

# High Unemployment Yet Few Small Firms: The Role of Centralized Bargaining in South Africa

Jeremy R. Magruder\*

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## Abstract

South Africa has very high unemployment, yet few adults work informally in small firms. One explanation is that large firms and unions can extend arbitration agreements to non-unionized firms, raising wages. While local labor market characteristics influence the location of these agreements, they are enforced in a spatially discontinuous way, allowing identification by spatial regression discontinuity. Centralized bargaining agreements are found to decrease employment by 8-14%, with losses concentrated among small firms. These effects are not explained by resettlement to uncovered areas, and are robust to a wide variety of forms for average spatial heterogeneity.

## 1 Introduction

Employment in large firms is both scarce and relatively highly paid in many developing countries. Without sufficient opportunities for employment in these large firms, most developing countries have developed massive, informal small-firms sectors<sup>1</sup>, which keeps their population employed (albeit at low wages). South Africa is different. While formal sector work remains highly regulated, highly remunerated and scarce, there are few small firm jobs of any sort, informal or otherwise. The outcome is an astronomical unemployment rate, where only 56% of prime-aged men and 40% of prime-aged women are actually working.

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<sup>1</sup>For an extended discussion of small firms in developing countries, see Liedholm and Mead (1987, 1999).

Understanding both why formal jobs are so scarce, and why so few adults create work for themselves despite the implausibility of finding a formal job are questions of fundamental policy importance to South Africa, and ones which can contribute to our knowledge of the underlying conditions which encourage (or discourage) a vibrant small firms sector.

One possible reason that unemployed adults do not create informal work for themselves is that South Africa succeeds in extending labor regulations even to very small firms, rendering them unprofitable. While there are many labor regulations in South Africa, one particular form of labor standards which has been implicated in the lack of small firms is the bargaining council system. In that system, employer organizations and unions may opt to participate in bargaining councils, which extend arbitration agreements beyond the firms and unions which make them to all workers in an industry in a given political demarcation, regardless of firm size or participation in the arbitration process. If large firms and unions agree to high standards with the explicit goal of reducing competition from small firms – potentially helping enforce these standards themselves – then this could inhibit the ability of adults to escape unemployment by starting a small firm.

Other potential motivations for South Africa's unemployment without entrepreneurship abound. For example, high wages and an extensive social safety net may increase the demand for leisure or render long periods of unemployed search more palatable. Entrepreneurial opportunities may be limited by low skill levels, liquidity constraints, and high crime, while high capital stocks may make large firms competitive with relatively low labor inputs. Identifying the employment effects of labor regulations, therefore, requires a careful analysis which would hold these conditions constant. This challenge, combined with a previous lack of available data on these agreements, has prevented clean estimates of the impact of bargaining council agreements on employment or industrial structure. As an additional challenge, these agreements are outcomes of a complex bargaining process between unions and firms with unclear and likely anti-competitive motives. Since centralized bargaining is not mandated, the firms which choose to pursue centralized bargaining may be those who

work in local labor markets where centralized bargaining would represent a large competitive advantage. Therefore, the identification assumptions of similarity in level, trend, or response to a bargaining council agreement which would be necessary for a simple OLS or difference-in-differences approach may not hold in practice.

However, local labor markets are spatially continuous across the intranational political boundaries in South Africa, while these agreements are enforced in a spatially discontinuous way. In fact, this context is near-ideal for spatial discontinuity, as agreements vary with space, across industries, and over time, creating enough policy groups to avoid the common error-component problems which have plagued existing labor regulation studies. This paper creates a database of bargaining council agreements and adopts the spatial fixed effects proposed in Conley and Udry (2008) and Goldstein and Udry (2008) to identify this discontinuity. This spatial discontinuity reveals large effects: industries which have an agreement in a particular town in a given year have about 8-14% lower employment and 10-21% higher wages than the same industry in uncovered neighboring towns. Firm sizes are also impacted, with 7-15% fewer employees in small firms and 7-12% fewer entrepreneurs, while there are smaller and insignificant effects on large firms and single employee firms. Utilizing town-year and town-industry fixed effects, I show that these spatial discontinuities are similar in magnitude and precision whether only inter-industry variation (within a town-year) or intertemporal variation (within a town-industry) is utilized, and that estimates increase in magnitude and precision if we consider only small towns who should be unable to endogenously influence these agreements. I further illustrate that, while both large and small firms move across borders in order to avoid these agreements, this border-jumping does not drive the employment effects measured here, so that these reductions in employment represent a net loss for the economy. Finally, I introduce weighted spatial fixed effects to test the robustness of estimates to a wide variety of potential spatial heterogeneities, and find that estimates are quite robust.

## 2 South Africa's Missing Small Firms

Unemployment in South Africa is extremely high, particularly among non-whites. The first two columns of Table 1 report data from the 2003 Labour force survey (described below), which indicates that only about 56% of 20-60 year old men and 40% of 20-60 year old women are actually working (the corresponding employment rates are 50% and 35% if we restrict our attention to the majority black population). These numbers correspond to a 34% unemployment rate in this prime-aged population (where unemployment is defined as wanting work)<sup>2</sup>. A large number of potential reasons for this unemployment exist, and the unemployment numbers and potential contributors for them are surveyed more extensively in a series of papers by Kingdon and Knight (e.g. 2004, 2006, 2008) as well as Banerjee et al (2008). Wages are high, due to high capital/labor ratios, a strong union presence, and extensive governmental labor market regulation in addition to the industrial bargaining agreements which are the focus of this paper (e.g. Butcher and Rouse 2000, Schultz and Mwabu 1998). Second, entrepreneurial skills may be absent in the population, as informal employment was squashed under Apartheid (e.g. Kingdon and Knight 2004). Third, some unemployment may be voluntary; a generous non-contributory pension program combined with the high wages earned by the employed leave many unemployed individuals with networks capable of supporting them (e.g. Bertrand, Mullainathan, and Miller 2003 for labor supply effects; Edmonds, Mammen, and Miller 2005 for network effects of pensions on living arrangements).

While it is clear that many adults are unemployed in South Africa, and there are a variety of potential motivations for this unemployment, it is unclear what adults are in fact doing. Labor force surveys in South Africa go to great lengths to measure any economic activity, identifying as workers individuals who engage in unpaid household work or tend household plots "even for only one hour" in the past week; this approach yields the low employment

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<sup>2</sup>Kingdon and Knight (2006) advocate this broad unemployment measure in this context, as local wages are more sensitive to that measure. Official unemployment numbers include a broader range of ages and held steady at 42% over the period of 2002-2004.

numbers described above. A very natural response to this unemployment would be for many to be either self-employed or working informally<sup>3</sup>. Yet row 2 of table 1 reveals that only 6-8 percent of prime age South Africans are self-employed<sup>4</sup>. These numbers are tiny compared to countries with similar levels of unemployment (e.g. Charmes 2000, Kingdon and Knight 2004). Moreover, what is perhaps most striking is that there are relatively few employees of small firms in general. The remaining rows of table 1 reports the percent of employees in each firm size category in South Africa. Particularly for men, we see very few workers in firms of fewer than 5 employees. For comparison purposes, I also include similar data from the 1995-96 Brazillian LSMS survey<sup>5</sup>. We see that, while unemployment is a great deal higher in South Africa, the distribution of firm sizes looks fairly similar – with one big exception. What is missing in South Africa, compared to Brazil, are the small firms with 2-4 employees.

Of the above explanations for high unemployment, one in particular which may suggest minimal small-scale employment in a high unemployment context is labor regulation. The South African labor market is highly regulated, with a variety of legislated labor standards as well as privately bargained arbitration decisions. Unlike in many other developing countries, South Africa is successful in enforcing labor and tax regulations on many small (and potentially informal) firms; an influential study found that the average business with fewer than 5 employees pays nearly R14000 (about \$2170) per employee in costs associated with tax and labor regulations<sup>6</sup>(SBP 2005). Moreover, unions and firms can extend labor standard arbitration to all workers in a given political district through bargaining councils. Small

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<sup>3</sup>This is particularly true as unemployment durations are very long, and there is some evidence that social connections may be important to find employment. Since jobs are scarce, job opportunities may be shared among very close relations (Magruder 2009, Seekings and Natrass 2005), leaving individuals with poor social connections with very limited opportunities to find work.

<sup>4</sup>Among the majority black population, the corresponding figure is about 6% for both men and women

<sup>5</sup>It is not common for household surveys in developing countries to ask respondents about the size of the firm they work for. Fortunately, the Brazillian LSMS is an exception. Brazil represents a particularly good comparison for South Africa as a country with a broadly similar income level and similarly extreme level of inequality.

<sup>6</sup>This estimate is the average over complying and non-complying firms. The greatest contributor to this estimated cost is VAT, though labor regulations are also important. Of course, small business respondents to this survey may overstate compliance.

businesses, in particular, have advocated aggressively against the extension of these labor standards; in 2005 South African President Thabo Mbeki announced that small businesses would be granted a blanket exemption from these bargaining council agreements within the year in his state of the union address (Mbeki 2005). However, under pressure from trade unions and employers organizations to the contrary, the government never enacted this blanket exemption (e.g. Cosatu Rejects 2005). The fact both that the government would consider a legal change to exempt small business and that it meant with strong opposition confirms the anecdotal and survey evidence that these regulations are enforceable.

The potential of labor regulations to affect employment has been extensively explored in economics and found generally mixed results; a summary of this literature through the late 1990s is available in Nickell and Layard (1999). Much of the recent literature (e.g. Bertrand and Kramarz 2002, Besley and Burgess 2004, Harrison and Scorce 2008) has adopted a difference-in-differences approach where a time series of data on the legislative environment in states is summarized by a before and after period. Difference in employment trends between "treatment" states which adopt a policy and "control" states which do not are then compared to estimate the effect of regulations on employment. A second approach is to utilize a spatial discontinuity (e.g. Holmes 1998; Dube, Lester, and Reich 2008), where neighboring counties or states are compared, under the assumption that geographically proximate counties share similar labor markets and incentives to form labor policy, but are differentially exposed. Many existent labor regulation studies use a variation which adopts some elements of each of these approaches (e.g. Card and Krueger 1994), so that changes in trends are compared across spatially proximate regions. The measure of each of these studies is how comparable of a control group can be developed without causing small sample problems; to determine which approach is best for South Africa will require a more careful description of the labor regulations to be studied.

### 3 Industrial Bargaining in South Africa

Unions in South Africa can bargain with employers in two primary ways. The 1995 Labor Relations Act codifies the right of employers to form employers organizations for their particular industry and region and bargain with unions centrally; the labor standards which result from this bargaining can then be applied to all employees working in the industry and region which the bargaining council presides over. That is, if employer organizations and unions decide to bargain centrally, than all employees – regardless of their union status – who work within that geographical region will work under the agreed-upon labor standards. Unions and employers may also choose to bargain unilaterally, resulting in plant level agreements(Bendix 2001). Both unilateral bargaining and centralized bargaining are observed in a wide variety of industries and areas in South Africa, so that different industries in the same location may be covered by different types of agreements, industries may be covered by unilateral agreements in some locations and centralized agreements in others, and industries in a particular location may be covered by centralized agreements in one year and not in another.

It is encoded in law that bargaining councils must be representative of firms and employee unions in their jurisdiction; however, the extent to which this law is enforced is unclear. The official wording is that councils must be "sufficiently" representative, leading to a great deal of bureaucratic discretion and contention (primarily from small employers) as to whether the agreements represent all interests (Bendix 2001). South Africa's political structure is that 354 magisterial districts (towns) are organized into one of 52 district councils; these in turn comprise 9 provinces. In principle, there is not a strict criteria over which groupings of magisterial districts can form a bargaining council; in practice, most bargaining councils represent collections of magisterial districts which map to political boundaries, either national, provincial, or at the district council level. In the model outlined below, I follow the empirical trend in presuming that other magisterial districts within the district council are the natural bargaining partners in determining whether to form a bargaining council

agreement, while empirical analysis will standardize bargaining council units to eliminate any potential endogeneity stemming from this choice.

Existing studies on the effects of arbitration on wages and unemployment in South Africa have imperfect information on the presence of bargaining council agreements and treat the endogeneity of union membership via industry and occupational fixed effects, which may be an imperfect control; these studies find that unions receive very high wage premia, particularly at the bottom of the income distribution (Schultz and Mwabu 1998), and that bargaining councils exhibit a smaller, though still present, wage premium (Butcher and Rouse 2001). However, since the right to bargain centrally is one which must be exercised voluntarily, we may be concerned that bargaining council agreements exist systematically in the industries, towns, and years in which local labor markets make them particularly profitable for the firms who pursue centralized bargaining.

Moll (1996) outlines a theoretical model discussing the implications of bargaining councils for large and small firms (and large firm incentives to form them). We may also imagine that large firm incentives depend on whether the large firm is unionized. Suppose that, in the absence of a bargaining council agreement, large unionized firms pay privately bargained wages ( $w^U$ ), while large non-unionized firms and small firms pay market wages ( $w^*$ ). Under a bargaining council agreement, all would pay the same bargaining council wage ( $w^{BC}$ ); following Moll (1996) in presuming that  $w^U > w^{BC} > w^*$ , it is clear that operating costs decrease for large unionized firms and increase for small firms and large non-unionized firms in the presence of a bargaining council agreement. As the supply curves for the three types of firms shift, equilibrium changes. If small firms have the lowest marginal products of labor (due to low capital stocks), we may imagine that their supply curve shifts in by the largest margin, resulting in an increase in the residual demand faced by the two types of large firms. Thus, large unionized firms benefit from less competition from small firms and lower wages, large non-unionized firms benefit from less competition from small firms but suffer from higher wages, and small firms lose by the greatest margins. The degree of these benefits,

and the degree to which small firms and large non-unionized firms are punished by the bargaining council agreement, are functions of local demand, local labor supply, production technologies at each firm size, and other local labor market characteristics, as the changes in the demand faced by each type of firm will depend on anything which influences local supply and demand curves. In a model developed in the appendix, I show more formally that large unionized firms will increase employment in response to a bargaining council agreement, while large non-unionized firms and small firms will decrease employment.

The differing profit incentives that employers face, outlined above, are clear. Therefore, the presence of a bargaining council agreement will clearly be related to some aggregation of the private incentives of the large firms who initiate centralized bargaining. However, unions could adopt a bargaining position which is more or less hostile to bargaining councils, so the decision to pursue centralized bargaining may depend on both firm and union incentives. Since most of South Africa's unions are aggregated into three large nationwide alliances who have centralized general policies towards bargaining councils (Bendix 2001), I model the union's role in bargaining as a cost  $C$  of adopting the bargaining council agreement; empirical analysis will be robust to any heterogeneity in this cost that is due to industry-specific local labor markets or town characteristics<sup>7</sup>.

Suppose that, in the absence of a bargaining council agreement, large unionized firms in town  $t$  earn profits  $\pi_t^U$ , and that large non-unionized firms earn  $\pi_t^*$ . Further suppose that all large firms each earn profits  $\pi_t^{BC}$  in the presence of a bargaining council agreement before paying cost  $C$  to the union, and that fraction  $\lambda_t$  of the total  $Q_t$  large firms in town  $t$  are unionized. A bargaining council agreement is a collective result of the preferences of large firms throughout a district council, thus, if town  $t$  belongs to district council  $DC$ , bargaining

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<sup>7</sup>Both COSATU's (South Africa's largest union alliance) official positions on bargaining council agreements and the discussion of commentators (e.g. Bendix 2001) suggest that unions have some support for these agreements due to the greater political support they receive from advocating for globally higher labor standards. We may also imagine that unions have varying incentives related to local labor market heterogeneity, for example, the amount of dues which can be received or local competition from uncovered workers. Empirical analysis will be robust to both of these possibilities.

council legislation is adopted if

$$\sum_{t \in DC} Q_t \pi_t^{BC} - C > \sum_{t \in DC} Q_t (\lambda_t \pi_t^U + (1 - \lambda_t) \pi_t^*) \quad (1)$$

Local labor demand, local labor supply, local production technologies, local unionization rates, and local product demand all determine the result of this relationship. We should expect to see bargaining councils in places where the majority of large firms would benefit from having a bargaining council – that is, places where prices would either increase a lot from a reduction in small firm production, where small firm production would decrease substantially from the presence of a bargaining council agreement, or where a large reduction in wages for unionized large firms may take place. In places where small firm production technologies are relatively inefficient, and large firms face little competition, the incentives to form a bargaining council agreement are weakened, while in places with a vibrant small firms sector, the incentives to enforce uniform wages may be high. Any econometric investigation into the effect of bargaining councils on employment and small firm employment would have to take these local labor market characteristics into account<sup>8</sup>.

## 4 Econometric Strategy

The focus of this paper will be on estimating bargaining council effects on employment, firm size, and wages. A linearized structural equation is given by

$$Y_{ity} = \alpha + \beta_1 BC_{ity} + \Gamma X_{ity} + \nu_{ity} + \xi_i + \delta_y + u_{ity} \quad (2)$$

where  $Y_{ity}$  may be employment, employment by firm size, or wages in industry  $i$  in town  $t$  during year  $y$ ;  $BC_{ity}$  denotes the presence of a bargaining council agreement, and

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<sup>8</sup>This suggests, for example, that the effect of treatment on the treated is likely different from the effect of treatment on the untreated, causing concerns over difference in difference estimates which do not control for spatial heterogeneity.

$X_{ity}$  are covariates including population. The model above suggests that the presence of a bargaining council agreement is related to many characteristics of local labor markets, including labor supply, small firm production technologies, etc. These are captured in  $\nu_{ity}$ , which is unobserved and may be correlated with  $BC_{ity}$  and other explanatory variables. As such, the presence of a bargaining council agreement is an endogenous variable, and OLS estimates of this equation are likely to be biased.

To solve this endogeneity problem, I note that the endogenous characteristics of local labor markets within a given industry are likely to be spatially continuous, so long as migration and trade are locally feasible. Formally, let  $R(t)$  denote the set of all towns within radius  $R$  of town  $t$ ,  $Z_{ity}$  be the vector  $[X_{ity}; BC_{ity}, \xi_i, \delta_y]$  and  $Z_{iR(t)y}$  denote the vector of  $Z_{it'y}$ ,  $\forall t' \in R(t)$ . Then, spatial continuity suggests that  $E[\nu_{ity}|Z_{iR(t)y}] = E[\nu_{it'y}|Z_{iR(t)y}]$  for  $R$  sufficiently small and  $t' \in R(t)$ . This assumption is identical to that made in standard regression discontinuity designs, suggesting a spatial regression discontinuity.

There is a long history of economists exploiting spatial discontinuities to identify the effects of regulations. Most papers have followed seminal work by Card and Krueger (1994) and Holmes (1998) in restricting the sample to individuals who reside near the border of the labor regulation's coverage, and presuming that endogenous labor market characteristics are similar among all individuals who live within some bandwidth of the border. While the spatial discontinuity in this approach is both intuitive and transparent, it does have two limitations. First, a border analysis not only compares individuals, towns, or counties to proximate ones, it compares all towns on one side of the boundary to all towns on the other (who may not be particularly proximate). While using border-region fixed effects can eliminate some of this heterogeneity, it remains an imperfect approach as it introduces a discontinuity into continuous space<sup>9</sup>. Second, restricting analysis to border regions removes the ability of observations which are more distant from the border to identify the effects

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<sup>9</sup>A different approach which solves this concern is presented in Dube, Lester, and Reich (2008). That study restricts the sample to border regions and uses contiguous county-pair fixed effects, a similar differencing approach to that used below. However, as that study also restricts the sample to border counties, it loses the capacity of non-border regions to improve the precision of covariate estimates, as discussed below.

of covariates with likely similar relationships across the full sample. If we are studying employment, the most important of these may be population, as larger cities mechanically employ more people.

This paper adopts the spatial fixed effects introduced in Conley and Udry (2008) and Goldstein and Udry (2008)<sup>10</sup>. The idea of this approach is identical to the standard fixed effects within estimator. For each observation, we can subtract off the mean of observations which are spatially proximate. Using spatial fixed effects, we have

$$\begin{aligned}
 Y_{ity} - \frac{1}{n_{R(t)}} \sum_{t' \in R(t)} Y_{it'y} &= \beta_1 \left( BC_{ity} - \frac{1}{n_{R(t)}} \sum_{t' \in R(t)} BC_{it'y} \right) \\
 &+ \Gamma \left( X_{ity} - \frac{1}{n_{R(t)}} \sum_{t' \in R(t)} X_{it'y} \right) + \nu_{ity} - \frac{1}{n_{R(t)}} \sum_{t' \in R(t)} \nu_{it'y} + \tilde{\delta}_t + \tilde{u}_{ity}
 \end{aligned} \tag{3}$$

where  $n_{R(t)}$  represents the number of towns in  $R(t)$  and  $\tilde{\delta}_t$  and  $\tilde{\varepsilon}_{it}$  similarly represent spatial deviations. By assumption,  $E[\nu_{it'y} | Z_{iR(t)y}] = E[\nu_{ity} | Z_{iR(t)y}]$  for  $t' \in R(t)$ . That is, this differencing removes the endogeneity contained in the  $\nu_{ity}$ . If we make an assumption of strict exogeneity similar to those used elsewhere in fixed effects, that is, that  $E[u_{ity} | Z_{iR(t)y}] = 0$ , then this within estimator will consistently estimate  $\beta_1$  and  $\Gamma$ .

This assumption and this identification are valid if the endogenous incentive to form bargaining councils, modeled above as a collection of characteristics of local labor markets, is spatially continuous (and hence contained in  $\nu_{ity}$ ). Identification is by spatial discontinuity: outcomes are compared only against those of proximate neighbors as are program status and covariates. This equation estimates whether, if your bargaining council status is greater than your neighbors' (i.e. you live on the bargaining council-side of a border), you have more employment than your neighbors. Interior magisterial districts for whom  $BC_{ity}$  is the same  $\forall t' \in R(t)$  have a spatial deviation of zero in the  $BC_{ity}$ , but still contribute to the estimation of employment effects of differences in population and time trends. The analogous

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<sup>10</sup>In both of these papers, these spatial fixed effects are used to control for unobserved soil quality variation which is presumed to be similar amongst nearby plots.

approach in conventional regression discontinuity is to allow a flexible relationship between the running variable and dependent variable and examining a discontinuous jump at the eligibility cutoff. The choice of radius  $R$  is somewhat ad hoc as are the equal weights on all nearby observations; robustness to alternate forms of spatial heterogeneity will be considered in the robustness section below.

## 4.1 Practical Concerns

Several concerns about the spatial discontinuity identification in this context demand further consideration. First, as mentioned above, bargaining council agreements usually apply to all magisterial districts which belong to a larger political entity, either a district council, province, or the entire nation. However, in a few cases individual magisterial districts are added or subtracted from these groups in the coverage of a bargaining council (usually either the biggest town or closest neighbors of an adjoining district council). Though these observations represent a small share of the data, we may still worry about the implications of these observations for analysis. If bargaining councils are choosing precisely where the boundary of coverage should stop, we may worry that the industry-specific labor markets are indeed discontinuous at those coverage differences in an important way. This problem is directly analogous to the problem of imperfect enforcement and incomplete take-up in more traditional regression discontinuity. Traditional regression discontinuity avoids the problem of endogenous take-up by instrumenting program receipt with program eligibility. I follow this approach in this paper: I describe a magisterial district-industry-year observation as eligible for the program if it belongs to a district council where at least one magisterial district has a bargaining council agreement in that industry-year and use that measure of eligibility as an instrument for program receipt. All first stages are strong; the minimum t-statistic of bargaining council eligibility on bargaining council status across specifications and samples is 8.6<sup>11</sup>.

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<sup>11</sup>This takes place in the specification of town-industry fixed effects and separate border effects. All other specifications have a t-statistic over 10. Coefficients of bargaining council status on bargaining council

Second, spatial fixed effects may imperfectly control for characteristics of local labor markets. This may be true because great circle distances are imperfect measures of true economic distance. For example, travel times may be large between two physically proximate towns due either to poor infrastructure or rugged terrain, or other labor regulations may vary discontinuously at political boundaries. Moreover, local labor markets may change over time in ways correlated with regulation (e.g. Dube, Lester, and Reich 2008). To deal with these concerns, I repeat all analysis with town-year and town-industry fixed effects in addition to spatial fixed effects. Town-year fixed effects compare across industries within the same town and year, and in doing so control for any ways in which the town is different from its spatially proximate neighbors that year, including ways in which that town may be politically valuable to unions, the possibility that local neighbors are in fact distinct labor markets due to geographical separation or legislation (presuming that all industries are similarly affected by the long travel times or other disruptions to spatial continuity), or the possibility of secular local time trends which affect all industries. Town-industry fixed effects examine how employment changes when bargaining council agreements are added or expire, and so control for any ways in which that industry’s local labor market differs from its spatially proximate neighbors which is constant over time<sup>12</sup>. These represent very different identification assumptions, and the similarity of results under these two approaches represents strong support for the causal channel.

Finally, two potential sources of non-independence among observations are well-known and relevant to this context. A first challenge to evaluating programs which are implemented at aggregate levels is that if individuals in a political district have correlated error terms, or there is autocorrelation in the error, then OLS produces inconsistent standard

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eligibility range from 0.74 to 1.01.

<sup>12</sup>Some care is required combining the spatial fixed effects with other fixed effects when other dimensions of the panel are not balanced. In this paper, this happens when using the wage, tenure, and DC ratio subsamples. In this case, the standard within estimator is biased, and so demeaning sequentially along spatial and other dimensions is not consistent. Unfortunately, a simple adjustment such as that in Davis (2002) is not possible, as the spatial fixed effects cannot be represented as a projection onto the column space of a number of dummy variables. As a result, I conduct these estimates using the full set of dummy variables for the additional fixed effects whenever the panel is unbalanced.

errors (Bertrand et al 2004). The standard solution is to cluster at the policy group level. Since bargaining councils vary on the district council-industry level<sup>13</sup> (there are 208 district council-industry groups and 52 district councils in the estimation sample), this context avoids the small group number concerns which have challenged some past studies of governmental policies (e.g. Donald and Lang 2007). Secondly, the error term may be spatially autocorrelated (Conley 1999). As identification rests on the difference between towns which are physically proximate and those which are in the same political district, it is desirable to construct standard errors which are robust to correlation amongst both groups. This paper allows observations to be related if either they are close spatially or in the same district council. This is the more computationally intensive procedure outlined in Cameron, Gelbach, and Miller (2006), and also a special case of the Conley (1999) spatial errors if "economic distance" is defined as equal amongst individuals who live either within a given physical distance or in the same District Council.

## 5 Data

Data are drawn primarily from two sources. The South African Labour Force Surveys are a nationally representative rotating panel conducted twice yearly from 2000 through the present, each iteration surveying around 70,000 people. I use the September surveys from 2000-2003. Unfortunately, the panel aspect has not been well-maintained, with household identifiers not remaining consistent from wave to wave. As such, I aggregate data to the magisterial district level and use it as a panel at that level. These data are not intended to be representative at the magisterial district level and are not publicly released at that level

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<sup>13</sup>Several of the bargaining councils extend agreements to entire provinces, while others operate only on the District Council level (and a small fraction operate for even smaller units). This makes it difficult to know, for certain, how to categorize observations (particularly for industries and towns which are not covered by a bargaining council agreement). The results presented here presume that, since some District Councils unilaterally receive bargaining councils, this is the true observation level (implicitly, this assumes that bargaining councils which exist react to considerations at the District Council level). An alternate assumption would be to assume that the observation unit is the province-industry level. Results which make this assumption and cluster simultaneously at the spatial-industry, province-industry, and town level are similar and available from the author.

to prevent mistaken inference (on, for example, the extent of the variation in employment in a particular town year to year). This concern, however, should not limit more robust econometric analysis, so long as the degree to which the data are not representative is unrelated to the variables of interest and local-level unobservable heterogeneity is properly controlled for. While magisterial district identifiers are not released, they can be inferred from personal identification codes. These identifiers remain unchanged since at least the 1997 October Household Survey, which published an association between number and local municipality names<sup>14</sup>. From this list, I determine the magisterial district of each sampling area, and determine the longitude and latitude for the population center of that magisterial district. The unit of analysis in this paper will thus be the magisterial district; since sampling weights are not designed to be representative at this level I do not use them. Therefore, I measure employment in a given industry in a given town as the number of people surveyed in that town who work in that industry<sup>15</sup>. We may be concerned that very large towns have different labor markets from their neighbors, and that we get little useful information out of small towns where relatively few individuals were surveyed. I exclude the top and bottom two percent of towns in terms of population from the analysis. Summary statistics of the variables which will be used are included in table 2.

The presence of bargaining council agreements in a given year is revealed by the South African Government Gazette, which publishes all agreements. A database compiled by the author reveals which industries in which magisterial districts were covered by an agreement in each year. This yields the outcome that 15 two-digit industries in South Africa are

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<sup>14</sup>Examining characteristics of magisterial districts between these two surveys reassures that the identifiers are in fact unchanged. A change in coding in 2004 limits the sample to 2000-2003.

<sup>15</sup>In a related point, it is not immediately obvious how to treat observations of 0 employment in some category in a particular town (of which there are many). On the one hand, these observations give useful and important variation – if bargaining councils are brutally effective, we may expect to see 0 small firm employees in a particular town-industry. On the other hand, when I (ultimately) take  $\log+1$  as a measure of employment, the log operator strongly emphasizes observations which are 0. This concern is lessened by the use of the simple count data rather than weighted counts – the difference between  $\log(1)$  and  $\log(301)$  is a lot more than the difference between  $\log(1)$  and  $\log(2)$ . Results which use the fraction of the population employed in that industry (available from the author) are similar in sign and in general more precisely estimated than the logged results presented here.

covered by bargaining council agreements for at least some of the sample period. Of these, 7 industries have cross-sectional variation in their coverage across the district councils of South Africa. In 2003, 22% of prime-age African and Coloured workers in South Africa work in two-digit industries where, in their magisterial district, some workers are covered by a bargaining council agreement<sup>16</sup>. Different industries have different minimum effective scales, limiting the potential for entrepreneurship in some industries. Table 3 reveals that 75% of the prime-age African and Coloured self-employed in South Africa work in two-digit industries which at least sometimes have bargaining councils – this suggests that these councils are being utilized more in industries where small scale firms are economically viable. In contrast, only about 43% of workers overall are working in these industries. Looking within industries which at least sometimes have a bargaining council, we see an even more interesting result. 48% of employees who work in one of these industries are covered by a bargaining council agreement. However, only 34% of self employed and 37% of small firm employees do, in contrast with 69% of large firm employees – that is, among industries which at least sometimes have bargaining council agreements, places with bargaining council agreements have limited small scale and self employment. The industries and the percentage of employment-weighted magisterial districts covered in years 2000 and 2003 are listed in table 4.

Industries are aggregated to the two-digit level. Bargaining councils sometimes define industries in a different way than the standardized coding used in the labour force survey, and so only include subsets of those two-digit industries (subsets which unfortunately do not always map to three-digit industries). This means that my measure of coverage includes individuals who are actually working in distinct, uncovered jobs as well as covered employees. In principle, this bias should result in conservative estimates due to measurement error, since the bargaining council agreements only cover a fraction of the workers in the two-digit industry. Two of the industries with variation end up in "other" categories. We might

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<sup>16</sup>The actual number of covered workers is probably lower, due to the aggregation at the 2 digit level. Aggregation challenges are addressed below.

worry that these categories are more heterogeneous than other two-digit designations, and that the bargaining councils represented (hairdressing, laundry services, and contract cleaning) represent a smaller fraction of the workers in the "other services" and "other business activities" industries. Additionally, a third industry (electrical manufacturing) is very small in scale (with only 25 small firm employees measured in South Africa across the 4 survey years considered here), and covered almost everywhere. I exclude these three industries in the analysis below, although similar analysis including these industries is available from the author.

## 6 Results

I present first results for which spatial heterogeneity has not been corrected for comparability with earlier studies. Specifically, I estimate

$$Y_{ity} = \alpha + \beta BC_{ity} + \Gamma X_{ity} + \xi_i + \delta_y + \varepsilon_{ity}$$

where  $\xi_i$  and  $\delta_y$  represent industry and year level fixed effects, respectively, and  $X_{ity}$  includes a quartic in log population.  $Y_{ity}$  variables include employment, large firm employment, self employment, and small firm employment (small firms are defined to be firms with fewer than 10 employees while large firms have more than 20), measured as the log of that variable plus 1. I perform two types of unadjusted estimations here: simple OLS and a difference in difference specification, which conditions on District Council-Industry fixed effects; results are reported in Panel A of table 5. All cells report the coefficient on bargaining council presence for a given sample and dependent variable. Across the board, the pooled OLS reveals that bargaining council agreements exist in towns that, if anything, have greater employment in that industry. Row 2 reports the difference-in-differences analysis, which conditions on district council-industry fixed effects (as the policy variable is determined at the district council level). Here, effects are negative and significant in both samples, re-

vealing that, as bargaining council agreements come into place, employment decreases by 7 %, as does small firm employment. Self employment decreases by about 6%. However, we may be concerned that bargaining council agreements are being adopted or eliminated endogenously in places with specific time trends in employment or small firm employment. In the analysis that follows, I utilize spatial fixed effects to control for this endogeneity and achieve identification through spatial discontinuity.

## 6.1 Employment Results

Column 1 of Table 6 reports the coefficients on the presence of a bargaining council agreement on employment from several spatially differenced estimations, where the estimation equation is the instrumental variables analogue of equation 3, and spatial deviations in bargaining council status are instrumented by spatial deviations in bargaining council eligibility. In all equations, the spatial fixed effect is taken at the 30-mile radius, so that each dependent and independent variable represents deviations of variables between the observation of interest and other observations in the same industry and within 30 miles, where distance is determined by the great circle method. All estimations are conditional on a quartic in log population and time fixed effects, and all errors are clustered among observations across all years of the same industry within 2 degrees of latitude or longitude, as well as among all industries, towns, and years in the same district council. Having a bargaining council agreement is not strongly associated with changes in log employment in the first row; once town-specific, town-year, or town-industry fixed effects are included, the estimate of the bargaining council effect becomes about 8-11%, and significantly different from zero. These coefficients are quite stable despite the very different identification assumptions: whether we look across industries at spatial deviations in employment, or across time within industries, we draw very similar inferences about the effects of bargaining councils<sup>17</sup>.

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<sup>17</sup>Very similar (and similarly precise) results are available if we use the fraction of the population employed in the industry instead of the logged specification.

## 6.2 Wage Results

Of course, the stated purpose of the bargaining council legislation is to improve working conditions rather than reduce employment. We can also ask if wages increase with bargaining council agreements. This analysis uses the subsample with at least one wage observation, which eliminates zero employment towns (and some with non-response to the wage question). One consequence of the smaller sample is that the 30 mile radius, in conjunction with various town-specific heterogeneity loses a lot of power; column 1 of table 7 indicates that we find a 10-21% effect on wages at this radius, though standard errors become large and the effect loses statistical significance as we consider town-year or town-industry fixed effects. Column 2 repeats the analysis with a wider 50 mile spatial radius for the spatial fixed effects; at this larger radius the town-year effects regain precision. Overall, industries represented by a bargaining council in a town have 21% higher mean wages than the same industry in neighboring towns, and 13% higher wages if we hold constant mean deviations across industries in that year. Since the wage data appear not to be sufficiently dense for a 30 mile radius with town-year heterogeneity, I report the following wage and worker characteristic regressions using the 50-mile spatial fixed effects (30 mile radii give similar, but sometimes less precise, point estimates and are available from the author). The motivation above suggested that small firms should see larger wage increases than large firms, as large firms often must pay union wages anyway. We can examine mean log wages for small firms (with fewer than 10 employees) and large firms (with more than 20) separately, in columns 3 and 4. Consistent with theory, wages in small firms are rising substantially, with (precisely measured) point estimates around 12-18%. In contrast, large firm wages are if anything decreasing in response to bargaining councils, consistent with the hypothesis that bargaining council wages are lower than privately bargained ones (though errors are too large to reject a null hypothesis of a zero effect). However, caution must be taken in interpreting wage estimates as a change in wages for individual workers, because the composition of employees is changing.

Table 8 tests the importance of worker composition effects. Column 1 reports wage estimates when we control for the fraction male, the average number of years of primary and secondary education, and a quadratic in average potential experience (age - education - six). We see that controlling for these observable characteristics attenuates the effect of bargaining councils on wages somewhat, with estimated effects dropping by 4 to 5 percentage points in each specification. Columns 2 through 4 looks at how each of these variables changes with bargaining council status, and we observe that the big difference is in the gender of employees. When a bargaining council is present in an industry, the fraction of the labor force which is male increases by 5 to 13 percentage points. Education and age of the labor force are not robustly associated with bargaining council status. The education result is consistent with other studies (e.g. Magruder 2009) which find that education is not a strong predictor of employment in South Africa. However, the age result is on surface somewhat surprising, as bargaining council agreements often include hiring and firing regulations as well as wage standards. In a high unemployment context, age is a poor proxy for tenure, which we might expect to increase in the presence of hiring and firing restrictions. We can directly investigate the effect of bargaining council agreements on tenure; this requires using the sub-sample which responded to the tenure question. In column 5, I report the effect of bargaining councils on mean log tenure at the plant. Mean tenures are increasing by about 18-19% in response to bargaining council regulations<sup>18</sup>.

### 6.3 Firm Size Results

Here, I divide firms into four groups: large firms, with at least 20 employees; small firms, with fewer than 10 employees; self-employment, and single-worker firms. Many self-employed

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<sup>18</sup>In the wage sample, we do not find precise estimates comparing within a town-industry over time, while in the tenure sample, we do not find precise estimates comparing within a town-year across industries. This may be a case of sticky wages, or constant turnover across industries within a labor market; however, it may also be a case of low power in these estimations, and we cannot rule out similarly-sized effects. Non-response in the tenure variable is not closely associated with non-response in the wage variable, and so the sample which has both of these is further reduced; examining the effects of bargaining councils on tenure and wages in this sub-sample produces similar, though sometimes noisier, estimates.

individuals thus are also represented in the small firms and the single-worker firms categories. From the model above, we expect the bargaining councils to have the largest effects on employment in small firms. In principle, bargaining councils should have an ambiguous effect on large firm employment, and the effect on self employment will depend on how many entrepreneurs run larger small firms and the enforcement capacity of the bargaining council. If most single-employee firms aspire to grow to multiple employees, or if single employees are themselves paid a wage, it may be that bargaining council legislation reduces employment in single-employee firms. However, since most single employee firms are owner-operated, it seems likely that single-employee firms are primarily impacted through these dynamic incentives, which may be weaker than the direct wage effects of the agreements. Therefore, we may anticipate smaller effects among single-employee firms.

Columns 2 through 5 of table 6 reports the result of this analysis for each of these dependent variables, where rows represent different fixed effects specifications (again, all specifications feature spatial fixed effects in addition to the other noted fixed effects). Here, effects for small firms and self-employment are larger and consistently reach standard statistical significance thresholds. Consistent with theory, bargaining councils reduce small firm employment substantially, with bargaining council employment being associated with a 7-15% decline. This effect remains very similar when we examine how spatial differences vary within industries in a town or town year and within an industry over time (just as in the overall employment effects). Self employment similarly declines by 7-15%. Large firm employment, in contrast, does not report a consistent effect. Coefficients are never significant and are always smaller than small firm employment estimates. Similarly, single-employee employment is not consistently related to bargaining council agreement status. This suggests that these bargaining councils are most effective against small firms, but not single-employee firms, as suggested by theory.

## 7 Robustness

Three challenges to the interpretation of this study are addressed here. First, though town-year and town-industry fixed effects eliminated the potential of a number of potential ways by which local labor markets could be spatially discontinuous, we may remain concerned that labor markets for a particular industry may vary discontinuously and change endogenously over time with bargaining council agreements. Second, similar statistical effects would be estimated if firms simply resettle on the opposite side of a border or if employment is across-the-board reduced by these bargaining council agreements, but these two regimes would have very different policy implications. Third, the spatial de-meaning used above makes the strong and perhaps unrealistic assumption that spatial heterogeneity is constant within 30-mile radius circles; these effects would be more credible if they were insensitive to a variety of potential spatial heterogeneities. This section examines these three possibilities.

### 7.1 Industry-specific, time-varying spatial discontinuities

If firms optimize their bargaining council status in each year, then any discontinuity which develops may result in discontinuous bargaining council coverage and challenge identification. However, equation 1 makes clear that the presence of a bargaining council is due to some collaboration of towns in the same political district. If local labor markets are relatively continuous, then nearby towns should have similar incentives to form a bargaining council and the spatial fixed effects approach solves the endogeneity problem. If they aren't, then it indicates that something about industry  $i$  in town  $t$  is different from industry  $i$  in neighboring towns. If town  $t$  has much lower employment than other towns in its District Council, then, as equation 1 makes explicit, town  $t$ 's preferences should not be strongly reflected in the presence or absence of a bargaining agreement. In particular, if town  $t$  is discontinuously different from its political neighbors in its incentive to form a bargaining council, than it will not be able to enact its optimal choice. As such, our concern for endogeneity is minimized.

However, if a dominate share of industry  $i$  is located in town  $t$ , then this concern may remain, and the presence of a bargaining council in town  $t$ 's district council may be a reflection of discontinuous labor market trends in town  $t$ . In table 9, I repeat all estimation with a sample of industries and towns where employment is no more than 20% of employment in that industry in that district council on average.

Here, despite the smaller sample, precision increases and point estimates rise. Among town-industry groups which are too small to independently effect bargaining council policy, we see employment fall by 11-14% relative to neighbors and other industries or other years within the town. Consistent with the idea that endogeneity is minimized in this subsample, results here line up precisely with theory, with the largest effects being on small firms, smaller and marginally significant effects on self employment, and consistently small and insignificant estimates on large firm and single-employee firm employment. An industry in a town which represents a small fraction of it's county's employment can expect to see a 10-12% decline in small firm employment, a 5-8% decline in self employment, and no change in it's large firm or single-employee firm employment relative to it's neighbors, and relative also to the variety of potential unobserved components at town, town-year, or town-industry levels. Also striking is how similar these numbers are to the difference-in-difference estimates presented in Panel B of table 5, which uses the same subsample, especially in contrast to making the same comparison with the full sample. In the subsample where we expect endogeneity to be minimized, the difference in difference estimator seems to perform almost exactly the same as the spatial regression discontinuity estimator.

## 7.2 Border jumping

The spatial fixed effects estimator has the limitation common to regression discontinuity studies that only local trends at the point of discontinuity are identified. In the case of employment, we may be concerned that firms could relocate to a town immediately on the opposing side of the border. That is, two possible regimes would result in similar

analysis: in the first, all towns with bargaining council agreements could be employing fewer people than their potential. This would indicate that, if we could control for all variables which determine employment except for the presence of a bargaining council agreement, the residual employment would appear as a spatial plateau. An alternate regime would be one where locally at the border, employment is depressed on the side with a bargaining council agreement and increased on the side without one, where one town's disadvantage is another's advantage. While both of these regimes indicate that firms prefer to operate outside of the bargaining council restrictions, the former is clearly a more important issue for policy. Figure 1<sup>19</sup> presents a graphical illustration of these two regimes. Here, locations on the positive side of the x-axis are presumed to have the bargaining council in place, while those on the negative side do not. If firms are merely jumping a border at  $x=0$ , then the bargaining council's effect on employment may look something like the dashed line, while if employment is being eliminated, the bargaining council's impact may more closely be represented by the solid line. Both approaches would yield similar spatial-fixed-effects estimates.

A direct test of the hypothesis of border jumping (similar to Holmes 1998) can be found by reexamining level effects, and asking whether log employment is different in a magisterial district if it is on the border of a bargaining council agreement than otherwise. That is, we can define  $Border_{k,it_y}^+$  to be equal to 1 if an industry-town-year observation is covered by a bargaining council agreement but is within  $k$  miles of another town in that industry and year which is uncovered by a bargaining council agreement and 0 otherwise. We can similarly define  $Border_{k,it_y}^-$  equal to 1 if the observation is uncovered but within  $k$  miles of a town which is covered by a bargaining council agreement in that industry and year. Then, we can determine if border regions are different from their counterparts in the same bargaining

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<sup>19</sup>This figure is similar to those presented in Holmes (1998).

council regime by regressing

$$Y_{ity} = \sum_k \beta_{1k} Border_{k,ity}^+ + \sum_k \beta_{2k} Border_{k,ity}^- + BC_{ity} + \gamma_t + \alpha_i + \varepsilon_{ity}$$

However, local labor markets in border regions may systematically differ from those interior regions, leading to faulty inference. Two controls are used here to control for spatial heterogeneity. First, the magisterial district-industry fixed effects used earlier were collinear with the spatial fixed effects and can still be used in this setting. These fixed effects identify the coefficients of interest off of only time variation, so they control for any local labor market characteristics which remain constant over time. Here, we can identify any border jumping successfully presuming only that changes in local labor market characteristics are summarized by changes in bargaining council status, and not changes in border status. Moreover, this analysis allows us to look simultaneously at border effects and at the effect of bargaining councils on average. Second, we could consider fixed effects at the District Council-Industry-Year level. As this is the level at which bargaining council decisions are made, anything about the local labor market which led to a bargaining council agreement existing or not in that year is controlled for, and border effects will be identified off of magisterial districts within district councils which are closer to the border than other magisterial districts within the same district council.

Table 10 reports the results of this analysis, which reveals that border jumping is taking place. When a bargaining council is formed near a given town but not including that town, that town sees a large increase in employment (column 1) and small firm employment (column 5). We similarly observe border jumping for large and small firms using only the spatial variation, which reveal that having a bargaining council in your district council-industry-year but being closer to the edge of the bargaining council regime is associated with some flight of large firms (column 4, row 1), and that not having a bargaining council, but being near towns that do, is associated with an increase in small firms (column 6, row 2) However, this

fact is unrelated to the bargaining council effect documented in this paper, as coefficients on bargaining council status are virtually unchanged by controlling for border status (columns 1, 3, and 5) for overall employment, large firms, and small firms. This is in part because border regions actually have more employment on the bargaining council side as well as the non-bargaining council side, and in part because there are some fairly complicated spatial dynamics, visible by comparing the effect of residing 30-50 miles out from the border to being within 30 miles of it. Regardless, we can conclude two things. First, border jumping is taking place, suggesting that firms do prefer to resettle outside of the bargaining council regime and offering supporting evidence that firms (and especially small firms) prefer to avoid bargaining council agreements. Second, this effect is not inflating our estimates of the employment implications of bargaining councils.

### 7.3 Average Spatial Heterogeneity

The spatial fixed effect estimator compares employment in a town to the average across all towns within some radius of it. However, while introspection may provide some guidance as to the proper choice of radius, any choice will remain somewhat ad hoc. Ultimately, any spatial discontinuity study will revolve around an assumption similar to the one set forth here, where endogeneity, represented by  $\nu_{ity}$  in equation 2, is assumed to be constant within some bandwidth. Finite data sets do require that spatial bandwidths are not arbitrarily small, and researchers face a trade-off between greater power through more observations per fixed effect, and greater specificity through increasingly local fixed effects. Therefore, a sensitivity analysis is desirable to see whether the identified results are sensitive to different assumptions on the form of the heterogeneity.

A pragmatic approach to identifying spatial discontinuities, then, would ask how estimates change as we change the weight which we put on observations which are at different distances in controlling for local heterogeneity. To formalize this, consider several sets  $R_g(t)$ , each of which contains towns within some radius  $R_g$  of town  $t$ , where  $g = 1, \dots, G$  and  $R_G(t)$

is the largest potential radius for the spatial similarity. One solution is to simply difference off a weighted mean

$$\begin{aligned}
Y_{ity} - \sum_g \frac{\rho_g}{n_{R_g(t)}} \sum_{t' \in R_g(t)} Y_{it'y} &= \beta Z_{ity} - \sum_g \frac{\rho_g}{n_{R_g(t)}} \sum_{t' \in R_g(t)} Z_{it'y} \\
&+ \nu_{ity} - \sum_g \frac{\rho_g}{n_{R_g(t)}} \sum_{t' \in R_g(t)} \nu_{it'y} + \tilde{\delta}_y + \tilde{u}_{ity}
\end{aligned} \tag{4}$$

If, in the true spatial process  $E[\nu_{ity}|Z_{iR_G(t)y}] = \sum_{g=1}^G \rho_g E[\nu_{it'y}|Z_{iR_G(t)y}]$  for  $t' \in R_g(t)$  and where  $\sum_g \rho_g = 1^{20}$ , then the conditional expectation of the  $\nu_{ity}$  is equal to the properly weighted conditional expectations of spatial heterogeneity in nearby observations, allowing consistent identification of  $\beta$ . Of course, we don't know the relative weights. However, for robustness analysis, we can choose a few rings, and solve this equation for a relatively fine grid of all possible weights over those rings. Then, we can infer whether coefficient estimates and statistical inference would be robust to a wide variety of assumptions on the spatial heterogeneity<sup>21</sup>.

## 7.4 Weighted Spatial Fixed Effects: Results

In what follows, I assume there are three different rings to the heterogeneity, relevant at 5, 30, and 50 miles from the source observation. I present figures depicting the coefficient estimate and t-statistics at every possible .01 weight for each of these rings for the speci-

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<sup>20</sup>This assumption can be justified if  $\nu_{ity} = \sum_{g=1}^G \nu_{ity}^g$ , that is, that the endogeneity takes the form of several spatial processes which are similar in expectation at different local radii, and where by assumption  $E[\nu_{ity}^g|Z_{iR_G(t)y}] = E[\nu_{it'y}^g|Z_{iR_G(t)y}] = \rho_g E[\nu_{it'y}|Z_{iR_G(t)y}]$  for all  $t' \in R_g(t)$ . Each component  $\nu_{ity}^g$ , then, will represent the part of the endogeneity which is similar among observations at radius  $g$ , and  $\rho_g$  is the relative weight of  $\nu_{ity}^g$  in  $\nu_{ity}$ . The conditional mean assumption suggests that while the overall disturbance  $\nu_{ity}$  may be related to estimation parameters, the fraction of the disturbance which is constant over a given radius is unrelated to the estimation parameters.

<sup>21</sup>An alternate approach would treat the  $\rho_g$  as parameters to be estimated, for example selecting the  $\rho_g$  which minimize the sum of squared error terms. In practice, putting the full weight on the most local ring always minimizes the sum of squared errors in this exercise. In the limit, this collapses to the town-industry fixed effects employed earlier, suggesting that we may prefer these estimates. In this paper, I take the robustness approach as the comparability between spatial and intertemporal estimates has been a strength of the analysis, and I illustrate that the effects highlighted here are robust to a wide variety of potential spatial weights.

fication of employment with town-year fixed effects (estimates on small firm employment, and those with or without the town-year fixed effects, are similarly robust in magnitude and significance<sup>22</sup>). I further restrict all weights to be weakly positive, and  $\rho_5 + \rho_{30} + \rho_{50} = 1$ . In each picture, the vertical (Y) axis represents the dependent variable, while the Z-axis represents the relative weight (out of 100) on the 5-mile fixed effect, and the X-axis represents the relative weight on the 30-mile fixed effect. Therefore, the evaluation at (0,0) represents the coefficient estimate or t-statistic resulting from a 50-mile spatial fixed effect, the evaluation at (100,0) represent the evaluation with a 5-mile fixed effect, the point (0,100) represents the evaluation with a 30-mile fixed effect, and interior points feature weighted fixed effects. Figures 2 and 3 show that point estimates are stable, large, and precise, regardless of the form of the spatial heterogeneity.

## 8 Conclusions

Bargaining council agreements are the outcome of a complex bargaining process. Their location is related to a variety of local labor market characteristics, which may lead to biases in OLS and difference-in-differences specifications. Under the assumption that labor markets are spatially continuous, this paper uses spatial fixed effects to determine that bargaining councils are associated with about 8-14% lower employment in a particular industry, 10-21% higher wages, and 7-15% less employment in small firms. We can control for potential heterogeneity by removing spatial deviations common to towns, town-years, or town-industries; whichever of these specifications are used result in similar (and similarly precise) point estimates. That is, an industry with a bargaining council has about 8-14% less employment than it's neighbors without a bargaining council. This is true if we compare it to how different industries in the same town compare to their neighbors, or if we compare how employment in that town and industry changes over time with bargaining council status.

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<sup>22</sup>Examining employment without town-year fixed effects, the effects at the 30 and 50 mile radiuses are imprecisely estimated, just as in table 6. As we approach more local fixed effects, point estimates become bigger and significant, just as they did with a town-industry fixed effect in table 6.

Industry-town observations which employ a relatively small fraction of the employees in their District Council experience the most severe consequences; that is, towns whose voices should receive little weight in the decision to form a bargaining council are the most severely impacted by its existence. The identification assumptions of spatial continuity can be weaker for these towns – if they differ substantially from their neighbors in their incentives to form bargaining councils they will be unable to implement their desired bargaining council status and so these estimates are particularly compelling. Moreover, while both small and large firms appear also to prefer avoiding these restrictions, and hence resettle on the opposite side of the border, this effect is unrelated to the estimated employment effect of bargaining councils.

Eight to fourteen percent is a large decrease in employment in a given industry. By means of comparison, Bertrand and Kramarz (2002) estimate that French restrictions on new retail firms led to a 3% decrease in retail employment, Besley and Burgess (2004) estimate that labor regulation reduced manufacturing employment in India by 7%, and Harrison and Scorse (2008) find that a 50% increase in the Indonesian minimum wage is associated with a 6% employment reduction. The bottom end of the point estimates, then, is as large as these effects of labor regulation found in other contexts. However, bargaining councils cannot explain all of the unemployment problem in South Africa. 22% of employees work in two-digit industries in places with bargaining council coverage, which corresponds to about 11% of the prime-age population. If each of these two-digit industries were to increase employment by 8-14%, it would cause a 0.88-1.54 percentage point total increase in employment. These effects are large and should be of interest to policy makers. However, the South African unemployment situation is severe enough that a one and a half percentage point increase in employment would leave South Africa with a severe unemployment problem. So while the unemployment effects of these policies are as big or bigger than other estimated labor regulation effects, other problems still contribute to such high unemployment in South Africa. Spatially continuous aspects of union behavior, labor market policies other than

bargaining council agreements, and the other voluntary and structural stories which may lead to high unemployment levels may play an important role. Similarly, the larger small firm effects is 4-6 times larger than the difference-in-differences effect of French entry regulations (Bertrand and Kramarz 2002). This policy is thus having its intended effect of restricting small firm profitability, in a context where the small firms sector was already anemic. Once again, however, the small firms sector in South Africa is so minimal that this 7-15% increase in these industries would leave small firm employment substantially below global norms. Further research remains important to learn about the other potential contributors to this problem.

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Table 1: Percentages of Employees in Firm Size Categories

	South Africa		Brazil	
	Male	Female	Male	Female
Percent Working	56.17	39.66	83.71	49.70
Percent Self-Employed	7.87	5.98	27.49	24.31
Number of Employees				
1	11.01	29.93	17.39	31.06
2-4	13.46	12.78	29.57	24.98
5-9	11.35	9.52	8.72	6.63
10-19	14.06	12.64	7.08	5.53
20-49	17.30	15.25	6.82	7.02
50 +	32.82	19.88	30.43	24.78

Notes

- 1 South African Data are from the September 2003 labour force survey, while Brazillian data are from the 1995-95 LSMS survey.
- 2 Statistics are for 20-60 year old adults.

Table 2: Summary Statistics

	mean	sd	min	max	N
Log Employment	0.966	1.015	0.000	4.564	4984
Log Large Firm Employment	0.333	0.601	0.000	3.664	4984
Log Small Firm Employment	0.736	0.895	0.000	4.127	4984
Log Self Employment	0.502	0.765	0.000	3.932	4984
Log Single Firm Employment	0.346	0.651	0.000	3.829	4984
Log Population	4.825	0.974	2.773	6.868	4984
Bargaining Council	0.352	0.478	0.000	1.000	4984
Mean log wage	5.220	0.787	1.569	9.798	2698
Large Firm log wage	5.724	0.670	3.178	8.476	1256
Small Firm log wage	5.029	0.832	1.569	9.798	2232
Fraction Male	0.548	0.371	0.000	1.000	2698
Potential Experience	23.925	9.218	0.000	66.000	2698
Worker Education	8.510	2.699	0.000	12.000	2698
Mean Log Tenure	1.194	0.772	0.000	3.761	2505

Table 3: Firm Size by Bargaining Council Status

Firm Size	Among All Industries		Among Bargaining Council Industries	
	Never a BC	BC Industry	BC absent	BC present
All	56.67	43.33	51.8	48.2
Self Employed	24.67	75.33	65.94	34.06
Small Firms	54.53	45.47	62.65	37.35
Large Firms	59.02	40.98	30.88	69.12

Notes

- 1 Summary statistics of adults aged 20-60 in September 2003
- 2 Bargaining Council Industries are covered by a Bargaining Council Agreement at least sometimes

Table 4: Bargaining Council Coverage

Industry	Fraction of workers	Fraction of magisterial districts covered	
		2000	2003
		Fishing	0.001
Textile Manufacturing	0.020	1	1
Metal Product Manf	0.015	1	1
Electrical Machinery Manf	0.003	1	0.91
Transport Equip Manf	0.005	1	1
Furniture Manf	0.011	0.831	0.827
Construction	0.101	0.343	0.204
Retail Trade	0.256	0.156	0.217
Hotels and Restaurants	0.058	0.336	0.266
Land Transport	0.019	1	1
Air Transport	0.001	1	0
Other Business Acts	0.053	0.22	0.191
Public Service	0.030	1	1
Recreational/Cultural Act	0.005	1	1
Other Service Activities	0.022	0.445	0.391

Notes

- 1 Fraction of workers is the average, 2000-2003
- 2 Fraction of magisterial districts is weighted by the working population in that industry in each magisterial district

Table 5: No Spatial Fixed Effects: OLS and Difference-in-Differences

	(1)	(2)	(3)	(4)
Dependent Variable	Employment	Large Firm Employment	Small Firm Employment	Self Employment
Fixed Effects				
		Panel A: Full Sample		
None	0.087 (0.088)	0.106 (0.074)	-0.015 (0.062)	-0.044 (0.062)
DC-Industry	-0.071** (0.031)	-0.047 (0.037)	-0.066** (0.032)	-0.062* (0.033)
N	4984	4984	4984	4984
		Panel B: DC-Ratio Sample		
None	0.060 (0.077)	0.068 (0.058)	-0.026 (0.054)	-0.057 (0.055)
DC-Industry	-0.137*** (0.048)	-0.017 (0.027)	-0.122*** (0.036)	-0.053 (0.034)
N	3551	3551	3551	3551

Notes

- 1 Presents IV estimates of the effect of bargaining council status on several specifications.
- 2 Columns Indicates the dependent variable, while rows indicate the sample and specification used. DC-Industry fixed effects represent a difference-in-differences specification
- 3 Bargaining Council (BC) status is instrumented with BC eligibility; a magisterial district-industry is BC-eligible if at least one magisterial district in the same district council has a BC in that industry.
- 4 Errors are clustered at the District Council Level
- 5 The DC-Ratio Sample is restricted to town-industry observations which represent less than 20% of the employment in that industry in that District Council on average

Table 6: Bargaining Council Effects on Employment

Dependent Variable	(1) Employment	(2) Large Firm Employment	(3) Small Firm Employment	(4) Self Employment	(5) Single Firm Employment
Fixed Effects Level					
None	-0.044 (0.039)	0.018 (0.046)	-0.103*** (0.037)	-0.104** (0.044)	-0.058 (0.042)
Town	-0.083** (0.035)	-0.052 (0.041)	-0.113*** (0.029)	-0.118*** (0.039)	-0.047 (0.036)
Town-Year	-0.106** (0.044)	-0.040 (0.044)	-0.150*** (0.030)	-0.149*** (0.052)	-0.091** (0.041)
Town-Industry	-0.076** (0.032)	-0.052 (0.038)	-0.070** (0.033)	-0.067** (0.034)	-0.017 (0.022)
N	4984	4984	4984	4984	4984

Notes

- 1 Presents IV estimates of the effect of bargaining council status on several specifications.
- 2 Each Column Indicates the dependent variable, while rows indicate the level of fixed effects used. Employment numbers are measured as  $\log(\text{Employment}+1)$
- 3 Bargaining Council (BC) status is instrumented with BC eligibilty; a magisterial district-industry is BC-eligible if at least one magisterial district in the same district council has a BC in that industry.
- 4 All errors are clustered within the industry over space and time and among all industries, towns, and years in a given District Council.

Table 7: Wage Effects of Bargaining Councils

Dependent Variable	(1) Wage	(2) Wage	(3) Small Firm Wage	(4) Large Firm Wage
Fixed Effects Level				
None	0.195** (0.086)	0.208*** (0.075)	0.183** (0.090)	0.136 (0.111)
Town	0.120** (0.053)	0.172*** (0.046)	0.135** (0.063)	-0.058 (0.120)
Town-Year	0.074 (0.101)	0.126** (0.055)	0.177** (0.073)	-0.055 (0.144)
Town-Industry	0.091 (0.059)		0.115 (0.114)	-0.069 (0.064)
Radius	30	50	50	50
Sample	Wage	Wage	Small Wage	Large Wage
N	2698	2698	2232	1256

Notes

- 1 Presents coefficients of Bargaining Councils on mean log wages, and mean log wages in small or large firms.
- 2 Rows consider different fixed effects.
- 3 Bargaining Council (BC) status is instrumented with BC eligibility; a magisterial district-industry is BC-eligible if at least one magisterial district in the same district council has a BC in that industry.
- 4 Results are conditional on spatial-industry (with a radius given in the radius row) and time fixed effects, and a quartic in log population.
- 5 All errors are clustered within the industry over space and time and among all industries, towns, and years in a given district council.

Table 8: Wage Effects: Employee Composition Controls

	(1)	(2)	(3)	(4)	(5)
Dependent Variable	Wage	Male	Education	Age	Tenure
Fixed Effects Level					
None	0.160** (0.062)	0.082*** (0.017)	-0.111 (0.205)	-0.404 (0.924)	0.190*** (0.051)
Town	0.138*** (0.022)	0.095*** (0.023)	-0.260* (0.156)	-1.151 (1.040)	0.193** (0.080)
Town-Year	0.076** (0.032)	0.131*** (0.037)	-0.221 (0.200)	-1.486 (1.262)	0.117 (0.071)
Town-Indus	0.080 (0.052)	0.041** (0.019)	-0.283*** (0.096)	-0.784 (0.852)	0.181** (0.075)
Radius	50	50	50	50	50
Sample	wage	wage	wage	wage	tenure
N	2698	2698	2698	2698	2505

Notes

- 1 Presents IV coefficients of Bargaining Councils on mean log wages, the fraction male, mean education, age, and log tenure, with analysis restricted to observations with at least one wage (wage sample) or tenure (tenure sample) observation.
- 2 Rows consider different fixed effects.
- 3 Bargaining Council (BC) status is instrumented with BC eligibility; a magisterial district-industry is BC-eligible if at least one magisterial district in the same district council has a BC in that industry.
- 4 All specifications are conditional on 50-mile spatial-industry and time fixed effects, a quartic in log population, and worker composition variables.
- 5 All errors are clustered within the industry over space and time and among all industries, towns, and years in a given district council.

Table 9: Bargaining Council Effects by Firm Size: DC Ratio Sample

Dependent Variable	(1) Employment	(2) Large Firm Employment	(3) Small Firm Employment	(4) Self Employment	(5) Single Firm Employment
Fixed Effects Level					
None	-0.116* (0.069)	0.011 (0.030)	-0.118** (0.053)	-0.050 (0.049)	-0.045 (0.046)
Town	-0.145*** (0.048)	-0.030 (0.028)	-0.119*** (0.044)	-0.069* (0.041)	-0.031 (0.040)
Town-Year	-0.143** (0.060)	-0.016 (0.039)	-0.124** (0.055)	-0.081 (0.059)	-0.046 (0.055)
Town-Industry	-0.114*** (0.042)	-0.015 (0.022)	-0.101*** (0.031)	-0.045 (0.030)	-0.019 (0.018)
N	3551	3551	3551	3551	3551

Notes

- 1 Presents IV estimates of the effect of bargaining council status on several specifications.
- 2 Each Column Indicates the dependent variable, while rows indicate the level of fixed effects used. Employment numbers are measured as  $\log(\text{Employment}+1)$ .
- 3 Bargaining Council (BC) status is instrumented with BC eligibility; a magisterial district-industry is BC-eligible if at least one magisterial district in the same district council has a BC in that industry.
- 4 All errors are clustered within the industry over space and time and among all industries, towns, and years in a given District Council
- 5 Sample is restricted to town-industry observations which represent less than 20% of the employment in that industry in that District Council on average

Table 10: Border Jumping

Dependent Variable	Employment		Large Firm Employment		Small Firm Employment	
	(1)	(2)	(3)	(4)	(5)	(6)
Bargaining Council side, 30 mile border	0.045** (0.021)	-0.097 (0.173)	0.028 (0.118)	-0.203* (0.112)	-0.034 (0.085)	-0.107 (0.152)
Not Bargaining Council side, 30 Mile Border	0.368** (0.151)	0.134 (0.089)	0.084 (0.084)	-0.027 (0.100)	0.478** (0.219)	0.253** (0.113)
Bargaining Council side, 50 mile border	0.109 (0.100)	0.121 (0.219)	-0.079 (0.134)	0.051 (0.134)	0.125* (0.068)	0.069 (0.198)
Not Bargaining Council side, 50 Mile Border	-0.149 (0.142)	0.048 (0.052)	0.073 (0.082)	0.057 (0.040)	-0.277* (0.155)	-0.039 (0.049)
Bargaining Council Agreement	-0.119** (0.048)		-0.001 (0.030)		-0.113** (0.053)	
Fixed Effects	Town- Indus	DC-Ind- Year	Town- Indus	DC-Ind- Year	Town- Indus	DC-Ind- Year
N	4984	4984	4984	4984	4984	4984

Notes

- 1 Presents IV estimates of employment or employment by firm size on bargaining council status, as well as being within 30 or 50 miles of the regime border, where the effect of being on a border is allowed to be asymmetric by which side of the border a town is on.
- 2 All results are conditional on time fixed effects and a quartic in log population
- 3 All errors are clustered within the industry over space and time and among all industries, towns, and years in a given district council.
- 4 DC-Ind-Year fixed effects are at the District Council-Industry-Year level
- 5 Bargaining Council (BC) and border status are instrumented with BC and border eligibility; a magisterial district-industry is eligible if at least one magisterial district in the same district council has a BC in that industry. It is border-eligible if it is within  $k$  miles of a magisterial district that is BC-eligible

Figure 1: Employment shortage vs. border jumping

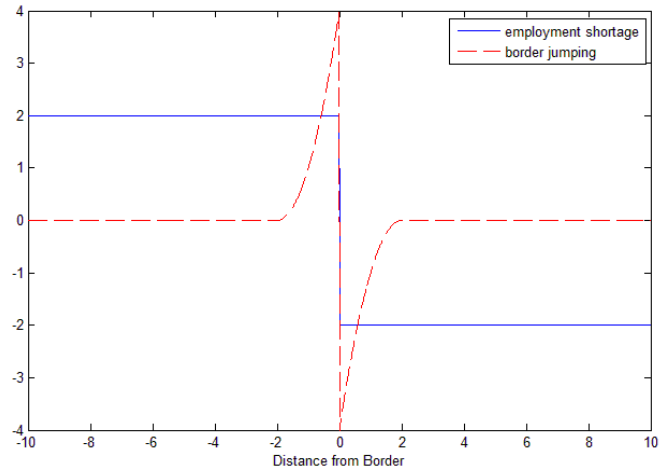


Figure 2: Employment Coefficients: Town-Year Fixed Effects

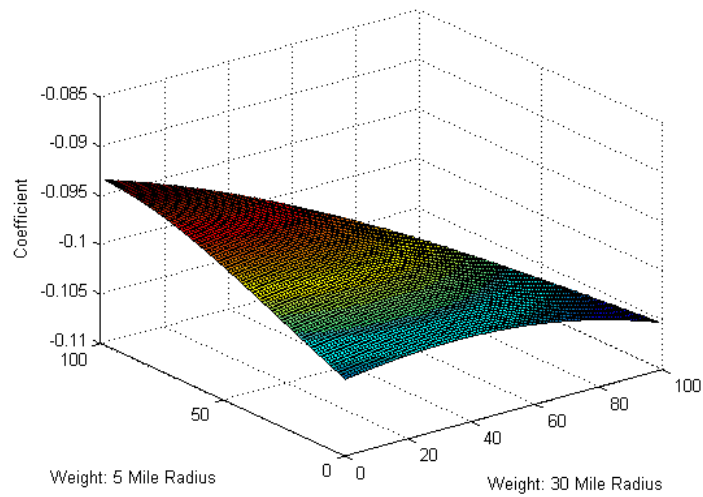


Figure 3: Employment t-statistics: Town-Year Fixed Effects

