Informality in Brazil:
Does Urban Land Use and Building Regulation Matter?

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Abstract

This paper analyzes the connection between informality and urban land use and building regulations in Brazil. Using a theoretical framework that relates the elasticity of demand in informal and formal sectors of the housing market, the paper argues that inappropriate regulations raise prices in the formal market and generate more untitled housing. Of the four types of urban regulations examined, zoning seems to have the largest impact on informal settlements. Controlling for the time of enactment of the regulation reinforces these conclusions. The findings are consistent with a market with double standards and refute the notion that formal and informal markets are completely independent. On a policy level, the results suggest that land use or building regulations should not be abolished but instead revised to address the issue of informality appropriately.

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Introduction

Informality is a major concern for developing countries. The United Nations estimates that more than one billion people live in precarious housing worldwide, accounting for 32 percent of the global urban population. Although the share of slums in Latin America has been decreasing, the problem of informal or irregular settlements is still widespread. In Brazil alone, nearly a third of the urban housing lacks sewer connections.

Among the many causes of informal (or irregular) housing settlements, this paper focuses on one possible source—inappropriate land use or building regulation. Until recently, few papers in economics addressed the effect of regulation on the quality of housing. Most analyses of housing demand and supply instead approached the topic as an application of the Coase theorem: land use regulation solves a property rights issue (see, for example, Bailey 1959). Regulating building height, for instance, protects nearby residents' view and access to sunlight, thus eliminating negative externalities and increasing the welfare of citizens. Another benefit of regulations is that they can reduce the information gap in the marketplace. Assuming that consumers do not know what reasonable standards are, setting minimum quality standards (with inspections) would also improve the welfare of residents.

The main debate in the economics literature involved comparing the efficiency of regulation with that of taxes or fees. The public finance theory states that using taxes or regulation yields the same results, making taxes an alternative to regulation in achieving any policy goal. However, regulation would be superior to taxes when conditions make taxing difficult or not feasible, as posed in classical theory. In any case, the assumption was that the standards defined by regulation were beneficial and the only economic question concerned the most efficient way to enforce such standards.

Regulation does, however, have a potential down side. One of the main debates on this subject in the 1970s was whether Tiebout-type equilibrium would concentrate income or not. In particular, it was argued that zoning might be a way to exclude low-income households migrating to the city. This debate was influential in the passage of Proposition 2-1/2 in Massachusetts and Proposition 13 in California. While the debate was more intense in the urban planning literature, the urban economics literature made an important contribution by identifying the potential negative effects of regulation.

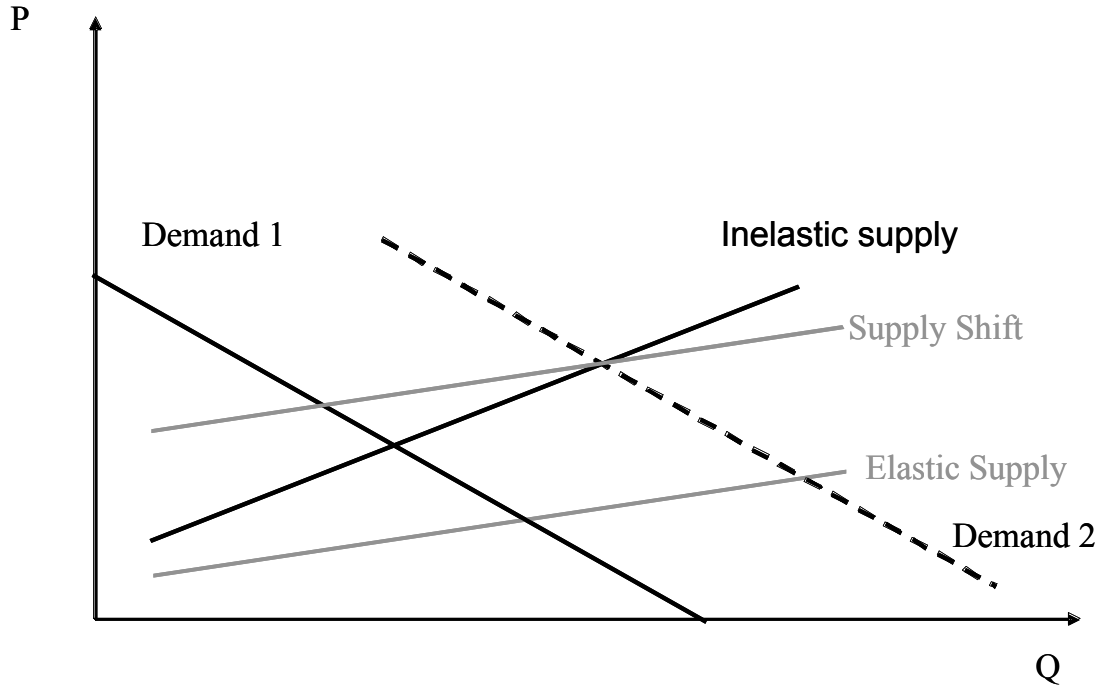
The literature on the impacts of land use on efficiency and distribution dates from the 1960s and focused essentially on zoning. As is well known, cities impose many different types of land use regulation. The seminal papers of Alonso (1964), Mills (1967), and Muth (1969)—considered the foundation of the new urban economics—demonstrated the importance of housing markets in urban economics. Even so, the literature in urban economics and real estate economics continued to develop independently.

One of the first studies to recognize that rising prices require not only rising demand but also limits on supply (through regulation) is by Ellickson (1977). Mayo and Angel (1993) were among the first to attempt to associate regulation with the elasticity of house supply. Malpezzi (1996) used the Wharton Residential Land Use Regulatory Index, which accounts

for many different urban regulations and the time needed to approve a new development, to show that an increase in regulation is associated with a less elastic supply of housing. Several analyses followed Malpezzi and the theme reached the mainstream in 2005 with the publication of "Regulation and the High Cost of Housing" in the *American Economic Review.* The papers included in that section (Glaeser, Gyourko, and Saks 2005a; Quigley and Raphael 2005; and Green, Malpezzi, and Mayo 2005) have significantly influenced the analysis of housing supply and urban growth.

The results of their work show that increased regulation is positively related to higher housing prices through a decrease in the elasticity of the housing supply. Figure 1 illustrates the main argument. Regulation makes the supply of housing more inelastic. As a result, increased demand for housing in an unregulated city implies a smaller rise in prices than in a regulated city. The stylized fact supporting this argument is the huge difference in price increases in slow-growing and fast-growing cities over the last 50 years, ranging from more than a 40 percent drop to more than 500 percent rise.

Figure 1: The Impact of Regulation on Housing Prices



Source: Glaeser, Gyourko and Saks (2005b)

Basic comparative static indicate that when demand significantly increases in cities with inelastic supplies of land, prices rise more sharply than in cities with more elastic supplies. Regulation may either make the housing supply less elastic or shift the supply curve up to the dashed line in Figure 1. While it is possible to argue for both outcomes, the impact on housing prices and quantity is qualitatively the same. In either case, prices would rise more quickly and quantity would grow more slowly in regulated cities than in unregulated ones.[1]

[1] There is evidence to support both hypotheses. Land use regulations may also increase exclusion in the cities "protecting" residents from migrants, although it would be difficult to determine whether this was through increased elasticity or through a shift in the supply curve.

The main difficulty with this analysis is the problem of endogeneity. Although not obvious in the United States (Gyourko, Saiz, and Summers 2006), cities that are more likely to regulate land use are likely to be the ones that are growing rapidly. In these cities, the supply of land may be limited because the area is more urbanized. Most recent studies attempt to control for endogeneity but often do not consider other reasons that prompt some cities to regulate, such as the desire to preserve the environment, open space, and other amenities. The residents of these cities would tend to value regulations more than residents of other cities.

Few studies recognize that in most developing countries with weak institutions and high poverty, higher housing prices in the formal market may induce households to build irregular dwellings, i.e., houses that do not comply with the regulations. Among the few attempts to connect regulation with the size of the informal market are Biderman (2006), Henderson (2007) and Lall, Wang and Da Mata (2006). In the latter study, regulation seems to be insignificant in determining slum formation. Dowal (2007) and other works he directed such as Avila (2007) provide some stylized facts about informality in Brazil and attempt to demonstrate its role in limiting the housing supply. Avila extends the analysis by furnishing evidence that links regulation with increases in informal housing.

Contributions of This Paper

While the connection between informality and excessive housing standards goes back to at least Turner and Fichter (1972), the innovation of this study is to apply the framework proposed for analyzing house price dynamics in the United States to developing countries. Unlike the few empirical papers in economics that attempt to connect regulation and land use, this paper models the substitution between the formal and the informal markets, using the differences between the two markets as the main variables. Using the time when regulation was enacted to estimate the impact also represents an innovation, leading to a more meaningful specification of the problem and to new possibilities in the sensitivity analysis of the results.

The following section briefly discusses the definition of informality and describes the housing market typology used in this paper. In the third section, a random utility model is proposed that assumes formal and informal markets are substitute goods, and that households will (dichotomously) decide which good to consume based on its utility. The main insight from this microeconomic foundation is that the variable of interest is the informal share of the housing market relative to the formal share. A significant change in the price of formal housing will be reflected in a change in the share of informal housing in the city. As a result, using relative values as independent variables controls for an important determinant of household choice between the formal and informal markets.

The fourth section discusses the econometric strategy that compares the average informal housing market share in municipalities that enacted land use or building regulation in the first half of the 1990s with the share in municipalities that enacted the law in the first half of the 2000s. The fact that municipalities in Brazil enacted regulations at different points in time provides a good opportunity to analyze whether there is a connection between regulation and the size of the informal market because the performance of each group of municipalities may differ.

The fifth section describes the data supporting the analysis and the variables used in specifying the equations. Municipalities are divided into groups according to the date they enacted regulation, using five-year intervals starting in 1980 and ending in 2005.

The sixth section presents the results of the econometric analysis. The main finding is that urban land use and building regulation do induce more informality. Although some of the estimates are not quite precise, the level of significance is usually above 85 percent. In addition, the impact of regulation is always positive (with the one exception of parceling) and the results for different groups of municipalities are highly consistent.

If land use or building regulations induce more informality, the implication is that the municipality enforces the regulation in the formal part of the city but not in the informal part. The cause of this double standard is not apparent. One explanation, suggested by the literature on zoning for the United States, is that regulation is a strategy for excluding the poor from public services. In the U.S. case, the main exclusion is from public education. In Brazil, it is excluding the poor from basic urban services.

Another explanation, found by Mayo and Angel (1993) in some developing countries, is that regulation creates red tape that fuels corruption. Of course, corruption among municipal inspectors in Brazil may be one reason why regulation leads to an increase in housing prices. Without this corruption, the impact of the regulation might be very small and the policy implications of the findings would be very different. The most reasonable assumption is that regulation does induce some exclusion of the poor, and its effect is magnified by corruption among municipal officials.

The paper concludes with a discussion of future research on policy alternatives to deal with the undesired impacts of regulation on informality. Simply eliminating the regulation is not an option; minimum housing standards are essential. Today, however, urban land use and building regulation sets minimum standards only for those who can afford formal housing and defines no standard for others. When setting minimum standards, regulators must keep in mind that not everyone can afford to comply with such standards. Addressing this problem is an important issue. If minimum standards are not accompanied by subsidization of those who cannot afford to meet them, the regulation should be reviewed.

Defining Informality

Conceptually, a house may be defined as informal or irregular if it does not comply with any regulation. However, no such measure is available. There are three possible ways to define informal houses using the Brazil's 1991 and 2000 census data. One option, adopted by Lall, Wang and da Mata (2006), is the concept of a "subnormal" census block. To be considered a "subnormal agglomeration," the block has to satisfy the following conditions: (a) form a group of more than 50 housing units; (b) occupy the land illegally; and (c) exhibit a disorderly pattern of urbanization and/or lack essential public services. This definition is very appealing because it includes both irregular and illegal units. The main problem, however, is that the municipality defines whether a block is subnormal or not, and political concerns may influence the definition.

The second option, adopted by Dowal (2007), defines an informal house as one not served by public utilities. The main shortcoming of this definition is that the supply of public services

such as water and electricity has become almost universal. The only service that is still growing at a slow pace in Brazil is sewerage. A reasonable proxy for informal housing would therefore be lack of a sewerage connection. But for our purposes this proxy would be inappropriate because the majority of the regulations that we analyze do not affect service provision.

The third definition of informal housing, used by Avila (2007) and Biderman (2006), is when residents declare they own the houses but not the land. This is usually the case in illegal settlements. The problem with this definition is that some irregular houses may have secure land tenure. In addition, it is not clear how the householder defines tenure. If land use regulation raises housing costs in the formal market and induces newcomers to choose informal housing, the number of irregular settlements might increase but the number of untitled houses might not. This may be less of an issue in the definition using lack of public services, which is not only highly objective but also does not guarantee that a house with public services complies with urban regulations. The main shortcoming of the "no title" proxy is that it is unavailable in the 1970 or 1980 censuses, which is not an issue here because the focus is on the 1990s. In addition, the definition is appealing because it seems very precise. For the 830 municipalities with reliable cadastre for informal settlements,[2] the correlation between the number of units measured by the census variable and the number of units reported by the municipalities is 95 percent.

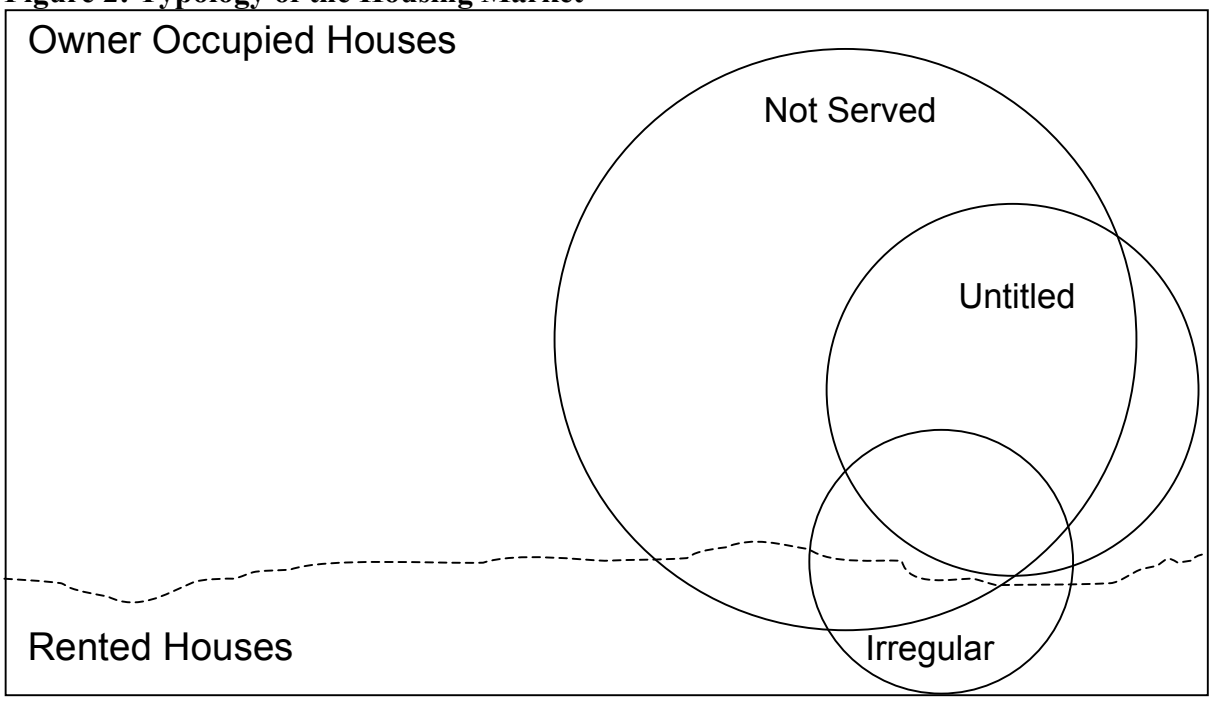
The three definitions of informal housing relate to different aspects of informal or irregular settlements. As Dowal (2007) proposed, a house can be informal or irregular if it (a) is in precarious condition; (b) lacks basic infrastructure; and (c) lacks secure land title. These three proxies are connected with each aspect noted above—the first is associated with subnormal agglomeration; the second with the lack of sewerage services; and the third with the lack of land tenure. A comprehensive classification could include all of these definitions, i.e., a house would be considered informal if it had any of the three characteristics. A very concise classification would consider a house informal if it had all three characteristics.

Figure 2 summarizes the data used for this study, recognizing that the informal or irregular market is part of the housing market. The rectangle represents the entire market, which includes both owner-occupied houses (86 percent of the total 2000 stock) and rented houses. Each of the two major groups can be divided into three subgroups. The intersection between the untitled group and rented houses is an empty set by definition, given that a renter cannot own the house or the land.[3]

[2] It is not possible to use cadastre information (supposed to be more precise) because we do not have a time series.

[3] If someone with no property title were to rent his or her house, this information would not be recoverable even using microdata. It is not reasonable to ask the tenant about the titling position of the owner. The diagram does not indicate that no untitled houses are rented out, but makes it explicit that there are no data on rented untitled houses.

Figure 2: Typology of the Housing Market



The figure is approximately in scale with census data. Although it might be expected that irregular housing would represent the largest group, the census information does not confirm this because of the use of the "subnormal" definition discussed above.

The proxies for informal or irregular housing have an institutional counterpart. During the dictatorship period (1964–1985), governments were not allowed to supply public services to illegal settlements. The municipality could not furnish services to an untitled property or provide title to a property located in unserviced land. To get approval for a new subdivision, the developer had to build distribution networks for basic infrastructure and provide lot connections. The municipality or other level of government was responsible for providing the trunk infrastructure (such as large sewerage and water mains). In some cases, developers would start the project without installing the distribution network using a temporary license. In this situation, they would usually leave part of the land to the municipality as collateral for implementing the infrastructure later. But developers would often fail to complete the project and the public land given as collateral would be occupied. Those houses would probably be classified as illegal, while other houses may have a land title but not a construction license. In practice, then, houses can have no sewerage connection and secure land tenure, or the other way around, as Figure 2 illustrates. In this study informality is defined as lack of secure land title identified as residents declaration that they own the houses but not the land.

Modeling the Demand for Housing

We start with a random utility model where the level of utility that a consumer derives from consuming a house is a function of a vector of individual characteristics (h_i), house

characteristics (p_j), and city characteristics (z_c).[4] That is, the utility derived by consumer i from consuming house type j is given by the scalar value:

$U(h_i, p_j, z_c, e_{j,i\,c}; \square)$

where $\square_{\square,j,c}$ are errors and unobserved individual, product, and city attributes[5] and $\square$ is a vector of parameters to be estimated. Since consumers with different characteristics make different choices, deriving the aggregate demand for house type j in city c requires integrating the choice function over the population of interest. Given that we are interested in only two broad types of housing, we can restrict j to two alternatives: j=0,1 where 0 is a formal house and 1 is an informal house.[6] Let:

$$A_{1,c} \equiv \left\{h : U(h_i, p_1, z_c, \varepsilon_{1,i,c}; \theta) \geq U(h_i, p_0, z_c, \varepsilon_{0,i,c}; \theta)\right\} \quad (1)$$

where $A_{1,c}$ is the set of values of h that make an informal house the preferred choice. Assuming that ties occur with probability zero and that $P_c(h)$ is the density of h in the population of interest, the proportion of informal housing in city c is given by:

$$s_{1,c}(h, p, z, \varepsilon; \theta) = \int_{h \in A_{1,c}} P(h)\mathrm{dh} \quad (2)$$

If we assume a linear function for utility, we will get a demand system derived from a standard discrete choice model that is used extensively in the literature.[7] Let:

$$\begin{aligned} &U(h_i, p_j, z_c, \varepsilon_{j,i,c}; \theta) = \alpha' h_i + \beta' p_j + \varphi' z_c + \varepsilon_{j,i,c} \equiv \theta' x_{j,i,c} + \varepsilon_{j,i,c} \\ &where \quad \theta' \equiv [\alpha, \beta, \varphi] \quad and \quad x'_{j,i,c} \equiv [h_i, p_j, z_c] \end{aligned} \quad (3)$$

If we assume that $\square_{i,j,k}$ is independent and identically distributed (i.i.d.), the proportion of informal housing in city c is given by:

$$s_{1,c}(h, p, z, \varepsilon; \theta) = \int_{\varepsilon} \prod \prod P(\theta' x_{1,c} - \theta' x_{0,c} + \varepsilon_{1,c} - \varepsilon_{o,c})\mathrm{d}\varepsilon \quad (4)$$

where we have integrated over each type of housing (formal and informal). From (4), it is clear why the McFadden model for analyzing multi-product sectors is so popular. Adding the hypothesis that $\square$ has type 1 extreme value distribution: exp(-exp(-$\square$)), we can solve (4) analytically:

$$s_{1,c}(h, p, z, \varepsilon; \theta) = \frac{e^{\theta' x_{1},c}}{1 + e^{\theta' x_{1},c} + e^{\theta' x_{0},c}} \quad (5)$$

[4] We allow that the utility of consuming housing in a city is not the same as consuming an otherwise identical housing in a different city.

[5] While it would be interesting to allow for different unobservables for each category (city, household and housing), the model is simplified to make it empirically feasible.

[6] We do not include the outside alternative, i.e., not consuming any house. Since we are treating owner-occupied and rental houses in the same way, the only group that will not be considered in the analysis are the homeless. In any case, this is a very small group in Brazil.

[7] The seminal reference is McFadden (1981), which is still the best introduction to the discrete choice model.

To estimate the share of the formal market, we derive an analogous expression. Using (5), it is easy to see that:

$$\ln(s_{1,c}) - \ln(s_{0,c}) = \ln(M_c s_{1,c}) - \ln(M_c s_{0,c}) = \theta'(x_{1,c} - x_{0,c}) \qquad (6)$$

where M_c is the total number of houses in city c. It is possible to estimate (6) by OLS. The dependent variable is the difference in the log of the total number of informal houses and the log of the number of formal houses (the left side of [6] hereafter denoted "relative market share"). The independent variables are the differences in aggregate characteristics in each submarket (informal, formal) of houses, households, and any other city characteristics that might influence the utility of consuming housing.

However, we will not use the specification implicit in (6) directly. As discussed in the following section, this result will be useful in finding a specification for our particular interest, i.e., the impact of regulation on informality.

The usual critique of this expression is that it implies that the effects of consumer characteristics are independent of observed product characteristics (Berry, Levinsohn and Pakes 1995). In particular, it generates substitution effects that depend only on product characteristics. If we were considering the whole real estate market, the traditional demand system derived from a discrete choice model would imply that demand for a luxury apartment and for a house in a slum would have the same cross-price derivative with respect to any other house if the market share of each product were the same. This well-known problem is called the Independence from Irrelevant Alternatives (IIA). Note that this is not a concern here because there are just two alternatives.

The literature attempting to estimate the impact of land use and building regulation in the United States considers the housing market with no product differentiation. This simplification is possible because informal housing is such a small portion of the U.S. market. This is not the case in developing countries in general, and certainly not in Brazil. With two differentiated products, if the elasticity of supply decreases (due to regulation), prices in the formal market would increase and informal housing would become more attractive. Households would substitute consumption in the informal market for consumption in the formal market, where demand would fall. Lower demand in the formal market may imply that the equilibrium price would not go up. Thus, estimating the impact of regulation on formal housing prices using the same hypothesis as in the United States may not work because the U.S.-based literature does not take into account the substitution effect. Nevertheless, the main point is similar. Regulations do have a beneficial side by improving the urban environment in general. The detrimental side is that they may promote the demand for informal housing.

It is also important to note that, in general, the impact of cost-increasing regulation on the growth of informality may be positive or negative. A regulation that pushes up housing prices may reduce immigration to the city. As a result, the regulated city may have slower growth in informal settlements than the unregulated city. We must therefore compare cities with the same population growth, the same income growth, and other basic characteristics, as is implicit in equation (6). If cities that adopted cost-increasing regulations during the 1990s

saw more rapid growth in informality than comparable cities that did not implement such regulations, we can say that the regulation was responsible for the increase in informal settlements.

Analytical Approach

Analysis of the Brazilian housing market with two submarkets (formal and informal) is particularly interesting because one group of municipalities enacted land use regulations during the 1990s and another group did not. Since we have proxies for informal land occupation in both the 1991 and 2000 censuses, as well as the dates that a few broad types of land use regulations were enacted, it is possible to compare the change in informal housing in cities that adopted regulation during the period (treatment group) with the change in municipalities that remained unregulated (control group).

As discussed later, the durability of the product (housing) makes timing a crucial variable in defining the groups for the analysis. For now we assume the two groups are well defined. As in the evaluation of any social program, we can use the outcomes of nonparticipants to estimate what participants would have otherwise experienced. The difference in outcomes is the estimated gross impact of the program. The strategy here is to use this approach to measure the impact of urban regulation on the size of the informal housing market.

This approach, however, has the same shortcoming that is common to any social program evaluation. The outcomes for municipalities that enacted urban regulations in the 1990s and for those that did not do so until 2000 are systematically different from what the outcomes of regulated municipalities would have been without enacting the regulation, producing selection bias in the estimated impacts. To make this comparison, the outcome of municipality *i* in period *t* is assumed to depend on a vector of observed characteristics and an idiosyncratic (unobserved) characteristic specified for each city. As will soon be clear, we will explore the pseudo-panel feature of the data to deal with the unobserved characteristic. Let:

r_c = 1 if city *c* implemented the regulation in the 1990s and 0 otherwise
τ_t = 1 if the data are from 2000 and 0 if they are from 1991

Let y_{jct} denote the relative market share of informal housing for city *c* at time *t*, and *j* indexes[8] the treatment: *j*=0 (treated) and *j*=1 (untreated). In addition, assume that the relative market share is linear, i.e.:

$$y_{jct} = \beta_1 + \beta_r r_c + \beta_\tau \tau_t + \beta_d j + \theta' x_{ct} + u_{jct}, \qquad \begin{array}{l} j = 0,1, \qquad c = 1,...,N, \\ t = 1991,2000, \qquad E(u_{jct}) = 0 \end{array} \tag{7}$$

Using the notation in equation (6) $y_{ct} \equiv \ln(s_{1,c,t}) - \ln(s_{0,c,t})$;[9] $x_{ct} \equiv x_{1,c,t} - x_{0,c,t}$; and we are allowing for differences in the average relative market share by city, time, and regulation represented by the β coefficients in (7). From (7) it is easy to see that $y_{1ct} - y_{0ct} = \beta_d + u_{1ct} - u_{0ct}$ and thus:

[8] In our demand system model, *j* represents the market (formal or informal). Since we do not need this index anymore (we are using the numbers that correspond to the products), we will use it for the treatment.
[9] Recall that we do not observe y_{jct}; we can observe only y_{ct}.

$$E(y_{1ct} - y_{0ct}) = \beta_d$$

That is, β_d is the main parameter of interest. The problem, once again, is that we just observe either y_{0ct} or y_{1ct}. However, what the econometrician actually observes is $y_{ct} = (1 - r_c\tau_t)y_{0ct} + r_c\tau_t y_{1ct}$. Looking at (7), it is possible to imagine the general econometric strategy, i.e., regressing the relative market share on dummies for treatment year, post-treatment year, and on the interaction between treatment and post-treatment years; and on controls. In this way, the coefficient of the interaction between time and city will be the measure of the impact. Alternatively, (7) could be estimated by first difference or fixed effects (which are exactly the same because we have just two census years), of course removing the idiosyncratic city component (r).

Before moving to the actual specification, let us decompose the measurement to enhance our understanding of the hypothesis behind the specification. Omitting the subscript c, the two differences implicit in the difference in difference (DD) estimator are:

$$\begin{aligned}
E(y_{1,2000} - y_{0,1991}|x, r=1) &= \beta_1 + \beta_r + \beta_\tau + \beta_d + \theta' x_{2000} + E(u_{1,2000}|x, r=1) \\
&\quad -\{\beta_1 + \beta_r + \theta' x_{1991} + E(u_{0,1991}|x, r=1)\} \\
&= \beta_\tau + \beta_d + \theta'(x_{2000} - x_{1991}) + E(u_{1,2000} - u_{0,1991}|x, r=1) \\
E(y_{0,2000} - y_{0,1991}|x, r=0) &= \beta_1 + \beta_\tau + \theta' x_{2000} + E(u_{0,2000}|x, r=0) \\
&\quad -\{\beta_1 + \theta' x_{1991} + E(u_{0,1991}|x, r=0)\} \\
&= \beta_\tau + \theta'(x_{2000} - x_{1991}) + E(u_{0,2000} - u_{0,1991}|x, r=0)
\end{aligned}$$

Differentiating the two differences above gives the DD parameter, β_d. The decomposition makes it clear why the method is called differences in differences: we first differentiate each group by time and then differentiate the differences between each group. That is, the impact from regulation will be measured by the growth in the relative market share of informal housing in regulated cities net of the growth on unregulated ones. For the estimator to be unbiased we need:

$$E(u_{1,2000} - u_{0,1991}|x, r=1) = E(u_{0,2000} - u_{0,1991}|x, r=0) \quad (8)$$

This is known as the "same time-effect condition." This condition is weaker than the necessary condition for identifying the impact on a cross section. The regulated and unregulated cities may be systematically different as long as these differences do not change during the period of interest. Even if the differences between the groups come from unobserved variables affecting the baseline, the difference will not matter for estimating the DD parameter. That is, the difference of the differences in the two groups will purge the selection bias that makes the distributions not comparable. In this sense, DD allows for unobserved confounders, which is one of the main reasons why this specification is popular.[10]

[10] Although appealing, DD is not a panacea. The seminal paper by Bertrand, Duflo and Mullainathan (2004) points out the problems related to ignoring the serial correlation of the data.

Condition (8) is similar to the traditional "selection in observables" condition. If we had just a cross section of observations for 2000, the selection in observables condition would be $E(u_{1,2000} \mid x,r=1)= E(u_{0,2000} \mid x,r=0)$. Of course, it is a weaker condition in the sense that it is easier to believe that the source of unobserved difference between the two groups will stay the same over time than it is to assume there are no unobservable (systematic) differences between the groups. In this situation, we have to look at the flow of new houses into the market because urban regulations do not apply to the entire stock. The argument is that an increase in formal housing prices will induce households to change their residential choices, and that will be reflected in the share of the informal market relative to that of the formal market.

So far we have assumed that the decision to enact urban regulations was random. This is one of the main problems when dealing with a non-randomly assigned treatment. The city decided to enact the regulation for both observed and unobserved reasons. If we assume a linear probability model conditioned on a vector of variables w_{ct}, i.e., a vector of variables influencing the decision to enact (or not to enact) the regulation, we get the following model for the observed response:[11]

$$y_{ct} = \beta_1 + \beta_r r_c + \beta_\tau \tau_t + \beta_d r_c \tau_t + \theta' x_{ct} + \beta_{dw}' r_c \tau_t w_{ct} + (1 - r_c \tau_t) u_{0ct} + r_c \tau_t u_{1ct} \qquad (9)$$

In this case, the expected impact of the regulation on the relative market share of informal housing would be measured by $\beta_d + \beta_{dw}' E(w_{ct})$ where $E(w_{ct})$ is the expected value of w_{ct}. This means that we will have a different impact depending on the population used for calculating this expected value. In particular, we will probably have different impacts for the population, the treatment group, and the control group.

Given the discussion above, specification (9) is the generic way to estimate the impact of land use regulation. That is, we have not been specific about the variables that will be used for the outcome, the cities that will be in the treatment group (r_c), the controls for housing demand (x_{ct}), and the variables influencing the decision to enact the regulation (w_{ct}). We have already mentioned that the outcome will be the relative market share of informal housing. Note, however, that we can define the treatment and the control in different ways. Since the regulations usually take some time to be enforced and housing is a highly durable good, we do not expect to see an immediate impact from the time of enactment.

Let us assume that, without the regulation, the informal housing market was growing at 1.0 percent per year; the formal market was growing at 4.0 percent per year; and the informal market initially represented 25 percent of the housing stock. To enact (and enforce) the regulation would induce another 1.0 percentage point increase in the informal market, meaning that the informal market would grow at 2.0 percent per year. Under these assumptions, let us compare three otherwise identical municipalities except that the first enacted regulation in year 1 and the second in year 2, and the third did not enact regulation (table 1).

[11] To derive (9), note that the observed response can be written as $y_{ct} = r_c \tau_t y_{1ct} + (1 - r_c \tau_t) y_{0ct}$.

Table 1: Market Share of Informal Housing

City	Year 0	Year 1	Year 2
1	25.00%	24.82%	24.64%
2	25.00%	24.46%	24.28%
3	25.00%	24.46%	23.92%

If we compare the variation in the informal market in City 1 (treated) with the variation in City 3 (control), we observe that the share of informal housing decreased by 0.36 percent in City 1 and by 1.08 percent in City 3. The net impact of the regulation would therefore be 0.72 percent. However, comparing City 2 (treated) with City 3 (control), the impact would be 0.36 percent. The timing of enactment may thus imply many different controls and treatment groups. For this reason, we use a multiple treatment/control group framework rather than an integer variable counting the years of enactment for the treatment group.

Defining Groups for Comparison

It is difficult to define the group to which a municipality belongs. The 2005 Basic Municipal Information Survey (*Pesquisa de Informações Básicas Municipais*—MUNIC), conducted by the Brazilian Institute of Geography and Statistics (*Instituto Brazileiro de Geografia e Estatística*—IBGE), gives the original date of enactment for several regulations but does not list the last update. The 2001 survey does not disclose any information on enactment year although the information was collected.[12] Another problem is that in some cases not all regulations were approved in the same year, and it is preferable to compare municipalities with the same set of standards except the one under analysis.

But the main difficulty is that time is continuous; the municipalities were enacting the regulations sequentially. Federal legislation did not require municipalities to enact urban regulations by a given date. We therefore have to define periods of enactment to construct groups of comparison. To start the process, we divide the sample into seven ranges (table 2).

Table 2: Groups Based on Period of Enactment of Urban Regulations

Date of Enactment		Centered Year	Group
From	To		
Any Year	1981	---	1
1981	1985	1983	2
1986	1990	1988	3
1991	1995	1993	4
1996	2000	1998	5
2001	2005	2003	6
2006	Any	---	7

Source: IBGE MUNIC 1999, 2001, 2005.

[12] The original date of the specific law in 2005 was also not disclosed to the public but IBGE kindly furnished this information.

The first threshold is connected to enactment of federal law 6766/79 (see Appendix B) and includes the municipalities that enacted regulation before 1981 (Group 1). We are not sure how local laws were defined before the federal law introduced major institutional change. As a result, we assume that the relationship between regulation and informality in the group of municipalities that enacted regulations prior to 1981 remained unchanged during the 1990s.

The second and third groups enacted regulations between 1981 and 1985 and between 1986 and 1990. Both groups might experience some lagged impacts from the new regulation. It is very unlikely that the 1986–1990 group would feel no impact since all the effects of the regulation would have to vanish within three years. In theory, the 1991–1995 group is more likely to show impacts since the municipalities were exposed to the regulation for seven years on average. The 1996–2000 group may or may not show impacts, but if they do, the impacts would be smaller than for the 1991–1995 group because the municipalities would be exposed to the regulation for just two years on average. The 2001–2005 and later groups are not expected to see any impact in the period analyzed since the regulations were not yet enacted.

Either the 1985–1990 group or the 1991–1995 group would be the most meaningful treatment group depending on the time needed to enforce the regulation, to notice significant changes in the flow of housing, and to have the impact of the regulation disappear.[13] If untitled housing declined more slowly in these municipalities than in similar cities, the cause should be connected to when the regulation was enacted. We define the control group as the municipalities that enacted regulations between 2001 and 2005.[14]

We opted to keep the maximum number of municipalities in the treatment and the control groups to obtain additional information on other independent variables that might strengthen the model. As discussed above, the impact on the 1996–2000 group is expected to be smaller than on the 1991–1995 group, and no impact is expected for municipalities that have not enacted regulation. In addition, it is impossible to predict the impacts on the 1986–1990 group relative to those on 1991–1995 group because the effects depend on the time needed to enforce the law and for the impacts to decline.

Now all the ingredients are in place to move from the generic specification (9) to the actual specification used in this paper. To do so, we first redefine the city dummies to include all groups. Then we incorporate the independent variable (relative share of the informal housing market), all treatments, and all controls.

$$\ln(s_{1ct}) - \ln(s_{0ct}) = \beta_1 + \sum_{j\neq 6}^{7} \beta_r^j r_c^j + \beta_\tau \tau_t + \sum_{j\neq 6}^{7} \beta_d^j r_c^j \tau_t + \theta'(x_{1ct} - x_{0ct}) + \sum_{j\neq 6}^{7} (\beta_{dw}^j)' r_c^j \tau_t w_{ct} + v_{ct} \quad (10)$$

[13] It is impossible to determine the exact point in time when the impact of a regulation in the housing market declines or disappears.

[14] Theoretically, the control group could consist of municipalities that had not enacted the regulation until 2005. However to compare this group with municipalities enacting regulation during the 1990s is not ideal for two reasons. First, the elapsed time is long (five or more years), and second, this is a very heterogeneous and large group of municipalities.

where $r_c^j = 1$ if city c is in group j and 0 otherwise; β_r^j is the idiosyncratic parameter for cities in group j; β_d^j is the average impact of regulation if there was no interaction with other variables; β_{dw}^j is the coefficient on the interaction of the treatment and the j group; and $v_{ct} \equiv (1 - r_c\tau_t)u_{0ct} + r_c\tau_t u_{1ct}$. Note that the vector of controls is defined as a difference in two vectors: $x_{1ct} - x_{0ct}$. Some controls, however, are at the city rather than the submarket level as discussed below. For the sake of simplicity, assume that $x_{0ct} = 0$ for those controls, so they require no specific notation.

Looking at specification (10), we can think about our estimator as a triple difference (TD) estimator. We first explicitly differentiate (the log of) the informal and formal housing market shares; we then implicitly differentiate the differentiated shares in 2000 from the differentiated shares in 1991; and finally, we differentiate the treatment group and the control group. The first difference comes from the micro foundations of the model, while the other two come from the analytical approach. It is important to note that the outcome variable measures the ratio of informal housing to formal housing. For instance, if 20 percent of the houses in a city were informal, the outcome would be (the log of) one quarter (20/80). We will test if (the log of) this ratio decreases more slowly in cities that enacted urban regulation during the period than in cities that did not.

Although the DD estimate implicit in specification (10) is very appealing, differentiating the covariates may wipe out all the variation in some instances. For example, our hypothesis is that regulation increases the price of formal housing, inducing households to move to informal housing. If this is true, controlling for formal housing prices would remove part (or all) of the effect of the regulation on informality. However, we may want to control for the initial (1991) formal house price because cities where prices were already high may respond differently to land use regulation than those where prices were initially low.

Data and Variables

To carry out the analysis, a comprehensive database was built from three major sources: censuses for 1991 and 2000; the MUNIC for 1999, 2001 and 2005; and municipal revenues and expenditures for 1992 and 2000 from the Brazilian Treasury Secretariat (*Secretaría do Tesouro Nacional*—STN). Census data were used to construct the dependent variable (ratio of informal to formal housing) and control variables for each city and for each submarket. The MUNIC data were used to construct the treatment and control groups, and the fiscal data provided an important control for differences in municipal investment and revenue patterns.

The starting point is identifying the groups described in the previous section. Enough observations for each group are needed to make the analysis robust. In theory, there would be 5,507 observations (the number of municipalities in Brazil in 2000), but only 4,491 cities existed in 1991. Over the decade, many municipalities were created by splitting one area into two or more new cities. Eliminating the municipalities created after 1991 or those that split between 1991 and 2000 reduces the sample to 3,613. Cities that did not provide the relevant information to the STN or to the MUNIC were also dropped. In sum, the analysis covers municipalities that did not split during the 1990s and have complete information. Depending on the model specification, the sample size varies from a minimum of 779 municipalities

(1,558 observations) to a maximum of 2,784 municipalities (5,568 observations) as shown in table 3.[15,16]

Table 3: Number of Observations and Municipalities by Type of Regulation

Regulation	Minimum		Maximum	
	Observations	Municipalities	Observations	Municipalities
Building Code	2,170	1,085	3,154	1,577
Urban Growth Boundary	3,664	1,832	5,568	2,784
Zoning	1,558	779	2,072	1,036
Parceling	1.994	997	2,714	1,357

Source: IBGE MUNIC 1999, 2001, 2005.

Independent Variables

The data on urban land use and building regulations come from the MUNIC surveys for 1999, 2001 and 2005.[17] The MUNIC collects the date of original enactment of regulations covering building codes, urban growth boundaries, zoning, parceling, master plan, and special zones of social interest (ZEIS).[18] Although the MUNIC gives the year of enactment of the regulation, it does not describe the characteristics of the regulation. This limitation is especially problematic in the case of master plans and ZEIS, which comprise a bundle of regulations that are impossible to disaggregate. For this reason, this analysis focuses only on building codes, urban growth boundary, zoning, and parceling as described below.

Building codes. This is the first urban regulation adopted in most municipalities, in some cases as early as 1929. The number of municipalities that have issued building codes began to

[15] These figures eliminate the municipalities in Group 7 (2006 and after), which are not covered by the 2005 MUNIC survey. The range of variation in the sample size matches the type of regulation analyzed. For example, only 779 municipalities provided information about zoning regulations but 2,784 reported urban growth boundaries. The higher number of observations compared to the number of municipalities in the sample reflects the two time periods used in the analysis.

[16] The approach differs from the one adopted by Lall, Wang, and da Mata (2006) and Avila (2007). Those authors used minimum comparable areas (*Areas Mínimas Comparavies*—AMC), developed by the Brazilian Institute of Applied Economic Research (*Instituto de Pesquisa Economica Aplicada*—IPEA). The AMC are defined in a way that aggregates municipalities that split during the 1990s. Here we chose not to use AMC because urban regulation is a municipal law, so it directly affects housing only within the jurisdiction. There might be indirect spillover effects on neighboring municipalities, but in that case all neighbors should be considered, not just the neighbors that split.

[17] IBGE started the MUNIC in 1999. The survey is conducted through questionnaires distributed to all city governments in Brazil to collect information on the administrative organization, finance and environmental management of municipalities. Its latest edition includes information about the public administration of the 5,560 municipalities existing in Brazil in 2004.

[18] The aim of the survey is to collect the original enactment date of each regulation, but that information is imprecise. The municipal employee who responds to the survey usually knows the current law, but not necessarily whether an earlier, similar law existed and, if so, when it was enacted. For instance, São Paulo had a parceling law in the 1960s but declared 1982 as the date of enactment. Thus, one must be cautious in using this information, especially to analyze conditions in the 1980s. This is one of the reasons why the present study concentrates on the 1990s.

rise steadily in the second half of the 1960s, showing peaks around the end of the 1960s, the end of the 1970s, mid-1980s, and then more uniform growth until 2005, when approximately 100 municipalities per year were enacting these regulations. Currently, some 2,500 municipalities have building codes.

Urban growth boundary. Brazil defines the urban growth boundary as the perimeter of the built-up area of a city or town. It is the local government's responsibility to establish this boundary. Outside the urban perimeter, in the rural area of the municipality, the local government is not expected to furnish certain basic public services such as water supply. While this restriction is not fully enforced today and several municipalities do provide services beyond the urban growth boundary, doing so was more difficult during the military regime, which considered urban development outside the boundary illegal.

Before 1978 approximately 400 municipalities had defined urban boundaries and currently at least 3,000 have done so. Between 1999 and 2001, just 22 municipalities defined urban boundaries. Unfortunately, the MUNIC changed the question on urban boundaries, making it impossible to define recent trends for this regulation. The 2001 MUNIC used a binary variable indicating whether the municipality had growth boundaries or not, but did not report the date of the law. In 2005 the MUNIC dropped the question altogether, probably because of the growing importance of the master plan in municipal management.

Zoning. The practice of establishing zoning regulations began in the early 1950s and grew uniformly until the late 1990s, when it dropped off considerably. The decline may reflect the fact that zoning regulations are often included in the master plan and thus does not require separate laws. Less than 200 municipalities had zoning before 1979; currently, more than 1,500 do.

Parceling. While urban growth boundaries define where development may or may not occur and building codes regulate construction standards, parceling regulations define how land within the urban boundary can be subdivided. It defines rules for site layout, land uses, service standards, and the characteristics of lots that can be converted to urban use.

Municipalities started to enact parceling regulations, or land subdivision standards, in the 1960s, marking a new way to address urban development. The number of municipalities with parceling regulations increased until the early 1980s, stayed almost the same for several years, and resumed growth in the 1990s, peaking by the end of the decade. In the first half of the 2000s, growth in the number of municipalities with parceling regulations declined substantially—probably because of the introduction of master plans.

Control Variables

Specification (10) does not describe the covariates or control variables. Determinants of residential choice in (6) are the natural candidates, although the fact that a variable is associated with residential choice does not necessarily mean it should be among the covariates.[19] In general, we like to control for variables that affect the outcome and the

[19] As argued before, it does not make sense to include price changes in the formal market as a control except if we assume that regulation induces informality for reasons unrelated to the price of formal housing. In other words, in this analysis we are interested in the indirect impact of regulation on prices that induce substitution among markets, i.e., the cross-price elasticity.

treatment, but are unaffected by the treatment, i.e. post-treatment responses. If such variables are not uniformly distributed across the groups, the estimated impact may not be connected to the treatment but rather to the difference in those covariates (by group). This type of control is called a confounder because the impact is confounded with the impacts of control variables that are not well balanced across groups.

If a variable affects the outcome but not the treatment, it is unclear whether we should control for it or not. For instance, in specification (10) assume that vectors *x* and *w* have no intersection. If $\gamma_{dw}=0$, controlling for *w* would make no difference in the results. At the same time, if $\gamma_{dw}\neq 0$, the only way to separate γ_d from γ_{dw} is to control for *w*. This is an "in-between" case, i.e., it will be difficult to identify if most variables are confounders, post-treatment responses, or something in between. Not controlling for a confounder would bias the estimator, but controlling for the post-treatment response would wipe out the impact. The case in between is less problematic and we may test if it is relevant through the interaction term.

In general, the goal is to include all confounders and to exclude post-treatment variables. We know that price changes are post-treatment, and regulations other than the ones under analysis are likely to be confounders. All other variables are more difficult to classify. Although we have theoretical reasons to classify the other urban regulations as (pure) confounders, it is not straightforward how to include these in the specification. In theory, we would like to compare municipalities that enacted all four regulations in the same period, with the control enacting the regulations in 2000–2005 and the treatment in 1991–1995.

One way to control for other regulations in the framework used here is to add dummies for the municipality in every other regulation. For instance, to estimate the impact of building codes, we would like to compare municipalities that are in the same group for urban growth boundaries, zoning, and parceling. But controlling for all possible combinations would clearly create too many controls. By using aggregated periods, we indirectly control of other regulations.

Since other regulations are likely to be confounders, we are basically concerned about how to control for them. In addition, there are two other types of variables at issue: variables at the city level, and specific characteristics of the untitled market relative to the rest of the housing market. These are called submarket variables. To select control variables, we first look at the determinants of housing supply and demand. If a variable is unevenly distributed across the groups, we would like to include it as a control; if it is well distributed across groups, controlling for it should make no difference in the estimates. For instance, the proportion of owner-occupied housing is well distributed, so controlling or not controlling for that variable does not change the analysis.

The variables used as controls are listed in table 4 and the rationale for their selection is explained below.

Table 4: Control Variables

Variable	Proxy
Rent	Value
Demographics	Population Density Migration Owner-Occupied Share
Productivity	Percent of University Graduates
Income	Gini Coefficient Poverty Share Income Share of Workers in Manufacturing
Fiscal	Property Tax Municipal Housing Expenditure Municipal Housing Programs
Political	Mayor from the Labor Party (PT) Votes for the PT Political Competition
Regional	North (NO) Northeast (NE) Center (CO) Southeast (SE) Southwest (SU)
Government	Submarket Variables
	Water Connection Electricity Sewage Trash–Home Collection Trash–Communal Collection Telephone
Developer	Water from Spring Septic Tank
Personal	Occupancy Rooms Relative Income Internal Piped Water No Bathroom
Location	No Car Percent Urban

Sources: Demographic Census 1991, 2000; IBGE MUNIC 1999, 2005; Brazilian Treasury Secretariat (STN) municipal revenues and expenditures for 1992 and 2000.

Demographics. It is reasonable to assume that municipalities with different demographic structures will behave differently, both in terms of their informal housing share and of their decision to enact urban regulations. Denser and more populated municipalities are expected to enact the regulations earlier. Denser municipalities are also expected to have a larger share of informal housing because they are more likely to experience land supply constraints.

Productivity growth. One important variable in any housing demand or supply estimation is productivity growth. If the city is growing because of productivity gains, housing prices may be increasing because everybody wants to move in. We would like to compare cities with similar productivity growth to isolate the impact of regulation. A host of research documents a strong positive correlation between the skill level of a city's population and its growth (Glaeser, Gyourko and Saks 2005b). Following those authors, we use the share of adults with university education as a control for productivity.

Income. Income can influence the share of informal settlements in several ways. First, given a similar income distribution, richer municipalities are expected to have less informality than poorer municipalities. The more unequal the income distribution, the more informality is likely. Among cities of the same size, municipalities with more poverty are likely to have more informal housing than those with less poverty. In addition, given the connection of many slums to the manufacturing sector,[20] we also control for the proportion of manufacturing workers in the labor force.

Fiscal variables A typical control in a housing supply or demand estimation is the property tax. High property taxes can make it more profitable to move to informal housing (assuming that there is no property tax in that market). In Brazil, however, low-income property is usually not taxed. Both property taxes and housing expenditures are fiscal variables that might confound our estimation but can also be a post-treatment effect. If the regulations push the middle class out of the city and the class that moves to informal settlements does not pay taxes, per capita revenue might fall in part because of the change in regulation.

Large public expenditures for housing are likely to imply less informality. If informal housing and public housing are alternative choices for the poor, higher spending on public housing might reduce informality. Since we cannot disaggregate municipal housing expenditures, it is possible that the proxy will mix the two different kinds of outlays. For instance, if the municipality spends most of its budget on slum upgrading, it may reduce informality (or irregularity) in the upgraded areas. But this might also signal that the government will eventually upgrade any slum, thus giving incentives to choose informal or irregular housing. The impact on informality is therefore ambiguous (Smolka 2003). Conversely, if municipal expenditures are concentrated on public housing, there is no reason to expect an increase in the size of the informal sector.

The main problem with these independent variables described so far is that they are highly correlated. The strategy is therefore to include them in the regressions one at a time in order to verify the impact of each.

Political factors. Since enacting regulation and imposing controls on informal settlements is a political decision, we want to control for political variables as well. First, we control for votes in the municipality for the Labor Party (*Partido dos Trabalhadores*—PT), which is committed to groups living in precarious conditions. In municipalities with more votes for the PT, it is possible that groups living in informal housing are more organized, which in turn makes these settlements a more attractive alternative than in municipalities where residents are not organized. In addition, groups living in informal houses might have more political

[20] There is anecdotal evidence that some slums were created by immigrants that moved to an area to work in a nearby plant.

pull in municipalities where the PT is in power. A final variable, political competition, is also included. This variable appears often in the political economy empirical literature and is defined as the difference in (the logs of) votes for the elected mayor and for the runner-up. In municipalities with less political competition, the argument goes, it could be easier to displace/evict people in illegal or irregular areas.

Among the control variables considered but not selected, we first looked at rents in the formal sector, which is an important characteristic of the real estate market. As tables A.1–A.8 show, it is clear that municipalities that enacted regulation in the 2000s and those that only did so after 2005, had larger untitled housing sectors and lower (average) rents in 1991. This relates to the fact that those municipalities are poorer. We did not include this variable in the specifications because (1) there is more variation in the share of untitled housing than in the rent level; and (2) income and rent are highly correlated and we already control for income.

Submarket Variables

We now turn to variables associated with the informal housing market in relation to the formal housing market according to the demand system model described earlier.[21] The three main players in any housing settlement are the buyer, the developer, and the government, and they are all likely to be affected by regulatory change. For instance, the government may be stricter about informal settlements after introducing a new regulation because its enactment may induce informality. The new regulation is also supposed to change the behavior of buyers, and developers may adapt to this new demand. In general, the variables that will be used at the submarket level attempt to reflect the behavior of those players.

The problem for defining whether a variable is a confounder or a post-treatment response is even more difficult at the submarket level. The main hypothesis of this study is that urban regulation induces households to move to informal settlements. The regulation might influence the marginal household. *Ceteris paribus*, the richest household that is indifferent between the informal and formal markets will prefer an informal house after enactment of the regulation. On average, income and direct investment in housing will change. However, there is an inelastic factor: serviced land. If the municipality provides public utilities for the new settlements, the change in residential choice would be easier and more households would be willing to locate in informal settlements. By the same token, if the developer is quick to supply on-site services, it would be easier to move to informal housing.

Using census data, it is possible to construct variables related to each player at the submarket level (see table 4). The proxy for the government reaction is piped water, sewerage infrastructure, electricity service, phone connection, and garbage collection. The proxy for the developer is an alternative sewerage system (such as septic tank) and piped water from a spring. To represent personal investment in the house, we included relative income, internal piped water, number of rooms, and the presence of at least one bathroom.

Each of the variables has shortcomings. The problem with using an electricity or water connection as a control variable is that those services are almost universally available in

[21] All variables related to income could be extracted at submarket level. However, estimating all variables both at the city and submarket levels is unnecessary and, in some cases, inappropriate because some are city characteristics (e.g., productivity).

Brazilian cities. The problem with the phone connection is that it is highly correlated with personal characteristics such as income. The problem with water connection from a spring is that it is correlated with natural conditions that may be out of the developer's control. As usual in using census data to analyze the real estate market in Brazil, the best variable to use is the sewerage connection, which is also consistent with multiple definitions of informality. In this case, connection to the general sewerage network represents a different player (government) than connection to a septic tank (developer).

Recall that, given our specification for the submarket variables (consistent with a random utility model), we are always looking at the difference between the informal (untitled) and formal (titled) sectors of the housing market. In some cities conditions in the informal market are more similar to those in the formal market; when this happens, we expect the informal submarket to grow more quickly independent of the regulation. Average differences at the submarket level have no clear pattern across the city-level variables. It is only when the distribution across submarkets is uneven that the share of untitled housing changes according to the regulation.

In general one would think that personal characteristics are certainly induced by the treatment, so controlling for them is unnecessary unless there is some form of segregation in the market. For instance, the middle class might never be able to move to the untitled segment of the housing market. The difference in relative income between the titled and untitled submarket may therefore control for different levels of segregation within cities, and that control is potentially important. If income in informal settlements is much lower than in formal settlements, it is likely that the market is highly segmented and that moving between the markets is more difficult. Thus, some specifications of the model will also control for differences in relative income.

Distribution of Control Variables by Groups of Municipalities

Municipalities that did not enact land use regulations until 2005 are smaller and less dense on average than any other group. Municipalities that enacted regulations between 2001 and 2005 are also smaller on average, but they are not necessarily the second less-dense group. Both groups also have the smallest proportion of workers in manufacturing and household heads with college degrees, as well as the highest shares of poverty. Municipalities that did not enact regulations are the poorest, while those that enacted regulation from 2001 to 2005 are the second poorest, although they do not seem to differ in terms of income distribution: the Gini coefficient is very stable across groups. The proportion of migrants to municipalities that did not enact regulations until 2000 is lower than for other groups, but the difference is not significant. There is virtually no difference in the share of owner-occupied houses.

As expected, the property tax in municipalities that did not enact the regulation until 2000 is considerably lower than the average of other groups, given the difference in income. However, the difference in their housing expenses is less pronounced than that in revenues. This is because of state and federal government grants. Regarding political variables, we cannot find any clear pattern by group. The proportion of votes for PT or the level of political competition is not uniform among groups, and the relative importance of these variables changes depending on the regulation. On average, then, municipalities that enacted regulations between 2001 and 2005 are poorer and less populated than municipalities that did

so earlier, but they are richer and larger than municipalities that did not enact the regulations until 2005.

Municipalities that did not enact the regulation until 2000 are unevenly distributed geographically. The municipalities in these groups are concentrated in the Northeast, a region associated with lower income. This result may indicate that northeastern municipalities are enacting urban regulations later than municipalities in other parts of the country. This is not an issue for the DD estimation, however, because timing differences control for regional differences.

Although municipalities that enacted regulation between 1996 and 2000 are usually poorer and less dense, the difference is small. Looking at the average across the groups, municipalities that enacted the regulation before 1996 are very similar in terms of observables in 1991 at the city level. This is interesting because differences in the impacts of each group are more likely to be associated with differences in the timing of enactment. In other words, the main differences observed are between municipalities that did and did not enact the regulation until 2005. This is likely the reason why including or excluding municipalities in this group does not affect the estimates.

Impacts of Regulation on Informality

To test the impact of land use and building regulation on untitled housing, variations of equation (10) are used to estimate the average treatment effect. The main concern with municipalities that enacted the regulation before 1986 is that (a) they may have recently changed the regulation,[22] and (b) the regulation may have been enacted so long ago that the impact on the flow of housing may have declined. Furthermore, a change in regulation may imply more or less increase in housing costs relative to the previous regulation. As a result, the group is not uniform.

The number of municipalities that did not enact any regulation until 2005 is very large. These are mainly small municipalities that follow a different path from other municipalities regardless of the regulations. Because these municipalities did not influence the size of the impact or the precision of the estimates on the groups of interest, they are excluded. Municipalities that adopted regulation before 1986 were not dropped from the analysis, but their coefficients are not shown for the reasons discussed above.[23]

Unconditional version. We start with an unconditional version of specification (10), that is, controlling just for group dummies, the year 2000 dummy, and the interaction of these two variables. The results for each regulation are presented in table 3 under specification (1). The unconditional average treatment effect using the DD estimator is generally consistent with the theory but not precisely estimated. The estimated impact for municipalities that enacted the regulation between 1991 and 1995 is positive in all specifications; for municipalities that enacted that regulation between 1986 and 1990, it is also positive with one exception (parceling). Most of the estimations, however, are not significant at 90 percent except for urban growth boundaries.

[22] It is possible to control only for the first enactment, not for changes in the legislation.
[23] All regression results are available upon request.

Table 5: Coefficients Explaining the Share of Informal Housing: Basic Controls
Dependent Variable: Relative Market Share of Untitled Housing

Regulation	Date of Enactment	(1) Coef	(1) Prob	(2) Coef	(2) Prob	(3) Coef	(3) Prob	(4) Coef	(4) Prob	(5) Coef	(5) Prob
Building Code	86-90	0.071	88%	0.070	88%	0.097	88%	0.102	88%	0.103	88%
	91-95	0.124	89%	0.123	89%	0.056	89%	0.057	89%	0.060	89%
		-		-		-		-		-	
	96-00	0.066	89%	0.067	89%	0.024	88%	0.020	88%	0.019	88%
Urban Growth Boundary	86-90	0.102	91%	0.102	91%	0.078	91%	0.082	91%	0.083	91%
	91-95	0.122	91%	0.121	91%	0.115	91%	0.118	91%	0.118	91%
	96-00	0.094	92%	0.093	92%	0.107	92%	0.110	92%	0.110	92%
Zoning	86-90	0.196	83%	0.194	83%	0.140	82%	0.139	83%	0.137	83%
	91-95	0.160	84%	0.158	84%	0.155	83%	0.155	83%	0.156	83%
	96-00	0.126	84%	0.123	84%	0.083	84%	0.082	84%	0.082	84%
Parceling	86-90	0.054	86%	0.055	86%	0.019	85%	0.019	86%	0.019	86%
	91-95	0.113	86%	0.114	86%	0.112	86%	0.110	86%	0.110	86%
		-		-		-		-		-	
	96-00	0.036	87%	0.035	87%	0.065	86%	0.066	86%	0.067	86%
Controls:											
Other regulations?		No		Yes		Yes		Yes		Yes	
Productivity?		No		No		Yes		Yes		Yes	
Demographics?		No		No		No		Yes		Yes*	

*The proportion of workers in manufacturing was added.

Sources: Demographic Census 1991, 2000; IBGE, MUNIC 1999, 2005; STN 1992, 2000.

The impact of urban growth boundaries is significant in all but one specification, and the impact has a positive sign for the expected groups, i.e., for municipalities that enacted regulation between 1986 and 1990 and between 1991 and 1995. The impact of building codes is estimated with at least 87 percent probability, and the sign is positive for the expected groups. The estimates for parceling and zoning are slightly less robust, at 84 percent and 81 percent. The impact of zoning is positive for the expected groups as well, and the impact of parceling is negative only for municipalities that enacted the regulation between 1986 and 1990.

In contrast, the signs for the impacts on municipalities enacting regulation between 1996 and 2000 are mixed. As expected, the impact for this group is always smaller than for municipalities that enacted regulation between 1991 and 1995 except for one specification. The magnitude of the impact on municipalities that enacted regulation in 1986–1990 is slightly smaller than that on municipalities that did so in 1991–1995. This result is also consistent because the relationship between those estimates depends both on the time of the regulation's enactment and the time it takes for its impact to decline.

Other regulations. Specification (2) adds controls for other regulations using the three-period definition (regulations enacted before 1986, between 1986 and 1995, and between 1996 and 1990). Adding controls for other regulations in general does not affect the impact of the

regulation under analysis. It is possible that we were already capturing the average impact of more than one regulation, so controlling for the others does not change the result.

Productivity. In specification (3), we add variables that are expected to influence housing market performance, starting with the proxy for productivity (the proportion of college graduates in the population). In cities where productivity is rising, demand for housing also increases. These municipalities are more likely to regulate and to attract more in-migrants, which may increase informality. But when comparing cities with the same productivity growth, the impact of the regulation decreases. This suggests that we were confounding the implementation of the regulation with productivity growth in the city.

Demographics. In specifications (4) and (5), we add demographic controls: first density and then the proportion of the labor force in manufacturing. When controlling for density, the impact increases slightly but is still below that in the unconditional specification (1). Controlling for the proportion of manufacturing workers makes no difference in the estimated impact of the regulation.

Table 6 drops the productivity variable and adds controls for income and fiscal proxies, which cannot be analyzed simultaneously because they are highly correlated.

Table 6: Coefficients Explaining the Share of Informal Housing: Basic + Fiscal Controls

Dependent Variable: Relative Market Share of Untitled Housing

Regulation	Date of Enactment	(6)		(7)		(8)		(9)		(10)	
		Coef	Prob	Coef	Prob	Coef	Prob	Coef	Prob	Coef	Prob
Building Code	86-90	0.146	89%	0.153	89%	0.117	88%	0.092	88%	0.117	88%
	91-95	0.168	90%	0.205	90%	0.086	88%	0.134	89%	0.086	88%
		-		-				-			
	96-00	0.033	90%	0.007	89%	0.016	88%	0.047	88%	0.016	88%
Urban Growth Boundary	86-90	0.134	92%	0.168	92%	0.093	90%	0.091	91%	0.093	90%
	91-95	0.130	91%	0.199	91%	0.083	90%	0.066	91%	0.083	90%
	96-00	0.114	93%	0.157	92%	0.088	91%	0.032	92%	0.088	91%
Zoning	86-90	0.176	85%	0.260	84%	0.173	82%	0.262	83%	0.173	82%
	91-95	0.092	86%	0.214	85%	0.098	83%	0.108	84%	0.098	83%
	96-00	0.059	86%	0.155	86%	0.068	83%	0.123	84%	0.068	83%
						-				-	
Parceling	86-90	0.079	87%	0.141	87%	0.043	85%	0.011	85%	0.043	85%
	91-95	0.073	88%	0.177	87%	0.017	86%	0.046	86%	0.017	86%
		-				-		-		-	
	96-00	0.064	88%	0.015	87%	0.102	86%	0.083	86%	0.102	86%
Controls:											
Other regulations?		Yes		Yes		Yes		Yes		Yes	
Productivity?		No		No		No		No		Yes	
Demographics?		Yes*		Yes*		Yes*		Yes*		Yes*	
Income?		Yes**		Yes***		No		No		No	
Fiscal?		No		No		Yes+		Yes++		Yes+	

* The proportion of workers in manufacturing was added.

** Income measured by the proportion of people below the poverty line.

*** Income measured by average income of residents.

+ Fiscal control measured by per capita property tax revenues.

[++] Fiscal control measured by per capita investment in housing.

Sources: Demographic Census 1991, 2000; IBGE, MUNIC 1999, 2005; STN 1992, 2000.

Income. When controlling for the share of the population below the poverty line, the impacts from enacting building codes and urban growth boundaries increase in magnitude. The impacts from zoning and parceling are less uniform, however, and depend on both the group and the specification. When using average per capita income in the city, the impact is greater for all regulations than in the unconditional specification (1). As mentioned above, poverty may be a consequence of the regulation (a post-treatment effect). Considering that the correlation between poverty and per capita income is 95 percent, the difference between the impact of controlling for income and for poverty might be related to this endogenous aspect of poverty, which may be a consequence of regulation.[24]

Fiscal variables. To check the impact of controlling for fiscal variables, we first tested controls for the property tax and then for municipal expenditures on housing. Controlling for either of those variables increases the impact of most regulations compared with specification (2).[25] The impact, however, is usually smaller than when we control just for income. Specification (10) controls for both the property tax and productivity, so the appropriate comparison is with specification (5).[26] The results are ambiguous, with some impacts higher and others lower. Per capita income thus has the most consistent response as a control for income-related variables.

Socio-political variables. Specifications (11) through (13) in table 7 add socio-political controls to the estimation. The base for comparison is specification (7) in table 5. Adding these controls generally decreases the impact of regulation, especially for municipalities enacting laws between 1985 and 1990. It is possible that the results were being confounded with the impacts of a set of policies implemented at the same time. That is, municipalities that were changing their land use regulation may have also changed their policies on informal housing. Specification (12) keeps the political variables and adds controls for government and developer reactions, generally increasing the magnitude of the impacts.

[24] By "endogenous aspect of poverty," we mean the possibility that the regulation may push the middle class out of the city. If this happens, the share of poverty in the city increases.

[25] We cannot make a comparison to specification (6) or (7), which both include income. The difference between specification (6) through (10) and specification (2) is just the income or fiscal variable.

[26] Specification (5) includes the same controls as specification (10) except for the fiscal variable, which is only present in specification (10).

Table 7: Coefficients Explaining the Share of Informal Housing: Basic + Fiscal + Socio-Political Controls

Regulation	Date of Enactment	(11)		(12)		(13)	
		Coef	Prob	Coef	Prob	Coef	Prob
Building Code	86-90	0.017	87%	0.022	87%	0.015	87%
	91-95	0.120	87%	0.125	87%	0.120	88%
		-		-		-	
	96-00	0.012	87%	0.021	87%	0.018	87%
Urban Growth Boundary	86-90	0.061	90%	0.061	90%	0.054	90%
	91-95	0.184	89%	0.169	90%	0.176	90%
	96-00	0.108	91%	0.107	91%	0.093	91%
Zoning	86-90	0.184	81%	0.216	82%	0.233	82%
	91-95	0.055	82%	0.102	82%	0.122	82%
	96-00	0.052	83%	0.070	83%	0.099	83%
Parceling	86-90	0.035	84%	0.062	84%	0.083	84%
	91-95	0.044	85%	0.089	85%	0.104	85%
		-		-			
	96-00	0.056	85%	0.021	85%	0.002	85%
Controls:							
Other regulations?		Yes		Yes		Yes	
Productivity?		No		No		No	
Demographics?		Yes		Yes		Yes	
Income?		Yes*		Yes*		Yes*	
Fiscal?		No		No		No	
Political?		Yes		Yes		Yes	
Government?		No		Yes		Yes	
Developer?		No		Yes		Yes	
Personal?		No		No		Yes	

* Income measured by average income of residents.

Sources: Demographic Census 1991, 2000; IBGE, MUNIC 1999, 2005; STN 1992, 2000.

Although the impact is not always estimated with great precision, the sign for municipalities that enacted the regulation between 1991 and 1995 is always positive. This result exists across many specifications and definitions.

Interpreting the Results

The regression results indicate that land use and building regulations enacted in the 1990s in some Brazilian cities had an 80 percent probability of slowing the decline in untitled housing. The evidence that a significant number of regulations were inappropriate is therefore quite strong.[27]

One explanation is the heterogeneity of Brazilian municipalities. To correct for problems of heteroskedasticity, the estimates use a weighted least squares approach. But heterogeneity in

[27] Defining what constitutes appropriate regulation is beyond the scope of this paper. Further research on the group of municipalities that experienced the largest declines in untitled housing and enacted at least one regulation in the 1990s may shed light on what types of regulation are appropriate.

this context is more than a technical problem—some municipalities probably enacted appropriate regulations. For instance, 9 out of 110 municipalities that enacted at least three urban land use regulations in the 1990s were among the cities with the largest declines in untitled housing. This group probably reduces the magnitude of the overall impact and increases the variance of the estimates.

Other aspects of the analysis potentially affect the magnitude and variance of the estimates. For instance, information exists about the year of enactment, but not whether the regulation was actually enforced. If some municipalities did not enforce the regulation, the magnitude would decrease and the variance would increase. Meanwhile, if municipalities enacting the regulation also implemented many other changes in housing policy (unobservables), the impact associated with a specific regulation could be overestimated. Municipalities did indeed enact "packages" of regulations. For this reason, we cannot sum up the effects of each regulation to estimate the aggregate impact of land use and building regulations on untitled housing.

Further evidence that reinforces the conclusion that land use and building regulations enacted during the late 1980s and early 1990s were inappropriate is that the results are consistent for groups defined by time of enactment. For example, the impact on untitled housing in municipalities that enacted the regulation between 1996 and 2000 is always lower than that in municipalities that enacted the same regulation between 1991 and 1995. This result is consistent because different years of enactment were pooled in the analysis.[28]

For municipalities that enacted the regulation from 1986 to 1990, the "centered" date of enactment is July 1, 1988, i.e. before the start of the period of observation which is September 1991. As a result, we cannot fully predict the impact of regulation for this group of municipalities (vis-à-vis the other groups) because the analysis does not cover the years between 1986 and 1991.

These arguments make it clear why the impact on informality in municipalities enacting the regulation in the first half of the 1990s should be larger than on those enacting it in the second half. They may also clarify why that it is impossible to predict how the impact on municipalities enacting the regulation in 1986–1990 compares with that on the other groups. If the regulation needs some time to be enforced, there might be no impact on informality in the first years after enactment. Indeed, if the regulation induced a run on licenses, the impact in the very first years might even be negative. It is therefore unsurprising to find some negative impacts among the late adopters.

As expected, the impact on municipalities enacting the regulation in the second half of the 1980s is larger than the impact on municipalities enacting the regulation in the second half of the 1990s for all specifications but one. For most regulations, however, the relation between the impacts on these two groups of cities changes with the specification. For the same regulation in some specifications, the impact on municipalities that were early adopters is larger in some cases while the impact on later adopters is larger in others. Parceling is an

[28] The group enacting a regulation during the first half of the decade was exposed to it for an average of five years more than the group enacting a regulation in the second half of the decade. If a panel by municipality by year were available, it would be possible to differentiate the groups by year of enactment. However, just two repeated cross-sections exist at the municipality level (1991 and 2000) and the best alternative is making five-year groups.

exception. The impact on municipalities enacting this regulation in 1991–1995 is always larger than that on municipalities enacting it in 1986–1990.

Another way to use the information on the three main groups of municipalities is to create a "synthetic estimator" for the average impact by year for each regulation. Using a continuous time approximation, it is possible to define an annual rate to make all magnitudes comparable:

$$
\begin{aligned}
&e^{2g_{1-2}} \equiv e^{\beta_5} \Rightarrow 2g_{1-2} \equiv \beta_5 \\
&e^{(2g_{1-2}+5g_{3-7})} \equiv e^{\beta_4} \Rightarrow 2g_{1-2} + 5g_{3-7} \equiv \beta_4 \Rightarrow 5g_{3-7} \equiv \beta_4 - \beta_5 \\
&e^{(4g_{3-7}+5g_{8-12})} \equiv e^{\beta_3} \Rightarrow 4g_{2-7} + 5g_{8-12} \equiv \beta_3 \Rightarrow 5g_{8-12} \equiv \frac{5\beta_3 - 4\beta_4 + 4\beta_5}{5}
\end{aligned}
\qquad (12)
$$

where $g_{t1\text{-}t2}$ represents annual change in the informal market in the treated municipalities that exceeds the decline in the informal market in the control group from year t1 to t2. In municipalities that enacted regulation in year 0, the share of untitled housing would decrease at an annual rate that is $g_{1\text{-}2}$ higher than the rate observed in the municipalities that did not enact the regulation. Using the same rationale, it is possible to find the annual rate for years 3 to 7 and from years 8 to 12 (keeping enactment at year 0). A good approximation for the cumulative difference between treated and control municipalities for the 9 years between the 1991 and 2000 demographic censuses would be:

$$\beta \equiv 2g_{1-2} + 5g_{3-7} + 2g_{8-12} \qquad (13)$$

where the cumulative rate is denominated as □. The above definition assumes that the relative share of untitled housing in a municipality that enacted the regulation in July 1991 would decrease the relative share of informality at a rate $g_{1\text{-}2}$ higher than in the municipalities that did not enact the regulation until June 30, 1993; at $g_{3\text{-}7}$ rate from July 1, 1993 until June 30, 1998; and at $g_{8\text{-}12}$ rate from July 1, 1998 until June 30, 2000. Substituting (12) on (13):

$$\beta = \frac{10\beta_3 + 17\beta_4 + 8\beta_5}{25}$$

It is therefore possible to calculate the accumulated impact using the estimates for □3, □4 and □5 from tables 5–7 after correcting for the continuous time assumption.[29] An alternative approach would be to center the groups so that one group, on average, enacted the regulation in September 1991. The three-period definition is an approximation for such a group. Municipalities that enacted the regulation between 1986 and 1995 are centered on January 1, 1991. Ignoring the first semester of the year, the estimated coefficient for this group is equivalent to □.

The complete set of estimates using the three-period definition is presented in table A.5. The key result is that the estimates are also consistent with the five-period definition, but slightly more robust. The accumulated rate estimated using the five-period coefficient is higher than

[29] The continuous time assumption implies that the nine-year rate is estimated by $e^{9(\beta/9)} - 1 = e^{\beta} - 1$

the estimates from the three-period definition, but the estimators follow the same pattern. This is further evidence that the estimates are internally consistent.

Table 8 shows the maximum, minimum, and average differences in performance for each regulation. For the five-period definition, the difference is based on the three estimators as discussed above. For the three-period definition, the rate is just the parameter estimated for the municipalities enacting the regulation between 1986 and 1995. Once again, this exercise cannot be considered a statistical test because it does not take the variance into account.

Table 8: Cumulative Impact of Urban Regulation on Untitled Housing

	Five-Period Definition*			Three-Period Definition**		
Regulation	Maximum	Minimum	Average	Maximum	Minimum	Average
Building Code	21.9%	7.2%	11.2%	19.9%	7.5%	10.9%
Urban Growth Boundary	28.8%	9.6%	17.3%	20.1%	7.6%	12.0%
Zoning	34.9%	13.6%	22.1%	26.4%	11.7%	17.3%
Parceling	19.9%	-3.8%	6.5%	17.5%	-1.0%	7.1%

* 1981-1985; 1986-90; 1991-1995; 1996-2000; 2000-2005.
** Before 1985; 1986-1995; 1996-2000.
Sources: Demographic Census 1991, 2000; IBGE, MUNIC 1999, 2005; STN 1992, 2000.

Zoning regulations generally have the largest impact, followed by urban growth boundaries and building codes. For these regulations, the impact is always positive and reasonably consistent over time. The impact of parceling is lower in almost all cases except in specification (13). In the five-period estimation, the main reason that the average difference is lower is connected to the low estimates (usually negative) for 1996–2000. This is also the primary reason why the impacts from enacting building codes are lower.

As is well known, zoning is a strategy for excluding the poor from certain parts of the city. In this case, however, the regulation probably pushed the poor to informal settlements (which in turn may have spatial consequences). Although the impact of parceling was lower, its cumulative impact is positive for all specifications except (10) (which corresponds to the minimum shown in table 6), and the coefficients are in line with estimates in most other specifications.

An alternative definition mentioned above is the two-period definition, where municipalities enacting regulation before 1991 are aggregated into one group and those enacting it between 1991 and 2000 are aggregated into another. This approach is appealing because the municipalities enacting the regulation in 1991–2000 are likely candidates for a treatment group.

The impacts using the two-period definition are presented in table A.6 and show mixed results. Most regulations show non-significant impact except for zoning. But although not always significant, most coefficients have a positive sign, as expected. The only exception is parceling which has a negative signs in some specifications; in case however, the significance of the coefficients is always below 29%. The problem of the two-period definition is (a) there

may be a delay in enforcing the regulation, and (b) the pools of municipalities are rather different in terms of length of exposure. Municipalities enacting the regulation in 1991–1995 were exposed to the regulation for seven years on average; municipalities enacting the regulation in 1996–2000 were exposed for an average of two years. The fact that the impacts vanish in the two-period definition is consistent with expectations, again reinforcing the results.

It is important to understand the meaning of the percentages in table 6. For instance, a 10 percent impact implies that the share of untitled housing would decrease 10 percent less in municipalities that enacted the particular regulation. In the 3,613 Brazilian municipalities analyzed here, the proportion of untitled housing declined from 7.6 percent in 1991 to 7.1 percent in 2000. Assuming that a municipality that did not enact regulation decreased its share of untitled housing at the average rate, an otherwise identical municipality that did enact the regulation would have a 7.2 percent share of untitled housing in 2000.

Although the difference may seem small, this phenomenon happens over time. Given the average reduction in untitled housing, a municipality starting with a 10-percent share of untitled housing and not enacting any new regulation would take 30 years to reduce that share by 2 percentage points. An otherwise identical municipality that enacted a regulation would take 32 years to make the same reduction. If the impact was 30 percent, the municipality that enacted regulation would need 44 years to reduce the share of untitled housing to 8 percent. This apparently tiny difference may thus imply up to 14 extra years to achieve a 2-percentage point reduction in untitled housing.

Implications for Policy and Directions for Future Research

This paper discusses the relationship between urban land use or building regulations and informality in Brazil. It presents evidence that the regulations had a significant impact on the growth of untitled housing in the 1990s. The heterogeneity of the municipalities suggests that some smart regulations, or combination of policies, neutralized the undesired impacts of regulation in some cases.

On average, however, urban regulation in Brazil has reinforced the double standard in the land market. If regulation increases housing costs in the formal market, the argument goes, some new households will be unable to afford a house in the formal market and will move to informal housing (increasing prices there as well). The cross-price elasticity between formal and informal housing determines how many households will make this shift. The increase in the likelihood of choosing the informal market will be reflected in a larger share of informal housing in the city.

The analysis uses a traditional residential choice model with random utility and aggregates the choices into a market share model by city. In the random utility model inspired by McFadden's seminal work, aggregate market shares can be used to measure the increase in the likelihood of choosing the informal market. A market with two alternatives—formal or informal—is assumed. The dual assumption is important to avoid the problem of cross-price

elasticity known as the Independence of Irrelevant Alternative (IIA)[30] when aggregating the shares.

The specification of differences between residential markets comes from the microeconomic model. The results make practical sense. Informal and formal markets are not independent; what happens in one affects the other. For instance, if informal housing conditions are very similar to formal housing conditions, substitution would be easy. There are many reasons why the markets could be similar or different. It is not possible to identify the cause of similarities or differences in the approach adopted here. However, the innovation of this paper is to control for this variable by analyzing the whole housing market rather than a piece of it.

The results confirm some general findings for the housing sector. As many other studies have documented, the supply of housing is more variable than the demand for housing. The main peculiarity in developing countries is that land use and building regulation induces more informality, not just higher housing prices. What is interesting is that the exclusionary aspect of regulation (excluding the poor from public services) may be similar in both developing and developed countries. But the exclusion mechanism in developing countries is more subtle. Since informal settlements may not have basic public utilities, there may be no tax redistribution from the rich to the poor within the municipality.

Tax redistribution is central to understanding why informality is a problem. The idea of establishing minimum standards for housing for the whole population is connected to the general goal of fairness and is the basis of any welfare program. Given that the regulation affects the whole market, general equilibrium impacts are expected. It is therefore difficult to set minimum standards for housing. It is certainly not enough just to regulate the standards unless everybody can afford the minimum. (But if everybody can afford the minimum standards, why impose the regulation in the first place?) What must be recognized is that those who cannot afford housing that meets minimum standards require subsidies (through tax redistribution or otherwise) in order to comply.

The definition of informal or irregular settlements is problematic, and the way it is defined has many consequences for policy. In this study, the analysis focuses on untitled housing primarily because this is the best measure available. In theory, an irregular house will have no title. In practice, however, there are many ways to obtain property titles in irregular settlements, especially after the 1988 Constitution. In any case, this is certainly a group worth studying because if the proportion of the population living in untitled housing is declining only slowly, some process may be pushing households to that residential choice.

This analysis relies mainly on demographic census data, which contain (self-declared) information about land titles in 1991 and 2000. Self-declaration may be problematic because people's beliefs about their tenure, and how these beliefs evolve over time, are unknown. The main advantage of using census data is that, given the coverage, it is extremely inexpensive compared with funding a comprehensive survey. In addition, the Brazilian census microdata cover the entire housing market (formal and informal) and provide proxies for the size of the

[30] The cross-price elasticity between two products in relation to a third product will be the same if they have the same shares. As discussed above, this would be problem if a multidimensional classification were assumed.

informal market. Similar information regarding informality can be found in other Latin American countries.[31]

By comparing untitled housing dynamics in municipalities that enacted land use or building regulations in the 1990s with similar municipalities that started to regulate only in the 2000s, it is possible to estimate the impact of regulation on the share of untitled housing. This analysis shows that enacting regulations in many Brazilian municipalities has in fact slowed the decline in informal housing. The estimation is precise enough and consistent with theory. Rethinking land use and building regulations may therefore be a way to deal with the spread of slums.

It is important to emphasize that this paper does not conclude that land use and building regulations should be abolished. Quite the opposite. But regulations should be reviewed for their impact on the real estate market. Land use and building regulations do have a role in making for a better urban environment, but they may also increase housing prices and informality. The main question is how to preserve the positive aspects of regulation and stimulate affordable house production.[32]

Extending the Analysis to the Household and Product Levels

It appears that several municipalities in the treatment group enacted packages of urban regulations. One solution would be to analyze the bundle of regulations enacted. An alternative would be to control for every regulation in every cohort, but there is not enough information to do so even using five-year intervals. In any case, it is clear that right after the first wave of democracy during the mid-1980s, a group of municipalities enacted a new set of urban regulations with significant impacts on housing markets.

To work with bundles of regulations, one approach would be using coincident cases. For example, a group of municipalities enacting three regulations in the first half of the 1990s could be compared with another group enacting the same three regulations in the early 2000s (ensuring that both groups did not enact additional regulations). The problem is that it is very likely that these groups will be too small to make statistical inferences at the city level.[33] However, if we move the analysis to the household level using microdata, it would be possible to have enough observations even with just two cities because the Brazilian census covers 10 percent of the population in municipalities with more than 10,000 inhabitants and 20 percent of the population in smaller municipalities. A municipality with 5,000 people would thus have a sample of 1,000 households.[34] If the lack of precision comes from the size of the sample, working at the household level may solve the problem.

[31] It is surprising that only a few studies have used census data for analyzing the informal housing market. We found only two examples: Pasternak (2001) and Cravino (2003).

[32] It is also relevant to consider the issue of the positive externalities generated by a regulation. For example, is the welfare gained by a density restriction larger than the welfare lost from extending sprawl? To what extent is the impact of regulation just red tape that raises housing prices without creating any positive externalities for the city?

[33] There are 110 municipalities that enacted three or more regulations in the first half of the 1990s. There are four possible combinations (A, B, C); (A, B, D); (B, C, D); and (A, B, C, D). If the enactment is distributed evenly, there would be 27 municipalities in each group.

[34] The down side of working at the household level is that we would see less variance because the data would be clustered by municipality and the standard error should be corrected for the cluster.

Another appeal of working at the household level is that it permits more detailed analysis of urban regulations in some cities.[35] Choosing particular cases based on the year of enactment and ensuring similar characteristics except for regulatory differences, it would be possible to study the regulations in each city (both in the treatment and in the control) and to quantify the impact more carefully. Pooling households in a group of cities with different sets of regulations, it may be possible to estimate the impact of each set, thereby increasing our understanding of the policies implemented in the late 1980s and early 1990s.

Another natural extension of this paper would be to analyze the real estate market in Brazil as a multi-product market. This would involve comparing housing products such as owner-occupied, rental, titled and untitled, and served or not served by public services.

Future Research: Understanding Implementation

Practitioners in Latin America have long recognized that inappropriate urban regulation increases the cost of formal housing and makes it more difficult to build affordable housing. Brazil attempted to remedy this problem by creating special zones of social interest or ZEIS in the early 1980s. The idea is reasonable in principle, but less so in practice. If the designated area is a slum, the ZEIS is used to regularize what is currently irregular. Defining ZEIS in vacant areas does not necessarily provide incentives to build affordable housing. The first difficulty is defining what can be built in those areas. Controlling the buyers is probably not feasible. Controlling density may backfire because there are many other high-density uses that might be more profitable than low-income housing. Furthermore, constraining areas just for low-income households would mean not taking advantage of the cross-subsidies that mixed land uses would generate.

Another problem with ZEIS is political. Declaring a slum a ZEIS may generate votes without obligating the municipality to invest in improving the area. Indeed, politicians are often willing to enact ZEIS but not always follow up by providing public services.

While there may be large political gains from defining a slum a ZEIS, the gain is not so clear in vacant areas. No clear group benefits from the regulation, so it is difficult to justify politically—especially because, if the ZEIS is successful, housing construction would done by the private sector. The alternative would be to allow a housing advocacy organization to develop the ZEIS, but such organizations are not usually interested in vacant areas.

Another option could be to authorize regulatory changes that benefit the affluent, such as zoning, but to charge for the changes and use the proceeds to subsidize construction of affordable housing. Such a policy is of course easy to define conceptually, but quite difficult to implement. A combination of taxes and subsidies—together with direct investment in affordable housing that does not repeat the traditional errors of public housing built at the urban fringe—could help minimize the problem of informality.

[35] An important next step would be to conduct field research to record not only the original date of enactment, but also to develop a typology of the regulations, document how regulations change over time, and ascertain how regulations are implemented once they are enacted. This information would add precision to the data provided by MUNIC.

Clearly there is a need for more research on how regulations are implemented in developing countries and what constitutes appropriate land use and building regulations in cities that have large informal housing markets. This paper sheds some light on which regulations should be reviewed, but research has a long way to go before it can lead to specific improvements in housing policies.

Research is also needed to measure the extent of the spatial impact of regulation beyond municipal boundaries. If regulation drives households to move to adjacent municipalities the impact measured in this study may very well be a lower bound.

Finally, we call attention for a methodological issue. Using the empirical approach of this paper it is not possible to apply a test of the joint significance of the all regulations at the same time because each regulation is estimated in different regressions. An alternative would be to estimate simultaneously the impact of all regulations and run an F test of the significance of the bundle. This is a priority task for further research.

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Appendix A: Descriptive Statistics and Alternative Model Specifications

Table A.1: Descriptive Statistics (1991) for Groups of Municipalities by Period of Enactment of Building Codes

Type	Variable	Enactment Period						
		Before 80	81-85	86-90	**91-95**	96-00	**01-05**	After 05
Dependent	Relative Share	-2.99	-2.85	-2.93	**-2.92**	-2.72	**-2.67**	-2.66
	Share	7.7%	9.4%	11.8%	**10.3%**	14.1%	**11.0%**	15.8%
Rent	Rent Value*	190.47	145.38	157.69	**174.86**	182.80	**142.66**	131.36
Demographic Structure	Population	62,031	38,887	33,605	**83,035**	47,043	**33,717**	15,570
	Density	223.59	179.28	96.08	**152.06**	236.59	**76.19**	57.41
	Migration %	3.0%	2.9%	3.5%	**3.0%**	3.0%	**2.9%**	2.6%
	Manufacture	25.3%	21.8%	22.6%	**19.8%**	21.8%	**17.3%**	16.6%
	Owner Occupied	66.9%	68.0%	67.6%	**67.9%**	68.7%	**71.2%**	71.3%
Productivity	% Graduates	0.5%	0.3%	0.3%	**0.4%**	0.4%	**0.3%**	0.3%
Income	Gini Coefficient	53.5%	53.8%	53.5%	**53.9%**	53.6%	**53.6%**	52.1%
	% Poverty	46.6%	54.0%	55.5%	**53.9%**	58.3%	**66.3%**	68.3%
	Income*	182.26	154.50	153.79	**160.01**	146.46	**119.68**	108.23
Fiscal	Property Tax	11.93	4.81	8.09	**5.13**	9.36	**5.23**	3.07
	Housing Expenditures	94.49	75.44	74.67	**85.05**	79.33	**68.31**	69.94
	Housing Programs	63.4%	63.0%	61.3%	**63.8%**	65.3%	**66.3%**	59.1%
Political	Mayor from PT	2.7%	0.6%	0.5%	**1.6%**	1.3%	**0.7%**	0.7%
	Votes for PT	4,682	1,240	1,252	**16,858**	2,767	**1,060**	326
	Political Competition	1.16	0.76	0.65	**0.78**	0.65	**0.49**	0.21
Regional	North (NO)	1.2%	3.3%	4.1%	**4.3%**	3.6%	**8.4%**	5.5%
	Northeast (NE)	12.9%	15.2%	15.3%	**18.1%**	31.1%	**46.9%**	44.0%
	Center (CO)	5.8%	14.7%	18.5%	**9.6%**	6.4%	**8.7%**	7.3%

	Southeast (SE)	58.2%	38.6%	30.2%	**41.1 %**	33.5%	**23.0 %**	31.9%
	Southwest (SU)	21.8%	28.3%	32.0%	**27.0 %**	25.5%	**13.1 %**	11.2%
Submarket Values (Untitled–titled)								
Government	Water Connection	-15.2%	-20.9%	-19.2%	**-17.3 %**	-19.9%	**-20.2 %**	-20.1%
	Electricity	-11.2%	-19.8%	-17.1%	**-16.9 %**	-17.2%	**-20.6 %**	-20.8%
	Sewage	-13.7%	-8.9%	-6.1%	**-8.4%**	-11.1%	**-5.8%**	-6.5%
	Trash–House Collection	-18.0%	-21.1%	-17.9%	**-16.1 %**	-20.6%	**-15.3 %**	-13.3%
	Trash–Community Collection	-0.9%	-0.8%	-0.8%	**-1.6%**	-1.2%	**-3.1%**	-2.6%
	No Phone	12.6%	10.2%	9.8%	**10.4 %**	11.0%	**6.9%**	6.2%
Developer	Water from Spring	7.6%	13.8%	12.6%	**10.9 %**	11.6%	**9.3%**	8.7%
	Septic Tank Sewage	-5.3%	-5.1%	-6.2%	**-5.0%**	-3.3%	**-4.8%**	-4.0%
Personal	Occupancy	-0.12	-0.12	-0.10	**-0.16**	-0.04	**-0.02**	-0.03
	Rooms	-1.62	-1.59	-1.45	**-1.44**	-1.47	**-1.28**	-1.23
	Relative Income*	-458.56	-404.27	-375.06	**-420.99**	-374.51	**-313.06**	-256.33
	Internal Piping	-18.2%	-21.8%	-20.6%	**-18.8 %**	-20.1%	**-20.3 %**	-20.0%
	No Bathroom	20.7%	26.5%	24.0%	**21.5 %**	23.2%	**23.6 %**	23.9%
Location	No Cars	15.7%	14.0%	12.1%	**12.8 %**	11.2%	**9.2%**	8.5%
	% Urban	-11.9%	-19.6%	-17.8%	**-16.4 %**	-18.0%	**-19.9 %**	-22.5%

* Rent Value and Income in December 2000 Reais, converted using the IGP-m.

Sources: Demographic Census 1991, 2000; IBGE, MUNIC 1999, 2005; STN 1992, 2000.

Table A.2: Descriptive Statistics (1991) for Groups of Municipalities by Period of Enactment of Urban Growth Boundaries

Type	Variable	Enactment Period						
		Before 80	81-85	86-90	**91-95**	96-00	**01-05**	After 05
Dependent	Relative Share	-2.71	-2.85	-2.80	**-3.15**	-3.00	**-2.63**	-2.35
	Share	12.1%	9.4%	10.3%	**8.0%**	8.9%	**14.9%**	23.1%
Rent	Rent Value*	150.18	169.46	151.12	**163.10**	156.34	**139.15**	126.45
Demographic Structure	Population	35,855	72,386	25,568	**39,295**	29,439	**23,901**	23,397
	Density	179.77	128.58	58.30	**112.26**	95.70	**110.22**	65.01
	Migration %	2.8%	3.3%	2.8%	**3.5%**	3.1%	**2.6%**	2.1%
	Manufacture	21.2%	21.3%	18.4%	**21.3%**	20.6%	**17.0%**	14.9%
	Owner Occupied	68.5%	67.4%	69.5%	**66.0%**	68.1%	**70.6%**	75.4%
Productivity	% Graduates	0.4%	0.3%	0.3%	**0.5%**	0.4%	**0.3%**	0.2%
Income	Gini Coefficient	53.3%	53.7%	53.0%	**53.1%**	52.7%	**52.9%**	51.8%
	% Poverty	58.2%	56.9%	60.5%	**48.2%**	54.3%	**69.5%**	78.1%
	Income*	143.69	147.01	132.08	**174.30**	153.99	**106.42**	80.86
Fiscal	Property Tax	7.83	5.88	3.35	**8.19**	7.73	**3.37**	1.48
	Housing Expenditures	80.02	85.29	66.99	**98.68**	79.57	**64.67**	63.95
	Housing Programs	63.6%	70.2%	58.4%	**63.5%**	59.0%	**64.0%**	57.8%
Political	Mayor from PT	1.1%	1.3%	0.2%	**1.1%**	0.8%	**1.4%**	1.0%
	Votes for PT	3,073	18,290	950	**1,521**	1,422	**649**	1,923
	Political Competition	0.58	0.81	0.45	**0.62**	0.51	**0.41**	0.18
Regional	North (NO)	1.7%	2.9%	3.2%	**3.8%**	2.3%	**5.7%**	10.9%
	Northeast (NE)	25.4%	18.8%	30.2%	**11.8%**	20.9%	**47.4%**	63.9%
	Center (CO)	5.6%	16.8%	11.4%	**11.8%**	7.2%	**7.0%**	7.0%
	Southeast (SE)	45.7%	42.8%	30.8%	**47.1%**	46.3%	**30.3%**	14.1%
	Southwest (SU)	21.5%	18.8%	24.4%	**25.4%**	23.2%	**9.6%**	4.1%
Submarket Values (Untitled–titled)								
Government	Water Connection	-19.0%	-20.7%	-20.3%	**-16.9%**	-17.0%	**-22.7%**	-19.3%
	Electricity	-15.7%	-20.2%	-20.0%	**-14.9%**	-15.7%	**-21.6%**	-23.5%
	Sewage	-9.8%	-11.1%	-7.5%	**-10.4%**	-9.7%	**-6.9%**	-2.7%

	Trash–House Collection	-16.9%	-18.6%	-17.0%	**-18.1%**	-15.8%	**-16.2%**	-9.8%
	Trash–Community Collection	-1.9%	-1.5%	-1.6%	**-1.0%**	-1.4%	**-3.2%**	-3.1%
	No Phone	9.3%	9.5%	8.0%	**11.8%**	10.8%	**6.4%**	3.3%
Developer	Water from Spring	10.3%	11.1%	10.7%	**10.6%**	9.6%	**9.3%**	7.3%
	Septic Tank Sewage	-4.8%	-4.3%	-5.8%	**-2.5%**	-4.5%	**-4.4%**	-4.1%
Personal	Occupancy	-0.04	-0.07	-0.06	**-0.10**	-0.05	**-0.04**	-0.07
	Rooms	-1.44	-1.46	-1.37	**-1.53**	-1.50	**-1.26**	-1.04
	Relative Income*	-370.02	-356.31	-338.84	**-447.47**	-372.86	**-270.23**	-178.22
	Internal Piping	-20.3%	-21.9%	-20.4%	**-19.1%**	-19.9%	**-20.4%**	-18.7%
	No Bathroom	22.4%	23.6%	25.7%	**20.5%**	22.5%	**25.5%**	23.5%
Location	No Cars	12.3%	12.9%	10.8%	**14.4%**	12.5%	**8.1%**	5.4%
	% Urban	-17.1%	-19.9%	-21.5%	**-16.3%**	-15.9%	**-25.2%**	-22.6%

* Rent Value and Income in December 2000 Reais, converted using the IGP-m.
Sources: Demographic Census 1991, 2000; IBGE, MUNIC 1999, 2005; STN 1992, 2000.

Table A.3: Descriptive Statistics (1991) for Groups by Period of Enactment of Zoning

Type	Variable	Enactment Period						
		Before 80	81-85	86-90	**91-95**	96-00	**01-05**	After 05
Dependent	Relative Share	-2.99	-2.87	-2.86	**-3.01**	-2.91	**-2.59**	-2.68
	Share	7.6%	11.9%	9.1%	**9.4%**	10.0%	**15.2%**	14.9%
Rent	Rent Value*	207.59	180.30	174.20	**180.92**	173.56	**161.71**	133.30
Demographic Structure	Population	166,305	50,551	63,995	**50,017**	61,587	**21,204**	14,599
	Density	434.90	352.15	179.55	**180.91**	269.75	**53.2**	42.25
	Migration %	3.1%	3.5%	3.4%	**3.4%**	3.3%	**2.8%**	2.6%
	Manufacturing	25.6%	27.1%	25.8%	**23.4%**	25.2%	**18.1%**	16.3%
	Owner Occupied	67.5%	67.9%	67.8%	**67.1%**	67.1%	**71.6%**	70.9%
Productivity	% Graduates	0.5%	0.4%	0.5%	**0.5%**	0.4%	**0.3%**	0.2%
Income	Gini Coefficient	54.0%	53.2%	54.2%	**53.8%**	52.6%	**53.0%**	52.5%
	% Poverty	44.2%	47.2%	49.7%	**45.2%**	49.9%	**64.1%**	68.4%
	Income*	197.70	186.08	176.48	**187.84**	172.14	**124.02**	107.86
Fiscal	Property Tax	16.06	12.62	10.92	**9.76**	16.93	**3.99**	2.06
	Housing Expenditures	99.08	92.39	85.61	**85.77**	94.56	**70.60**	68.36
	Housing Programs.	70.4%	66.1%	65.6%	**65.3%**	64.7%	**68.3%**	59.2%
Political	Mayor from PT	3.0%	2.0%	0.7%	**0.5%**	2.0%	**0.0%**	0.8%
	Votes for PT	25,781	2,587	2,176	**1,200**	4,733	**246**	235
	Political Competition	1.32	1.04	0.83	**0.99**	0.83	**0.38**	0.24
Regional	North (NO)	2.8%	2.7%	3.1%	**2.7%**	1.9%	**4.2%**	5.9%
	Northeast (NE)	10.1%	18.8%	12.3%	**9.6%**	19.8%	**38.3%**	42.9%
	Center (CO)	3.9%	8.9%	9.2%	**10.5%**	6.2%	**7.5%**	8.9%
	Southeast (SE)	55.9%	41.1%	26.4%	**39.3%**	36.8%	**30.8%**	32.8%

	Southwest (SU)	27.4%	28.6 %	49.1%	**37.9 %**	35.3 %	**19.2 %**	9.6%
Submarket Values (Untitled–titled)								
Government	Water Connection	-16.3%	-17.3 %	-17.5%	**-14.6 %**	-15.3 %	**-20.2 %**	-20.6%
	Electricity	-10.3%	-14.6 %	-15.3%	**-12.1 %**	-14.0 %	**-19.6 %**	-21.2%
	Sewage	-13.5%	-12.3 %	-7.8%	**-7.5 %**	-9.8%	**-8.4 %**	-6.8%
	Trash–House Collection	-19.5%	-17.8 %	-19.3%	**-17.7 %**	-19.9 %	**-14.0 %**	-14.0%
	Trash–Community Collection	0.8%	-0.5%	-0.4%	**-1.3 %**	-0.5%	**-3.1 %**	-2.7%
	No Phone	14.5%	11.7 %	12.6%	**12.3 %**	12.4 %	**8.0 %**	6.2%
Developer	Water from Spring	8.9%	8.8%	11.7%	**10.9 %**	8.8%	**10.3 %**	9.4%
	Septic Tank Sewage	-6.9%	-6.8%	-8.7%	**-5.9 %**	-6.0%	**-5.7 %**	-3.5%
Personal	Occupancy	-0.03	-0.16	-0.10	**-0.16**	-0.06	**-0.03**	-0.05
	Rooms	-1.66	-1.60	-1.67	**-1.58**	-1.62	**-1.37**	-1.23
	Relative Income*	-520.06	-453.56	-463.54	**-442.73**	-463.01	**-282.44**	-262.17
	Internal Piping	-19.8%	-21.9 %	-21.8%	**-18.3 %**	-20.3 %	**-20.5 %**	-19.7%
	No Bathroom	21.2%	25.6 %	24.8%	**22.6 %**	22.2 %	**25.3 %**	23.6%
Location	No Cars	16.8%	15.5 %	15.3%	**15.9 %**	14.7 %	**7.3 %**	8.4%
	% Urban	-11.5%	-13.3 %	-15.7%	**-12.4 %**	-11.8 %	**-20.7 %**	-22.8%

* Rent Value and Income in December 2000 Reais, converted using the IGP-m.
Sources: Demographic Census 1991, 2000; IBGE, MUNIC 1999, 2005; STN 1992, 2000.

Table A.4: Descriptive Statistics (1991) for Groups by Period of Enactment of Parceling

Type	Variable	Enactment Period						
		Before 80	81-85	86-90	**91-95**	96-00	**01-05**	After 05
Dependent	Relative Share	-2.96	-2.96	-2.88	**-3.04**	-2.97	**-2.71**	-2.62
	Share	8.4%	9.3%	9.0%	**9.9%**	9.1%	**12.5 %**	15.9%
Rent	Rent Value*	212.64	184.80	193.49	**188.13**	167.81	**127.55**	125.74
Demographic Structure	Population	105,234	99,846	33,418	**46,220**	48,755	**17,028**	13,585
	Density	360.35	300.10	126.77	**121.58**	185.78	**39.30**	45.16
	Migration %	3.1%	3.2%	3.4%	**3.4%**	3.5%	**2.7%**	2.5%
	Manufacturing	27.6%	26.8%	24.5%	**23.2 %**	22.6%	**15.6 %**	15.6%
	Owner Occupied	68.1%	67.6%	68.7%	**67.5 %**	66.6%	**69.5 %**	71.4%
Productivity	% Graduates	0.5%	0.4%	0.4%	**0.5%**	0.4%	**0.2%**	0.2%
Income	Gini Coefficient	53.5%	53.3%	53.7%	**53.9 %**	52.9%	**53.1 %**	52.4%
	% Poverty	44.6%	46.8%	50.4%	**47.1 %**	50.3%	**66.3 %**	70.6%
	Income*	192.55	184.23	167.97	**183.54**	171.13	**116.99**	100.63
Fiscal	Property Tax	13.53	12.30	7.24	**10.99**	11.84	**2.50**	1.91
	Housing Expenditures	90.93	92.87	78.22	**87.96**	91.76	**66.77**	67.34
	Housing Programs	69.7%	62.2%	63.6%	**66.7 %**	65.9%	**62.6 %**	58.7%
Political	Mayor from PT	2.0%	1.7%	1.1%	**1.3%**	1.5%	**0.6%**	0.7%
	Votes for PT	9,925	13,965	1,397	**1,524**	3,859	**146**	264
	Political Competition	1.40	0.92	0.48	**1.00**	0.74	**0.44**	0.17
Regional	North (NO)	2.7%	2.1%	3.3%	**3.2%**	2.1%	**2.9%**	6.3%
	Northeast (NE)	12.1%	12.2%	8.6%	**9.1%**	17.6%	**45.0 %**	47.0%
	Center (CO)	6.8%	9.0%	11.0%	**12.3 %**	8.6%	**5.8%**	8.2%
	Southeast (SE)	53.4%	45.2%	28.7%	**41.3 %**	39.7%	**29.8 %**	30.9%
	Southwest (SU)	25.0%	31.4%	48.3%	**34.1 %**	32.1%	**16.4 %**	7.6%

Submarket Values (Untitled–titled)								
Government	Water Connection	-19.0%	-17.9%	-17.2%	**-15.5%**	-16.1%	**-19.8%**	-20.6%
	Electricity	-11.2%	-13.1%	-14.4%	**-13.7%**	-14.2%	**-20.7%**	-22.1%
	Sewage	-13.7%	-11.3%	-7.3%	**-9.3%**	-9.2%	**-8.0%**	-6.2%
	Trash House Collection	-20.7%	-18.3%	-18.2%	**-18.6%**	-18.5%	**-13.3%**	-13.5%
	Trash Community Collect	0.3%	-0.1%	-0.8%	**-1.1%**	-0.9%	**-3.2%**	-2.9%
	No Phone	13.6%	11.5%	11.4%	**11.4%**	12.2%	**8.0%**	5.7%
Developer	Water from Spring	12.3%	11.5%	11.6%	**11.3%**	9.5%	**8.6%**	8.7%
	Septic Tank Sewage	-6.8%	-6.4%	-7.2%	**-5.0%**	-5.2%	**-4.7%**	-3.5%
Personal	Occupancy	-0.05	-0.15	-0.11	**-0.11**	-0.13	**-0.06**	-0.03
	Rooms	-1.55	-1.59	-1.54	**-1.60**	-1.60	**-1.39**	-1.20
	Relative Income*	-481.42	-439.92	-440.88	**-456.74**	-434.75	**-304.94**	-243.40
	Internal Piping	-18.2%	-20.3%	-20.1%	**-19.1%**	-19.5%	**-22.9%**	-19.9%
	No Bathroom	20.5%	22.8%	22.3%	**22.1%**	22.2%	**25.8%**	24.1%
Location	No Cars	15.4%	14.3%	15.3%	**13.8%**	14.4%	**9.0%**	7.9%
	% Urban	-14.7%	-17.3%	-14.9%	**-12.2%**	-13.8%	**-19.3%**	-23.3%

* Rent Value and Income in December 2000 Reais, converted using the IGP-m.
Sources: Demographic Census 1991, 2000; IBGE, MUNIC 1999, 2005; STN 1992, 2000.

Table A.5: Impact of Urban Regulation on Untitled Tenure: Three-Period Definition
Dependent Variable: Relative Market Share

Regulation	Regulation Enactment	(1)		(2)		(3)		(4)		(5)	
		Coef	Prob	Coef	Prob	Coef	Prob	Coef	Prob	Coef	Prob
Building Code	before 85	0.059	90%	0.060	90%	0.026	90%	0.026	90%	0.026	90%
	86-95	0.100	90%	0.100	90%	0.074	90%	0.076	90%	0.078	90%
	96-00	-0.066	89%	-0.067	89%	-0.024	88%	-0.021	88%	-0.020	88%
Urban Growth Boundary	before 85	0.048	92%	0.049	92%	0.010	92%	0.010	92%	0.011	92%
	86-95	0.110	92%	0.110	92%	0.098	92%	0.101	92%	0.102	92%
	96-00	0.094	92%	0.093	92%	0.108	92%	0.110	92%	0.111	92%
Zoning	before 85	0.218	85%	0.216	85%	0.191	84%	0.187	84%	0.186	84%
	86-95	0.176	85%	0.174	85%	0.146	84%	0.146	84%	0.146	84%
	96-00	0.126	84%	0.123	84%	0.083	84%	0.082	84%	0.081	84%
Parceling	before 85	0.092	88%	0.092	88%	0.047	87%	0.041	87%	0.039	87%
	86-95	0.087	88%	0.087	88%	0.070	87%	0.069	87%	0.069	87%
	96-00	-0.036	87%	-0.035	87%	-0.065	86%	-0.066	86%	-0.067	86%
Controls:											
Other laws?		No		Yes		Yes		Yes		Yes	
Productivity?		No		No		Yes		Yes		Yes	
Demographics?		No		No		No		Yes		Yes*	

* The proportion of workers in manufacturing was added.

Table A.5 (continued)

Regulation	Regulation Enactment	(6)		(7)		(8)		(9)		(10)	
		Coef	Prob	Coef	Prob	Coef	Prob	Coef	Prob	Coef	Prob
Building Code	Before 85	0.095	91%	0.138	91%	0.092	90%	0.063	90%	0.056	90%
	86-95	0.158	91%	0.181	91%	0.130	90%	0.115	90%	0.100	90%
	96-00	-0.033	90%	-0.007	89%	-0.020	88%	-0.047	88%	-0.016	88%
Urban Growth Boundary	Before 85	0.073	93%	0.113	93%	0.062	92%	0.021	92%	0.001	92%
	86-95	0.132	93%	0.183	93%	0.107	92%	0.073	92%	0.087	92%
	96-00	0.114	93%	0.158	92%	0.082	92%	0.030	92%	0.088	91%
Zoning	Before 85	0.123	86%	0.264	86%	0.241	84%	0.204	84%	0.223	84%
	86-95	0.128	87%	0.234	86%	0.187	85%	0.173	85%	0.128	84%
	96-00	0.059	86%	0.155	85%	0.124	84%	0.123	84%	0.068	83%
Parceling	Before 85	0.048	89%	0.173	88%	0.059	87%	0.025	87%	-0.010	87%
	86-95	0.076	89%	0.161	88%	0.035	87%	0.029	87%	-0.010	87%
	96-00	-0.065	88%	-0.015	87%	-0.061	86%	-0.083	86%	-0.102	86%
Controls:											
Other laws?		Yes		Yes		Yes		Yes		Yes	
Productivity?		No		No		No		No		Yes	
Demographics?		Yes*		Yes*		Yes*		Yes*		Yes*	
Income?		Yes**		Yes***		No		No		No	
Fiscal?		No		No		Yes+		Yes++		Yes+	

* The proportion of workers in manufacturing was added.

** Income measured by the proportion of people below poverty line.

*** Income measured by average income of residents.

+ Fiscal control measured by per capita property tax revenues.

++ Fiscal control measured by per capita investment in housing.

Table A.5 (continued)

Regulation	Regulation Enactment	(11)		(12)		(13)	
		Coef	Prob	Coef	Prob	Coef	Prob
Building Code	Before 85	0.015	89%	0.020	89%	0.020	89%
	86-95	0.074	89%	0.079	89%	0.073	89%
		-		-		-	
	96-00	0.013	87%	0.022	87%	0.018	87%
		-		-		-	
Urban Growth Boundary	Before 85	0.042	91%	0.034	91%	0.041	91%
	86-95	0.127	91%	0.119	91%	0.118	91%
	96-00	0.110	91%	0.109	91%	0.095	91%
Zoning	Before 85	0.074	83%	0.102	83%	0.112	83%
	86-95	0.111	83%	0.151	84%	0.170	84%
	96-00	0.052	83%	0.070	83%	0.099	83%
Parceling	Before 85	0.064	86%	0.095	86%	0.104	86%
	86-95	0.039	86%	0.076	86%	0.094	86%
		-		-			
	96-00	0.057	85%	0.021	85%	0.002	85%
Controls:							
Other laws?		Yes		Yes		Yes	
Productivity?		No		No		No	
Demographics?		Yes		Yes		Yes	
Income?		Yes***		Yes***		Yes***	
Fiscal?		No		No		No	
Political?		Yes		Yes		Yes	
Government?		No		Yes		Yes	
Developer?		No		Yes		Yes	
Personal?		No		No		Yes	
*** Income measured by average income of residents.							

Sources: Demographic Census 1991, 2000; IBGE, MUNIC 1999, 2005; STN 1992, 2000.

Table A.6: Impact of Urban Regulations on Untitled Tenure: Two-Period Definition
Dependent Variable: Relative Market Share

Regulation	Regulation Enactment	(1) Coef	(1) Prob	(2) Coef	(2) Prob	(3) Coef	(3) Prob	(4) Coef	(4) Prob	(5) Coef	(5) Prob
Building Code	Before 91	0.109	56%	0.110	57%	0.106	57%	0.104	56%	0.103	56%
	91-00	0.046	27%	0.047	28%	0.136	72%	0.137	72%	0.134	71%
Growth Boundary	Before 91	0.063	52%	0.066	55%	0.058	47%	0.058	47%	0.058	47%
	91-00	0.035	29%	0.037	31%	0.033	26%	0.033	26%	0.034	27%
Zoning	Before 91	0.069	67%	0.070	68%	0.035	36%	0.037	38%	0.038	39%
	91-00	0.105	85%	0.105	85%	0.112	86%	0.114	87%	0.114	87%
Parceling	Before 91	-0.038	14%	-0.038	15%	-0.031	12%	-0.027	10%	-0.025	10%
	91-00	-0.018	11%	-0.020	12%	-0.010	6%	-0.008	5%	-0.002	1%
Controls:											
Other laws?		No		Yes		Yes		Yes		Yes	
Productivity?		No		No		Yes		Yes		Yes	
Demographics?		No		No		No		Yes		Yes*	

* The proportion of workers in manufacturing was added.

Table A.6 (continued)

Regulation	Regulation Enactment	(6) Coef	(6) Prob	(7) Coef	(7) Prob	(8) Coef	(8) Prob	(9) Coef	(9) Prob	(10) Coef	(10) Prob
Building Code	Before 91	0.085	49%	0.189	85%	0.089	46%	0.040	22%	0.054	31%
	91-00	0.001	1%	0.099	58%	-0.003	2%	-0.057	33%	0.078	45%
Growth Boundary	Before 91	0.110	82%	0.140	90%	0.082	63%	0.062	50%	0.073	56%
	91-00	0.074	60%	0.103	75%	0.060	46%	0.042	34%	0.056	42%
Zoning	Before 91	0.098	86%	0.136	96%	0.078	71%	0.048	49%	0.034	34%
	91-00	0.121	93%	0.176	99%	0.093	79%	0.044	45%	0.084	72%
Parceling	Before 91	-0.020	8%	-0.003	1%	-0.014	5%	0.025	10%	-0.009	4%
	91-00	-0.005	3%	0.013	8%	0.036	21%	0.000	0%	0.049	29%
Controls:											

Other laws?	Yes	Yes	Yes	Yes	Yes
Productivity?	No	No	No	No	Yes
Demographics?	Yes*	Yes*	Yes*	Yes*	Yes*
Income?	Yes**	Yes***	No	No	No
Fiscal?	No	No	Yes+	Yes++	Yes+

* The proportion of workers in manufacturing was added.
** Income measured by the proportion of people below poverty line.
*** Income measured by average income of residents.
+ Fiscal control measured by per capita property tax revenues/
++Fiscal control measured by per capita investment in housing.

Table A.6 (continued)

Regulation	Regulation Enactment	(11)		(12)		(13)	
		Coef	Prob	Coef	Prob	Coef	Prob
Building Code	Before 91	0.150	66%	0.150	67%	0.148	67%
	91-00	0.132	64%	0.135	66%	0.162	75%
Growth Boundary	Before 91	0.019	15%	0.025	19%	0.024	19%
	91-00	0.068	47%	0.067	47%	0.067	47%
Zoning	Before 91	0.000	0%	0.004	4%	-0.003	2%
	91-00	0.136	89%	0.129	87%	0.124	86%
Parceling	Before 91	0.153	52%	0.126	44%	0.084	31%
	91-00	0.051	28%	0.048	26%	0.016	9%
Controls:							
Other laws?		Yes		Yes		Yes	
Productivity?		No		No		No	
Demographics?		Yes		Yes		Yes	
Income?		Yes***		Yes***		Yes***	
Fiscal?		No		No		No	
Political?		Yes		Yes		Yes	
Government?		No		Yes		Yes	
Developer?		No		Yes		Yes	
Personal?		No		No		Yes	

*** Income measured by average income of residents.

Sources: Demographic Census 1991, 2000; IBGE, MUNIC 1999, 2005; STN 1992, 2000.

Appendix B. Background on Urban Land Use and Building Regulation in Brazil

The first attempt to regulate the housing market in Brazil dates back to 1937. The main goal of Law 58/37 was to protect buyers from the very loose definition of private property in the Civil Code (Law 1088), which allowed the buyer or seller of a parcel or house to cancel the deal any time before the acquisition was registered in the notary. According to the 1937 law, the landowner had to get approval for a subdivision from city hall and to register it in the notary before announcing the sale of lots.

Law 58/37 did not, however, establish any penalty for failing to comply with the regulation. As a result, most developments continued to operate without city approval (Osório 2003). The buyer's protection was reinforced in 1949 with Law 649, which recognized private contracts between buyer and seller as legally binding. As such, the buyer was able to get the land title in the legal system (*adjudicação compulsória*) using just the private contract (if registered in the housing notary), even if the seller refused to sign the title transfer. In 1967 the new Constitution increased the power of the municipalities, defined different kinds of subdivisions, and introduced the concept of land property rights.

For many years, each new federal law reinforced the autonomy of the municipalities to enact urban regulations, but no comprehensive guidelines were issued until 1979. The 6766/79 law, which remains very influential today, explicitly defined the authority of the municipalities. It also set some urban development standards such as a minimum lot size, but left it up to municipalities to define most regulations, including the maximum floor-area-ratio (FAR), urban growth boundaries, parceling, zoning, and building standards. In addition, municipalities were given the authority to approve a master plan (*plano diretor*) governing many aspects of urban development, some of which were not covered by the urban regulations.[36] A major change in terms of enforcement was that failure to comply with the regulations would be a punishable crime.

The 1979 legislation significantly increased the requirements for registering a house. The law established the standards that a subdivision must meet as a condition for approval. As a result, registering a house located in an area developed after 1979 required 12 certificates. The law reinforced private property guarantees, but linked the property title to compliance with the law. Moreover, in an effort to promote enforcement, municipalities were forbidden to build service infrastructure in non-regularized settlements. The intention of the legislation—to protect housing buyers and to guarantee urban standards—was good, but the law ended up hurting one of the groups that it intended to help by making it much more difficult for the poor to regularize their properties.

While the 1979 federal law was the first to explicitly require compliance, some municipalities had enacted urban regulations long before then. In fact, some municipalities had their own building codes even before the first federal legislation in the 1930s. For instance, São Bernardo do Campo, an important municipality in the metropolitan area of São Paulo, enacted a building code in 1929.

[36] The master plan can govern many other urban aspects and also propose other standards. For example, it can include regulations related to economic development and industrial activity.

To understand the pattern of growth in the number of municipalities that enacted urban regulations, one needs to look at the development of the federal laws. It is possible, for instance, that the number of municipalities enacting some type of urban regulation began to grow in the late 1960s because of the 1967 Constitution. Another peak, at end of the 1970s, may be connected to federal Law 6766/79. After that there was a relatively stable period until the late 1980s, when the number of municipalities enacting urban regulations resumed growth—possibly due to approval of the 1988 Constitution.

The trend of having more strict urban regulations at the federal level was supported by most urban planners and architects. What legislators and urban planners seem to have ignored is the intrinsic contradiction of this policy. For instance, according to Law 6766/79, if a subdivision does not comply with the parceling regulations, houses located within it cannot be registered. When this happens, the buyer can stop paying the seller and deposit the due payments in a special account that the municipality can use to furnish the services not provided by the developer. The problem, however, is that people who buy lots or houses in subdivisions without services often do not have title to the properties. It appears that buyers who cannot afford to purchase a house that fully complies with all (costly) urban regulations tacitly collude with sellers willing to parcel and sell unserviced land. The bottom line is the poor are unable to register their houses, and illegal or irregular settlements continue to exist.