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Enforcement of Labor Regulation and Informality[†]

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Enforcement of labor regulations in the formal sector may drive workers to informality because they increase the costs of formal labor. But better compliance with mandated benefits makes it attractive to be a formal employee. We show that, in locations with frequent inspections, workers pay for mandated benefits by receiving lower wages. Wage rigidity prevents downward adjustment at the bottom of the wage distribution. As a result, lower paid formal sector jobs become attractive to some informal workers, inducing them to want to move to the formal sector. (*JEL* J31, J63, J88, K31, O15)

There is weak compliance with labor regulations in many developing countries. One of the most visible manifestations of this problem is the existence of large informal sectors, which are often argued to be harmful for workers, and detrimental to growth. In this setting, one view of labor inspections is that they promote formality by detecting and punishing informal employment.

In practice, because of high costs of enforcement and scarce resources, labor inspectors often focus on formal firms since they are easier to find than informal firms. Formal firms violate several aspects of labor law, but they are less likely to hire informal workers than informal firms. Therefore, labor inspectors generally miss the main source of informal employment.

In this context, it is possible that frequent targeting of formal firms by labor inspectors could drive workers to the informal sector by causing an increase in the

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costs of formal employment. This line of reasoning, however, neglects that labor inspectors may be enforcing compliance with mandated benefits which are highly valued by workers, and potentially increase the attractiveness of the formal sector. In this alternative scenario, the response of workers depends on their valuation of the benefits being enforced.

We study the impact of labor inspections on labor market outcomes in Brazil by exploring municipal variation in the level of enforcement in 2000. Earlier work by Cardoso and Lage (2007) argues that labor inspections mainly target compliance with job severance contributions by formal firms, and our data is consistent with their argument.

Our empirical results show that in response to a rise in labor inspections we observe an increase in formal employment, a decrease in informal employment, a rise in nonemployment, a decline in wages at the top of the formal wage distribution, and an increase in informal wages. All of the movement from the informal to the formal sector is among the self-employed.[1]

Our argument is that in the early 1990s labor inspectors started enforcing compliance with mandated benefits, namely contributions in advance to the job severance fund, and job severance payments upon dismissal. This occurred because formal firms were much easier to find than informal firms and there was low compliance with job severance contributions. In addition, revenues from advanced contributions could be counted as increases in government revenue. This contributed to an improvement in public accounts (at least in the short run), and governments had great interest in their collection. Job severance benefits were also highly valued by workers, who could access part of the severance entitlement even if they were not dismissed.

As a result of increased enforcement, formal workers pay for more generous mandated benefits by receiving lower wages. The value that workers place on these benefits is potentially higher than their cost to employers because they are untaxed. In addition, wage rigidity (e.g., through minimum wages) prevents downward adjustment at the bottom of the wage distribution. This causes formal sector jobs at the bottom of the wage distribution to become more attractive to informal workers, leading them to switch to the formal sector. In the process, wages in the informal sector adjust upward. Unemployment may increase if the minimum wage makes it harder to find a job in the formal sector. Together with an increase in informal wages, this rise in (formal) unemployment also helps to clear the market (as in Harris and Todaro 1970).

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 proceed with our analysis it is essential to investigate the main determinants of enforcement.

The technology of enforcement is relatively simple. Labor inspectors are assigned to enforcement offices located in cities across Brazil. They choose which firms to

[1] Self-employed workers do not necessarily have to be informal. However, in Brazil, only 15 percent of self-employed workers pay social security contributions. It is therefore safe to assume that almost all of the self-employed effectively operate in the informal sector.

visit, essentially based on a list of anonymous reports of violations of the labor code. They travel by car from their base city to the city where the inspected firm is located. This suggests that there are two important inputs to this technology: the number of inspectors in each enforcement office, and the distance they need to travel from the base to each particular city. Cities located at a farther distance from the nearest enforcement office are less likely to receive a visit from the labor inspector. Moreover, in locations where there is an abundance of labor inspectors, distance to the enforcement office is a less important constraint.

Our empirical work compares the differential impact of distance to the nearest enforcement office on labor market outcomes in each city, across states with different numbers of labor inspectors. Identification is based on the idea that travel time is less of a constraint in locations with greater availability of inspectors. Moreover, we control for labor market outcomes in an earlier period, when enforcement was a less important activity. Therefore, we are able to check how exposure to different levels of enforcement (induced by variation in distance and inspectors in the state) produced changes in labor markets that looked similar before enforcement ever became important.[2] Our data includes information on labor market outcomes in each city in 2000 and 1980, measures of enforcement, distance from each city to the nearest enforcement office, the number of inspectors working in each state, and several other variables, such as distance to the state capital, measures of transportation costs, and city and state characteristics.

We find that an increase in the level of enforcement in a city leads to an increase in the share of the population in formal employment, an increase in nonemployment, a decrease in self-employment, a reduction in formal wages, and an increase in earnings of those who are self-employed (most of whom are informal workers). There is little change in the employment and wages of those who are informal employees. Our analysis shows 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. In addition, we discuss potential problems with our analysis, and present evidence that they are unlikely to be important.

This paper builds on and contributes to a long literature. Our theoretical framework draws on Harris and Todaro (1970), Fields (1975, 2004), McDonald and Solow (1985), Maloney (2004), and Levy (2008).[3] Although labor regulation is strict in Brazil, there is surprisingly large wage and employment flexibility (e.g., Barros and Mendonca 1996). The reason for this may be low enforcement. When interpreting our findings, we think of a simple static model of the labor market with minimal rigidities, except for a minimum wage. All restrictions to firing and hiring enter the labor demand function implicitly as costs of labor. [4]

[2] 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.

[3] See also Harberger (1962); Bulow and Summers (1986); Acemoglu (2001); and Albrecht, Navarro, and Vroman (2006). Several other papers try to empirically distinguish segmented and nonsegmented models of the labor market, such as Dickens and Lang (1985); Heckman and Hotz (1986); Maloney (1999); or Filho, Mendes, and Almeida (2004).

[4] There are several recent other contributions to the literature on informality. They include work by Schneider and Enste (2000); Friedman et al. (2000); Amaral and Quintin (2006); Galiani and Weischelbaum (2007); Boeri

Modern surveys of the role of labor market institutions include Nickell and Layard (1999) or Kugler (2007), among many others. The increasing availability of micro data led to the emergence of several studies examining the effect of labor market regulations in developing countries.[5] Two papers are especially close to ours. Besley and Burgess (2004) explore within country and time series variation in labor reforms in India to study the effect of labor regulations on productivity, investment, employment and poverty. Marrufo (2003) examines the social security reform in Mexico. This is one of the few papers that considers labor market policy in a multi-sector labor market (using a Harberger model with two sectors and worker heterogeneity).

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

The next section of the paper provides background information on the Brazilian labor market, its institutions, and the structure of the enforcement process. Section II describes the data. Section III explains the empirical strategy. Section IV shows the empirical results. Section V presents a simple theoretical framework for interpreting our findings, and Section VI concludes.

I. Labor Market Regulation and Enforcement in Brazil

A. *Labor Regulations*

On paper, Brazil has one of the least flexible labor markets 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 (e.g., Barros and Corseuil 2001). For example, the law establishes a maximum working period of 44 hours a week, a maximum period for continuous shift work of 6 hours, a minimum overtime pay of 1.5 times the normal hourly wage, paid leave of at least four-thirds of the normal wage, and a paid maternity leave of 120 days. In addition, the employer must contribute monthly to social security and to a job security fund (FGTS). The FGTS is essentially a severance pay individual account. It accumulates for as long as the worker remains employed with the firm. The employer makes monthly contributions of 8 percent of the employee's current wage to the fund (10 percent from 2001 onward). Cardoso and Lage (2007) estimate that for a worker to receive a net wage of R$100, the employer must disburse approximately R$165.

and Garibaldi (2006); Loayza, Oviedo, and Serven (2005); de Paula and Scheinkman (2006); Bosch, Goni, and Maloney (2007); and Perry et al. (2007). Especially related to us are studies on informality and inequality as Fields (1979, 2004) or Bourguignon (1990).

[5] See, for example, Kugler (1999, 2001, 2004), Eslava et al. (2006), Ahsan and Pages (2007), Petrin and Sivadasan (2006), or the studies in Heckman and Pages (2004).

Firing a worker in Brazil is not significantly more difficult than firing a worker in other Latin American countries, although (on paper) it is 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. The interim 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 percent of paid hours are not worked. On top of that, if a dismissal induces a drop in motivation, the overall decline in production is probably above 25 percent (Barros and Corseuil 2001). 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. In particular, the law establishes that a penalty equal to 40 percent of the fund accumulated during the worker's tenure with the firm needs to be paid to the worker. 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 (e.g., Neri 2002).

It is important to highlight that severance payments are not subject to income taxation in Brazil (unlike most countries). Therefore, workers value one Real of FGTS more highly than one Real of gross salary. Moreover, firms pay taxes on profits, which can add up to more than 30 percent. As a result, the cost of FGTS to the firm is much smaller than the value of FGTS to the worker.[6] This has strong implications for the role of enforcement, which we explore below.

B. *Enforcement of Labor Regulation*

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 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),[7] 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

[6] Coordination between workers and firms may be difficult even if there are gains. For example, firms may not be able to credibly commit ex ante that they will contribute to FGTS and pay severance upon dismissal. Enforcement of these benefits could help solve this commitment problem.

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

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 1970s and 1980s. The labor code was much less strict at the time, and the inflation of the 1970s and hyperinflation of the 1980s and early 1990s contributed to an erosion of the nominal value of fines. However, labor inspections gained importance in the second half of the 1990s. Labor regulation became much stricter after the 1988 Constitution, and inflation stabilized as well. Moreover, the strong government deficit in the mid-1990s led 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* (Cardoso and Lage 2007). In theory, an inspection can be triggered either by a random firm audit, or by a report (often anonymous) of noncompliance 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 noncompliance reports, most inspections are triggered by these anonymous reports.

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 (an informal firm), since there are no records of its activity. As a result, a large fraction of informal employment is left out of the inspectors' reach. Inspectors face a performance-based pay scheme. In particular, up to 45 percent of their wage is tied to the efficiency of the overall enforcement system. Their monthly base wage is fairly competitive (between US$2,490 and US$3,289 in 2004).

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 original fine, who deliberates on its fairness, and the result is reported to the head of the *subdelegacia*. If firms do not contest the fine and pay it within 10 days of their notification, there is a 50 percent 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 R$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 R$16 and R$160 per employee.

C. *What Are Labor Inspectors Really Doing?*

Although the number of inspectors was relatively low in the early 2000s, inspectors were able to reach a significant part of the total labor force in formal firms. In 2002, 304,000 firms were visited by labor inspectors, reaching more than 19 million workers (Cardoso and Lage 2007). Approximately 17 percent of these firms received a notification of noncompliance with some aspect of the law. Moreover, less than 3 percent of the workers in these firms were found to be informal and registered as a result. This is a small number given that 50 percent of employment in Brazil is informal. This likely reflects the fact that informal workers are concentrated in small and informal firms outside the reach of labor inspectors, but it may also suggest that, among the different types of violations of labor law, informality is not the main target of the inspections. [8] According to Cardoso and Lage (2007), the focus of labor inspectors is on the lack of payment of the job security fund (and health and safety conditions on the job).

If we study ILO reports on labor inspections and informality, we learn that labor inspectors target mainly formal firms in most countries, precisely because they are easier to find than informal firms.[9] Berg (2010), from the ILO office in Brazil, writes that "because of the way the system is structured, the majority of the workers that have been registered have been informal workers working in formal firms. They comprise approximately 2.2 percent of the total number of workers covered by labour inspection activities. Registering informal workers in formal firms is an important accomplishment and should not be downplayed, but as most of informality concerns informal workers working in unregistered firms, other approaches are also needed."[10]

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

II. Data

The paper explores several sources of data, which we describe in more detail in the online Appendix A. First, we use administrative data on the enforcement of

[8] All violations are punishable with fines. Inspectors issue fines for the nonregistration of workers, disobedience of the official work period or hours worked, noncompliance with the mandatory wage payments (including minimum wages), and missing FGTS contributions or health and safety violations. But fines are inaccurate measures of enforcement for two reasons. First, fines require a violation of the law, and much enforcement may have a deterrent effect not translated into fines. Second, inspectors avoid issuing fines, and try first to negotiate with the firm in nonlitigious ways (Cardoso and Lage 2007).

[9] For example, in an ILO document Salter (1998) writes: "Since labour inspectors in developing countries are not well enough resourced even to inspect adequately and often enough the medium and large enterprises, they can rarely if ever turn their attention to the problems of informal sector workers." For a detailed review of labor inspections in Latin America see issue number 6 of the *Revista Latino-Americana de Derecho Social* (2008).

[10] In recent years, there has been an attempt to directly tackle informality, at least in specific sectors, through the formation of small teams tied to a particular sector (see Berg 2010, and Pires 2008).

labor regulations, collected by the Ministry of Labor. These data contain, among other things, information on number of regional labor offices, number of inspected firms, and number of inspectors per state. Our main measure of enforcement is the log inspections per firm in the city. This is computed with log number of inspections in the city multiplied by 100 (plus one) minus the log of the number of firms in the city. We also compute the proportion of workers inspected and the number of inspectors per firm in the state. Second, we compute several city-level labor market indicators using a sample of the Brazilian Census in 2000. These include the share of workers who are registered, unregistered, or self-employed; the share of nonemployed (either unemployed or out of the labor force); and the distribution of wages for each type of worker. From the 1980 Census, we construct measures of the proportion of the population in each sector and the distribution of wages in each sector. Third, we use detailed information on other city-level characteristics from *Instituto de Pesquisa Economica Aplicada* and *Instituto Brasileiro de Geografia e Estatistica*. This includes, among other things, information on the city's GDP per capita, the share of different sectors on the GDP of the city, and city geographical characteristics. Data on municipal participation in PETI, a program for the eradication of child labor, and the number of recipients of *Bolsa Familia* is provided by the Ministry of Social Development. The online Appendix explains in detail the time series harmonization of the data required by the creation and destruction of cities over time. Fourth, we proxy the institutional development of the city with 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. Fifth, we compute the distance and travel time between each city and the nearest *subdelegacia* in the state. 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 is available from an auto insurance company. In the remainder of this 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.

Sample statistics for the main variables we use are presented in Table 1. Both in 1980 and 2000 we account for close to 90 percent of the population using the four groups we consider: formal wage earners, informal wage earners, self-employed and nonemployed. In the average city in Brazil, formal wage earners comprise of only 14 percent of the working age population (aged 23–65) in 2000, and about 30 percent of the labor force. Average wages are higher for formal wage earners than for informal wage earners. Earnings are even higher for the self-employed, but so is the variance of earnings. There are about 3.2 inspections per 100 firms in a typical Brazilian city, with 22 percent of the workers being covered. This means that large firms are disproportionately targeted for inspections. On average, there are 6 inspectors per 10,000 firms in the state. Cities are located two hours from the nearest enforcement offices and 4.5 hours from the state capital. In a typical city, individuals work mostly in the service sector.

Unfortunately, there are time discrepancies between the different variables that we could not overcome. Notably, enforcement is measured in 2002, while labor

Table 1—Summary Statistics

Number of observations	5,242
Share population nonemployed in 2000	0.37
	(0.0931)
Share population formal jobs in 2000	0.14
	(0.0940)
Share population informal jobs in 2000	0.16
	(0.0643)
Share population self-employed in 2000	0.19
	(0.0897)
Monthly wages in formal sector in 2000	396.34
	(150.57)
Monthly wages in informal sector in 2000	335.73
	(149.15)
Monthly wages self-employed in 2000	474.22
	(276.10)
Share population nonemployed in 1980	0.42
	(0.0516)
Share population formal jobs in 1980	0.15
	(0.1092)
Share population informal jobs in 1980	0.14
	(0.0899)
Share population self-employed in 1980	0.25
	(0.1154)
Monthly wages in formal sector in 1980	507.67
	(166.31)
Monthly wages in informal sector in 1980	278.26
	(88.10)
Monthly wages self-employed in 1980	494.38
	(241.12)
Number of inspections per 100 firms per firm city in 2002	3.24
	(5.9339)
Proportion of workers targeted by inspections in 2002	0.22
	(3.5263)
Inspectors per 10,000 firms in the state in 2002	5.88
	(3.2744)
Distance to the nearest labor office (hours)	1.96
	(1.7124)
City distance to the state capital city (hours)	4.50
	(2.5476)
GDP per capita city in 2000	4,537.15
	(5,865.23)
Population city in 2000	26,951.00
	(160,814)
Area of the city in km^2 in 2000	1,048.38
	(2,603.93)
Share GDP agriculture in 2000	0.29
	(0.1842)
Share GDP manufacturing in 2000	0.20
	(0.1713)
Share GDP services in 2000	0.51
	(0.1533)

Notes: 2000 values are in 2000 Reais; 1980 values are in 1980 Cruzeiros. For some of these variables there are slightly less of 5,242 nonmissing observations.

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

market outcomes are measured in 2000. Nevertheless, given that we rely mainly on cross-sectional variation (in distance and the availability of inspectors) to identify our main models, this is unlikely to be a central concern. And in our empirical work we will make limited direct use of our measures of enforcement.[11]

III. Empirical Strategy

The goal of our empirical work is to understand whether enforcement of labor regulation at the city level affects earnings and the employment of formal, informal, and self-employed workers. We explore two measures of enforcement: the logarithm of the number of inspections per firm in the city (computed as the number of visits by labor inspectors times 100 plus one, divided by the number of firms); and the logarithm of the proportion of workers targeted by inspections. The main obstacle we face is that enforcement is not randomly allocated across cities. This happens for at least two important reasons. First, enforcement may be stricter in cities where violations of labor law are more prevalent. This happens if inspections are triggered mainly through reports of illegal activity. Second, enforcement may be stricter in cities with more developed institutions. Intrinsic violations of the labor law, or better developed institutions, are probably correlated with labor market outcomes.

Finding a solution for this problem requires knowledge of the technology of enforcement. As explained above, labor inspectors are assigned to enforcement offices. They choose which firms to visit, essentially based on anonymous reports of violations of the labor code. They travel by car from their base city to the city where the inspected firm is located. This suggests that there are two important inputs to this enforcement technology: the number of inspectors in each enforcement office, and the travel distance from the base to each particular city. Cities located at a farther distance from the nearest enforcement office are less likely to receive a visit from the labor inspector. Moreover, this constraint will be more important in areas with low numbers of labor inspectors (relatively to their potential workload).

In our empirical work, we compare the differential relationship between distance to the nearest enforcement office and labor market outcomes in each city, across states with different numbers of labor inspectors. We control for labor market outcomes in an earlier period, when enforcement was a less relevant activity. Therefore, we are able to check how exposure to different levels of enforcement produced changes in labor markets that looked similar before enforcement ever became important.[12]

We start by considering the following reduced form model:

$$Y_{cs} = \alpha + \psi D_{cs} + \eta_s + \beta(I_s \times D_{cs}) + \boldsymbol{\delta}\mathbf{XCity}_{cs} + \sigma Y_{cs}^{1980} + u_{cs},$$

[11] Results are similar using the number of inspectors per state for 2001 and 2002. Furthermore, even though 2002 was an election year in Brazil, there is no systematic difference in aggregate enforcement activity in 2002 and neighboring years.

[12] We would have liked to estimate a model where we directly compare the differential effect of distance on labor market outcomes across states in 2000 and 1980. However, the measures of labor market outcomes in the two census years are not exactly the same. Furthermore, we do not know the location of the full universe of enforcement offices in 1980, and the number of inspectors per state in that year. Even though they are likely to be correlated with the values we observe in 2000, they are not the same. Therefore, we use data on labor market outcomes in 1980 only as control variables in our regressions.

where Y_{cs} is the outcome of interest in city c and state s, D_{cs} is distance to the nearest enforcement office from city c and state s, $\mathbf{XCity}_{cs}$ is a vector of city controls, I_s is the (number of labor inspectors (per firm) in the state, η_s is a state fixed effect, and u_{cs} is the residual. Y_{cs}^{1980} is our best approximation for the outcome of interest in 1980. β is the parameter of interest and measures the differential impact of distance to the enforcement office on labor market outcomes across states with different abundance of labor inspectors.

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 nonrandom location of enforcement offices. Any remaining variation is given by the differential effect of distance across states with varying numbers of inspectors. We control for several city-level characteristics ($\mathbf{XCity}_{cs}$): log income per capita and log population size in 1970, 1980, and 1991, and city latitude, longitude, altitude, and area. One could also be concerned that the number of state-level labor inspectors is simply correlated with other state-level characteristics ($\mathbf{XState}_s$), such as its level of development or institutional quality, which interacted with distance, could also affect outcomes of interest. Therefore, we add to 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).

In addition, distance to the nearest enforcement office could potentially be capturing distance to relevant markets, which affects economic activity in the city (with consequences to labor market outcomes). This can interact with state characteristics. We therefore add to the proposed reduced form the distance to the state capital ($DCapital_{cs}$) and an index of transportation costs to the nearest capital ($TCosts_{cs}$). Both are good measures of distance to important markets in the state. They enter the model on their own, interacted with the four state institutional variables, and interacted with the log of the number of inspectors per firm in the state. Therefore, in our final model, we will only explore the variation in distance to the nearest enforcement office remaining after we control for distance to large markets and urban centers. In Section IVD, we show that, with this empirical strategy, it is unlikely that we are confounding the effect of enforcement on labor markets with that of other variables.

In summary, the main empirical specification in the paper is:

$$
\begin{aligned}
(1) \quad Y_{cs} = {} & \alpha + \beta(D_{cs} \times I_s) + \psi_0 D_{cs} + \psi_1 (D_{cs})^2 + \phi(D_{cs} \times \mathbf{XState}_s) \\
& + \psi_2 DCapital_{cs} + \psi_3 (DCapital_{cs})^2 + \psi_4 (DCapital_{cs} \times I_s) \\
& + \tau(DCapital_{cs} \times \mathbf{XState}_s) + \psi_5 TCosts_{cs} + \psi_6 (TCosts_{cs})^2 \\
& + \psi_7 (TCosts_{cs} \times I_s) + \rho(TCosts_{cs} \times \mathbf{XState}_s) \\
& + \delta \mathbf{XCity}_{cs} + \sigma Y_{cs}^{1980} + \eta_s + u_{cs},
\end{aligned}
$$

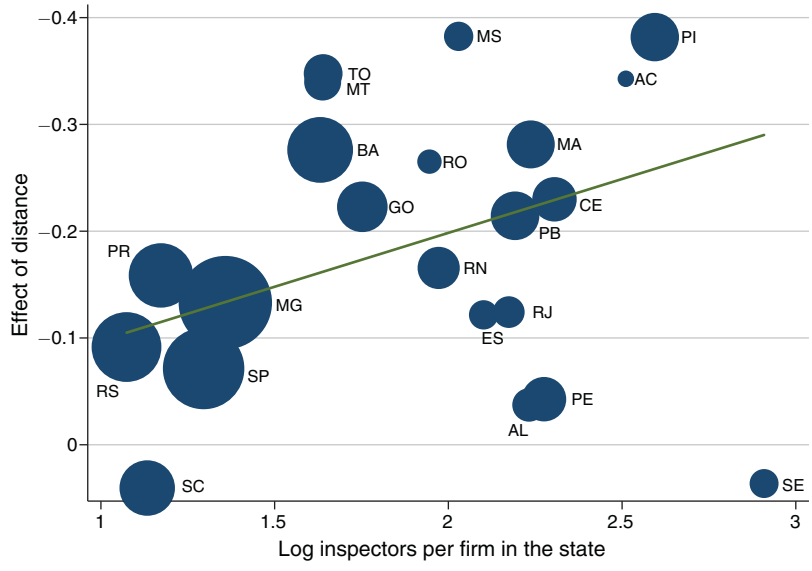

FIGURE 1. EFFECT OF DISTANCE ON INSPECTIONS PER FIRM IN THE CITY ACROSS BRAZILIAN STATES

Note: See note for Figure 3.

where all the variables are defined above. We estimate equation (1) by least squares, and cluster the standard errors at the state level. The main outcomes of interest include labor earnings and employment of formal, informal, and self-employed workers. We are also interested in the share of informal workers in the city, city poverty, inequality, and the city unemployment. In addition, we analyze how distance relates to our measures of enforcement. In particular, we are interested in the estimates for the average marginal effect of distance on enforcement, which is given by

$$\beta I_s + \psi_0 + 2 \times \psi_1 D_{cs} + \phi \mathbf{XState}_s .$$

Graphical Illustration.—Figures 1, 2, and 3 illustrate our procedure. For each state we run a regression of a measure of enforcement (the log of the number of labor inspections per 100 firms in the city) on distance to the nearest enforcement office. Each circle in Figure 1 represents a coefficient of one of these regressions, which is plotted against the log number of inspectors per 10,000 firms 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. We then fit a regression of these estimated distance coefficients on the amount of inspectors per state. The slope of the regression line is positive and significant showing that coefficients are disproportionately negative in states with fewer inspectors.[13]

[13] Each state is represented by two letters as follows: Acre-AC, Alagoas-AL, Amapa-AP, Bahia-BA, Ceara-CE, Espirito Santo-ES, Goias-GO, Maranhao-MA, Mato Grosso-MT, Mato Grosso do Sul-MS, Minas Gerais-MG, Paraiba-PB, Parana-PR, Pernambuco-PE, Piaui-PI, Rio de Janeiro-RJ, Rio Grande do Norte-RN, Rio Grande do Sul-RS, Rondonia-RO, Roraima-RR, Santa Catarina-SC, Sao Paulo-SP, Sergipe-SE, Tocatins-TO. Amazonas and Para are excluded from the analysis because distance by road is not very meaningful in those states, and Distrito Federal is

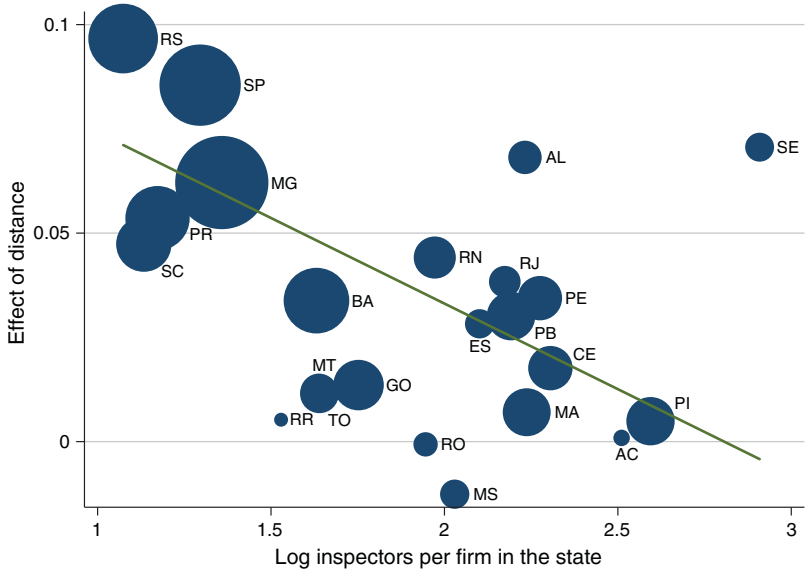

FIGURE 2. EFFECT OF DISTANCE ON THE SHARE OF INFORMAL WORKERS IN THE CITY ACROSS BRAZILIAN STATES

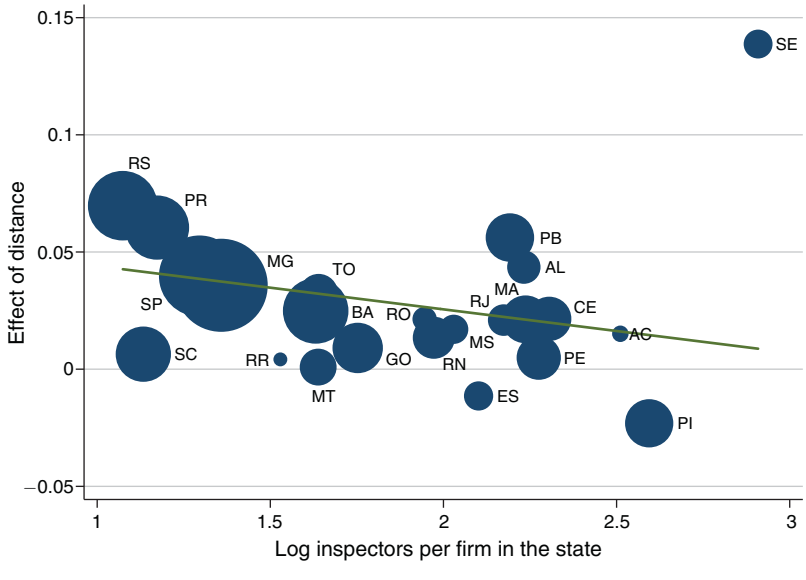

FIGURE 3. EFFECT OF DISTANCE ON THE FORMAL-INFORMAL WAGE DIFFERENTIAL IN THE CITY ACROSS BRAZILIAN STATES

Note: In Figure 1 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. = 0.101, s.e. = 0.046). The size of each circle is the number of cities in each state. Figures 2 and 3 can be interpreted analogously. Figure 2 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. = −0.041, s.e. = 0.010), while Figure 3 plots the coefficients of a regression of the formal-informal wage differential at the city level (in 2000) on distance, against the log number of inspectors per firm in the state (coeff. = −0.019, s.e. = 0.011).

integrated with GO. Figures A1, A2 and A3 in online Appendix B use weights of the number of cities in each state. The patterns are similar, but the slope of the wage premium line is marginally insignificant (p-value = 0.11).

If this is the case, and if enforcement has an impact on labor markets, we expect the relationship between distance and, for example, the share of informal workers or the formal-informal wage premium in the city, to be more pronounced in states with low numbers of inspectors. Figures 2 and 3 show that this is true for outcomes measured in 2000. In drawing Figure 2, we run a regression for each state of the share of informal workers in each city on the distance to the nearest enforcement office. The estimated coefficient is then regressed on the log number of inspectors per 10,000 firms in the state. Figure 3 replicates this exercise but looking at the formal-informal wage premium in the city. 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. Both figures indicate that stronger enforcement leads to a reduction in informality and in the formal-informal wage premium.

IV. Empirical Findings

A. *Does the Impact of Distance on Enforcement of Labor Regulation Vary across States with Different Abundance of Labor Inspectors?*

This section shows that distance to the nearest enforcement office and the number of inspectors per state are important determinants of the level of enforcement of labor regulation in a city. To establish this, we estimate equation (1) using the as dependent variables two alternative measures of enforcement: the log of the number of inspections per 100 firms in the city, and the log of the ratio of workers covered by inspections to total workers in the city.

Table 2 reports estimates of the average marginal effect of distance on enforcement (i.e., $\beta I_s + \psi_0 + 2 \times \psi_1 D_{cs} + \phi \mathbf{XState}_s$, evaluated at the mean of these variables) and the coefficient on the interaction of distance with the log number of inspectors per 10,000 firms in the state. Each column corresponds to a different measure of enforcement. As expected, distance is strongly and negatively correlated with both measures of enforcement. More importantly, β is positive and significant. This implies that although the impact of distance on enforcement is negative, it is less negative in states where inspectors are abundant.

B. *Wages and the Distribution of Employment across the Formal and Informal Sectors*

This section investigates the effect of enforcement on the employment composition and earnings in each sector of the labor market.

Table 3 reports the effect of enforcement on the share of the adult population in the city in each employment category in 2000. The model in specification I excludes 1980 controls (Y_{cs}^{1980}), but they are included in specification II. The specific 1980 variable included in each specification depends on the outcome of interest. For example, when the outcome is the proportion of individuals in the working age population working as formal wage earners, the 1980 control is the proportion of individuals who work as wage earners and pay social security. The reasoning is

Table 2—The Determinants of Labor Inspections

Dependent variable:	Log inspections per firm in the city (1)	Log proportion of inspected workers in the city (2)
Average marginal effect of distance to the nearest labor office	−0.1 [0.020]***	−0.22 [0.04]***
Distance to the nearest labor office (hours) × Inspectors per firm in the state	0.212 [0.100]**	0.612 [0.186]***
Observations	5,242	5,242
R^2	0.35	0.40

Notes: Standard errors in brackets clustered at the state level. The table reports the least squares estimates of the regression of the log of the number of inspections per firm in the city and the log of the proportion of inspected workers 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 (reported in the table) and interactions with state level variables (average access to justice; average political concentration; average management quality in public administration; and the log of GDP per capita in the state averaged across 1970, 1980, and 2000), 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, area of the city, city altitude, city latitude and city longitude. 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 and per capita income in 1970, 1980, and 1991. The table reports both the coefficient on the interaction of distance and inspectors per firm in the state, and the average marginal effect of distance on enforcement. Variables described in the appendix.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

analogous for other types of individuals. In cities with stricter enforcement there is more formal employment, more nonemployment, and less self-employment. There is no statistically significant effect of enforcement on the share of informal wage earners. Results are similar across the two specifications.

In order to assess the magnitude of these coefficients, we compare Tables 2 and 3. The coefficient on the interaction in column 1 of Table 2 is 0.212 (corresponding to roughly 20 percent of a standard deviation in the log number of inspections per firm in the city). This implies that a 21.2 percent increase in inspections causes a 1.6 percent decrease in the proportion of formal workers (from a mean of 14 percent), a 2.3 percent decrease in the proportion self-employed (with mean 19 percent), and a 1.7 percent increase in the proportion nonemployed (with mean 37 percent).[14]

Table 4 analyzes the different percentiles of the wage distribution at the city level. The two specifications distinguish models with and without 1980 controls. For each percentile of the distribution of wages in the sector in 2000, we control for the corresponding percentile of the distribution of wages in the same sector in 1980. The results show that an increase in enforcement is associated with a decline in

[14] Alternatively, one may look at Table 3. If we change the number of inspectors per firm in the state from the tenth to the ninetieth percentile (from 3 to 10 inspectors per 10,000 firms; in logs, from 1.13 to 2.31), the (presumably negative) impact of increasing distance by one hour on formal employment declines by 1.9 percent (=(2.31-1.13) × 0.016), it increases by 2.7 percent for self-employment, and it declines by 2 percent for nonemployment.

Table 3—Enforcement of Labor Regulations and the Composition of Employment in the City

	I. Baseline specification (1)	II. Specification including 1980 variables (2)
Panel A. Formal wage earners		
Distance to the nearest labor office (hours) × inspectors per firm in the state	0.016 [0.005]***	0.013 [0.005]***
1980 share in sector	—	0.301 [0.045]***
R^2	0.67	0.7
Observations	5,242	5,242
Panel B. Informal wage earners		
Distance to the nearest labor office (hours) × inspectors per firm in the state	0.004 [0.005]	−0.001 [0.005]
1980 share in sector	—	0.222 [0.036]***
R^2	0.39	0.44
Observations	5,242	5,242
Panel C. Self employed		
Distance to the nearest labor office (hours) × inspectors per firm in the state	−0.023 [0.007]***	−0.011 [0.006]*
1980 share in sector	—	0.345 [0.051]***
R^2	0.42	0.49
Observations	5,242	5,242
Panel D. Non-employed		
Distance to the nearest labor office (hours) × inspectors per firm in the state	0.017 [0.006]***	0.014 [0.005]**
1980 share in sector	—	0.428 [0.056]***
R^2	0.36	0.4
Observations	5,242	5,242

Notes: Standard errors in brackets clustered at the state level. The table reports the least squares estimates of the regression of the log of the proportion of individuals in each sector in the city on the distance to the nearest labor office (hours) interacted with the number of labor inspectors in the state. The controls in specification I are state dummies, distance to the nearest labor office, its square (reported in the table) and interactions with state level variables (average access to justice; average political concentration; average management quality in public administration; and the log of GDP per capita in the state averaged across 1970, 1980, and 2000), 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, area of the city, city altitude, city latitude and city longitude. 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 and per capita income in 1970, 1980, and 1991. In specification II we include the dependent variable measured in 1980. Variables described in the Appendix.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

wages at the top of the formal wage distribution, an increase in wages among the self-employed, and an increase in wages for highly paid informal wage earners. As a result, the differential between wages of formal and informal workers falls. These

Table 4—Enforcement of Labor Regulations and Wage Distribution, by Employment Status

	I. Baseline specification			II. Specification including 1980 variables		
	P10 (1)	P50 (2)	P90 (3)	P10 (4)	P50 (5)	P90 (6)
Panel A. Formal wage earners						
Distance to the nearest labor office (hours)	0.024	−0.012	−0.049	0.019	−0.016	−0.054
× inspectors per firm in the state	[0.016]	[0.014]	[0.027]*	[0.016]	[0.014]	[0.025]**
R^2	0.52	0.61	0.4	0.52	0.61	0.40
Observations	5,232	5,232	5,232	5,232	5,232	5,232
Panel B. Informal wage earners						
Distance to the nearest labor office (hours)	0.018	0.017	0.036	0.018	0.016	0.032
× inspectors per firm in the state	[0.032]	[0.020]	[0.017]*	[0.032]	[0.020]	[0.017]*
R^2	0.58	0.76	0.67	0.58	0.76	0.67
Observations	5,242	5,242	5,242	5,242	5,242	5,242
Panel C. Self employed						
Distance to the nearest labor office (hours)	0.06	0.063	0.043	0.064	0.056	0.033
× inspectors per firm in the state	[0.025]**	[0.017]***	[0.018]**	[0.025]**	[0.017]***	[0.019]*
R^2	0.72	0.79	0.7	0.73	0.80	0.71
Observations	5,241	5,241	5,241	5,241	5,241	5,241

Notes: Standard errors in brackets clustered at the state level. The table reports the least squares estimates of the regression of the percentiles of the distribution of wages for workers in each sector in the city on the distance to the nearest labor office (hours) interacted with the number of labor inspectors in the state. The controls in specification I are state dummies, distance to the nearest labor office, its square (reported in the table) and interactions with state level variables (average access to justice; average political concentration; average management quality in public administration; and the log of GDP per capita in the state averaged across 1970, 1980, and 2000), 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, area of the city, city altitude, city latitude and city longitude. 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 and per capita income in 1970, 1980, and 1991. In specification II we include the dependent variable measured in 1980. Variables described in the appendix.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

effects remain strong after controlling for the corresponding percentiles of the 1980 wage distribution in each sector.

To examine the magnitudes, we compare, again, the coefficients in Table 4 relative to the coefficient in column 1 of Table 2. The estimates imply that a 21.2 percent increase in enforcement leads to a 4.9 percent reduction in wages at the top of the formal wage distribution, a 3.6 percent increase in wages at the top of the informal employee wage distribution, and a roughly 6 percent increase across the self-employed earnings distribution.

Section V discusses a model of enforcement which is consistent with our empirical results. It assumes that labor inspectors target mainly formal firms, and they enforce compliance with mandated benefits that are valued by workers. More standard views of the role of enforcement are at odds with the data. Suppose labor inspectors were mainly targeting informal employment, thereby increasing the probability that an informal employer is caught and punished, we would expect to see a decrease in the demand for informal workers, and a resulting decline in their wages. In order for labor markets to clear, formal employment would then rise and formal wages

would fall. Finally, we would expect informal employees to be much more affected than self-employed individuals, who generally do not have a stable employment relationship with a third party (being targeted by labor inspectors). Although we see a rise in formal employment and a decline in formal wages in the data, the other predictions of this simple model are at odds with the data.[15]

Alternatively, if labor inspections mainly operate by increasing labor costs of formal firms, which have little value for employees, we expect to see a rise in informality, which again we do not observe. Our finding that enforcement leads to an increase in nonemployment can, however, be explained in this setting, since an increase in labor costs in the formal sector can drive workers to informality and to nonemployment.

C. *Enforcement and Access to Developed Labor Markets*

An important concern with our main findings reported in Tables 3 and 4, is whether we could be mistakenly capturing the differential impact of access to developed labor markets across states. Even though we control for several state and city level characteristics, one ultimately cannot guarantee that the only reason the effect of distance to the enforcement office on labor market outcomes varies with the number of inspectors available in the state is because it induces variation in enforcement of labor regulation across cities.

Notice that our specification is quite demanding. First, we could think that distance to the nearest enforcement office is capturing distance to important economic centers, since enforcement offices tend to locate in medium to large cities. However, we include in the regression two good measures of distance to large economic centers: distance to the state capital, and an index of transportation costs to the state capital. We interact both variables with the log number of inspectors per firm in the state. Therefore, we are studying the role of distance to the enforcement office for cities located at the same distance and facing the same travel cost to the state capital. Notice also that we include state level dummies in the regressions, and we interact distance to the nearest enforcement office (and distance and transportation costs to the state capital) not only with the abundance of inspectors in the state, but also with other state level variables, namely average GDP per capita across 1970, 1980, and 2000, and measures of institutional quality in the state.

Second, we present results where we control for labor market characteristics in each city in 1980. We argued earlier that in this period enforcement of labor market regulation was a much less relevant activity than in 2000. Controlling for these variables

[15] Our empirical work ignores heterogeneity across firms. While it is plausible that inspectors target mainly formal firms, as resources increase they may increasingly shift their attention to informal firms. It is difficult to know how important this phenomenon is in practice. Berg (2010) shows that there is a severe scarcity of resources for inspections. For example, in 2008, Brazil had roughly one inspector per 30,000 workers, whereas the ILO standard for an industrializing economy is one inspector per 15,000 workers. In the online Appendix, Table A2, we examine how the average number of workers covered per inspection varied with enforcement, by essentially running the model of equation (1) using as dependent variable the log number of workers per inspection (which only exists for cities with a positive number of inspections). We find that this quantity actually rises with the level of enforcement in the city. This result suggests that, at least as a first approximation, the phenomenon just described is empirically unimportant.

implies that we are estimating how differential levels of enforcement lead to a differential evolution in the labor market characteristics of cities that looked similar in the 80s.

It is also important to understand whether an economic model of enforcement would cause problems for our identification strategy. Online Appendix D develops a model that suggests that our assumptions on unobservables are not at odds with it. Our procedure allows for unobservable city characteristics that are correlated with distance to the enforcement office, and also with labor market outcomes, such as the level of informality in the city. For example, we expect enforcement offices to be located in large cities, with an existing set of functioning public institutions. One could also have unobservable state characteristics that are correlated with the number of inspectors per firm in the state, such as the underlying propensity to have violations of the labor code in that state. The crucial thing is to rule out plausible interactions between the two.

We investigate this issue in detail in the online Appendix C. In Table A3, we find no formal evidence that the interaction between the number of inspectors in the state and distance from each city to the nearest enforcement office is correlated with several city level variables proxying institutional quality, different dimensions of public policy, and local economic conditions.

D. *Robustness to Different Specifications*

This section examines the robustness of our main results, reported in Tables 3 and 4, to different specifications. First, in Tables A4 and A5 of the online Appendix, we replicate Tables 3 and 4 using different controls. In specification (1), we simply control for distance to the nearest enforcement office and its square, distance to the nearest enforcement office interacted with the log number of inspectors per firm in the state, distance to the state capital and its square, distance to the state capital interacted with the log number of inspectors per firm in the state, and state fixed effects. In the following specifications, we add cumulatively: specification (2) interactions between the two distance variables mentioned above and 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); specification (3) city latitude, longitude, altitude and area; specification (4) log population and log per capita income in 1970; specification (5) log population and log per capita income in 1980; specification (6) log population and log per capita income in 1991; and specification (7) an index of transport specification ation costs to the nearest capital interacted with the four state levels above and the log number of inspectors per firm in the state (baseline specification). Specification (7) is the baseline model as reported in Tables 3 and 4. To this specification we then add separately specification (8) interactions between a measure of state inequality (captured with the variance of specification the log per capita GDP across cities in each state) with the three distance measures (distance to the nearest enforcement office, distance to the state capital, and the index of transportation costs to the nearest capital); specification (9) interactions between the log number of beneficiaries of *Bolsa Familia* in the state, with the three distance measures; specification (10) interactions between the proportion of municipalities in PETI in the state, with the three distance

measures. The results of Table 3 and 4 are robust across different controls. The only exception refers to the effect of enforcement on wages at the top of the formal wage distribution in Table A5, which is not always statistically significant.

In addition, we have extended the exercise of Table A3 to more outcomes. Table A6 in the online Appendix relates the interaction between distance and scarcity of inspectors to different types of public expenditures, both in 1990 and 2000. This is a way to account for the fact that different municipalities may be offering different public services. With minor exceptions, we do not find systematic differences in the patterns of public expenditure in cities with different enforcement.

Our identifying variation comes from the interaction of distance to the nearest enforcement office with a measure of scarcity of inspectors in the state, measured by the log number of inspectors per firm in the state. One may worry whether most of the variation in this measure of scarcity comes from the numerator (number of inspectors in the state) or the denominator (number of formal firms in the state), and in general, what are the main determinants of this variable. We examine these issues in detail and present the results in the online Appendix. In particular, in Table A7 we begin by regressing the log number of inspectors per firm in the state on average GDP and population in the state over the years 1970, 1980, 1990, and 2000, and then we add to the regression the numerator (log number of inspectors) and the denominator (log number of firms) separately. We observe that they have roughly the same contribution to the R^2 of the regression. Therefore, we next examine what happens to our main result when we use alternative denominators. The results are shown in Tables A8 and A9 in the online Appendix, and they are similar to our main results.

Finally, given that there are only 24 clusters, one may worry about standard clustering procedures. Tables A10 and A11 in the online Appendix show that our results are robust to the wild bootstrap procedure recommended in Cameron, Gelbach, and Miller (2008). All coefficients that were statistically significant before remain so, and (perhaps surprisingly) two more become significant.

V. A Simple Theory

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), McDonald and Solow (1985), Bulow and Summers (1986), Maloney (2004), and Levy (2008). There is also an important literature integrating search models and informality, namely Acemoglu (2001); Albrecht, Navarro, and Vroman (2006); Bosch (2007); and Meghir, Narita, and Robin (2010). In our paper, we abstract from labor market frictions, central in the latter set of papers, and in much recent work on models of the labor market (Shimer 2010). They are not needed to understand the mechanism we emphasize, so we proceed with the simpler competitive model in mind. Online Appendix E presents the model that we next summarize.

The standard view of enforcement of labor regulation is quite different from the one we propose. Labor inspectors are seen as imposing higher costs of employment on formal firms by demanding compliance with costly regulation. As a result, there is a shift in employment toward the informal sector. Similarly, if labor inspectors are also able to target informal firms then they will increase the costs of hiring informal

workers, leading to a decrease in informal employment and wages. Both views are at odds with the data. Therefore, we need a different model.

We have argued above that in Brazil labor inspectors tend to enforce mandated benefits that are valuable to workers. When developing our model, the goal is thus to understand the implications of an increase in mandated benefits in the formal sector (resulting from stricter enforcement of these benefits) on employment and wages in the formal and informal sectors. In a simple competitive model with no rigidities, we expect some pass through of benefits to formal wages. The effects on formal and informal employment depend on the rate of pass through. It is possible that there are no effects on employment if formal wages fully adjust to reflect the cost of these benefits to employers.

In such a simple model, the rate of pass trough will depend on the valuation of benefits by the employees. The model has ambiguous predictions with regard to the effect of enforcement on employment and wages in each sector, which depend on this valuation. A central, nonstandard aspect of severance pay in Brazil is that employees may value contributions to the severance pay fund more than it costs employers to make these contributions, because firms pay taxes on profits but workers do not pay taxes on severance payments. This implies that for each Real the worker receives as severance pay (net of taxes), the firm needs to disburse less than one Real. If we enlarge the potential set of mandated benefits to health and safety, which are also being potentially enforced with stricter inspections in our data, then the costs of providing better health and safety conditions on the job may also be below the value workers place on them. Workers and firms may fail to coordinate on an efficient solution, if firms cannot perfectly commit to severance payments.

Under these conditions, the formal sector becomes more attractive, and we would expect the supply of formal workers to increase and the supply of workers to the informal sector to decline. As a result, there would be a decline in formal sector wages, an increase in formal sector employment, an increase in informal wages, and a decline in informal employment. We would also expect a rise in total employment since jobs in both sectors are more attractive.

In sum, the general model yields ambiguous predictions regarding informal employment. The central parameter in this model is the valuation of benefits by employees. There are at least two important aspects to add to this simple framework. First, suppose there are minimum wages in the formal sector so that formal wages cannot fully adjust downward. Jobs become rationed in the formal sector if minimum wages are binding, so assume workers can choose to be informal or to search for a job in the formal sector (or be unemployed). Formal wages will not decline in this case, so they cannot offset the value of mandated benefits, and the attractiveness of formal sector jobs increases. What adjusts to offset the value of mandated benefits is the unemployment rate among workers searching for a formal sector job. Empirically, we expect some of this to be true at the bottom but not at the top of the formal wage distribution.

An additional aspect to consider is the distinction between informal self-employed and informal wage earners. One view is to consider informal wage earners as working (mainly) in a segmented sector, as in a very standard model of the informal sector, and informal self-employed as being mobile between the formal and

informal sectors of the economy (e.g., Bosch and Maloney 2010). Although reality is unlikely to be so extreme, we find some support for this view in our empirical work, so it is the one we adopt in interpreting it.

This is not the only model that would predict that enforcement of mandated benefits reduce informality. Any model where benefits in the formal sector increase, and wages and unemployment do not adjust fully, so that informal workers have no incentive to move to the formal sector, will have a similar prediction. One may want, for example, to consider a model with search frictions. However, the frictionless model we consider provides us with the essential intuition for this problem.

Interpretation of Empirical Findings.—Our results can be interpreted in light of the arguments just sketched. But which model is the relevant one, the one with or the one without wage rigidity? Although there is no heterogeneity in the models presented above, it surely exists in the data. Therefore, it seems reasonable to assume that there is some rigidity 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.

We observe than an increase in the enforcement of mandated benefits in the formal sector leads to a reduction in formal wages, and an increase in formal sector employment. These two results together suggest that it is likely that workers put a higher value on mandated benefits than firms do ($v > 1$ in online Appendix E), at least in the model with no binding minimum wage. Otherwise, formal wages would still decline, but formal sector employment would not rise.

The reason is the following. 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 leads to a rise in formal sector employment, if wages are flexible. Labor supply would expand in the formal sector since this sector becomes more attractive, and labor demand would also increase if there was a large decline in wages.

However, there could be some wage rigidity. The minimum wage may prevent formal wages from falling at the bottom of the wage distribution. This would explain the lack of response to enforcement that we observe at the bottom of the formal wage distribution.

If employment expands in the formal sector, then where do the new workers come from? Empirically, we observe a reduction in self-employment, which makes sense under the interpretation that there is possible mobility between self and formal employment. 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 targeting informal contracts, or indirectly through a deterrent effect). In that case, we would expect lower formal sector wages, resulting in an increase in labor supply in this sector. We would also have to observe a fall in informal and self-employment. Both the decline in formal wages and the reduction in informal employment are at odds with the data.

There is an increase in the share of individuals who are nonemployed, which could be attributed to downward wage rigidity. As mentioned above, at the bottom of the formal wage distribution wages do not adjust, so some employees are

dismissed. Furthermore, self-employed workers are being induced to search for work in the formal sector, in spite of a higher risk of unemployment, because formal sector jobs are made more attractive by the fact that 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 which workers put on benefits is higher than their cost to the firm).

Finally, it is interesting to notice that there are no statistically significant effects on the employment of informal wage earners, and there are only changes at the top of their wage distribution. One hypothesis is that they are part of a segmented branch of the labor market in Brazil, mainly shielded from changes in the labor market (except, perhaps, for high earners).[16]

Up to now, we have ignored corruption. However, in the real world, increased enforcement may indicate more frequent corruption opportunities, especially for firms breaking the law. One way to model this is to consider an increase in the costs of hiring either formal or informal workers. Nevertheless, corruption by itself cannot explain our 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).

VI. Conclusion

In this paper, we present a new analysis of labor markets in developing countries, which emphasizes the role of weak enforcement of labor market regulation. We use data from Brazil, a country where informality is common, and labor law is strict but weakly enforcement.

We find that stricter enforcement leads to an increase in formal sector employment and nonemployment, and a decrease in employment in the informal sector, most of which seems to be due to a reduction in self-employment (rather than a reduction in the proportion of informal wage earners in the economy).

We also find that earnings fall at the top of the formal wage distribution, and they rise across the distribution of earnings among the self-employed. There may also be some increase in the wages of informal employees, but that result is weaker and less robust than the results for formal and self-employed earnings.

If labor inspectors are mainly targeting informal firms and informal workers, then inspections would increase the cost of using informal employment (negative shock to the demand for informal workers). In this case, we would expect a decrease in informal sector employment and wages, which is at odds with the data.

Alternatively, suppose labor inspectors target mainly formal firms, enforcing compliance with costly regulations. In this scenario, the increase in labor costs

[16] It is instructive to compare wages of formal, informal, and self-employed workers. Using data from the 2000 census, in Figure A4 in online Appendix C, we plot the densities of log monthly wages for each group. The left tail of the self-employed wage distribution coincides with the left tail of the informal wage distribution, and the right tail of the self-employed wage distribution coincides with the right tail of the formal wage distribution. Based on wages alone, self-employed workers are in-between formal and informal employees. Figure A5 in online Appendix C plots the density of years of education for each of these three groups and for those who are nonemployed. Self-employed workers are the least educated among the four groups considered in this figure. Based on wages alone it seems that self-employment is a better substitute for formal employment than informal employment, but the reason for this is not a similarity in years of education between self-employed and formal workers.

occurs in the formal sector, and, as a result, we would expect a decline in formal employment and a decline in formal wages, which is also at odds with the data.

The model we propose assumes that labor inspectors target formal firms, but that they enforce compliance with regulations, which are highly valued by workers. In fact, a recent detailed study of labor inspections in Brazil (Cardoso and Lage 2007), argues that enforcement affects mainly the compliance with mandated benefits in the formal sector. This model generates predictions broadly consistent with our findings: a decline in formal earnings, an increase in formal employment, an increase in informal earnings, and an increase in informal employment.

The addition of minimum wages to this model further improves the agreement of its predictions and our empirical results. At the top of the formal wage distribution, workers bear the cost of mandated benefits by receiving lower wages. But 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 low-skilled workers to search for formal jobs, resulting in lower levels of informality. Because of minimum wages, nonemployment may also increase.

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