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The decision on unconstitutionality of earmarking and its impact on the housing access: Evidence from São Paulo State, Brazil

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An important challenge in the earmarking literature is to isolate causal effects of earmarking decisions on public goods supply. Following the Brazilian Supreme Court's decision on unconstitutionality of São Paulo State's earmarking policy to face housing deficit, in September 1997, earmarked resources to fund the CDHU were forbidden. The Supreme Court's decision created an uncertain housing policy scenario,especially, to households with a housing deficit. The Supreme Court intervention can be assumed to be exogenous to a housing deficit econometric model. The decision on unconstitutionality of the earmarking mechanism for housing construction by the Brazilian Court allows identifying an exogenously determined pre and post treatment situation, with the households between zero and five-minimum wages and households between five and six-minimum wages defining the treatment-group and control-group, respectively, along the lines of the eligibility rule by the CDHU. We apply a difference-in-difference empirical model with non-linear estimators. Our results suggest that low-income households under uncertainty face an average increase of 1.8% in the probability to remain in a housing deficit condition and indicate that low-income households depend on the government earmarking policy.

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1. Introduction

The Universal Declaration of Human Rights considers housing as a fundamental human right since 1948. However, housing in Brazil only became a social right in 2000 following the Brazilian Constitutional Amendment 26. This good has an important role in the well-being of individuals and families, not only as a good per se, but also due to the access to other goods and services that it provides, such as public transportation, sanitation, public health, and safety [Campos and Guilhoto, 2017]. Better housing conditions have a direct impact on long-term human capital [Rothwell and Massey, 2015], which in turn affects firm productivity, have the potential to support sustainable routes out of poverty by enabling economic and social opportunities, and integrates less-skilled classes into the urban economic system [Turok, 2016].

Housing goods relate productive activities (private investments) and social fixed capital (public investment), as discussed by Hirschman, 1988; then, housing bottlenecks directly impact firms productivity. Urban Economics literature is concerned with this relationship and addresses theoretical models underlining how housing provides access to the labor market and to goods and services in urban areas that improve consumers' utility [Alonso et al., 1964,Mills, 1967,Muth, 1975,Fujita and Ogawa, 1982,Anas and Kim, 1996,Wasmer and Zenou, 2002,Brueckner and Zenou, 2003]. Additionally, many studies on American cities discuss the impact of public housing policies on labor markets, education, private housing markets and migration [Murray, 1977,Susin, 2002,Sinai and Waldfogel, 2005,Field, 2007], but none, to our knowledge, have focused on the effect of earmarking mechanisms on housing deficit. Therefore, we focus on the evaluation of earmarked revenues to deal with the housing deficit in the State of São Paulo, Brazil.

Earmarking is the procedure of setting aside certain taxes or Government revenues for a specific program. Understood initially as rigidity imposed on government action, which would compromise the optimality of government revenue allocation, earmarking taxes meanwhile has been highlighted as a feasible policy mechanism, especially, after Buchanan, 1963. Following this paper, many other authors discussed the benefits of this practice. Regarding the case of Brazil, in which earmarking taxes is a common policy, the Brazilian Federal Constitution of 1988 earmarks a large part of tax revenue to expenditures considered essential, such as education, public health and social care. The one analyzed here, low-income housing programs, however, is not included in this category.

In recent Brazilian economic history, the discussion on housing public policies is associated with housing deficit issues, due to its contribution to improving well-being. Housing policies developed in Brazil have occurred concomitantly to rapid urbanization [Valladares, 1983]. This process accelerated since 1950 with the rural exodus, the acceleration of industrialization, and fast population growth in Brazil, which required the expansion of the housing supply [Santos, 1999, de Mello and Novais, 2009]. In the State of São Paulo, the heavy inflow of migrants has resulted in annual migration rates above the national average between 1930 and 1970 [Cano, 1997]. To overcome the housing deficit, important policies have included the decree-laws during the Vargas period (1930-1945), the Housing Financial System (SFH)[1] and the Housing National Bank (NBH) [2]. [Campos and Guilhoto, 2017]. All of these public policies were promoted at the federal government level.

Although public policies promoted at the federal level focus on multiple income ranges (social classes), the creation of the *Urban and Housing Development Company* (CDHU) [3] in 1949 focused in particular on low-income housing at the state level (São Paulo). However, macroeconomics crisis, since the collapse of the dictatorial government, have reduced housing funding to housing programs in the State of São Paulo [Oliveira, 2002], driving the state government to incorporate earmarked taxation to fund housing construction.

Under this context, housing policy has been developed by the state government with own financial resources and rules. The funding for the program stemmed from state government's fiscal power and public spending decisions in social areas. Under these circumstances, the state government of São Paulo enacted the State Law 6,556 from November 1989 that expanded the Tax on Services and Merchandises Circulation (ICMS). The ICMS tax increased from 17% to 18%, marking the incremental part for financing the housing construction. On the one hand, this increase provides funding to CDHU; but, on the other hand, this measure deepens inequality due to the bigger relative impact on low-income households than on higher-income households.

This paper looks for evidence for this relationship by examining the effect of earmarking mechanisms on the probability of remaining in housing deficit. A major obstacle to measuring this causal effect is the endogeneity between state government's funding condition and housing access. In the case of São Paulo's State Law 6,556 from November 30^{th}, 1989, earmarking tax revenues may have been an easy alternative for the local authority to justify the increase in tax rates, given that the incremental tax revenue is legally designated to housing programs, which aims to benefit the population. Hence, the local population may approve the respective increase in tax rates more willing than if the increase had no certain benefit to them.

To overcome endogeneity, we explore the earmarking unconstitutionality decision by the Brazilian Supreme Court in September, 1997. Since 1998, the earmarked aliquot can be directed to whatever the state government decides, given that Supreme Court did not change the ICMS rate. Clearly, the judges' decision breaks the aforementioned links and enables us to isolate the causal effects of earmarking on the housing deficit. Following the decision, low-income households began facing a higher taxation rate on their goods and services consumption plus uncertainty in housing investments that are directed by the state government.

Using data from the Brazilian National Household Sample Survey (PNAD) [4],. we assess whether the State of São Paulo is committed to confronting the housing deficit associated with the household uncertainty, thereby shedding light on the housing deficit condition of the low-income households. We are testing whether the earmarked mechanism rolls as a commitment tool for governments, or, the dual form, how sensitive low-income household's housing demand is to earmarked revenue changing.

The decision on unconstitutionality by the Brazilian Supreme Court enables identifying a pre and post

[1] Sistema Financeiro de Habitação.
[2] Banco Nacional de Habitação.
[3] Companhia de Desenvolvimento Habitacional e Urbano.
[4] Pesquisa Nacional por Amostra de Domicílios

treatment period. Households between zero and five minimum wages and households between five and six minimum wages define a treatment-group and control-group, respectively, according to the eligibility rule of the CDHU. Variation in the earmarking policy does not affect the control-group, serving as a quasi-control group for eligible households who may experience changes in the housing deficit condition. This structure allows us to estimate the difference-in-difference empirical model, using non-linear estimators with a binary outcome.

This paper contributes to the existing literature by connecting Urban Economics and Public Finance literature on housing access in two particular ways. First, we examine earmarking as a mechanism of reducing housing inequality, while extending the existing literature on housing consumption by considering channels of welfare gains derived from uncertainty reductions. Secondly, this paper contributes to the Public Finance debate on earmarked taxes dealing with causal effects identification.

The resulting estimates of unmarked tax impact suggest that low-income households under uncertainty face an average increase of 1.8% on the probability to continue in the housing deficit condition. Estimating for income-groups, households who earn a monthly income between one and two minimum wages present a higher sensitivity to an earmarked tax (5.9%), while households that earn between four and five minimum wages face a lower probability (1.3%). We also estimate the baseline models taking into account heterogeneity in response. Male headed-households face larger probability (2.8%) to remain without home ownership than female headed-households (1.3%). Among migrants and non-migrants, the probability to remain in the housing deficit condition increases by 2.2% and 1.2%, respectively. Analyzing households that live in the Metropolitan Area of São Paulo, Representative Municipalities and Non-Representative Municipalities, the highest probability estimated is related to households living in the former area (3.1%). These effects provide evidence that low-income households depend on the government subsidy.

In the next section of the paper, we briefly discuss earmarking taxes and empirical approach to evaluate its impact. In section 3, we discuss the background to the housing programs in Brazil and in the State of São Paulo as well as legal framework that supports the earmarking mechanism. In section 4, we develop a simple model of housing demand under uncertainty and how uncertainty on subsidy affects household utility. Section 5 describes our database, and section 6 discusses our empirical strategy of identification. Section 7 presents the main results, heterogeneity in response estimations and robustness tests. Finally, section 8 offers concluding remarks.

2. Earmarking taxes

The Public Finance literature discusses advantages and disadvantages of earmarking revenues, leaving the debate open for further theoretical and empirical discussions. One of the advantages of earmarking taxes is the lower political cost when increasing tax rates [Michael, 2015, Bös, 2000]. In this case, the population may accept more willing the increase when they know the destination of the incremental tax revenue. Another advantage is the transparency of government actions [Michael, 2015]. With this mechanism, the population has a guarantee that certain areas will receive public investment independently of governors, thereby, contributing to the knowledge on the policies of the governor. Earmarking also has the opportunity of reducing agency problems regarding provision of public goods, facilitating tax payers' monitoring of spending level [Dhillon and Perroni, 2001]. This may also keep some crucial decisions away from the political arena [Michael, 2015].

As Nguyen-Hoang and Duncombe, 2012 argue when analyzing this mechanism for financing highways in the United States, if expenditure in this area is earmarked to a more stable source of revenue (e.g. transportation-related sources) than income or sales taxes, then earmarking may reduce the volatility of highway investment. Therefore, this may also represent an alternative for reducing the variability of investment in relevant areas such as education and health.

Even though some arguments may be presented in favor of earmarking taxes, many authors criticize this mechanism [Oakland, 1984]. Against this practice, one may point out that earmarks are not frequently re-evaluated according to the need of investment in the area analyzed, which may lead to overinvestment or underinvestment, i.e. a non-optimal allocation of resources. Clague and Gordon, 1940 mention that earmarking gasoline taxes in the United States resulted in overinvestment in highways, to the detriment of other important areas. Besides, depending on the taxes earmarked and the level of government linked to this revenue, this practice may represent an obstacle in reforming the tax system [Michael, 2015].

Estimating the impact of earmarking on spending and on other outcome variables is not an easy task due to an endogeneity issue regarding the implementation of this mechanism: by deciding to set aside a part of the total revenue to a certain purpose, the government may have the intention to increase spending in this area. Therefore, an eventually verified higher spending in the focused area may be due to government's concern for this area, not to the mechanism itself. For this reason, treating this endogeneity is crucial when estimating the effect of earmarking to identify its impact.

The empirical effects of earmarking tax revenues on the amount invested in the focused public policy, as verified by some papers, are ambiguous. For example, Dye and McGuire, 1992 find that earmarking may actually have no effect or reduce the level of total spending, especially on highway investment. Using Ordinary Least Squares (OLS) regressions, the authors estimate the increase on spending for every extra dollar earmarked in the United States in 1984 and 1988. To control for the endogenous effect mentioned above, Dye and McGuire, 1992 use lagged total state taxes earmarked per capita as instrument to develop a Two-Stage Least Squares (2SLS) regressions. The little or null effect of earmarking persists even after this procedure.

Nguyen-Hoang and Duncombe, 2012, in turn, do not consider Dye and McGuire, 1992's instrument as appropriate to control for the mentioned endogeneity. To analyze the effect of earmarking revenues for highway funding on the volatility of highway budget in the United States, the authors use three and four-year lagged federal highway apportionment as instrument for federal highway grant expenditure. After performing a Hansen J-test to confirm the exogeneity of the instruments, Nguyen-Hoang and Duncombe, 2012 use a 2SLS estimator.

In our paper, instead of analyzing the effect of earmarking on the amount invested, we address the outcome related to the mechanism studied, the housing deficit. As another distinguishable feature of this paper, our approach controls for the endogeneity as follows: instead of comparing the moments before and during the earmarking validity, we address, in our empirical strategy, the moments during and after the mechanism. Given that the State Government's policy analyzed ended in 1997 by a Brazilian Supreme Federal Court's decision, we consider this impact as exogenous, in the way that the end of the mechanism was not defined by the same institution responsible for spending in the area focused.

3. The Brazilian Public Housing Policies and Earmarking Mechanism

In this section, we focus on the public housing policies at the federal and state government level (São Paulo) in Brazil, providing a brief background to the mechanisms developed to foster social housing. The main public housing policy originates from the creation of the Low-income Housing Foundation (FCP)[5]'s and Social Security's housing portfolios to encourage the low-income housing supply, whose origin dates to the 1930s [Bonduki, 1994]. Such measures aimed at providing labor supply in urban areas in a context of urban industrialization [Bonduki, 1994, Oliveira, 2002].

Until 1964, public housing programs were limited to those mechanisms, which were facing funding restrictions [Yoshimura, 2004]. From 1964 onwards, the federal government enacted the Law 4,380 setting up the Housing Funding System (SFH) [6]. In view of creating a national housing program, the federal government stimulated the creation of the National Housing Bank (BNH) [7] and the Guarantee Housing Fund

[5] Fundação Casa Popular.
[6] Sistema de Financeiro da Habitação.
[7] Banco Nacional da Habitação.

for Employee (FGTS) [8] as funding resources to confront the housing deficit and increase home-ownership growth rate for low-income households [Yoshimura, 2004, Campos and Guilhoto, 2017].

However, between 1986 and 2002, no advances were made in the housing programs. Federal public policies were not focused on the housing sector; instead they were comprised for overcoming fiscal imbalances, control inflation, reduce external vulnerability inherited from the dictatorial governments (1964-1984), put in order the political arena after the president impeachment in 1992, and handle the foreign-exchange crisis (1999) [Yoshimura, 2004, Federal, 2012, Campos and Guilhoto, 2017]. The only two housing programs launched during the 1990s did not meet the expectation due to the fiscal adjustments of the macroeconomic policy [Yoshimura, 2004].

The state level's public housing policy measures need to be understood within such context and the relationship between the federal and state level public housing policies. For the state level analysis, the discussion is restricted to social housing, given the focus of this research. Oliveira, 2002 points out that the housing policy developed by the state government had been integrated into the national housing system until the mid-1980s. Evidently, the period of macroeconomic instability negatively affected low-income housing policy at the state level. Due to the aforementioned fragilities of the federal housing policy and the administrative discontinuities, there was a low degree of planning and weak integration with other urban policies. Yet local practices have been strengthened with regard to combating the housing deficit [Cardoso et al., 2013].

Under an emerging low-income housing policy, the State of São Paulo created the so-called CDHU in 1949. CDHU is a state government company hierarchically connected to the Housing Department. The CDHU's financial history can be divided into three stages. The first stage (1967-1983) was linked to the federal funding apparatus, the second one (1984-1989) can be considered as a transitional period, and the third one (1990-2000) as funding based on earmarked state resources [Oliveira, 2002].

Focusing on that last phase, state funds stemmed mainly from the rise (from 17% to 18%) in Tax on Services and Merchandises Circulation (ICMS) [9], according to the State Law 6,556 from November 1989. The incremental tax revenue, related to the increase on the ICMS, would be designated to raise capital for the State of São Paulo Bank (CEESP) [10], which could be used solely for financing housing programs. Those programs were primarily designated to low-income households (with monthly incomes less than 5 minimum wages[11]), with monthly payments related to the subsidized financing not exceeding 20% of their income. In December 27^{th}, 1990, the State Law 7,003 included two more banks [12], which would receive the incremental tax revenue, to invest in housing programs.

Later, the State Law 7,646 of December 26^{th}, 1991 renewed the mechanism for one more year, albeit reducing the allowed ratio of the payment from 20% to 15% for incomes of households receiving less than 3 minimum wages. The 20%-ratio remained the same for households receiving between 3 and 5 minimum wages. In the following years, the tax revenue earmark was renewed. The last changes in the Law occurred in December 27^{th}, 1995, when the State Law 9,331 designated 5% of the revenues to urbanizing slums, and in December 20^{th}, 1996, renewed the earmark for one more year.

Contrary to the State Law mentioned, the Brazilian Federal Constitution (FC) of 1988 forbids earmarking tax revenues to an agency or fund, or to expenses not related to public health or education (Braz. Const. art. 167, §4 (1988)). For this reason, on September 18^{th}, 1997, the Brazilian Supreme Federal Court declared

[8] Fundo de Garantia por Tempo de Serviço.

[9] Imposto sobre Circulação de Mercadorias e Serviços.

[10] Caixa Econômica do Estado de São Paulo.

[11] The program also provides low-income houses for households that earn between five and ten minimum wages. However, households from this group have no priority. According to Oliveira, 2002, 62% of borrowers earn between 0-3 minimum wages, 23% earn between 3.1 and 5 minimum wages and 15% earn between 5.1 and 10 minimum wages. Then, 85% of families compose low-income households, proving that the low-income households' priority has been enforced, as provided by the State Laws 7,003 and 7,646.

[12] Nossa Caixa Nosso Banco and Banco do Estado de São Paulo

that the State Law 6,556 of the State of São Paulo was against the FC, and housing programs do not meet the art. 167 §4 defined type of binding tax revenues, along with the destination of the revenue to an agency such as CEF and CDHU.

In practice, the mechanism was feasible in providing low-income households access to the housing market by reducing the indirect costs on housing prices (indirect subsidy), providing temporary and regressive monthly discounts on payment instalments (direct subsidy) for up to 23 years, and distributed accordingly to households' monthly income [Oliveira, 2002].[13]

The target clientele of the programs was based on "open demand". People enrolled in the programs who were interested in the acquisition of housing, after they were made aware of its by public notices. In the selection process, the applier needed to gather information on their household's monthly income, personal documents and provide a mailing address. The selection process is based on random selection, by lot.

Table 1 provides the number of houses constructed between 1986-2000. A large increase of housing supply is noticiable and the total number of municipalities attended by the CDHU program after the tax-earmarking (year 1989). However, after the Supreme Court's decision the housing construction and number of attended municipalities' growth rate decreased in comparison to earmarked period.

Low financial resources and high construction costs might explain the housing supply decrease between 1995 and 1999. We focus on the financial resources by funding source and on the construction cost. Table 2 shows the large relative participation of ICMS-Housing on total revenue. After 1997 the ICMS-Housing relative participation on funding sources decreased due to borrowers' payment increases. Additionally, the ICMS-Housing amount transferred from the state government to the housing program decreased year after year, resulting in a real total revenue decrease. On the investment cost side, Table 3 provides the annual investment amount by cost structure. Shortly, these data show that housing construction investment has been presenting greater decreasing rates, year after year, since 1997.

Even though the government had to transfer at least 0.75% of total ICMS state collection to promote housing public policy, Table 4 shows that the relative participation of ICMS-Housing on the total ICMS collection by the State of São Paulo is greater than that enforced by the law. However, after the unconstitutionality decision, the relative participation continuously decreased yearly. Clearly, the decreasing behavior is a consequence of macroeconomic issues affecting the total ICMS collection, but the ICMS-Housing decreasing rate was dropping faster than the total collection rate, as shown in Table 4. Moreover, while housing construction growth rate was increasing until 1998, the investment growth rate was decreasing since the Supreme Court's announcement. This last discussion about CDHU revenue, source of revenues and investment provides some evidences on the impact of the Supreme Court's decision on the low-income housing construction in the State of São Paulo.

Therefore, to analyze the impact of earmarked state government revenue on the housing deficit in São Paulo, we have to compare the housing funding situation in different moments, pre and post the state government decision to earmark the revenues. However, the earmarking mechanism used as a public policy reflects the state government's need for funding to face the decreasing funding transfer from the federal government. In other words, the State Law 6,556 is endogenous to the state government decision and would not identify the actual impact on the housing deficit situation, as aforementioned. Then, in order to isolate the exogenous component of earmarking mechanisms, we instrument this mechanism by using the Federal Court decision, exploring pre and post SFC decision.

[13] The indirect cost is different in the Metropolitan Area compared to the rest of the state. House construction costs in the Metropolitan Area are higher than outside due to the indirect construction costs, such as landscaping, infrastructure, sanitation, electricity and equipment costs contracted by the municipality or state government. This indirect subsidy reduces the final prices significantly in the rest of the State of São Paulo.

4. Conceptual Framework

In this section, we present a simple theoretical model whose main function is to show the direct impact of earmarking on housing consumption under uncertainty. We assume that there are only housing and non-housing goods, and households make their decisions on consumption based on prices, budget constraint and preferences. From a Cobb-Douglas utility function, we may represent household utility as:

$$U_{i,t}(H_{i,t}, C_{i,t}) = H_{i,t}^{\alpha_i} C_{i,t}^{1-\alpha_i} \tag{1}$$

with $H_{i,t}$ representing housing goods consumed by household i in t, $C_{i,t}$ non-housing goods consumed by household i in t and α_i the fraction of income spent on housing goods by household i, exogenously defined. Budget constraint is defined as:

$$\mathrm{E_t}(P_{i,t}^H)H_{i,t} + P_t^C C_{i,t} \leq W_{i,t} \tag{2}$$

with $W_{i,t}$ representing household income in t, $\mathrm{E_t}(P_{i,t}^H)$ the expected housing goods price in t to household i, and P_t^C non-housing goods price, which we normalize in 1. Maximizing utility, household consumption on housing goods is:

$$H_{i,t} = \frac{\alpha_i W_{i,t}}{\mathrm{E_t}(P_{i,t}^H)} \tag{3}$$

Housing policy affects household decision through expected prices. Given that housing consumption represents a cash flow of monthly payments, although household decisions are made in one period, this process is based on expected total cost in buying a house. Moreover, households take into account their expectation of the government's commitment on housing subsidies, which may be influenced by the announcement of an earmarking policy:[14]

$$\mathrm{E}_t(P_{i,t}^H) = \sum_{j=t+1} \frac{m[1 - \mathrm{E}_t(S_{i,j}|A_{j-1})]}{(1+r)^t} \tag{4}$$

With m representing a constant monthly cost of housing, $\mathrm{E}_t(S_{i,j}|A_{j-1})$ is the expectation in t for housing subsidy to household i on j, given the previously announced earmarking policy (A_{j-1}), and r is the constant discount rate. A_{j-1} is defined as:

$$A_{j-1} = \begin{cases} 0, & \text{if the earmarking policy for } j \text{ was announced in } j-1 \\ 1, & \text{otherwise} \end{cases} \tag{5}$$

Assuming that, in the given period households make their decision, they expect the announced earmarking policy to continue for the following periods, and the expected subsidy to be constant throughout payment periods, implies $\mathrm{E}_t(S_{i,j}|A_{j-1}) = \mathrm{E}_t(S_i|A_{t-1})$. Using this hypothesis, expected housing price is given by:

$$\mathrm{E}_t(P_{i,t}^H) = \frac{m[1 - \mathrm{E}_t(S_i|A_t)]}{r} \tag{6}$$

From (3), we may verify that the impact of the expected subsidy on housing consumption is positive:

$$\frac{\partial H_{i,t}}{\partial[\mathrm{E}_t(S_i|A_{j-1})]} = \frac{\alpha_i W_{i,t} r}{m[1 - \mathrm{E}_t(S_i|A_{j-1})]^2} \tag{7}$$

However, the impact of earmarking on housing consumption depends on how earmarking affects expected

[14] For mathematical simplification purposes, we assume that households make their first payment in $t+1$.

subsidy:

$$\frac{\partial[\mathrm{E}_t(S_i|A_{j-1})]}{\partial A_{j-1}} \lesseqgtr 0 \tag{8}$$

From (6) and log-linearizing (3), household decision is:

$$\log H_{i,t} = \log W_{i,t} + \epsilon_{i,t} \tag{9}$$

with

$$\epsilon_{i,t} = \log \alpha_i - \log[1 - \mathrm{E}_t(S_i|A_t)] - \log m + \log r \tag{10}$$

In our empirical estimation, we analyze different groups of household, based on their annual income.[15] Therefore, in our empirical analysis, we observe:

$$\sum_{l \in G_n} \log H_{l,t} = \sum_{l \in G_n} \log W_{l,t} + \sum_{l \in G_n} \epsilon_{l,t} \tag{12}$$

With $G_n \in G$, G representing the groups of households analyzed. For example, the group of households with income between 0 and 5 minimum wages is G_{0-5}.

5. Database

The databases used in this impact evaluation come from the National Household Sample Survey (PNAD). The survey is conducted by means of a sample of housing units, taken from a master sample, so as to guarantee that results represent the several geographic levels and are collected by the Brazilian Institute of Geography and Statistics (IBGE).[16] Every quarter annually a total of 211,344 permanent private housing units are surveyed, in approximately 16,000 enumeration areas, distributed amongst about 3,500 municipalities.

The PNAD database is aimed at producing continuous information about the participation of the population in the labor market in association with demographic and educational characteristics. It also aggregates information on other permanent surveys (such as child labor, migration, fertility, etc.). Other questions can also be insert in the questionnaire depending uppon demand.

To define the housing deficit variable, we follow *João Pinheiro* Institute[17] methodology while assuming some alterations.[18] According to *João Pinheiro* Institute (JPF)[19], housing need is not restricted to households who did not buy their own house, but also extends to cohabitation (more than one family living in the same house), density dweller per room (more than three people), low-quality of construction materials (harnessed wood, straw and uncoated adobe) and dwelling kind (improvised or collective houses).[20]

The creation of the dependent variable follows a specific algorithm, associating several variables from

[15] Analogously, for a household i out of the income range benefited by the housing program, equation 10 is given by:

$$\epsilon_{i,t} = \log \alpha_i - \log m + \log r \tag{11}$$

[16] *Instituto Brasileiro de Geografia e Estatística* (IBGE), www.ibge.gov.br.

[17] *Fundação João Pinheiro* (FJP), www.fjp.mg.gov.br.

[18] FJP takes into account housing rent participation on household income as a measure of housing deficit. Then, as we disagree about this methodology, we did not input it inside our dependent variable algorithm but as a control for restrained demand. FJP also incorporates garbage destination, piped water, sewage and electricity supply, for example. This kind of amenities might be solved by improving neighborhood infrastructure. Then, if the criteria is not related to housing structure, we dropped it from the dependent variables.

[19] We highlight that our housing deficit variable is not similar to JPF's one because we need the comparability among all of them over several years. The Foundation does not recalculate the index using just comparable variables among all questionnaires of the Census publications. Therefore, JPF approach is not suitable for evaluating the impact.

[20] Additional variables could be taken into account, however, comparability would be jeopardized.

the PNAD database. Firstly, we just calculate housing deficit if it is urban (city, village or isolated urban area). Subsequently, if dwelling kind is private but improvised and collective, or family number is bigger than one, or dweller density by room is bigger than three, then the household composes the housing deficit condition. We assume that households face a housing deficit if they have at least one of the aforementioned characteristics.

Housing demand reacts to income and demographic characteristics [Mankiw and Weil, 1989, Mankiw and Weil, 1991, Quigley and Raphael, 2004]. For the econometric estimation, we use age of the head of household, life cycle, years of schooling, race, migrant and population projection as demographic control characteristics. The life cycle variables were also created using the PNAD database. Li, 1977, Börsch-Supan, 1986, Börsch-Supan and Pitkin, 1988, Haurin et al., 1991, Li and Yao, 2007 pointed out that the life cycle components directly affect housing demand. In a practical way, we categorized four life cycles, such as households that have 1) living settings with a wife/husband or a partner with children, 2) living settings with a wife/husband or a partner without children, 3) does not live with a wife/husband or a partner with children and 4) does not live with a wife/husband or a partner without children. All aforementioned variables only measure information related to the head of the household. Table 5 summarizes the variable descriptions and PNAD codes combination that generate all control and outcome variables. Additionally, Table 6 provides summary statistics for the main variables used in the regressions pre and post the Supreme Court decision, based on the PNAD dataset and weighted by household sampling weight.

In the treatment group, 3.9% of households compound the housing deficit condition under earmarking mechanism, i.e., 398,517 households (HH). After the mechanism of transfer becoming unconstitutional by the Supreme Court, 2.7% of households compounds the housing deficit condition, representing 220.168 households. The pre-treatment group's yearly average is 132,839 households and the post-treatment group's yearly average is 110,084 households. For the control group, between 1995-1997 and 1998-1999 periods, 2% (40,787 HH) and 0.7% (11,225 HH) of households had been facing a housing deficit; a yearly average of 13,595 and 5,612 households, respectively. In other words, while the mean of the housing deficit condition has decreased between the periods 1995-1997 and 1998-1999, the decrease seems to be more pronounced for the control group than for the treatment group. This is consistent with the decreasing of the housing construction and the ICMS transfer's rate, as discussed before.

Additionally, the data also shows that control variables face little variations in terms of proportion pre and post Supreme Court intervention, but all of them are statistically significant at the 1% level. Table 7 (columns 1 and 2) provides additional evidences on the parallel trend hypothesis. We run difference in mean tests on the trend of the curves for treatment and control groups before the Supreme Court intervention. All results do not reject the null hypothesis, supporting the earlier observation that both groups have parallel trends.

6. Identification Strategy

To identify the causal effect of the Brazilian Supreme Court's unconstitutionality decision on the low-income housing deficit, we exploit the variation on earmarking mechanism using the year in which the primary injunction was granted by the Federal Court and the eligible household group. Since not all eligible groups (household's monthly income up to five minimum wage) received a house from the CDHU Program by the time of the survey, we employ an intent-to-treat (ITT) analysis, i.e., all eligible households are compounded to the treated group. Briefly, the results measure household probability to remain in the housing deficit condition after the STF's decision.

Assuming that individuals' behavior is utility maximizing, following the theoretical model, and is related to the expectation on transfer's continuity, then, under the households' expectation for government's transferring continuity, utility $U^a_{i,t}$ is greater than utility $U^b_{i',t}$, with $U^a_{i,t}$ and $U^b_{i',t}$ representing the utility when government is committed and non-committed, respectively, as shown by Equation 1. From Equations 10 and 9 and using the probit approach, for $\forall i \neq i'$, $i, i' \in G_{0-5}$ and $W_{i,t} = W_{i',t}$, the stochastic of $\epsilon_{i,t}$ is assumed mutually independent and identically distributed with extreme value type III. Mathematically, the probability function is:

$$= P(logW_{i,t} + \epsilon^{a}_{i,t} > logW_{i',t} + \epsilon^{b}_{i',t}) \tag{13}$$

With $\epsilon^{b}_{i',t} < \epsilon^{a}_{i,t}$ because under non-commitment the subsidy is not expected.

However, we cannot observe all attributes that affect preferences, and household income is not the only factor impacting utility. Then, the functional representation of observable attributes affecting preferences is well-known as representative utility and it is a typically linear parameter of observable attributes, $X_{i,t}$ [Schroeder, 2010]. Then, the $U^{a}_{i,t}$ probability of being greater than $U^{b}_{i',t}$ probability can be rewritten in the following general manner:

$$P(\beta X^{a}_{i,t} + \epsilon^{a}_{i,t} > \beta X^{b}_{i',t} + \epsilon^{b}_{i',t}) \tag{14}$$

$$P(\epsilon^{b}_{i',t} - \epsilon^{a}_{i,t} > \beta(X^{a}_{i,t} - X^{b}_{i',t})) \tag{15}$$

$$\phi_{\xi}(\xi_{i,t} > \beta X_{i,t}) = exp(-e^{\beta X_{i,t}}), X \in \Re \tag{16}$$

With $\xi_{it} = \epsilon^{b}_{i,t} - \epsilon^{a}_{i,t}$ being the difference in preference related to the unobservable attributes, $X_{i,t} = X^{a}_{i,t} - X^{b}_{i',t}$ reflects the difference between preferences under both the expectation situations described in the theoretical model, and ϕ_{ξ} is a cumulative distribution function, hereafter assumed to be a normal distribution. Given ξ_{it} is unobservable, only the probabilistic statement can be provided [Schroeder, 2010]. Moreover, this last expression is usually written in the following linear form:

$$y^{*}_{i,t} = \beta X_{i,t} + \xi_{i,t} \tag{17}$$

with,

$$y^{*}_{i,t} = \begin{cases} 1, & \text{if } U^{b}_{i',t} < U^{a}_{i,t} - \forall i \neq i' \\ 0, & \text{otherwise} \end{cases} \tag{18}$$

As we are interested in evaluating the impact of unmarked tax on the housing deficit probability, a reference/control group is required. The households in the control group also make their decision on housing consumption, facing the same utility maximization, although without expectations, given that they are not eligible to the program. Thus, the maximization of the expected utility implies that home-owners face greater expected utility ($U^{c}_{i,t}$) than households in the housing deficit condition ($U^{d}_{i',t}$). Then, as discussed above, the outcome variable takes 1 if $U^{c}_{i,t} < U^{d}_{i',t}, \forall i \neq i'$, $i, i' \notin G_{0-5}$, and $W_{i,t} = W_{i',t}$ and 0, otherwise.

Having data on the housing deficit before and after the Supreme Court decision and given that the eligible household condition did not change, we are able to exploit the time variation and define the treatment and control groups in a nonlinear difference-in-difference (DID) empirical approach.

Difference-in-differences estimation is one of the most important identification strategies in applied economic research [Angrist and Krueger, 1999, Athey and Imbens, 2006, Bertrand et al., 2004, Blundell and Dias, 2009, Heckman et al., 1999, Lechner et al., 2011]. However, the housing deficit model leads to a binary outcome and then, according to Ai and Norton, 2003 and Puhani, 2012, one possibility to deal with nonlinearity in the conditional expectation of the outcome is to transfer the difference-in-difference identification strategy to latent variable models like probit or logit [21]. This transfer is needed because the treatment effect is not constant across the treated population when the expectation on the outcome variable is

[21] Clearly, Linear Probability Model (LPM) could be applied; however, according to Horrace and Oaxaca, 2006, the LPM estimator produces estimated coefficients that are biased and inconsistent. Despite the theoretical debate on LMP results, we estimated the empirical specification using LPM and then we checked the predicted probability. As a result, this does not fit within the probability interval, and thus we use a probit estimator.

bounded [Athey and Imbens, 2006]. To address this issue, Blundell and Dias, 2009 and Lechner et al., 2011 propose to apply to the difference-in-difference assumption a constant difference between groups across time, not to observe limited dependent variable itself, but instead, to the unobserved latent linear index.

Therefore, our basic specification is an augmented version of the probit approach discussed above. We aggregate to the Equation 17 the control group and the cross-dummies $MARK_{i,t}$, which identifies the Supreme Court resolution that caused uncertainty in the low-income housing market. The final empirical equation is:

$$y^*_{i,t} = \gamma MARK_{i,t} + \beta X_{i,t} + \alpha\mu_t + \varphi\nu_s + \epsilon_{i,t} \tag{19}$$

Where:

- $y^*_{i,t}$ is the dichotomous household's housing condition in household i and year t, taking 1 for households in the housing deficit condition and 0 otherwise.
- $MARK_{i,t}$ is an indicator variable that takes the value 1 if household i is eligible to the housing program ($Treat = 1$) during the years after the Supreme Court's decision ($A = 1$) and 0 otherwise.
- $X_{i,t}$ is a vector of control variables that vary both across household i and time t such as age, life cycle, income, years of schooling, race, migrant, population projection, Supreme Court decision year dummy and (non)eligible dummy.
- μ_t is a year fixed effect common to all household in period t capturing all time-varying effects common to controls and treatments.
- ν_s is census situation fixed-effects that captures all spatial level factors that vary across metropolitan or self-representative regions but are fixed over time.
- ϵ_{it} is a zero mean variable assumed to be independent of the covariates and fixed effects.

Using households' housing condition (housing need or not) during 1995 to 1999 (PNAD database) as the dependent variable, it gives us a cross-section with five years [22] (pooled probit). We consider 1995 to 1997 to be pre-treatment, while 1998 and 1999 correspond to post-treatment period. Since the Supreme's decision was determined in September, 1997 (PNAD's reference period finishes in September of the current year) and unconstitutionality came into force in 1998, we use 1998 as the threshold period.

Considering a simple nonlinear model, such as probit, treatment effect on the treated at the time of treatment in a nonlinear distribution is:

$$\tau(Treat = 1, A = 1, X) = \phi(\gamma + \beta X_{i,t} + \alpha + \varphi) - \phi(\beta X_{i,t} + \alpha + \varphi) \tag{20}$$

With, $E[U^1|Treat = 1, A = 1, X] = \phi(\alpha\mu_t + \varphi\nu_s + \gamma MARK_{it} + \beta x_{i,t})$, $E[U^0|Treat = 1, A = 0, X] = \phi(\alpha\mu_t + \varphi\nu_s + \beta X_{i,t})$, and ϕ is the conditional distribution function of the standard normal distribution and U^0 is the eligible household utility under the earmarked tax program (pre-treatment) and U^1 is the eligible household utility under non-earmarked tax (post-treatment).

As discussed in Athey and Imbens, 2006, Ai and Norton, 2003 and Puhani, 2012, cross difference in a nonlinear difference-in-difference model does not identify causal effects, as it is used in linear models. Unlike Ai and Norton, 2003, Puhani, 2012 shows that treatment effect on treated is identified by the difference between two cross differences, and the cross difference/derivative in probit model without interaction term is generally nonzero. Hence, the treatment effect is zero if and only if the coefficient γ of interaction $MARK_{it}$

[22] Exceptionally, in 1994 the PNAD research was not produced. Following years are not included in analysis due to massive federal housing policy developed since 2002 and sampling changes after 2000. By doing this, we avoid additional confounding in the econometric estimation.

is zero, and because ϕ is a strictly monotonic function, the sign of γ is equal to the sign of the treatment effect.

The key point of this empirical exercise is that the Brazilian Supreme Court's decision on the housing funding mechanism (earmark tax) used by the State of São Paulo is exogenous to housing deficit policy developed by state government. Judges from the Supreme Court have autonomy and are obliged to protect the constitutional legislation; hence, they have no compromise to decide in favor of state government when the governor infringes the law. Thus, Supreme Court decision provides an exogenous variable that breaks the simultaneous determination of earmarking tax, political interest and funding condition.

Clearly, using the nonlinear approach, one does not obtain the causal effects on demands equations but rather an impact on the predicted probability function; in other words, one does obtain the equation to predict the probability that a low-income household will remain in deficit housing condition after the incremental taxation to finance housing construction is not mandatory anymore. Examples of probit and logit estimations can be found in Li, 1977, who applied a binary logit model on owner/renter decision, while Lee and Trost, 1978, King, 1980 and Rosen et al., 1983 used a binary probit model to estimate housing expenditure.

7. Impact on Housing Deficit

7.1. Main Results

The model discussed in Section 3 does not define the direction of the causal effect on the housing deficit after the Supreme Court's decision due to uncertainty. As aforementioned, the effect is not clear because the resources transferred to the low-income housing program become uncertain, depending on the government's commitment, which can increase, decrease or maintain the funding after the STF decision. We are interested in understanding the impact of unmarked revenue on the probability of low-income households' access to housing.

Table 8 reports our baseline regression results, focusing on households' monthly income up to six minimum wages (the treatment group takes households totalling up to five minimum wages monthly and control group takes households with monthly income between five and six minimum wages). This control group is preferred to the others because we can exploit, to some extent, the income discontinuity of eligible households (treatment group) and those families more similar to eligible households than the richer ones. The coefficients of the difference-in-difference are positive, statistically significant, and robust to control and fixed effect variables changing. The impact of the Supreme Court's decision on housing access probability is 1.8%. In other words, the households probability to remain in housing deficit condition increased under non-earmarking scnario.

We also estimate the baseline model splitting the treatment group into five subgroups by income range. Our objective to split up the treatment group is to examine some different patterns from the econometric estimation analyzing all treatment groups separately. The Group I is made of those households in the income interval [0-1], Group II is between (1-2], Group III is between (2-3] and Group IV and Group V are represented by a monthly income between (3-4] and (4-5] minimum wages, respectively.

Table 9 presents the results from our base line specification by income subgroup. The five columns display the estimated coefficients of the effect of earmarking mechanism's absence on the household's probability to remain in the housing deficit condition by income group. The results suggest a negative effect between the earmarked revenue and the housing deficit condition for all groups. Quantitative impacts are particularly strong for households in Group II (column II) and less important in terms of magnitude for households from Group I (column I).

For Group I, the coefficient corresponds to an increase of 0.39% (0.0038 in absolute terms) in the household's probability to remain in the housing deficit condition. The impact is greater for Group II (5.91%), Group III (2.75%), Group IV (3.10%) and Group V (1.43%) when compared to Group I. Therefore, the results from Tables 8 and 9 suggest that the earmarked budget had a relevant influence on overcoming the housing deficit in the low-income household group, even for all income-groups in the State of São Paulo.

7.2. Heterogeneity in response

Seeking a better understanding of how earmarking mechanism actually works, its strengths and weaknesses, we exploit some dimensions of potential heterogeneity in the responses. Heterogeneity in the responses may be associated, among other factors, to geographic, socio-demographic, or other characteristics and/or initial conditions. In order to shed light on the differential effects of unmarked revenue on the housing deficit, we evaluate that effect by gender, migrants, non-migrants, and households living in the Metropolitan Area, the Representative Municipalities and the Non-Representative Municipalities.

7.2.1. Gender

Table 10, column 6, presents results using the same econometric identification, but estimated separately for male and female headed households. The results from the entire gender subsample estimation (without monthly income-range) reveal that male-headed households suffer a larger impact on their housing deficit probability (2.87%) than female-headed households' probability (1.37%). In terms of magnitude, male-headed household's probability is 2.1 times bigger than female-headed household probability.

We also estimated econometric regressions after splitting the gender subsample by monthly income-group. As shown on Table 10, male-headed households by groups appear as significant and positive signals; however, among female-headed household some estimated coefficients are negative (Group I and II). Quantitatively, the Group II (18.23%) and Group V (1.09%) have the highest and the lowest impact among all male-headed households' results, respectively. In the regressions by female-headed income-groups, Group I (15.79%) has the highest probability to leave the housing deficit condition while the Group III (8.21%) has the highest probability to remain.

Even though by income-group the gender heterogeneity for Group I and Group II housing deficit's probability is decreasing, these tests are also consistent with the previous for all male-headed households and for the Groups III, IV and V and ALL female-headed households. The gender heterogeneity estimations suggest that male-headed households are more dependent on earmarked funded housing policy programs than female-headed household.

7.2.2. Regions

Analyzing geographic regions of the State of São Paulo, we can exploit the heterogeneity in the responses by region, namely the Metropolitan Area of São Paulo (MASP), the Representative Municipalities (RM) and the Non-representative Municipalities (NRM).[23] Table 11 presents results from the regressions identical to those from the main results, but ran separately for each of the three geographic regions. Considering MASP region, the results show an increase by 3.15% (ALL) in the probability of remaining in the housing deficit condition. By income-groups, all results are statistically significant and have the expected signals. Quantitatively, the impact is bigger in MASP than in NRM regions, which are positive and statistically significant. However, the impact of the program for households from RA regions appear as significant coefficient but have negative signals for all regressions, except for the Group II regression. The results provide two clear patterns: in the MASP and NRM regions, the unmarked revenue reduces the household probability of leaving the housing deficit condition, while in the RM region the absence of the housing mechanism is negatively affecting the household's probability to remain in the housing deficit condition. In

[23] We are using the PNAD survey. As discussed in the Dataset Section, we are not able to identify each municipality. The IBGE chooses tract areas randomly that are interviewed during ten years, until the next Census survey, when new tracts are randomized again. The RMSP and the Representative Municipalities always have tract areas chosen, but for the Non-representative Municipalities (NRM) it is chosen randomly among all of NRM.

other words, low-income households that live in the RM regions experienced the opposite impact to those who live in MASP and NRM regions.

7.2.3. Migrants

The State of São Paulo has the greatest migration rate among all states of Brazil. Low-income households from other states are especially likely to experience the housing deficit condition. Then, in order to shed further light on the driving forces behind the impact of the earmarking mechanism, Table 12 shows the results for migrant households and non-migrant households. Each regression uses the same specification as before.

All results are statistically significant and present positive coefficients for all groups, except for Group I (Migrant) and Group II (Non-migrant), which have negative signals. For migrants by income group, households from Group V (0.61%) have a lower probability to remain in the housing deficit condition than households from Groups II (15.06%), III (6.7%) and IV (2.35%). The most impacted group is Group II. For the households from the non-migrant group, the earmark mechanism's absence affects Group IV's probability by 2.36%, which is the highest rate among all income-groups. Focusing on the results from the regressions on the entire sample, the results have signals consistent with the main results. Quantitatively, the impact on the migrant household probability (1.24%) of remaining in the housing deficit condition is smaller than non-migrant household's probability (2.28%).

7.3. Robustness

7.3.1. Pre-existing trend tests

The main remaining concern in our specification is related to unobserved factors in the dynamic behavior of housing deficits over time. The serial correlation in the outcome variable would, for example, undermine the estimated results. According to Bertrand et al., 2004, serial correlation affects the statistical inference in such a way that it become more likely that it does not reject the null hypothesis. Considering this concern, we estimate the baseline model for the years 1997 and 1998 and the years 1997 and 1999, seeking to overcome any serial correlation in the housing deficit outcome.

We estimate the second cohort (1997 and 1999) due to housing construction characteristics in Brazil, i.e., highly time consuming and labor-intensive. Then, some of the house stock showed on Table 1 in year 1999 stems from 1998 housing construction. As the average period of construction is usually greater than 12 months, the cohort 1997 and 1998 would not capture the effect of the unmarked revenues because the current housing construction (year 1998) might still be financed by using funding from 1997 (before the STF decision). In other words, 1998 housing supply could be linked to 1997 funding resources generating misleading conclusions.

Table 13 presents the results dealing with pre-existing trends for both cohorts. The coefficient estimated for the entire sample from the first cohort (column 6) has a negative signal and is statistically significant. By group, the results present positive signals and are statistically significant, except for the coefficient of Group I, which has a negative signal. Although the Groups II-V provide support for the results outlined above, the results from the first cohort can be biased, as discussed. Then, we estimate the baseline econometric model taking into account the second cohort only. The results for the coefficients on the unmarked revenue are all positive and statistically significant, supporting our main results, as shown in Table 13.

7.3.2. Testing similar economies without STF intervention

Our concern is related to earmarking mechanism's empirical identification. It is possible that the difference-in-difference approach is capturing other effects than the absence of the earmarking mechanism. Then, we estimate the baseline model for five Brazilian States, whose economies are similar in some aspects as a robustness test, following Chagas, 2012.[24] We evaluate the coefficients that capture the earmarking mechanism impact for the States of Minas Gerais, Espírito Santo[25], Rio de Janeiro, Paraná and Rio Grande do Sul, in which there were no earmarking changes related to housing policies. The three first states are spatially localized in the same macro-region of Brazil and share spatial characteristics and social-demographic and economics spillovers. Both of the last mentioned states are localized in the south of the country, but have economic and demographic similarities.

We expect to find statistically non-significant estimated coefficients or negative coefficients. Statistically non-significant coefficients would support the hypothesis that nothing else was affecting low-income households probability after 1997 in the State of São Paulo other than the STF's decision, while negative coefficients would be interpreted as a reduction to low-income household's deficit condition in those selected states, opposed to the estimated results for the State of São Paulo.

Table 7 shows the trend test for all the selected states. All the results support the parallel trend hypothesis before the Supreme Court decision. All econometric models estimated provide statistically significant coefficients, but the coefficients' signals were not as expected. The results reported in Table 14 show some groups with positive coefficients, in the States of Minas Gerais (Group II, III, IV and V), Espírito Santo (Group IV) and Paraná (Group V). However, all estimated coefficients for the States of Rio de Janeiro and Rio Grande do Sul are negative, as expected.

Although the robustness tests for the income-groups provide non-expected results, we find more evidence supporting than opposing the main results when we estimate the baseline model for the entire sample. Thus, these results show that low-income households are facing less constraint to move from a housing deficit to a house-ownership condition in the selected states than households living in the State of São Paulo.

Briefly, the results from the econometric models, considering the full sample, provide strong evidence in favor of the opposite effect for households in housing deficit condition found in the State of São Paulo – all of the coefficients' signals are negative (see Table 14). In other words, low-income households living in these selected states are more likely to have their own house than those similar households living in the State of São Paulo after the STF's decision.

7.3.3. Robustness tests for control group

Although the treatment and control group are clearly defined by CDHU criteria, we address additional tests focusing on the control-group. In order to identify any kind of confounding related to the control group definition, we use the same selected states as control group for each of the income-range groups mentioned before and for the entire sample, using the same baseline empirical model. From this empirical exercise, we expect to find the same estimated coefficient's signals in the main results.

By income-groups, the results do not consistently support the main results (Table 15). Some of the coefficients estimated have negative signals for the State of Minas Gerais (Group II), Espírito Santo (Groups I and III), Rio de Janeiro (Groups I, II and III), Paraná (Group V) and Rio Grande do Sul (Group I). The Group IV is the unique income-group consistent among all estate estimations, having a positive signal.

We also use the same approach to evaluate the results when the full sample is considered. The results reported in Table 15 (columns 6) are strongly consistent, supporting the coefficients' signals from the main results. We find all coefficients with positive signals and as statistically significant. Quantitatively, we did not expect to find the same estimated coefficients' magnitude among the main results compared to the baseline,

[24] The author evaluate the impact of tax substitution on the price of pharmaceutical products in the State of São Paulo and uses the States of Minas Gerais, Rio de Janeiro, Paraná and Rio Grande do Sul in comparison to the State of São Paulo as robustness tests.

[25] This state is not similar in terms of economic results compared to the others; however, we evaluate the results for this state due to its spatial localization, i.e., it borders the State of São Paulo.

because the first approach compares different income classes, while, in the latter approach, the comparison is between similar income-groups.

7.3.4. Municipality Housing Policy

We are also concerned about omitted variables. It is possible that governments of municipalities make use of public housing policies to face the housing deficit in the municipality or eventually interrupt those public policies. Then, one of these two decisions affects the outcome variable directly (the probability to have access to housing).

Using PNAD database we are not able to identify each municipality. Even if we could identify municipal-level data, the State of São Paulo aggregates 645 municipalities (2010 Census), and no information on public housing programs at the municipality level is available. To overcome this limitation, we propose two scenarios as follow: First scenario takes a representative city that did not develop a housing program at the municipality level, but the mayor still decided to promote public housing policies immediately after the STF decision. It would influence the housing deficit ratio by reducing the housing deficit of the State of São Paulo. Second scenario takes again a representative city that develops housing program, but the mayor decides to interrupt the program. Clearly, it would influence the housing deficit ratio by increasing the housing deficit of the State of São Paulo. In both scenarios the estimated coefficient by the DID approach would be facing any kind of confounding due to the impossibility to control for municipality housing programs.

Under the first scenario, we would not be sure whether that estimated negative effect (probability to leave housing deficit condition) came from the unbinding resource decision, because municipality housing policies could have been developed at the same time as the STF decision. Then, all reducing effects would be due to the municipality's intervention instead of the unbinding mechanism improving the funding policy as proposed by the public finance literature.

From the results reported before, the estimated coefficients have positive signals, positively affecting the housing deficit probability (considering the entire sample estimations). Thus, the relations discussed under the first scenario hypothesis are not a problem we have to overcome. However, we have to handle the second one.

Even under a positive treatment effect, additional confounding could arise after the Supreme Court's decision. If the municipality's government decides to interrupt any housing public policy that has been developed before the STF's decision, it would affect the housing deficit directly. Thus, under the second scenario, it would reinforce the housing deficit ratio in the State of São Paulo and would lead to an overestimation of the real effect of the earmarking mechanism. The second scenario is unlikely. The Brazilian Supreme Court's decision occurred after the middle of the mayor's administrative term. The year 1999 was an election year. An abrupt decision would jeopardize a mayor's or party's reelection, under the assumption that politicians are a power seeking.

Overall, the robustness exercises suggest that there is a positive causal effect of the earmarking mechanism's absence on low-income household's probability of remaining in the housing deficit condition when baseline estimations consider the entire sample. The robustness results by income-group provide less support in the direction of the causal effect. However, these tests do not reject the causal effects direction for all income-groups analyzed together.

8. Final Remarks

This paper provides new evidence on the earmarking mechanism as a protective policy for low-income households to combat the housing deficit. By assessing the relationship between earmarked taxation and the housing deficit probability, we provide empirical support to estimate households' probability to remain in a housing deficit condition in the absence of earmarked funded housing policies.

Although many empirical studies have been unable to find a clear empirical effect of binding revenues or taxes on outcomes, we explore the unconstitutionality decision by the Brazilian Supreme Court as exogenous to deal with the endogeneity. Using a non-linear difference-in-difference approach, we isolate the causal effect of the earmarking mechanism comparing pre and post Supreme Court's decision among treated and control groups. This methodology enables us to evaluate the intention-to-treat on low-income households.

Our results suggest that earmarked funding is a way to guarantee a steady and reliable funding source for low-income households to access the housing market and reduce consumer uncertainty. We can also highlight that budgetary flexibility was not efficient for people that compound a housing deficit. In this way, as soon as earmarked funding was no longer used, the program did not have a priori legal claim on the revenues so that housing programs had to compete for funding amongst all other government costs and investments (personnel cost, for example, which is a non-productive sector). Therefore, although we did not have access to the destination of the additional taxation after the Supreme Court decision, our descriptive statistics point to a housing program budget reduction even when the total collection shows an increasing rate, providing additional support to our econometric estimations.

Additionally, we can also pay attention to the cost-benefits for government housing programs using an input-output model as an empirical strategy, given the estimated impact presented in Table 8 (column 7). To run this exercise, we based our analysis on SINAPI/IBGE. According to this institute, low-income housing, as supplied by CDHU, costs US\$ 13,854.8 by housing unit and US\$19,570 by apartment unit. House prototype refers to a 38.3 m^2 house size with two bedrooms, one living room, one bathroom, one kitchen and one external laundry without ceiling. Apartment prototype refers to a 55.9 m^2 size with two bedrooms, one living room, one bathroom, and kitchen and laundry adjoined. From the estimated impact, 4,111 households were affected in absolute terms due to the absence of earmarked revenues. Then, the non-invested fraction by the government was US\$ 55,227,770 (in houses) or US\$80,450,397 (in apartments).

Using Campos and Guilhoto, 2017 generators, we are able to estimate the total impact on macroeconomic variables (employment level, GDP, tax collection and production). Empirically, the authors split the low-income housing into six housing typologies. Each of these housing typology reflects the input purchase and production factors payment. We just take into account typologies II and V estimation because CDHU most commonly supply apartments with no elevator (Type II) and house complex (Type V). The conduction of this exercise describes Type II only. [26] Table 16 shows the generators.

Concerning Production generators (Type II), it is estimated that an investment shock in the amount specified above would generate an increase of US\$ 263.1 million in the Brazilian economy. This effect can be divided as direct, indirect and induced multipliers. The first effect would arise from the impact generated by the initial shock of US\$ 80.4 million. The indirect effect would correspond to purchases made in other sectors due to increased housing construction, i.e., US\$ 51.2 million would be generated in the production of other sectors due to the induced demand. The induced effect multiplier represents the increase in the order of US\$ 131.4 million from the induced wage increases. From the total generated in Brazilian economy, 24.6 % of São Paulo's investment spills over to the other 26 Federal Units, and then US\$ 693.6 million would be retained in State of São Paulo.

Hereafter, we focus on total generators and spatial spillovers. Focusing on GDP, we estimate US\$ 156.9 million would be increased in the Brazilian economy due to the housing construction, of which US\$ 120.3 million would be retained in the State of São Paulo. The impact on state tax revenue (ICMS) would be US\$ 9 million and US\$ 62.9 million in federal tax revenue (IPI). The collection generator rates in São Paulo State's economy would be 63.9% and 18.5%, respectively . The employment generator shows 50,225 new employment generated in the whole economy (or 34,475 in the whole state) if the earmarking mechanism had been used or the government's commitment remained.

These results suggest that the State of São Paulo's economy (household, firms and government) lose out in terms of production, GDP, tax collection and employment. Clearly, we are not able to show where

[26] The same exercises are possible for Type V generators, since the estimated budget for this typology is readily available. To find the results, see Table 16.

financial resources from ICMS-Habitação had gone if not to housing programs and if it would had been used to pay back higher costs debts or to invest in other demanded public goods with higher generators than housing. However, we are clearly underestimating the benefit, given that we are not able to calculate the impact of housing access on education, labor supply and health, as discussed in the housing literature [Susin, 2002, Sinai and Waldfogel, 2005, Field, 2007, Rothwell and Massey, 2015].

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Appendix

Table 1 – Housing Construction by CDHU Company and Housing Deficit

Year	Housing Supply[1]	Municipalities Attended[1]	Total Housing Deficit[2]	Housing Deficit (Treatment Group)[2]
1986	791	12	-	-
1987	884	18	-	-
1988	4,984	54	-	-
1989	8,665	35	-	-
1990	**12,164**	**83**	-	-
1991	9,793	80	-	-
1992	36,702	196	-	-
1993	20,773	106	-	-
1994	23,881	101	-	-
1995	9,035	40	202,103	136,983
1996	24,984	103	225,356	133,737
1997	37,114	164	176,002	127,798
1998	**44,900**	**115**	**148,933**	**110,987**
1999	8,191	29	130,672	109,181
2000	5,629	51	-	-
Total	248,490	1,187	883,066	618,686

Notes: [1] Source: Royer, 2002. [2] The results stem from PNAD dataset using the algorithm developed by the authors. Considering only urban Census situation (city, village or isolated urban area), we assume households face housing deficit if they are fitted at least within one of these situations - even dwelling kind is private but improvised and collective, family number is bigger than one, or dweller density by room is bigger than three. See Database section for further information.

Table 2 – Total Revenue by Funding Resources[1]

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	1995		1996		1997		1998		1999	
Financial Sources	R$	%	R$	%	R$	%	R$	%	R$	%
Tresuary Resource	**599,673**	**0.81**	**684,211**	**0.93**	**608,631**	**0.83**	**523,767**	**0.71**	**480,633**	**0.65**
-Economic Subvention	7,608	1.03	0	0.00	0	0.00	0	0.00	0	0.00
-Subscription of Share	592,063	80.38	684,211	82.50	608,631	85.85	523,767	75.52	480,633	75.21
—ICMS	***590,447***	***80.16***	***682,334***	***82.27***	***604,985***	***85.34***	***505,399***	***72.87***	***476,201***	***74.51***
—Guarapiranga Program	439	0.06	1,877	0.23	1,382	0.19	12,232	1.76	0	0.00
—Habitar Brasil Program	0	0.00	0	0.00	2,264	0.32	6,135	0.88	4,432	0.69
—Others[2]	1,177	0.16	0	0.00	0	0.00	0	0.00	0	0.00
CDHU Resources	**136,912**	**18.59**	**144,863**	**17.47**	**99,996**	**14.11**	**136,268**	**19.65**	**158,576**	**24.81**
-Borrowers	66,767	9.06	86,937	10.48	89,527	12.63	127,455	18.38	140,152	21.93
-Financial Income	68,944	9.36	56,465	6.81	7,464	1.05	7,950	1.15	17,051	2.67
-Others[3]	1,200	0.16	1,460	0.18	3,005	0.42	863	0.12	1,372	0.21
Other Financial Sources[4]	**22**	**0.00**	**277**	**0.03**	**309**	**0.04**	**33,551**	**4.84**	**-112**	**-0.02**
Total Revenue	**736,606**	**100.00**	**829,351**	**100.00**	**708,936**	**100.00**	**693,586**	**100.00**	**639,097**	**100.00**

Source: based on data from Oliveira, 2002. [1] in million of Reais (R$ - Brazilian currency) and deflated relative to 1999 price [2] Equipment as Asset;[3] Financial Revenue and Other Revenues and [4] Security deposit.

Table 3 – Public Housing Program Investment[1]

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	1995		1996		1997		1998		1999	
Investments	R$	%	R$	%	R$	%	R$	%	R$	%
Housing Construction	**189,222**	**62.4**	**588,689**	**66.4**	**563,294**	**81.4**	**401,916**	**71.4**	**153,602**	**52.3**
Complementary Expenditures	**113,757**	**37.5**	**296,700**	**33.5**	**128,122**	**18.5**	**159,997**	**28.4**	**139,767**	**47.6**
—Projects	835	0.3	1,692	0.2	1,245	0.2	1,695	0.3	1,993	0.7
—Land Cost	58,163	19.2	205,300	23.2	25,839	3.7	13,763	2.4	17,076	5.8
—Manangment	27,418	9.0	42,986	4.8	38,855	5.6	57,466	10.2	36,389	12.4
—Other Production Cost	27,342	9.0	46,722	5.3	62,183	9.0	87,073	15.5	84,309	28.7
Fixed Asset	**283**	**0.1**	**1,043**	**0.1**	**327**	**0.0**	**1,331**	**0.2**	**172**	**0.1**
Total	**303,261**	**100**	**886,432**	**100**	**691,743**	**100**	**563,244**	**100**	**293,541**	**100**

Source: based on data from Oliveira, 2002. [1] in million of Reais (R$ - Brazilian currency) and deflated relative to 1999 price.

Table 4 – Tax Collection, Tax Transfering, Housing Construction and Investment's Growth Rate[1]

	(1)	(2)	(3)	(4)	(5)
	1995	1996	1997	1998	1999
ICMS State Collection[2]	R$ 25,919,950	R$ 27,985,661	R$ 27,820,099	R$ 26,862,491	R$ 24,694,373
–Growth Rate	-	7.97%	-0.59%	-3.44%	-8.07%
ICMS-Housing[3]	R$ 592,063	R$ 684,211	R$ 608,631	R$ 523,767	R$ 480,633
–Growth Rate	-	15.56%	-11.05%	-13.94%	-8.24%
ICMS State / ICMS-Housing	2.284%	2.445%	2.188%	1.950%	1.946%
Housing Supply[4]	R$ 9,035	R$ 24,984	R$ 37,114	R$ 44,900	R$ 8,191
—H. Supply Growth rate	-	176.52%	48.55%	20.98%	-81.76%
Total Investment[5]	R$ 303,261	R$ 886,432	R$ 691,743	R$ 563,244	R$ 293,541
—Total Cost Growth rate	-	192.30%	-21.96%	-18.58%	-47.88%

Sources: [1] in million of Reais (R$ - Brazilian currency) and deflated relative to 1999 price constant [2] Finbra, [3] and [4] based on data from Oliveira, 2002, [5] based Royer, 2002. prices

Tabela 5 – Variables Description for the Econometrics Model

Symbol	Variable	Description	Source	Period	PNAD Code
D	Housing deficit	Total of household restrained demand.	PNAD	1995-1999	v0211 v0213 v0212 v0217 v0216 v0219
X_1	Age	Mean age of head of household.	PNAD	1995-1999	v8005
X_2	Life cycle	Total of married and single with or without children living together in the same house. For this control, we observe four life cycles: lives with wife/ husband with or without children and lives alone with or without children.	PNAD	1995-1999	v1001 v1141 v1142
X_3	Income	Mean household monthly income in real terms, using the price deflator proposed by Corseuil and Foguel, 2002.	PNAD	1995-1999	v4614
X_4	Years of schooling	Identify highest schooling years attended by head of household.	PNAD	1995-1999	v0607
X_5	Race	Head of household race.	PNAD	1995-1999	v0404
X_6	Migrant	If head of household was born in the same state he/she lives in.	PNAD	1995-1999	v0502
X_7	Population projection	Mean of mass of population estimated.	PNAD	1995-1999	v4609
P	Housing rent	Rent paid by the household in terms of minimum wage.	PNAD	1995-1999	v0208 v2081
A	Public policy	Binary variable to identify earmarking unconstitutionality.	STF	1997*	

Source: Elaborated by the authors. * In the econometric estimations, we use year 1998 to identify Supreme Court's decision, period the decision came into force.

Table 6 – Descriptive Statistics

	Before (1995-1997)			After (1998-1999)		
Variables	**Mean**	**Std. Err.**	**N**	**Mean**	**Std. Err.**	**N**
Housing Deficit						
–Control group	0.020	0.142	1,995,360	0.007	0.084	1,565,277
–Treatment group	0.039	0.193	10,251,787	0.027	0.162	8,126,208
House onwer	0.170	0.376	12,185,439	0.171	0.377	9,662,238
Cycle II	0.071	0.257	12,247,147	0.085	0.279	9,691,485
Cycle III	0.056	0.229	12,247,147	0.058	0.235	9,691,485
Cycle VI	0.858	0.349	12,247,147	0.840	0.366	9,691,485
Age	45.895	16.656	12,247,147	46.075	22.641	9,691,485
Monthly Income	1135.348	551.819	12,247,147	1179.762	563.095	9,691,485
Black	0.065	0.247	12,244,659	0.067	0.250	9,691,485
White	0.688	0.463	12,244,659	0.677	0.468	9,691,485
Brown	0.239	0.427	12,244,659	0.247	0.431	9,691,485
Migrant	0.582	0.493	8,868,483	0.595	0.491	6,933,821
Population Projection	17,300,000	562,771	12,247,147	17,900,000	580,861	9,691,485
Years of schooling	5.237	3.345	12,227,135	5.626	3.471	9,670,873
Metrop. Region	0.417	0.493	12,247,147	0.424	0.494	9,691,485
Autorepre. Region	0.180	0.384	12,247,147	0.178	0.382	9,691,485

Notes:All the differences between before and after means are statistically significant at 1% level.

Table 7 – Testing Trend Parallel Hypothesis

	SP		MG		ES		RJ		PR		RS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Diff	t-test	Diff	t-teste	Diff	t-test	Diff	t-test	Diff	t-test	Diff	t-test
Group I	-0.003	-0.229	-0.014	-0.729	-0.019	-0.839	-0.007	-0.592	-0.015	-1.301	0.014	1.252
Group II	0.002	0.205	-0.003	-0.393	-0.010	-0.336	-0.002	-0.188	-0.005	-0.780	0.000	-0.026
Group III	-0.002	-0.190	-0.010	-1.142	-0.017	-1.497	-0.001	-0.052	-0.001	-0.021	0.004	0.305
Group IV	-0.001	-0.093	0.003	0.349	0.002	0.112	0.002	0.150	0.002	0.153	-0.001	-0.083
Group V	0.002	0.185	0.005	1.584	0.003	0.195	0.004	0.840	0.001	0.124	0.005	0.354
ALL	0.00005	0.006	-0.004	-0.603	-0.008	-0.609	0.000	-0.012	-0.002	-0.299	0.004	1.219

Notes: All the trend differences between treatment and control groups are not statistically significant during earmarking mechanism using (pre-treatment). The trend mean tests from columns (3)-(12) use as control-groups low-income households from the states of Minas Gerais (MG), Espírito Santo (ES), Rio de Janeiro (RJ), Paraná (PR) and Rio Grande do Sul (RS).

Table 8 – Main Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
τ	0.0073***	0.0172***	0.0058***	0.0072***	0.0176***	0.0170***	0.0187***
	(0.0002)	(0.0003)	(0.0002)	(0.0002)	(0.0003)	(0.0003)	(0.0007)
Covariates	N	Y	N	N	Y	Y	Y
Cesus Area f.e.	N	N	Y	N	Y	N	Y
Year f.e	N	N	N	Y	N	Y	Y
Psedo R	0.008	0.105	0.028	0.009	0.106	0.106	0.107
N. Obs	15,692,002	15,692,002	15,692,002	15,692,002	15,692,002	15,692,002	15,692,002

Notes: * significant at 10%, ** significant at 5% and *** significant at 1%. Standard errors in parentheses. The dependent variable: ($y^* = 1$) if households compound housing deficit and 0 otherwise. Independent variables: dummy equal 1 indicating house-owner and 0 otherwise, dummy indicating life cycles, age, monthly income, dummy vector indicating race (black, white, brown and asian), dummy indicating migrants head-household, population projection, area and time fixed effects, and $MARK$ cross-dummy ($Treat = 1$ if year is 1998 and $A = 1$ if household is eligible for housing program), here represented by τ (as Equation 20).

Table 9 – Main Results by Low-income Households Groups

	(1)	(2)	(3)	(4)	(5)
	Group I	Group II	Group III	Group IV	Group V
τ	0.0039***	0.0591***	0.0275***	0.0310***	0.0134***
(s.e)	0.0024	0.0039	0.0017	0.0017	0.0006
Covariates	Y	Y	Y	Y	Y
Cesus Area f.e.	Y	Y	Y	Y	Y
Year f.e	Y	Y	Y	Y	Y
Psedo R	0.1271	0.1228	0.1265	0.1087	0.12
N. Obs	4,272,514	4,657,651	5,617,201	5,659,453	5,491,907

Notes: * significant at 10%, ** significant at 5% and *** significant at 1%. Standard errors in parentheses. The dependent variable: ($y^* = 1$) if households compound housing deficit and 0 otherwise. Independent variables: dummy equal 1 indicating house-owner and 0 otherwise, dummy indicating life cycles, age, monthly income, dummy vector indicating race (black, white, brown and asian), dummy indicating migrants head-household, population projection, area and time fixed effects, and $MARK$ cross-dummy ($Treat = 1$ if year is 1998 and $A = 1$ if household is eligible for housing program), here represented by τ (as Equation 20). The group divisions follow the households monthly-income (HMI): HMI between $[0, 1]$ belongs to Group I; between $(1, 2]$ belongs to Group II; $(2, 3]$ Group III; between $(3, 4]$ belongs to Group IV; between $(4, 5]$ belongs to Group V; and between $[0, 5]$ belongs to group ALL.

Table 10 – Housing Deficit Probability by Gender

	(1)	(2)	(3)	(4)	(5)	(6)
	MALE					
	Group I	Group II	Group III	Group IV	Group V	ALL
τ	0.0272***	0.1823***	0.0116***	0.0252***	0.0109***	0.0287***
(s.e)	0.0004	0.0022	0.0002	0.0003	0.0001	0.0002
Covariates	Y	Y	Y	Y	Y	Y
Cesus Area f.e.	Y	Y	Y	Y	Y	Y
Year f.e	Y	Y	Y	Y	Y	Y
Psedo R	0.1309	0.1263	0.1295	0.1132	0.1184	0.1063
N. Obs	3,053,853	3,445,551	4,309,806	4,408,118	4,387,798	11,540,274
	FEMALE					
	Group I	Group II	Group III	Group IV	Group V	ALL
τ	-0.1579***	-0.0003***	0.0821***	0.0373***	0.0465***	0.0137***
(s.e)	0.0029	0.0002	0.0016	0.0008	0.0013	0.0002
Covariates	Y	Y	Y	Y	Y	Y
Cesus Area f.e.	Y	Y	Y	Y	Y	Y
Year f.e	Y	Y	Y	Y	Y	Y
Psedo R	0.1399	0.1936	0.1765	0.1281	0.1650	0.1208
N. Obs	1,218,661	1,212,100	1,307,395	1,251,335	1,104,109	4,151,728

Notes: * significant at 10%, ** significant at 5% and *** significant at 1%. Standard errors in parentheses. The dependent variable: ($y^* = 1$) if households compound housing deficit and 0 otherwise. Independent variables: dummy equal 1 indicating house-owner and 0 otherwise, dummy indicating life cycles, age, monthly income, dummy vector indicating race (black, white, brown and asian), dummy indicating migrants head-household, population projection, area and time fixed effects, and $MARK$ cross-dummy ($Treat = 1$ if year is 1998 and $A = 1$ if household is eligible for housing program), here represented by τ (as Equation 20). The group divisions follow the households monthly-income (HMI): HMI between $[0, 1]$ belongs to Group I; between $(1, 2]$ belongs to Group II; $(2, 3]$ Group III; between $(3, 4]$ belongs to Group IV; between $(4, 5]$ belongs to Group V; and between $[0, 5]$ belongs to group ALL.

Table 11 – Housing Deficit Probability by Region

	(1)	(2)	(3)	(4)	(5)	(6)
	METROPOLITAN AREA OF SÃO PAULO					
	Group 1	Group 2	Group 3	Group 4	Group 5	ALL
τ	0.0316***	0.0942***	0.0352***	0.0391***	0.0324***	0.0315***
(s.e)	0.0005	0.0011	0.0004	0.0006	0.0005	0.0003
Psedo R	0.1115	0.1183	0.0995	0.1119	0.0939	0.0985
N. Obs	2,131,476	2,093,856	2,624,996	2,734,600	2,663,492	7,246,388
	REPRESENTATIVE MUNICIPALITIES					
τ	-0.2232***	0.0881***	-0.0134***	-0.0526***	-	-0.0092***
(s.e)	0.0050	0.0021	0.0004	0.0013	-	0.0002
Psedo R	0.2290	0.1463	0.1608	0.1299	-	0.1165
N. Obs	2,131,476	2,093,856	2,624,996	2,734,600	-	5,736,466
	NON-REPRESENTATIVE MUNICIPALITIES					
τ	0.0104***	0.0297***	0.0150***	0.0120***	0.0259***	0.0058***
(s.e)	0.0005	0.0005	0.0003	0.0002	0.0006	0.0001
Psedo R	0.1919	0.0998	0.1040	0.0988	0.1601	0.0980
N. Obs	1,366,459	1,668,815	1,924,992	1,847,356	1,652,042	5,736,466

Notes: * significant at 10%, ** significant at 5% and *** significant at 1%. Standard errors in parentheses. The dependent variable: ($y^* = 1$) if households compound housing deficit and 0 otherwise. Independent variables: dummy equal 1 indicating house-owner and 0 otherwise, dummy indicating life cycles, age, monthly income, dummy vector indicating race (black, white, brown and asian), dummy indicating migrants head-household, population projection, area and time fixed effects, and $MARK$ cross-dummy ($Treat = 1$ if year is 1998 and $A = 1$ if household is eligible for housing program), here represented by τ (as Equation 20). The group divisions follow the households monthly-income (HMI): HMI between $[0, 1]$ belongs to Group I; between $(1, 2]$ belongs to Group II; $(2, 3]$ Group III; between $(3, 4]$ belongs to Group IV; between $(4, 5]$ belongs to Group V; and between $[0, 5]$ belongs to group ALL. Using subsample for Representative Area Group V, the econometric estimation does not converge.

Table 12 – Housing Deficit Probability by Migration Condition

	(1)	(2)	(3)	(4)	(5)	(6)
	MIGRANT					
	Group I	Group II	Group III	Group IV	Group V	ALL
τ	-0.0116***	0.1506***	0.0667***	0.0235***	0.0061***	0.0124***
(s.e)	0.0002	0.0031	0.0015	0.0004	0.0002	0.0002
Covariates	Y	Y	Y	Y	Y	Y
Cesus Area f.e.	Y	Y	Y	Y	Y	Y
Year f.e	Y	Y	Y	Y	Y	Y
Psedo R	0.1928	0.2026	0.1387	0.0957	0.2050	0.1211
N. Obs	1,707,508	1,972,253	2,178,758	2,171,142	2,184,694	6,484,876
	NON-MIGRANT					
	Group I	Group II	Group III	Group IV	Group V	ALL
τ	0.0194***	-0.0169***	0.0118***	0.0236***	0.0180***	0.0228***
(s.e)	0.0003	0.0001	0.0002	0.0004	0.0003	0.0002
Covariates	Y	Y	Y	Y	Y	Y
Cesus Area f.e.	Y	Y	Y	Y	Y	Y
Year f.e	Y	Y	Y	Y	Y	Y
Psedo R	0.1042	0.0927	0.1142	0.1106	0.0867	0.0897
N. Obs	2,565,006	2,685,398	3,330,425	3,399,134	3,241,591	9,207,126

Notes: * significant at 10%, ** significant at 5% and *** significant at 1%. Standard errors in parentheses. The dependent variable: $(y^{*} = 1)$ if households compound housing deficit and 0 otherwise. Independent variables: dummy equal 1 indicating house-owner and 0 otherwise, dummy indicating life cycles, age, monthly income, dummy vector indicating race (black, white, brown and asian), dummy indicating migrants head-household, population projection, area and time fixed effects, and $MARK$ cross-dummy ($Treat = 1$ if year is 1998 and $A = 1$ if household is eligible for housing program), here represented by τ (as Equation 20). The group divisions follow the households monthly-income (HMI): HMI between $[0, 1]$ belongs to Group I; between $(1, 2]$ belongs to Group II; $(2, 3]$ Group III; between $(3, 4]$ belongs to Group IV; between $(4, 5]$ belongs to Group V; and between $[0, 5]$ belongs to group ALL.

Table 13 – Trend Robustness of Housing Deficit Effect of Earmarked Tax

	(1)	(2)	(3)	(4)	(5)	(6)
	1997 and 1998 years only					
	Group I	Group II	Group III	Group IV	Group V	ALL
τ	-0.0163***	0.0116***	0.0084***	0.0141***	0.0177***	-0.0014***
(s.e)	0.0003	0.0002	0.0001	0.0002	0.0003	0.0000
Covariates	Y	Y	Y	Y	Y	Y
Cesus Area f.e.	Y	Y	Y	Y	Y	Y
Year f.e	Y	Y	Y	Y	Y	Y
Psedo R	0.1161	0.1359	0.1539	0.0882	0.1144	0.1009
N. Obs	1,696,581	1,757,978	2,094,014	2,212,652	2,228,786	6,255,551
	1997 and 1999 years only					
	Group I	Group II	Group III	Group IV	Group V	ALL
τ	0.0155***	0.0220***	0.0704***	0.1301***	0.0543***	0.0229***
(s.e)	0.0003	0.0003	0.0010	0.0025	0.0010	0.0003
Covariates	Y	Y	Y	Y	Y	Y
Cesus Area f.e.	Y	Y	Y	Y	Y	Y
Year f.e	Y	Y	Y	Y	Y	Y
Psedo R	0.1406	0.1662	0.1361	0.1618	0.1432	0.1169
N. Obs	1,825,954	1,956,848	2,468,197	2,353,547	2,362,514	6,670,756

Notes: * significant at 10%, ** significant at 5% and *** significant at 1%. Standard errors in parentheses. The dependent variable: $(y^{*} = 1)$ if households compound housing deficit and 0 otherwise. Independent variables: dummy equal 1 indicating house-owner and 0 otherwise, dummy indicating life cycles, age, monthly income, dummy vector indicating race (black, white, brown and asian), dummy indicating migrants head-household, population projection, area and time fixed effects, and $MARK$ cross-dummy ($Treat = 1$ if year is 1998 and $A = 1$ if household is eligible for housing program), here represented by τ (as Equation 20). The group divisions follow the households monthly-income (HMI): HMI between $[0, 1]$ belongs to Group I; between $(1, 2]$ belongs to Group II; $(2, 3]$ Group III; between $(3, 4]$ belongs to Group IV; between $(4, 5]$ belongs to Group V; and between $[0, 5]$ belongs to group ALL.

Table 14 – Robustness of Housing Deficit Effect of Earmarked Tax using Similar Brazilian States

		(1)	(2)	(3)	(4)	(5)	(6)
State		Group I	Group II	Group III	Group IV	Group V	ALL
RJ	τ	-0.0454***	-0.0090***	-0.0352***	-0.0286***	-0.0669***	-0.0063***
	(s.e)	0.0015	0.0013	0.0004	0.0004	0.0005	0.0001
	Psedo R	0.1182	0.0953	0.0863	0.1028	0.1079	0.0862
	N. Obs	1,396,984	1,766,126	1,866,080	1,829,418	1,753,095	5,580,779
ES	τ	-0.0265***	-0.0147***	-0.0633***	0.0288***	-0.1974***	-0.0284***
	(s.e)	0.0007	0.0004	0.0013	0.0007	0.0064	0.0004
	Psedo R	0.1705	0.1017	0.1247	0.1122	0.1736	0.1093
	N. Obs	397,449	477,177	408,504	393,931	194,261	1,587,995
MG	τ	-0.1176***	0.1150***	0.0398***	0.0294***	0.0321***	-0.0118***
	(s.e)	0.0007	0.0004	0.0013	0.0007	0.0064	0.0004
	Psedo R	0.1253	0.1077	0.1235	0.1318	0.0885	0.0855
	N. Obs	1,826,268	2,474,079	2,346,099	2,128,775	1,814,121	7,435,326
RS	τ	-0.0166***	-0.0174***	-0.0310***	-0.0154***	-0.0116***	-0.0240***
	(s.e)	0.0002	0.0002	0.0003	0.0002	0.0002	0.0002
	Psedo R	0.1595	0.1088	0.1097	0.1327	0.0867	0.1310
	N. Obs	1,044,816	1,418,479	1,433,168	1,398,603	1,242,645	4,329,603
PR	τ	-0.0010***	-0.0476***	-0.0248***	-0.0099***	0.0048***	-0.0174***
	(s.e)	0.0001	0.0006	0.0003	0.0001	0.0001	0.0001
	Psedo R^2	0.1745	0.1159	0.1390	0.0993	0.0881	0.1199
	N. Obs	1,483,005	1,887,827	1,865,100	1,642,067	1,464,897	5,930,240

Notes: * significant at 10%, ** significant at 5% and *** significant at 1%. Standard errors in parentheses. The dependent variable: $(y^* = 1)$ if households compound housing deficit and 0 otherwise. Independent variables: dummy equal 1 indicating house-owner and 0 otherwise, dummy indicating life cycles, age, monthly income, dummy vector indicating race (black, white, brown and asian), dummy indicating migrants head-household, population projection, area and time fixed effects, and $MARK$ cross-dummy ($Treat = 1$ if year is 1998 and $A = 1$ if household is eligible for housing program), here represented by τ (as Equation 20). The group divisions follow the households monthly-income (HMI): HMI between $[0, 1]$ belongs to Group I; between $(1, 2]$ belongs to Group II; $(2, 3]$ Group III; between $(3, 4]$ belongs to Group IV; between $(4, 5]$ belongs to Group V; and between $[0, 5]$ belongs to group ALL.

Table 15 – Control Group Robustness of Housing Deficit Effect of Earmarked Tax using Similar Brazilian States

		(1)	(2)	(3)	(4)	(5)	(6)
State		Group I	Group II	Group III	Group IV	Group V	ALL
RJ	τ	-0.0027***	-0.0004***	-0.0084***	0.0034***	0.0042***	0.0022***
	(s.e)	0.0000	0.0000	0.0001	0.0001	0.0000	0.0000
	Psedo R	0.0998	0.0874	0.1141	0.1104	0.1152	0.0972
	N. Obs	2,410,086	3,164,365	4,223,869	4,229,459	3,985,590	21,272,781
ES	τ	-0.0208***	0.0024***	-0.0188***	0.0874***	0.0137***	0.0004***
	(s.e)	0.0006	0.0003	0.0003	0.0016	0.0003	0.0000
	Psedo R	0.0973	0.0783	0.1065	0.0942	0.1148	0.1037
	N. Obs	2,018,903	2,546,954	3,447,528	3,286,668	3,122,998	17,279,997
MG	τ	0.0045***	-0.0004***	0.0176***	0.0083***	0.0031***	0.0069***
	(s.e)	0.0001	0.0000	0.0001	0.0001	0.0000	0.0000
	Psedo R	0.0924	0.0769	0.1032	0.0912	0.1078	0.0992
	N. Obs	2,808,597	3,841,545	4,673,115	4,498,043	3,923,844	23,127,328
RS	τ	-0.0109***	0.0094***	0.0738***	0.0180***	0.0011***	0.0089***
	(s.e)	0.0002	0.0001	0.0003	0.0000	0.0001	0.0000
	Psedo R	0.0896	0.0775	0.1020	0.0843	0.1162	0.1031
	N. Obs	2,636,309	3,426,268	4,363,091	4,075,043	3,770,283	21,622,242
PR	τ	0.0016***	-0.0112***	0.0398***	0.0013***	-0.0152***	0.0029***
	(s.e)	0.0002	0.0001	0.0003	0.0000	0.0001	0.0000
	Psedo R	0.1068	0.0858	0.1117	0.1016	0.1125	0.1073
	N. Obs	2,263,622	3,022,422	3,996,661	3,850,295	3,581,167	20,021,605

Notes: * significant at 10%, ** significant at 5% and *** significant at 1%. Standard errors in parentheses. The dependent variable: ($y^{*} = 1$) if households compound housing deficit and 0 otherwise. Independent variables: dummy equal 1 indicating house-owner and 0 otherwise, dummy indicating life cycles, age, monthly income, dummy vector indicating race (black, white, brown and asian), dummy indicating migrants head-household, population projection, area and time fixed effects, and $MARK$ cross-dummy ($Treat = 1$ if year is 1998 and $A = 1$ if household is eligible for housing program as placebo), here represented by τ (as Equation 20). The group divisions follow the households monthly-income (HMI): HMI between $[0, 1]$ belongs to Group I; between $(1, 2]$ belongs to Group II; $(2, 3]$ Group III; between $(3, 4]$ belongs to Group IV; between $(4, 5]$ belongs to Group V; and between $[0, 5]$ belongs to group ALL. The control-groups come from the selected states.

Table 16 – Generators and Cost-Benefit Estimation

		1	2	3	4	5	6
Typology	Generators	Direct	Indirect	Induced	Total	Spillover RBR	Retained Revenue
Type II	Production	1.000	0.638	1.633	3.271	24.6%	75.4%
	GDP	0.632	0.338	0.981	1.951	23.3%	76.7%
	ICMS	0.000	0.033	0.079	0.112	36.1%	63.9%
	IPI	0.000	0.096	0.135	0.231	31.2%	68.8%
	Employment	229.887	81.588	313.193	624.668	31.4%	68.6%
	Production	$ 80,450,397	$ 51,295,173	$ 131,407,679	$ 263,153,249	$ 64,735,699	$ 198,417,550
	GDP	$ 50,820,516	$ 27,192,234	$ 78,929,885	$ 156,942,635	$ 36,567,634	$ 120,375,001
	ICMS	$ -	$ 2,662,908	$ 6,363,626	$ 9,026,535	$ 3,258,579	$ 5,767,956
	IPI	$ -	$ 7,731,283	$ 10,844,714	$ 18,575,997	$ 5,795,711	$ 12,780,286
	Employment	$ 18,495	$ 6,564	$ 25,197	$ 50,255	$ 15,780	$ 34,475
Type V	Production	1.000	1.163	1.663	3.826	25.4%	74.6%
	GDP	0.619	0.368	0.976	1.963	24.3%	75.7%
	ICMS	0.000	0.035	0.079	0.114	37.3%	62.7%
	IPI	0.000	0.068	0.150	0.218	32.2%	67.8%
	Employment	236.840	101.064	312.959	650.863	32.5%	67.5%
	Production	$ 55,227,770	$ 64,224,374	$ 91,838,259	$ 211,290,402	$ 53,667,762	$ 157,622,640
	GDP	$ 34,191,512	$ 20,307,251	$ 53,902,304	$ 108,401,067	$ 26,341,459	$ 82,059,608
	ICMS	$ -	$ 1,932,972	$ 4,346,425	$ 6,279,397	$ 2,342,215	$ 3,937,182
	IPI	$ -	$ 3,733,397	$ 8,300,734	$ 12,034,131	$ 3,874,990	$ 8,159,141
	Employment	$ 13,080	$ 5,582	$ 17,284	$ 35,946	$ 11,682	$ 24,263

The generators were calculated by Campos and Guilhoto, 2017 using input-output tables from 2009. We use US$ 55,227,770 (in houses) or US$80,450,397 (in apartments) as initial shock and the total effected household stemmed from econometric estimation, presented on Table 8 (column 7). Type II and Type V refer to house and apartment constructions, respectively.