Determinants of Changing Informal Employment in Brazil, 2000–2010

David Fairris and Erik Jonasson

University of California, Riverside, National Institute of Economic Research, Stockholm

28 February 2016

February 2016

Determinants of Changing Informal Employment in Brazil, 2000–2010

DAVID FAIRRIS
University of California, Riverside
david.fairris@ucr.edu

ERIK JONASSON
National Institute of Economic Research, Stockholm
erik.jonasson@konj.se

Abstract: This paper explores possible causal determinants of changing wage and salary informality over the period 2000–2010 in Brazil. We utilize demographic census and other institutional data sources from the opening and closing years of the decade, informality regressions in both years that exploit variation across workers and municipalities in informality rates and their causal determinants, and a Blinder-Oaxaca decomposition of changing mean informality rates over the decade. Among the determinants considered are: changes in labor law enforcement, a near doubling of the real value of the minimum wage, the emergence and growth of conditional cash transfer programs, and changing industry composition and labor force demographics. We find that two of the most important policy changes over this period – the increase in the real value of the minimum wage and the dramatic expansion of conditional cash transfer programs – contribute positively, not negatively to informality. Among the factors accounting for the decline in mean informality rates over this time are rising rates of labor law enforcement, rising education levels, increased numbers of workers with spouses in the formal sector, and changes in industry composition, which explain between 16% and 57% of the mean decline in informality over the period. However, most of the decline is accounted for by the changing estimated coefficients on the industry categorical variables – that is, by the changing way in which industrial composition translates into informality.

Acknowledgements: We acknowledge the assistance of Celso Amorim Araújo, Pedro Aratanha, Régis B Oliveira, Rubens Morita, Marcos de Almeida Rangel, Hélio Zylberstajn, Tania Costa, Pedro Carneiro, Rita Almeida, and Steven Helfand in helping us to attain data and to better understand their institutional meaning and context. Steven Helfand and Pedro Aratanha offered useful comments on an earlier version of this research, as did participants in a presentation at the 2013 Eastern Economic Association Meetings in New York and the 10th International Conference on Developments in Economic Theory and Policy in Bilbao, Spain. Rubens Morita, Régis B Oliveira, and Amber Qureshi provided excellent research assistance.

1. Introduction

Brazil witnessed a rather significant decline in labor informality over the first decade of the 21st century – a decline that brought informality from roughly 50% to 40% of the urban labor force. Economic growth was rapid over much of the period; enforcement of labor law violations was made more efficient; the real value of the minimum wage more than doubled; and the largest conditional cash transfer program in the world – *Bolsa Família* – was begun. In addition, industrial composition changed over the period, as did several demographic features of the labor force, including average education levels and age. How have these factors contributed to changing informality over the period?

Using Demographic Census data and other institutional data sources over the period 2000 to 2010, we explore the determinants of informality by exploiting variation in informal employment across workers and municipalities and estimating cross-sectional informality regressions in both years. The change in mean informality rates over the period are decomposed using a Blinder-Oaxaca decomposition. We employ an instrumental variables analysis to identify the causal impact of enforcement efforts, conditional cash transfers, and the minimum wage. Various robustness analyses are also presented, including a municipal fixed effects estimation that controls for time-invariant features of municipalities over the period.

The first insight from analysis of the data is that while informality fell by about 10 percentage points over this period, over 80% of the decline took place among wage and salary workers as opposed to the self-employed. We tailor our model to capture the determinants of informality in this particular segment of the informal economy and focus our empirical analysis on this segment only. We begin with a review of the literature on the determinants of informality.

2. Determinants of Wage and Salary Informality in Brazil

Two major policy changes during the first decade of the 2000s in Brazil, with potentially major consequences for the extent of informality in the country, were the emergence and growth of the conditional cash transfer program *Bolsa Família* and the near doubling of the real value of the minimum wage.

Bolsa Família originated in 2003 with the new Lula administration in Brazil. It brought together under one umbrella existing municipal and federal cash transfer programs[1] and expanded the federal conditional cash transfer (CCT) component significantly, growing within a brief period of time to become the single largest CCT program in the world. By decade's end, *Bolsa Família* was serving roughly one-quarter of the poorest households in the country, sending cash to many families conditional on their achieving targeted goals for the health, nutrition, and education of their children, but also granting unconditional cash transfers to the very poorest households.

Evidence is clear that the program had a significant impact on rising school attendance and ultimately educational levels in Brazil (Cardoso and Souza, 2003; Glewwe and Kassouf, 2012). Empirical research on informality suggests most strongly that rising education levels tend to depress informal sector employment. However, there is an additional channel through which *Bolsa Família* may affect informality. Program rules

[1] *Bolsa Escola* was one such program. It became a federal conditional cash transfer program in 2001, following experimentation with conditional cash transfers in several municipalities dating back to the early 1990s (Soares 2012).

establish clear per capita family income maxima for eligibility, but income is self-reported and verification is possible only when workers' income is independently reported to federal authorities – that is, only for workers in the formal sector of the economy. Hall (2008) argues that this feature of the program might cause some workers to shun formal sector employment and to choose, instead, work in the informal sector, where they would be more likely to qualify for benefits, possibly through fraudulent reporting of income. He cites anecdotal evidence from a Brazilian study (2008, p. 815) showing precisely this sort of incentive operating among temporary rural workers. De Brauw, Gilligan, Hoddinott, and Roy (2014) employ household panel data and a difference-in-differences identification strategy to establish credible evidence that urban area recipients of the program reduce labor hours in the formal sector and increase hours in the informal sector in comparison to a control group of nonparticipants.

The real value of the minimum wage doubled in the first decade of the 21st century in Brazil, as policy increasingly focused on reducing poverty, including among the working population. In a conventional two-sector model, with a covered and uncovered sector, theory predicts that an increase in the minimum wage should raise wages and reduce employment in the covered (formal) sector, and, as workers gravitate to the uncovered (informal) sector, wages should fall and employment should rise therein. However, in Brazil – as is true of several other Latin American economies – the impact of the minimum wage on wages in the formal and informal sectors is more complex.

There is significant evidence to suggest that the minimum wage has so-called "lighthouse" and "numeraire" effects on wages in both sectors (Maloney and Nuñez 2004). That is, the minimum wage appears to be viewed as a (lighthouse) signal of fairness in wage setting and as a useful (numeraire) index for wage increases over time, for workers both above and below the actual statutory minimum in the formal and informal sectors alike. Evidence of wage clustering around multiples of the minimum wage can be found in both the formal and informal sectors in several Latin American economies (Neri et al. 2001; Amadeo et al. 2000; Fairris et al. 2008).

Several empirical investigations into the minimum wage in Brazil confirm the existence of wage increases above and below the minimum, in both the formal and informal sectors, following a minimum wage hike (e.g., Fajnzylber 2001 and Lemos 2009). Moreover, there is some evidence to suggest that the impact on wages is greatest in the informal sector (Maloney and Nuñez 2004). This obviously complicates the story of the likely employment impact of the minimum wage. Is the conventional prediction still correct – in this case, implying that informal employment growth due to spillover effects is offset by rising informal sector wages due to lighthouse and numeraire effects? Evidence to date seems to suggest that indeed minimum wages decrease formal sector employment and increase informal sector employment, consistent with the dominance of the spillover effect (Fajnzylber 2001, Carneiro 2004), but the estimated impacts are not always statistically significantly different from zero (Lemos 2009).

The extent of informality in a society is ultimately the result of the individual choices of employers and workers. On the employer side, businesses must decide whether to operate legally or under the radar, for all or some subset of their workforce. This decision hinges on the relative costs and benefits of operating formally versus informally. One possible cost of informality is the risk of being caught and fined by the authorities for violating labor law.

The issue of labor law enforcement in Brazil is a complicated one. Until the late 1980s, there appears to have been little enforcement of laws affecting work and workers. This changed when a new set of labor standards was included in the 1988 Constitution, and by the early 1990s there existed a staff of roughly 3,000 highly-paid and professional inspectors – a number that would remain largely unchanged in the two decades to follow (Berg 2010). Compliance with labor regulations is the responsibility of the Ministry of Labor in Brazil, and enforcement is delegated to ministry offices which are sprinkled throughout the country.

Despite relative stasis in the number of inspectors the effectiveness of inspections was enhanced enormously in the period from the late 1990s to the late 2000s through two developments. First, a system of incentive pay was introduced which linked inspector income to the achievement of specific performance targets. Second, teams of inspectors were given increased freedom to work with non-compliant firms to explore ways of bringing firms into compliance that would prove beneficial to both workers and firms – an approach that moved away from repeated inspections and enforcement to one focusing on making compliance "sustainable" in the long run (Pires 2008). Labor Ministry data reveal that between 1996 and 2008 the number of workers brought into formal sector status through labor inspections more than doubled (Berg 2010, p. 15).

Experts on labor standards compliance in Brazil are clear that much of the progress in enhancing formality during this period was accomplished through the formalization of informal workers in large, formal sector firms, since inspectors focused their energies during this period almost exclusively on such firms (Cardoso and Lage 2007). There may be an unintended, positive impact on formality stemming from stepped-up compliance with constitutionally-mandated benefits such as severance pay or health and safety standards as well. If improvements in these areas attract informal workers to formal sector jobs, and if wages fall as a consequence, formal sector firms might be encouraged to expand their workforces (Ameida and Carneiro 2012).

The empirical evidence linking inspections to formality is relatively sparse. Simulations with Brazilian data, employing a two-sector matching model with formal and informal sectors, suggest that increased enforcement reduces informality (Ulyssea 2010). Almeida and Carneiro (2009) use a rich data set on the intensity of inspections across Brazilian cities and data on formal-sector firms to show that enforcement reduces firm size, which, because small firm size is a major identifier of likely informal sector status, suggests that costly compliance may push firms into informality. Finally, Almeida and Carneiro (2012) utilize the same Brazilian inspections data and the 2000 Brazilian Census to explore directly the link between inspections and informality, and find evidence of increased formality in cities with high levels of enforcement.

Changes in the demographic composition of the labor force during this period might also have contributed to declining informality. Education, age, and gender, among other features of the workforce, are clearly correlated with informal sector status.[2] The explanations for this observed correlation are varied and controversial. We take no strong view on whether the correlation reflects labor market segmentation, and thus the forced relegation of a subset of workers to informal sector status, or instead competitive labor

[2] Mello and Santos (2011) offer evidence suggesting that increased educational attainment accounts for part of the decline in informality over the period of our investigation.

markets, and thus a setting in which workers freely choose to locate among the informal wage labor force. While the empirical evidence is clear that the typically dispossessed – the young and old, women, and the uneducated – are disproportionately to be found in the informal sector workforce (e.g., Perry et al. 2007), whether this is by force or by choice is less clear.

Maloney (2004) reports that roughly 30% of surveyed informal salary workers in Brazil would not wish to work in the formal sector. Work time flexibility in the informal sector may be attractive to women with children and to older workers who have retired with pension benefits from the formal sector. Almost 20% of those women who prefer working in the informal sector in Brazil cite household chores or needing time for other activities as the reason for choosing to work informally (Maloney 2004). The young may not find value in the pension and health benefits common to formal-sector status. And those who have spouses working in the formal sector, and thus qualifying for family benefits by virtue, may be free to locate in the informal sector without significant loss. On the other hand, the less educated are almost assuredly there by force and not choice. Perry et al. (2007, p. 62) state: "….graduation to formal salaried work is unlikely for youth who drop out of school before completing at least a full course of secondary education." Arguably, a portion of the women, elderly, and younger workers in the informal sector are also likely to be there not by choice.

Leaving aside the labor market segmentation debate, what has happened to these demographic features of the population and labor force over the course of the decade 2000–2010? Well before the first decade of the current century, Brazil was undergoing a rather significant demographic shift in the age of the population. Declining fertility rates and rising life expectancy were leading to an aging of the population. By the 2000s, the declining fertility rates were impacting the working age population. Berg (2010) reports that household data in Brazil reveal a fall in the percentage of the population ages 15–24 from 18.6% to 17.7% over just the few years 2005–2008 (p. 12). We find an increase in the average age of the labor force in our data, consistent with the trends observed on fertility and life expectancy rates.

Another factor limiting the youth population in the labor force is increased school enrollments, making young people less available for work. The percentage of youth ages 15–17 enrolled in school has climbed steadily since the early 1990s. As noted above, the *Bolsa Família* program of the 2000s had a marked impact on this trend; Berg (2010) reports that the percentage of youth in this age category economically inactive increased from 57% in 1999 to 65% in 2008. This shows up in our data not just on the aging of the labor force, but also in rising education levels of those engaged in active employment. We find an increased percentage of women in the labor force, as well as an increased percentage of individuals with spouses working in the formal sector. The aggregate effect of these changes, as well as those discussed above, awaits statistical analysis.

We are unable to directly capture several features of the Brazilian economy that have been linked to declining informality during this period. The first is trade liberalization and rapid economic growth. Annual growth in GDP was 4.2% during the period 1999–2008, and exports grew by almost 80% over the period (Paz, 2012). Export-led growth expansions are known to be particularly conducive to employment growth in the formal sector (Corseuil and Foguel 2012). A second factor is increased availability of credit for small, formal sector firms. Catão et al. (2009) show that credit to firms expanded dramatically over the period 2003–2008 in Brazil, from roughly 15% of GDP to around 22%, and then use Brazilian data covering the period 2002–2006 to show that this credit deepening contributed to declining informality. Finally, the Simples Law, enacted in 1996, created a system of tax simplification for small and micro enterprises and various tax exemptions as well. Berg (2010, p. 17) cites two studies exploring the link between the Simples Law and declining informality in Brazil in ensuing years, one of which covers the 10-month period following the law's onset and showing evidence of a 13 percentage-point increase in formal licensing among retail firms who were eligible for the benefits of the law compared to a control of similar firms that were ineligible.

We believe that changes in industry composition over the period may allow us to pick up some of these otherwise uncaptured factors contributing to declining informality over the period. All three are plausibly related to changing industry composition, or to the changing ways in which a given industry composition relates to informality. To cite just one example, Catão et al. (2009) suggest that informality should vary across municipalities, business credit held constant, depending on industrial composition and therefore the varying need for external funding. Moreover, as business credit expands over the decade, the way in which industrial structure translates into formality should also change, with formality growing most in those industries which are most in need of external funding. We control for municipal-level industry composition in our regressions, and for its changes over time in our decomposition analysis, but are obviously unable to parse out the various causal mechanisms that are at work behind the scenes of these changes in industrial structure and composition. We turn, now, to the specifics of our empirical methodology and data.

3. Empirical Methodology and Data

3.1. Empirical methodology

To analyze the drivers of changing informal wage and salary employment over the first decade of the 21st century in Brazil, we estimate probability models using worker- and municipal-level data, one for 2000 and one for 2010, based on data drawn from the Demographic Census of these two years, and from various institutional data sources to be discussed below. Using the probability regression results from the two periods, we decompose the change in mean informality rates over the period into changes in the means of explanatory variables and changes in the estimated regression coefficients. We use a linear probability model specified as follows;

$$Prob(is_{im} = 1) = \boldsymbol{X_{im}\beta} + \boldsymbol{Z_m\gamma} + \varepsilon_{im} \qquad (1)$$

$Prob(is_{im} = 1)$ denotes the probability that worker i in municipality m is employed in the informal sector (employment in the formal sector = 0). $\boldsymbol{X}$ denotes a vector of worker characteristics, including education, age, and gender – many of which are hypothesized to be related to likelihood of informal employment – and $\boldsymbol{Z}$ **is** a vector of municipal characteristics, including variables capturing labor law enforcement, conditional cash transfers, minimum wage effects, and industrial composition, each of which are hypothesized to affect the likelihood of municipal-level informal employment. $\boldsymbol{\beta}$ and $\boldsymbol{\gamma}$ are vectors of coefficients to be estimated and ε is an error term, assumed to follow a normal distribution with zero mean and variance σ.

We use a linear probability model since this allows the most straightforward interpretation of the decomposition findings.[3] The model gives the following relationship between the independent variables and the dependent variable:

$$\overline{\iota s} = \overline{\boldsymbol{X}}\widehat{\boldsymbol{\beta}} + \overline{\boldsymbol{Z}}\widehat{\boldsymbol{\gamma}} \tag{2}$$

Upper bars indicate means and hats indicate estimated coefficients. Let subscripts 0 and 1 denote year 2000 and year 2010, respectively. Using a Blinder-Oaxaca decomposition, the change in the mean informality rate over the period is given by:

$$\overline{\iota s}_1 - \overline{\iota s}_0 = \left(\overline{\boldsymbol{X}}_{\boldsymbol{1}}\widehat{\boldsymbol{\beta}}_{\boldsymbol{1}} + \overline{\boldsymbol{Z}}_{\boldsymbol{1}}\widehat{\boldsymbol{\gamma}}_{\boldsymbol{1}}\right) - \left(\overline{\boldsymbol{X}}_{\boldsymbol{0}}\widehat{\boldsymbol{\beta}}_{\boldsymbol{0}} + \overline{\boldsymbol{Z}}_{\boldsymbol{0}}\widehat{\boldsymbol{\gamma}}_{\boldsymbol{0}}\right) \tag{3}$$

By adding and subtracting terms, expression (3) can be re-stated as:

$$\overline{\iota s}_1 - \overline{\iota s}_0 = \left[(\overline{\boldsymbol{X}}_{\boldsymbol{1}} - \overline{\boldsymbol{X}}_{\boldsymbol{0}})\widehat{\boldsymbol{\beta}}_{\boldsymbol{0}} + (\overline{\boldsymbol{Z}}_{\boldsymbol{1}} - \overline{\boldsymbol{Z}}_{\boldsymbol{0}})\widehat{\boldsymbol{\gamma}}_{\boldsymbol{0}}\right] + \left[(\widehat{\boldsymbol{\beta}}_{\boldsymbol{1}} - \widehat{\boldsymbol{\beta}}_{\boldsymbol{0}})\overline{\boldsymbol{X}}_{\boldsymbol{1}} + (\widehat{\boldsymbol{\gamma}}_{\boldsymbol{1}} - \widehat{\boldsymbol{\gamma}}_{\boldsymbol{0}})\overline{\boldsymbol{Z}}_{\boldsymbol{1}}\right] \tag{3'}$$

The first term in square brackets is the change in mean informality accounted for by changes in elements of the $\boldsymbol{X}$ and $\boldsymbol{Z}$ vectors, and the second term is the change accounted for by changes in the structural parameters.[4] These two terms are commonly referred to as the explained and the unexplained parts, respectively, of the change in the dependent variable. The second term is ‘unexplained’ in the event that the changes over time in the estimated coefficients have no straightforward explanation.

Some of the independent variables are unlikely to be exogenous to the variation in informality rates, and so we utilize instrumental variables techniques to render them causally determinative. This is clearly the case for labor law enforcement and conditional cash transfers. Labor law enforcement may be successful in reducing informal employment, but, to the extent enforcement is targeted accordingly, municipalities with high degrees of informality will also contain inordinately high enforcement efforts. Our discussion above suggests that conditional cash transfers influence informality rates through their impact on the informal/formal relative wage, which is clearly endogenous in the informality regression. The expansion of conditional cash transfers might be expected to shift relative labor supply to the informal sector, thereby lowering the informal/formal relative wage.

3.2. Worker-level variables

Variable definitions appear in Table 1. We relegate to a “data appendix” more specificity regarding variable measurement and sources. The dependent variable in the analysis is a binary variable, taking the value of 1 if the worker is employed in the informal sector and 0 if the worker is employed in the formal sector. As explanatory worker characteristics ($\boldsymbol{X}$) we include gender (female=1), age, education, a vector of race and ethnicity categories, and disability. The first three are discussed in some detail in the literature review, and

[3] We estimated probability models with probit and logit specifications and obtained qualitatively very similar results.

[4] The choice of weights in the decomposition is arbitrary. β_0 and γ_0 can be replaced by β_1 and γ_1, with the corresponding changes in the second term, so that expression (3') becomes $\overline{\iota s}_1 - \overline{\iota s}_0 = \left[(\overline{\boldsymbol{X}}_{\boldsymbol{1}} - \overline{\boldsymbol{X}}_{\boldsymbol{0}})\widehat{\boldsymbol{\beta}}_{\boldsymbol{1}} + (\overline{\boldsymbol{Z}}_{\boldsymbol{1}} - \overline{\boldsymbol{Z}}_{\boldsymbol{0}})\widehat{\boldsymbol{\gamma}}_{\boldsymbol{1}}\right] + \left[(\widehat{\boldsymbol{\beta}}_{\boldsymbol{1}} - \widehat{\boldsymbol{\beta}}_{\boldsymbol{0}})\overline{\boldsymbol{X}}_{\boldsymbol{0}} + (\widehat{\boldsymbol{\gamma}}_{\boldsymbol{1}} - \widehat{\boldsymbol{\gamma}}_{\boldsymbol{0}})\overline{\boldsymbol{Z}}_{\boldsymbol{0}}\right]$.

are hypothesized to affect informality in precisely the ways discussed therein. Race and disability may relate to informality status based on discriminatory placement practices as hypothesized in dual or segmented labor market models.

We include two additional explanatory variables related to worker characteristics. The first indicates whether the worker has a spouse working in the formal sector. Some have hypothesized that *formal* sector employment for one household member may encourage *informal* employment for other household members (e.g., Perry et al., 2007). This is especially possible if some benefits of formal employment cover the entire family or household, if the expected after-tax income in the formal sector is close to the (untaxed) income from informal employment, and if other household members value the flexible hours or other aspects of informality. Search theory, and especially the importance of worker referrals in employer search, offers an alternative hypothesis – namely, those with spouses in the formal sector are more likely to be offered, and perhaps to take employment in the formal sector.

The second variable is an interaction term that equals 1 if there are young children present in the household and the worker is the only adult woman in the household. This variable is included based on the hypothesis that women with young children, who do not have other (adult female) household members assisting them with childcare, are those most in need of the working time flexibility associated with an informal job.

All worker-level variables included in the empirical analysis are derived from the Brazilian Demographic Census from years 2000 and 2010. The micro data from the Census are based on the long-form questionnaire and consist of 20.4 million observations for 2000 and 20.6 million observations for 2010. The large number of observation makes the data representative at the municipal level, which is an advantage over other data sources. There were 5507 municipalities in Brazil in year 2000 and 5565 in 2010. The National Household Sample Survey (PNAD), which has been used in previous analyses of informality, is an annual survey covering the entire country of Brazil but is only representative at the state level. The Monthly Employment Survey (PME), which has also been used to document and analyze informality, covers only six major metropolitan areas and hence is unable to reveal developments outside the major metropolitan areas of Brazil.

In our analysis we restrict the sample in several ways. We include only urban wage employees of age 15 to 65 years who report a monthly income and work in the private sector of the economy. Hence we exclude all people residing in rural areas according to the Census definition. Regions are defined as rural and urban on an administrative basis, not on population density or the size of cities, towns or villages. According to the Census data, 81 percent of the Brazilian population lived in urban places in 2000 and 84 percent in 2010. Rural areas are generally dominated by agriculture and the majority of the rural labor force is engaged in family farming. The rural labor market therefore deserves a different analytical framework and the notion of formal and informal work has limited applicability.

Table 1. Variable definitions

Variable	Description
Worker-level variables	
Informal employment ^{d}	Worker is employed in the informal sector (=1, 0 otherwise).
Age	Worker's year of age.
Primary education or less ^{d}	Worker has primary education or less (base variable in regressions).
Secondary education ^{d}	Worker has completed secondary education.
College education ^{d}	Worker has completed college education.
Female ^{d}	Worker is female.
Female with child ^{d}	Worker is female with young children (10 years or younger) in the household. No other adult female in the household.
Formal-sector spouse ^{d}	Worker has spouse working in the formal sector.
Race ^{d}	Indicator variables for black, Asian, white, mixed, and indigenous (white is base variable in regressions).
Disabled ^{d}	Worker has reduced working ability (eyesight disability, hearing disability, permanent mental disability, or other disability).
Municipal-level variables	
CCT coverage	Conditional cash transfer payments per capita in municipality (R$ per month x10).
Labor law enforcement	Number of workers inspected by labor inspections as share of total number of wage workers in municipality.
Minimum-wage bindingness	Share of formal workers paid multiples of the minimum wage minus the share of informal workers paid multiples of the minimum wage.
CCT take-up rate	Instrumental variable for *CCT coverage*. Share of eligible households receiving Bolsa Familia payments (in 2010) or payments from any cash transfer program excluding unemployment benefits and pensions (in 2000).
Drive time to labor office	Instrumental variable for *Labor law enforcement.* Traveling time from municipal seat to responsible labor inspection office.
Urbanization	Share of households in municipality residing in an urban area.
Industry categories	16 fractional (share) variables giving the share of workers in the municipality employed in agriculture, fishing, extraction, manufacturing, utilities, construction, retail trade, housing, transportation, finance, real-estate services, public administration, education, health services, other public services, and domestic services. Domestic services is the base sector in regressions.

Note: Dichotomous variables are indicated by superscript *d*.

When restricting the sample to the urban labor force, informality was 40 percent in 2010, compared to 50 percent ten years earlier. In Table 2, the informal sector is decomposed into wage workers, self-employed, and domestic workers. The biggest share of the decline in informality has taken place among wage workers; the drop from 26.4 to 18.3 percent corresponds to about 80 percent of the overall decline in informality. Thus, in this study we exclude self-employed workers and focus solely on wage employees.

Table 2. Formal and informal employment, 2010 and 2000

	2010	2000
Formal employment	59.1%	49.9%
Informal employment	40.1%	50.1%
of which:		
Wage employees	18.3%	26.4%
Self-employed	16.1%	17.6%
Domestic employees	5.6%	6.0%

Note: Urban labor force, 15-65 years of age, excluding unpaid workers.
Sources: Demographic Census, 2000 and 2010.

3.3. Municipal variables

The vector of municipal characteristics (***Z***) consists of a set of institutional, policy-related variables and another set of variables capturing the industrial composition of the local economy. The variables accounting for industrial composition are constructed from the Census data and defined as the share of workers in different industries, 16 categories in all. We also include as a control variable the share of households in the municipality that are urban. The degree of urbanization is meant to control for differences in quality of and access to infrastructure, agglomeration economies, and other aspects of urbanization that may have an effect on the formalization of the labor market.

The main policy-related variables constitute municipal level measures of the reach of conditional cash transfers, the enforcement of labor regulations, and the impact of the minimum wage. For two of these, there are legitimate concerns with endogeneity bias if direct measures of these municipal-level features are employed, and so we turn to instrumental variables procedures to rid the estimated impacts of such bias.

a) Conditional cash transfers (CCTs)

The Census data contain information on the receipt of conditional cash transfer benefits. However, exploring the relationship between CCTs and informality with direct measures of *CCT coverage* is fraught with problems of endogeneity; informality rates across municipalities might well be affected by such transfers, but transfers are also likely to be a function of the municipal level of informality, which is a likely marker for low family income and thus eligibility for CCTs. Thus, we utilize an instrumental variable (IV) procedure to capture the impact of CCTs on informal employment. The instrument we employ is the *CCT take-up rate* among the population that is eligible for the program. One can think of this measure as capturing, across municipalities, both the awareness of the program among the eligible population and the efficiency of processing applications for social transfers by municipal authorities. We expect the IV estimate of the *CCT coverage* impact on informality to be positive. On the one hand, the program is likely to decrease informality by raising levels of education. However, on the other hand, CCTs might cause workers to opt for informal employment as a way of hiding labor earnings, the extent of which can only be verified by federal authorities when income is generated from formal employment. Having controlled for education elsewhere in the regression, it is this latter aspect of CCT programs that we expect to capture.

b) Labor law enforcement

Regarding labor law enforcement, we were provided, by the Brazilian Labor Ministry, with data on "the number of workers affected" by inspections (i.e., the sum total of workers in all inspected firms) in both 2000 and 2010, measured at the level of the municipality.[5] To create the labor law enforcement variable, we divide the numbers on workers affected by the total number of employees in the municipality, which is derived from the Census data. Of course, labor law enforcement may well be endogenous in the informality equation, so long as enforcement is targeted to those areas of high informality, and so we instrument this intensity measure with drive time to labor office – the time it takes to drive from each municipal seat to the local Labor Ministry office responsible for labor law enforcement, based on the procedure adopted by Almeida and Carneiro (2009, 2012). The identifying assumption is that the closer an employer is to a local labor office, the stricter is the enforcement of labor regulations. Almeida and Carneiro argue that the drive time measure serves as an ideal instrument for an "intensity of enforcement" variable in that the former is likely to be directly (negatively) related to enforcement intensity and yet affect informality only through its impact on the intensity of enforcement.

Almeida and Carneiro utilize drive time between each municipality and the *nearest* Labor Ministry office delegated with labor law inspections. We utilize their data from 2002 on drive time and match this to the municipalities in which individuals are located in our 2000 Census data. For the later period, we rely on information received directly from the Brazilian Labor Ministry indicating which specific Labor Ministry office is in fact *directly responsible* for enforcement in each municipality in 2010. We access drive time in this case using a google-maps based search engine in 2014. It turns out that the nearest Labor Ministry office to a particular municipality is not always the one directly responsible for local labor law enforcement; thus, Almeida and Carneiro employ a faulty measure of drive time in their analysis. While there is significant overlap in the two approaches – the labor ministry office that is directly responsible for labor law enforcement in a given municipality is also typically the nearest – there are nonetheless also some discrepancies. We have no option but to use their data for 2002, but as a robustness check, we make some attempts later in the paper to render the two approaches similar, creating two samples in which the office that is directly responsible is in fact also the nearest office.

Drive times may differ over two time periods for several reasons, holding aside the issue of measurement inconsistencies. We know, for example, that the number of local labor ministry offices has changed; three offices closed and six new offices opened during the decade 2000–2010.[6] This is likely to alter the drive time for labor inspectors as they make their way to municipalities to inspect firms. Moreover, new roads may have been built, thereby reducing drive time, or congestion may have worsened, thereby increasing

[5] The numbers reflect each inspection, even if a given firm is inspected more than once, and even if the repeated inspection regards the same, initial violation.
[6] There were 143 labor ministry offices in 2010.

drive time. It is these differences we hope to exploit in the decomposition analysis to discern the changing contribution of enforcement efforts to changing informality over time.[7]

c) *Minimum wage effects*

Our last policy-related variable is aimed at capturing the impact of the near doubling, in real terms, of the minimum wage in Brazil over the course of the decade 2000-2010. While the minimum wage is the same throughout Brazil, and thus does not vary across municipalities, its impact on municipal labor markets is nonetheless likely to vary depending on the relation between the minimum wage and the average wage or average relative (formal/informal) wage in the municipality.

We try to capture the impact of the minimum wage on municipal labor markets, based on the now well-established finding that minimum wages have both "lighthouse" and "numeraire" effects in many Latin American countries, including Brazil. The literature has captured these normative features of minimum wages by exploring the existence of spikes or clusters around multiples or even fractions of the minimum wage in both formal and informal wage distributions. These effects are largely normative (as opposed to statutory) and we hypothesize (and show evidence to support the claim) that they vary across municipalities. In particular, we hypothesize that strong lighthouse or numeraire effects in the wage-setting process in the formal sector have a positive impact on the formal/informal relative wage. Thus, *ceteris paribus*, the stronger the lighthouse effect in the formal sector of a municipality, the higher is the formal/informal relative wage in the municipality, and the higher is the rate of informality, as employers on the margin opt for informal sector status or employ informal sector workers in outsourcing arrangements instead of employing formal sector workers directly.

We try to capture this lighthouse effect in the following manner. We first account for the share of formal workers in a municipality receiving exactly 1 to 4 multiples of the minimum wage, and then account for the share of workers paid one-half and 1 to 4 multiples of the minimum wage in the informal sector as well.[8] The difference between these two shares – the formal and informal – gives us a measure of the 'relative strength' of the normative role of the minimum wage in the wage-setting process in the two sectors. We define *minimum wage bindingness* as the share of workers paid in multiples of the minimum wage in the formal sector subtracted by the share of workers paid in multiples of the minimum wage in the informal sector. The rationale of the variable is the following. If the lighthouse effect is more evident in the formal sector than in the informal sector we suggest that the minimum wage has a larger effect – stronger "bite" – in the formal than in the informal sector. As a consequence, an increase in the (national) minimum wage will affect wages more in the formal sector than in the informal sector, which increases the formal/informal sector relative wage.[9] A higher formal/informal sector relative wage, in turn, is likely to increase informality. We specify the informality regression by including *minimum wage bindingness* as an

[7] We note that drive time is not some constant multiple of distance, based for example on an average speed measure for the country or region, and thus represents expected elapsed time in driving between two distances.
[8] As shown in the appendix figures there are spikes in the wage distribution at multiples of R$151 in 2000 and R$510 in 2010, which were the levels of the minimum wage in those two respective years.
[9] In cross-municipal regressions not reported here, the minimum wage bindingness variable was associated positively with the municipal formal/informal sector relative wage, controlling for a series of other municipal characteristics.

independent variable directly, confident that it captures, in an exogenous fashion, the impact of minimum wages on worker and employer incentives to locate in the informal sector.

Thus, going forward we create instrumental variables for enforcement and conditional cash transfers, with two instruments, and the minimum wages bindingness itself appears as an independent variable in the informality regression. Tests for weak instruments are soundly rejected,[10] but because the model is just identified we are unable to test formally that the instruments satisfy the conditional moment restriction – i.e., that they are valid.

d) Descriptive statistics

Descriptive statistics of the variables included in the empirical analysis are provided in Table 3. Information for some of the institutional variables is not available for all municipalities. The regression samples, therefore, only include 5284 municipalities for 2000 and 5481 for 2010. A consequence of this is that the rates of informality in the wage labor force (24 and 30 percent for 2010 and 2000, respectively) are not fully consistent with the rates implied by those given in Table 2. We return to a discussion of changes in means in the decomposition analysis below, focusing here on a few important observations regarding the data.

Evident among the worker characteristics is the increased level of education. In year 2000, about 47 percent of the workforce had secondary education or more. Ten years later this share had increased to 57 percent. The female percentage of wage and salary workers increased by almost three percentage points and the mix of race and ethnicity categories changed slightly. Among the institutional variables, the most striking development over time is the increased coverage of social transfers to poor households. Between 2000 and 2010 per capita conditional cash transfer payments increased 50 fold. This, of course, is a development largely driven by the emergence and growth of the *Bolsa Família* program.

The intensity of labor inspections increased only slightly over the period – by less than two percentage points. We note that the raw numbers include multiple counting of workers, depending on the number of times a workplace is inspected. Even taking this into account, that inspections touch such a high percentage of the workforce is rather impressive. Finally, while the normative commitment to paying multiples (or fractions) of the minimum wage is greater in the informal sector in both periods (which is consistent with findings in the literature more generally), the difference declines over the period, portending an increase in the formal/informal relative wage.

As for changes in the composition of the labor market, sectors such as manufacturing, construction, and retail trade have increased somewhat in relative importance, whereas a smaller share of the labor force works in public administration and education. Thus informality has decreased considerably over the past decade despite the fact that the public sector – in which employment is most certainly formal – has decreased its importance as an employer.

[10] The F-tests for joint significance of the three instruments in each of the first-stage runs are well over 10 – the rule of thumb proposed by Staiger and Stock (1997).

Table 3. Descriptive statistics

Variable	2010 Mean	2010 Std. dev	2000 Mean	2000 Std. dev
Worker characteristics				
Informal employment ^{d}	0.244	0.43	0.297	0.46
Age	35.0	11.2	33.8	10.7
Primary education or less ^{d}	0.43	0.49	0.53	0.50
Secondary education ^{d}	0.42	0.49	0.35	0.48
College education ^{d}	0.15	0.36	0.12	0.32
Female ^{d}	0.37	0.48	0.34	0.48
Female with child ^{d}	0.09	0.29	0.10	0.30
Formal-sector spouse ^{d}	0.23	0.42	0.18	0.39
Race - black ^{d}	0.08	0.27	0.07	0.25
Race - white ^{d}	0.52	0.50	0.60	0.49
Race - Asian ^{d}	0.01	0.10	0.00	0.07
Race - mixed ^{d}	0.39	0.49	0.32	0.47
Race - indigenous ^{d}	0.00	0.04	0.00	0.05
Disabled ^{d}	0.03	0.18	0.02	0.13
Institutional characteristics				
CCT coverage	5.43	7.57	0.10	0.29
Labor enforcement	0.52	0.63	0.50	0.66
Minimum-wage bindingness	0.00	0.12	-0.03	0.08
CCT take-up rate (IV)	0.28	0.17	0.02	0.04
Distance to labor office (IV)	1.15	1.64	0.74	1.22
Urbanization	0.87	0.17	0.88	0.16
Industry categories				
Agriculture	0.07	0.09	0.06	0.09
Fishing	0.00	0.02	0.00	0.01
Extraction	0.01	0.02	0.00	0.01
Manufacturing	0.16	0.11	0.16	0.09
Utilities	0.01	0.01	0.01	0.00
Construction	0.09	0.03	0.08	0.03
Retail trade	0.21	0.05	0.19	0.04
Housing	0.04	0.02	0.05	0.02
Transportation	0.05	0.02	0.06	0.02
Financial services	0.01	0.01	0.02	0.01
Real-estate services	0.09	0.05	0.07	0.04
Public administration	0.04	0.03	0.06	0.04
Education	0.05	0.02	0.07	0.02
Health services	0.04	0.02	0.04	0.02
Other public services	0.04	0.01	0.04	0.01
Domestic and other services	0.09	0.03	0.09	0.03
Number of observations	*3,482,077*		*2,574,077*	
Number of municipalities	*5481*		*5284*	

Note: Categorial (dummy) variables are indicated by superscript *d*. Institutional variables are defined at municipal level. Variables for industry categories are defined as share of the municipal labor force in the respective sector.

Sources: Demographic Census 2000 and 2010; Base Estatcart de Informações Municipais 2000 and 2009; Ministry of Labor.

4. Empirical Results

Table 4 gives the results of the estimated informality regressions for 2000 and 2010. By way of overview, we note that all of the estimated coefficients on the institutional and demographic variables are of the predicted signs, when clear predictions were made, and all are statistically significantly different from zero. Tables 3 and 4 provide all of the relevant information required for the decomposition analysis, which can be found in Table 5.

The decomposition analysis in Table 5 allows us to explore separately the impact of changing estimated coefficients and changing means of determinative variables on the overall changing mean rate of informality over the period. This is done for two different sets of weights to insure consistency in the findings (see the discussion in footnote 4 above). For each right-hand side, determinative variable, we calculate the weighted impacts of the changing coefficients and the changing means on the change in mean informality over the period. This is done for weight 1 in columns 1 and 2 of Table 5, respectively. The percentage of the overall change in mean informality (0.053) accounted for by these respective changes is given in columns 3 and 4 of the table. Columns 5–8 give the same information, but using the second set of weights. For any given determinative variable, we can add the percentages in columns 3 and 4 (or 7 and 8 for the second set of weights) to give the summative change in mean informality accounted for by changes in both estimated coefficients and means over the period. This is the approach we take in the discussion below, but we also direct the readers' attention to the separate contributions of coefficients and means when the results are interesting.

It is common in decomposition exercises to attribute the change in means over a period to something "known" or "explained," and to refer to the change in estimated coefficients as representing "unknown" or "unexplained" forces. In some respects this makes sense, but a strong institutional understanding of the background features that structure the relationship between a given independent variable and a dependent variable, and how these structural features have changed over time, may allow the researcher to offer speculative explanations for the changing coefficients themselves. In several instances, we offer such speculative explanations.

Finally, the decomposition analysis is derived from a straightforward exercise focusing on the magnitude of estimated coefficients and means; it does not discriminate between variables that are statistically significant or insignificant in accounting for variation in informality rates across municipalities. We focus our attention in a discussion of the decomposition results largely on those variables that are significant from a statistical perspective.

Looking first at municipal level measures of the policy variables and their changes over time, we can begin with the minimum wage effects. The results in columns 1 and 2 of Table 5 suggest that the more important is the minimum wage as a wage-setting norm in the formal sector, the larger is informal employment. This is consistent with the hypothesis that the stronger the relative impact of the minimum wage norm in the formal sector, the larger is the formal/informal relative wage and thus the higher is the rate of informality, as employers on the margin opt for informal sector status or employ informal sector workers in outsourcing arrangements instead of employing formal sector workers directly.

Table 4. Regression Results

	2010		2000	
	Coefficient	Std error	Coefficient	Std error
CCT coverage	0.016***	0.001	0.068*	0.035
Labor enforcement	–0.165***	0.045	–0.583***	0.210
Minimum-wage bindingness	0.137***	0.026	0.152***	0.058
Age	–0.020***	0.000	–0.029***	0.000
Age squared	0.025***	0.000	0.035***	0.001
Secondary education [d]	–0.095***	0.002	–0.109***	0.004
College education [d]	–0.096***	0.003	–0.108***	0.005
Female [d]	0.022***	0.002	–0.002	0.002
Female with child [d]	0.021***	0.001	0.031***	0.002
Formal-sector spouse [d]	–0.079***	0.001	–0.070***	0.003
Race - black [d]	0.019***	0.003	0.032***	0.009
Race - mixed [d]	0.025***	0.003	0.048***	0.010
Race - Asian [d]	0.033***	0.004	0.045**	0.018
Race - indigenous [d]	0.057***	0.012	0.077***	0.015
Disabled [d]	0.033***	0.002	0.049***	0.004
Urbanization	0.020	0.028	0.104	0.094
Agriculture	–0.067	0.145	1.338**	0.598
Fishing	0.306*	0.181	1.673**	0.704
Extraction	0.161	0.235	0.998	0.647
Manufacturing	–0.137	0.153	1.142*	0.669
Utilities	–0.515	0.446	2.375	1.817
Construction	–0.671***	0.175	0.967	0.691
Retail trade	0.415***	0.132	1.350**	0.576
Housing	0.139	0.233	1.462***	0.554
Transportation	–0.581***	0.190	–0.292	0.668
Financial services	0.734	0.897	3.406	2.085
Real-estate services	0.464	0.318	2.336	1.708
Public administration	–0.557***	0.205	1.734*	0.943
Education	0.206	0.193	2.369***	0.784
Health services	–0.481	0.345	1.520	1.872
Other public services	0.890**	0.393	2.777	1.760
Constant	0.600***	0.142	–0.268	0.619
Sample size	3,482,077		2,574,077	

Note: The dependent variable is the categorical variable *Informal*, which equals 1 if the worker is employed informally and zero if employed formally. Levels of statistical significance of the estimated coefficients are indicated by asterisks: 10 % (*), 5% (**), and 1% (***).

Table 5. Decomposition of change in mean informality rates, 2000–2010.

	Weight 1				*Weight 2*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$(\beta_1–\beta_0) \times X_1$	$(X_1– X_0) \times \beta_0$	Change in β	Change in X	$(\beta_1–\beta_0) \times X_0$	$(X_1– X_0) \times \beta_1$	Change in β	Change in X
CCT coverage	–0.279	0.360	–525.1%	677.1%	–0.005	0.086	–9.8%	161.7%
Labor enforcement	0.216	–0.007	405.8%	–13.0%	0.211	–0.002	396.5%	–3.7%
Min wage bindingness	0.000	0.004	0.1%	7.5%	0.000	0.004	0.8%	6.7%
Age	0.329	–0.033	618.9%	–62.4%	0.319	–0.022	598.6%	–42.1%
Age squared	–0.144	0.032	–270.7%	59.3%	–0.134	0.022	–252.7%	41.4%
Secondary educ [d]	0.006	–0.008	10.8%	–14.4%	0.005	–0.007	9.0%	–12.6%
College educ [d]	0.002	–0.004	3.1%	–7.0%	0.001	–0.003	2.4%	–6.3%
Female [d]	0.009	0.000	16.5%	–0.1%	0.008	0.001	15.3%	1.1%
Female with child [d]	–0.001	0.000	–1.8%	–0.4%	–0.001	0.000	–1.9%	–0.2%
Formal-sector spouse [d]	–0.002	–0.003	–4.1%	–6.0%	–0.002	–0.004	–3.3%	–6.9%
Race - black [d]	–0.001	0.000	–2.0%	0.9%	–0.001	0.000	–1.6%	0.5%
Race - mixed [d]	–0.009	0.003	–17.3%	5.8%	–0.008	0.002	–14.4%	3.0%
Race - Asian [d]	0.000	0.000	–0.2%	0.4%	0.000	0.000	–0.1%	0.3%
Race - indigenous [d]	0.000	0.000	–0.1%	–0.1%	0.000	0.000	–0.1%	–0.1%
Disabled [d]	–0.001	0.001	–1.0%	1.6%	0.000	0.001	–0.5%	1.1%
Urbanization	–0.073	–0.002	–137.4%	–3.2%	–0.074	0.000	–139.9%	–0.6%
Agriculture	–0.096	0.014	–179.6%	26.4%	–0.081	–0.001	–151.9%	–1.3%
Fishing	–0.005	0.002	–8.8%	3.2%	–0.003	0.000	–6.2%	0.6%
Extraction	–0.005	0.003	–9.7%	4.9%	–0.003	0.000	–5.5%	0.8%
Manufacturing	–0.211	0.003	–396.0%	5.7%	–0.207	0.000	–389.6%	–0.7%
Utilities	–0.031	0.012	–59.1%	21.6%	–0.017	–0.002	–32.8%	–4.7%
Construction	–0.155	0.012	–291.0%	23.3%	–0.134	–0.009	–251.5%	–16.2%
Retail trade	–0.197	0.028	–370.4%	52.1%	–0.178	0.009	–334.3%	16.0%
Housing	–0.054	–0.016	–101.8%	–30.7%	–0.069	–0.002	–129.6%	–2.9%
Transportation	–0.015	0.003	–28.2%	4.8%	–0.017	0.005	–32.9%	9.5%
Financial services	–0.037	–0.007	–70.3%	–13.2%	–0.043	–0.002	–80.6%	–2.8%
Real-estate serv.	–0.159	0.037	–299.7%	69.9%	–0.130	0.007	–243.7%	13.9%
Public admin.	–0.084	–0.047	–158.2%	–87.7%	–0.146	0.015	–274.1%	28.2%
Education	–0.103	–0.045	–193.1%	–85.1%	–0.144	–0.004	–270.8%	–7.4%
Health services	–0.077	–0.003	–144.1%	–5.0%	–0.080	0.001	–150.7%	1.6%
Other publ. serv.	–0.079	–0.004	–147.6%	–6.7%	–0.081	–0.001	–152.2%	–2.1%
Constant	0.869	0.000	1632.5%	0.0%	0.869	0.000	1632.5%	0.0%
Sum	**–0.388**	**0.335**	**–729.5%**	**629.5%**	**–0.147**	**0.093**	**–275.6%**	**175.6%**

Note: "Weights 1" refers to a decomposition made according to expression (3'): $\overline{\iota s}_1 - \overline{\iota s}_0 = [(\bar{\boldsymbol{X}}_1 - \bar{\boldsymbol{X}}_0)\hat{\boldsymbol{\beta}}_0 + (\bar{\boldsymbol{Z}}_1 - \bar{\boldsymbol{Z}}_0)\hat{\boldsymbol{\gamma}}_0] + [(\hat{\boldsymbol{\beta}}_1 - \hat{\boldsymbol{\beta}}_0)\bar{\boldsymbol{X}}_1 + (\hat{\boldsymbol{\gamma}}_1 - \hat{\boldsymbol{\gamma}}_0)\bar{\boldsymbol{Z}}_1]$. "Weights 2" refers to a decomposition made according to the following expression: $\overline{\iota s}_1 - \overline{\iota s}_0 = [(\bar{\boldsymbol{X}}_1 - \bar{\boldsymbol{X}}_0)\hat{\boldsymbol{\beta}}_1 + (\bar{\boldsymbol{Z}}_1 - \bar{\boldsymbol{Z}}_0)\hat{\boldsymbol{\gamma}}_1] + [(\hat{\boldsymbol{\beta}}_1 - \hat{\boldsymbol{\beta}}_0)\bar{\boldsymbol{X}}_0 + (\hat{\boldsymbol{\gamma}}_1 - \hat{\boldsymbol{\gamma}}_0)\bar{\boldsymbol{Z}}_0]$. For brevity, the first term of the expression is referred above to as "$(\beta_1–\beta_0) \times X$" and the second term as "$(X_1– X_0) \times \beta$".

Note, in comparing columns 1 and 2 of the table, that the quantitative magnitude of the minimum wage effect declines over time, suggesting a declining impact on informality, but the two effects are only slightly different in magnitude and, from a statistical perspective, not significantly different from one another. A comparison of means at the start and end of the decade (Table 3) suggests that the relative commitment to the norm in the informal sector declines rather significantly, thereby portending a growth in informality over the period. The influence of the minimum wage in wage-setting is purely normative in the informal sector, and as the minimum wage rose significantly over the decade, it is perhaps not surprising that its relative impact on the wage structure would diminish in the informal sector.

The changing estimated coefficients on the minimum wage variable suggest an increase in informality, as do the changing means. Columns 3 and 4 of Table 5 give the two impacts separately as a percentage of the absolute mean change in informality (0.053) for the first set of weights and columns 7 and 8, for the second set of weights. This offers an alternative measure of the quantitative impact. Here, we see that neither impact – changing coefficients or changing means – exceeds more than 10% of the overall change in mean informality, with the combined percentage impact being 7.6%.[11] The interpretation, then, is that changing minimum wages over this period would, all else constant, have portended a rise in informality of 0.4 percentage points (7.6% of 5.32).

Turning to the role of government conditional cash transfer payments, we hypothesize that their existence may encourage formal sector workers to move to informal-sector status in order to hide income and thereby qualify for government transfers. The results in Table 4 reveal that increased conditional cash transfers do indeed increase informality, as hypothesized. The estimated impact of a change in transfer payments on informality declines over the period (although the larger coefficient in 2000 is much less precisely estimated), indicating a possible decline in informality for any given amount of such payments. However, the very dramatic increase in transfers per capita over this period (by a magnitude of over 50) portends a significant rise in informality. The decomposition analysis in Table 5 reveals that the increased coverage and magnitude of transfer payments over the period swamps the changing coefficients effect, which combined predicts a rise in informality of roughly 8 percentage points, or roughly 152% (677%–525%) of (the absolute value of) the mean change in informality over the period.

Regarding labor law enforcement and its impact on informality, we find, as hypothesized, that increased enforcement lowers informal employment. However, comparing the effects over time, while enforcement efforts appear to have slightly increased over the period (Table 3), the effectiveness of these enforcement efforts (as judged by the changing coefficients in Table 4), at least so far as they concern rooting out and reducing informal employment, declined rather dramatically. Combining the magnitude of the change over the period in estimated effects with the rather paltry increase in enforcement efforts, the sum suggests an increase in informality of nearly 20 percentage points, or roughly 400% of the mean change in informality over the period. This is huge, but recall that we are worried about the integrity of the estimated coefficient in 2000 due to mismeasurement of the drive time variable, an issue we return to in the section below on robustness checks.

[11] Note that this is true for both columns 3 + 4 as well as columns 7 + 8. The combined impacts – whether expressed as the direct contribution to the change in mean informality or as a percentage of the mean change in informality – are, by construction, necessarily the same regardless of the weights used.

Changing demographics also account for the observed decline in informal employment over this period. Two demographic features with strong negative estimated impacts on informality are years of schooling and whether or not an individual possesses a spouse working in the formal sector. The demographics also change over the period in rather dramatic ways – the percentage of the population having completed secondary school rises by 7 percentage points, the percentage having completed college by 3 percentage points, and the percentage with a formal-sector spouse by 5 percentage points.

It is widely known that schooling is one way of increasing the chances of attaining a formal sector wage and salary position. Brazil, like many Latin American countries over this period, set in motion policies to expand formal education, including increased direct government expenditures on education but also conditional cash transfers to encourage parents to keep their children enrolled in school. The regression results in Table 4 suggest that having a secondary degree decreases the probability of being in the informal sector by roughly 10 percentage points, and having a college degree by roughly a similar amount, with minor changes in estimated impact over time. Focusing just on the growth in educational attainment over this period, the enhanced numbers of workers with secondary and college degrees combined accounts for roughly 20% of the decline in informality. Factoring in the impact of changes in estimated coefficients, increased educational attainment accounts for 7.5% of the decline in informality, or 0.4 percentage points.

Having a spouse in the formal sector decreases the probability of informal sector employment by between 6% and 7% of the mean decline in informality, depending on which weights are used (Table 5), and because the changing estimated impacts also portend a decrease in informality, the aggregate impact amounts to roughly 10% of the decline, or roughly 0.5 percentage points. We suspect the explanation for this result is grounded in job search theory. Spouses who work in the formal sector may earn higher wages, which would allow for enhanced time spent in search for a non-working spouse, but they are also able to offer positive referrals for their spouses to employers looking to hire. Formal sector employers are keen to hire productive employees, and especially so in societies like Brazil where it is costly to fire a worker. The job search literature is clear that internal referrals are an inexpensive and efficient way for employers to screen for quality in a job search. The observed increase in the percentage of individuals reporting having formal sector spouses is probably largely a demand-side story – the growing availability of formal sector positions goes to spouses of existing formal sector workers. But, there may be a causal component to this observed relationship as well: to the extent formal sector workers increasingly possess spouses, and especially ones that desire employment in the formal sector, this reduces job search costs for employers and may nudge some firms to opt for formality as a result.

The case of the variable *Female* is interesting. There was an increase in female representation among the wage and salary workforce over the decade – from 35% to 37% – but also a rather dramatic change in how being female translates into informality. In 2000, the estimated impact of being female on informal sector status was statistically insignificantly different from zero, whereas in 2010 it had become positive and statistically significant. Many of the estimated coefficients on the demographic variables change over time, and some statistically significantly so, but none change positively and with such magnitude as this estimated coefficient. In the aggregate, the impact of this variable in the decomposition analysis portends a rise in informality, all else constant, of over 16%, or 0.85 percentage points.

One way of unpacking this finding is to introduce some interactive terms into the specification. Could this result, for example, have something to do with the expansion of transfer programs over the period? Could women, especially, be shifting from formal to informal sector status in order to qualify for the *Bolsa Familia* conditional cash transfer program, thereby rendering the relationship between female and informality positive when before, in 2000 when conditional cash transfers were largely absent, it was insignificantly different from zero? When we interact *Female* and *CCT coverage* in the 2010 specification, the coefficient for female becomes insignificantly different from zero, whereas the interaction term is positive and statistically significant. Hence, all of the positive estimated impact of gender on informality in 2010 is accounted for by women receiving transfer payments.

Turning, finally, to changing industry composition effects, we see that it is the changing way in which industry translates into informality (i.e., changes in the estimated coefficients) that accounts for the largest portion of the decline in informality over this time period. Between 2000 and 2010, six of the estimated coefficients on the industry composition variables switch signs from positive to negative, and all of the remainder that were positive in 2000 fall in absolute value. The one estimated coefficient that is negative in 2000 becomes more so in 2010. In every industry, the propensity toward informal employment falls and does so sizeably and often statistically significantly. Collectively, the changing employment mix itself accounts for roughly 17% (0.9 percentage points) of the decline in informality. However, it is the changing way in which this mix translates into informality that carries the day. If the employment mix had remained unchanged over the period, the changing estimated coefficients would nonetheless portend a decline in informality that accounts for well over 100% of the actual decline.

By way of summary, our empirical analysis sheds only a dim light on the causal determinants of declining informality over the decade 2000-2010. In the aggregate, and focusing only on changes in composition effects – the "explained" components in the decomposition analysis – rising rates of labor law enforcement, rising education levels, increased numbers of workers with spouses in the formal sector, and changes in industry composition explain between 57% or roughly 3 percentage points (weight 1) and 16% or roughly 0.85 percentage points (weight 2) of the decline in informality over the period. However, if all the other changes in composition effects are factored in, informality rates are predicted to rise, not fall, and far and away the biggest contributor in this predicted increase in informality is the increased coverage of conditional cash transfer payments, which alone portend a rise in informality of between 36 percentage points (weight 1) and 9 percentage points (weight 2)!

5. Robustness Checks and Extensions

Several robustness checks or extension exercises are presented in this section. The first accounts for inconsistencies in drive time differences across the period of examination. The second accounts for instances in which municipalities changed fundamental character over the period due, for example, to mergers or separations between communities. The third explores our primary results with controls for municipal fixed effects.

5.1. Drive Time Inconsistencies

There are three reasons why drive times may differ across the two time periods under examination in this paper: (1) systematic difference in the programs used to calculate drive times in the two periods; (2) the fact that in 2000 Almeida and Carniero calculate drive time between any given municipality and the nearest labor ministry office (instead of the actual labor ministry office in charge of inspections in that municipality); and (3) legitimate changes in drive time due, for example, to the construction of new roads, altered speed limits, increased congestion, and the closure or opening of labor ministry offices. We would like our estimates of the impact of enforcement on informal employment to be identified off of legitimate changes in drive times over the period.

We have strong suspicions that the programs used to derive drive times are different over the two periods. When drive time in 2010 is regressed on a constant and drive time in 2000, while the correlation is very high – indeed the estimated coefficient on drive time in 2000 is virtually equal to 1 – there is a constant of 0.3, or roughly one-third of an hour, which amounts to about 15% of the average drive time of two hours in 2000. One possible explanation for an average increase in drive times is the error cited in (2) above – we would expect drive times to be lower in 2000 because they are calculated for the nearest labor ministry office rather than the one truly responsible for inspections in a particular municipality. But, when we run the same regression on a sub-sample of municipalities for which the nearest labor ministry office is indeed the one responsible for labor inspections (to be discussed further below), we find similar results: an estimated coefficient equal to 1 and a constant that, in this case, is over two-thirds of an hour. We would not expect average drive times to rise so significantly over time, and therefore conclude that the programs used to calculate drive times differ across the two time periods. It is important to note, however, that so far as this type of programming inconsistency results in a linear transformation of true drive time (as suggested by the regression results above), it can be shown that our structural estimates of the impact of enforcement on informality are unaffected. We thus leave aside this issue.

In order to shed light on the extent of the error committed by Almeida and Carneiro, and its effect on the estimated impact of enforcement on informality, we gathered drive times to the *nearest* labor ministry office for each municipality in those nine states with only two labor ministry offices in 2010. Coupled with drive time data to the *accurate* office, we are able to identify the subset of municipalities in these states for which the nearest labor ministry office is indeed the accurate one responsible for labor conditions and inspections. Of the 1330 municipalities in the 9 states with only two labor ministry offices, there are 216 instances (or roughly 16% of the sample) for which the nearest office is *not* the one responsible for inspections. It is difficult to know to what extent this translates to the larger group of states with more than two labor ministry offices, but it gives us a sense of the possible extent of the error committed by Almeida and Carneiro.

To this sample, we add municipalities in the five states with only one labor ministry office, in which case the nearest office is, by necessity, the accurate office. This adds over 100 municipalities (including Brasilia) to the sample. With these data we can hazard an answer to the question, "How, if at all, would our main results change if drive times in 2000 reflected distances to the accurate labor ministry office rather than to the nearest?" We should begin by noting that these samples are roughly 15% the size of samples for the main results (Table 4). Compared to the main results, the estimated coefficient on the enforcement variable is –0.067 in 2010 and –0.55 in 2000. While the estimated coefficients in 2000 are reasonably similar (–0.55 versus –0.58), the coefficient in 2010 with the new, smaller sample is less than half the size of that in the main results (–0.067 versus –0.17). Thus, this is further evidence to suggest that the effectiveness of enforcement efforts in reducing informality declined rather significantly over the period 2000 to 2010.

A different approach to detecting possible contamination of our main results due to differences in the measurement of drive time is to trim the main sample to eliminate outliers that are likely to be the result of various measurement inconsistencies. We eliminate any changes in drive times over the period representing more than 20% (in absolute value) of the average drive time (of two hours) in 2000. The sample sizes fall significantly (but by nowhere as much as in the exercise above) – by roughly 40% for the 2010 sample and by roughly 30% for the 2000 sample. The estimated coefficient on enforcement in 2010 for this sub-sample is less, by almost half, than the one in Table 4 (–0.09 versus –0.17). For 2000, the estimated coefficients for the sub-sample and main set results are virtually identical (–0.58 versus –0.6). The efficiency of labor law enforcement in reducing informal employment falls over the period in these results as well. Thus, we conclude that, while the precision of the estimates of the decomposition may be compromised, it appears to be the case that increased labor law enforcement over the period had a reduced impact on lessening informality during these years.

5.2. The Changing Character of Municipalities over the Period

The number of municipalities in Brazil grew from 5507 in year 2000 to 5565 in 2010. These new municipalities emerged either as separations from single existing municipalities or as mergers of parts of two, or in some cases even three, existing municipalities. In total, 130 of the municipalities existing in 2010 were "affected" by municipal re-organizations between 2000 and 2010, either as being newly-created or as an existing municipality losing part of its original land. A concern here is that this re-organization of municipalities might have changed fundamentally the character of some of the original municipalities between the two periods. This, in turn, could mislead an analysis explaining the change in informal employment in terms of changing observable municipal characteristics. As a robustness check we excluded these 130 municipalities affected by re-organizations in our regression analysis. This reduced the sample by only about 2.5%. Coefficient estimates did not change notably.

5.3. Municipal Fixed Effects

Could the estimated coefficients above, and especially those for the municipal-level variables, be biased due to unobserved time-invariant attributes of municipalities? To control for municipal fixed effects, we begin by noting that an equivalent method of obtaining the two sets of regression results for the 2000 and 2010 samples is to run a single time-interacted regression on the pooled samples from both years. In the time-interacted model, all variables (plus the constant) are interacted with a dummy variable D_i, which equals 1 if the observation belongs to the 2010 sample and equals 0 if it belongs to the 2000 sample:

$$prob(is_{i,t}) = X_{i,t}\beta_0 + (D_{i,t} \cdot X_{i,t})\beta_1 + u_{i,t} \qquad (4)$$

In this fully time-interacted model, the vector of coefficients β_0 equals the vector of coefficients β_{2000} and to obtain the coefficients β_{2010} one adds the β_1 coefficient to the β_0 coefficient for each variable.

The ultimate reason for specifying a fully time-interacted model is to be able to control for unobservable municipal fixed effects. Unobserved municipal characteristics that are fixed over time may affect informality and also be correlated with our key explanatory variables. This seems especially possible regarding the variables captured at the municipal level, such as '*light house effects,' enforcement*, and *cash transfers per capita*. To address this problem, we add municipal dummy variables $M_{i,m}$, which equal 1 if individual i is a resident of municipality m and equals 0 otherwise, to the time-interacted regression equation. We estimate a two-stage least square model and correct for clustering in the estimation of standard errors just as was done in the previous analysis.

The estimated coefficients appear in Appendix Table A1. (Note: The estimated coefficient for 2010 is, in each case, the sum of the estimated coefficient for 2000 and the estimated coefficient on its interaction with 2010.) The estimated coefficients on the individual-level variables are resoundingly robust – with regard to both statistical significance and quantitative impact. Moreover, we find a very similar pattern in the fixed effects analysis as was found in the original results regarding industrial composition – namely, within virtually every industry the tendency toward informality declines significantly over the course of the decade. The results for our three institutional variables – all measured at the municipal level – are more mixed.

The signs are all in accordance with the earlier, non-fixed effects results: conditional cash transfers and minimum wages tend to increase informality, whereas labor law enforcement decreases it. However, the statistical significance of the fixed effects results is compromised in these findings; while both of the estimated impacts of conditional cash transfers on informality are statistically significant, the minimum wage impact is only significant in 2010, and neither of the enforcement impacts is statistically significantly different from zero. In terms of quantitative impacts, while all but one (i.e., the conditional cash transfer estimates in 2010) of the estimated fixed effects coefficients fall within three standard errors of the non-fixed effects estimated coefficients, this concordance is due in part to the general lack of precision in the fixed effects estimates themselves. Regarding the decomposition effects, the percentage accounted for by changes in both estimated coefficients and means is altered quite substantially in the fixed effects analysis. Using the first set of weights, for example, the combined percentage of the overall mean change accounted for by conditional cash transfers is 25.7% (versus 152% in the non-fixed effects analysis), by labor law enforcement is 1.8% (versus almost 400%), and by minimum wages is –0.26% (versus 7.6%). While these results do not threaten the general validity of the earlier findings, they do offer cause for some concern about their overall integrity.

6. Conclusion

This paper explores the significant decline in wage and salary informal employment over the period 2000–2010 in Brazil. We utilize census data from the beginning and ending years of the decade along with other institutional data sources, informality regressions that exploit variation in informality across workers and municipalities for these two years, and then decompose the changing mean informality rate over the decade into its determinants using a Blinder-Oaxaca decomposition. Among the determinants considered are: enhanced labor law enforcement, a near doubling of the real value of the minimum wage, the emergence and growth of conditional cash transfer programs (and most importantly *Bolsa Família* – the largest conditional cash transfer program in the world), and changing industry composition and labor force demographics.

We find that two of the most important policy changes over this period – the increase in the real value of the minimum wage and the dramatic expansion of conditional cash transfer programs – contribute positively, not negatively to informality. Among the various determinants of informality analyzed in this paper, four – namely, rising education levels, increased incidence of workers having a spouse in the formal sector, increased labor law enforcement, and the changing mix of industries – account, collectively, for between 16% and 57% of the decline in the mean informality rate over the period. The single largest factor explaining the decline in informality in our results are changes in the set of estimated coefficients on the industry categorical variables – that is, by the changing ways in which industry translates into informality.

References

Almeida, Rita and Pedro Carneiro. 2009. Enforcement of Labor Regulation and Firm Size. *Journal of Comparative Economics 37*, no. 1:28-46.

____. 2012. Enforcement of Labor Regulation and Informality. *American Economic Journal: Applied Economics 4*, no. 3:64–89.

Amadeo, Edward Joaquim; Indermit S. Gill and Marcelo Cortes Neri. 2000. Brazil: The Pressure Points in Labor Legislation. Economics Working Papers 395 Graduate School of Economics, Getulio Vargas Foundation (Brazil).

Berg, Janine. 2010. Laws or Luck? Understanding Rising Formality in Brazil in the 2000s. Working paper 5, ILO Office in Brazil, International Labor Organizaion.

Cardoso, Eliana and André Portela Souza. 2003. The Impact of Cash Transfers on Child Labor and School Attendance in Brazil. Department of Economics Working Paper 0407, Vanderbilt University.

Cardoso, Adalberto, and Telma Lage. 2007. As Normas e os Factos. Editora FGV, Rio de Janeiro, Brasil.

Carneiro, Francisco. 2004. Are Minimum Wages to Blame for Informality in the Labour Market? *Empirica 31*, no. 4:295–306.

Catão, Luis A. V.; Carmen Pagés and Maria Fernanda Rosales. 2009. Financial Dependence, Formal Credit, and Informal Jobs: New Evidence from Brazilian Household Data. IZA Discussion Papers, 4609 Institute for the Study of Labor (IZA).

Corseuil, Carlos Henrique L. and Miguel Nathan Foguel. 2012. Economic Expansion and Increase in Labour Market Formality: A Poaching Approach. *Revista Brasileira de Economia*, 66, 207–24.

Costa, Ana Carla A. and Joao M.P. De Mello. 2008. Judicial Risk and Credit Market Performance: Micro Evidence from Brazilian Payroll Loans, in S. Edwards and M. Garcia (eds.), *Financial Markets Volatility and Performance in Emerging Markets*, pages 155–184, National Bureau of Economic Research.

de Brauw, Alan, Daniel O. Gilligan, John Hoddinott, and Shalini Roy. 2014. *Bolsa Família* and Household Labor Supply. Forthcoming, Economic Development and Cultural Change.

Fairris, David; Gurleen Popli and Eduardo Zepeda. 2008. Minimum Wages and the Wage Structure in Mexico. *Review of Social Economy 66*, no. 2:181–208.

Fajnzylber, Pablo. 2001. Minimum Wage Effects Throughout the Wage Distribution: Evidence from Brazil's Formal and Informal Sectors. *Anais do XXIX Encontro Nacional de Economia 98*, ANPEC – Associação Nacional dos Centros de Pósgraduação em Economia.

Glewwe, Paul and Ana Lucia Kassouf. 2008 The Impact of the Bolsa Escola/Familia Conditional Cash Transfer Program on Enrollment, Drop out Rates and Grade Promotion in Brazil. Working Papers 08_16, Universidade de São Paulo, Faculdade de Economia, Administração e Contabilidade de Ribeirão Preto.

Hall, Anthony. 2008. Brazil's Bolsa Família: A Double-Edged Sword? *Development and Change 39*, no. 5:799–822.

Henley, A., Arabsheibani, R., and Carneiro, F. 2009. On Defining and Measuring the Informal Sector: Evidence from Brazil. *World Development 37,* no 5: 992–1003.

Kumar, Anjali and Manuela Francisco. 2005. *Enterprise Size, Financing Patterns, and Credit Constraints in Brazil: Analysis of Data from the Investment Climate Assessment Survey*. World Bank Publications.

Lemos, Sara. 2009. Minimum Wage Effects in a Developing Country. *Labour Economics 16*, no. 2:224–37.

Maloney, William F. 2004. Informality Revisited. *World Development 32*, no. 7:1159–78.

Maloney, William F. and Jairo Nuñez Mendez. 2004. Measuring the Impact of Minimum Wages: Evidence from Latin America, in J. Heckman and C. Pagés (eds.), *Law and Employment: Lessons from Latin America and the Caribbean.* The University of Chicago Press.

Mello, R.F. and Santos, D.D. 2011. "Aceleração educacional e a queda recente da informalidade," Proceedings of the 37th Brazilian Economics Meetings.

Neri, M., G. Gonzaga, and J. M. Camargo. 2001. Salário mínimo, "efeito-farol" e pobreza. *Revista de Economia Política*, *21*, no 2: 78–90.

Paz, Lourenco. 2012. The Impacts of Trade Liberalization on Informal Labor Markets: An Evaluation of the Brazilian Case. MPRA Paper 38858, University Library of Munich, Germany.

Perry, Guillermo E.; William F. Maloney; Omar S. Arias; Pablo Fajnzylber; Andrew D. Mason and Jaime Saavedra-Chanduvi. 2007. *Informality: Exit and Exclusion*. World Bank Publications.

Pires, Roberto. 2008. Compatibilizando Direitos Sociais Com Competividade: Fiscais Do Trabalho E a Implementação Da Legislação Trabalhista No Brasil. Discussion Papers 1354, Instituto de Pesquisa Econômica Aplicada - IPEA.

Rajan, Raghuram G and Luigi Zingales. 1998. Financial Dependence and Growth. *American Economic Review 88*, no. 3:559–86.

Soares, Fábio Veras, Rafael Perez Ribas, and Rafael Guerreiro Osório. 2010. Evaluating the Impact of Brazil's Bolsa Família: Cash Transfer Programs in Comparative Perspective. *Latin American Research Review 45*, no. 2: 173–190.

Soares, Sergei. 2012. Bolsa Familia, Its Design, Its Impacts and Possibilities for the Future. Working Paper No. 89. International Policy Center for Inclusive Growth. United Nations Development Programme.

Staiger, Douglas, and James H. Stock. 1997. Instrumental Variables Regression with Weak Instruments. *Econometrica* 65, no. 3: 557–586.

Straub, Stéphane. 2005. Informal Sector: The Credit Market Channel. *Journal of Development Economics 78*, no. 2:299–321.

Tornarolli, Leopoldo, Diego Battistón, Leonardo Gasparini, and Pablo Gluzmann. 2014. Exploring Trends in Labor Informality in Latin America, 1990–2010, Working Paper No. 159. CEDLAS, Universidad Nacional de La Plata, Argentina.

Ulyssea, Gabriel. 2010. Regulation of Entry, Labor Market Institutions and the Informal Sector. *Journal of Development Economics 91*, no. 1:87–99.

Data Appendix

a) Informal employment

We define workers as part of the formal sector if they possess a signed labor card (*carteira de trabalho assinada*) and as informal otherwise. As discussed by Henley et al. (2009), there are alternative ways of defining informal employment in Brazil, based on, for example, employer size or social security contributions. Using the PNAD survey from 2004, they show that the correlation between the labor card definition and the social-security definition is 0.85, suggesting a large overlap between the two definitions. The correlation between the labor card definition and the definition based on employer size (workers in firms of less than six employees and self-employed) is 0.65. The Demographic Census provides no information on employer size, which prevents us from using a firm-size based measure of informality. Such measures, however, are sensitive to substantial miss-classification, since many small employers may hire all their employees on an entirely formal basis, and some large employers employ a portion of their workforces on an informal basis. As for social-security contributions, the Census data only provide this information for the self-employed and for employers. Irrespective of data availability, we prefer the labor-card definition since a signed labor card is mandated by law for all employees and since this is what entitles workers to several social benefits.

b) Conditional cash transfers

Federal conditional cash transfer (CCT) programs grew dramatically during the first decade of the 21st century in Brazil. The first CCT program in Brazil emerged in 1991, and throughout the 1990s CCTs spread rapidly, but largely at the municipal level.[12] The Federal government initiated its first conditional cash transfer program in 1996 which was targeted at reducing child labor in especially dangerous industries. Municipal-level programs spread rapidly in the later years of the decade, and in 1998 the Federal government began subsidizing the transfers in a host of these municipal-level CCTs. The first nationwide CCT program targeted to increase children's education and health status emerged in Brazil in 2001. It was referred to as Bolsa Escola, and was a precursor to the *Bolsa Família* CCT program which began in 2003. The growth in both benefit levels and reach during the remainder of the decade was dramatic.

The Census data contain information on households receiving conditional cash transfer payments. In order to measure the municipal-level "take-up rate," our primary task is to capture the eligible population. We assume that reported income in the Census is a more accurate reflection of true income than what is reported to local authorities in order to qualify for *Bolsa Família*. Thus, some households who are not eligible for CCTs according to Census data may nonetheless report receiving such payments (see et al. 2010, for a discussion of targeting issues with *Bolsa Família* and for estimates of the high percentage of recipients who are in fact ineligible for the program). Despite the fact that some recipients are ineligible, our measured take-up rate should still reflect both the extent to which the program is well-known and the efficiency of processing by local authorities.

In 2010, we begin by eliminating households from our sample that contain individuals who are not family members, since eligibility involves family (not household) income per capita. There is great agreement in

[12] Soares (2012) contains an excellent history of CCT programs in Brazil.

the literature that "household" and "family" are fairly synonymous in Brazil, and indeed our analysis of these exclusion restrictions suggest this is the case; less than 0.5% of households contain members who are not related to the household head. This includes households with domestic servants, relatives of domestic servants, boarders, and individuals living in collective domiciles. We then turn to the derivation of household (i.e., family) income per capita. Pension benefits are excluded from the calculation of household earned income, and pensioners are not counted in the "per capita" number for purposes of eligibility. Earned income – including earnings from employment as well as rental income, income from investments, and interest income – is the primary category here, excluding direct and conditional cash transfer payments. This is captured as monthly income in the month of July of the survey year, and household income is the aggregation of the monthly income of family members. Household income is then divided by the number of family members.

Eligibility criteria for Bolsa Familia are clearly stated: for the year 2010, very poor families (with a per capita household income of 70 Reas or less) are eligible, as are poor households (with per capita household income greater than 70 but less than or equal to 140) so long as they have a child present in the household who is 17 years of age or less. For each municipality, we calculate, using Census data, the take-up rate among the eligible households – that is, the number of eligible families receiving Bolsa Familia benefits divided by the number of eligible families in the municipality. We employ this variable to capture knowledge of the program by municipal residents and the efficiency of local administrative authorities in submitting applications (and also perhaps the lack of scrutiny of these authorities in pursuing those who do not meet the "conditions" involving school attendance and health exams of children). We believe this variable to be truly exogenous in the informality equation.

For the year 2000, the information in the Demographic Census data is less precise concerning conditional cash transfers received by households. Individuals are asked only for the total monthly amount of social transfers received, regardless of transfer program or type of transfer. In deriving the municipal take-up rate in 2000 we begin by excluding households with individuals who are disabled or unemployed. By doing so, we avoid the inclusion of disability and unemployment benefits in our measure of cash transfers received. We then sum all forms of income (labor income, rental income, income from alimony etc.) – except cash transfers – in the household and divide by the number of household members.[13] If this per-capita household income is less than R$90 *and* if there are children 7–15 years old in the household, we consider the household eligible for CCTs. The municipal take-up rate is then defined as the number of eligible households receiving cash transfers divided by the total number of eligible households in the municipality, in the same way as for 2010.

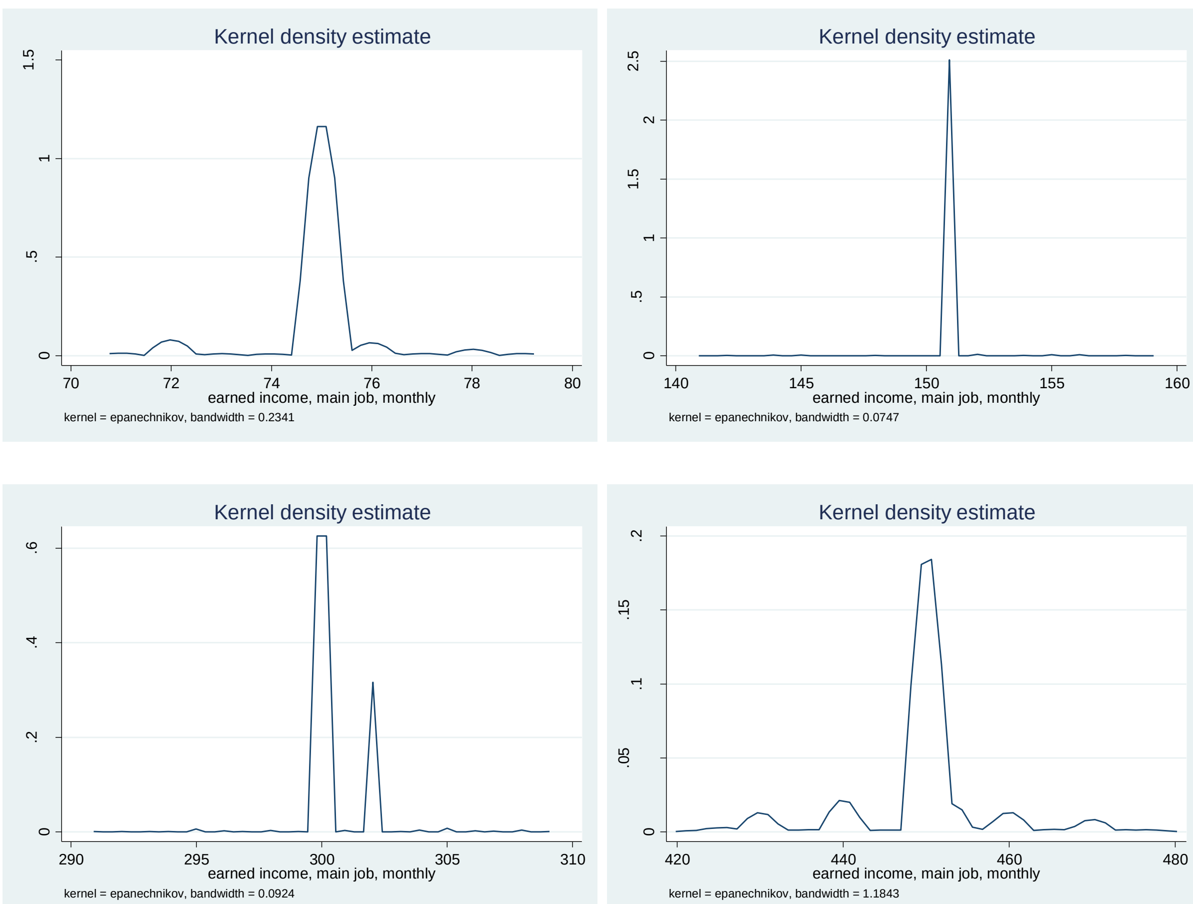
c) Minimum wage effects

Regarding the *minimum wage bindingness* measure, Figures A1 and A2 in the appendix show selected segments of the wage distribution for fulltime workers in the informal sector for 2000 and 2010, respectively. In year 2000 the minimum wage was 151 real. Spikes can be observed, in Figure A1, at half the minimum wage (75 real) and at the minimum wage. But, there are also spikes in the distribution at exactly twice the minimum wage, 302 real. In 2010, the minimum wage was 510 real. Figure A2 reveals a certain spike at 255 real – exactly one-half the wage minimum – despite a well-known tendency for surveyed workers to round off reported salary measures – in this case, perhaps to 250 real. The spikes

[13] We follow Cardoso and Souza (2003) in isolating conditional cash transfer recipients using the 2000 Demographic Census.

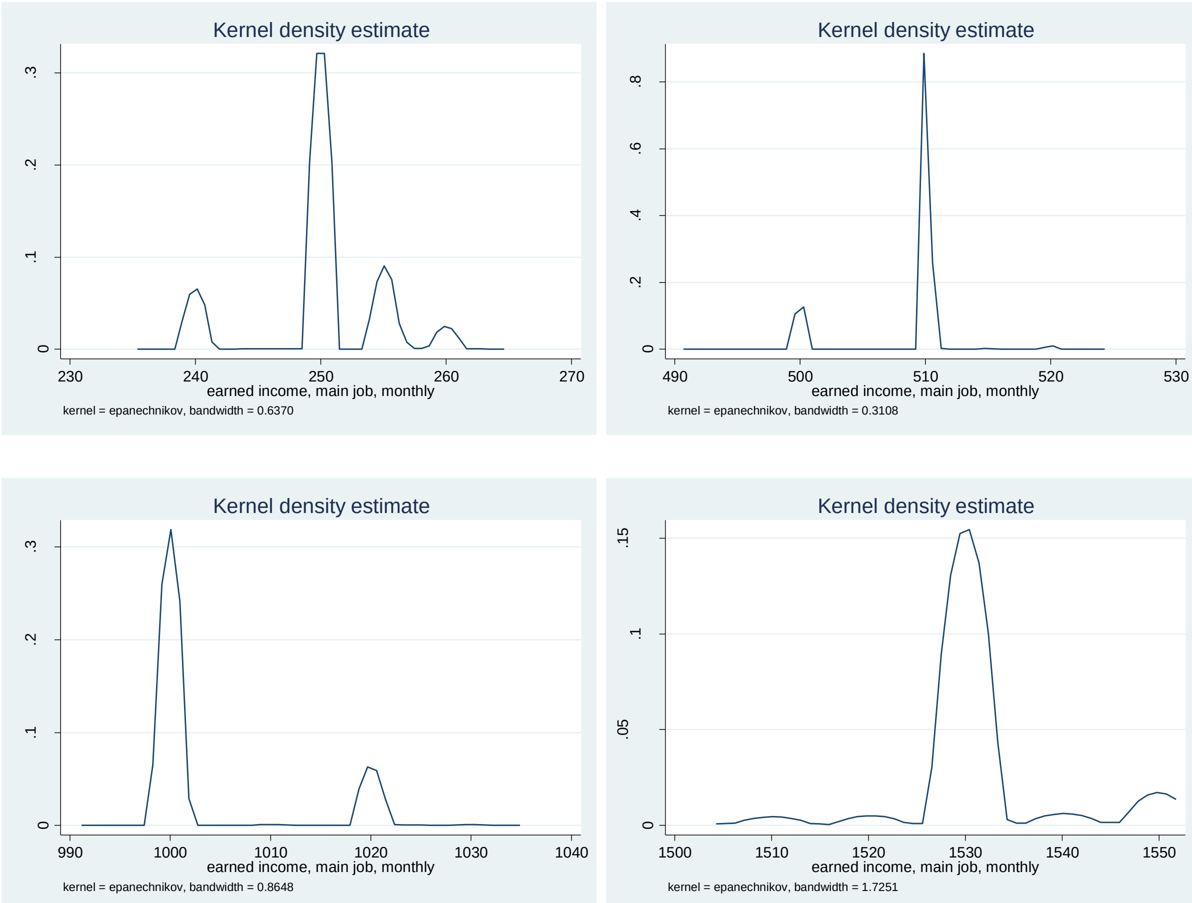
appear to be real phenomena; they take place at the exact minimum wage and at multiples thereof. This provides evidence, at the national level, of “lighthouse” and “numeraire” effects of the minimum wage in Brazil, as has been observed in the previous literature. Figure A3 and A4 show the corresponding wage distributions in the formal sector, with similar spikes at multiples of the minimum wage, both for year 2000 and 2010. The strength of the minimum wage as a wage-setting norm in the informal sector does seem to vary across municipalities, both in absolute terms and in relative terms (compared to the formal sector). On average, 23 percent of the informal work force was paid in multiples of the minimum wage in 2010. This share, however, varies from a few percent in some municipalities to over 60 percent in other municipalities. The *difference* between the informal and formal sectors in the strength of this norm also varies across municipalities, which is key to our analysis. In some municipalities the share of workers paid multiples of the minimum wage in the *informal* sector is much lower than the corresponding share in the formal sector. In other municipalities the reverse is true.

Figure A1. Wage distribution in the informal sector, 2000



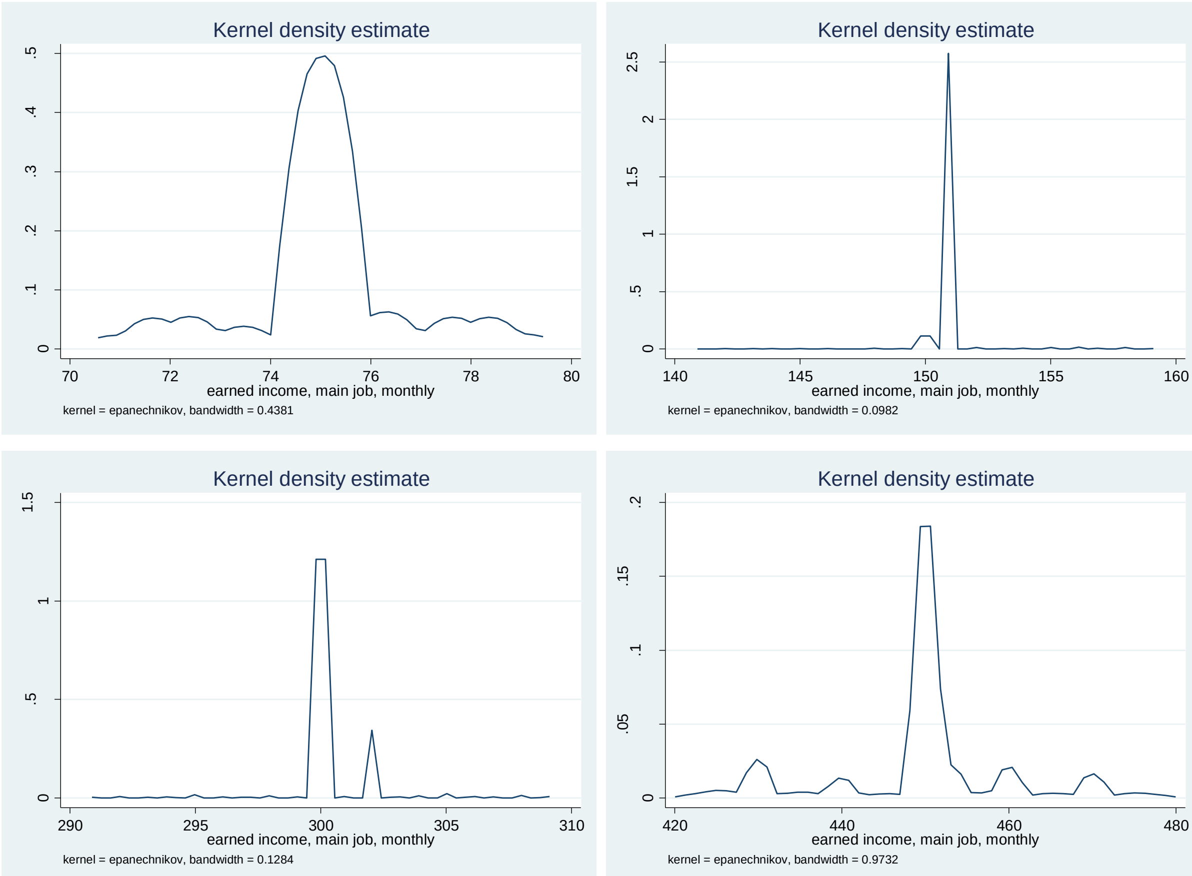
Source: Demographic Census, 2000.

Figure A2. Wage distribution in the informal sector, 2010



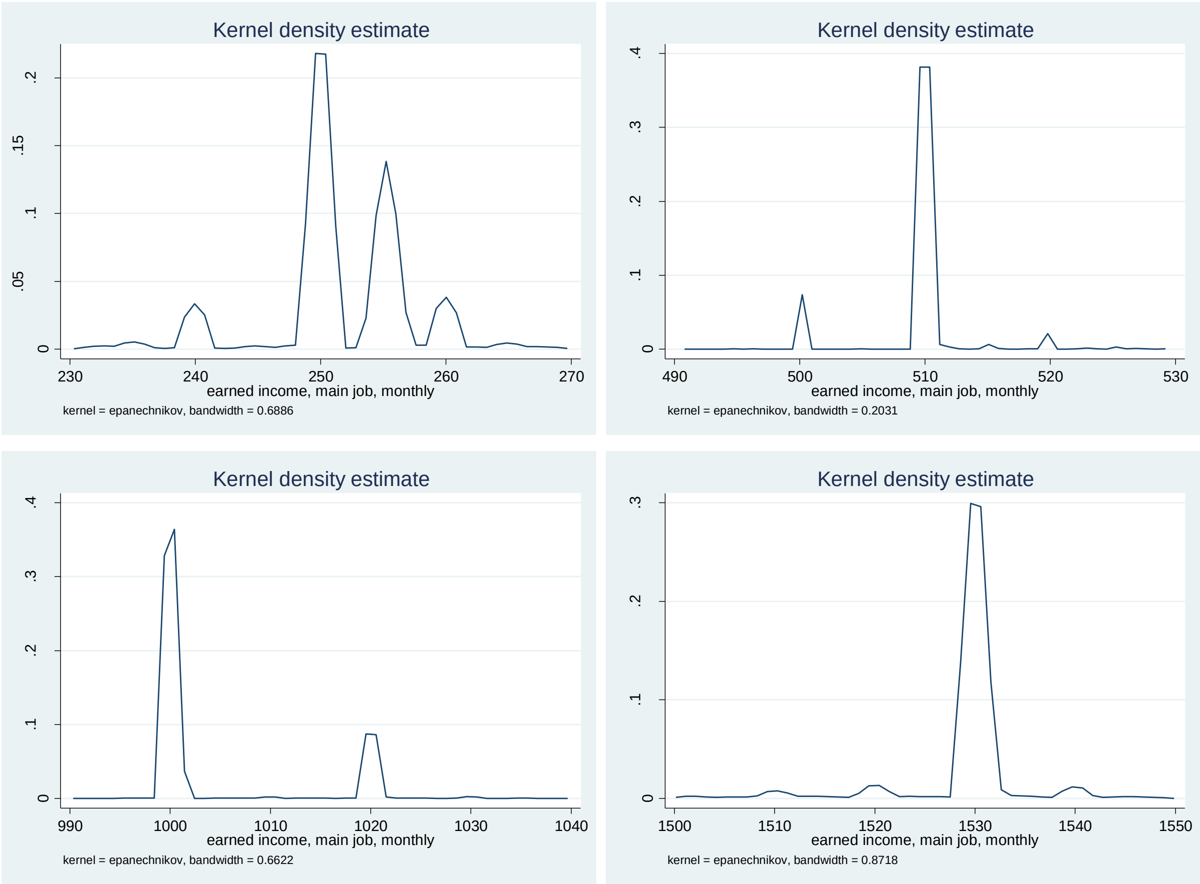
Source: Demographic Census, 2010.

Figure A3. Wage distribution in the formal sector, 2000



Source: Demographic Census, 2000.

Figure A4. Wage distribution in the formal sector, 2010



Source: Demographic Census, 2010.

Table A1. Regression Results: municipal fixed effects

	2010		2000	
	Year-interaction coefficient	Std error	Coefficient	Std error
CCT coverage	0.000	0.000	0.003***	0.001
Labor enforcement	0.002	0.002	–0.005	0.045
Minimum-wage bindingness	0.067***	0.019	0.002	0.013
Age	0.010***	0.000	–0.029***	0.000
Age squared	–0.012***	0.000	0.036***	0.000
Secondary education [d]	0.013***	0.002	–0.110***	0.004
College education [d]	0.011***	0.004	–0.115***	0.004
Female [d]	0.026***	0.003	–0.005**	0.002
Female with child [d]	–0.009***	0.002	0.032***	0.002
Formal-sector spouse [d]	–0.012***	0.001	–0.059***	0.001
Race - black [d]	–0.002	0.002	0.013***	0.002
Race - mixed [d]	–0.003*	0.002	0.011***	0.002
Race - Asian [d]	–0.027***	0.006	0.045***	0.006
Race - indigenous [d]	0.004	0.012	0.045***	0.008
Disabled [d]	–0.014***	0.003	0.044***	0.003
Urbanization	0.021	0.014	–0.071***	0.026
Agriculture	–0.215*	0.112	0.120	0.093
Fishing	–0.113	0.094	0.207	0.138
Extraction	–0.220**	0.094	0.073	0.193
Manufacturing	0.034	0.109	–0.367***	0.116
Utilities	0.311	0.271	–0.281	0.231
Construction	–0.322	0.208	0.000	0.162
Retail trade	–0.137	0.116	0.077	0.086
Housing	–0.150	0.118	0.033	0.110
Transportation	–0.009	0.118	0.007	0.102
Financial services	–0.472	0.366	0.371	0.547
Real-estate services	–0.028	0.104	–0.270*	0.144
Public administration	0.035	0.143	–0.042	0.101
Education	0.443***	0.136	–0.026	0.114
Health services	0.359	0.242	–0.344	0.235
Other public services	–0.018	0.303	0.338*	0.173
Constant	–0.250**	0.120	1.020***	0.079
Sample size		*3,841,496*		

Note: The dependent variable is the categorical variable *Informal*, which equals 1 if the worker is employed informally and zero if employed formally. Levels of statistical significance of the estimated coefficients are indicated by asterisks: 10 % (*), 5% (**), and 1% (***).