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The impact of centralized bargaining on spillovers and the wage structure in monopsonistic labour markets

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The impact of centralized bargaining on spillovers and the wage structure in monopsonistic labour markets

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Abstract: How does centralized bargaining affect the broader wage structure? And what does this tell us about the (non-)competitive dynamics of such labour markets? I study large contracted wage increases negotiated by centralized bargaining councils in South Africa, using matched employer–employee tax panel data from 2008 to 2018. First, my stacked event-study of bargaining council firms shows sharp wage increases in bargaining councils, concentrated in mid-wage and mid-size firms. Second, I observe spillovers on firms competing in the same labour market as the bargaining council, as estimated by worker flows, such that more connected firms increase wages more—a prediction of monopsonistic models that contrasts with competitive models. Third, I discuss evidence that the effects of contract changes on bargaining council firms differ by the firm's average wage, decreasing the size of low-wage firms but having neutral or positive effects on the size of higher-wage firms. Altogether, these bargained wage increases compress the overall wage and job structure upwards, highlighting an interplay between institutional regulation, monopsonistic competition, and firm heterogeneity which reaches far beyond the direct impact of bargaining council firms.

Key words: wage structure, centralized bargaining, monopsony, spillovers, stacked event-study

JEL classification: C50, J31, J42, J52

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1 Introduction

What are the institutions that govern the wages and flows of workers in the South African labour market? While competition is a powerful force, centralized bargaining is estimated to govern the employment contracts of 30 per cent of South Africa's workers. By compiling bargaining council agreements between 2008 and 2018, then matching them with workers and firms using tax data, I demonstrate that the effects of bargaining councils extend to the *majority* of workers. In particular, I show that large negotiated wage increases result in observable wage gains for workers in both contractually bound firms *and* firms connected to them through worker flows. These results show more generally how regulation influences the wage structure by propagating through interconnected local labour markets.

One way to think about the broader effects of bargaining councils is in terms of employer power. Though once considered limited to company towns, monopsony power—one form of employer power—is increasingly recognized as pervasive (Dube et al. 2019; Sokolova and Sorensen 2021). The relevance of monopsony power extends to regulatory policies in developing countries such as Brazil and India (Lagos 2019; Muralidharan et al. 2017), substantially altering the effects predicted under a competitive framework. Firms have monopsony power when they can mark down wages from marginal productivity without many workers quitting in response: they can pay workers less with limited effect on production. Such lower quit responses can arise, for example, from search friction or job differentiation (Burdett and Mortensen 1998; Card et al. 2018). One way to constrain such monopsony power is through minimum wages, which may be set through unions of organized workers (Manning 2003a). While there is a large literature on collective bargaining, the underlying model of the labour market reverses its likely effect, from introducing wedges into the market (competitive case) to countering existing employer power (monopsonistic models).

There are also particular predictions from models of monopsonistic competition for the labour market structure more broadly. First, when minimum wages cover part of the labour market, as in the case of local bargaining councils, wage spillovers onto nearby firms are predicted. Nearby firms paying just above the relevant minimum wage are no longer as attractive as before relative to the next best covered firm, meaning they have to increase wages to retain workers (Manning 2003b). Second, the effect of the 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, mid-productivity firms may *increase* in size as the cost of the marginal worker decreases, and high-productivity firms paying higher wages may be largely unaffected (Dickens et al. 1999). The net employment effect can vary, but the structure of jobs concentrates more towards higher-wage, more productive firms. Third, even with such reallocation, the net effect on the share of workers in such high-productivity jobs will depend on the level of the minimum wage, the level of local wages, and the amount of monopsony power—factors a local bargaining council may take into account.

I investigate these predictions relating collective bargaining to broader monopsonistic labour markets. Does centralized bargaining actually affect wage setting, thereby constraining the markdowns associated with employer power? Do these constraints spill over onto nearby firms, as predicted by monopsonistic models? And how do these constraints influence the structure of jobs in the broader labour 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 firms and nearby firms. I measure nearby firms as firms that are not contractually bound by bargaining councils, but draw from the same local labour market as measured by the network of worker flows.

This study contributes to several literatures. First, I provide evidence on 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 et al. 2012; Budlender and

Sadeck 2007; Magruder 2012). Bargaining council workers are mostly in the upper-middle parts of the firm earnings distribution, with higher firm wage premia (largely accounted for by minimum wages), but also higher log value added per worker. South Africa's bargaining council firms have a much lower proportion of women, 30 per cent compared to 50 per cent in other firms, and consequently bargaining council minimum wages explain over *one-third*of the overall gender wage gap in formal sector wages. I also provide estimates of the effects of wage contract changes on these firms. There remains ambiguity in the literature as to whether unions have a measurable impact on wages, a 'first stage' for which I find strong evidence (Blandhol et al. 2020; DiNardo and Lee 2004). These sharp wage increases following contracted wage increases are concentrated in mid-wage (40th to 80th percentile) and mid-size (15–100 workers) firms, consistent with most bargaining council workers being in the upper-middle parts of the distribution.

Second, I approach the labour market as a network of firm dependencies by providing causal evidence on spillovers in wage setting (Derenoncourt et al. 2021; Fortin et al. 2021; Staiger et al. 2010). 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 per cent of formal sector workers in South Africa, firms that are closely connected through overlapping local labour markets account for at least another 30 per cent. A key advantage of my study is that my matched employer–employee data allow me to trace the spillovers through these worker transitions between bargaining council, spillover, 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 et al. 2011). In terms of the aggregate wage structure, these spillover wage effects have a comparable additional effect on the middle tercile of the firm average wage distribution as bargaining council wage changes, and a much larger effect in the bottom tercile due to the more even distribution of spillover firms.

Third, I discuss how the mass of workers reallocate away from low-wage firms and towards higher-wage firms. The combination of effects on bargaining council and spillover firms therefore alters the labour market structure substantially. A key developmental consequence is an ambiguous effect on the net job opportunities and firm productivity in a country of high unemployment. I discuss how these effects themselves depend on the local wage structure, for example by considering heterogeneity by local average wages (Kaitz index). I develop the analysis of wage structure and aggregate effects, among other aspects, in ongoing work.

In Section 2 I review the literature on South Africa's bargaining councils, and then proceed to profile these labour market institutions using the merged data set of bargaining council agreements and tax data that I have compiled. I evaluate the effects of large contract changes on bargaining council firms in Section 3, and on spillover firms in Section 4. I discuss evidence on reallocation, robustness, and heterogeneity in Section 5, and conclude with my plan for further analysis in this paper.

2 Description of South African bargaining councils

2.1 Brief review of the South African bargaining council literature

Bargaining councils have perhaps been the central institutional feature of the South African labour 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 labour market in the following way, from least organized to most (Levy et al. 2014). First, about one-third of all workers are informally employed, typically without adhering to minimum conditions such as a written contract (Bassier et al. 2020). Second, the Basic Conditions of Employment Act (BCEA) and Labour Relations Act form the minimum conditions of employment and are applicable to all employment relationships—in reality, covering formal sector workers. These regulations are most relevant to uncovered formal sector workers, who include both low-wage workers who are completely unrepresented and high-wage workers who negotiate bilaterally with their employers.[1] Third, 'sectoral determinations' are set unilaterally by the government for selected industries, mostly made up of low-wage workers.[2] Fourth, any workers can become union members, and workers can seek a union recognition agreement if at least 30 per cent of the workplace belongs to the union. Fifth, 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. The precedence of organizational structures is not straightforward; for example, bargaining council agreements can vary some conditions *below* the BCEA. This is actually common practice: I find that nearly half of bargaining council agreements increase the number of overtime hours beyond the maximum set in the BCEA, or decrease the period of notice required for termination.

Over the last three decades, there have been over a dozen studies of union and collective bargaining premia in South Africa, including Moll (1993, 1996), Schultz and Mwabu (1998), Butcher and Rouse (1999), Bhorat et al. (2012), Flowerday et al. (2017), and von Fintel (2017). Wittenberg and Kerr (2019) provide an excellent review of the union premium literature, focusing on union wage premia and providing updated estimates. 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. They document a unionization rate of about 30 per cent in the South African labour market, and estimate a union wage premium of 25–30 per cent, 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.

The literature focused on bargaining councils has been more limited than the union literature in that there is no clean identifier for bargaining councils. A contribution of this paper is to compile a publicly available data set classifying industries and regions into each bargaining council, with wages for each bargaining council separately by year. In addition, the existing papers use household survey data, which are limited by non-representativeness at the bargaining council level (as opposed to the tax data, where I observe the full population of firms). Bhorat et al. (2012) combine survey data from 2005 with gazetted bargaining council documents to estimate a bargaining council premium of about 9 per cent above the union premium. They instrument union membership here by union membership of others in the household, which Wittenberg (2014) notes requires a logically inconsistent data-generating process. While I

[1] A national minimum wage was introduced in 2019, and is applicable to all workers. My period of analysis is from 2008 to 2018, so I do not focus on this.

[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.

do not focus on the level of the bargaining council wage premium, I do provide an estimate below of 15 per cent in addition to the union premium. Flowerday et al. (2017) consider 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 per cent. These 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.

A major study in this literature on South African bargaining councils is that by Magruder (2012), who finds negative employment effects, which are concentrated among smaller firms. 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, such as the quality of household survey data used[3] and the endogeneity of boundaries defined by the bargaining councils.[4] On aggregate I do not detect these decreases in firm size, and indeed many of Magruder's specifications (including the preferred ones) do not detect negative firm size effects for larger firms. However, I do discuss reallocation effects, where I find decreases in firm size for the low-wage subgroup of firms, in addition to neutral or positive effects for larger firms. The policy conclusions from such a reallocation are unclear; for example, if workers are mostly reallocating 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 spillover effects of bargaining councils, or discussed their overall impact on the labour market structure. My analysis on spillovers also contributes more generally to the literature on regulatory policies in South Africa, such as state-set minimum wage effects, where spillovers have only been considered in passing (Bhorat et al. 2013; Dinkelman and Ranchhod 2012; Piek et al. 2020). This may be particularly salient for studies evaluating the effects of the recent national minimum wage.

2.2 Description of the matched panel

I provide detail on how I construct my main matched firm panel in Appendix Section D3. 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. 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 three-digit industry level, though some are defined at the two- or four-digit levels. 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 Labour 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,b). 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

[3] The location of households is measured, not firms; and the industry is only measured at the two-digit level, whereas many bargaining councils are more accurately defined at the three- or four-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.

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

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.

I use my constructed data set to profile the South African formal labour market into its constituent regulatory regimes as described above. Bargaining council firms cover 40 per cent of formal sector workers. While I do not see union membership in these data, I merge in union density at the municipality by industry level using the Quarterly Labour Force Survey (QLFS) for the corresponding years.[5] About 20 per cent of workers are part of high-union- density industry-locations but are not part of bargaining councils. A further 15 per cent of formal sector workers are neither in bargaining councils nor in high-union-density cells, and the balance—about 25 per cent—are uncovered. We can also factor in the 33 per cent of workers in the informal sector in order to have a profile of the entire labour market, and in this case approximately 30 per cent of workers are part of bargaining councils, 15 per cent are in the union category, 10 per cent are only covered by sectoral determinations, and 20 per cent are uncovered.[6]

What are the characteristics of firms in these different regulatory regimes, and in particular bargaining council firms? Table A1 presents some comparisons between bargaining council firms, sectoral determination firms, and uncovered firms (as the omitted category). I begin by comparing the firm effects following Abowd et al. (1999) or AKM, which isolates the wage premium associated with the firm, including bargaining council or union effects.[7] On average, bargaining council firms are high-wage firms, and sectoral determination firms pay low wages. Accounting for industry and location fixed effects, however, firms covered by sectoral determinations pay slightly higher, highlighting that sectoral determinations tend to be in low-wage industries. As explained above, I do not observe union membership directly, but can control for it using a proxy of union density of the industry-location from the QLFS. Controlling for unionization, the bargaining council specific premium is about 15 per cent (column 3). Bargaining council firms have much less within-firm wage inequality than uncovered firms (columns 4 and 5), a characteristic 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 seems to be entirely accounted for by industry, since bargaining councils *do* have unconditionally higher value added per worker, but this difference disappears as soon as industry fixed effects are introduced into the cross-sectional regression.

Figure A1 shows that in addition to these differences in firm wage premia, the sorting of workers by firm wage premia differs for bargaining council versus other firms. For the same AKM firm effect percentiles (i.e. the values of wage premia are comparable), I plot the mean AKM worker effect. This is a proxy for worker quality, and shows the steep profile of increasing worker quality with firm wage premia in

[5] The QLFS is a representative survey of households focusing on labour market information, and is collected by the national statistics agency, Statistics South Africa (StatsSA 2020). Approximately 20,000 households and 70,000 individuals are surveyed on a quarterly basis. This is a primary source of data for national labour market statistics, such as the employment rate.

[6] One alternative source is the QLFS. Using the 2019 quarter 4 release, 30 per cent of workers say they are a member of a trade union. In response to the question, 'Who sets your salary?', only 8 per cent say their wage is set by a bargaining council, a further 22 per cent say their wage is negotiated between their union and employer, 10 per cent say bilateral negotiations, 55 per cent say it is unilaterally set by their employer, and 5 per cent say they do not have a regular wage increment. These proportions are lower than implied by my constructed data set, but not necessarily inconsistent: a worker may not know they are part of a bargaining council, and many workers are not part of a union but are part of a bargaining council. Budlender and Sadeck (2007) investigate what proportion of workers are part of bargaining councils using survey data on industry and location, and find a similar figure to me of 30 per cent.

[7] This follows Abowd et al. (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 (2019) for more details and an application in the context of these South African tax data.

non-bargaining council firms. This sorting of high-wage workers to high-wage firms is observed in several countries (Engbom and Moser 2018; Song et al. 2018). However, such sorting is substantially flatter 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. Despite these differences, the figure also shows 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 effects of bargaining councils, but also emphasize the possibilities for adequate matching in a credible research design.

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 areas of the firm earnings distribution, increasing in proportion from about 20 per cent in the lowest decile up to 70 per cent in the 8th decile, and then dropping off to 30 per cent 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—that is, 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 E3 in the Data Appendix (Section D3) 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 one 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 ZAR2,500 per month or as high as ZAR10,000 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 (I set this as the value of 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 added firms to about half the firm wage for the highest value added firms. Note that this minimum wage gap is constant across the deciles of value added on average, despite the strong association between minimum wages and value added between each bargaining council, as noted in Table E3.

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 levelling 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 per cent compared to over 50 per cent for the rest of the labour 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. This is consistent with my previous analysis in Bassier (2019), 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).

Using the decomposition as in Figure A3, I further estimate that over 80 per cent of the gender gap in firm average wages is accounted for by differences in minimum wage gaps. Thus the minimum wage gaps explain over one-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 also differences even within bargaining council firms, as shown in the bottom panel of Figure A3. Women tend to be towards the lower minimum wage of bargaining councils: 20 per cent 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, the public sector bargaining council has a completely different gender impact to the private sector: there are more women than men in the public sector, and minimum wages actually favour women here. I explore these decompositions further in ongoing analysis for this paper, including the substantial gender-equalizing effect of the public sector.

Overall, the matching of agreements into the tax data reveals a profile of bargaining councils that shows higher firm wages and lower within-firm inequality, though with wide variation across value added, minimum wages, and other characteristics. Two common features, namely the lower proportion of women and the location of the mass of workers towards the higher parts of the firm earnings distribution, highlight the potential for marginal gains of bargaining council workers to increase gender and wage inequality among workers. On the other hand, the high value of profit per worker observed within bargaining councils highlights the potential for gains of these workers to reduce overall income inequality.

3 Treatment effects of contracted wage increases

3.1 Empirical design

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 2012–16 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 labour' occupational category (usually the lowest), where 'large' is defined as greater than 3 per cent. I exclude similarly large increases in the preceding two 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).[8] 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 the regression sample, where connected is defined as firms

[8] 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.

with more than 1 per cent of worker flows to bargaining councils. Figure B4 shows how much the wage effect is attenuated when including contaminated controls, from about 4 per cent to 3 per cent, 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 event years, and for firms to have at least ten 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):

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

Identification of the main coefficients of interest δ_t arises from comparing bargaining council firms to similar firms within the same location. Pre-event $\delta_{t<0}$ is a test of pre-trends up to three years prior, and δ_t are all interpreted relative 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 D4 shows clean pre-trends for the outcomes in this case, except again for firm size.

3.2 Results

Figure B2 shows large average wage increases following the contract changes. The 25th percentile of within-firm wages increases by 4 per cent post-implementation, with flat pre-trends. The increase is slightly lower for median within-firm wages (3 per cent), and in general appears to decrease as I consider higher percentiles of within-firm wages (though at the 80th percentile there is still a 2 per cent increase). New hires are also paid 2–3 per cent 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 in a literature of about a dozen studies of union and collective bargaining premia. A positive wage effect of union bargaining has also been challenged in other settings (Blandhol et al. 2020; DiNardo and Lee 2004).

The wage increases vary substantially by wage of the firm: at the lower quantiles of across-firm wages (low-wage firms) 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 per cent wage increases.[9] This in fact implies some impact over and above the contracted wage increases, perhaps related to within-firm wage spillovers. The wage increases are lower for the top quantiles again, 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 (15–100 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

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

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 per cent at the event year (statistically significant in event years 0 and 1), after flat pre-trends. Interpreted directly, this implies a firm labour supply elasticity of about 1.5, which suggests considerable monopsony power in line with Bassier (2019).[10] However, this is very likely biased as an estimate of a reduction in turnover since I cannot differentiate between voluntary quits and involuntary fires. Indeed, firm size does not show detectable changes, with a confidence interval between –0.02 and 0.01. Unemployment insurance payments respond strongly, with flat pre-trends and an increase of about 1.5 per cent—implying a positive co-variance between wages and amenities in these contracts. As we noted in Figure B2, 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 reassurance that these estimates do not carry substantial bias from possible endogeneity of bargaining contracts to prior firm performance.

The main specification given by Equation 1 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 per cent increase in wages, as in the main results above. However, using this sparser specification, firm size exhibits a pre-trend that disappears with the additional controls. Figure B4 also shows what happens when we fail to exclude potentially 'contaminated' controls: the wage effect is about 3 per cent instead of 4 per cent. This hints strongly at the spillover results presented in the following section.[11]

Although these results show a sharp impact of the contracted wages, the level of contracted wages can be below (due to firm wage setting) or above (due to weak enforcement) the actual level of wages. As a bounding exercise for the 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 per cent) than when using observed wages (3 per cent). While the level of wages may exhibit substantial non-compliance across these firms, this implies that the dynamic changes in wages follow the contracted wages quite closely.

4 Spillover effects of contracted wage increases

4.1 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 incentivize workers against leaving towards the treated firms.

[10] Using the 3 per cent wage effect, yielding a separations elasticity of –0.75, and the formula in Manning (2003a) that the firm labour supply elasticity is –2 times the separations elasticity.

[11] 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 per cent to 3 per cent implies a spillover wage effect of about 2.2 per cent. This is very close to several of the direct estimates of wage spillovers in the next section.

To quantify these wage responses, or spillovers, from connected untreated firms, a key question is how the treatment dosage is defined. 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 labour market. Labour-constrained firms are competing over this same labour pool, meaning that wage spillovers operating through the labour 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 et al. (2013).

My flow measure is also very close to one used by Arnold (2020) in defining labour markets. In deriving a measure of labour 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.[12] In my data, 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 1 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 labour 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. 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?

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

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* labour 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 $flow_{j(c)}$ variable. As a generated regressor there will be noise in this variable compared to the true value of the firm connectivity, and this will attenuate the coefficient towards 0. I avoid this attenuation bias by randomly splitting firms in each industry-location, and instrumenting the average flow for the firm's own sample by the average flow for the complement sample within each industry-location. This approach has been used by Bassier et al. (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 2, where I compare highly connected firms (greater than 5 per cent 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.

[12] Theoretically, the division by size is motivated by that idea that the option value of a job in A for a worker in B may decrease with A's size, since flows are more likely to a larger A, conditional on the same value.

A quick comment on the constructed regressor, the flow variable. Why not simply compare firms in the same industry-location as bargaining council firms? First, industry and location are in fact not good indicators of whether firms draw from the same labour market. One way to see this is to compare the flow of employment–employment firm switches across the full sample of workers. Only 30 per cent of switches are within the same SIC (Standard Industrial Classification) one-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.

4.2 Descriptives of spillover firms

What do these spillover firms look like? Table C1 shows the characteristics of firms by the proportion of their flows to bargaining council firms (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.[13] In this stacked event firm data set used in the main specification (restricted to event-year –1 and firms with more than ten workers), nearly one-third of bargaining 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), but fewer workers (four million compared to five million). The firm size is considerably larger in bargaining council firms than in spillover or other firms (270 compared to 170); the proportion of women is much lower in bargaining council firms; and the AKM firm wage effect is 10–15 per cent higher in bargaining council firms. 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 centres (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 centre, 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 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 are bargaining council firms. The implications are that there is a lot of movement across industries, as well as firm earnings classes. The spillovers observed below therefore propagate diffusely through the economy.

4.3 Results

Figure C3 shows the spillover effects on firm wages. For an indication of magnitude, I scale the outcomes by the average flow in approximately the top 24,000 firms.[14] To interpret this, the figure shows that average wages in industry-locations with greater than 5 per cent of pre-event worker flows to the relevant bargaining council experienced an increase of nearly 4 per cent at the 25th percentile of within-firm

[13] 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.

[14] This is an arbitrary scaling, since every firm in the entire economy has a measure of flows, even if most were 0. The value 24,000 is chosen because it is the number of firms with more than 5 per cent of their flows towards bargaining councils. It is also a comparable figure to the number of firms in bargaining councils.

wages and 3.5 per cent at the 50th percentile of within-firm wages. This is as large as the comparable wage effects on directly treated firms. There is a substantial and significant effect across the distribution of within-firm wages, ranging from about 2 to 4 per cent. 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 per cent in the middle-waged firms. 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.[15]

Figure C4 shows other firm outcomes. Separations decrease strongly in the post-period, equal to about a 4 per cent decrease in separations and implying a firm labour supply elasticity of about 2.[16] Firm size may decrease, with a point estimate of –2 per cent, but not statistically significant. As for bargaining council firms, unemployment insurance payments increase strongly, up to 2 per cent by the final event year. For each of these outcomes, including the wages, the effect appears strongest in the final event year. This implies some kind of lagged response, as wages increase by the first year after the event in bargaining council firms, and may take a year to propagate outwards through worker flows.

Once again, profit margins 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 et al. 2011). How plausible is this trade-off in terms of the observed reduction in profit and increase in wages? I perform a counterfactual simulation in which I increase each firm's wage bill by 3 per cent, and then reduce firm profits by that 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 reassuring that the average reduction in profit per worker implied is 3.3 per cent. This is not far from the decrease in profit per worker estimated for spillover firms, which ranges from 3 to 7 per cent.

In Table C2 I provide the point estimates for the final period as well as for alternative specifications of these spillover regressions. Column 1 shows the responses for the 25th and 50th percentiles of within-firm wages, as well as firm size and profit margin.[17] Column 2 shows the estimates using ordinary least squares (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. 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 that enabled the contracted wage increases to begin with. 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. Column

[15] For example, if a bargaining council is defined by the three-digit industry code, I exclude all firms in the same *two-digit* industry code. This means that adjacent three-digit industry codes that may be included in bargaining contracts do not enter the regression. This also means that the spillovers are potentially *underestimated*, since these firms that are excluded are high-flow firms (depending on the linearity of the relationship of spillovers to the flow measure).

[16] The firm labour supply elasticity is the elasticity of firm size to wages, which is equal to –2 times the separations response (Manning 2003a). The spillover shock is plausibly exogenous to the firm, identifying movements along the firm labour supply curve. However, this separations elasticity is likely biased, especially if the wage increases are a response to changing separations towards bargaining council firms.

[17] The event-year 2 effect is not statistically significant for the profit margin, but as shown in Figure C4, it is significant with comparable magnitude in event-year 1. It is also significant in the final year for several alternative specifications.

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 per cent for the wage spillovers (all strongly significant with relatively flat pre-trends), and from 0 to –2 per cent for the firm size effects (some significant, some not).

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.

Figure C5 provides an indication of the importance of the spillover effects relative to the direct effects on bargaining council firms. I use the full firm by year panel in the tax data, and estimate a cross-sectional bargaining council firm wage premium as in Table A1, except by tercile of firm wages. The figure shows in green the effect of subtracting this estimated bargaining council premium from bargaining council firms, as an indication of the effect of removing the wage effect from bargaining councils. Average firm wages across the economy decrease most in the middle tercile, by about 7 per cent. I then estimate the flows from each firm to bargaining councils, and multiply this flow by my estimate of spillovers above. This simulation shows that further subtracting out the plausible spillover effect of bargaining councils changes the wage distribution by a comparable magnitude to the direct effect of bargaining councils, and that it has a much larger effect at the bottom tercile. This is driven by the relative location of bargaining council and spillover firms along the firm wage distribution, as shown in Figure C2. Overall, this simulation suggests that the firm wage distribution would shift down substantially without bargaining council wage premia when taking into account their direct and indirect effects. However, 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.

5 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 suggestive evidence of reallocation, considering robustness of these results to various specification concerns, and highlighting important aspects of heterogeneity.

5.1 Reallocation

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 with suggestive evidence of increases in firm size for higher-wage firms. Theoretically, reallocation of workers from low-wage to higher-wage firms can occur when firms are labour constrained, a minimum wage forces low-wage firms to downsize, and those workers transfer or reallocate towards the higher-wage firms. Even in a high-unemployment environment such as South Africa, bargaining council firms could plausibly be labour constrained if the jobs require skills and experience that are not easily found at the prevailing wage and if workers do not easily switch firms. Alternatively, monopsonistic wage setting by higher-wage firms may mean that the minimum wage increase results in an increase in firm size for larger firms as in standard monopsony models (Dickens et al. 1999). 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 D1, I show the coefficients of the main bargaining council firm specification 1, with additional interacted indicators for the 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.[18] 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 D2 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 in Table D2 the coefficient is positive and significant using other 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 is the question of the productivity effects, as workers move towards higher value added firms. In ongoing work for this paper, I explore these questions further.

5.2 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.

Column 2 presents the results from a specification that 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 while retaining enough events. Bargaining councils generally renegotiate wages annually, with major rounds at three-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 D3 shows the event-studies, notably a flatter pre-trend for the wage spillovers.

[18] 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.

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 per cent 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 (Equation 1) 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 D4 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 1) with the full set of fixed effects as specified there, and with the additional weighting of firms by the propensity score weights.[19] 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 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.[20] The results are similar to the main specification.

5.3 Heterogeneity

I discuss heterogeneity in these main results across four dimensions. First, I run an AKM regression on all firms in rolling three-year periods, which allows me to divide firms into high and low firm fixed effects based on event pre-periods.[21] 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 reallocation effects I presented earlier (Section 5.1), where lower-wage firms decrease in firm size and higher-wage firms expand. A similar contrast is observed for spillover firms. The magnitudes of the wage spillovers are much larger for low firm fixed effect firms, but the own-wage elasticities driven by the firm size effects are of opposite signs.

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[19] Results available on request.

[20] 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.

[21] This follows Abowd et al. (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 (2019) for more details and an application in the context of these South African tax data.

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 labour 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 labour market median. Columns 3 and 4 stratify within each event the industry-regions with low versus high Kaitz ratios, respectively. For firms with a low minimum wage relative to the median local wage, effects are generally more muted: the bargaining council wages only increase by 3 per cent, 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 labour supply elasticity within each bargaining council, columns 5 and 6 show bargaining council events with above and below median monopsony power (low and high labour supply elasticities, respectively). Theory predicts that higher monopsony power should be associated with muted bargaining council firm size responses, but strong spillovers. 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. Given the coarseness of the divisions between events, this evidence is merely suggestive, though consistent with predictions from monopsonistic models.

Finally, I evaluate heterogeneity by sex. 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 2.

6 Conclusion

This paper demonstrates the direct and indirect impact of centralized collective bargaining on the labour 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 aggregate. Firms that are strongly connected to the same labour market as these bargaining council firms also see wage increases of a similar magnitude, together with a decrease in profit margins. These effects are heterogeneous by the pre-period wage of the firms, such that low-wage bargaining council firms decrease in size while mid- and higher-wage firms see wage increases with neutral or positive firm size effects. These effects are also larger when the minimum wage is set higher relative to the local average wage, a difference I observe by comparing high-wage to low-wage regions for the same nationally set bargaining council wages.

I plan to develop several aspects of this paper. First, I am laying out a model that will help to interpret these direct, spillover, and reallocation effects. Using a network structure of firm dependencies, the model will show the impact of a minimum wage that covers part of the labour market, with heterogeneous firms and admitting varying degrees of monopsony power. Second, I will develop the reallocation analysis by testing robustness and showing the net effect on employment, inequality, and productivity. Third, I plan to further investigate the mechanisms associated with the spillovers, in particular by exploiting the worker-level data.

The labour 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 labour 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. While my study focuses on the flow of workers connecting firms through overlapping local labour markets, there are several other mechanisms for spillovers, such as norms of fairness (workers in spillover firms may benchmark their level of a 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). Whatever the source of these spillovers, altogether the propagation of centralized wage regulation provides some hope for reversing the trend of rising between-firm inequality in several countries (Card et al. 2013; Song et al. 2018).

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Appendix A: Figures: description of bargaining councils

Table A1: Bargaining council premia

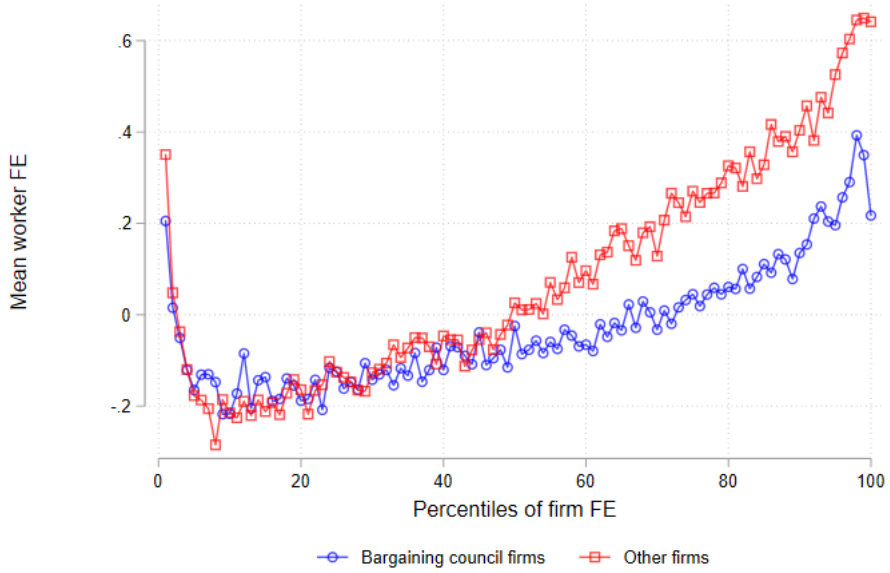
	AKM firm FE			Within-firm wage inequality		Value added pp
				p90–p50	p50–p10	
	(1)	(2)	(3)	(4)	(5)	(6)
Bargaining council	0.297***	0.315***	0.148**	–0.153***	–0.111**	–0.010
	(0.060)	(0.041)	(0.061)	(0.027)	(0.048)	(0.051)
Sect. determination	–0.198***	0.098	0.003	–0.155***	0.067	–0.079
	(0.057)	(0.060)	(0.069)	(0.033)	(0.042)	(0.053)
Obs.	644,789	639,710	544,864	1,678,578	1,678,550	1,029,023
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			

Note: 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 one-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 QLFS of 2010–16. 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.

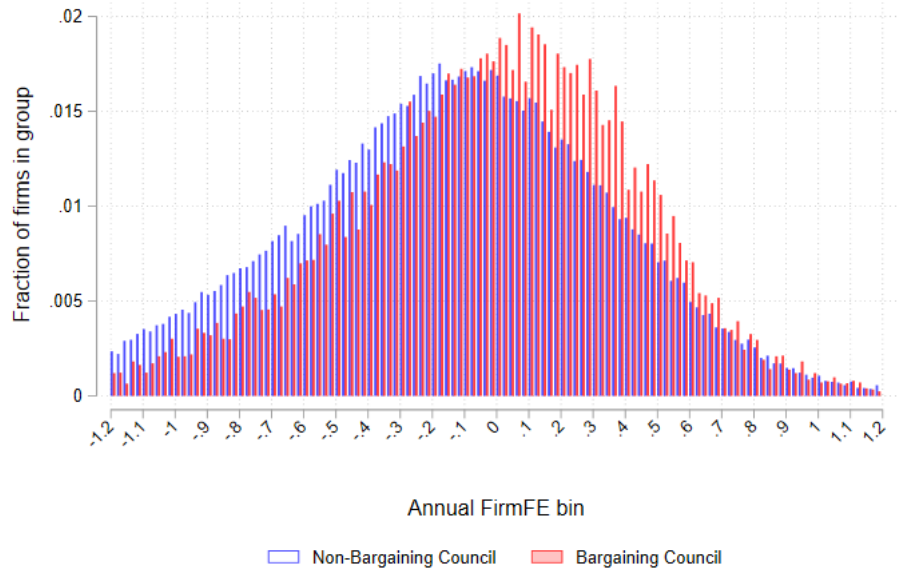
Source: author's own calculations.

Figure A1: Bargaining councils, AKM firm FE, and sorting

(a) Sorting of firm and worker FE



(b) Distribution of firm FE

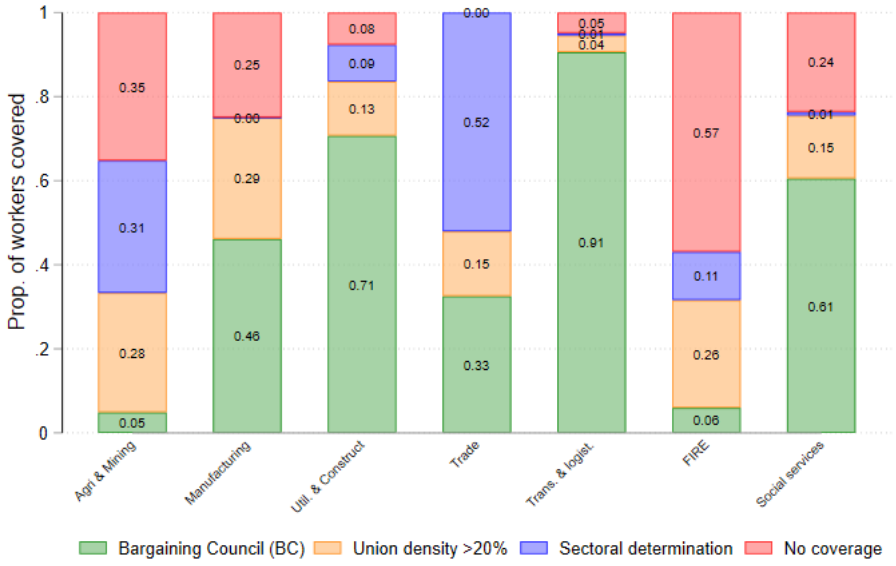


Note: AKM firm and worker FE refer to the respective components from a regression of log annualized wages on two-way fixed effects for workers and firms, estimated over the full sample from 2008–18. The figures are plotted at the unweighted firm level. The percentiles of firm FE are calculated on the full distribution—that is, 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).

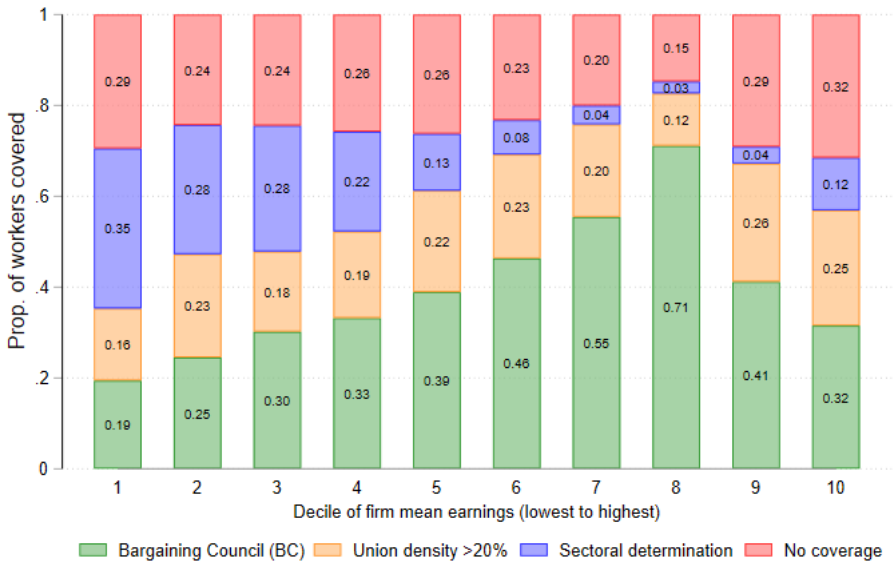
Source: author's own calculations.

Figure A2: Distribution of workers by regulatory regime

(a) By industry



(b) By firm average earnings decile



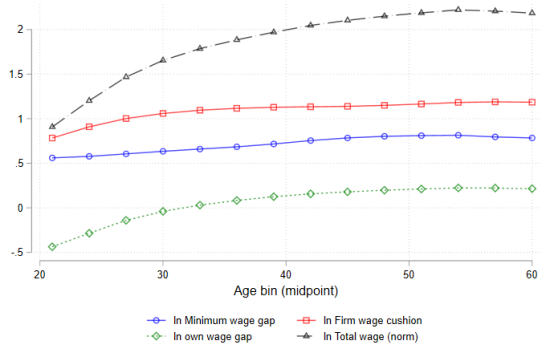
Note: I divide firms into mutually exclusive 'regulatory regimes' of bargaining councils (wages bargained over between worker unions and employer collectives), non-bargaining council unions (wages bargained by unions within each firm), sectoral determinations (wages set by government), and formal sector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

Source: author's own calculations.

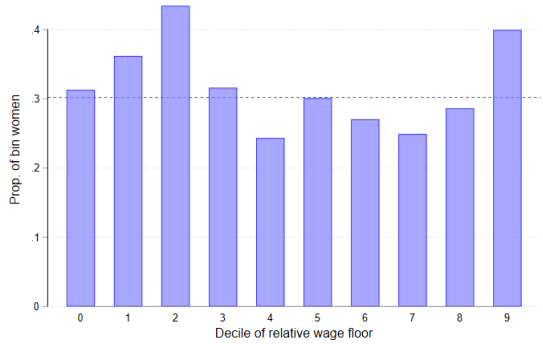
Figure A3: Decomposition of wages: floors and cushions

(a) Comparison of regimes

(b) Age profile



(c) Density of women among bargaining council wage floors

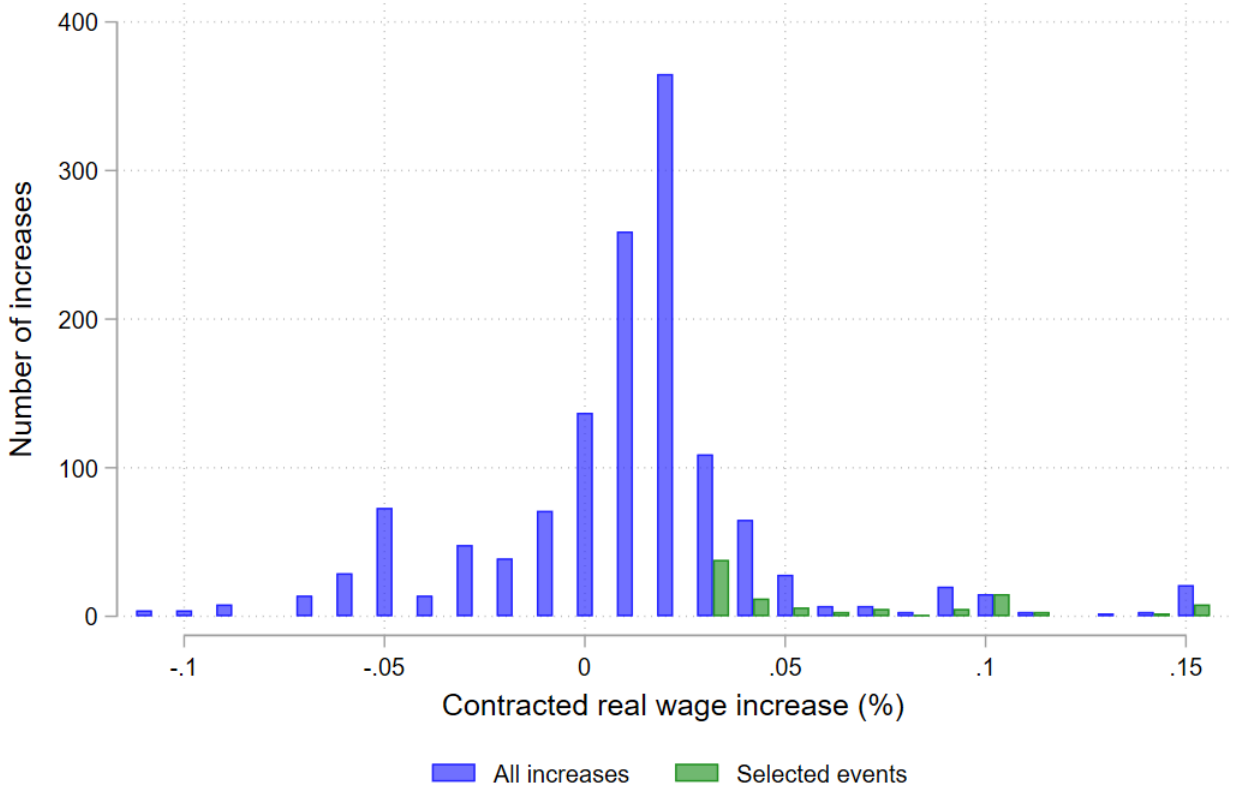


Note: the analysis follows the decompositions suggested in Card and Cardoso (2021). I decompose the wage into a baseline (percentile 1 of wages), the gap between the baseline and the bargaining council wage floor, the gap between the wage floor and the firm average wage ('firm cushion'), and the gap 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 those that are not covered. The middle 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. The bottom 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).

Source: author's own calculations.

Appendix B: Figures: effects of contract changes on bargaining council firms

Figure B1: Bargaining council wage increases and selected events, 2008–18

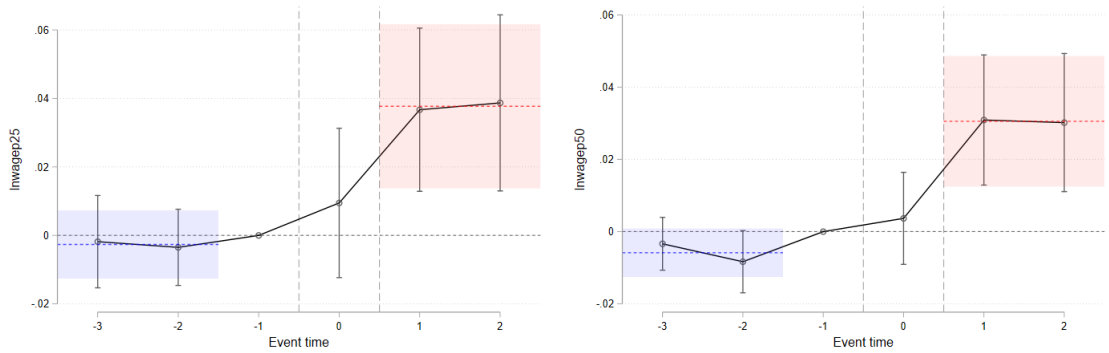


Note: out of all annual bargaining council wage increases, events are selected based on (1) a minimum of a 3 per cent real wage increase; (2) at least three pre- and three post-periods (implying only 2011–16 admitted); and (3) no real wage increases greater than 3 per cent in the pre-period. The final bar in the figure includes all increases greater than 15 per cent.

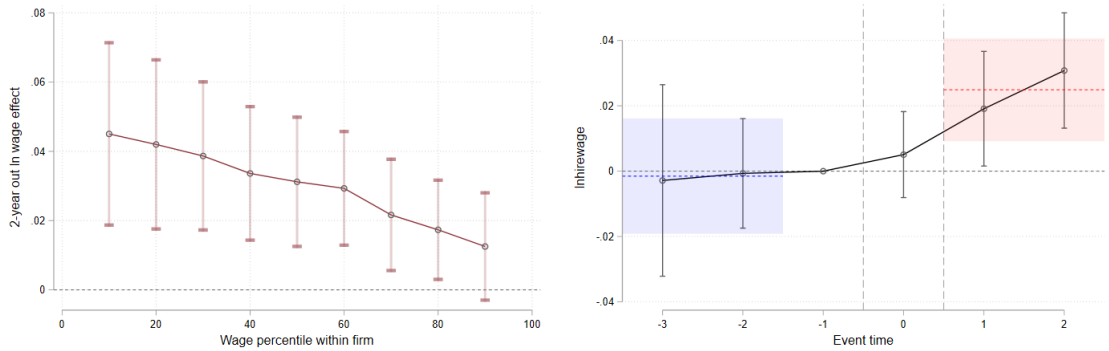
Source: author's own calculations.

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

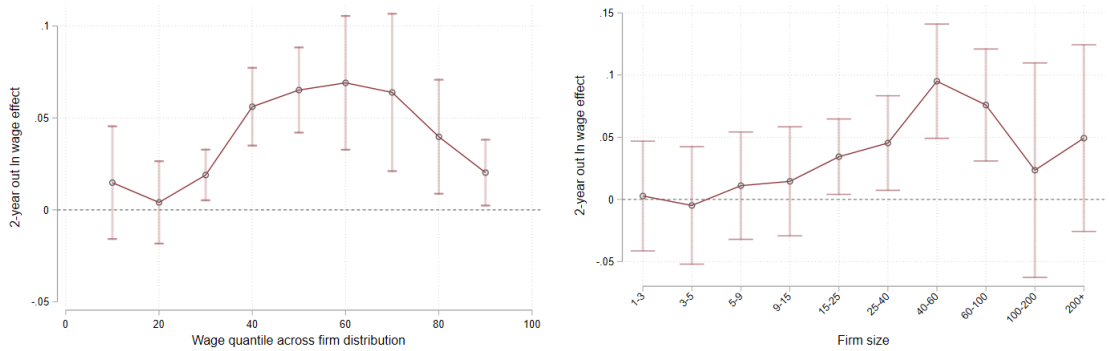
(a) Within firms: p25 and p50



(b) By quantile within firms and new hires



(c) By quantile of across-firm wages and firm size

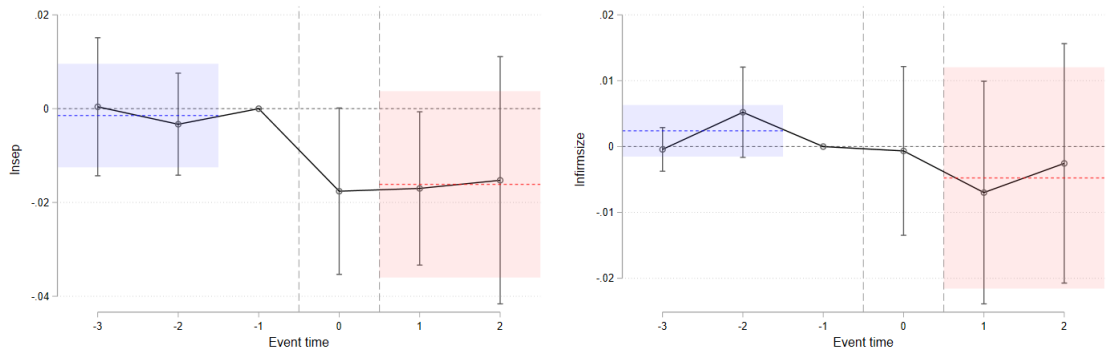


Note: each event contains non-treated control firms, and are stacked in a panel unique on event, firm, and year combinations. Firms with more than 1 per cent of worker flows to the bargaining council are excluded from the control. The regression is run at the unweighted firm level, is restricted to balanced firms with more than ten 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 within-firm wages and quantiles 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.

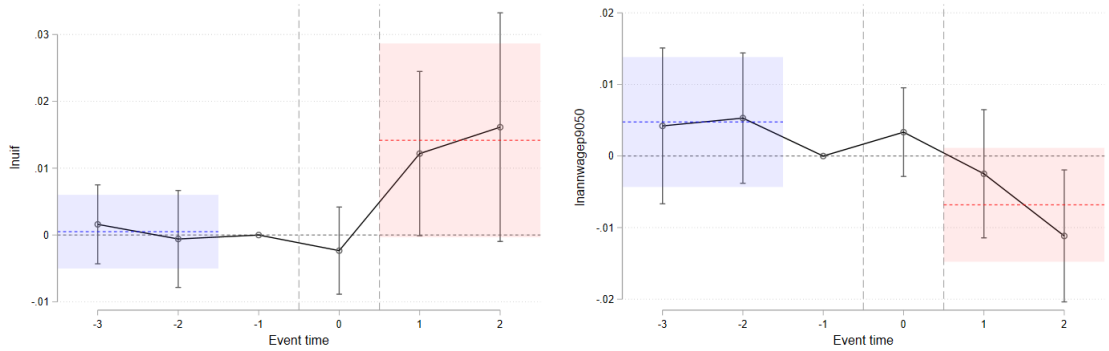
Source: author's own calculations.

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

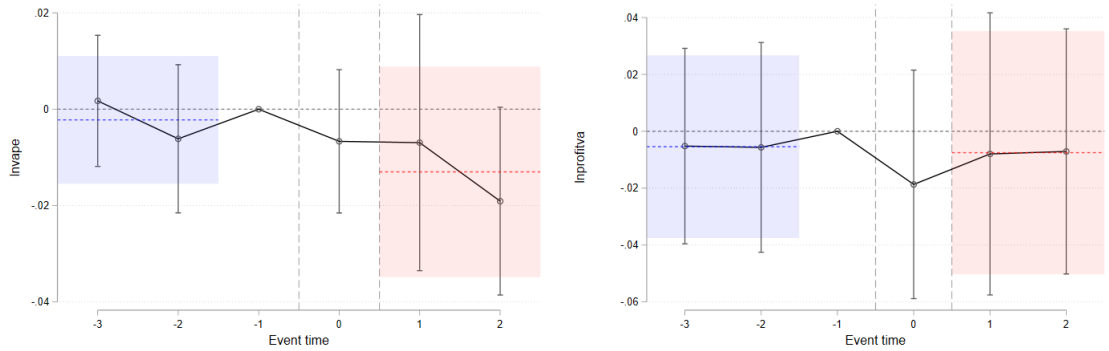
(a) Separations and firm size



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



(c) Value added per worker and profit margin

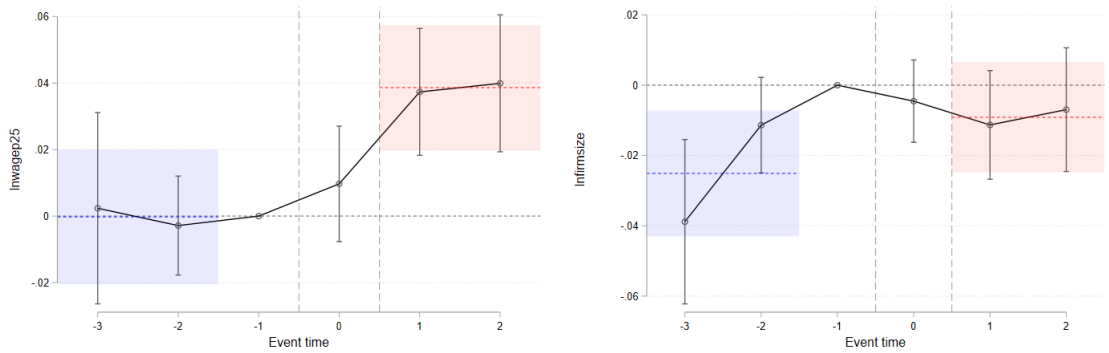


Note: see text or previous figure for specification details. All outcomes are logged. The top panel outcomes are the log of the proportion of a firm's workers that separate in each year, and the log of the number of workers per firm. Value added per worker is defined as firm sales minus capital and intermediate 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.

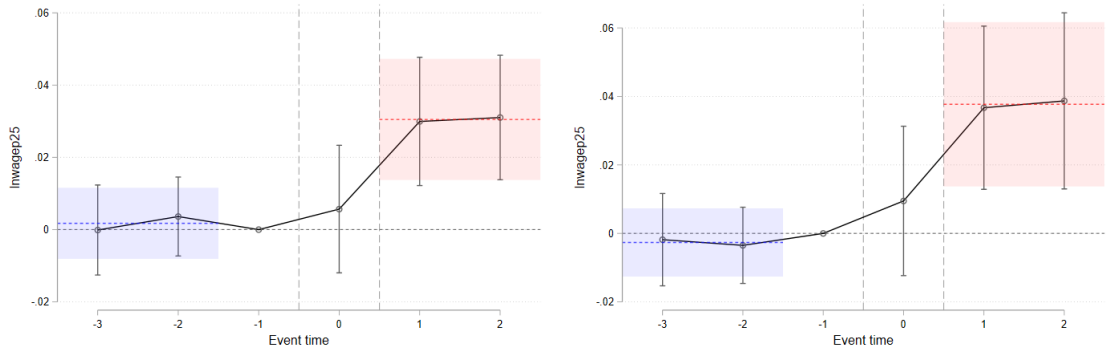
Source: author's own calculations.

Figure B4: Effect of contracted wage increases: rejected specifications

(a) Only location–time fixed effects: annualized wage and firm size



(b) Including versus excluding high-spillover firms



Note: the panels follow the main sample and specification, except for the following adjustments. The top panel includes only firm and event-year fixed effects—that is, 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).

Source: author's own calculations.

Appendix C: Figures: spillovers of contract changes

C1 Descriptives of spillover firms

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

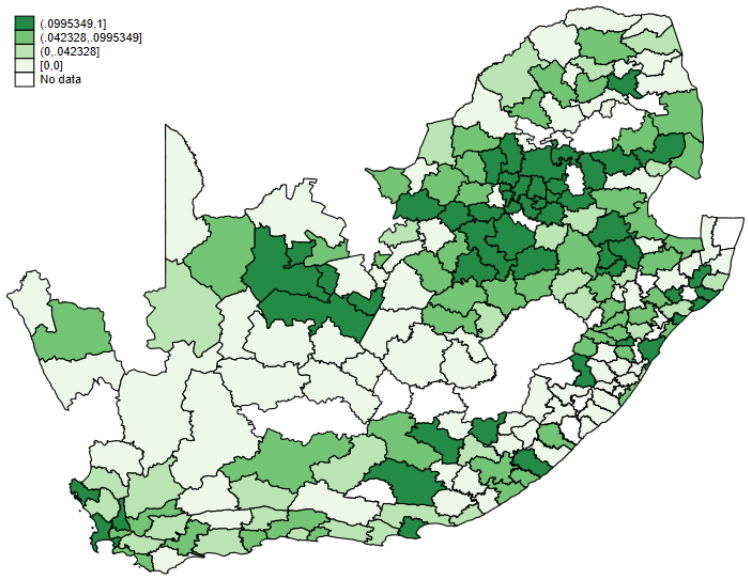
Statistic	(1) Treated	(2) High	(3) Medium	(4) Low	(5) Unconnected
flow to BC firms	0.32	0.15	0.07	0.02	0
workers	5,254,124	730,481	3,283,394	1.74e+07	6.07e+07
numfirms	19,345	6,473	18,622	97,527	345,325
firmsize	272	113	176	179	176
wage	112,157	104,893	111,151	118,888	123,760
wage p20	44,597	39,732	42,202	43,783	45,911
wage p50	83,346	78,529	82,257	87,651	92,441
wage p90p50	3.1	3.1	3.12	3.09	3.11
growth emp	0.05	0.04	0.06	0.06	0.06
growth wage	0.1	0.1	0.08	0.08	0.08
profitpe	216,582	244,141	256,005	235,529	250,281
value added pp	392,225	412,029	438,344	427,073	452,178
EEsep	0.1	0.09	0.1	0.12	0.12
EEhire	0.1	0.09	0.1	0.11	0.11
churn	0.48	0.49	0.5	0.53	0.52
female	0.24	0.41	0.4	0.48	0.48
worker FE	0.05	0.06	0.05	0.1	.13
firm FE	–0.07	–0.21	–0.18	–0.22	–0.18

Note: the sample is event year –1 of the main stacked event by firm by year balanced panel regression sample (restricted to firms with more than ten 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 (subtracting 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.

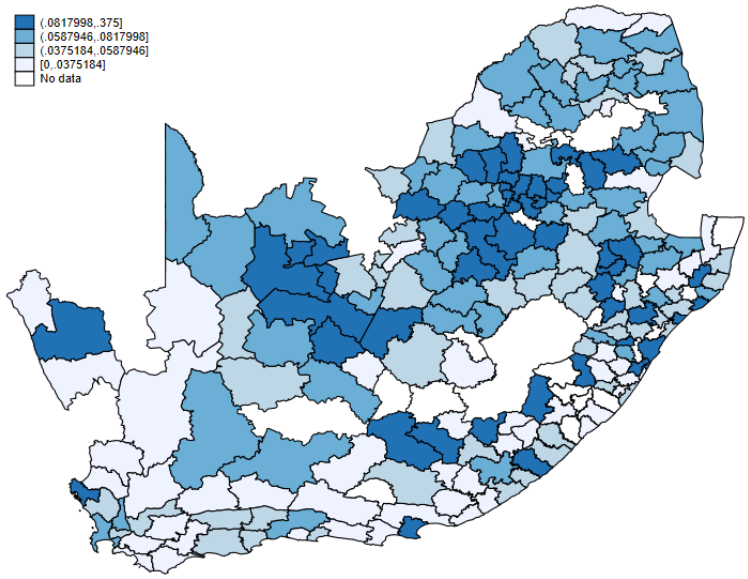
Source: author's own calculations.

Figure C1: Location of MEIBC firms and spillovers

(a) Bargaining council firms



(b) High-flow spillover firms

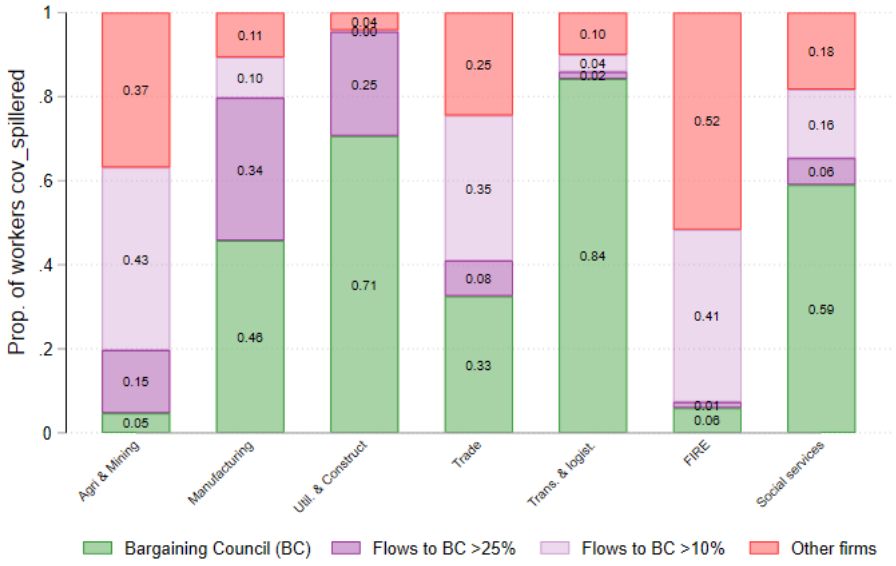


Note: the maps show municipalities in South Africa (approximately 232 distinct areas). The average proportion of bargaining council (top panel) and spillover (bottom panel) is plotted, with darker shades corresponding to higher proportions. Firms are classified as high-flow firms if they have more than 5 per cent of flows to bargaining council firms—that is, the high- and medium-flow categories in Table C1. The sample is from the Metals and Engineering Industry Bargaining Council (MEIBC) firms and their connected spillover firms.

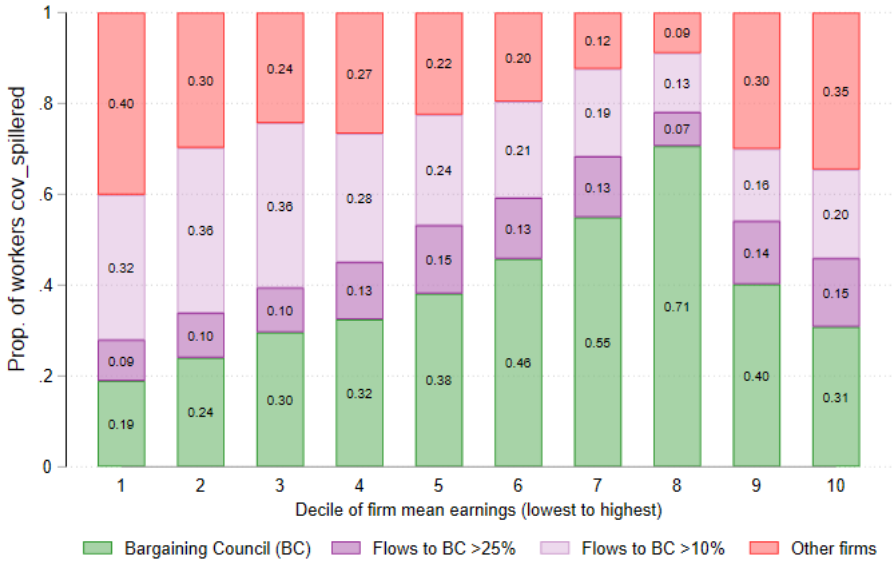
Source: author's own calculations.

Figure C2: Distribution of bargaining council, spillover, and other workers

(a) By industry



(b) By firm average earnings decile



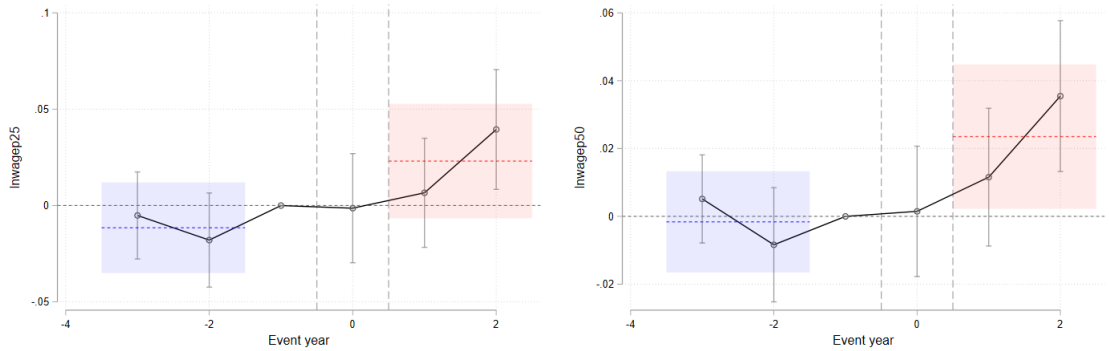
Note: flows to BC refer to average firm separations to and hires from bargaining council firms. Other firms include all other formal sector firms in the sample. The sample is all formal sector firms from 2008 to 2018 using the SARS tax data.

Source: author's own calculations.

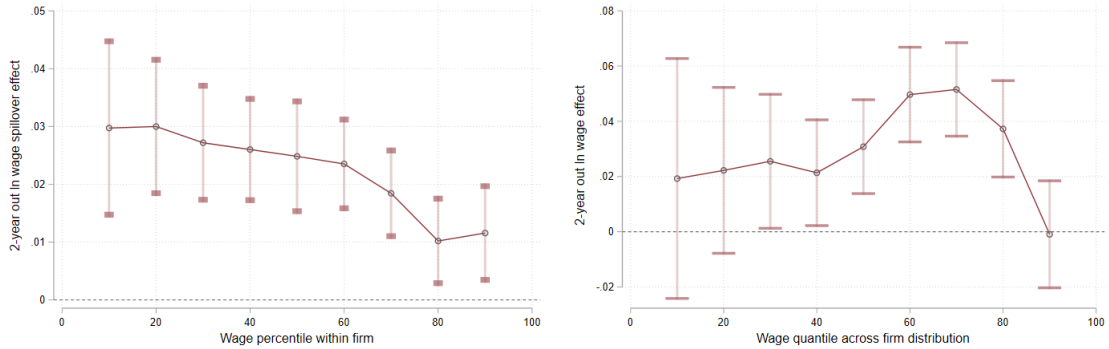
C2 Effects of wage contract changes on spillover firms

Figure C3: Spillover effects on wages by average flow in local markets

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



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

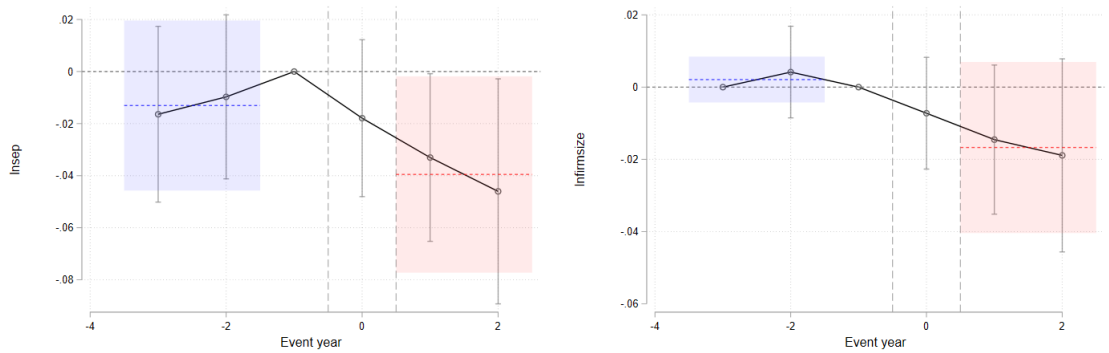


Note: the regression specification largely follows the specification used in the previous section for the effect of wage contracts on bargaining council firm outcomes. Each event contains non-treated control firms, and are stacked in a panel unique on event, firm, and year combinations. Firms with more than 1 per cent 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 ten 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 labour 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.

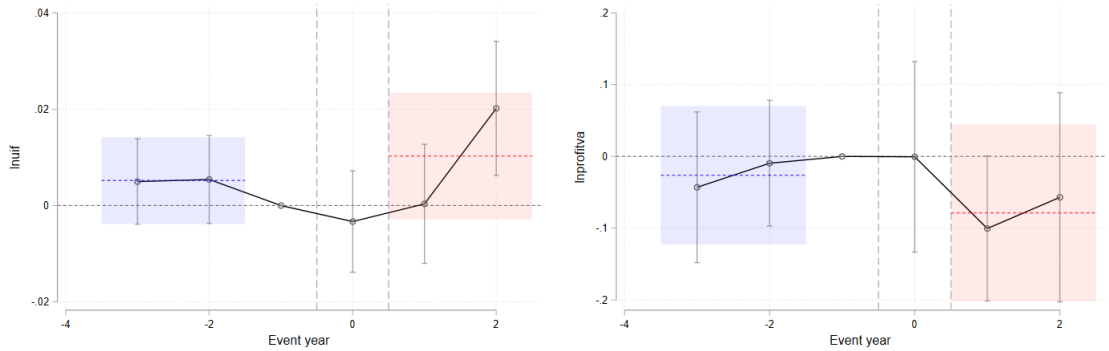
Source: author's own calculations.

Figure C4: Spillover effects on other outcomes by average flow in local markets

(a) Separations and firm size



(b) UI co-payments and profit margin



Note: see Figure C3 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. UI co-payments are amounts paid towards a worker's UI fund to be paid out in the case of retrenchment.

Source: author's own calculations.

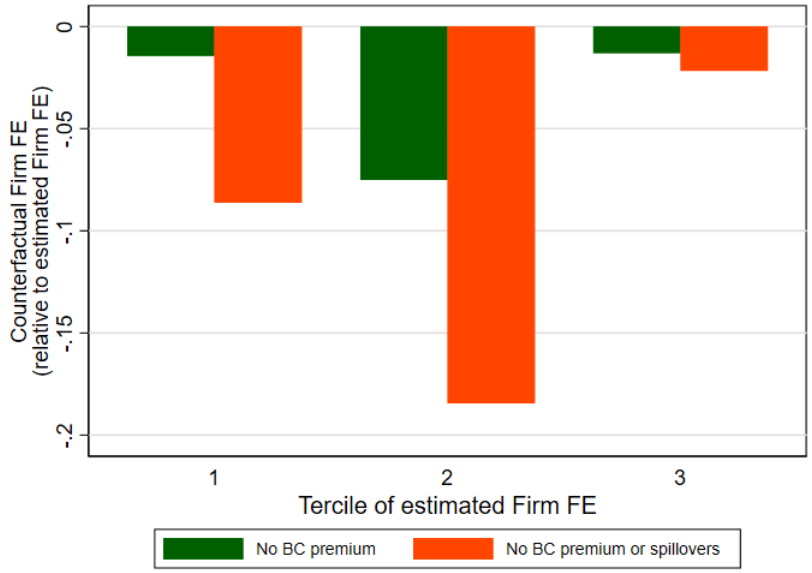
Table C2: Spillover effects from bargaining council wage contracts, event-year 2 coefficients from alternative specifications

	(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

Note: 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 seventh column divides the flow measure by the industry-location size as in Arnold (2020).

Source: author's own calculations.

Figure C5: Simulated effect on firm wage distribution



Note: this static simulation shows in the green bars what the change in wages, by tercile, would be if bargaining council firms subtracted their premium, where this bargaining council premium is the coefficient from a regression of AKM firm fixed effects on bargaining council coverage, by tercile of the firm wage distribution. The orange bars additionally subtract out the simulated spillover effect, which is estimated as the bargaining council premium multiplied by the quasi-experimental estimate of the cross-wage elasticity multiplied by flows to the bargaining council. The sample is all formal sector firms from 2008–18.

Source: author's own calculations.

Appendix D: Other tables and figures

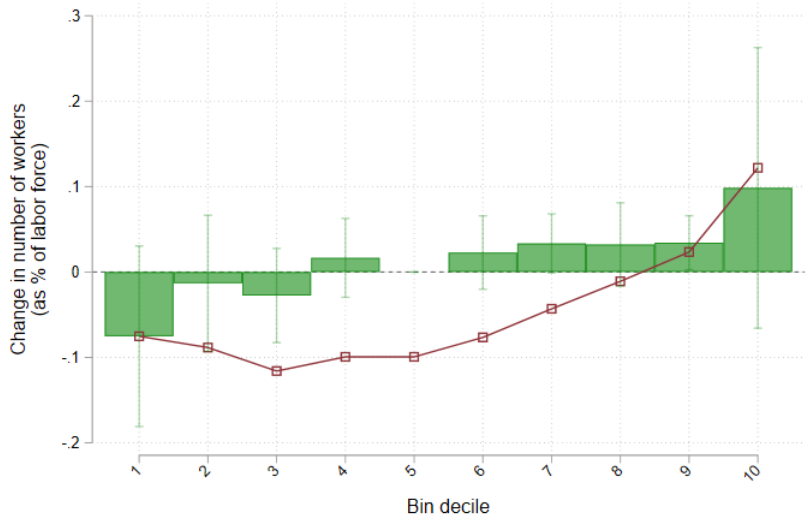
D1 Reallocation

Figure D1: Summary of reallocation effects by pre-event wage

Note: 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.

Source: author's own calculations.

Figure D2: Difference-in-differences histograms by bin of value added per worker



Note: 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 fifth decile). The main outcome is the count of workers in each firm.

Source: author's own calculations.

D2 Robustness

Table D1: Alternative specifications for bargaining council and spillover effects

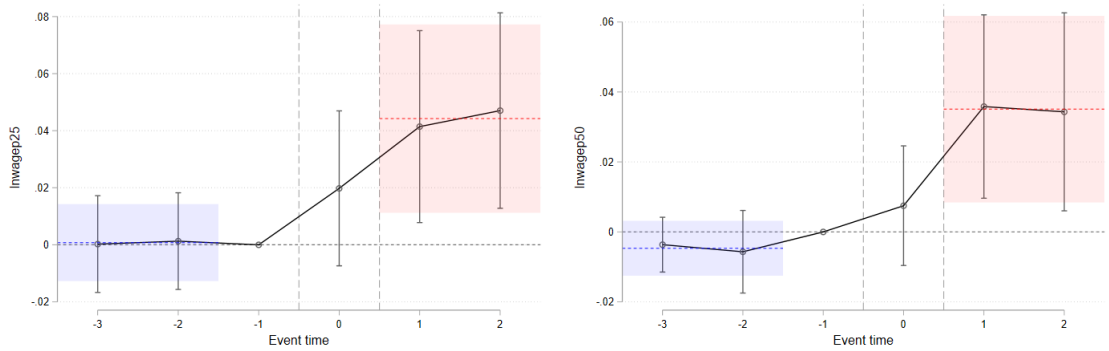
Outcome	(1) main	(2) pretrendFE	(3) nopreBC	(4) sizewgt	(5) pwgt	(6) nation1
lnwagep50	0.030	0.025	0.034	0.040	0.061	0.046
	(0.010)	(0.012)	(0.014)	(0.016)	(0.011)	(0.010)
lnfirmsize	–0.003	–0.003	–0.004	–0.050	–0.034	–0.011
	(0.009)	(0.010)	(0.012)	(0.058)	(0.010)	(0.010)
lnsep	–0.015	–0.015	–0.011	–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

Note: 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 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.

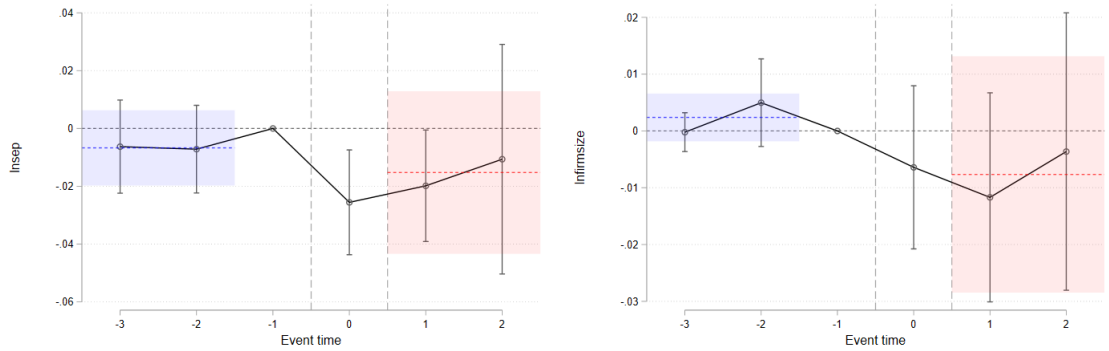
Source: author's own calculations.

Figure D3: No prior large contracted wage increase

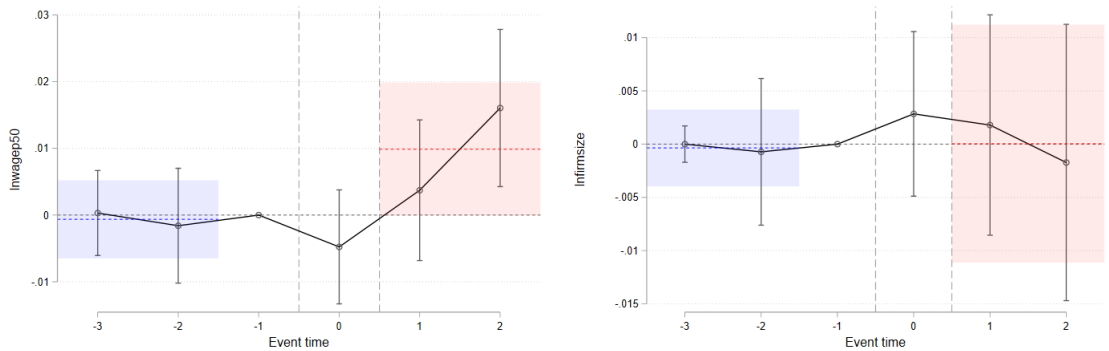
(a) Bargaining council firm wages: p25 and p50



(b) Bargaining council firm separations and size



(c) Spillover firm wages and size

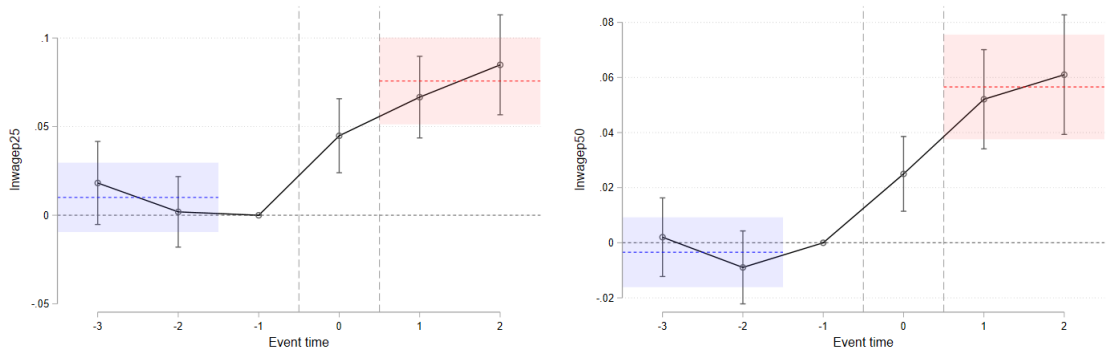


Note: the specification follows the primary specifications, except events with large minimum wage increases just before the pre-period window are excluded. This leaves about half the number of events.

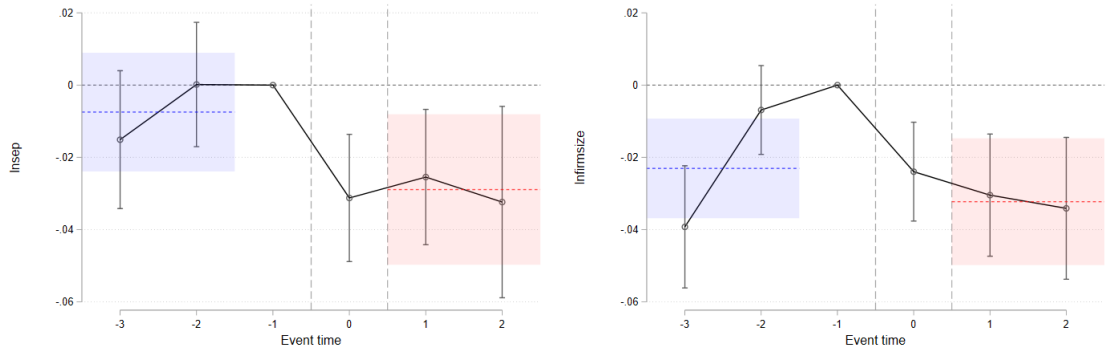
Source: author's own calculations.

Figure D4: Propensity score weighting

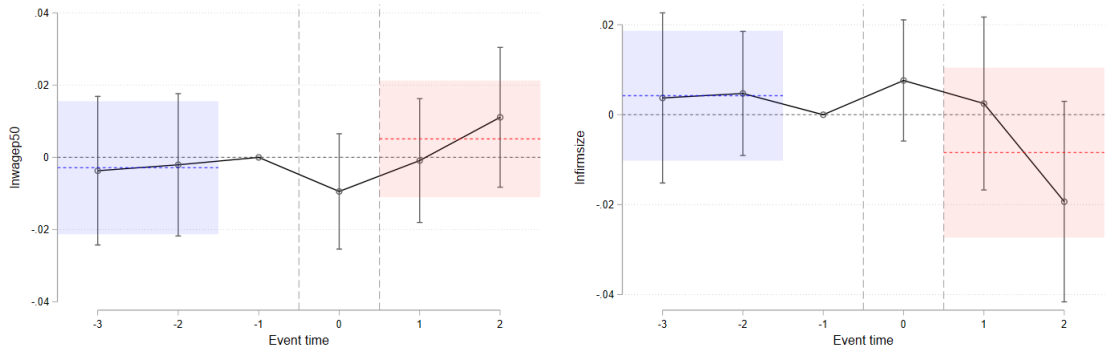
(a) Bargaining council firm wages



(b) Bargaining council firm separations and size



(c) Spillover firm wages and size



Note: 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.

Source: author's own calculations.

D3 Heterogeneity

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

Outcome	(1) ffeq0	(2) ffeq1	(3) kaitz0	(4) kaitz1	(5) lseq0	(6) lseq1	(7) fem0	(8) 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

Note: the following 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 (LSE) into bargaining councils with low firm labour supply elasticities (more monopsonistic market) and bargaining councils with high firm labour supply elasticities (more competitive market); columns 7 and 8 (fem) restrict the firm-level outcomes to male and female 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 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.

Source: author's own calculations.

Appendix E: Data construction

The SARS administrative tax data set provides a near-universe of formal sector individual labour market wage outcomes and firm balance sheet information. It is periodically updated, with the latest round of available years extending from financial year 2008 to 2018.[22] 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 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 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 data set 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.

E1 Compiling bargaining council agreements

The government gazette publishes bargaining council agreements, which may be found online at www.greengazette.co.za. Bargaining council contracts are generally set in three-year terms and pegged to inflation (plus a negotiated amount). By going through at least 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 Labour Research Service, and check each wage against the actual three-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.[23] For each bargaining contract, I select the SIC 5 classification code that best matches the wording in the contract. This may be at the three-, four-, or five-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

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

[23] 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 Bargaining Council (East London) and the Motor Ferry Bargaining Council were excluded because they overlap directly on industry-location with other bargaining councils (Building of Southern and Eastern Cape, and Motor, respectively). The Furniture Bargaining Council (South Western Districts) had no firm-level matches in the tax data. The public sector bargaining councils are the Public Service Coordinating Bargaining Council (PSCBC) and the South African Local Government Bargaining Council (SALGBC).

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 1,600 records of wages from different bargaining council clusters by industry and location.

Table E1 provides these details on industry and location for each bargaining council. The average wage for all bargaining councils is about ZAR4,500, 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 'labourer' (41 per cent of listed wages) or 'general worker' (30 per cent 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 straightforward. The minimum wages are highest in the New Tyre Manufacturing, Transnet, and Public Service Coordinating Bargaining Councils, and lowest in Hairdressing and Furniture Manufacturing Bargaining Councils for Kwa-Zulu Natal.

Table E1: Bargaining council industry and location

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

Note: bargaining council names are shortened for presentation. Province abbreviations are Eastern Cape (EC), KwaZulu-Natal (KZN), and Western Cape (WC). The SIC 5 code is an industry code following the SIC 5 classification system. Location indicates the geographic level of location assignment: national, provincial (9 in South Africa), or district council (52 in South Africa). Clusters refer to location-specific units within each bargaining council.
Source: author's own summary based on gazetted bargaining council documents published by the South African government.

E2 Matching bargaining council agreements to tax data

Table E2 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 aged 20–60 years old, restricting to one job per worker, and merging in firm-level variables (National Treasury and UNU-WIDER 2020b). 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. I also merge in data from the firm balance sheet side, using data from the same period (National Treasury and UNU-WIDER 2020a). Firm profit and turnover are slightly higher in 2009, but are otherwise stable. The firm exit rate (the firm is not observed in the next year) and outsource probability (defined when one-third or more than 500 workers switch to another firm at the same time) are similarly higher for the incomplete years.

Table E2: 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

Note: 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 ZAR36,000, 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 one-third of all workers or 500 workers in a firm switch to another firm. Note that the years 2008 and 2009 are incomplete.

Source: author's own summary based on SARS tax data panel.

There are a few caveats to the matching process. First, the industry variable is crucial and I follow the best practice as laid out in Budlender and Ebrahim (2020). Within this industry code, I select the SIC 5 industry classification system. Second, 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 councils, 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.

Third, 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 1,595 total potential records (11 years by 145 bargaining council clusters), I have matched 90 per cent with wage records. In terms of number of workers over the entire panel, about 30 per cent are presumed to be covered by bargaining councils and 70 per cent uncovered. This is in line with the estimate in Budlender and Sadeck (2007) using survey data.

E3 Matching bargaining council agreements to tax data

What is the quality of the final matched data? One indication is the strong 'first stage' estimated in Section 3. Wages jump sharply as expected when there are large increases in contracted bargaining council minimum wages. While this does not preclude error—that is, 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 2 describes the matched data in further detail, comparing bargaining council firms with other formal sector firms in the economy.

In Table E3 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 ZAR30,000 per year or ZAR2,500 per month (2016 inflation-adjusted). Incidentally, this is far below the 2019 national minimum wage of ZAR3,500. There are also higher minimums, such as in the Tyre Bargaining Council (ZAR120,000 per year or ZAR10,000 per month) and MEIBC (ZAR76,000 per year or ZAR6,300 per month). It is worth noting the low proportion of women in bargaining councils generally, with an average of 30 per cent compared to over 50 per cent for other firms. Indeed, the large bargaining councils listed above all comprise less than one-quarter women, except for Chemical. Lastly, regarding labour market parameters, the firm-level rent-sharing is generally *lower* than in other firms, perhaps because wages are set more sectorally, through some industries have high rent-sharing elasticities (such the Tyre Bargaining Council). The firm labour supply elasticity is closer to the average, though again with substantial variation.

Using Table E3, 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 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 = 0.00).

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 E3: 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.)	Labour 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	0.26	0.67
Ave. private BC	3,062,582	67,377	45	2.6	291,245	492,254	84,537	56,253	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	51,083	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	71,281	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	63,630	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	47,994	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	33,564	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	44,034	37	35	23	0.10	–0.07	0.14	0.80
Fishing	37,346	535	70	2.9	276,013	446,932	73,713	38,427	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	33,990	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	26,237	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	28,754	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	26,952	37	17	26	–0.04	–0.25	0.17	1.03
Hairdressing	8,638	959	9	2.0	115,545	242,726	55,626	31,501	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	42,600	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	52,262	36	44	64	–0.32	–0.56	0.30	0.89
Leather	25,556	708	36	2.5	274,409	456,064	75,200	57,845	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	31,708	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	76,733	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	32,923	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	33,995	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	54,161	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	62,481	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	60,229	34	33	54	–0.07	–0.31	0.13	0.69
Tyre	15,942	231	69	2.6	231,778	415,325	89,347	120,743	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	60,519	37	38	32	–0.06	–0.40	0.20	0.78

Note: of the 38 non-government bargaining councils, 13 are not shown due to poor matching in the tax data or no associated wage event: Amanzi (water); Building (except for the Cape); Canvas Goods; Diamond Cutting; Restaurant, Catering and Allied Trades; Furniture in the Eastern Cape and South Western Districts; Grain; Motor Ferry; Sugar Manufacturing. KZN refers to KwaZulu-Natal province and WC refers to Western Cape province. In the columns, inequality refers to mean within-firm inequality, min. wage refers to the average bargaining council negotiated minimum wage over the period, churn refers to the sum of separations and hires as a proportion of firm size (subtracting the change in firm size), worker and firm FE are the average respective components from an AKM regression, and rent-sharing and labour supply elasticities are estimated across all firms within each bargaining council. The sample is all formal sector firms from 2008 to 2018 using the South African Revenue Service (SARS) tax data.

Source: author's own calculations.