have a different set of bargained wages or conditions. There are 145 clusters in total. As a consequence of the clusters, the majority of the clusters are defined by district council locations even though nearly half of the bargaining councils are nationally based. In total, I consolidate nearly 1600 records of wages from different bargaining council clusters by industry and location.

Table F1 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 contract, which usually covers occupations such as "laborer" (41% of listed wages) or "general worker" (30% of listed wages). I cross-check these categories with the QLFS data, though this is a limited check given that the QLFS data are not representative at the district council level, they do not have as much detail on industry, and their correspondence with occupational categories in the agreements is not straight-forward. The minimum wages are highest in the New Tyre Manufacturing, Transnet and Public Service Coordinating bargaining councils, and lowest in hairdressing and

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

furniture manufacturing for Kwa-Zulu Natal.

Table F1: **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 SIC5 classification system. Location indicate the geographic level of location assignment, i.e. national, provincial (9 in South Africa) or district council (52 in South Africa). Clusters refer to location specific units within each bargaining council. Source: Gazetted bargaining council documents published by the South African government.

F.2 Matching bargaining council agreements to tax data

Table F2 briefly summarizes the tax data panel between 2008 and 2018. I construct the tax panel by combining all available worker-level variables for tax years, restricting to workers of ages 20 to 60 years old, restricting to one job per worker, and merging in firm level variables. The wage variable is just the wage code 3601. While the focus of this data project is not the panel, it is worth mentioning that the years 2008 and 2009 are likely incomplete and thus not fully comparable to other years. For example, the separation rates for 2008 and 2009 are much higher, which is unsurprising since incomplete records would result in workers dropping in and out of the panel as if they were separating from jobs. From the firm balance sheet side, the profit and turnover are slightly higher in 2009, but are otherwise stable. The firm exit rate (firm is not observed in the next year) and outsource probability (defined when a third or more than 500 workers switch to another firm at the same time) are similarly higher for the incomplete years.

Table F2: **Summary stats on tax panel**

	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.

There are a few caveats to the matching process. Firstly, the industry variable is crucial and I follow the best practice as laid out in J. 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.

Thirdly, the bargaining council industry descriptions vary in how narrow the industry and location scopes are. For example, in terms of industry the food retail bargaining councils cover a narrow set of workers and can be assigned a four-digit industry code (6211). On the other hand, the metal and engineering bargaining council covers a range of manufacturing activities relating to metals production and is most accurately described at the broad two-digit level (35). The examples in terms of location vary from national bargaining councils (chemical) to district council based bargaining councils (laundry). Note that the division between clusters and bargaining councils reflects organizational rather than classification differences. The clothing manufacturing industry for example is one bargaining council, but assigns different wages for 33 industry-location clusters. The building industry on the other hand seems to register entirely separate bargaining councils for locations, such as Bloemfontein compared to the Cape of Good Hope.

Out of a total of 1595 total potential records (11 years by 145 bargaining council clusters), I have matched 90% with wage records. In terms of number of workers over the entire panel, about 30% are presumed to be covered by bargaining councils and 70% uncovered. This is in line with the estimate in D. Budlender and Sadeck (2007) using survey data.

F.3 Matching bargaining council agreements to tax data

What is the quality of the final matched data? One indication is the strong "first stage" estimated in section 4. Wages jump sharply as expected when there are large increases in contracted bargaining council minimum wages. While this does not preclude error, i.e. both firms that are left out and firms that are mistakenly classified as part of a bargaining council, it does give assurance that the indicator is meaningful enough for information to pass through from the contracts to the observed wages of workers. Section 3 describes the matched data in further detail, comparing bargaining council firms with other formal sector firms in the economy.

In table F3, I show characteristics by bargaining council focusing on those with an event or large wage increase at some point, as highlighted in my main analysis above. There is considerable variation across the different bargaining councils for each characteristic. The largest non-government bargaining council is the Metals and Engineering Industry (MEIBC), both in terms of workers (over 800,000) and firms (16,000). There are several other large bargaining councils with hundreds of thousands of workers, such as Civil Engineering, Road Freight and Logistics, Motor Industry, and Chemical. There are also several small bargaining councils, which are more locally defined and in narrower industries, such as Laundry in KwaZulu-Natal, Meat Trade in Gauteng or Hairdressing.

The most profitable on a per person basis are the Meat Trade, Road Freight and Logistics, and Road Passenger bargaining councils. However, all of the bargaining councils have a high average per person profit that is far above the average wage. The bargaining council minimum wages go as low as around R30,000 per year or R2,500 per month (2016 inflation-adjusted). Incidentally, this is far below the 2019 national minimum wage of R3,500. There are also 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 labor market parameters, the firm level rent-sharing is generally *lower* than in other firms, perhaps because wages are set more sectorally, through some industries have high rent sharing elasticities (such the Tyre BC). The firm labor supply elasticity is closer to the average, though again with substantial variation.

Using this table F3, we can use cross-sectional regressions to summarize the associations (weighted by the number of firms). These coefficients should of course not be interpreted causally, but rather as descriptively. Higher value added is strongly associated with higher minimum wages (p-value= 0.01), and marginally significantly associated with profits (p-value= 0.13). Wages and firm wage premia are also strongly associated with the minima (p-values of 0.00 and 0.02), and so is the average worker quality (as proxied by AKM worker effects, p-value= 0.00). Separations are not associated with the minimum wages, which may be surprising, though they are strongly negatively associated with firm wage premia (p-value=.00).

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

Table F3: **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 labor supply elasticities are estimated across all firms within each bargaining council. The sample is all formal sector firms from 2008 to 2018 using the SARS tax data.