

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

Jeremy R. Magruder\*

November 11, 2011

## Abstract

South Africa has very high unemployment, yet few adults work informally in small firms. This paper tests whether centralized bargaining, by which unionized large firms extend arbitration agreements to non-unionized smaller firms, contributes to this problem. While local labor market characteristics influence the location of these agreements, their coverage is spatially discontinuous, allowing identification by spatial regression discontinuity. Centralized bargaining agreements are found to decrease employment in an industry by 8-13%, with losses concentrated among small firms. These effects are not explained by resettlement to uncovered areas, and are robust to a wide variety of controls for unobserved heterogeneity.

---

\*University of California, Berkeley. I thank Guojun He and Charles Seguin for excellent research assistance and John Bellows for alerting me to some of the data used in this study. I also thank Michael Anderson, Lori Beaman, Tim Conley, Ann Harrison, Elisabeth Sadoulet, Paul Schultz, Chris Udry and seminar audiences at Berkeley, USC, UCSD, the World Bank, the University of Stellenbosch, the University of Cape Town, The University of the Witwatersrand, the University of Pretoria, Yale, and Northwestern for many helpful comments, and also Economic Research Southern Africa for organizing a workshop and facilitating seminars in South Africa. All remaining mistakes are naturally my own.

# 1 Introduction

Wage-setting institutions, including collective bargaining and labor legislation, are politically charged issues for countries at all stages of development. Concern over the first-order theoretical implications of these labor market distortions – that mandating improved working conditions should induce lower employment – has been somewhat attenuated by the vast empirical literature which has examined these issues. The large majority of these studies have taken place in OECD countries, and are characterized by a finding of small employment effects (Blau and Kahn 1999; for a survey of evidence from developing countries see Freeman 2009). It seems likely that the labor market response to wage-setting institutions changes as countries develop. After all, for wage-employment tradeoffs to be economically important, two structural features must hold: first, there must be substantial labor supply at low wages (below the cut of the proposed standards), and second, the government must have the capacity to actually enforce these regulations. We might expect labor markets in middle income countries to exhibit these two features, and particularly so in countries with high rates of inequality. There, a large fraction of the labor force experiences living standards similar to those in much poorer countries and may be willing to work at very low wages. At the same time, the government’s tax base and enforcement capacity may have grown stronger, attaching credibility to labor law<sup>1</sup>.

In South Africa, a middle income country with very high inequality, the labor market appears heavily distorted. Similar to many other low and middle income countries, formal sector work is dominated by large scale employers, where it is highly regulated, highly remunerated, and scarce. However, very differently than many peer countries, there are few small firm jobs of any sort, informal or otherwise<sup>2</sup>. The outcome is an astronomical

---

<sup>1</sup>Indeed, previous studies of minimum wages in Latin America have provided evidence that minimum wage increases yield increases in wages in both the formal and informal sectors (Freeman 2009), suggesting that legislated labor standards can impact the labor market across the labor supply distribution in middle income countries.

<sup>2</sup>For an extended discussion of small firms in developing countries, see Liedholm and Mead (1987, 1999).

unemployment rate, where only 56% of prime-aged men and 40% of prime-aged women are actually working. Understanding why there is so little employment in small firms is a question of fundamental policy importance to South Africa. This is particularly true if labor standards contribute to the problem, as then policy prescriptions are immediate. In that case, conclusions may also have direct analogues when similar policies are implemented in other contexts.

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<sup>3</sup>. Similar to centralized bargaining structures across Western Europe (Nickell 1997) and in Argentina and Brazil (Carneiro 1997; Cardoso and Gindin 2009), 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 goal of reducing competition from small firms, then this could limit the viability of small firm enterprise, restricting the options available to the unemployed. However, despite the importance of centralized bargaining in a variety of countries, there have been few empirical studies which have provided strong causal evidence of the employment or industrial structure effects of centralized bargaining agreements in any context, leaving us with scant evidence to inform policy.

Understandably, then, these bargaining councils are at the center of a vigorous policy debate in South Africa, with small firms arguing that the labor standards impose unfair costs, while large firms assert that these labor standards are not punitive and union alliances argue that they are necessary for worker protection. However, clean identification of the effects of these agreements on employment and industrial structure which could inform policy has been elusive thus far. There are several challenges to identification. First, other potential

---

<sup>3</sup>Presumably, informal firms could not be compelled to comply with bargaining council agreements and thus the presence of this or any other labor law seems unlikely to be the primary reason for the absence of informal small firms.

motivations for South Africa's unemployment without entrepreneurship abound, many of which are surely empirically relevant. 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. Moreover, the legacy of Apartheid looms large; the majority black population was prohibited from entrepreneurship during Apartheid. While these laws are no longer in place, their effects on skillsets and culture may have a lingering impact. Identifying the employment effects of labor regulations, therefore, requires a careful analysis which would hold these conditions constant. Further complicating analysis, 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 particularly large competitive advantage.

This paper estimates the effects of bargaining councils using several methods, taking advantage of the tremendous amount of variation in the data: agreements vary with space, across industries, and over time. As a benchmark, a difference-in-differences estimates significant negative effects of bargaining councils on employment and small firm employment. As it is possible that trends in local labor market characteristics are related to secular trends in bargaining council status, confounding that estimator, the paper then takes advantage of the fact that local labor markets should be spatially continuous within South Africa, while these agreements are enforced in a spatially discontinuous way. This suggests a spatial regression discontinuity estimation. Using distance to policy regime borders as the running variable, the spatial regression discontinuity reveals even larger negative employment effects which are both visibly and statistically significant. Finally, this paper adopts the spatial fixed effects proposed in Conley and Udry (2008) and Goldstein and Udry (2008) as its preferred estimator, arguing that these spatial fixed effects have several advantages over a more

traditional regression discontinuity in a spatial context. These spatial fixed effects estimates report consistent and large effects: industries which have an agreement in a particular magisterial district in a given year have about 8-13% lower employment and 10-21% higher wages than the same industry in uncovered neighboring magisterial districts. Firm sizes are also impacted, with 7-16% fewer employees in small firms and 7-15% fewer entrepreneurs, while there are smaller and insignificant effects on large firms and single employee firms. Utilizing magisterial district-year and magisterial district-industry fixed effects, I show that these spatial discontinuities are similar in magnitude and precision whether only inter-industry variation (within a magisterial district-year) or intertemporal variation (within a magisterial district-industry) is utilized, and that estimates increase in magnitude and precision if we consider only small magisterial districts who should be unable to endogenously influence these agreements. I further illustrate that, while firms do 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. The ten percent employment effect on covered industries is large relative to those which have been identified in other labor regulation studies, and accounts for about a percentage point of unemployment. These bargaining councils thus have an effect which is economically significant and should be of interest to policy makers both in South Africa and in other middle income countries. However, they cannot explain the majority of unemployment in South Africa, and therefore bargaining councils are best understood as exacerbating an existing and severe problem.

## **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>4</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 (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 (see 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, 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 numbers described above. A very natural response to this unemployment would be for many to create entrepreneurial work<sup>5</sup>. Yet row 2 of table 1 reveals that only 6-8 percent of prime age South Africans are self-employed<sup>6</sup>. These numbers are tiny compared to countries with similar levels of unemployment (e.g. Charmes 2000, Kingdon

---

<sup>4</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.

<sup>5</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 2010, Seekings and Natrass 2005), leaving individuals with poor social connections with very limited opportunities to find work.

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

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>7</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 well-enforced 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 many other developing countries, South Africa is successful in enforcing labor and tax regulations on many small 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>8</sup>(SBP 2005). Moreover, unions and firms can extend labor standard arbitration to all workers in a given political district through bargaining councils. Small 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

---

<sup>7</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>8</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.

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; surveys of this literature are available in Blau and Kahn (1999), Freeman (2009), and Nickell and Layard (1999). Much of the recent literature (e.g. Bertrand and Kramarz 2002, Besley and Burgess 2004, Harrison and Scorse 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 2010), 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 adopt 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, then all employees who work within that geographical region will work under the agreed-upon labor standards, regardless of their union status. 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 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 the choice of bargaining council size (and to determine the "potential" bargaining council units for magisterial district-industry observations which are not covered by a bargaining council).

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, magisterial districts, 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. 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. While the intuition behind

these labor market responses is straightforward, I develop a model in the web appendix which shows 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 magisterial district characteristics<sup>9</sup>.

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

$$\sum_{m \in DC} Q_m \pi_m^{BC} - C > \sum_{m \in DC} Q_m (\lambda_m \pi_m^U + (1 - \lambda_m) \pi_m^*) \quad (1)$$

---

<sup>9</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.

Local labor demand, local labor supply, local production technologies, local unionization rates, and local product demand all determine the result of this relationship. 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.

## 4 Data and Descriptives

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 to prevent mistaken inference (on, for example, the extent of the variation in employment in a particular magisterial district 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>10</sup>. From this list, I determine the magisterial district of each sampling

---

<sup>10</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.

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 magisterial district as the number of people surveyed in that magisterial district who work in that industry<sup>11</sup>. We may be concerned that very large magisterial districts have different labor markets from their neighbors, and that we get little useful information out of small magisterial districts where relatively few individuals were surveyed. I exclude the top and bottom two percent of magisterial districts 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 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>12</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

---

<sup>11</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.

<sup>12</sup>The actual number of covered workers is probably lower, due to the aggregation at the 2 digit level. Aggregation challenges are addressed below.

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 are covered, 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. These bargaining councils cover heterogeneous places in South Africa, and there is substantial variation, both geographical and intertemporal. In the appendix, I present maps showing which magisterial districts I code as always, sometimes, or never covered by a bargaining council agreement in each industry, as well as a table which identifies the number of magisterial districts which add and remove bargaining councils in each industry in each year. Industries are quite heterogeneous in their coverage patterns.

Industries are aggregated to the two-digit level. While many of the bargaining councils are defined over two-digit industries, some are defined 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, the unit reported in the surveys). 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 seems likely to 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 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.

## 5 Econometric Model

The focus of this paper will be on estimating bargaining council effects on employment, firm size, and wages. Model predictions suggested that bargaining councils may reduce overall employment and small firm employment, but that they should have an ambiguous effect on large firm employment. A linearized structural equation is given by

$$Y_{imt} = \alpha + \beta_1 BC_{imt} + \Gamma X_{imt} + \xi_i + \delta_t + \nu_{imt} \quad (2)$$

where  $Y_{imt}$  may be employment, employment by firm size, or wages in industry  $i$  in magisterial district  $m$  during year  $t$ ;  $BC_{imt}$  denotes the presence of a bargaining council agreement, and  $X_{imt}$  are covariates including population and, in different specifications, magisterial district, magisterial district-year, or magisterial district-industry fixed effects. The discussion 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 contained in  $\nu_{imt}$ , which may be correlated with  $BC_{imt}$  and other explanatory variables. Below, I'll consider several assumptions on  $\nu_{imt}$  to generate a variety of potential control groups and illustrate the robustness of identified trends to a variety of underlying assumptions.

Presuming we can adequately characterize  $\nu_{imt}$  and develop a comparable control group, any analysis still requires a standardization on bargaining council size. As discussed 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 magisterial district or closest neighbors of an adjoining district council). In fact, in a few cases bargaining councils cover only a single magisterial district. Though these observations represent a small share of the data, we may still worry about the implications of these observations for analysis, particularly in the estimates which emphasize differences across space at bargaining council borders and which are the focus of this paper. Moreover, this fact forces consideration of potential bargaining council sizes for places with no current bargaining council in constructing adequate control groups, which is necessary to identify the proper unit of analysis.

A direct solution to this problem is to treat bargaining council adoption as incomplete take-up as discussed in the impact evaluation literature. In that literature, presuming we have exogenous assignment of treatment but incomplete take-up among the treated, the common solution is to instrument actual treatment status with assignment to treatment. I follow that approach in this paper. Since most bargaining councils are assigned at larger political boundaries than magisterial districts, 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: all t-statistics of bargaining council eligibility on bargaining council status are over 9.8<sup>13</sup>. A visual first stage is presented in the appendix, which includes maps depicting both the actual coverage and the instrumented coverage in each industry. The appendix also presents the formal estimates of this first stage in the paper's main specifications.

Second, regardless of the strategy used to solve the identification problem, two potential sources of dependence among observations are well-known and relevant to this context. A

---

<sup>13</sup>Coefficients of bargaining council status on bargaining council eligibility range from 0.77 to 1.00.

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 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>14</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 (discussed in Donald and Lang 2007). Secondly, the error term may be spatially autocorrelated (Conley 1999). As the primary identification strategy will rest on the difference between magisterial districts 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 either the same district council in the primary analysis<sup>15</sup>. 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.

## 5.1 Econometric Benchmark 1: Difference-in-Differences

Our ability to estimate equation 2 depends on how we can characterize  $\nu_{imt}$ . Differences-in-differences remain the dominant approach in the literature and make the assumption that  $E[\nu_{imt}|X_{imt}, \xi_i, \delta_y] = \tilde{\nu}_{iDC}$ , allowing a simple District Council-Industry fixed effect to

---

<sup>14</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.

<sup>15</sup>The less robust Spatial RD analysis clusters over space and within district council-industries as I describe below.

eliminate the endogeneity concern. In that case, we can estimate

$$Y_{imt} = \alpha + \beta BC_{imt} + \Gamma X_{imt} + \xi_i + \delta_t + \tilde{\nu}_{iDC} + \varepsilon_{imt} \quad (3)$$

where  $\xi_i$ ,  $\delta_t$ , and  $\tilde{\nu}_{iDC}$  represent industry, year, and District Council-Industry fixed effects, and  $X_{imt}$  includes a quartic in log population.  $Y_{imt}$  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. Bargaining council status is instrumented with bargaining council eligibility, as described in section 4.1. Panel A of table 5 performs the difference-in-differences estimation on the main sample. All cells report the coefficient on bargaining council presence for a given sample and dependent variable. Across the board, a difference-in-differences suggests that effects of bargaining councils are negative and significant. As bargaining council agreements come into place, employment decreases by 7%, as does small firm and self employment. 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, thus violating the necessary assumption for a difference-in-differences estimation.

## 5.2 Econometric Benchmark 2: Spatial Regression Discontinuity

An alternate assumption notes that the endogenous characteristics of local labor markets within a given industry are likely to be spatially continuous<sup>16</sup>, so long as migration and trade are locally feasible. Formally, let  $R(m)$  denote the set of all magisterial districts

---

<sup>16</sup>It is possible that, as a legacy of Apartheid, some labor market characteristics may not be spatially continuous due to poor infrastuctural connections (the Apartheid government did purposefully separate racial groups). If true, this would challenge identification in this paper. I address this issue in the section 6.1 by running specifications featuring either town-year or town-industry fixed effects. Since barriers which have lingered since Apartheid should both effect all industries in a given town and all years within the same industry, these approaches will be robust to this concern. As it turns out, both yield consistent results, suggesting that lingering spatial barriers do not drive the empirical trends documented here.

within radius  $R$  of magisterial district  $m$ ,  $Z_{imt}$  be the vector  $[X_{imt}, BC_{imt}, \xi_i, \delta_t]$  and  $Z_{iR(m)t}$  and  $n\nu_{iR(m)t}$  denote the vectors of  $Z_{im't}$  and  $\nu_{im't}$ ,  $\forall m' \in R(m)$ . Then, spatial continuity suggests that

$$E [\nu_{imt}|Z_{iR(m)t}] = E [\nu_{im't}|Z_{iR(m)t}] \text{ for } R \text{ sufficiently small and } m' \in R(m). \quad (4)$$

This assumption is similar to that made in standard regression discontinuity designs, suggesting a spatial regression discontinuity. As with other regression discontinuity designs, the appeal of spatial regression discontinuity is that we may expect (endogenous) local labor market characteristics to vary smoothly over space. In this case, spatial continuity may be confounded if firms are capable of resettling on one side of the policy regime, and spatial discontinuity analysis will have to take this possibility seriously. Also as in other regression discontinuity designs, spatial RD will not estimate policy effects which vary smoothly over policy borders; for example, if elevated wages affect equilibrium wage rates in adjacent district councils, then the spatial RD will miss these effects.

Despite these concerns, there is a long history of economists exploiting spatial discontinuities to identify the effects of regulations. Space is somewhat different from conventional running variables employed in regression discontinuities in two ways. First, it is two dimensional, and legal borders tend not to precisely collapse on any one of those two dimensions (for example, they tend not to correspond to longitudinal lines). Second, there is less of an a priori reason to suspect a systematic relationship between space and the outcome variable than there is with many running variables. Most papers in this field have resolved these issues by collapsing the spatial data into a single dimension of "distance to the border", following seminal work by Card and Krueger (1994) and Holmes (1998), and simply taking the side of the border that an individual resides on as random for individuals within some bandwidth of the border. Once we have reduced space to a single dimension, familiar graphical and statistical analysis can proceed as in other regression discontinuity studies.

As noted above, population has an almost mechanical relationship with employment. Thus, to implement the RD I first estimate

$$Y_{imt} = \alpha + g(\text{pop}_{imt}) + u_{imt} \quad (5)$$

where  $g(\cdot)$  is a quartic in log population. By examining  $u_{imt}$  as the unexplained variation which we may expect to be discontinuous at bargaining council borders, we examine the variation in employment which is not explained by differences in survey population density over the support of the running variable.

Heterogeneity in bargaining council size poses a second challenge. When we sum over all bargaining councils, small bargaining councils are disproportionately represented among observations which have bargaining councils and are close to the border, while large bargaining councils are disproportionately represented among bargaining council observations which are far from the border. If we want to examine the running variable over meaningful lengths, we will be forced to interpret changes in employment over the support of distance to the border as a combination of changes deriving from the effect of the bargaining council and changes related to the composition of bargaining councils present at that difference<sup>17</sup>. To lessen the extent of these differences, I exclude bargaining councils where no observations are more than 50 miles from the border and which represent 17% of bargaining council observations, though similar figures are generated excluding bargaining councils which have no observations more than 30, 75, or 100 miles from the border<sup>18</sup>.

Figure 1 (A), (B), (C), and (D) present a scatter plot of binned data for employment, small firm employment, large firm employment, and self employment, where the X-axis represents distance from the closest border of the industry's own bargaining council and the scatter plot shows means for each 10-mile bin. Positive distances indicate that the

<sup>17</sup>I note that a similar trend does not occur at the non-bargaining council side of the border, as small and large bargaining councils both have observations which are near and far.

<sup>18</sup>My data do not include a direct measure of bargaining council size. Here, I determine it as the maximum distance any observation in that province-industry-year is from the border. The median bargaining council has observations up to 140 miles from the border.

observation is on the Bargaining Council side of the border. Each figure also overlays a fitted estimate of the trend with distance to the border, where the trend is a kernel-weighted local polynomial regression estimated separately on each side of the border using an epanechnikov kernel and a ten-mile bandwidth.

Figure 1 (A) reveals that for overall log employment, there is a clear drop in employment at the bargaining council border, while figure 1 (B) shows a similar drop is even more pronounced for small firm employment. Figure 1 (C) examines log large firm employment and again finds evidence of a discontinuity at the bargaining council border, though it appears to be driven by an increase in employment on the non-bargaining council side of the border and is smaller in magnitude than the other discontinuities presented here. Finally, figure 1 (D) looks at self employment, and also finds evidence that self employment is reduced within bargaining councils.

All of these RD estimates are larger in magnitude than the difference-in-difference estimates. To assess statistical significance, I tighten the bandwidth to 50 miles on either side of the boundary, and estimate

$$Y_{imt} = \alpha + \beta BC_{imt} + \gamma_1 Dist_{imt} + \gamma_2 BC_{imt} * Dist_{imt} + g(pop_{imt}) + \varepsilon_{imt}$$

where  $Dist_{imt}$  is the distance of magisterial district  $m$  from the bargaining council border in industry  $i$  in year  $t$ , and other variables are as above. Panel B of Table 5 reports the results of this exercise, where errors are clustered over space and at the District Council-Industry level. Across the board, estimated effects from the Spatial RD design are large and significant, with Employment declining by 36%, Small Firm and Self Employment declining by 30%, and Large Firm Employment decreasing by 23%. The standard errors are also large, and we cannot rule out similarly sized effects to those in the difference-in-difference estimation.

## 6 Spatial Fixed Effects

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, magisterial districts, or counties to proximate ones, it compares all magisterial districts on one side of the boundary to all magisterial districts on the other (who may not be particularly proximate). In other words, the guiding assumption to the above approach is that  $E[\nu_{imt}|Z_{iR(m)t}] = E[\nu_{im't}|Z_{iB(m)t}]$  for  $B$  sufficiently small and  $m' \in B(m)$ , where  $B(m)$  is the industry's bargaining council border. Note that this assumption is somewhat different from assumption 4, as the geographic area spanned by  $B(m)$  may be quite large, even at small bandwidths, as space has two dimensions. 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>19</sup>.

Practically, this has three consequences. First, we reduce our precision substantially. In contrast to a difference-in-difference approach or an approach where we compare only across small regions of space, we fail to control for the very notable spatial and time-invariant heterogeneity in employment. On top of that, restricting analysis to border regions removes the ability of observations which are more distant from the border to identify the effects 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. In practice, this loss of precision resulted in large standard errors for the RD estimates. Second, we lose smaller bargaining councils from the data. There are a number of ways to address this issue, but in general when we collapse space to a single dimension and examine what happens as we move along that dimension, we will be challenged by bargaining councils which are small. A number of these are important in South

---

<sup>19</sup>A different approach which solves this concern is presented in Dube, Lester, and Reich (2010). 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.

Africa, and the generalizability and interpretation of results would be affected by excluding them altogether. Finally, the raw spatial RD ignores the panel nature of the data. When bargaining council agreements change over time, a raw spatial RD will change the spatial mapping to distance to the border, and lose track of the underlying labor market conditions which may remain similar in a district over time as that district is moved around within the mapping of "distance to the border." Given that difference-in-differences estimates were more conservative than spatial-discontinuity estimates, it seems ideal to estimate a specification which accounts for constant unobservable characteristics of magisterial districts.

This paper adopts the spatial fixed effects (SFE) estimator introduced in Conley and Udry (2008) and Goldstein and Udry (2008)<sup>20</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.

Thus, if  $n_{R(m)}$  represents the number of magisterial district-year observations in  $R(m)$ , we can represent the spatial fixed effects estimator as

$$Y_{imt} - \frac{1}{n_{R(m)}} \sum_{m' \in R(m), t'} Y_{im't'} = \beta_1 \left( BC_{imt} - \frac{1}{n_{R(m)}} \sum_{m' \in R(m), t'} BC_{im't'} \right) + \Gamma \left( X_{imt} - \frac{1}{n_{R(m)}} \sum_{m' \in R(m), t'} X_{im't'} \right) + \delta_t - \frac{1}{n_{R(m)}} \sum_{m' \in R(m), t'} \delta_{t'} + \nu_{imt} - \frac{1}{n_{R(m)}} \sum_{m' \in R(m), t'} \nu_{im't'} \quad (6)$$

If we assume that  $\nu_{imt}$  is spatially continuous and constant up to flexible trend controls, then  $E \left[ \nu_{imt} - \frac{1}{n_{R(m)}} \sum_{m' \in R(m), t'} \nu_{im't'} \mid BC_{iR(m)t}, X_{iR(m)t}, \delta_t \right] = 0$  and this within estimator will consistently estimate  $\beta_1$  and  $\Gamma$ . In this specification, identification is by spatial and intertemporal discontinuity: outcomes are compared only against those of proximate neighbors as are program status and covariates. This equation estimates whether, if a magisterial district's bargaining council status is greater than its neighbors' (i.e. the magisterial district lies on the bargaining council-side of a border), then the magisterial district has less employ-

---

<sup>20</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.

ment than its neighbors. Interior magisterial districts have a spatial deviation of zero in bargaining council status, but still contribute to the estimation of employment effects of differences in population and time trends. The analogous 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.

Finally, in addition to a benchmark spatial fixed effects regression, I repeat all analysis with magisterial district-year and magisterial district-industry fixed effects in addition to spatial fixed effects. Magisterial district-year fixed effects specifications will be robust to any dimensions in which the magisterial district is different from its spatially proximate neighbors that year, which includes the possibility that the magisterial district may be politically valuable to unions, the possibility that local neighbors are in fact distinct labor markets due to terrain or infrastructural separations 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. Magisterial district-industry fixed effects control for any ways in which that industry's local labor market differs from its spatially proximate neighbors which is constant over time<sup>21</sup>.

---

<sup>21</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.

The use of the full set of dummies adds an extra complication when combined with the space and political jurisdiction clusters described below. As described in Cameron, Gelbach, and Miller (2006), clustering in multiple dimensions can have the outcome that some diagonal elements of the estimated variance-covariance matrix are negative, which happens on the variance estimates associated with some of these nuisance parameters in these subsamples. However, as inference is robust to clustering unilaterally on either dimension, and the standard errors on the coefficient of interest remain well-behaved (in the sense that relative magnitudes between jointly clustering and clustering in either dimension stays similar), I continue to report these "correct" standard errors in tables here.

## 7 Spatial Fixed Effects 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 6, 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, magisterial districts, and years in the same district council. Having a bargaining council agreement is associated with a significant 8% reduction in log employment in the first row; including magisterial district-specific, magisterial district-year, or magisterial district-industry fixed effects keeps estimates of the bargaining council effect between 8-13%, and always remains 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>22</sup>.

### 7.1 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 individuals thus are also represented in the small firms and the single-worker firms categories. From the model above, we expect bargaining councils to have the largest effects on employ-

---

<sup>22</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.

ment 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 significant. Consistent with theory, bargaining councils reduce small firm employment substantially, with bargaining council employment being associated with a 7-16% decline. This effect remains very similar when we examine how spatial differences vary within industries in a magisterial district or magisterial district 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.

## **7.2 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 magisterial districts (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 magisterial district-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 magisterial district-year or magisterial district-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 magisterial district-year effects regain precision. Overall, industries represented by a bargaining council in a magisterial district have 21% higher mean wages than the same industry in neighboring magisterial districts, and 14% higher wages if we hold constant mean deviations across industries in that year<sup>23</sup>. 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 4 and 5. Consistent with theory, wages in small firms are rising substantially, with (precisely measured) point estimates around 12-20%. 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. Column 5 controls for the fraction male, the average number of years of primary and secondary education, a quadratic in average potential experience (age - education - six), and the fraction of the workforce which is black, and finds that these controls attenuate wage effects by about 5 percentage points. In the appendix, I present estimated worker composition effects, and

---

<sup>23</sup>Since the wage data appear not to be sufficiently dense for a 30 mile radius with town-year heterogeneity, I report the following wage 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)

find that the primary change in composition is that women and workers with low tenure are being systematically disemployed by bargaining councils.

## 8 Robustness

There are two sets of concerns which could confound analysis. A first set is econometric: the spatial fixed effects may be misspecified as they impose homogeneity assumptions over space and over time. In the appendix, I weaken these two assumptions and find that estimates are robust to alternate spatial weights and to eliminating restrictions over time (e.g. estimating spatial-industry-year fixed effects). Second, there are two sets of economic endogeneity explanations which are explored here. First, we may remain concerned that if a few labor markets are very different from their neighbors and are important in determining bargaining council status, then there may be discontinuities in labor markets which are systematically related to bargaining council coverage. 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.

### 8.1 Manipulation by Dominant Magisterial Districts

If some magisterial districts are very different from their neighbors and can determine bargaining council policy, then we may not expect secular trends in these districts to be continuous over space but they would be related to bargaining council policy. However, equation 1 makes clear that the presence of a bargaining council is due to some collaboration of magisterial districts in the same political district. If local labor markets are relatively continuous, then nearby magisterial districts 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 magisterial district  $m$  is different from industry

$i$  in neighboring districts. If magisterial district  $m$  has much lower employment than other magisterial districts in its District Council, then, as equation 1 makes explicit, magisterial district  $m$ 's preferences should not be strongly reflected in the presence or absence of a bargaining agreement. In particular, if magisterial district  $m$  is discontinuously different from its political neighbors in its incentive to form a bargaining council, then 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 magisterial district  $m$ , then this concern may remain, and the presence of a bargaining council in magisterial district  $m$ 's district council may be a reflection of these discontinuous labor market trends. In table 8, I repeat all estimation with a sample of industries and magisterial districts where employment is no more than 20% of employment in that industry in that district council on average.

Despite the smaller sample, precision increases and point estimates rise. Among magisterial district-industry groups which are too small to independently effect bargaining council policy, we see employment fall by 12-16% relative to neighbors and other industries or other years within the magisterial district. 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 magisterial district which represents a small fraction of its county's employment can expect to see a 10-14% decline in small firm employment, a 5-9% decline in self employment, and no change in its large firm or single-employee firm employment relative to its neighbors.

## 8.2 Border Jumping

As with other regression-discontinuity estimators, manipulation of the running variable could lead to mistaken inference. Here, we may be concerned that firms could relocate to a magisterial district immediately on the opposing side of the border which would cause large

estimated spatial discontinuities but be a minimal concern for policy.

Some evidence against the importance of border-jumping was presented in Figure 1, which indicated that employment levels remain depressed deep into the interior of bargaining councils. A direct test (similar to Holmes 1998) would ask whether log employment is different in a magisterial district if it is on the border of a bargaining council agreement than otherwise. Specifically, suppose we divide the bargaining council into several distance groups, so that we collect all magisterial district-industry-year observations which are uncovered by a bargaining council but are either within 0-30 or 30-50 miles of a covered magisterial district-industry-year observation, and similarly observations which are covered by a bargaining council but within 0-30 or 30-50 miles of an uncovered magisterial district-industry-year observation<sup>24</sup>. Then, we can test whether each of these border distance groups are different from their counterparts in the same bargaining council regime by regressing

$$Y_{imt} = \sum_k \beta_{1k} DistGroup_{imt}^k + \beta_2 BC_{imt} + \gamma_t + \alpha_i + \varepsilon_{imt}$$

However, local labor markets in border regions may systematically differ from those interior regions. Two controls for spatial heterogeneity are used here. First, the magisterial district-industry fixed effects used earlier can still be used in this setting. These fixed effects identify control for any local labor market characteristics which remain constant over time and allows us to examine simultaneously the effect of changing bargaining council status and changing being on the border of a bargaining council. Second, I use fixed effects at the District Council-Industry-Year level. Here, border effects are identified off of magisterial districts which are closer to the border than other magisterial districts in the same district council (though bargaining council effects cannot be identified from this specification.)

---

<sup>24</sup>One could also measure distance to the border by geographic distance to the border (as opposed to distance to the nearest town on the other side of the border), as the measure in the Spatial Regression Discontinuity section is constructed. Estimates using that measure show less evidence of border jumping and an identical estimate of the robustness of employment effects of bargaining councils. The measure of distance between two observations is preferred here as it more accurately reflects the distance measure which underlies estimates.

Table 9 reports the results of this analysis, which reveals that border jumping is taking place. When a bargaining council is formed near a given magisterial district but not including that magisterial district, that magisterial district sees a large increase in employment (column 1). 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 magisterial districts that do, is associated with an increase in small firms (column 6, row 2). In other words, we do find evidence of manipulation - firms are purposefully locating outside of the coverage of a bargaining council.

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 immediately on the bargaining council side of the border as well as the non-bargaining council side (see the estimate on column 1, row 1), 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.

## 9 Conclusions

Bargaining council agreements are the outcome of a complex bargaining process, which challenges inference as to their effects. This paper uses difference-in-differences, spatial regression

discontinuity, and spatial fixed effects to demonstrate that bargaining councils are associated with about 8-13% lower employment in a particular industry, 10-21% higher wages, and 7-16% less employment in small firms. Under spatial fixed effects, these estimates are further found to be robust to additional magisterial district, magisterial district-year and magisterial district-industry fixed effects. That is, an industry with a bargaining council has about 8-13% less employment than its neighbors without a bargaining council. This is true if we compare it to how different industries in the same magisterial district compare to their neighbors, or if we compare how employment in that magisterial district and industry changes over time with bargaining council status. Magisterial district-Industry observations which employ a relatively small fraction of the employees in their District Council experience the most severe consequences; that is, magisterial districts 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 magisterial districts – 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 thirteen percent is a large decrease in employment in a given industry. By means of comparison, Bertrand and Kramarz (2002) estimate that French entry regulations reduce food retail employment by about 7%, Besley and Burgess (2004) estimate that labor regulation reduced manufacturing employment in India by 7%, and Harrison and Scorce (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. Similar centralized bargaining systems exist in Western Europe and Latin America; these estimates suggest that if enforcement

is possible, and there is a large labor supply to low-productivity small firms jobs, then these centralized bargaining structures may contribute strongly to unemployment. Balancing this concern with improved labor conditions may be an important priority, particularly for low and middle income countries. 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 industries were to increase employment by 8-13%, it would cause a 0.88-1.43 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. The small firm effects is similarly large, and, unlike French entry regulations (Bertrand and Kramarz 2002) hurts, rather than helps, small firms. This policy is thus 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 7-16% 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.

## References

- (2005): "Cosatu Rejects Plan to Bypass Centralized Bargaining," *The Star*, February 15, 6.
- BANERJEE, A., S. GALIANI, J. LEVINSOHN, Z. MCLAREN, AND I. WOOLARD (2008): "Why Has Unemployment Risen in the New South Africa?," mimeo, MIT.
- BENDIX, S. (2001): *Industrial Relations in South Africa*. Juta and Co., Ltd, 4th edn.

- BERTRAND, M., E. DUFLO, AND S. MULLAINATHAN (2004): “How Much Should We Trust Difference-in-Differences Estimates?,” *Quarterly Journal of Economics*, 119(1), 249–275.
- BERTRAND, M., AND F. KRAMARZ (2002): “Does Entry Regulation Hinder Job Creation? Evidence From the French Retail Industry,” *The Quarterly Journal of Economics*, 117(4), 1369–1413.
- BERTRAND, M., S. MULLAINATHAN, AND D. L. MILLER (2003): “Public Policy and Extended Families: Evidence from Pensions in South Africa,” *World Bank Economic Review*, 17(1), 27–50.
- BESLEY, T., AND R. BURGESS (2004): “Can Labor Regulation Hinder Economic Performance? Evidence from India,” *The Quarterly Journal of Economics*, 119(1), 91–134.
- BLAU, F. D., AND L. M. KAHN (1999): “Institutions and Laws in the Labor Market,” in *Handbook of Labor Economics, Volume 3*, chap. 25, pp. 1399–1461. Elsevier-North Holland.
- BUTCHER, K. F., AND C. E. ROUSE (2001): “Wage Effects of Unions and Industrial Councils in South Africa,” World Bank Policy Research Working Paper No. 2520.
- CAMERON, A. C., J. B. GELBACH, AND D. L. MILLER (2006): “Robust Inference with Multi-Way Clustering,” mimeo, University of California, Davis.
- CARD, D., AND A. B. KRUEGER (1994): “Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania,” *The American Economic Review*, 84(4), 772–793.
- CARDOSO, A., AND J. GINDIN (2009): “Industrial Relations and Collective Bargaining: Argentina, Brazil, and Mexico Compared,” ILO Industrial and Employment Relations Department Working Paper No. 5.
- CARNEIRO, F. G. (1998): “Productivity Effects in Brazilian Wage Determination,” *World Development*, 26(1), 139–153.
- CHARMES, J. (2000): “Informal Sector, Poverty and Gender: A Review of Empirical Evidence,” <http://www.wiego.org/papers/charmes3.pdf>, Background Paper for the World Development Report 2001.
- CONLEY, T. G. (1999): “GMM Estimation with Cross-Sectional Dependence,” *Journal of Econometrics*, 92(1), 1–45.
- CONLEY, T. G., AND C. R. UDRY (2008): “Learning About a New Technology: Pineapple in Ghana,” *The American Economic Review*, forthcoming.
- DAVIS, P. (2002): “Estimating Multi-Way Error-Components Models with Unbalanced Data Structures,” *Journal of Econometrics*, 106(1), 67–95.
- DONALD, S. G., AND K. LANG (2007): “Inference with Difference-in-Differences and Other Panel Data,” *The Review of Economics and Statistics*, 89(2), 221–223.

- DUBE, A., T. W. LESTER, AND M. REICH (2010): “Minimum Wage Effects Across State Borders: Estimates Using Contiguous Counties,” *Review of Economics and Statistics*, 92(4), 945–964.
- EDMONDS, E. V., K. MAMMEN, AND D. L. MILLER (2004): “Rearranging the Family? Income Support and Elderly Living Arrangements in a Low-Income Country,” *The Journal of Human Resources*, 40(1), 186–207.
- FREEMAN, R. B. (2009): “Labor Regulations, Unions, and Social Protection in Developing Countries: Market Distortions or Efficient Institutions?,” NBER Working Paper No. 14789.
- GOLDSTEIN, M., AND C. UDRY (2008): “The Profits of Power: Land Rights and Agricultural Investment in Ghana,” *The Journal of Political Economy*, forthcoming.
- HARRISON, A., AND J. SCORCE (2008): “Multinationals and Anti-Sweatshop Activism,” *The American Economic Review*, forthcoming.
- HOLMES, T. J. (1998): “The Effect of State Policy on the Location of Manufacturing: Evidence from State Borders,” *The Journal of Political Economy*, 106(4), 667–705.
- KINGDON, G. G., AND J. KNIGHT (2004): “Unemployment in South Africa: The Nature of the Beast,” *World Development*, 32(3), 391–408.
- (2006): “The Measure of Unemployment When Unemployment is High,” *Labour Economics*, 13(3), 291–315.
- (2008): “Unemployment in South Africa, 1995-2003: Causes, Problems, and Policies,” *Journal of African Economies*, 16(5), 813–848.
- LIEDHOLM, C., AND D. MEAD (1987): “Small Scale Industries in Developing Countries: Empirical Evidence and Policy Implications,” MSU International Development Paper No. 9.
- LIEDHOLM, C., AND D. C. MEAD (1999): *Small Enterprises and Economic Development*. Routledge, London and New York.
- MAGRUDER, J. R. (2010): “Intergenerational Networks, Unemployment, and Persistent Inequality in South Africa,” *AEJ: Applied Economics*, 2(1), 62–85.
- MBEKI, T. (2005): “Address of the President of South Africa, Thabo Mbeki, at the Second Joint Sitting of the Third Democratic Parliament, Cape Town.,” <http://www.info.gov.za/speeches/2005/05021110501001.htm>.
- MOLL, P. (1996): “Compulsory Centralization of Collective Bargaining in South Africa,” *The American Economic Review*, 86(2), 326–329.
- NICKELL, S. (1997): “Unemployment and Labor Market Rigidities: Europe versus North America,” *Journal of Economic Perspectives*, 11(3), 55–74.

- NICKELL, S., AND R. LAYARD (1999): “Labor Market Institutions and Economic Performance,” in *Handbook of Labor Economics, Volume 3c*, ed. by O. Ashenfelter, and D. Card, pp. 3029–3084. Elsevier-North Holland.
- SBP (2005): “Counting the Cost of Red Tape to Business in South Africa,” Discussion paper, Small Business Project.
- SCHULTZ, T. P., AND G. MWABU (1998): “Labor Unions and the Distribution of Wages and Employment in South Africa,” *Industrial and Labor Relations Review*, 51(4), 680–703.
- SEEKINGS, J., AND N. NATTRASS (2005): *Class, Race, and Inequality in South Africa*. Yale University Press, New Haven and London.
- STATISTICS SOUTH AFRICA (PRODUCER) (1997): “October Household Survey, 1997,” South African Data Archive (distributor). [www.nrf.ac.za/sada](http://www.nrf.ac.za/sada).
- (2000-2003): “South Africa Labour Force Surveys, September 2000, September 2001, September 2002, September 2003,” Pretoria, South Africa. South African Data Archive (distributor). [www.nrf.ac.za/sada](http://www.nrf.ac.za/sada).

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	0.963	1.013	0.000	4.564	5048
Log Large Firm Employment	0.330	0.599	0.000	3.664	5048
Log Small Firm Employment	0.735	0.893	0.000	4.127	5048
Log Self Employment	0.502	0.763	0.000	3.932	5048
Log Single Firm Employment	0.346	0.651	0.000	3.829	5048
Log Population	4.824	0.972	2.773	6.868	5048
Bargaining Council	0.351	0.477	0.000	1.000	5048
Mean log wage	5.216	0.787	1.569	9.798	2728
Large Firm log wage	5.724	0.669	3.178	8.476	1260
Small Firm log wage	5.025	0.832	1.569	9.798	2261
Fraction Male	0.546	0.371	0.000	1.000	2728
Potential Experience	23.957	9.245	0.000	66.000	2728
Worker Education	8.495	2.704	0.000	12.000	2728
Mean Log Tenure	1.194	0.774	0.000	3.761	2528

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: Benchmark Estimates: Difference-in-differences and Spatial RD

Dependent Variable	(1) Employment	(2) Large Firm Employment	(3) Small Firm Employment	(4) Self Employment
Panel A: Difference-in-Difference Estimates				
Bargaining Council	-0.070** (0.031)	-0.047 (0.036)	-0.066** (0.032)	-0.062* (0.033)
N	5048	5048	5048	5048
Panel B: Regression Discontinuity Estimates				
Bargaining Council	-0.364** (0.145)	-0.228*** (0.079)	-0.303** (0.145)	-0.303** (0.111)
Distance to the Border	0.002 (0.004)	0.002 (0.002)	0.001 (0.004)	0.002 (0.004)
Bargaining Council *	-0.002 (0.007)	0.000 (0.003)	-0.002 (0.006)	-0.003 (0.005)
N	1451	1451	1451	1451

Notes

- 1 Presents IV estimates of the effect of bargaining council status on several specifications.
- 2 Columns indicate 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 Difference-in-Differences errors are clustered at the district council level while Spatial RD errors are clustered at the district council-industry level

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	-0.081** (0.039)	-0.012 (0.041)	-0.114*** (0.038)	-0.100** (0.042)	-0.055 (0.041)
Magisterial District	-0.102*** (0.036)	-0.061 (0.040)	-0.119*** (0.029)	-0.119*** (0.039)	-0.048 (0.035)
Magisterial District-Year	-0.130*** (0.044)	-0.051 (0.045)	-0.158*** (0.028)	-0.149*** (0.054)	-0.092** (0.041)
Magisterial District-Industry	-0.075** (0.032)	-0.051 (0.038)	-0.071** (0.033)	-0.067** (0.034)	-0.017 (0.022)
N	5048	5048	5048	5048	5048

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, magisterial districts, 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	(5) Wage
Fixed Effects Level					
None	0.205** (0.091)	0.216*** (0.076)	0.192** (0.090)	0.136 (0.112)	0.146*** (0.048)
Magisterial District	0.141** (0.061)	0.179*** (0.048)	0.150** (0.066)	-0.062 (0.121)	0.140*** (0.055) <sup>+</sup>
Magisterial District-Year	0.122 (0.096)	0.141*** (0.049)	0.196*** (0.068)	-0.056 (0.145)	0.079*** (0.026)
Magisterial District-Industry	0.091 (0.059)		0.116 (0.114)	-0.069 (0.064)	0.085*** (0.030)
Radius	30	50	50	50	50
Worker Composition?	No	No	No	No	Yes
Sample	Wage	Wage	Small Wage	Large Wage	Wage
N	2728	2728	2261	1260	2728

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, magisterial districts, and years in a given district council.
- 6 The <sup>+</sup> standard error is the maximum error from clustering either only among space or within district councils as the estimated variance was negative (following Cameron, Gelbach, and Miller 2006)
- 7 Column 5 includes worker gender, age, potential experience, and race controls

Table 8: Bargaining Council Effects by Firm Size: Low Employment Sample

Dependent Variable	(1) Employment	(2) Large Firm Employment	(3) Small Firm Employment	(4) Self Employment	(5) Single Firm Employment
Fixed Effects Level					
None	-0.138** (0.059)	0.009 (0.031)	-0.131*** (0.050)	-0.057 (0.047)	-0.046 (0.046)
Magisterial District	-0.155*** (0.047)	-0.020 (0.023)	-0.128*** (0.042)	-0.073* (0.040)	-0.032 (0.038)
Magisterial District-Year	-0.160*** (0.058)	-0.005 (0.032)	-0.139*** (0.054)	-0.088 (0.059)	-0.048 (0.053)
Magisterial District-Industry	-0.118*** (0.041)	-0.017 (0.022)	-0.104*** (0.031)	-0.047 (0.030)	-0.019 (0.018)
N	3631	3631	3631	3631	3631

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, magisterial districts, and years in a given district council
- 5 Sample is restricted to magisterial district-industry observations which represent less than 20% of the employment in that industry in that district council on average

Table 9: Border Jumping

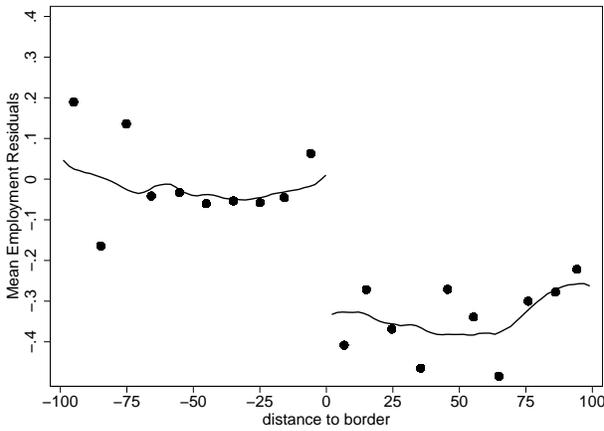
Dependent Variable	Employment		Large Firm Employment		Small Firm Employment	
	(1)	(2)	(3)	(4)	(5)	(6)
Bargaining Council side, 30 mile border	0.130*** (0.050)	-0.094 (0.296)	-0.038 (0.071)	-0.269* (0.152)	0.072 (0.131)	-0.011 (0.292)
Not Bargaining Council side, 30 Mile Border	0.218*** (0.083)	0.251** (0.098)	0.150 (0.110)	0.059 (0.104)	0.185 (0.124)	0.244** (0.103)
Bargaining Council side, 50 mile border	0.143 (0.120)	0.141 (0.219)	-0.078 (0.129)	0.105 (0.158)	0.174 (0.105)	0.120 (0.200)
Not Bargaining Council side, 50 Mile Border	-0.159 (0.145)	0.051 (0.050)	0.073 (0.077)	0.054 (0.033)	-0.275* (0.149)	-0.028 (0.053)
Bargaining Council Agreement	-0.124** (0.051)		-0.004 (0.030)		-0.126** (0.062)	
Fixed Effects	MD- Indus	DC-Ind- Year	MD- Indus	DC-Ind- Year	MD- Indus	DC-Ind- Year
N	5048	5048	5048	5048	5048	5048

Notes

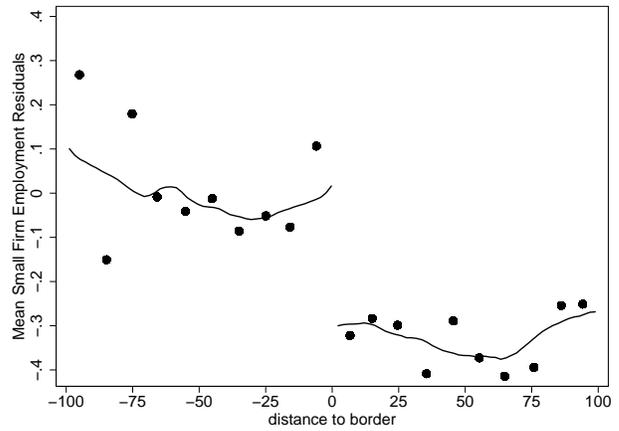
- 1 Presents IV estimates of employment or employment by firm size on bargaining council status, as well as being within 30 or 50 miles of the regime border, where the effect of being on a border is allowed to be asymmetric by which side of the border a magisterial district is on.
- 2 All results are 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, magisterial districts, 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: Regression Discontinuity Estimates

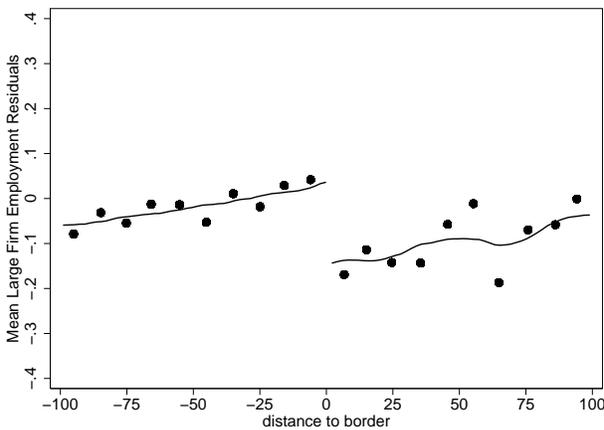
(a) Employment Residuals



(b) Small Firm Employment Residuals



(c) Large Firm Employment Residuals



(d) Self Employment Residuals

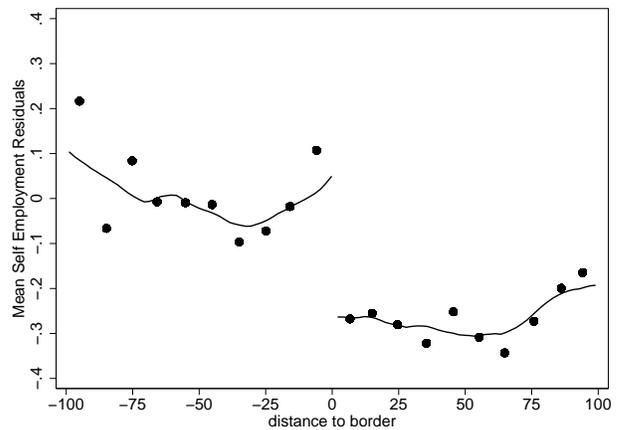


Figure compares average employment residuals for various firm sizes against distances to the border of a bargaining council regime, in miles. Positive distances indicate that the town is on the covered side of the bargaining council border (i.e. that the town is covered by a bargaining council), while negative distances indicate that the town is on the uncovered side.