Mandated Benefits, Employment, and Inequality in a Dual Economy

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Abstract

We study the effect of enforcing labor regulation in an economy with a dual labor market. We use data from Brazil, a country with a large informal sector and strict labor law, where enforcement affects mainly the degree of compliance with mandated benefits (severance pay; health and safety conditions) in the formal sector, and the registration of informal workers. We find that stricter enforcement leads to higher unemployment but lower income inequality. We also show that, at the top of the formal wage distribution, workers bear the cost of mandated benefits by receiving lower wages. Wage rigidity (due, say, to the minimum wage) prevents this downward adjustment at the bottom of the income distribution. As a result, formal sector jobs at the bottom of the wage distribution become more attractive, inducing the low skilled self-employed to search for formal jobs.

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1. Introduction

A large fraction of the labor force in the developing world works in the informal sector. Therefore, any study of employment or inequality in these countries should consider interactions between the formal and informal sectors. Nevertheless, it is striking that most empirical studies of the effects of labor market regulation are based either on a single labor market model, or on the assumption that regulation only affects the formal sector.

In this paper, we study the impact of enforcing labor regulation on labor market outcomes in Brazil, a country where more than 40% of the workforce is informal. Short of variation in labor regulation, studying variation in enforcement is a promising alternative for studying the effects of regulation, since its effectiveness is tied to the degree of compliance.

In Brazil, enforcement affects mainly the provision of mandated benefits in formal jobs (through severance pay, or health and safety conditions), so an increase in enforcement will translate primarily into an increase in these benefits (Cardoso and Lage, 2007).[2] To a smaller extent, enforcement also affects the formalization of informal contracts. [3] Our goal is to understand the impact of enforcing mandated benefits in an economy with a dual labor market, by analyzing the simultaneous response of formal and informal employment and earnings.

The effect of enforcement on labor market outcomes such as employment and wages depends on the extent to which workers value the enforced benefit, the elasticities of labor demand and labor supply in the formal and informal sectors, and wage rigidities (caused, say, by the minimum wage). The rate at which mandated benefits such as severance pay pass through to wages is likely to be high in Brazil since severance pay is untaxed, and workers can draw from the firm's severance pay fund (e.g., to buy a house) even if they are not dismissed. However, minimum wages impose downward wage rigidity, limiting the extent to which wages can decline

[2] In line with this reasoning, we define as informal all workers who are not registered as formal workers, and therefore who are not eligible for such mandated benefits such as severance pay. Being registered has a very precise meaning, since all registered workers possess what is called *Carteira de Trabalho*, loosely translated as work permit. One could define informal workers in alternative ways: workers who are not covered by the social security system and who do not make contributions to social security; workers who work in informal firms; and others. These definitions are different but they are related, and we use the one that is more appropriate for our study.

[3] The ability to have a more direct impact of enforcement on the formalization of labor contracts of informal workers is hampered by the fact that most informal workers work in informal firms, which are hard to identify, while most labor inspections target legally registered firms.

in response to an increase in benefits. We present a simple model that explains these points, and we use it to interpret our empirical results.

The main empirical challenge in our analysis comes from the fact that enforcement is not randomly distributed across cities. On one end, enforcement may be stronger in cities where reports of labor violations are more frequent. On the other end, enforcement may be stronger in cities with better institutions. In order to make progress we need a plausibly exogenous source of variation in enforcement. A natural idea is to investigate constraints to enforcement.

There are several constraints to the activity of labor inspectors, one of the most important ones being geography: a city will receive fewer visits from labor inspectors the farther it is located from an enforcement office. Furthermore, distance will be a particularly strong constraint to enforcement in states where labor inspectors are a particularly scarce resource. Therefore, in order to identify the effect of enforcement on labor market outcomes we explore the differential effect of distance on enforcement across states with differential availability of labor inspectors.[4]

Figures 1A, 1B and 1C show the intuition of our procedure. In order to construct Figure 1A, for each state, we run a regression of the degree of enforcement (measured by the log of number of inspections per firm in the city) on distance to the nearest enforcement office (measured in hours of travel by car). Each circle represents a coefficient of one of these regressions, which is plotted against the log number of inspectors per firm in the state. The size of the circle is the inverse of the standard error of the estimated coefficient. All coefficients are negative, indicating that cities located away from enforcement offices have low levels of enforcement. More importantly, these coefficients are disproportionately negative in states with low endowments of inspectors. The slope of the regression line is positive and significant.

If this is the case, we expect the relationship between distance and labor market variables of interest, such as unemployment or informality, to be more pronounced in states with low numbers of inspectors. We show that this is true in figures 1B and 1C. In drawing Figure 1B, we start by regressing, for each state, the share of informal workers in each city in 2000 on the distance to the nearest enforcement office. Then we regress the estimated coefficient for each state on the log number of inspectors per firm in the state. For Figure 1C we do the same but we

[4] A similar identification procedure is used by Rajan and Zingales (1998) who examine the effect of financial dependence on growth, Goldberg and Pavnick (2003), who study the effect of trade reform on informality, and Verhoogen (2008), who studies the impact of trade incentives on quality upgrading. Several difference-in-difference strategies (and other grouping estimators) account for location and time effects and implicitly instrument the variable of interest with the omitted interaction between location and time (e.g., see Meghir and Whitehouse, 1995).

use the unemployment rate in the city in 2000 as the outcome of interest, instead of looking at the share of informal workers. All regressions are weighted by the inverse of the estimated variance of the coefficient. Again, the slopes of the regression lines in the figures are statistically different from zero (as reported in the note of the figures).

This procedure is valid if the effect of distance on labor market outcomes does not vary across states (except through its effect on enforcement), or if this variation is not correlated with the number of state inspectors. This assumption may not hold if, for example, those cities which are far from enforcement offices are also small, rural, and remotely located, and at the same time, those states with a large number of inspectors engage in active regional policies favoring small and remote cities. One defense against this argument is that decisions about regional policy and about the number of inspectors per state are probably done by different institutions, and even at different administrative levels (state vs. federal). Our belief in the validity of this procedure can be backed by empirical evidence.

Figures 2A and 2B display to two checks of the validity of our procedure (several more are presented below in the empirical section). For several reasons, discussed in detail in the paper, labor inspections only became effective in the 1990s. Hence, we do not expect the relationship between distance to the nearest enforcement office (measured in 2002) and city level variables measured in 1980, such as the share of informal workers, or GDP per capita, to vary systematically with the number of inspectors in the state (also measured in 2002). Figures 2A and 2B (similar to Figures 1B and 1C, with different dependent variables) document that this is indeed the case (we cannot reject that the slopes of the regression lines are equal to zero).

A formal empirical analysis presented below shows that a 10% increase in the level of enforcement in a city (measured by the annual number of labor inspections per firm in the city) leads to: a 0.6 percentage point (p.p.) increase in the share of the population in formal employment; a 0.6 p.p. increase in non-employment; a 1 p.p. decrease in informal employment; an 1.8% reduction in formal wages; a 2% increase in earnings of those who are self-employed (most of whom are informal); and a reduction in inequality (measured by Theil's index). There is little change in the employment and wages of those who are informal employees. These results show that even if labor market reform has a direct impact only in the formal sector, it will strongly affect workers outside of the formal sector because of linkages across markets.

Our study is original in several dimensions, namely the use of variation in enforcement to understand the effect of labor regulation, the assembly of a new administrative dataset with information on labor inspections in each city in Brazil, and the explicit integration of the formal and informal sectors (and linkages between the two sectors) in an empirical analysis of the effects of labor regulation. However, the paper also builds on and contributes to a long literature.

The theoretical framework on which we draw upon follows Harberger (1962), Harris and Todaro (1970), Fields (1975, 2005), MacDonald and Solow (1985), Bulow and Summers (1986), Acemoglu (2001), Maloney (2004), and Albrecht, Navarro and Vroman (2006).[5] Although labor regulation is strict in Brazil, there is surprisingly large wage and employment flexibility (e.g., Barros and Mendonca, 1996, Barros, Cruz and Mendonca, 1997). The reason for this may be low enforcement. Therefore, when interpreting our findings we think of a model with minimal rigidities, except for frictions in the job search process in the formal sector and a minimum wage. More recent contributions to the literature on informality include work by Schneider and Enste (2000), Friedman, Johnson, Kaufmann, and Zoido-Lobaton (2000), Amaral and Quintin (2005), Galiani and Weischelbaum (2007), Boeri and Garibaldi (2006), Loayza, Oviedo and Serven (2005), de Paula and Scheinkman (2006), Bosch, Goni and Maloney (2007), and World Bank (2007). Especially related to us are studies of inequality in economies with dual labor markets, such as Fields (1979, 2005), or Bourguignon (1990).

Modern surveys of the role of labor market institutions include Layard and Nickell (1999), or Kugler (2007), among many others. The increasing availability of micro data lead to the emergence of several studies examining the effect of labor market regulations in developing countries, such as Kugler (1999, 2001, 2004), Kugler and Kugler (2003), Eslava, Haltiwanger, Kugler and Kugler (2006), Ahsan and Pages (2007), Petrin and Sivadasan (2006), or the studies in Heckman and Pages (2004). Two papers are especially close to ours. Besley and Burgess (2004) explore within country (district level) and across time variation in labor reforms in India to study the effect of labor regulations on productivity, investment, employment and poverty. We explore a very different source of institutional variation, and use labor market data disaggregated at the city level. Marrufo (2003) examines the consequences of the reform of social security in

[5] Several papers try to empirically distinguish different models of the labor market (segmented and non-segmented). See e.g., Dickens and Lang (1985), Heckman and Hotz (1986), Maloney (1999), Filho, Mendes and Almeida (2004), Navarro-Lozano and Schrimpf (2004), Bosch and Maloney (2006), Almeida and Bourguignon (2006).

Mexico, using a Harberger model with two employment sectors and worker heterogeneity. This paper is one of the few that considers labor market policy in a multi-sector labor market.

Finally, we relate to the large literature on the labor market effects of mandated benefits (Summers, 1989, Lazear, 1990), both in the U.S. (e.g., Gruber, 1994) and in developing countries (e.g., Gruber, 1994, 1997, Kugler, 2005, MacIssac and Rama, 1997). Relatively to this literature, our model allows the informal sector to respond to changes in mandated benefits.

This paper proceeds as follows. In the next section, we provide background information on the Brazilian labor market, its institutions, and the structure of the enforcement process. Section 3 presents the simple theoretical framework that guides our work. Section 4 describes the data. Section 5 explains the empirical strategy. Section 6 shows the empirical results, and discusses the main lessons for labor markets in developing countries. Section 7 concludes.

2. Labor Market Regulation and Enforcement in Brazil

2.1 Labor Regulations

On paper, Brazil has one of the least flexible labor market regulations in the world. The law establishes that all employees must have a work permit where the employment history of the worker is registered (*carteira de trabalho*). This permit entitles the worker to several benefits, such as a retirement pension, unemployment insurance, and severance payments. The labor code is largely written into the Brazilian constitution, which makes any amendments very difficult. The constitution of 1988 introduced several changes to the labor code, which increased the degree of worker's protection (see e.g., Barros and Corseuil, 2001). For example, the law establishes that the maximum work period is of 44 hours a week, the maximum period for continuous shift work is 6 hours, minimum overtime pay is 1.5 times the normal hourly wage, paid leave is at least 4/3 of the normal wage, paid maternity leave is 120 days, and the employer must contribute monthly to social security and to a job security fund, the FGTS. This a fund administered by the government, employers and employees, which accumulates for as long as the worker remains employed with the firm. The employer makes monthly contributions of 8% of the employee's current wage to the fund (10% from 2001 onwards).[6] Adding up all the costs,

[6] As a consequence the accumulated FGTS of a worker in a given firm is proportional to its tenure. Only workers that are dismissed for an unfair reason or those that are retired have access to this fund. Workers can also use their FGTS in exceptional circumstances like when buying a house or paying large health expenses. Upon dismissal,

in order for a worker to receive a net wage of Reais 100, the firm needs to disburse approximately Reais $165,7 (Cardoso and Lage, 2004).

Firing a worker in Brazil is not significantly more difficult than firing a worker in other Latin American countries, although it is definitely more costly. Employers must give advance notice to workers and, in the interim period, workers are granted two hours a day to search for a job. This period is never smaller than one month and recently it became proportional to workers' tenure. During this period, employers cannot change the worker's wage. This implies that approximately 25% of paid hours (2 out of 8 possible hours in each working day) are not worked. If there is a drop in motivation, the productivity of a dismissed worker also falls once he is given notice of dismissal so the overall decline to production is likely to be above 25% (Barros and Corseuil, 2001, argue that the fall in production is near 100%). Workers who are fired without cause have the right to receive compensation paid by the employer, over and above what was accumulated in the worker's job security fund (FGTS). In particular, the law establishes that a penalty equal to 40% of the fund accumulated during the worker's tenure with the firm is due to the worker.[7] Therefore, dismissal costs increase with the duration of the work contract. One obvious perverse effect of such high severance pay is that several workers force their dismissal, potentially increasing turnover rates, and increasing the firm's costs (see, e.g., Neri, 2002).

There is one final aspect that should be emphasized: severance payments received by the worker are not subject to income taxation (this is not true in most countries). This means that workers value one Real of FGTS more highly than one Real in gross salary. Moreover, firms pay taxes on profits, which can add up to more than 30%. As a result, the cost of FGTS to the firm is much smaller than the value of FGTS to the worker.[8]

2.2. Enforcement of Labor Regulations

Firms weight the costs and benefits of complying with strict labor regulation. They may decide to hire informally or to hire formal workers without complying fully with specific features of the labor code (e.g., avoid the provision of mandatory health and security conditions, or avoid payments to social security). The expected cost of evading the law is a function of the probability

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workers have access to the entire fund, including all the funds accumulated in previous jobs, plus a penalty in proportion to the fund accumulated during the tenure in the last firm.

[7] This charge was elevated to 50% after 2001 (outside our period of analysis), with the additional 10% going directly to the government. For a period after 2001, the FGTS contribution was also raised from 8 to 8.5%.

[8] Coordination between employees and firms may difficult even though there are clear gains to doing it if $v>1$.

of being caught and of the monetary value of the penalties (fines and loss of reputation). In turn, the probability of being caught depends on the firm's characteristics (such as size and legal status)[9] and on the degree of enforcement of regulation in the city where the firm is located. The Ministry of Labor is in charge of enforcing compliance with labor regulation in Brazil. Given the size of the country, enforcement is first decentralized at the state level (the state level labor office is called *delegacia*) and then at a local level, the subregion (the local labor office is called *subdelegacia*). A *subdelegacia* is located in a city, but its catchment area generally includes more than one city (or *municipio*). In each state, the *delegacia* is always located in the state capital and the number of *subdelegacias* within the state is a function of the size and economic importance of each region. For example, the state of Sao Paulo has 21 *subdelegacias* while other smaller states, like Acre or Amapa, only have one *subdelegacia*, which coincides with the *delegacia*.

Labor inspections were probably of little relevance during the 70's and 80's. In the late 80's the Brazilian economy had several hyperinflation episodes and this contributed to a significant depreciation of the nominal value of fines. However, during the second half of the 90's labor inspections gained importance. There are several reasons behind this change. On one end, labor regulation became stricter after the 1988 Constitution. One the other end, the strong government deficit in the mid 1990s lead the government to search for alternative ways to collect revenue, and labor inspectors started being used as tax collectors. Their main goal was to collect job security contributions, which helped reduce the size of the government deficit, at least in an accounting, sense (since they cannot be used directly by the government to fund its expenditure). It was probably only after this change that labor inspections gained prominence.

Inspectors are affiliated with a specific *subdelegacia* but, to deter corruption, they must periodically rotate across *subdelegacias*. The maximum period an inspector can stay in one *subdelegacia* is twelve months (Cardoso and Lage, 2007). In theory, an inspection can be triggered either by a random firm audit, or by a report (often anonymous) of non-compliance with the law. Workers, unions, the public prosecutor's office, or even the police can make reports. In practice, since the number of labor inspectors is low relatively to the number of non-compliance reports, most inspections are triggered by these anonymous reports.

[9] Cardoso and Lage (2007) argue that the integration of firms in international trade and the need to comply with international quality standards (e.g., ISO certificate) implicitly forces firms to comply with regulation. For example, it is often the case that firms who which to export need to prove their compliance with labor regulations and cannot resort to any forms of child labor or slavery.

Inspectors assess the compliance of each inspected firm with several dimensions of labor law (e.g., worker's formal registration, severance pay, minimum wage regulation, hours of work). Almost all of the targeted firms are formal firms because it is difficult to visit a firm that is not registered, since there are no records of its activity. As a result, an enormous fraction of informal employment is left out of the inspectors' reach. Inspectors face a performance based pay scheme. In particular, up to 45% of their wage is tied to the efficiency of the overall enforcement system (1/3 is tied to the inspectors own performance while 2/3 is tied to the system's global performance). Their base salary is also competitive. In 2004, their monthly wage was between USD 2,490 (starting position) and USD 3,289 (top management).

When faced with violations of the labor code, inspectors must immediately notify the firm. The firm then has 10 days to present evidence in its defense. After that period, the process is re-examined by a different inspector from the one issuing the fine, who deliberates on its fairness, and the result is reported to the head of the *subdelegacia* (*subdelegado*). If firms do not contest the fine and pay it within 10 days of their notification, there is a 50% discount on the amount of the fine. Alternatively, if firms file an appeal, they must deposit the total value of the penalty until a second decision has been reached. In practice, small and medium firms pay the fines early to take advantage of the discount. Larger firms, with their own legal departments, tend to refute the deliberations, and often avoid the payment of any fines. Fines can be either fixed, or indexed to firm size and profitability. For example, a firm is fined by Reais 446 for each worker that is found unregistered during an inspection. Depending on its size and profitability, if a firm does not comply with the mandatory contributions to the FGTS, then it can be fined an amount between Reais 16 and Reais 160 per employee.[10]

Although the number of inspectors was relatively low in the early 2000s when compared with a decade before, inspectors were able to reach a significant part of the total labor force in formal firms in Brazil. In 2002, 304,000 firms were visited by labor inspectors, reaching more than 19,000,000 workers (Cardoso and Lage, 2007). Of these, approximately 17% of the firms received a notification of non-compliance with the law, but less than 3% of the workers were registered as a result, a small number given that 50% of employment is informal in Brazil. This could reflect the fact that informal workers are concentrated in small and informal firms outside

[10] Cardoso and Lage (2007) argue that the magnitude of the fines is quite reasonable to work as a deterrent to crime, and that the main problem is their enforcement.

the reach of labor inspectors, but it may also suggest that, among the different types of violations of labor law,[11] formalization is not the sole (or even the main) target of the inspections. According to Cardoso and Lage (2007), the main target for labor inspectors is the lack of payment of the job security fund and compliance with health and safety conditions on the job.

The Ministry of Labor makes an effort to apply homogeneous criteria for enforcing labor regulation throughout the country (e.g., by providing training and using similar software) but, in practice, this is very difficult to achieve because the country covers a very large and diverse geographical area. Inspectors are also likely to be very heterogeneous. Moreover, they have to travel different distances and face varying workloads depending on where they are located. This will give rise to substantial regional variation in the degree of enforcement across cities, which we explore econometrically.

3. Theoretical Background

In interpreting our findings we consider a simple two sector model of the labor market, drawing on Lewis (1954), Harberger (1962), Harris and Todaro (1970), Fields (1975), MacDonald and Solow (1985), Bulow and Summers (1986), and Maloney (2004). There is also an important literature integrating search and informality, namely Acemoglu (2001), Albrecht, Navarro and Vroman (2006), and Bosch (2007).

We start with a simple (general equilibrium) model with a formal and an informal sector, and no minimum wage (which will be introduced later). W_F and W_I denote wages in the formal and informal sectors, respectively. For simplicity, employers can hire formal and informal workers simultaneously. Employers hiring formal workers face taxes T, so the cost of labor is: $W_F + T$. T can translate into benefits for employees (e.g., social security, severance pay, health and safety conditions), and the value of T for formal employees is vT, where $v \geq 0$ (v can be smaller, equal or even larger than 1). It is illegal to operate in the informal sector, and therefore employers face an expected penalty of P per worker employed in that sector (where P is the product of the penalty and the probability of being caught). We focus on the hiring decisions

[11] All violations are punishable with fines. Inspectors issue fines for the non-registration of workers, disobedience of the official work period or hours worked, non-compliance with the mandatory wage payments (including minimum wages), missing FGTS contributions or health and safety violations. It is useful to note that fines may be inaccurate measures of enforcement for two reasons. First, we only see a fine if a violation is detected, and much enforcement may have a deterrent effect not translated into fines. Second, inspectors avoid issuing fines, and try to first negotiate with the firm non-litigious ways to solve the illegality they observe (Cardoso and Lage, 2007).

only, and ignore the decision of the firm to be formal or informal.[12] Finally, we consider a (residual) household sector, which absorbs the non-employed population (e.g, individuals who decide not to work because their reservation wage is higher than the market wage in each sector). The total number of individuals in the economy is *N*, who can be either working in the formal sector (N_F), working in the informal sector (N_I), or non-employed (N_H). Labor markets are competitive, and equilibrium wages and quantities of labor in each sector are determined by the intersection of supply and demand.

We start by modeling an increase in enforcement as an increase in *T* since, as explained in section 2, most of the enforcement activity in Brazil concerns: i) guaranteeing the payment of contributions to the severance pay fund, as well as compliance with firing rules and payments; ii) health and safety conditions. It is also possible that enforcement increases the cost of hiring informal workers, by making detection more probable, corresponding to an increase in *P*, so we examine this case later. We represent the labor market with the following equations:

$$\text{Demand for Formal}: D_F = a\text{-}b(W_F + T) + c(W_I + P) \quad (1)$$

$$\text{Demand for Informal}: D_I = e + f(W_F + T) - g(W_I + P) \quad (2)$$

$$\text{Supply of Formal}: N_F = h + i(W_F + vT) - jW_I \quad (3)$$

$$\text{Supply of Informal}: N_I = k - l(W_F + vT) + mW_I \quad (4)$$

$$\text{Equilibrium in Formal}: D_F = N_F \quad (5)$$

$$\text{Equilibrium in Informal}: D_I = N_I \quad (6)$$

$$\text{Resource Constraint}: N_F + N_I + N_H = N \quad (7)$$

Equations (1) to (4) characterize the demands and supply for each type of labor. *T* and *P* depend directly on enforcement *E*, and for now it will be convenient to define *T=E* and *P=0*. Equations (5) to (7) characterize the equilibrium. With the exception of the intercepts of the equations, it is natural to assume all the parameters are positive (if the two types of labor are substitutes). This formulation is arbitrary, but it is possible to derive demand and supply equations for each labor market from a model where individuals maximize utility and firms maximize profits. The assumption of linearity simplifies our calculations and does not affect our main conclusions.

[12] A firm is generally defined to be formal if it pays taxes (e.g., De Paula and Scheinkman, 2006).

Differentiating the system above with respect to an increase in enforcement ($T=E$), and denoting derivatives as lower case letters (e.g., $d_p = \frac{dD_p}{dT}$):

$$d_F = -b(w_F + 1) + cw_I \quad (8)$$
$$d_I = f(w_F + 1) - gw_I \quad (9)$$
$$n_F = i(w_F + v) - jw_I \quad (10)$$
$$n_I = -l(w_F + v) + mw_I \quad (11)$$
$$d_F = n_F \quad (12)$$
$$d_I = n_I \quad (13)$$
$$n_F + n_I + n_H = 0 \quad (14)$$

The solution to this system is complex. One way to greatly simplify our calculations is to set $c=f=0$ (no cross-sector linkages in demand), which is an unrealistic assumption but helps us gain some insights about mechanics of this system. The solution to equations (8)-(14) in this case becomes:

$$d_F = \frac{(1-v)b(jl - im - ig)}{(b+i)(g+m) - jl} = n_F \quad (15)$$
$$d_I = \frac{(1-v)lbg}{(b+i)(g+m) - jl} = n_I \quad (16)$$
$$n_H = \frac{(1-v)[bi(g+m) - bl(g+j)]}{(b+i)(g+m) - jl} \quad (17)$$
$$w_F = \frac{vjl - (b+vi)(g+m)}{(b+i)(g+m) - jl} \quad (18)$$
$$w_I = \frac{-(1-v)bl}{(b+i)(g+m) - jl} \quad (19)$$

The denominator in all these expressions is positive if $im-jl>0$, which should happen unless cross effects of wages in the supply equations (i.e, j and l) are very strong (stronger than own effects, i.e., i and m). Below we assume this condition holds, and also that $i>j$. The sign of expressions involving $(1-v)$ depends on whether v is smaller or larger than 1 (i.e., whether the valuation workers place on mandated benefits is smaller or larger than their cost to employers). Finally if we examine some of the central components in the numerators of the equations above (writing the equation numbers below and the relevant numerator next to it):

$(15): b(jl - im - ig) < 0 \; if \; im > jl$

$(16): lbg > 0$

$(17): bi(g+m) - bl(g+j) > 0 \; if \; im - jl \; and \; i > j$

$(18): vjl - (b+vi)(g+m) < 0 \; if \; im > jl$

$(19): bl > 0.$

This means that:

$d_F, n_F \leq 0 \; if \; v \leq 1; \; d_F, n_F > 0 \; if \; v > 1$

$d_I, n_I \geq 0 \; if \; v \leq 1; \; d_I, n_I < 0 \; if \; v > 1$

$n_H : \geq 0 \; if \; v \leq 1; \; n_H < 0 \; if \; v > 1$

$w_F \leq 0$

$w_I \leq 0 \; if \; v \leq 1; \; w_I > 0 \; if \; v > 1.$

In this simple model, as a result of an increase in enforcement we expect a contraction in the labor demand curve because formal workers become more expensive. We also expect an expansion of the formal labor supply curve, since formal jobs become more attractive. Taking v=1 provides a useful baseline case, in which employers and employees put exactly the same valuation in those benefits mandated by regulation. If there were no wage rigidities, then the equilibrium wage in the formal sector would decrease by an amount equal to the cost of the mandated benefits, with no change in formal employment (Lazear, 1990), and no change in the informal sector. This case is depicted in figure 3 which plots the demand and supply of workers in the formal (D_F and S_F) and informal sectors (D_I and S_I), in economies with and without enforcement (e.g., S^E_F vs. S^{NE}_F). [13]

Several empirical papers estimate the extent to which payroll taxes and mandated benefits translate into lower wages to be quite large (e.g., Gruber, 1994, 1997, Marrufo, 2001, Kugler, 2005, Heckman and Pages, 2003).[14] At the bottom of the wage distribution, it is likely that the pass-through rate is below 100%, because of downward wage rigidity due to, say, a minimum wage (we discuss this case below). At the top of the wage distribution, it is possible that it is

[13] The assumptions here are absence of asymmetric information between workers and firms, wage rigidity, or credit constraints. In this case mandated benefits that are valued equally by employers and employees are borne by workers in the form of lower wages, and have no effects on employment. While these assumptions may hold at the top of the wage distribution, they are unlikely to be true at the bottom, which can lead us to see some effects of mandated benefits on employment, as we show below in a model with a minimum wage (e.g., Summers, 1989, Mitchell, 1990, Lazear, 1990).

[14] Heckman and Pages (2003) estimate rates of pass-through close to 90% in OECD countries, while Marrufo (2001) and Kugler (2005) have estimates closer to 60-80% for Mexico and Colombia. Gruber (1994, 1997) stands out for estimating 100% pass-through rates, for the US (maternity benefits) and Chile (payroll taxes).

close to 100%, especially for job severance pay since workers can easily gain access to the job severance fund.

It is even plausible that $v>1$ in Brazil (which would imply larger than 100% pass through rates in this model) if: i) firms pay taxes on profits but workers do not pay taxes on severance payments (this is the case in Brazil), which means that for each Real the worker receives as severance pay (net of taxes) the firm needs to disburse less than one Real; ii) the costs of providing better health and safety conditions on the job are below the value workers place on them. This case is shown in figure 4. In this case, we would also expect total employment to increase since jobs in both the formal and informal sectors are more attractive.[15]

It is also possible that $v<1$. Workers may not be fully informed of their rights to severance pay, and they may perceive the probability of ever using the amount on their severance pay fund to be below 1. Our empirical results will help us discern the most plausible scenario.

Let us assume now that, in our model, an increase in enforcement translates into an increase in the parameter P. In this case, firms are urged to reclassify their informal workers as formal (under the penalty of being fined), and then comply with social security, payroll, or severance payments. Therefore, there will be a contraction in the demand for informal workers. The result would be a shift in employment from the informal to the formal sector, and a decline in wages in both sectors. This is shown in figure 5 in a very simplistic way, since we do not allow the full equilibrium effects to take place in the figure (we keep the demand for formal workers and supply of informal workers fixed).[16]

It is also important and realistic to consider a case where there is downward wage rigidity at least in the formal sector, due to the existence of a minimum wage (although there could be other reasons for downward wage rigidity).[17] If the minimum wage is binding, there will be involuntary unemployment in the formal sector, and a queue for formal sector jobs. One simple way to incorporate this in the model is to assume that workers are risk neutral in the sense that

[15] Recall that, for simplicity, we assume there is no change in the demand for informal labor ($c=f=0$). In a more general model we would expect the equilibrium demand curve for informal labor in an economy with enforcement to the left of the original curve, since the equilibrium cost of formal labor (inclusive of the cost of mandated benefits) is below what it was in an economy without enforcement.

[16] Formally, P is analogous to T, but the analysis is simpler since workers place no value on P). For brevity, we omit the full analysis from the paper.

[17] Based on the evidence discussed in Cardoso and Lage (2007), we assume that an increase in enforcement translates mostly to an increase in the compliance with mandated benefits (through severance pay, health and safety conditions). The authors do not argue that enforcement translates into additional compliance with the minimum wages and, thus, we do not explore this channel in our model.

they only care about the expected wage in the formal sector (the formal wage times the probability of employment given that one has joined the queue), that they can only queue for a formal sector job if they are unemployed, and that they are selected from the queue at random. This model is reminiscent of Harris and Todaro (1970) and subsequent work.

Assuming that the minimum wage is binding, the main equations for this model are:

$$\text{Demand for Formal}: D_F = a\text{-}b(\overline{W_F} + T) + c(W_I + P) \qquad (20)$$

$$\text{Demand for Informal}: D_I = e + f(\overline{W_F} + T) - g(W_I + P) \qquad (21)$$

$$\text{Supply of Formal}: N_F = h + i(\overline{W_F} + vT)(1-U) - jW_I \qquad (22)$$

$$\text{Supply of Informal}: N_I = k - l(\overline{W_F} + vT)(1-U) + mW_I \qquad (23)$$

$$\text{Equilibrium in Formal}: D_F = N_F(1-U) \qquad (24)$$

$$\text{Equilibrium in Informal}: D_I = N_I \qquad (25)$$

$$\text{Resource Constraint}: N_F + N_I + N_H = N \qquad (26)$$

Relatively to the model in equations (1)-(7), $\overline{W_F}$ is the binding minimum wage, N_F is the number of individuals willing to queue for a job in the formal sector, and *U* is the proportion of such individuals who become formal workers (1-*U* is the proportion who remain in the queue, and who are unemployed).

As above, assume that there are no cross effects in demand (*c=f=0*) and that own effects of wages on supply are stronger than cross effects (*im-jl>0, i-l>0*). Then, differentiating with respect to *T*, and solving the system (using lower case letters to denote derivatives):

$$d_F = \frac{-N_F b(g+m) - (1-U)(\overline{W_F} + vT)b[jl - i(g+m)]}{N_F(g+m) + (1-U)(\overline{W_F} + vT)[i(g+m) - jl]} < 0 \qquad (27)$$

$$n_F = \frac{[b(\overline{W_F} + vT) - vN_F(1-U)][jl - i(g+m)]}{N_F(g+m) + (1-U)(\overline{W_F} + vT)[i(g+m) - jl]} \lessgtr 0 \text{ if } b(\overline{W_F} + vT) - vN_F(1-U) \gtrless 0 \qquad (28)$$

$$d_I = n_I = \frac{[b(\overline{W_F} + vT) - vN_F(1-U)]gl}{N_F(g+m) + (1-U)(\overline{W_F} + vT)[i(g+m) - jl]} \gtrless 0 \text{ if } b(\overline{W_F} + vT) - vN_F(1-U) \gtrless 0 \qquad (29)$$

$$n_H = \frac{[vN_F(1-U) - b(\overline{W_F} + vT)][jl - im + g(l-i)]}{N_F(g+m) + (1-U)(\overline{W_F} + vT)[i(g+m) - jl]} \gtrless 0 \text{ if } b(\overline{W_F} + vT) - vN_F(1-U) \gtrless 0 \qquad (30)$$

$$u = \frac{b(g+m) - v(1-U)^2[jl - i(g+m)]}{N_F(g+m) + (1-U)(\overline{W_F} + vT)[i(g+m) - jl]} > 0 \qquad (31)$$

$$w_I = \frac{[vN_F(1-U) - b(\overline{W_F} + vT)]l}{N_F(g+m) + (1-U)(\overline{W_F} + vT)[i(g+m) - jl]} \lessgtr 0 \text{ if } b(\overline{W_F} + vT) - vN_F(1-U) \gtrless 0. \qquad (32)$$

The number of workers employed in the formal sector declines. Given that wages are fixed at the minimum wage (assuming that it is binding) and that there is an increase in mandated benefits, firms want to hire less formal workers. As a result, for a fixed informal wage, there is an increase in the unemployment rate in the formal sector (*U*). Formal jobs are now more attractive if you are able to get them, but it becomes less likely that someone in the queue for formal jobs is able to start working in the formal sector. The remaining derivatives in the system have an ambiguous sign, which depends on the following partial equilibrium question: if informal wages were kept fixed, would the formal sector be more or less attractive after the increase in enforcement? It is possible to show that the answer to this question depends on the sign of $b\left(\overline{W_F}+vT\right)-vN_F\left(1-U\right)$ (which is crucial for equations 28, 29, 30, and 32).[18] If the formal sector becomes more attractive with the additional enforcement then the proportion of workers in the household and informal sectors declines, and the informal wage increases. This is to prevent all informal sector workers from moving to the formal sector. The opposite happens if the formal sector becomes less attractive.[19]

In a simple competitive model, enforcement is always distortionary and welfare reducing. More generally, the welfare implications of increases in enforcement are mixed. The standard view is that taxes and mandates impose distortions and reduce welfare. However, if formal jobs are intrinsically more productive than informal jobs, there may be a role for promotion of formality (Acemoglu, 2001), as long as it does not involve pure reclassification of workers doing

[18] Suppose that $w_I = 0$ (this would be a partial equilibrium argument). Then the equations of the model are just (15), (17) and (19) with the caveat that $w_I = 0$ (and we will continue to assume that *c*=0). Taking derivatives and solving for *u* we get: $u = \frac{b + iv\left(1-U\right)^2}{i\left(\overline{W_F}+vT\right)\left(1-U\right)+N_F}$. The attractiveness of the formal sector is measured by the expected wage one faces if one decides to search for a formal job, $\left(\overline{W_F}+vT\right)\left(1-U\right)$, so we ask: what is the sign of $\frac{\partial\left(\overline{W_F}+vT\right)\left(1-U\right)}{\partial T} = v\left(1-U\right)-u\left(\overline{W_F}+vT\right)$? Substituting *u* for the expression above we get that $\frac{\partial\left(\overline{W_F}+vT\right)\left(1-U\right)}{\partial T} \geq 0 \; if \; b\left(\overline{W_F}+vT\right)-vN_F\left(1-U\right) \leq 0.$

[19] This model assumes that there can be no search in the formal sector while employed in the informal sector. While this is restrictive, we would have similar predictions from a model where it is possible to search while employed in the informal sector, but the probability of a successful search is smaller than if search is done while unemployed.

the same job. Similarly, if workers are credit constrained, mandated benefits such as severance payments may be welfare enhancing (Alvarez and Veracierto, 2001).[20]

In our empirical work we will divide the informal sector in two: informal wage earners and self-employed. This distinction may be important if, following some authors, there is duality within the informal sector (e.g., Fields, 1990, 2005, Maloney, 2004). While it is true that there is a group of informal workers who could be working in the formal sector if that was their choice, there is another group which operates in a segmented labor market, queuing for a formal sector job (as in the more traditional view of the informal sector; e.g., Dickens and Lang, 1985).[21] In the first model we presented in this section there is perfect mobility between the formal and informal sectors, while in the second model there is some mobility but workers may be forced to queue for a job in the formal sector while being unemployed. We do not model an informal sector completely segmented from the rest of the economy (but we allow for it in the empirical work).

4. Data

The paper explores several sources of data. First, we use administrative data on the enforcement of labor regulations (in 2002), collected by the Department of Inspections at the Ministry of Labor for our project. This data contains information on the number and location of regional labor offices, number of inspected firms, number of fines issued in each city, and number of inspectors per state. Our measure of enforcement is the log number of inspections in each city (plus one) minus the log of the number of firms in the city (log inspections per firm in the city).

Second, we compute several city level labor market indicators using the 10% sample of the Brazilian Census in 2000, containing detailed information on labor market outcomes for 15 million individuals. In particular, we compute the share of workers who are registered, unregistered, or self-employed, the share of non-employed, average wages for each type of worker, and measures of income and wage inequality in the city (including several percentiles of the income and wage distributions, and the city 90-10 income and wage ratio). We also compute similar statistics for individuals in different gender, age and education groups. Table A2 reports

[20] Several authors consider non-competitive models of the labor market, in which firms have some monopsony power. In these models mandated benefits can increase the bargaining power of workers, allowing them to increase their total compensation package (e.g., Saint-Paul, 1995, Ljunqvist, 2002). As a response, there can be an increase in the wage of informal workers to keep them indifferent across sectors.

[21] In the empirical work we will not be able to rigorously distinguish between upper and lower tier informal workers. However, there is a suggestion in the literature that informal wage earners belong in the lower tier, while part of self-employed workers are likely to be in the upper tier (e.g., Bosch and Maloney, 2006).

the proportion of the adult population in each employment category.[22] Registered and unregistered wage earners, self-employed, and non-employed individuals, together account for 87% of the adult population.[23] Therefore, in the empirical work we focus on these four groups. Informal employment and self-employment are considered two separate categories, as emphasized in the recent literature (Maloney, 2004, Fields, 1990, 2005). Finally, we have also computed some measures of past informality, poverty and inequality in the city using the 1980 Brazilian census.[24] In 2002, there are 5,513 cities in Brazil.

Third, we use detailed information on other city level characteristics from two statistical and research institutes in Brazil - *Instituto de Pesquisa Economica Aplicada* (IPEA), and *Instituto Brasileiro de Geografia e Estatistica* (IBGE).[25] In particular, we collect information on the city's GDP per capita (2000), total number of firms (2000), average firm size (2000), share of agriculture in GDP (2000), share of manufacturing in GDP (2000), share of services in GDP (2000), geographical city characteristics (including geographical area, altitude, longitude and latitude), city transportation costs (1995), total federal transfers to each city (1990), the city head count poverty index and the city Theil inequality index. The total number of firms (2002) in each city comes from the *Cadastro Central de Empresas*, collected by *IBGE*, which only includes formal firms. We also use past city level variables published by IPEA for the years 1970, 1980, and 1991, including city population, per capita income, average years of schooling and share of population in urban areas. Because some of the cities in 2000 did not exist in the 70's, 80's or even 1991, we use the more aggregate definition of minimum comparable unit (MCU), published by the IPEA, to obtain an estimate of these city variables in previous years.[26] For all cities in a given year, we know to which MCU each city was previously mapped into. Then, we computed

[22] In the 2000 Census, each individual is classified into one of the following 10 categories: registered domestic worker, unregistered domestic workers, registered wage earner, unregistered wage earner, employer, self-employed, unpaid apprentice, unpaid employee (usually in family business), working for self-consumption, and without status (or not employed).

[23] The remaining 13% are formal and informal domestic employees (0.8% and 2.5% respectively), employers (1.5%), interns or apprentices (0.1%), unpaid employees (3.5%), and individuals working only for self-consumption (4.6%). These individuals are excluded from our analysis. These are small groups of the population and unlikely to be too much affected by changes in enforcement.

[24] In the 1980 Census there is no information on the whether the worker has an official work permit. Instead, the survey collects information on whether the worker makes social security contributions. Hence, in 1980 the definition of informal worker differs from the one used in 2000. In 1980 a worker is considered informal if he/she do not make any social security contributions. We expect the two definitions to be correlated, since almost no unregistered workers pay social security contributions.

[25] These statistics are publically available at http://www.ipeadata.gov.br and http://www.sidra.ibge.gov.br/.

[26] In 1970 and 1980 there existed 71% and 72% of the cities that existed in 2000, while in 1991 there existed 82% of the cities in 2000. A MCU is an area (set of cities) which is defined in such a way that can be compared over time.

the average value of each variable for each MCU (weighted by population size in each city), and assigned it to each city in the MCU.

Fourth, we use information on the institutional development of the city, published by IBGE, used in Naritomi, Soares and Assuncao (2007), and kindly made available by the authors. These measures include an index of the access to justice in the city, an index of managerial capacity in the city and an index of political concentration in the city (based on a Hirshman-Herfindhal index of the shares of the political parties). The index of managerial capacity in the city measures the quality of local administration, and is used by the Ministry of Planning to monitor the administrative performance of cities. Access to justice measures the penetration of the rule of law, in particular the existence of courts or justice commissions in the city. We also consider state aggregates of these variables, by averaging across cities.

Fifth, we compute the distance and travel time (by car) between each city and the nearest *subdelegacia* in the state. The transportation of inspectors from the *subdelegacia* to each firm is made using ground transportation, usually car. Hence, the enforcement of labor regulation will be easier and less costly the closer a *subdelegacia* is from the city where the firm is located. We construct a measure of the accessibility of inspectors to firms by using the travel time from each city to the nearest *subdelegacia* within the state (minimum distance). Data on travel times and travel distances between any two Brazilian cities is available from one of the largest Brazilian auto insurance companies (BB), which collects very detailed information on distances across cities.[27] When firms are located in cities that have a *subdelegacia* the measure assumes the value zero. We also construct the distance between each city and the state capital. In the remaining of the paper we focus on travel time as the most relevant measure of distance. A third measure of the remoteness of the city, or of its access to markets, is an index of transportation costs between each city and the nearest capital city taken from IPEA (1995). Sample statistics for the main variables we use are presented in table 1.

There are time discrepancies between the different variables. Notably, enforcement is measured in 2002, while labor market outcomes are measured in 2000. This is due to limitations

[27] This information is available online at www.bbseguroauto.com.br. When collecting information on distances. We have faced two obstacles First, could not find information online for those cities that have only recently been recognized as cities. In these cases, we have located the closest nearby city (using maps) and used that information instead. Second, most on the cities in Amazonas use the maritime rather than the ground transportation both for goods or persons. Hence, the travel distance by car is meaningless for this state and, hence, we have excluded it from the analysis.

in our data, since we were only able to collect enforcement data for 2002. Nevertheless, given that we rely mainly on cross-sectional variation (in distance and the availability of inspectors) to identify our main models, we believe this is not an important concern. The reduced form relationship between distance, availability of inspectors, and labor market outcomes does not suffer from this problem. Furthermore, the level enforcement is likely to be highly correlated over time within the same city, and so are labor market outcomes. We explain below that our estimates should be interpreted as long run (perhaps even steady state) effects of enforcement on labor market outcomes. Under this interpretation, measuring enforcement in 2002 instead of in 2000 should not be a substantial problem.

5. Empirical Strategy

Our main empirical specification is the following:

$$Y_{ij} = \alpha + \beta E_{ij} + \delta X_{ij} + \eta_j + u_{ij} \qquad (33)$$

where Y_{ij} is the outcome of interest in city i and state j, E_{ij} is enforcement in city i and state j, X_{ij} is a vector of city level controls, η_j is a state fixed effect, and u_{ij} is the residual. β is the parameter of interest and measures the impact of enforcement on labor market outcomes. The main outcomes we consider are the share of informal workers in the city, poverty, inequality and unemployment, and earnings and employment of formal, informal, and self-employed workers. Enforcement is measured with the logarithm of the number of inspections per firm in the city (computed as the number of visits by labor inspectors plus one, divided by the number of firms).

For some labor market outcomes (such as the proportion of formal workers), it is possible to relate β to equations (15) or (27) (one important difference being that in (27) β is a function of T, which means that (33) should be nonlinear in T). There are, however, two concerns. First, for some outcomes such as poverty or education we do not have an explicit model anywhere in the paper. Equation (33) should be seen as a reduced form equation, but we can easily interpret the resulting estimates. Second, as mentioned before, in this section we consider two types of informal workers: informal wage earners and self-employed. This distinction is important empirically and may be justified with models of duality within the informal sector (referenced above), but the theoretical model of section 3 is not rich enough to capture it (lumping together all informal workers in a single sector), and introducing this in the model would complicate it. It

is usually said there is an upper tier of informal workers who can freely move between the formal and informal sectors and a lower tier who cannot move out of the informal sector.

Our empirical findings below suggest that enforcement of labor regulation induces some changes in employment status between being a formal employee and self-employed, although not the transitions into and out of being an informal employee. We interpret this evidence as being suggestive that self-employed workers may tend to be mainly in the upper tier, and informal wage earners in the lower tier of the informal sector.[28]

Estimating equation (33) using ordinary least squares may result in biased estimates of β since E_{ij} is potentially correlated with u_{ij}. There are two main reasons for this concern. First, enforcement may be stricter in cities where violations of labor law are more prevalent. This could happen because inspections are triggered mainly through reports of illegal activity. Second, enforcement may be stricter in cities where institutions are better developed. Intrinsic violations of the labor law, or better developed institutions, are probably correlated with labor market outcomes.

In order to address this problem we studied the constraints to enforcement throughout Brazil. There are several reasons why enforcement varies across cities, one of the most important ones being geography: a city will receive fewer visits from labor inspectors the farther it is located from an enforcement office. Furthermore, distance will be a particularly strong constraint to enforcement in states where labor inspectors are a particularly scarce resource. It is the differential effect of distance on enforcement across states with differential availability of labor inspectors that we use to identify the effect of enforcement on labor market outcomes.

In practice, the procedure is as follows. We start by collecting data on two determinants of enforcement: the distance between each city and the nearest regional enforcement office, and the number of labor inspectors in each state. Either of these measures on its own would be controversial if used as an instrument for enforcement: enforcement offices locate in relatively large city which have different labor markets than smaller and more remote cities, and states with large numbers of inspectors (after normalizing this measure by the number of firms in the state) may be states where violations of labor law are especially important. Therefore, we prefer to include both variables in the regression.

[28] Although we could have considered self employed and workers "without a carteira de trabalho" in the same category (and our main messages would hold), we believe this is a more transparent presentation of the results.

We instrument the degree of enforcement in each city with the interaction between distance and the number of inspectors (per firm) in each state (which is a measure of distance *adjusted* by the local availability of inspectors), while controlling for distance and state fixed effects, in addition to a very rich set of city level controls (some of which are also interacted with state level characteristics). State fixed effects account for the fact that states with different numbers of inspectors per firm may also be different in other dimensions, while distance to the nearest enforcement office accounts for the non-random location of enforcement offices. Any remaining variation is given by the differential effect of distance across states with varying numbers of inspectors. Distance will be a greater constraint to enforcement in cities where the supply of labor inspectors is smaller, and therefore it should have a disproportionately large effect on enforcement (and labor market outcomes) in states where the number of inspectors is low. Below we discuss in detail some of the main concerns with this empirical strategy and we show why they are unlikely to be important. Figures 1 and 2, discussed in the introduction, clearly show the intuition behind our method.

We include as additional controls several city level characteristics: income per capita, population size, average schooling, and share of the population living in urban areas in 1970, 1980 and 1991, city latitude, longitude, altitude and area, and two measures of institutional development in the city, taken from Naritomi, Soares and Assuncao (2007). Finally, one could be concerned that the number of state level labor inspectors is simply correlated with other state level characteristics, like its level of development or institutional quality, which interacted with distance, could also affect the city level outcomes of interest. Therefore we include in the model the interaction between distance to the enforcement office and other state characteristics: the log of the average of per capita GDP in the state between 1970 and 2000, and measures of city level institutions averaged at the state level (access to justice, governance and political concentration). Other controls are distance to the state capital and log of transportation costs to the nearest capital interacted with the four variables above, and with the log of the number of inspectors per firm in the state.

Table 2 provides formal evidence that the interaction between the number of inspectors in the state and distance from each city to the nearest enforcement office is uncorrelated with several city level variables proxying institutional quality or different dimensions of regional policy. One way to think about confounding interactions between other state characteristics and

distance to large city centers on one side, and our instrument on the other, is to consider the role of state level policies to reduce regional inequality (associated with distance to large cities). One possibility is road construction, but since we measure distance to the nearest *subdelegacia* of the Ministry of Labor in travel time by car (not in miles), the quality of the road infrastructure is already accounted for. Since the transportation of goods in Brazil is done mainly by train, we investigate whether the interaction of distance and state inspectors per firm affected the likelihood of each city to have a train station. The coefficient is negative but statistically insignificant. Second, we checked whether enforcement could be capturing variation in the quality of other city institutions. If states with more inspectors per firm tried to minimize the impact of distance to focal cities on the access to institutions, this correlation would be present even after we instrument labor inspections. We proxy city level institutional quality using three indices: access to justice, governance, and political concentration. The empirical findings do not show evidence that this is a significant source of concern.

Third, we look at city level inequality in social infrastructure, measured by the log number of households with access to piped water, sanitation, and electricity (normalized by the number of individuals in the city). We find no correlation between the instrument and access to water and sanitation. There is a small correlation with access to electricity, but it has the opposite sign to what one would expect if it were capturing confounding variation in other state policies. Moreover, looking directly at the log of current transfers from states to cities (drawn from state tax revenues) per capita, we find no strong correlation between our instrument and this variable.

Fourth, we assessed whether the instrument is correlated with the enforcement of other types of law, by looking at the number of homicides per 100,000 individuals in the city, and again we found no statistically significant effects.

Fifth, the level of development of the state may itself be inequality reducing and could be correlated with the number of available inspectors per state. For example, in more developed states the quality of (private) transportation may be better so that roads are less of an obstacle, and goods and information may flow easily across cities (even if they are remote). This may affect the way economic activities are distributed across cities. The first thing to notice is that the instrument is not correlated with either city size (measured by log population) or log GDP per capita. More interestingly, when we use as the dependent variable the shares of GDP attributed to agriculture, industry and services, these are also not correlated with our instrumental variable.

Therefore, the basic structure of economic activities in the city is not substantially affected by the variable we use to instrument enforcement, although (as we will show in the next section) the structure of the labor market will see some changes.

Finally, we show that the instrument cannot predict past values of the main variables of interest in this paper: city level informality (the share of workers not paying social security), the unemployment rate, inequality (Theil index), and the poverty rate measured in 1980. In sum, this table shows that the instrument is both theoretically plausible and empirically credible.[29]

Table 3 reports estimates for the coefficient on the instrument, and the average marginal effect of distance on enforcement. The relevant F-statistic measuring the strength of the first stage relationship is shown at the bottom of each column. Since we are using a large set of controls, for transparency we report three different specifications. In the first column we regress enforcement (measured by the log number of inspections per firm in the city) on distance to the nearest labor office (measured in travel time), its interaction with the number of labor inspectors per firm in the state (the instrument), and state fixed effects. In the second column we add distance to the state capital and its interaction with state inspectors per firm. In the third column we present the full specification. Across columns, the marginal effect of distance on enforcement is negative, and the coefficient on the interaction of distance and inspectors in the state is positive, showing that the effect of distance is smaller in states with more inspectors. The coefficient on this interaction is similar across columns, and the F-statistic is always high so there is no concern with the instrument being weak (Stock and Yogo, 2003).

6. Empirical Findings

6.1 Aggregate Labor Market Outcomes

Table 4 reports least squares (panel A) and instrumental variables (panel B) estimates of the effect of log inspections per firm in the city on the share of informal workers (defined as those without a work permit and the self-employed, constructed from the Census), the head count

[29] One could be concerned that the interaction term we emphasize is hard to identify in small samples, and that the findings in table 2 are mainly due to imprecision. This is not likely to be the case. In appendix table A4 we show not only the coefficient on the interaction of distance and state inspectors, but also the coefficients on the interactions between distance and all the other state level variables, which we use as control variables in table 2. The only interaction term that is systematically unrelated with the dependent variables in table 2 is the one we use as an instrument for enforcement.

poverty ratio, the unemployment rate, and the Theil inequality index in the city (variables taken from IBGE). The controls and instruments were described in detail in the previous section and are also described at the base of the table. The instrumental variable estimates shows that a 10% increase in the log of inspections per firm in the city (corresponding to 0.1 standard deviations of the variable, or sd) leads to a 1.5 percentage points (or pp), reduction in the proportion of informal workers in the city (0.1 sd), a 0.9 pp (0.15 sd) increase in the unemployment rate, a 0.012 (0.1 sd) reduction in the Theil inequality index, and a 0.5 pp (0.025 sd) reduction in the poverty rate.

The IV estimates are larger in absolute value than the OLS estimates.[30] This suggests that cities with more crime also have stricter enforcement, which could happen because inspection activities respond to complaints about illegal behavior, and these are more common in places with more violations of the law. Table A5 (in the appendix) shows that the findings in table 4 are robust to the inclusion of city sectoral composition, average firm size in the city, and additional worker variables as controls, two variables which may be correlated with the structure of the labor market in the city.

6.2 Wages and the Distribution of Employment across the Formal and Informal Sectors

While the results we just reported are interesting, a more detailed analysis of the labor market is needed to understand the mechanisms behind them. This section investigates the effect of enforcement on the quantities and prices in each sector of the labor market (constructed from the Census). In particular, we examine movements in the proportion of workers who are formal, informal, or self-employed, and in the distribution of wages for each of these groups.

Table 5 reports the effect of enforcement on the share of the adult population in the city in each employment category, in 2000. In cities with stricter enforcement there is more formal employment, more non-employment, and less self-employment. There is no statistically significant effect of enforcement on the number of informal wage earners. If workers shift

[30] It is not surprising that OLS estimates of equation (1) are biased, because inspections are triggered through (anonymous) reports of violations of the law. Furthermore, we can also show that enforcement is strongly related with almost all institutional variables in Table 2. Therefore, IV will be different from OLS. It is also possible that the effect of enforcement varies across cities, in which case the IV estimate is a weighted average of effects (e.g., Imbens and Angrist, 1994, Carneiro, Heckman and Vytlacil, 2006). Indeed, if we include in the model an interaction of enforcement with, for example, the share of informal workers in the city in 1980, we find that the effect of enforcement declines with the level of past informality in the city (result available on request from the authors).

sectors in response to changes in enforcement, then the composition of workers in each sector is bound to be affected as well. In table 6 we estimate the effect of enforcement on the distribution of schooling for each type of workers. Each column refers to a different quantile of the schooling distribution, which is used as the dependent variable in the regressions. The table shows a statistically significant decrease in the education of formal workers, and a statistically significant increase in the education of self-employed individuals. This suggests that those individuals leaving self-employment to join the formal sector come from the bottom of the distribution of schooling in both sectors.

As argued in section 3, mandated benefits may have differential effects across the wage distribution (because the minimum wage can be a source of downward wage rigidity for low wage formal workers). Therefore, we analyze different percentiles of the wage distribution, as opposed to focusing only on the mean. Table 7 documents the effect of enforcement on wages. The top two panels correspond to the OLS and IV regressions we have been presenting so far in the paper. However, in light of the results of table 6, we present a third panel where we also control for the education of workers in each sector. For each quantile of the distribution of wages in the sector, we control for the corresponding quantile of the distribution of schooling in the same sector. Although it is unlikely that schooling is an exogenous variable in these regressions, we hope that this procedure allows to distinguishing true changes in wages from changes in the composition of workers. The findings show that an increase in enforcement is associated with a decline in wages at the top of the formal wage distribution, and an increase in wages among the self employed (with no statistically strong effect on the distribution of wages for informal wage earners). As a result, there is a fall in the differential between the wages of formal and informal workers (although this result is not shown directly, it is available on request). These effects remain strong, but slightly attenuated, once we control for changes in schooling across sectors.[31]

6.3. Interpretation

Our results can be interpreted in light of the arguments sketched in section 3. Throughout the discussion there is a tension: which model is the relevant one, the one with or the one without

[31] We estimate a simpler specification where the instrument for enforcement is distance alone, and not distance interacted with state inspectors. The reason this is not our main specification, we are not confident that distance alone is a valid instrument, although we conjecture that the bias should not be very large. The findings (not reported) show that, for most outcomes, the results are consistent with those reported in the paper. The main differences relate to income inequality, where coefficients are insignificant. Nevertheless, we still find a strong reduction on the formal wage premium, measured relatively to informal or relatively to self-employed workers.

wage rigidity? Although there is no heterogeneity in the models presented above, it surely exists in the data. In light of this fact, it seems reasonable to assume that there is some rigidity, especially at the bottom of the wage distribution, but perhaps not at the top. Not only is this statement sensible, it is also supported by the data.

An increase in the enforcement of mandated benefits in the formal sector leads to reduction in formal wages. If that is the case then it is likely that $v>1$ (workers put a higher value on mandated benefits than firms do). The minimum wage may prevent formal wages from falling at the bottom of the wage distribution.

There is an increase in the share of individuals who are non-employed which, given that $v>1$, can be probably attributed to downward wage rigidity (as shown in section 3, in a model without wage rigidity, $v>1$ implies that increased enforcement should lead to less non-employment). On the one hand, at the bottom of the formal wage distribution wages cannot adjust, and some employees are likely to be dismissed. On the other hand, if the fall in wages is smaller than the increase in the value of other formal job benefits (because of wage rigidity, or because the value workers put on benefits is higher than their cost to the firm), formal jobs become more attractive, inducing self-employed workers to search for work in the formal sector, in spite of a higher risk of unemployment.[32] If these jobs are at the bottom of the distribution of formal wages, it is natural that those workers leaving the self-employment sector have low levels of schooling (which would explain our empirical results on schooling).

If the cost of mandated benefits to the firm is below the value they have to workers (e.g., tax-free job severance payments, health and safety on the job), then an increase in mandated benefits could explain a rise in formal sector employment, if wages are fully flexible. Labor supply to the formal sector would increase since this sector becomes more attractive, and labor demand would also increase if there were a large enough decline in wages. The contraction in the supply of self-employed workers causes an increase in their wages. Formal sector employment could also increase due to worker registration (through direct action of labor inspectors, or indirectly through a deterrent effect).

[32] Using data from IPEA, we can compute the number of people searching for a job in each city, by subtracting the number of employed from the number of active individuals. The problem with this measure is that we cannot distinguish search in the formal and in the informal sector. If we regress the (log) number of individuals searching on enforcement (instrumented) we estimate that a one standard deviation increase in enforcement increases total search by 1/3 of a standard deviation.

Finally, it is interesting to notice that there are no statistically significant effects neither on wages nor employment of informal wage earners. One hypothesis is that they are part of a segmented branch of the labor market in Brazil, shielded from changes in the labor market.

We have ignored corruption so far. Increased enforcement may indicate more frequent corruption opportunities (not stricter regulation), especially for firms breaking the law. One way to model this is as an increase in the costs of hiring either formal or informal workers. However, corruption by itself cannot explain out data since it would imply a decline in wages in both sectors, and possibly a decline in employment in both sectors (because of higher labor costs).

6.4. Inequality

The empirical findings suggest that income inequality declines when enforcement increases. The second panel of table 7 shows a compression in the distribution of wages both in the formal and in the informal (self-employment) sectors. Moreover, there is a decline in wage differentials between formal and self-employed workers. However, we also have to consider that the non-employment rate increases with enforcement and this could lead to higher levels of city income inequality.

The results in table 8 show that changes in non-employment are not uniform across the distribution of income. We consider 6 groups of individuals, according to their position in the distribution of household per capita income: 0-10 percentile, 10-25, 25-50, 50-75, 75-90, and 90-100. Enforcement affects non-employment mostly for the poorest individuals in society, so it is not surprising that, in spite of the decline in inequality, we observe small decreases in poverty. Table 8 also shows that losses in employment are large among females, low skilled workers, and young workers, which are especially vulnerable groups (see also Heckman and Pages, 2004).

7. Conclusion

This paper studies the effect of an increase of enforcement of labor regulation on unemployment and inequality, using city level data from Brazil. We explore variation in the enforcement of labor market regulations using a new administrative dataset with information on the intensity of enforcement activity for all cities in Brazil. We interpret our findings in light of standard multi-sector models of the labor market in developing countries, which integrate formal and informal sectors and unemployment in a single framework.

We find that an increase in enforcement leads to higher unemployment but lower inequality. We also document that: i) employment flows between the formal and self-employment sectors, but informal wage earners are in a segmented sector of the economy; ii) mandated benefits are borne by workers in the form of lower wages at the top of the formal wage distribution, but not at the bottom where downward wage rigidity may be important; iii) as a result, formal jobs at the bottom of the wage distribution become more attractive, inducing those who are low skilled and self-employed to search for employment in the formal sector.

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Figure 1A: Effect of Distance on Enforcement Across Brazilian States

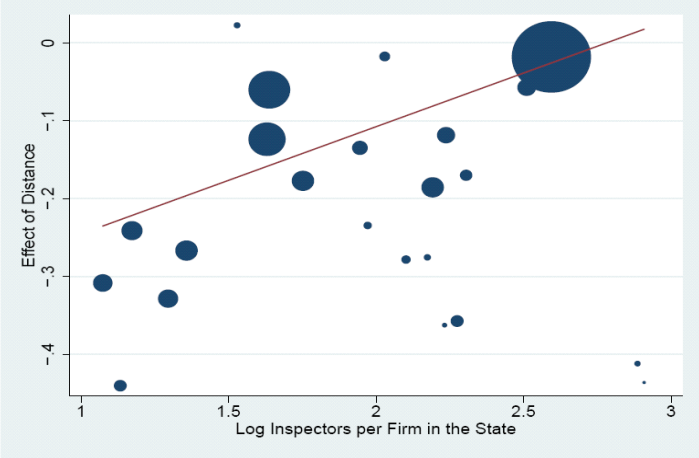


Figure 1B: Effect of Distance on Informality Across Brazilian States

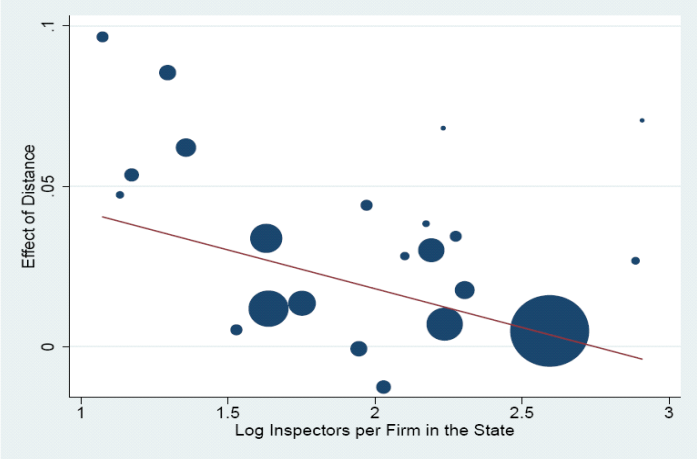
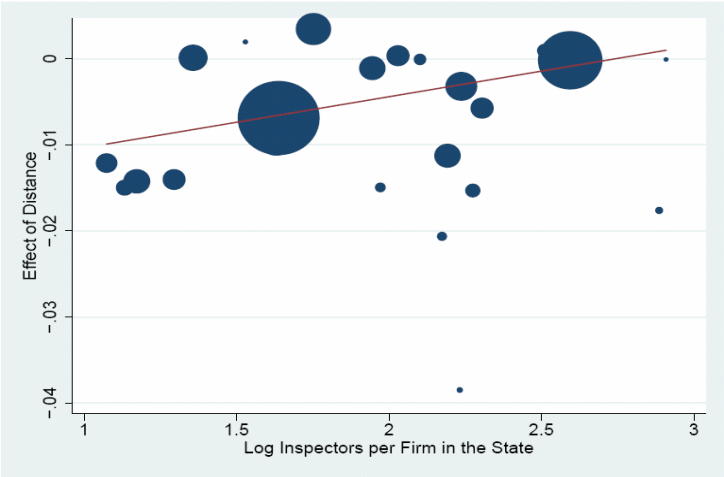


Figure 1C: Effect of Distance on Unemployment Across Brazilian States



Note: In Figure 1A we run, for each Brazilian state, a regression of the degree of enforcement (measured by the log of number of inspections per firm in the city in 2002) on distance to the nearest enforcement office (measured in hours of travel by car). Each circle represents a coefficient of one of these regressions, which is plotted against the log number of inspectors per firm in the state (coeff.=0.138, s.e.=0.029) . The size of each circle is the inverse of the standard error of the estimated coefficient. Figures 1B and 1C can be interpreted analogously. Figure 1B plots the coefficients of a regression of the share of informal workers (in 2000) in each city on distance, against the log number of inspectors per firm in the state (coeff.=-0.024, s.e.=0.006), while Figure 1C plots the coefficients of a regression of the unemployment rate at the city level (in 2000) on distance, against the log number of inspectors per firm in the state (coeff.=0.006, s.e.=0.002).

Figure 2A: Effect of Distance on Past Informality Across States

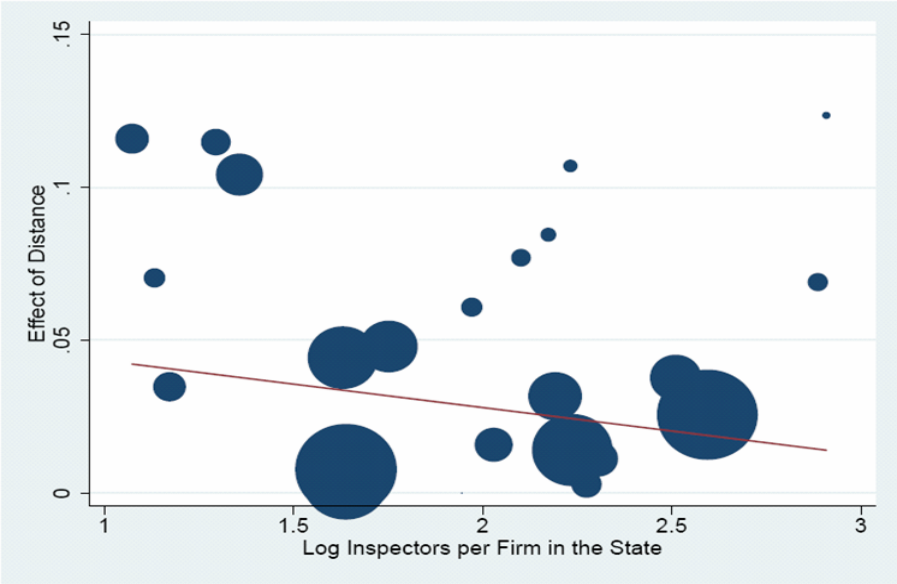
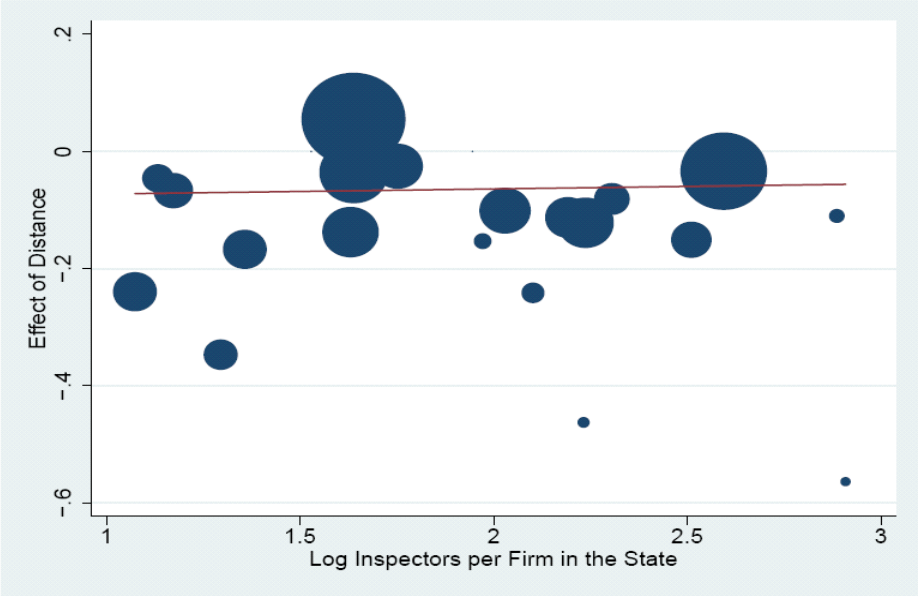


Figure 2B: Effect of Distance City GDPpc in 1980 Across Brazlian States



Note: In Figure 2A we run, for each Brazilian state, a regression of the share of informal workers in the city in 1980 on distance to the nearest enforcement office (measured by hours of travel by car). Each circle represents a coefficient of one of these regressions, which is plotted against the log number of inspectors per firm in the state (coeff.=-0.015, s.e.=0.014). The size of each circle is the inverse of the standard error of the estimated coefficient. Figure 2B can be interpreted analogously. Figure 2B plots the coefficients of a regression of the GDP per capita in the city in 1980 on distance, against the log number of inspectors per firm in the state (coeff.=0.009, s.e.=0.046).

Figure 3. Increase in the Cost of Mandated Benefits Equally Valued by Firms and Workers

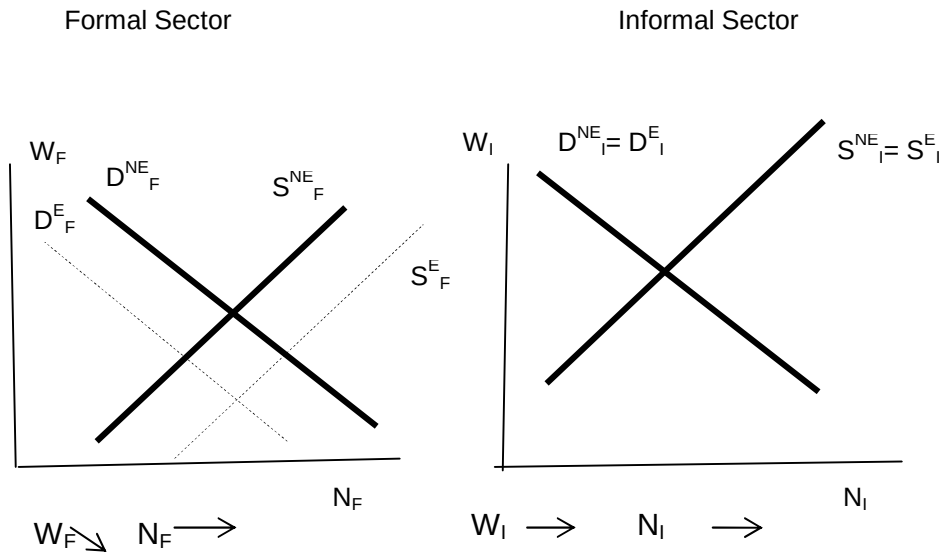


Figure 4. Increase in the Cost of Mandated Benefits Valued More Highly by Workers than Firms

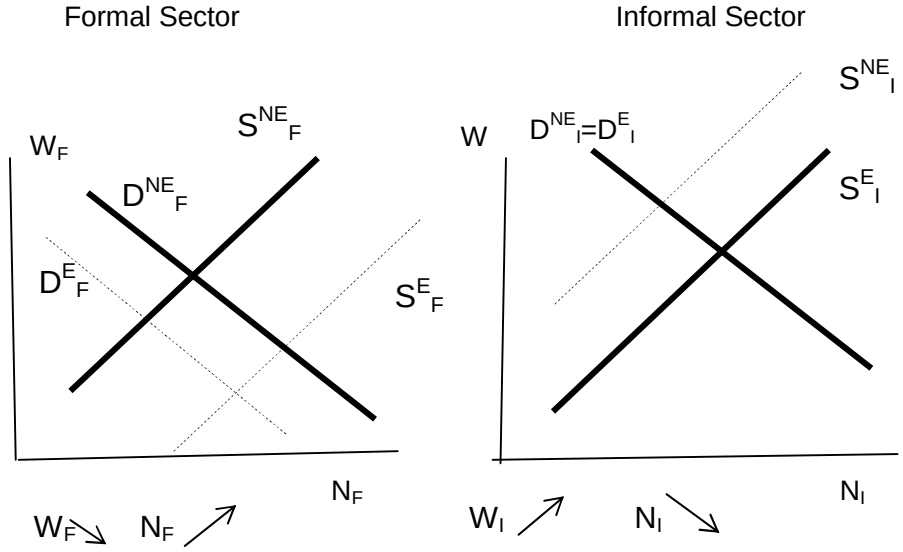


Figure 5. Increase in the Cost of Hiring Informal Workers

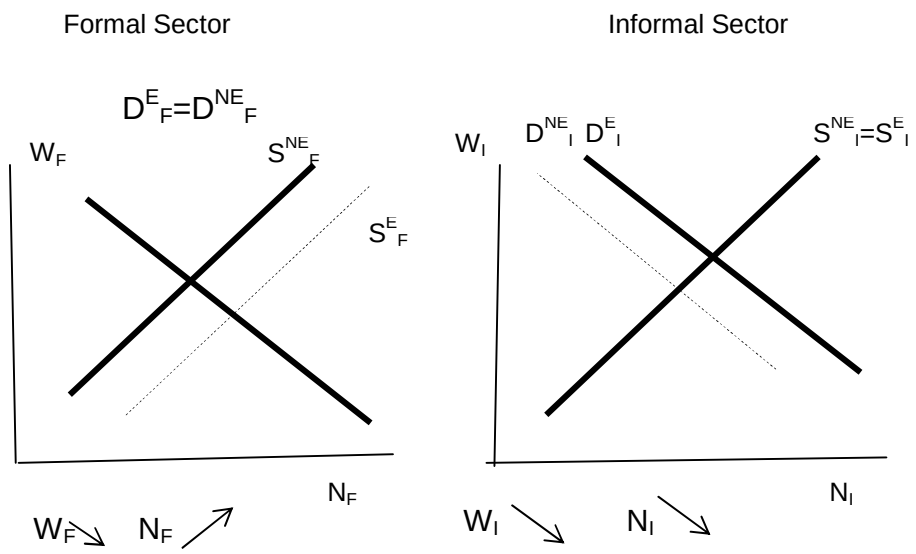


Table 1: Summary Statistics

	Obs	Mean	S.D.	Min	Max.
	(1)	(2)	(3)	(4)	(5)
Log Inspected Firms per firm City	5,505	0.94	0.99	0.00	4.78
Log Inspectors per firm in the state	5,513	1.693	0.53	1.07	2.96
Distance to the nearest labor office (hours)	5,287	1.96	1.73	0.00	13.91
City distance to the State capital city (hours)	5,272	4.50	2.56	0.00	14.99
City transportation costs	5,495	5.89	0.78	0.39	8.69
City Latitude	5,507	-16	8	-34	5
City Longitude	5,507	46	6	32	73
City Altitude	5,507	412	293	0	1628
Log City Geografical Area	5,507	6.20	1.28	1.06	11.99
Access to Justice City	5,506	0.90	0.83	0.00	3.00
Governance City	5,505	3.17	0.91	1.00	5.85
Political Concentration City	5,504	0.23	0.10	0.07	1.00
Share Informal Workers City	5,507	0.74	0.17	0.22	1.00
Poverty Rate City	5,507	0.46	0.23	0.03	0.93
Unemployment Rate City	5,507	0.11	0.06	0.00	0.59
Theil Inequality Index City	5,507	0.52	0.11	0.19	1.27
Share Population Jobless	5,507	0.37	0.09	0.00	0.78
Share Population Formal Jobs	5,507	0.14	0.09	0.00	0.51
Share Population Informal Jobs	5,507	0.16	0.06	0.01	0.49
Share Population Self-Employed	5,507	0.20	0.09	0.00	0.70
Log wages in formal sector	5,497	5.93	0.35	3.69	7.65
Log wages in informal sector	5,507	5.73	0.42	4.47	7.38
Log wages self-employed	5,506	6.00	0.58	3.77	8.27
Log GDP per capita City	5,507	8.08	0.76	6.14	12.13
Log population City	5,507	9.36	1.11	6.68	16.16
Share migrants City	5,507	0.44	0.22	0.03	1.00
Log number firms City	5,505	5.09	1.52	0.00	13.05
Log Av. Firm size City	5,505	3.29	0.82	0.73	7.49
Share GDP Agriculture	5,492	0.29	0.19	0.00	0.86
Share GDP Manufacturing	5,507	0.20	0.17	0.00	0.95
Share GDP Services	5,507	0.52	0.16	0.03	0.97
Years schooling formal sector	5,504	6.18	1.42	0.00	11.16
Years schooling informal sector	5,507	5.29	1.39	1.52	10.80
Years schooling self-employed	5,506	4.45	1.59	0.32	10.29

Source: Brazilian Ministry of Labor (2002), Population census (2000), IPEA, IBGE.

Table 2: City Characteristics and the Instrumental Variable

	N. Obs	Distance to the nearest labor office (hours) * Inspectors per firm in the state
	Method: OLS	
	(1)	(2)
Train Stations City (dummy)	5,242	-0.025
		[0.020]
Access to Justice City	5,244	-0.037
		[0.041]
Managerial Capacity City	5,243	-0.035
		[0.041]
Political Concentration City	5,243	-0.002
		[0.004]
Households Piped Water pc City	5,242	-0.014
		[0.041]
Households Sanitation pc City	5,242	-0.001
		[0.078]
Households Electricity pc City	5,242	-0.02
		[0.011]*
Current Transfers from State to City	4,518	0.044
		[0.063]
Homicide Rate City	5,242	-0.067
		[0.074]
Log Population City	5,242	-0.039
		[0.032]
Log GDP pc City	5,242	0.022
		[0.025]
Share Agriculture in GDP City	5,228	0.002
		[0.007]
Share Manufactiring in GDP City	5,242	-0.007
		[0.008]
Share Services in GDP City	5,242	0.006
		[0.007]
Share Informal Workers City (1980)	5,242	-0.004
		[0.005]
Unemployment Rate City (1980)	5,242	0.002
		[0.001]*
Theil Index City (1980)	5,242	0.008
		[0.006]
Poverty Rate City (1980)	5,242	0.002
		[0.004]

Robust standard errors in brackets, * significant at 10%; ** significant at 5%; *** significant at 1%. The table reports the least squares estimates of the regression of each of the variables reported at the top of each row on the distance to the nearest labor office (hours) interacted with the log number of labor inspectors in the state. The controls are state dummies, distance to the nearest labor office, its square and interactions with state level variables, distance to the state capital city, its square and interactions with the number of inspectors per firm in the state and interactions with other state variables, city transportation costs, its square and interactions with the number of inspectors per firm in the state and interactions with other state variables, city altitude, city latitude and city longitude. Other state variables include average access to justice, political concentration, management quality in public administration and the GDP per capita in the state. City transportation cost is the transport cost between each city and the nearest capital city in 1995. We also include the log of total population, per capita income, average years of schooling and share of population in urban areas, in 1970, 1980, and 1991 (variables describ

Table 3: The Determinants of Labor Inspections

Dependent Variable:	Log Inspected Firms per firm in city	Log Inspected Firms per firm in city	Log Inspected Firms per firm in city
Method:	OLS	OLS	OLS
	(1)	(2)	(3)
City distance to the nearest labor office (hours) * Inspectors per firm in the state	0.068	0.139	0.183
	[0.017]***	[0.026]***	[0.048]***
City distance to the nearest labor office (Average Marginal Effect)	-0.237	-0.156	-0.095
	[0.012]***	[0.015]***	[0.015]***
City distance to the nearest labor office (hours)	Yes	Yes	Yes
City distance to the nearest labor office squared	Yes	Yes	Yes
City distance to the nearest labor office (hours) * State Level Institutional Quality	No	No	Yes
City distance to the State capital city (hours)	No	Yes	Yes
City distance to the State capital city squared	No	Yes	Yes
City distance to the State capital city (hours) * Inspectors per firm in the state	No	Yes	Yes
City distance to the State capital city (hours) * State Level Institutional Quality	No	No	Yes
City transportation costs	No	No	Yes
City transportation costs squared	No	No	Yes
City transportation costs * Inspectors per firm in the state	No	No	Yes
City transportation costs * State Level Institutional Quality	No	No	Yes
City Institutional quality	No	No	Yes
City altitude, latitude and longitude	No	No	Yes
State level dummies	Yes	Yes	Yes
City level characteristics in 91, 80 and 70	No	No	Yes
Observations	5,284	5,269	5,240
R squared	0.22	0.24	0.37
F-test (H0: Distance to the nearest labor office (hours) * Inspectors per firm in the state = 0)	15.95	28.82	14.65

Robust standard errors in brackets, * significant at 10%; ** significant at 5%; *** significant at 1%. The table reports the least squares estimates of the regression of the log of the number of inspected firms per firm in the city on the distance to the nearest labor office (hours) interacted with the number of labor inspectors in the state. The controls are state dummies, distance to the nearest labor office, its square and interactions with state level variables, distance to the state capital city, its square and interactions with the number of inspectors per firm in the state and interactions with other state variables, city transportation costs, its square and interactions with the number of inspectors per firm in the state and interactions with other state variables, city altitude, city latitude and city longitude. Other state variables include average access to justice, political concentration, management quality in public administration and the GDP per capita in the state. City transportation cost is the transport cost between each city and the nearest capital city in 1995. We also include the log of total population, per capita income, average years of schooling and share of population in urban areas, in 1970, 1980, and 1991. Variables described

Table 4: Enforcement of Labor Regulation and City Level Informality, Poverty, Unemployment, and Inequality (2000)

Dependent Variable:	Share Informal Workers	Poverty Rate	Unemployment Rate	Inequality Index
	(1)	(2)	(3)	(4)
Panel A: OLS				
Log Inspected Firms per firm in city	-0.018	-0.009	0.003	-0.008
	[0.002]***	[0.001]***	[0.001]***	[0.002]***
Observations	5,240	5,240	5,240	5,240
Panel B: IV				
Log Inspected Firms per firm in city	-0.150	-0.052	0.093	-0.119
	[0.043]***	[0.023]**	[0.026]***	[0.043]***
Observations	5,240	5,240	5,240	5,240

Standard errors in brackets, * significant at 10%; ** significant at 5%; *** significant at 1%. Panel A reports the estimated coefficients of the regression of the city variables reported at the top of each column on the log of the number of inspected firms per firm in the city, controlling for state level dummy variables and several city level characteristics. Panel B reports the instrumental variable estimates using city distance to the nearest labor office interacted with the number of state level inspectors per firm in the state as an instrument for enforcement. City level characteristics include distance to the nearest labor office, its square and interactions with state level measures of institutional quality, distance to the state capital city, its square and interactions with the number of inspectors per firm in the state and interactions with state level measures of institutional quality, city transportation costs, its square and interactions with the number of inspectors per firm in the state and interactions with state level measures of institutional quality, city altitude, city latitude and city longitude. It also includes the city level controls for income per capita, population, average schooling and share of urban population in 1991, 1980 and 1970.

Table 5: Enforcement of Labor Regulations and the Composition of Employment in the City

	No Job Status (Unemployed and Out of the Labor Force)	Formal Wage Earners	Informal Wage Earners	Self Employed
	(1)	(2)	(3)	(4)
		Panel A: OLS		
Log Inspected Firms per firm in city	0.001	0.01	0.002	-0.006
	[0.001]	[0.001]***	[0.001]*	[0.001]***
Observations	5240	5240	5240	5240
		Panel B: IV		
Log Inspected Firms per firm in city	0.065	0.062	0.018	-0.097
	[0.028]**	[0.021]***	[0.017]	[0.032]***
Observations	5,240	5,240	5,240	5,240

Standard errors in brackets, * significant at 10%; ** significant at 5%; *** significant at 1%. Panel A reports the estimated coefficients of the regression of the city variables reported at the top of each column on the log of the number of inspected firms per firm in the city, controlling for state level dummy variables and several city level characteristics. Panel B reports the instrumental variable estimates using city distance to the nearest labor office interacted with the number of state level inspectors per firm in the state as an instrument for enforcement. City level characteristics include distance to the nearest labor office, its square and interactions with state level measures of institutional quality, distance to the state capital city, its square and interactions with the number of inspectors per firm in the state and interactions with state level measures of institutional quality, city transportation costs, its square and interactions with the number of inspectors per firm in the state and interactions with state level measures of institutional quality, city altitude, city latitude and city longitude. It also includes the city level controls for income per capita, population, average schooling and share of urban population in 1991, 1980 and 1970.

Table 6: Enforcement of Labor Regulations and Schooling of the Labor Force, by Employment Status

	Formal Wage Earners			Informal Wage Earners			Self Employed		
	P10	P50	P90	P10	P50	P90	P10	P50	P90
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				Panel A: OLS					
Log Inspected Firms per firm in city	-0.085	-0.148	-0.068	-0.016	-0.011	0.016	-0.009	0.007	0.08
	[0.016]***	[0.028]***	[0.027]**	[0.010]	[0.012]	[0.024]	[0.012]	[0.014]	[0.028]***
Observations	5,237	5,237	5,237	5,240	5,240	5,240	5,239	5,239	5,239
				Panel B: IV					
Log Inspected Firms per firm in city	-0.540	-1.723	-1.642	-0.208	0.033	0.497	0.016	0.495	1.217
	[0.325]*	[0.664]***	[0.626]***	[0.166]	[0.227]	[0.473]	[0.194]	[0.297]*	[0.518]**
Observations	5,237	5,237	5,237	5,240	5,240	5,240	5,239	5,239	5,239

Standard errors in brackets, * significant at 10%; ** significant at 5%; *** significant at 1%. Panel A reports the estimated coefficients of the regression of the city variables reported at the top of each column on the log of the number of inspected firms per firm in the city, controlling for state level dummy variables and several city level characteristics. Panel B reports the instrumental variable estimates using city distance to the nearest labor office interacted with the number of state level inspectors per firm in the state as an instrument for enforcement. City level characteristics include distance to the nearest labor office, its square and interactions with state level measures of institutional quality, distance to the state capital city, its square and interactions with the number of inspectors per firm in the state and interactions with state level measures of institutional quality, city transportation costs, its square and interactions with the number of inspectors per firm in the state and interactions with state level measures of institutional quality, city altitude, city latitude and city longitude. It also includes the city level controls for income per capita, population, average schooling and share of urban population in 1991, 1980 and 1970.

Table 7: Enforcement of Labor Regulations and Wage Distribution, by Employment Status

	Formal Wage Earners			Informal Wage Earners			Self Employed		
	P10	P50	P90	P10	P50	P90	P10	P50	P90
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				Panel A: OLS					
Log Inspected Firms per firm in city	0.007	-0.002	-0.015	0.02	0.006	0.004	0.033	0.01	0.005
	[0.004]*	[0.004]	[0.006]**	[0.006]***	[0.003]*	[0.005]	[0.006]***	[0.005]**	[0.007]
Observations	5,230	5,230	5,230	5,240	5,240	5,240	5,239	5,239	5,239
				Panel B: IV					
Log Inspected Firms per firm in city	0.108	-0.085	-0.326	0.041	0.061	0.112	0.310	0.308	0.192
	[0.077]	[0.070]	[0.133]**	[0.096]	[0.066]	[0.092]	[0.119]***	[0.109]***	[0.113]*
Observations	5,230	5,230	5,230	5,240	5,240	5,240	5,239	5,239	5,239
				Panel C: IV - Controls for Schooling					
Log Inspected Firms per firm in city	0.131	-0.033	-0.240	0.049	0.059	0.091	0.308	0.269	0.114
	[0.080]	[0.068]	[0.122]**	[0.096]	[0.066]	[0.087]	[0.118]***	[0.100]***	[0.106]
Observations	5,230	5,230	5,230	5,240	5,240	5,240	5,239	5,239	5,239

Standard errors in brackets, * significant at 10%; ** significant at 5%; *** significant at 1%. Panel A reports the estimated coefficients of the regression of the city variables reported at the top of each column on the log of the number of inspected firms per firm in the city, controlling for state level dummy variables and several city level characteristics. Panel B reports the instrumental variable estimates using city distance to the nearest labor office interacted with the number of state level inspectors per firm in the state as an instrument for enforcement. Panel C replicates Panel B but adds as control the percentile of the schooling distribution in each city (10th percentile in column one, 50th in the second, 90th in the third). City level characteristics include distance to the nearest labor office, its square, interactions with state level measures of institutional quality, distance to the state capital city, its square, interactions with the number of inspectors per firm in the state, interactions with state level measures of institutional quality, city transportation costs, its square and interactions with the number of inspectors per firm in the state and interactions with state level measures of institutional quality, city altitude, city latitude and city longitude. It also includes the city level controls for income per capita, population, av. schooling and share of urban population in 91, 80 and

Table 8: Enforcement of Labor Regulations and Non-Employment in the City, by Vulnerability

	Share Individuals Out of the Labor Force, by Income and Vulnerability Groups								
	Percentile 0-10	Percentile 10-25	Percentile 25-50	Percentile 50-75	Percentile 75-90	Percentile 90-100	Females	Low Educated Workers	Younger Workers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
					Panel A: OLS				
Log Inspected Firms per firm in city	0.008	0.005	0.001	-0.001	-0.005	-0.001	0.002	0.001	-0.001
	[0.002]***	[0.001]***	[0.001]	[0.001]	[0.001]***	[0.001]	[0.002]	[0.002]	[0.001]
Observations	5,240	5,131	5,238	5,240	5,240	5,240	5,240	5,240	5,240
					Panel B: IV				
Log Inspected Firms per firm in city	0.115	0.096	0.052	0.006	0.002	-0.010	0.074	0.083	0.065
	[0.037]***	[0.034]***	[0.023]**	[0.018]	[0.018]	[0.019]	[0.034]**	[0.033]**	[0.029]**
Observations	5,240	5,131	5,238	5,240	5,240	5,240	5,240	5,240	5,240

Standard errors in brackets, * significant at 10%; ** significant at 5%; *** significant at 1%. Panel A reports the estimated coefficients of the regression of the city variables reported at the top of each column on the log of the number of inspected firms per firm in the city, controlling for state level dummy variables and several city level characteristics. Panel B reports the instrumental variable estimates using city distance to the nearest labor office interacted with the number of state level inspectors per firm in the state as an instrument for enforcement. City level characteristics include distance to the nearest labor office, its square and interactions with state level measures of institutional quality, distance to the state capital city, its square and interactions with the number of inspectors per firm in the state and interactions with state level measures of institutional quality, city transportation costs, its square and interactions with the number of inspectors per firm in the state and interactions with state level measures of institutional quality, city altitude, city latitude and city longitude. It also includes the city level controls for income per capita, population, average schooling and share of urban population in 1991, 1980 and 1970.

Table A1: Proportion of Labor Market Fines in the City (2002)

	Obs	Average	SD
	(1)	(2)	(3)
Worker's Formal Registration	1,453	0.22	0.31
Mandatoty Work Period	1,453	0.10	0.20
Mandatory Work Pause Period	1,453	0.09	0.17
Wage	1,453	0.09	0.18
FGTS Contributions	1,453	0.26	0.32
Other (incl. Health, Security Restrictions)	1,453	0.23	0.29

Source: Brazilian Ministry of Labor (2002)

Table A2: City Employment Composition

	Obs.	Share Total Population
	(1)	(2)
Domestic worker with formal work permit	5,507	0.008
Domestic worker without formal work permit	5,507	0.025
Employee with work permit	5,507	0.137
Employee without work permit	5,507	0.163
Employer	5,507	0.015
Self-Employed	5,507	0.196
Unpaid apprentice	5,507	0.001
Unpaid employee	5,507	0.036
Worker self-consumption	5,507	0.046
No employment status	5,507	0.373

Source: Census (2000)

Table A3: Distribution of City Wages by Employment Status

	Percentile 10	Percentile 50	Percentile 90
	(1)	(2)	(3)
Formal Wage Earners	5.03	5.60	6.51
Informal Wage Earners	4.52	5.34	6.37
Self-employed	4.35	5.40	6.69

This table shows percentiles of the wage distribution for the formal wage earners, informal wage earners and self-employed, respectivley.

Table A4: City Characteristics and the Instrumental Variable

	N. Obs	Distance to the nearest labor office (hours) * Inspectors per firm in the state	Distance to the nearest labor office (hours) * Managerial capacity in the state	Distance to the nearest labor office (hours) * Access to Justice in the state	Distance to the nearest labor office (hours) * GDP pc in the state	Distance to the nearest labor office (hours) * Political Concentration in the state
			Method: OLS			
	(1)	(2)	(3)	(4)	(5)	(6)
Train Stations City (dummy)	5,242	-0.025	0.068	0.01	-0.10	-0.53
		[0.020]	[0.032]**	[0.021]	[0.032]***	[0.164]***
Access to Justice City	5,244	-0.037	-0.085	(0.01)	0.07	0.03
		[0.041]	[0.063]	[0.047]	[0.061]	[0.317]
Managerial Capacity City	5,243	-0.035	-0.063	0.08	-0.01	0.35
		[0.041]	[0.068]	[0.054]	[0.062]	[0.348]
Political Concentration City	5,243	-0.002	0.002	(0.01)	0.00	-0.02
		[0.004]	[0.007]	[0.006]**	[0.007]	[0.045]
Households Piped Water pc City	5,242	-0.014	0.005	(0.15)	0.10	-0.01
		[0.041]	[0.078]	[0.050]***	[0.070]	[0.522]
Households Sanitation pc City	5,242	-0.001	-0.121	0.21	0.18	2.01
		[0.078]	[0.135]	[0.116]*	[0.129]	[0.786]**
Households Electricity pc City	5,242	-0.02	-0.034	0.05	0.03	0.10
		[0.011]*	[0.017]*	[0.014]***	[0.016]*	[0.081]
Current Transfers from State to City	4,518	0.044	0.165	(0.15)	-0.19	-1.21
		[0.063]	[0.084]**	[0.061]**	[0.070]***	[0.375]***
Homicide Rate City	5,242	-0.067	-0.056	0.22	-0.09	0.22
		[0.074]	[0.121]	[0.088]**	[0.127]	[0.611]
Log Population City	5,242	-0.039	-0.116	(0.04)	0.04	0.52
		[0.032]	[0.055]**	[0.036]	[0.059]	[0.306]*
Log GDP pc City	5,242	0.022	0.094	(0.02)	-0.12	-0.46
		[0.025]	[0.047]**	[0.033]	[0.044]***	[0.243]*
Share Agriculture in GDP City	5,228	0.002	0.057	(0.01)	-0.05	-0.15
		[0.007]	[0.011]***	[0.009]	[0.011]***	[0.058]***
Share Manufactiring in GDP City	5,242	-0.007	-0.017	0.01	-0.01	-0.05
		[0.008]	[0.013]	[0.009]	[0.013]	[0.072]
Share Services in GDP City	5,242	0.006	-0.041	0.00	0.05	0.20
		[0.007]	[0.013]***	[0.009]	[0.011]***	[0.067]***
Share Informal Workers City (1980)	5,242	-0.004	0	(0.02)	0.01	0.07
		[0.005]	[0.009]	[0.006]**	[0.009]	[0.052]
Unemployment Rate City (1980)	5,242	0.002	0.002	(0.00)	0.00	-0.01
		[0.001]*	[0.002]	[0.002]	[0.002]	[0.009]
Theil Index City (1980)	5,242	0.008	-0.031	0.01	0.06	0.17
		[0.006]	[0.010]***	[0.008]	[0.010]***	[0.052]***
Poverty Rate City (1980)	5,242	0.002	-0.012	0.01	0.01	-0.04
		[0.004]	[0.006]**	[0.005]*	[0.006]**	[0.028]

Robust standard errors in brackets, * significant at 10%; ** significant at 5%; *** significant at 1%. The table reports the least squares estimates of the regression of each of the variables reported in each row on the distance to the nearest labor office (hours) interacted with the number of labor after controlling for all the variables as in column (3) of table 4. Households with piped water, sanitation and electricity are measured with the logarithm of number of households with these amenities normalized by the total number of individuals in the city. When not reported city characteristics refer to either year 2000 or 2002 depending on the data availability. More details on the construction of the variables are provided in the Data section.

Table A5: Enforcement and City Outcomes (2000): Robustness to Additional City, Firm and Worker Controls

Dependent Variable:	Share Informal Workers	Poverty Rate	Unemployment Rate	Inequality Index
	(1)	(2)	(3)	(4)
Panel A: OLS				
Log Inspected Firms per firm in city	-0.013	-0.008	0.003	-0.008
	[0.002]***	[0.001]***	[0.001]***	[0.002]***
City Sector GDP composition	Yes	Yes	Yes	Yes
City Firm and Worker characteristics	Yes	Yes	Yes	Yes
Observations				
Panel B: IV				
Log Inspected Firms per firm in city	-0.132	-0.058	0.076	-0.131
	[0.040]***	[0.022]***	[0.022]***	[0.043]***
City Sector GDP composition	Yes	Yes	Yes	Yes
City Firm and Worker characteristics	Yes	Yes	Yes	Yes
Observations				

Standard errors in brackets, * significant at 10%; ** significant at 5%; *** significant at 1%. Table reports the same specifications as in table 6 but includes additional city level controls to capture the city's sector composition as well as firm and worker characteristics. City sector GDP composition includes the city's share of GDP in agriculture, industry and services. City firm and workers characteristics include av. age, share females, share migrants and av. firm size in the city.