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Is Progressive Property Tax Progressive? Evidences from São Paulo

Working Paper WP18CB1

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February 2018

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Abstract

This paper uses an unusual opportunity to estimate the capitalization of the property tax on property value. In 2002 the city of São Paulo changed the property tax structure moving from uniform rate to progressive with six different brackets. This is a unique opportunity since expenditure has not changed accordingly and a typical difficulty in estimating capitalization is that a change in property tax is followed by a change in expenditures. If capitalization occurs, progressivity would increase prices for the low end of the market and decrease prices for the high end, questioning how progressive is the policy indeed. We compare the prices before progressivity is implemented with prices after the fiscal reform for the low and high end of the market showing that there is indeed capitalization in the case of São Paulo. We test the robustness of this result and then discuss the consequences of such a finding for urban policy.

Keywords: Property tax; tax capitalization; difference-in-difference; property tax incidence, property tax progressivity

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Abbreviations and Acronyms

CBD	Central Business District
EMBRAESP	Empresa Brasileira de Estudos do Patrimônio
GDP	Gross Domestic Product
IBGE	Instituto Brasileiro de Geografia e Estatística
MCMV	MinhA Casa Minh Vida – Brazilian Social Housing Program
RDD	Regression Discontinuity Design
R$	Real – Brazilian currency
SELIC	Sistema Especial de Liquidação e Custodia (Special Clearance and Escrow System – Brazilian Central Bank overnight interest rate)

Is Progressive Property Tax Progressive? Evidences from São Paulo

Introduction

The goal of this paper is to estimate the percentage of property tax capitalization[1] in property value. To do the estimation we use a policy decision that opens an opportunity for identification: in 2002, São Paulo Municipality changed the tax rate schedule from uniform to progressive. Since this change was not directly connected to a change in local expenditure, as we argue further, with some caution, it may be considered as exogenous to the problem at hand. Furthermore, we can precisely estimate the percentage change in the tax rate, so we can analyze the magnitude of the incidence of the property tax on prices (capitalization). This is an old issue, but it is still unresolved given the typical endogeneity between property tax rates and local expenditure (Palmon and Smith 1998). Estimating property tax capitalization is key in managing and implementing local tax policy.

Estimating tax capitalization using the change in uniform schedule to a progressive tax allows us to analyze the progressivity itself. Increasing progressivity may be fair to the poorest group of landowners, but with capitalization it may hurt the poorest group of tenants. Considering capitalization in the equation, it is not clear if the program is progressive for the whole population. To fully understand the main arguments presented here, it is reasonable to come back to previous literature.

More than forty years ago, Oates (1969) did the first attempt to test the Tiebout Hypothesis (Tiebout 1956). By gauging education spending per pupil and property tax capitalization on New Jersey municipalities, Oates showed that real estate prices rise more with public expenditures than it decreases with property taxes, a result that could be evidence in favor of Tiebout hypothesis. A well-known debate has emerged which doesn't seem to have a conclusion yet, even with almost fifty papers published on this matter (as far as we could find). [2]

This lack of conclusion, at least on the econometrical side, can be explained due to the complexity of designing a good estimation strategy. The main point is that it is not possible to decompose (negative) effect on prices given the higher rate of the property tax from the (positive) effect given the higher level of expenditure. But, despite this complexity and all the literature divergence, it seems that there is an agreement on the existence of capitalization – the main debate is in which degree it occurs.

Recently, however, scholars on urban planning and urban economics started worrying about property prices. Glaeser (2008), for example, argues that unaffordable housing prices could lead

[1] Property tax capitalization is the change on property prices due to a change on its property tax. Because property tax reduces the expected future private yield on the land, it capitalizes into lower current land values (Yinger et. al. 1988). Thus, as higher the property tax is, *ceteris paribus*, lower is the property value; and as lower the property tax is, higher the property price – as it was intensively discussed over the last four decades.

[2] See Yinger et. al (1988) for a complete summary of this debate until 1988 and Sirmans (2008) for a more recent literature review.

cities to inefficiency because it makes harder for some citizens to afford living standards. Since cities need human capital (both skilled and unskilled) to guide economic growth, the more expensive the average price of a dwelling, the less attractive the city is to these workers (McCann 2001; Glaeser 2005). Smolka and Biderman (2012) argue that high housing prices is one of the causes of urban informality. This approach partially explains the high incidence of slums in Latin American cities as a mixed effect of high levels of immigration of poor families with unaffordable property prices, directing these families to precarious suburban dwellings. As several authors vastly explored, informality is a huge barrier to social mobility and access to rights (Smolka 2003; Schechinger 2004).

To fight back against the rise of unaffordable property prices, literature recommends cities have several urban policies to reduce median property values, such as curtailment of building restrictions (Glaeser 2009; Borrero and Schechinger 2007; Biderman 2007; Green 1999; Lall and da Mata 2006). The main argument of these studies is that cities with cheaper property prices reduce informality since formal market prices would approximate irregular property prices, discouraging informality.

Having this literature on mind, we have, on one hand, property tax capitalization literature, in which higher taxes leads to lower property prices. Nowadays, on the other hand, we have new literature arguing towards affordable property prices and urban development. We believe that both sets of literature could be understood together. That is, property taxes could be, in some situations, used strategically to reduce property prices, making them more affordable.

To better understand this dilemma, let us assume full capitalization. In this case the increase in taxes will be fully compensated by a reduction in price. However, taxes are paid annually, while to buy a house, the property must be paid for at once. So, increasing property taxes is equivalent to giving a credit to the buyer. One of the main problems with social housing is credit is usually supplied by the government. Consequently, a progressive property tax in theory makes it more difficult for the poor to get into the housing market. If the tax is also shifted forward to renters, it is also more expensive to rent. The political economy behind it is quite complex however. Property owners at the low end of the market increase their wealth (because their house is worth more) and their income (since they must pay less in taxes).[3] The problem is with tenants: it is more difficult to enter the market and the rent increases. However, in a country with more than 75 percent of property owners at any level of income like Brazil, it is difficult to change the policy back to a uniform rate.

In this sense, property tax capitalization represents an important issue to urban planners: its possibility to reduce real estate values could be a practical and effective tool to drive the average property prices to a lower level, avoiding these previously described undesirable effects. In other words, property taxes could be a very good tool to reduce housing prices and, consequently, of great value to urban planners, who would have only to understand the mechanism that lead property taxes to capitalize into property prices to determine optimum levels of taxation both to local public finances and to average real estate values, contributing thus to reduce urban informality. On the other hand, property tax exemptions to low assessed properties could be a

[3] We may well be double counting the effects in this statement. Our point is from the political economy perspective; the impact is both in the flow as in the stock.

contradictory policy because it would make those properties more expensive for families who most need it.

It is important to note that there are very few considerations about this possibility among researchers and practitioners. Despite these two important literatures on urban economics, we did not find any work that tried to formally argue on this issue. However, some scholars have considered the possibility of using property taxes to reduce real estate values. First, Bahl and Lynn (1992) give a modest suggestion on this matter (p. 167-168). De Cesare and Smolka (2004) follow this argument to a Latin America urban informality approach. Bai, Li and Ouying (2012), finally, argue that a property tax could be used in Chinese cities to reduce property prices which rose thanks to the great economic growth of that country in past decades.

This article has six sections, including this introduction. We first describe the property tax reform that took place in São Paulo City in the early 2000s. Then we explain our estimation strategy that is a variant of the difference-in-difference approach because our treatment variable is continuous. In the fourth section we present the results of our estimation. We found evidence of a strong capitalization, around 20 percent for a 1 percent decrease in the tax rate. The fifth section checks the robustness of the results and makes us suspicious about the magnitude found in the previous section. The final section concludes the article discussing the consequences of the findings.

São Paulo's Progressive Property Tax Rates as an Instrument to Analyze Capitalization

São Paulo's Law 13.250, of December 2001, reformed the property tax regime in São Paulo City, introducing progressive tax rates depending on the property cadastral value. The law has been enforced since January 2002. Previous legislation imposed a uniform tax-rate of 1 percent of the cadaster value to be paid yearly, with an option to have a monthly payment. The new law created six different brackets with different rates according to the assessed value of the property, including exemptions for the first bracket. The law has exempted almost one million properties from paying property taxes, down from a total of 2.5 million. Table 1 presents the criteria to define the rate applied to each property.

Table 1: Property tax rates in 2001 according to assessed value in São Paulo

Cadastral Value in Reais (R$)	Marginal Rate	Deduction	Average Rate
Up To R$20,000	0%	-	0%
From R$20,00 to R$50,000	0.8%	-	0.8%
From R$50,000 to R$100,000	1.0%	R$100	0.8% to 0.899%
From R$100,000 to R$200,000	1.2%	R$300	0.9% to 1.049%
From R$200,000 to R$400,000	1.4%	R$700	1.05% to 1.224%
Above R$400,000	1.6%	R$1500	1.225% to 1.600%

Source: *São Paulo's Law 13.250/2001*

The criteria, as can be noticed, use lump-sum deductions on the tax return at each bracket. The idea of the deduction is to guarantee that the tax paid by property owners will not be discontinuous. Notice that a property assessed at R$50 thousand will pay R$400 per year using 0.8 percent tax rate or using 1 percent tax rate and deducting R$100. The exception is to the

group taxed at 0.8 percent who doesn´t have deductions on the tax. This structure avoids taxpayers from paying very low values per month (this is one of the rationales), but it creates a discontinuity in the tax term. For instance, a property assessed at 19,999 in 2001 would not pay any tax while a property assessed at 20,001 would pay R$160. It is also straightforward that, given this structure, it is incorrect to say that a real estate assessed at, for example, R$ 100,000.00 would be taxed at 1.2 percent. This will be the marginal rate but not the average. Before the property tax reform, the marginal and average rates where evidently equal. The last column on Table 1 calculates the range of average rates for each bracket.

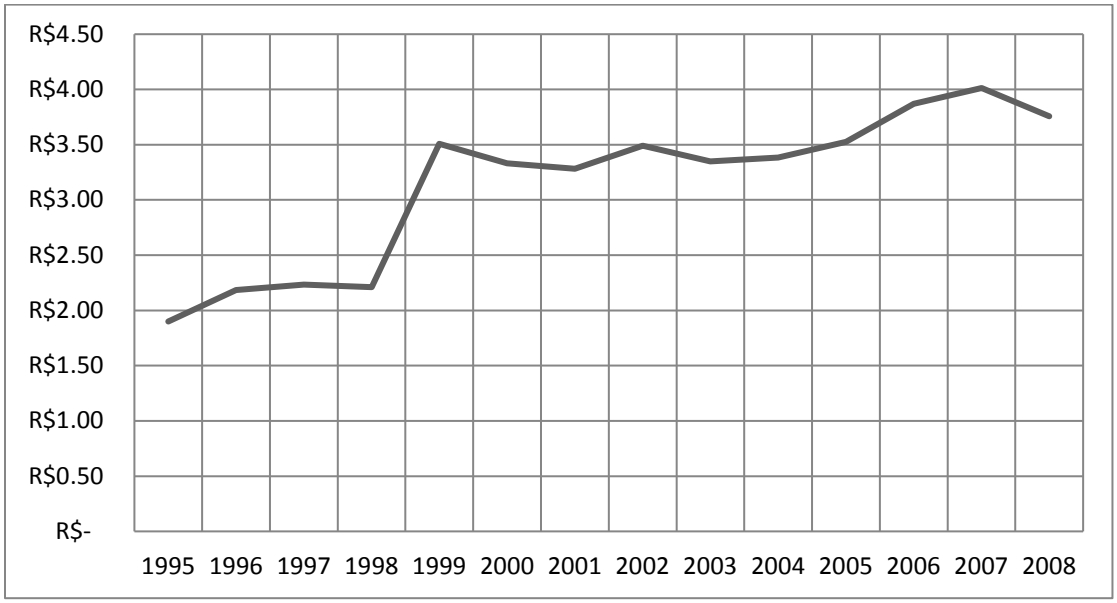
The assessed value in São Paulo is also defined by municipal law, which needs to be approved by the municipal council. Several attributes of the property are analyzed in this assessment, such as square meters, depreciation, construction materials and location. The latter includes research on market prices associated with property location. Reassessing property for tax purposes is evidently very unpopular. Before 2009, reassessment was done very seldom: since 1990 it was reassessed just in 1995, 2001 and 2010. When the property is not reassessed, the original prices are readjusted based on last year's inflation. In 2011, a new municipal law made it mandatory to reassess property values (for tax purposes) every other year. However, the municipality failed to implement a massive reassessment in 2013.[4]

Of course, the criteria for defining the brackets presented on Table 1 changes every year to incorporate at least previous inflation. Thus, for our purposes, we must recalculate the average rate every year.

We feel secure to argue that the law was only to promote equality on tax payment and has no relationship to public expenditures. To prove that, consider figure 1 below, which presents São Paulo's property tax revenue updated to 2013 values. The increase on property tax revenue in 2002 is very low (around 6 percent) considering other year's increases, such as 1998-1999. We do not understand what led to this increase, but it is hard to believe that it was connected to the tax reform implemented three years later. By 1999 the change in the tax schedule was not even under considerations since it was a proposal from the mayor elected in 2000 – Marta Suplicy. The curve from 1999 to 2005 shows no significant disturbance on property tax revenue for the City of São Paulo. If progressivity has had an impact on total revenues, we might be confounding the impact from the tax change in volume not in schedule as we claim.

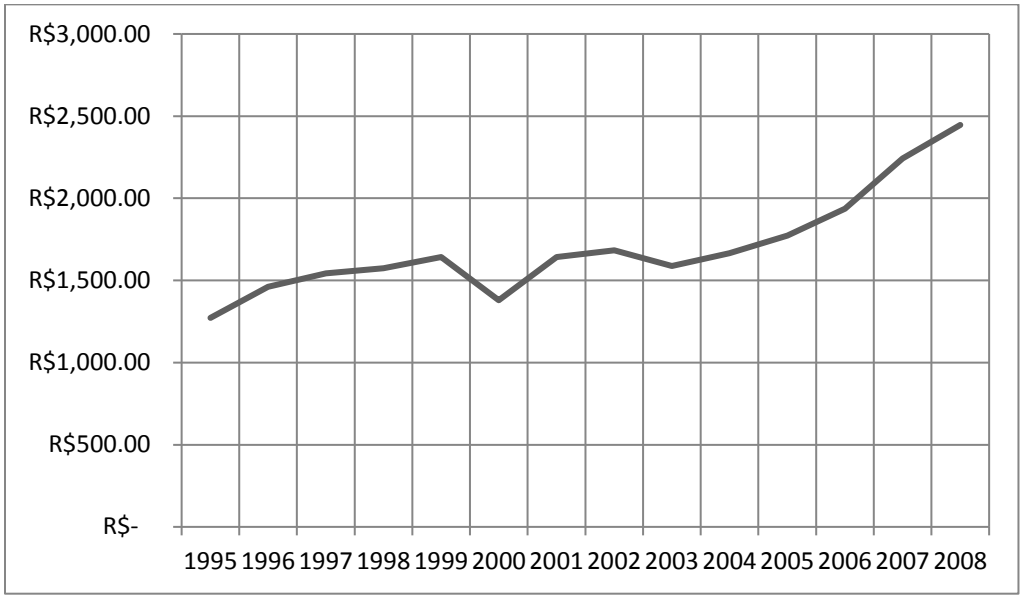
[4] São Paulo's Justice due to a legal process initiated by the opposition party and the São Paulo's Industry Federation prevented the municipality to introduce the reassessment. Given that, the mayor decided just to readjust the previous assessed values by inflation. This is a good example of how much reassessing property values for tax purposes have strong political consequences in Brazil and explains why reassessment are so infrequent.

Figure 1: Property tax revenue in billions of R$. São Paulo, 1995-2008



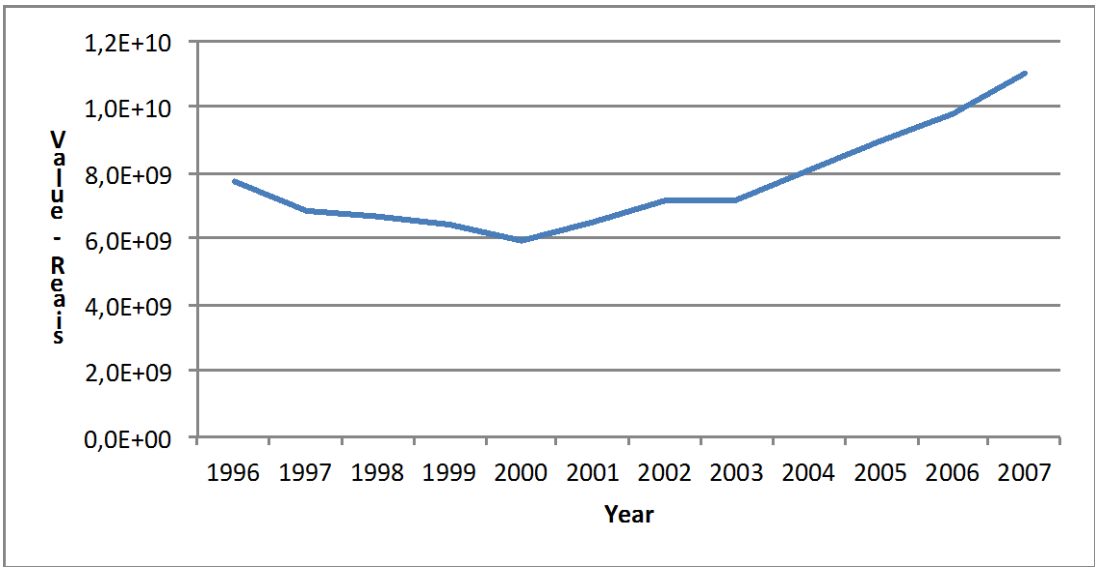
Another possible confounding mechanism would be through the expenditure change. We can also see that per capita public expenditures have not significantly changed in the period. Although there is a small increase from 1999 to 2000, this represents no more than 2.4 percent of per capita expenditure. Therefore, the change in the tax schedule is not associated with a change in property tax revenues, nor on per capita expenditures. So, we are confident that we are not confounding the impact on housing price with some indirect impact through the change in local government revenues and expenditures.

Figure 2: Per capita public expenditures. São Paulo, 1995-2008



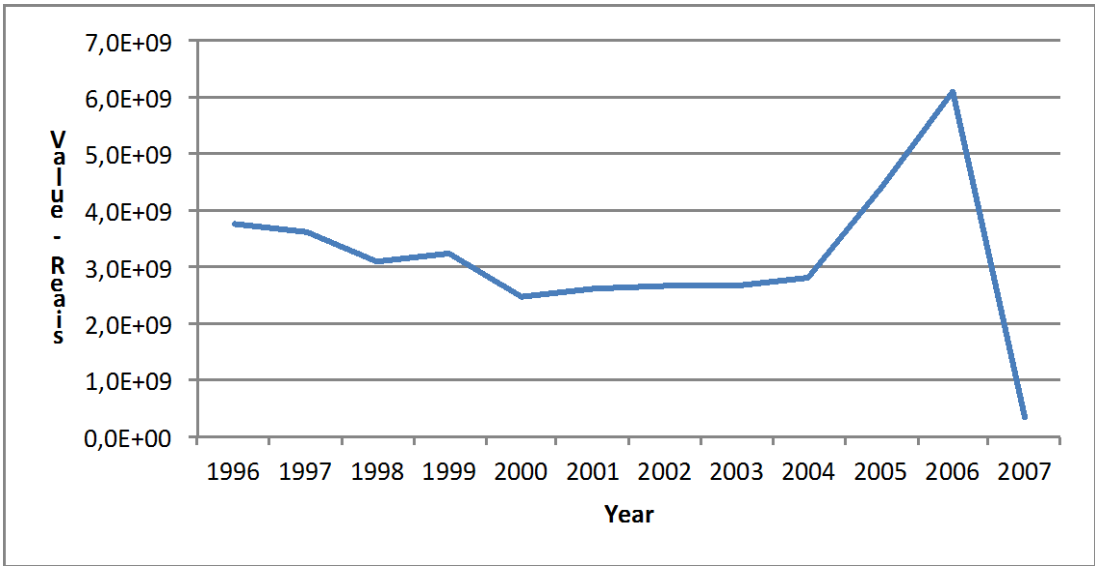
There is still another confounder to be analyzed. It is possible that the government that implemented the progressivity also changed the expenditure schedule towards the poor. This policy would be consistent with progressivity. In other words, we would have the usual scheme where property tax capitalization would be mixed with an increase in expenditures. Usually one cannot compare two cities with different property taxes because cities with higher tax rates also have higher expenditures. In this case, the group with lower tax rates could have received a higher share of expenditures. In Brazil in general, and particularly in São Paulo, the poor use public schools and hospitals. So, total expenditure in education and health could have changed in such a way that it would be capitalized into the benefitted group housing. In Figure 3 we can see that this is not exactly the case.

Figure 3: Expenditure in health and education in São Paulo City



Although expenditures in health and education have been increasing since 2000, the movement was smooth. It is hard to perform a structural break test with such a small-time series, but it is clear from the Figure that there was not a discontinuity in 2002. We must be careful regarding the trend since it may have induced a smooth increase in the value of housing for the lowest income segments. Finally, regarding public finance confounders, we must check the municipality's expenditure on housing. If the municipality decreased investment in housing, there would be a decrease in the supply of housing for the poor pressuring the prices for this segment. Figure 4 however does not support this interpretation.

Figure 4: Public expenditures in housing projects in São Paulo City



Although there is an increase in housing expenditure in 2005 and 2006 followed by a drastic drop in 2007, there is no apparent discontinuity in 2002. Considering a moving average, the 2005-2007 period would be very similar to the previous years' average. In short, we believe that there is no fiscal channel confounding the identification of the capitalization into housing prices.

Dataset and Empirical Strategy

This research combines three datasets. The first is the cadaster from the municipality of São Paulo, containing administrative records with housing values for property tax purposes. The second is a complete set of information from all new apartment buildings released in São Paulo's Metropolitan Region (size, number of rooms, bathrooms, facilities in the building, etc.) from Embraesp.[5] The advantage of working with new releases is that we do not have to worry about housing depreciation (Biderman 2001). Furthermore, Embraesp has actual sale price that is better information than asking prices that are often used for analyzing housing prices. Finally, we use the Brazilian Census of 2010 for some control variables.

To merge those datasets, we have geo-referenced the Embraesp dataset and spatially joined it to the Digital Map of the City that has the "fiscal blocks." Fiscal blocks are defined for fiscal purposes. Within its perimeter the value of the square meter is uniform for fiscal purposes (splitting land and structure). Since we know where the building surveyed by Embraesp is located, we know to which fiscal block it belongs and consequently the price per square meter for fiscal purposes. Using the apartment[6] area we can estimate in which bracket the property is and consequently the average property tax rate associated with that property. We chose to analyze the period between 1995 and 2008 so we will have the same number of years before and

[5] In this database there is information only on apartment buildings, except for the case of gated communities.
[6] From now on we will use the terms apartment and property interchangeably when referring to our sample of analysis since it includes just apartments as commented in the previous footnote.

after the tax change and will not have to deal with the Brazilian monetary reform that took place in July 1994.

Table 2: Average Prices* and Number of Observations by Bracket Group in the Sample

Bracket Group	1995-2008		1995-2001			2002-2008		
	Obs	Share	Obs	Share	Average Price*	Obs	Share	Average Price*
Exempt	2,223	34.5%	1,083	39.8%	279,302	1,140	30.7%	203,270
0.8%	1,681	26.1%	761	28.0%	487,355	920	24.8%	395,538
1.0%	1,208	18.8%	482	17.7%	837,914	726	19.5%	685,831
1.2%	796	12.4%	243	8.9%	1,562,361	553	14.9%	1,306,133
1.4%	354	5.5%	107	3.9%	2,414,537	247	6.7%	2,365,959
1.6%	174	2.7%	46	1.7%	5,172,996	128	3.4%	4,686,881
Total	6,436	100%	2,722	100%	717,561	3,714	100%	807,792

*Market Prices in January 2014 $R deflated by IGPM
Source: Embraesp and São Paulo Cadastral Office

The decision to use average tax rates is because the relevant tax for capitalization is the average. It is well known that the marginal tax is the relevant rate for making decisions. This is a very fundamental part of microeconomics. For instance, the decision to buy a larger apartment will depend on the marginal rate. However, the extra value expended (saved) by an increase (decrease) in the tax depends on the average rate. For example, a property that is assessed at value R$250 thousand will pay R$2,800 per year in taxes after the reform, compared to R$2,500 before the reform. Those R$300 extra per year may be (partially) capitalized into prices. If we use the marginal tax, we would say that R$1,000 (3,500-2,500) would be expected to be capitalized into prices. However, the property owner does not face R$1,000 extra per year but R$300. Table 2 compares prices by group before and after the tax reform.

We can notice that exempt apartments represent the clear majority among groups. Although our sample is composed just of new building releases, it is like the total stock of housing since the city estimated in 2001 that around 30 percent of the housing stock would be exempt from taxation after the reform.[7] It is important to note that the share of the exempted group is declining. This is true also for the three groups in the lower end (exempt, 0.8 percent and 1 percent). The share of the 1.2 percent group remained stable while the share of the most expensive apartments increased. This is probably reflecting that the market is offering more apartments in the high end of the spectrum and/or that housing prices are increasing at a faster rate than inflation (all numbers are in July 2014). The second alternative is unlikely since average prices in each bracket are going down. However, since there are more apartments in the more expensive brackets, average price for the whole sample has increased. It is important to note that cadastral records define the bracket while the market price is calculated using Embraesp information.[8] This is the reason why average prices are not consistent with Table 1.

[7] We also compare other variables concluding that our sample is similar to the total stock of housing at least in terms of observable variables.
[8] In other words, we need cadastral value to find the tax bracket, but we need market price to do the analysis. The spatial join was the strategy we adopted to link this information.

An ideal experiment to assess the level of capitalization would be to randomly select properties and changes to their tax rate. After a period, it would be possible to compare the price change in properties that were randomly selected with the price change in properties that were not selected. This price difference would be the impact (on prices) of the tax rate change. We do not have the ideal experiment because properties that have their tax rate changed were not randomly selected. It may be the case that the demand for the low end of the market was not immediately satisfied by supply, but this was not happening at the high end of the market, for instance. The way we will control for other sources of variation will be using a set of control variables. More specifically we will be running regressions with (variations of) the following specification:

$$ln(p_{it}) = \square + \square_1 y_t + \square_2 \square_i + \square_3 y_t \square_i + \gamma g_t + \square_x \boldsymbol{X}_{it} + \square_h \boldsymbol{H}_{it} + \square_{it} \qquad (1)$$

Where $\ln(p_{it})$ is the natural logarithm of price of housing i at year t; y_t is a dummy variable that takes value 1 if the year t is 2002 or more recent (i.e. a "step" variable); $\square_i$ is 1 minus 100 times the tax rate associated to housing i **after** progressivity was implemented; g_t is a trend, i.e. a variable that assumes the value 0 in the first year (1995), 1 in the second (1996) etc. $\boldsymbol{X}_{it}$ is a set of socio-demographic attributes of housing i at year t in the census block where the building is located; $\boldsymbol{H}_{it}$ is a set of housing attributes of housing i at year t; $\square$, $\square\gamma$ and $\boldsymbol{\beta}$ are parameters to be estimated by the regression and $\square_{it}$ is a spherical error clustered by census blocks since there might be more than one building released in the same block.

Notice that specification (1) represents essentially the *difference-in-difference* approach except that it is slightly more cumbersome because the treatment variable ($\square_i$) is continuous. To better understand this (crucial) variable one has to keep in mind that it defines the counter factual in a subtle way. It gives the average tax rate of the property even before progressivity was implemented as if progressivity was implemented. In other words, a property that was worth 25 thousand in 2001 would have $\square_i$ =0.8 although this property would be paying 1 percent in 2001. With this strategy we construct a continuous of treatment groups. If the property average rate is below 1 percent the treatment variable will be negative; if it is above 1 percent the treatment variable will be positive. To make it clearer, let us compare the change in price for a property that got exempt after progressivity was implemented with the change in price for a property that (theoretically) is paying 1 percent in average. Formally we are comparing the following:

$$\begin{aligned} &E[\ln(p_{it})|y_t=1; \square_i=-1; \boldsymbol{X}_{it}; \boldsymbol{H}_{it}] - E[\ln(p_{it})|y_t=0; \square_i=-1; \boldsymbol{X}_{it}; \boldsymbol{H}_{it}] \\ &- \{ E[\ln(p_{it})|y_t=1; \square_i=0; \boldsymbol{X}_{it}; \boldsymbol{H}_{it}] - E[\ln(p_{it})|y_t=0; \square_i=0; \boldsymbol{X}_{it}; \boldsymbol{H}_{it}]\} = -\square_3 \qquad (2) \end{aligned}$$

Where $E[\bullet]$ is the expectancy operator. Because the (uniform) tax rate before progressivity was implemented was set at 1 percent, properties that were still paying 1 percent are our "traditional" control group and that is the reason why we take off one from the (100 times) tax rate. This is a strategy for keeping the treatment equal to zero to this hypothetical control group. Since the tax rate is not discontinuous, except for the exempt group (compared to the 0.8 percent group) there are very few properties that actually stayed at 1 percent (a property with cadastral value identical to R$150 thousand would be the only case). But this is not an issue. What the expected values on (2) are showing is that $-\square_3$ estimates the percentage price change due to capitalization (from a 1 percent decrease in tax) even if there is no property paying exactly a 1 percent tax rate.

The expectancy calculated in (2) is important to interpret the magnitude of $\square_3$. To understand it better let us call p_{11} the price of an apartment after the tax reform that was exempt; p_{10} the price of an apartment before the tax reform that would be exempt; p_{01} the price of an apartment after the tax reform that has an average tax rate equals to 1 percent and; p_{00} the price of an apartment before the tax reform that has an average tax rate equals to 1 percent. Assuming that:

$\{p_{it}, y_t, \square_i/\boldsymbol{X}_{it}; \boldsymbol{H}_{it}\}$ are independent (3)

we can write (2) as:

$$E[\ln\{(p_{11}/p_{10})/(p_{01}/p_{00})\}|\boldsymbol{X}_{it}; \boldsymbol{H}_{it}] = -\square_3 \rightarrow E[(p_{11}/p_{10})/(p_{01}/p_{00})|\boldsymbol{X}_{it}; \boldsymbol{H}_{it}] = \exp\{-\square_3\} \quad (2')$$

So, the natural logarithm exponential of $-\square_3$ is the expected value of the relation between (one plus) the change in price for apartments exempt and (one plus) the change in price for apartments taxed at 1 percent. We can interpret it as the additional percent change in price associated with a 1 percent reduction in tax. Considering the hypothesis of independency (3), the price change in apartments exempt in excess to the price change in apartments at 1 percent rate might be caused by the tax reform.[9] So we can identify (and measure) capitalization with specification (1).

The advantage of using a continuous treatment is that we compare all groups simultaneously. We can think about this treatment variable as measuring the intensity of treatment. As it gets more negative (down to -1), the higher the treatment will be. As it gets more positive (up to 0.6), the lower the treatment will be. An analogy would be with the doses of a medicine. The result using a continuous treatment is more trustworthy since there might be a continuous difference in housing demand and supply to confound the impact we are observing with something that was happening in time. Batista (2014), comparing the brackets as if they were discontinuous, found evidence of capitalization, but, in this case, it is possible that there was a difference by income group since low-income classes increased their income at a faster rate than high-income classes in the last 15 years.

The main point of this empirical strategy is the assumption of conditional independence in (3). This condition is not so demanding as in Wales and Wiens (1974) or King (1977) partially because of the difference-in-difference specification adopted in this paper but mainly because of the quasi experiment opportunity given the change in the tax schedule. It is however still possible that we may be confounding our results with some other phenomenon. When we use the difference-in-difference we are automatically correcting for factors that are not changing over time. However, it is still possible that some elements were changing over time precisely when the tax reform was implemented. It is interesting that the only paper using a difference-in-difference approach for estimating capitalization that we could find in the literature is Zhang (2013).

[9] Typically, the causal effect in a difference-in-difference framework would be given by the change in the treatment group minus the change in the control group. In the notation proposed above, this would mean: $[p_{11} - p_{10} - (p_{01} - p_{00})]$. We are measuring a "composed" impact compared to a "simple" impact that is the usual measure in this framework.

The second point to be noticed in assumption (3) is that we are claiming conditional independence. Conditional independence is almost always true: if you can control for all other sources of variation the relation between the dependent and independent variable might be the causal effect of the latter on the former. The problem is controlling for all sources of variation. More technically, we are worried about variables that would impact differently the groups with the same timing as the property tax reform.

So, it is very important which variables we control for. If we do not consider a variable that is correlated to housing price and simultaneously to the group and timing, our estimation would be biased. Compared to cross section estimation, we do not have to worry with variables that are correlated to housing prices and groups if they are not correlated with the timing of the tax reform. By the same token, we do not have to worry with variables that are correlated with housing prices and the tax reform timing but are not correlated with the groups. In this sense we are better off than we would be using time series. But there are still some variables that might be correlated with the three key variables in the model and we should attempt to control for those sources of bias in the estimation. We can also control for variables that are just correlated with prices to reduce the variance of the estimator. Table 3 presents a list of the control variables chosen in this study.

Table 3: List of Control Variables

Variables	Description
Rooms	Number of rooms of the apartment
Baths	Number of baths of the apartment
Garage	Number of Garages available for each apartment
Elevator	A dummy equal to 1 if the building has one or more elevators
Vertical	A dummy equal to 1 if the building is not a house and has three or more floors
Common Area	The common area of the building (in m^2)
Floor Area	The floor area of the apartment (in m^2)
CBD Distance	The (log of) distance from the building to the Center Business District
CEPAC Area	A dummy equal to 1 if there was a public intervention within 1km radius
Local Income	Average income in the Census Block on launching year
Rail Station	A dummy equal to 1 if, on the launch year, there was a rail station within 1km radius
Unemployment	Average of unemployment rate at São Paulo in previous semester
Poorest Purchasing Power	Number of basic baskets that the 20% poorest families could purchase in last semester
Cooperative	A dummy equal to 1 if the building was produced by a cooperative
Price System	A dummy equal to 1 if the price is defined by building costs
Inflation	The Brazilian inflation rate on that month
Interest Rate	The Brazilian interest rate on that month
Real Estate Credit	The total real estate credit on Brazilian economy on that month
Brand Share	The share of the brand on last 6 months

The number of rooms, baths, parking spots, and elevators, along with the floor area and vertical stories evidently affect housing prices and do not need to be further clarified. Common area includes all building area for common use, such as ballrooms, pools, halls, etc. It is also straightforward that this variable affects price. It is not clear if those variables are connected to

the group and simultaneously to the tax reform timing. However, we kept them to increase the precision of the estimates.

The distance from the Center Business District is a classical variable in property price studies. For this, we use the intersection between Bandeirantes Avenue and Marginal Pinheiros where the property price is the highest in the metropolitan area (Biderman 2001).[10] Regarding the Urban Operation Consortium variable, we refer ourselves to the regions where, after 2004, they have received a large investment through the sale of building rights. Biderman, Sandroni and Smolka (2006) show that prices on this perimeter and their boundaries have increased faster than average prices in São Paulo City. So, this variable is certainly correlated with price and timing and it is likely to be correlated with groups since this is an area concentrated in the high end of the market.

"Local Income" is the average per capita income of all the families living in the census block where the building is located. This is connected to the groups and may be connected to the housing price if there is segregation or if it is a proxy for building quality. We are not sure if this demographic variable is connected to the timing of the tax reform or not.

The Rail System dummy is equal to one if, on a 1 Km radius, there is a subway or commuter rail station at the time the building was released. It is well known that the proximity to stations impacts housing prices. The "Cost System" consists of a specific model of housing sale where the buyer agrees to pay for the construction cost of the apartment. Usually it results in a sharply cheaper price. Once again, we have no idea if those variables are correlated to groups or the tax reform timing.

Finally, we have basic macroeconomic variables: GDP growth in the last trimester prior to the building release and reference interest rate (SELIC), released by the Central Bank of Brazil. Those variables change in time exclusively and, consequently, they also work as trend variables. This is important since those variables may avoid us confounding the impact of the tax reform with a macroeconomic trend.

Results

Table 4 presents the estimation of capitalization due to a 1 percent decrease in the tax rate for four different specifications. We start with a basic model just estimating the impact with no controls except the trend (i.e. we did not consider any $\boldsymbol{X}$ or $\boldsymbol{H}$) and then we gradually add controls. Detailed results are presented in the appendix on Table A1. We report $\exp\{-\square_3\} - 1$ since this is the percentage change in price due to a 1 percent decrease in taxes as discussed in the previous section and formally presented in equation (2'). We decided to keep the trend since it has a large impact on the estimates showing that for some reason, not necessarily connected to

[10] The CBD is often defined as been the place where property prices are the highest in the city as assumed in the canonical Alonso-Muth-Mills model. We have also tried our regressions using Praça da Sé (ground zero for São Paulo) and the middle of the Paulista Avenue – another locus of business activity – and the results were essentially identical.

the intervention, the price of the lower end of the market was decreasing at a slower pace than the upper end (recall from Table 2 that all prices – by bracket – were going down).[11]

Not controlling for other variables bias the results overestimating capitalization. On the other hand, after controlling for building and location attributes, the capitalization estimated is quite stable around 8 percent, except when we add macroeconomic variables when it increases slightly to 10 percent (in any case, there is no significant difference between the coefficient for any specification except for specification (1)). We are not very comfortable with specification (6) since macroeconomic variables are very collinear to the trend, so our favorite specification is (5). The coefficient of interest is always significant at 0.1 percent.

Table 4: Capitalization estimated (%) for different specifications

Results for different specifications	**Specification Model**					
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Capitalization	15.7%***	8.6%***	8.24%***	7.6%***	8.9%***	10.1%***
Building Attributes	No	Yes	Yes	Yes	Yes	Yes
Neighborhood Attributes	No	No	Yes	Yes	Yes	Yes
Purchasing Conditions	No	No	No	Yes	Yes	Yes
São Paulo Economics	No	No	No	No	Yes	Yes
Macroeconomic variables	No	No	No	No	No	Yes
N	6431	6431	6431	6431	6431	6431

** p<0.05, ** p<0.01, *** p<0.001*
Source: Embraesp, São Paulo Cadastral Office and IBGE

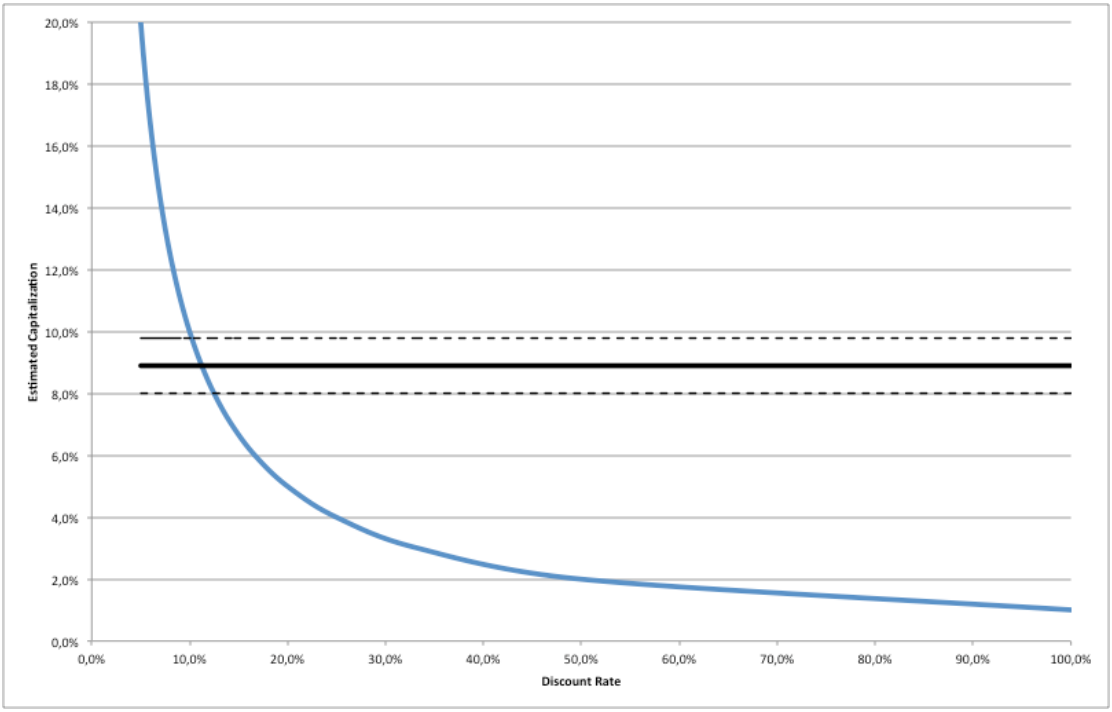
We must be careful in interpreting this magnitude. Reducing the tax rate by 1 percent means that the owner will not have to pay 1 percent every year forever. Consequently, the tax exemption is like winning a perpetuity that is worth 1 percent of the property value. As it is well known, the present value of any perpetuity is its face value over the interest rate. Consequently, if our estimation is correct, to have full capitalization, the market would be discounting the value of the tax exemption at around 11 percent per year.

Quang Do and Sirmans (1994) estimate the discount rate for tax capitalization in California to be around 8 percent but other studies quoted in this paper such as Yinger et al. (1988) inter alia, assume the discount rate to be between 3 percent and 6 percent. Considering a 6 percent discount rate, we can say that a rough approximation would be a capitalization of 47 percent. So, roughly speaking, we may say that 50 percent of the tax was capitalized into prices while the other half was somehow shifted forward or backward. This result would be compatible with equal elasticities both for demand and for supply. Evidently, we do not have elements to say anything about the possible shifting in the tax since we are not estimating elasticities (and it is not the goal of this paper, anyway). The important issue is that to analyze capitalization we need a discount rate.

[11] Using the same specifications, the capitalization estimated without trend was 48% in specification (1) and the lowest value was 19% well above the capitalization estimated with a trend. This value is too high as discussed in this section.

Figure 5 shows full capitalization depending on the discount rate (blue line). We have also added the capitalization estimated in specification (5) (black solid line) and one standard deviation below and above this value (black dashed line). To have full capitalization with a 6 percent discount rate the estimated capitalization should be around 17 percent (16.7 percent to be precise). Capitalization of half of the tax change is reasonable for traditional elasticities; if demand and supply precisely the same elasticity a change in the tax should be split evenly. However, the traditional argument is that land is totally inelastic and consequently all tax change should be shifted to land prices. We cannot really test the hypothesis of full capitalization because the level of capitalization depends on the (subjective) discount rate that the market uses that is not observable by the analyst.

Figure 5: Capitalization as a Function of the Discount Rate



In short, it is very difficult to account for all the elements of capitalization, but it seems that we have a reasonable magnitude, except that the cadastral value is often below the market value and our estimation use market value. It is possible that the cadastral value is more precise for new apartments and our sample has just new apartments as discussed before. However, in this case, the buyer would not be considering that the real value of property tax will go down in time, so we would need to add a hypothesis of monetary illusion. If the difference between the cadaster value and the market value is 50 percent, we would have full capitalization at a 6 percent discount rate and accept the full capitalization hypothesis.

Even though we have a reasonable magnitude of capitalization, it is still possible that we are not getting it right. If our estimation is still biased it is a major issue and we must attempt to check it. The way we will do this robustness check is estimating the impact with different samples and empirical strategies. This is the goal of the next section.

Robustness Check

We adopt three strategies to check the robustness of our results. The first one is using a shorter term of analysis. So, we run regressions using a slight modification of specification (5) in Table 4 reducing the years covered by our sample getting closer to the tax reform year. We drop the trend since we are working on the time dimension. For instance, it is not possible to have a trend when we constrain the sample just to 2001 and 2002. The consequence is that we cannot compare the magnitude of the estimates, but we can check if the result survives to the constraint in the time frame.

The second strategy is using a "fake" dummy for the year of the intervention. We will constrain the sample from 1995 to 2001 and use the dummy for 1998. We expect to see no impact in a year that nothing has happened to the tax schedule. This is like using a placebo in the control-trial estimates in medicine.

Finally, we will constrain the sample just to apartments below the first bracket of the progressive tax. This constraint will give us a sharp discontinuity resulting from the change in the tax schedule. The treatment group in this case will be the apartments that reduced the tax rate from 1 percent to no tax and the control group will be the apartments that have reduced the tax from 1 percent to 0.8 percent. The difference between those two groups will reflect a reduction (forever) of 0.8 percent in the rate. Given the sharp discontinuity we can use Regression Discontinuity Design to check for the robustness of our results.

Changes in the Period of Analysis

Table 5 shows the capitalization estimated for different periods using specification (5) in Table 4. As we reduce the sample the precision diminishes, as expected. This is the traditional tradeoff between precision and bias due to the reduction in the sample side. At the same time the magnitude is going down. Looking at Table A.2 it is possible to see that if the standard deviation was constant, capitalization would be significant at 5 percent even constricting the sample just to two years before and after the tax reform (column 5). It means that this test is confirming the previous results and we are more convinced that the change in tax schedule indeed capitalized in housing prices.

Table 5: Capitalization estimated (%) for different periods

Results	**Period Analyzed**					
	1996-2007	**1997-2006**	**1998-2005**	**1999-2004**	**2000-2003**	**2001-2002**
Capitalization	21%***	19%***	16%***	12%***	8%**	6%
N	5422	4382	3512	2729	1876	904

** p<0.05, ** p<0.01, *** p<0.001*
Source: Embraesp, São Paulo Cadastral Office and IBGE

The magnitude is going down as we constrain the sample. There are two possible interpretations for that result. The first is that we were overestimating the magnitude of capitalization. In other words, we were confounding capitalization with some other phenomenon that was happening in

time and our control variables were not able to catch such an element. We believe that we capture most of it using the trend. The second interpretation is that buyers were not totally convinced that progressivity would stay for a long time. As a matter of fact, there were some legal disputes over progressivity in the beginning. As courts were accepting progressivity, it was probably more credible, and households incorporated it as perpetuity. It makes sense also that households would be more positive about progressivity after 2005 since there was a change in the mayoral party administration and the property tax term was not changed.

The problem with the second interpretation is that this robustness test might be questioned. If it is correct that households were not positive that progressivity would prevail, results on Table 5 would be underestimating capitalization. In other words, we would be confounding capitalization with households' expectancy. So we need another way to check for the robustness of our results.

Placebo Analysis

The second strategy adopted was to use a "fake" year for the date of the tax reform. We constrain our sample to the period between 1995 and 2001 and define 1998 as the "fake" year of the tax reform. We run regressions with the 6 specifications used before. Table 6 presents the results.

Table 6: Capitalization estimated (%) using placebo for the tax reform year

Control Variables	Specification Model					
	(1)	(2)	(3)	(4)	(5)	(6)
Capitalization	-21.5%***	-16.0%***	-18.4%***	-12.7%***	4.4%	4.3%
Building Attributes	No	Yes	Yes	Yes	Yes	Yes
Neighborhood Attributes	No	No	Yes	Yes	Yes	Yes
Purchasing Conditions	No	No	No	Yes	Yes	Yes
São Paulo Economics	No	No	No	No	Yes	Yes
Macroeconomic variables	No	No	No	No	No	Yes
N	2719	2719	2719	2719	2719	2719

** p<0.05, ** p<0.01, *** p<0.001*
Source: Embraesp, São Paulo Cadastral Office and IBGE

The results from this "placebo analysis" reinforce that there was indeed capitalization. In the period analyzed we see that there is no impact on 1998 (*vis a vis* later years) for specification (5). For other specifications we can notice a negative sign meaning that the lower end of the market was probably decreasing prices (the opposite that we noticed for the whole period). Since the year dummy means nothing this result is probably due to the omission of variables that is corrected in specification (5).

Regression Discontinuity Analysis

The way we did the difference-in-difference analysis is not conventional. The reason we use this variation of the method is due to the characteristic of the intervention. As in most tax schedules, there is no discontinuity when you move around brackets. However, in this case, until 2015,

there was a discontinuity in the first bracket. An apartment below the first threshold was exempt from paying any property tax after progressivity was implemented, while an apartment above this threshold reduced the tax to 0.8 percent (compared to 1 percent previously). The continuity scheduled was valid just for apartments above the second bracket.[12]

This discontinuity represents an opportunity to use a traditional difference-in-difference approach to estimate the same phenomenon. Furthermore, it gives an opportunity to check the robustness of the result exploring the discontinuity using Regression Discontinuity Design (RDD) techniques. To do so we must change both the definition of the cross-section variable as well as the specification proposed in equation (1). The new "base" specification can be written as:

$$ln(p_{it}) = \square + \square_1 y_t + \delta_2 \tau_i + \delta_3 y_t \tau_\square + \gamma g_t + \alpha_1 r_{it} + \alpha r^2_{it} + \alpha_3 r^3_{it} + \alpha_4 r^4_{it} + \square_x \boldsymbol{X}_{it} + \square_h \boldsymbol{H}_{it} + \square_{it} \quad (4)$$

Where the main change is the new variable differentiating control and treatment, $\tau_\square$. This is an indicator variable that will take the value 1 if the cadastral value of the property is below the threshold for exemption. For instance, in 2002, $\tau_\square$ would be 1 if the cadastral value of the property was below R$20,000.00. We are also adding the variable r_{it} that represents the "running variable" in the RDD jargon. It will have the value of the cadaster less the threshold for exemption. Using 2002 again it would be the cadaster value of the property less 20,000, but this value is changing over the years.

It is easy to see that specification (4) is a traditional difference-in-difference estimation. In this case, the coefficient of interest is δ_3 as we can see formally:

$$E[\ln(p_{it})|y_t=1;\ \tau_i=1;\ \boldsymbol{X}_{it};\ \boldsymbol{H}_{it}] - E[\ln(p_{it})|y_t=0;\ \tau_i=1;\ \boldsymbol{X}_{it};\ \boldsymbol{H}_{it}]$$
$$- \{\ E[\ln(p_{it})|y_t=1;\ \tau_i=0;\ \boldsymbol{X}_{it};\ \boldsymbol{H}_{it}] - E[\ln(p_{it})|y_t=0;\ \tau_i=0;\ \boldsymbol{X}_{it};\ \boldsymbol{H}_{it}]\} = \delta_3 \quad (5)$$

If we consider again the hypothesis of independence (3) we can write (5) as:

$$E[\ln\{(p_{11}/p_{10})/(p_{01}/p_{00})\}|\boldsymbol{X}_{it};\ \boldsymbol{H}_{it}] = \delta_3 \rightarrow E[(p_{11}/p_{10})/(p_{01}/p_{00})|\boldsymbol{X}_{it};\ \boldsymbol{H}_{it}] = \exp\{\delta_3\} \quad (5')$$

So, the natural logarithm exponential of δ_3 is the expected value of the relation between (one plus) the change in price for apartments exempt and (one plus) the change in price for apartments taxed at 0.8 percent. We can interpret it as the additional percent change in price associated with a 0.8 percent reduction in tax. Considering the hypothesis of conditional independency (3) the price change in apartments exempt in excess to the price change in apartments at 0.8 percent rate might be caused by the tax reform. So, we can identify (and measure) capitalization with specification (4) restricting the sample to apartments that are below the second threshold.

Table 7 presents the estimations from variations of specification (4) repeating the same strategy adopted in Table 4 except that the definition of control and treatment changed, and we add a

[12] To illustrate, consider the thresholds in Table 1. An apartment with a cadastral value equal to R$19,999.00 would pay no tax. An apartment with cadastral value equals do R$20,001.00 would pay R$160.01 per year in property tax. However, an apartment with cadastral value equals to R$49,999 would pay R$399.99 while an apartment with cadastral value equal to R$50,001 would pay R$400.01.

polynomial on the "running variable." Once again, we are showing just the estimated capitalization in percentage and detailed results are in the appendix. We have also divided the result by 0.8 so the results are directly comparable with the results reported on Table 4.

Table 7: Capitalization estimated (%) using Regression Discontinuity Design

Control Variables	Specification Model					
	(1)	(2)	(3)	(4)	(5)	(6)
Capitalization	9.1%***	3.0%*	2.4%	3.1%*	2.3%	3.3%***
Building Attributes	No	Yes	Yes	Yes	Yes	Yes
Neighborhood Attributes	No	No	Yes	Yes	Yes	Yes
Purchasing Conditions	No	No	No	Yes	Yes	Yes
São Paulo Economics	No	No	No	No	Yes	Yes
Macroeconomic variables	No	No	No	No	No	Yes
N	3768	3768	3768	3768	3768	3768

** p<0.05, ** p<0.01, *** p<0.001*
Source: Embraesp, São Paulo Cadastral Office and IBGE

The results from this exercise are somehow worrisome. First, the impact is much lower than estimated before, around 3 percent or lower. This would mean a low level of capitalization. Using Figure 5 one can see that we can consider full capitalization only if the discount rate was around 30 percent. Second, the coefficient is not even significant for two specifications including our favorite option (5). Given this result we cannot be so positive about the capitalization of the tax rate on prices. Following the tradition in RDD analysis we constrain the sample to values closer to the threshold for specification (5).

Table 8: Capitalization estimated (%) for Different Samples

Results	**Assessed Value Constrained (1)**			
	4 to 44K	**8 to 38K**	**12 to 32K**	**16 to 26K**
Capitalization	3.6%***	1.9%	1.1%	-4.5%
N	3150	2410	1393	757

(1) The values are references for 2002 adjusted for changes in the thresholds around the years
** p<0.05, ** p<0.01, *** p<0.001*
Source: Embraesp, São Paulo Cadastral Office and IBGE

When we constrain the sample to be closer to the discontinuity point we have a result questioning our previous findings. Although the first constraint is significant the following coefficients are not. Furthermore, the magnitude is falling as we constrain the sample indicating that the result may not be causal. The highest constraint gives a negative impact of the tax on prices. In other words, this final test makes us much less confident that the change in the tax schedule indeed has been capitalized into prices. We need to research more deeply what is going on, but one possibility would be manipulation from the developers attempting to make apartments that "just fit" in the exemption zone.

Conclusion

In this paper we explore an opportunity to identify if property tax is capitalized into housing prices. In São Paulo City the property tax was reformed in 2001 (effective on 2002), changing from a uniform to progressive rate. It is a good identification opportunity because there is no reason to believe that the change in the tax structure was connected to local expenditure. In other words, there is no reason to believe that at the same time expenditures were changed in terms of income groups. So, in theory, we do not face the traditional problem of identification: it is not possible to decompose the effect on housing prices from the change in taxes and the effect from the change in expenditure (Yinger et al. 1988).

The analysis adapts the difference-in-difference approach using a continuous treatment variable. We need to adopt such a strategy because the change in the tax rate was not continuous except for the lowest brackets. Part of the properties was exempt, and another part reduced their rate from 1 percent to 0.8 percent. For rates above 0.8 percent the average rate increases continuously although the marginal rate changes in steps (up to 1.6 percent).

We also analyze just the first two brackets comparing properties that were exempt with properties that reduced the tax rate to 0.8 percent and use the more traditional difference-in-difference approach with a control group and a treatment group. However, the idea of using a continuous treatment is attractive since it is more unlikely that something would be happening in time for each different group that would lead us to confound the impact from the tax reform with something not observable by us. We must keep in mind that those two groups in the first brackets have a larger weight on the main (continuous) estimation because: 1) they represent 58 percent of the sample (see Table 2) and 2) the difference in the rate is more pronounced for the exempt group. So, it is curious that we got a different result analyzing just those apartments than we got from the whole sample.

Our initial econometric approach brought strong evidence of capitalization of the property tax on housing prices. The coefficients are highly significant and the magnitude considerably large. Our estimations pointed to 9 percent capitalization for a 1 percent reduction in tax. This magnitude is compatible with full capitalization discounted at an 11 percent real rate. Since cadastral values are usually below market value in São Paulo City we may have full capitalization with a 6 percent discount rate that is a very reasonable value.

Testing for robustness of our results revealed that we have to indeed be careful with them. Constraining the sample to years closer to the tax reform reveals capitalization going down as we reduce the sample, but it was significant and within a reasonable range for the magnitude. Using a placebo for the year of the reform reinforces our findings. The placebo did not affect housing price showing that our results are more credible. This result makes us believe that there might be a capitalization effect.

If there is capitalization, the welfare gain of the poor with progressivity is debatable. A family that does not own its house is certainly worse off with exemption than with the uniform rate. This family will have to pay in advance, let us say, 10 percent more for the house instead of paying an equivalent perpetuity. Even in a world with no credit restriction there is no loan that

will have to be repaid in such a long term. It is true that a loan with a 30-year term is like perpetuity. But we do not live in a credit-unconstrained world and 30 years credit in Brazil is very far from reality. This is the main problem in social housing, the lack of credit for low-income households. Even ambitious programs such as *Minha Casa Minha Vida* are facing big problems to reach the very poor. So, exemptions in property tax may hurt the very group it is intending to benefit.

The difficulty in reversing this policy is that in Brazil (and São Paulo is not an exception), 75 percent of the households declare to live in their own house. This proportion is (more or less) uniform for any income class. So, Brazil is a country of landowners. From a landowner perspective, she gained 10 percent in wealth with the exemption. So, progressivity in property tax may be hurting poor tenants and those families attempting to enter the real estate market but those are the minority even among the poor.

There are two interrelated problems with this way of reasoning. The first is that declaring to live in their own house does not mean that the family has a title of the land. A considerable part of the poor declaring to live in their own house live in an informal settlement. Households living in informal settlements in theory are not enforced to pay property taxes, although there are some anecdotes about households that do not have a title but pay property taxes. If this group does not pay taxes they are not affected by progressivity and consequently gain nothing from the policy. Second, although poor families attempting to enter the housing market may be the minority, those families will be forced into the informal market if the formal market price has increased. Consequently, exemption may be contributing to the growth of informal settlements.

In short, with significant capitalization it is difficult to believe that progressivity in property tax is a sound policy. Notice that we have not considered the dead weight loss connected to the substitution effect for the group that had its (marginal) tax rate increased (and for the substitution effect the relevant tax is the marginal). The possible benefit for equality is partially vanished with capitalization. It may be even worse for the poor. So, with capitalization, progressivity might be perverse. Local governments might be aware of these facts before deciding to reform their property tax structure.

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Appendix: Detailed Results from Regressions

Table A1: Detailed Regression Results for Different Specifications

Variable	Model 1 b/se	Model 2 b/se	Model 3 b/se	Model 4 b/se	Model 5 b/se
$\square_3$	-0.392***	-0.180***	-0.177***	-0.189***	-0.186***
	(0.034)	(0.017)	(0.017)	(0.018)	(0.018)
$\square_2$	-1.284***	-0.503***	-0.420***	-0.415***	-0.423***
	(0.025)	(0.015)	(0.015)	(0.015)	(0.015)
$\square_1$	0.059**	-0.096***	-0.090***	-0.081***	-0.040***
	(0.02)	(0.01)	(0.01)	(0.01)	(0.012)
Room		0.246***	0.145***	0.145***	0.145***
		(0.005)	(0.006)	(0.006)	(0.006)
Floor Area		0.004***	0.003***	0.003***	0.003***
		0	0	0	0
Distance to CBD		-0.195***	-0.172***	-0.178***	-0.172***
		(0.007)	(0.007)	(0.007)	(0.007)
Price System		-0.023	-0.120***	-0.121***	-0.123***
		(0.02)	(0.019)	(0.019)	(0.019)
Garages			0.237***	0.235***	0.233***
			(0.007)	(0.007)	(0.007)
Common Area			-0.000**	-0.000**	-0.000***
			0	0	0
Elevator			0.271***	0.272***	0.264***
			(0.02)	(0.02)	(0.02)
CEPAC Area				-0.093***	-0.079***
				(0.019)	(0.019)
Rail Station				0.017	0.021
				-0.013	-0.013
Density				0	0
				0	0
National Interest Rate					0.006***
					(0.001)
National GDB growth					0.024
					(0.091)
Constant	13.444***	12.375***	12.017***	12.045***	11.902***
	-0.016	-0.018	-0.027	-0.03	-0.036
R-sqr	0.561	0.894	0.911	0.911	0.912
N	6562	6558	5349	5346	5344
BIC	11721.3	2423	843.9	843	815.3

** p<0.05, ** p<0.01, *** p<0.001*

Source: Embraesp, São Paulo Cadastral Office and IBGE

Table A2: Detailed Regression Results for Different Samples

Variables	1996-2007 b/se	1997-2006 b/se	1998-2005 b/se	1999-2004 b/se	2000-2003 b/se	2001-2002 b/se
$\square_3$	-0.188***	-0.176***	-0.145***	-0.109***	-0.075**	-0.055
	(0.017)	(0.019)	(0.02)	(0.022)	(0.026)	(0.038)
$\square_2$	-0.429***	-0.431***	-0.425***	-0.485***	-0.509***	-0.576***
	(0.015)	(0.017)	(0.019)	(0.021)	(0.025)	(0.037)
$\square_1$	-0.036**	-0.021	-0.027*	-0.01	0.049*	
	(0.012)	(0.013)	(0.013)	(0.014)	(0.025)	
Room	0.173***	0.167***	0.150***	0.149***	0.136***	0.155***
	(0.006)	(0.007)	(0.007)	(0.008)	(0.01)	(0.014)
Floor Area	0.003***	0.002***	0.003***	0.003***	0.003***	0.003***
	0	0	0	0	0	0
CBD Distance	-0.203***	-0.215***	-0.211***	-0.204***	-0.192***	-0.169***
	(0.007)	(0.008)	(0.009)	(0.01	(0.012	(0.019
Price System	-0.104***	-0.080***	-0.095***	-0.111***	-0.091*	-0.109*
	(0.02)	(0.023)	(0.024)	(0.028)	(0.036)	(0.054)
Garages	0.195***	0.205***	0.162***	0.147***	0.161***	0.136***
	(0.007)	(0.007)	(0.008)	(0.009)	(0.012)	(0.018)
Common Area	0.000***	0.000**	0.001***	0.001***	0.001***	0.001***
	0	0	0	0	0	0
Density	0	-0.000*	0	0	-0.000**	0
	0	0	0	0	0	0
Rail Station	0.019	0.009	0.014	0.007	0.017	0.032
	(0.014)	(0.015)	(0.016)	(0.018)	(0.022)	(0.033)
CEPAC Area	-0.073***	-0.135***	-0.120***	(omitted)		
	(0.02)	(0.024)	(0.034)			
Interest Rate	0.006***	0.008***	0.007***	0.004**	-0.015**	0.008
	(0.001)	(0.001)	(0.001)	(0.001)	(0.005)	(0.013)
GDB growth	0.027	0.042	0.115	0.22	0.182	0.356
	(0.091)	(0.102)	(0.109)	(0.124)	(0.151)	(0.216)
constant	12.187***	12.168***	12.193***	12.293***	12.640***	12.157***
	-0.031	-0.035	-0.036	-0.041	-0.092	-0.253
R-sqr	0.912	0.913	0.922	0.924	0.925	0.932
N	5536	4480	3578	2772	1895	894
BIC	1238.3	1146.9	611.6	519.2	387	191.7

** p<0.05, ** p<0.01, *** p<0.001*

Source: Embraesp, São Paulo Cadastral Office and IBGE

Table A3: Detailed Regression Results for Different Specifications Restricting the Sample to Years Before the Tax Reform (1995-2002) and Placebo for the Tax Reform Year (1998)

	Model 1 b/se	Model 2 b/se	Model 3 b/se	Model 4 b/se	Model 5 b/se
$\square_3$	-0.188***	-0.114***	-0.101***	-0.096***	-0.094***
	-0.047	-0.023	-0.022	-0.022	-0.021
$\square_2$	-1.178***	-0.482***	-0.379***	-0.380***	-0.380***
	-0.035	-0.02	-0.019	-0.019	-0.019
$\square_1$	-0.017	-0.053***	-0.074***	-0.080***	-0.053***
	-0.03	-0.015	-0.013	-0.014	-0.014
Room		0.204***	0.119***	0.120***	0.117***
		-0.008	-0.008	-0.008	-0.008
Floor Area		0.005***	0.003***	0.003***	0.003***
		0	0	0	0
CBD Distance		-0.127***	-0.114***	-0.112***	-0.111***
		-0.01	-0.009	-0.009	-0.009
Price System		-0.071**	-0.124***	-0.125***	-0.132***
		-0.025	-0.023	-0.023	-0.023
Garage			0.186***	0.186***	0.188***
			-0.011	-0.011	-0.011
Common Area			0.001***	0.001***	0.001***
			0	0	0
Elevator			0.365***	0.364***	0.356***
			-0.03	-0.029	-0.029
Rail Station				0.060*	0.068**
				-0.026	-0.026
Density				0	0
				0	0
GDP Growth					-0.088
					-0.117
Interest Rate					0.009***
					-0.001
constant	13.452***	12.336***	11.881***	11.883***	11.659***
	-0.022	-0.025	-0.038	-0.041	-0.049
R-sqr	0.515	0.887	0.911	0.911	0.913
dfres	2846	2842	2519	2517	2515
BIC	4690.7	563.2	-162.3	-152.2	-198.5

Source: Embraesp, São Paulo Cadastral Office and IBGE