Spillover effects from inter-governmental transfers between local jurisdictions in Brazil

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Abstract

This paper estimates fiscal spillover effects between local jurisdiction in Brazil. The central government transfers revenues to local jurisdictions. By using an Event Study design, it shows evidences that arguable exogenous changes in revenues of a local jurisdiction has null causal spillover effects on public expenditures and number of firms of a nearby jurisdiction. When restricting the sample for jurisdictions that receive the revenue shock that are larger than their nearby jurisdictions, the result of null fiscal spillover still holds. This novel results cast doubts on a rich theoretical literature that predicts fiscal spillover's existence and also on the current empirical literature that estimates positive fiscal spillovers. JEL: H27,H30,H50,H61

1 Introduction

A current debate going on in Brazil is whether the country should decentralize its power by giving more autonomy to states and municipalities and trading off the central government discretion. In 2018's presidential elections, the candidate Jair Bolsonaro would repeatedly say that *We (Brazilians) need more Brazil and less Brasilia*, where Brasilia is the capital, center of the political decision making. The fiscal decentralization debate has going on in the country since a long time ago. However, in order to understand the repercussion of such a policy, the current status quo must be first studied. This paper concerns the repercussion of an important fiscal instrument that relates to fiscal (de)centralization.

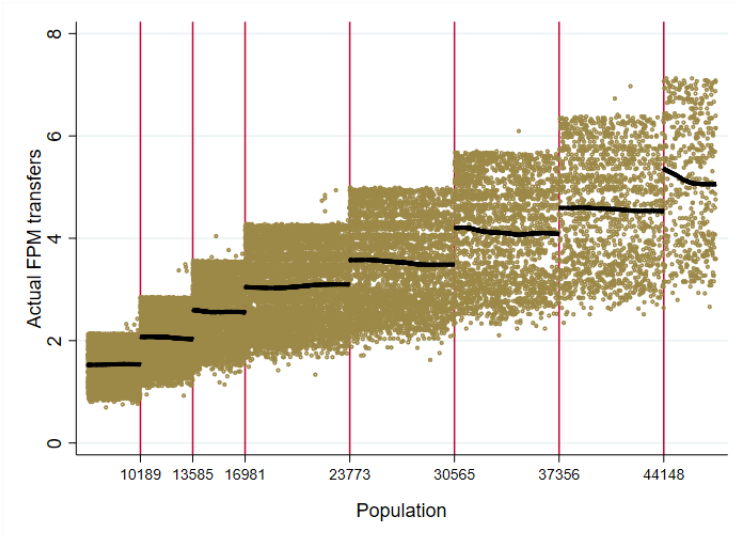
One of the most important fiscal transfers between central government and local jurisdiction is the Brazilian Fundo de participacao de Municipios - FPM - (Municipal Participative fund), which unconditionally redistributes revenues from the central to local government. The FPM makes unconditional transfers to local jurisdictions (municipalities) according to their FPM's coefficients. The local jurisdictions' coefficients magnitudes are nationally determined according to fixed population thresholds. A jurisdiction's coefficients abruptly change when it crosses a fixed populations' thresholds. A jurisdiction crosses a threshold when there is a positive or a negative change in its population size and if it is near enough to the threshold. This threshold crossing is key for generating positive or negative variations (shocks) in the jurisdiction's FPM's revenues. The threshold crossing date, also denoted as event, is what allows the Event Study design used in this paper. Crossing from the left lower threshold to the right upper threshold means higher revenue and is denoted as a positive event, when the reverse crossing direction happens, it is denoted as a negative event. The Figure 1 below shows the FPM's revenue by local jurisdiction population for illustrative purposes.

A shock in a focal jurisdiction's revenues might or might not have effects that go beyond this jurisdiction borders and affect neighboring jurisdictions, this is what this paper refers as spillovers effect. This paper studies whether these revenues shocks effects are restricted to the jurisdiction of destination or have spillovers effects on its neighbors. This question is important for the fiscal decentralization debate. Identifying spillover effects is also important because it reveals if neighbor jurisdictions' fiscal authorities, firms and workers respond to other jurisdiction's revenues' shocks.

This paper shows evidences that a positive event has a immediate, long lasting and direct effect on the focal jurisdiction's FPM's revenue by 20% (increase or decrease). However, the Event Study design allows to show that the negative mechanical effect has a initial impact of 15% in the year immediately after the event and then it takes three years more to reach the 20% impact, in other words, the events are asymmetric in the short run. This paper also shows that events have direct effect on total public expenditure of its own jurisdiction, they are about to 7% in the immediately year after event and it takes four or more year to reach and stabilize at 10%. These are a novel contributions in the Brazilian context that shows an (a)symmetric effect between positive and negative events and how these effects behave along time.

More important novel results are found when estimating spillovers effects on public budget and num-

Figure 1: FPM tranfers in 10,000 BRL across population (1998 prices)



Source: authorized reproduction of Corbi et al. (2019).

The Figure plots jurisdiction's FPM revenues in 10,000 BRL for all jurisdictions with less than 50,000 inhabitants over the estimated population in the previous period. Black lines are running-means over population bins of 200 inhabitants. Red vertical lines are transfers' coefficients population thresholds. Note that when the population crosses the red lines (thresholds), discontinuities between FPM revenues emerges. They are lower in the left hand side bracket and higher in right hand side bracket. Further notice that the discontinuity is visible at a national level even-though FPM revenues differs between jurisdictions with same coefficient in different states.

ber of firms. Firstly, positive and negative events have precise estimates of null causal spillover effects on neighbor's total public expenditures and number of firms. When restricting to a sample in which the neighbor has a smaller population size than the focal jurisdiction, the estimations of null spillover effects still hold. Note that in this restricted sample, spillover effects existence are more likely to exist because a larger jurisdiction is more likely to affect a smaller jurisdiction. It is also noteworthy that the standard metrics method in this setting of FPM revenue shocks is a Fuzzy RD identification strategy, this paper innovates by using an Event Study design approach.

Castro and Mattos (2021) is the most similar study to this paper, in the same setting, they estimate positive fiscal spillover effects for bordering jurisdictions' on public expenditures. Different from the null spillover effects estimates presented here, they find positive and economic relevant fiscal spillovers. Adopting an Fuzzy RDD analysis, they estimates spillovers effects on outcomes such as total expenditures and others specific type of expenditures, such as health and education. The present paper also cite theoretical models that predicts positive spillover existence. In this context, I cast strong doubts on a currently rich theoretical and empirical literature that argue for positive spillover existence.

This paper has seven sections including this Introduction. The second section is a literature review. The third section presents the institutional background, it briefly describes the Brazilian and the FPM institutional setting. The forth section presents the database format and descriptive statistics. The fifth section, explains the identification strategy, implicitly identification assumptions and internal validity tests. The sixth section present the results, it shows Figures containing direct and spillover effects estimations. Finally, the seventh section makes the final remarks.

2 Spillovers between local jurisdictions

This paper defines fiscal spillover as the effects from fiscal shocks in a focal jurisdiction on a neighbor jurisdiction's outcomes. Specifically, this paper look into fiscal spillovers from a focal jurisdiction on public expenditures and number of firms of a neighbor jurisdiction. Note, however, that the conventional spillover definition, according to Isen (2014), is when residents of one jurisdiction consume, whether in a tangible way or not, the public goods of another jurisdiction.

BESLEY and CASE (1995) argue that decision makers use the fiscal decisions of neighboring jurisdictions as a benchmark for their own jurisdiction fiscal decision making. In BESLEY and CASE (1995) setting, the asymmetry of information between politician and voters might create incentives for the voter to compare the incumbent politician's performance and the neighboring jurisdictions' politicians' performance and vice and versa. In this way, a taxation competition might arise. For example, in a setting where many jurisdictions are rising taxes, a rent-seeking politician can take ride on this nearby jurisdictions' by increasing its taxes. In such an environment, voters might be more willing to accept to pay more taxes arising taxes spillovers.

The Tiebout (1956) model considers mobile households that "vote with their feet": they choose a jurisdiction of residence that provides the fiscal/public good/services package that best suits to their preferences. For example, a family with high school teenagers might choose somewhere with good schools to live or a high skill worker might prefer to live in a jurisdiction where his skills have a higher marginal benefit. It is also possible to think that firms will choose jurisdictions in a way to maximize their profits.

In Tiebout (1956), the jurisdictions don't adapt to economical agents preferences or profits, they only care about the median voter in order to maximize permanence in power. In this way, while nearby workers and firms responds to focal jurisdiction fiscal changes, the neighbor public budget remains static. The economical agents choose a jurisdiction that offer the best bundle of services, public goods, taxation, climate, cultural and other social factors. Therefore, it can be inferred that economical agents are maximizing their utilities or well-being and/or wages/profits by picking the optimal jurisdiction to reside subject to restrictions, such as credit, budget and others.

This paper tackles these issues by estimating fiscal spillover effects from focal jurisdiction on labor market outcomes of nearby jurisdictions, such as number of workers and firms. It is expected that if there is a FPM's revenues shock and, consequently, a boom in focal jurisdiction's public expenditures and labor market as documented in Corbi et al. (2019), then economical agents from nearby jurisdiction would migrate to the focal jurisdiction, this hypothesis is tested in Section 5.

In what refers to the empirical literature of fiscal spillovers, Baicker (2005) estimate positive public expenditures spillover effects between U.S.'s states. Baicker (2005) studies how jurisdiction strategically spends its revenues and tax their citizen. The paper uses mandated increases in medical spending as a source of potentially exogenous variation. In the author's words: *...the analysis shows that each dollar of state spending causes spending in neighboring states to increase by almost 90 cents. The most predictive measure of neighborliness is the degree of population mobility between states, suggesting that rational concerns about adverse migration may drive the interdependence of state spending policy.* They argue that one of the reason for positive fiscal spillover existence is that exists incentives for not driving away taxpayers neither attracting welfare recipients from other states.

In the Brazilian context, Castro et al. (2021) uses the same source of variation of this paper, FPM revenue shocks. They estimate health spillovers between Brazilian local jurisdictions. In a first stage, they estimate a direct effect in which if a jurisdiction crosses from the lower left bracket to the higher right bracket then it spends more in health, chiefly by hiring new doctors. They also find negative fiscal spillover effects in health expenditures. The estimated spillovers on neighbor jurisdictions are asymmetric though: neighbor jurisdiction with smaller population relative to the focal jurisdiction decrease its health expenditure. However, health expenditures spillover effects on relatively larger neighbor jurisdiction are not identifiable. Furthermore, their estimates indicate the existence of positive health expenditure spillover effects on neighbor jurisdiction's population's health outcomes. In the authors words: *... results suggest a reduction in the spread of infectious diseases in the neighbors, with fewer residents hospitalized with gastrointestinal infections.* This is compatible with the conventional spillover effects mentioned above, it seems that residents from a relatively smaller jurisdiction are consuming public goods from another jurisdiction in a intangible way.

Castro and Mattos (2021) estimate spillovers effects in total expenditures and other specific expenditures. By arguing that the jurisdiction crossing date is exogenous along the years, they estimate expenditures causal spillover effects in form of elasticities between 0.4 and 0.8. In their paper, the spillover effects magnitudes varies according to which FPM population threshold the neighbor jurisdiction is situated in. This means that Castro and Mattos (2021) estimate spillover's effects' heterogeneity across neighbor jurisdiction's relative population size.

In contrast to the previously mentioned studies, Isen (2014) isolate arguable exogenous variation of simultaneous increases in taxation and public expenditures in a focal jurisdiction's effects on a neighbor jurisdiction's fiscal budget. For all jurisdictional types of revenue sources (bonds, income, property, and sales tax), they find evidence of null spillovers effects, ruling out relatively small effects. This paper and Isen (2014) results arise questions about theoretical models that presume spillovers on the local level. Note that, in Isen (2014) context, the source of revenue is simultaneously issued in form of taxation in the focal jurisdiction. In this way, Isen (2014) is estimating the simultaneous taxation and expenditure spillovers effects, which are found to be null. In this paper context, the source of revenues shocks is a transfer from the central government‘s taxes on income and on industrialized products production at a national level.

Oates (1999) makes comments on the basics of Fiscal Federalism theory. He argues that in the absence of monetary prerogatives and with highly open economies that cannot contain much of the expansionary effect of fiscal stimuli, local governments simply have very limited means for traditional macroeconomic control of their economies. Similarly, the easy mobility of labor and capital can seriously constrain attempts to redistribute income inside local jurisdictions. This scenario is very similar to the Brazilian context in which this paper is based upon, where jurisdictions have credit constraint, limited taxation autonomy and most of revenues consists of transfers for specific and general public goods provision. In this scenario, fiscal spillovers are not expected to exist.

3 Institutional Background

Brazil is a Federal system with 26 states and 5,568 local jurisdictions. Jurisdictions are in charge of a significant portion of public goods provision related to education, health, and small-scale infrastructure Corbi et al. (2019). The Fundo de Participação dos Municipios (FPM) plays a major role to the analysed jurisdictions (municipalities). For example, in the median sample year, 2006, the transferred was about to R$29.5 billion Brazilian Reais (US$14.8 billion in 2019 prices) from the national government to local jurisdictions Corbi et al. (2019). The FPM bureaucratic mechanism is not difficult to understand.

Firstly, the central government annually gathers revenues from 23.5% of the income and industrialized products taxes. Note that the annually total fund value varies according to business cycles as it consists of income and industrialized products. These taxes are centrally collected from industries and firms/workers. The FPM fund annually distributes fixed shares to all states according to the their population of 1989Mendes et al. (2008). The total FPM share of each state is fixed and negatively correlated to current states' social economic development level: poor states has a higher share of the fund Brollo et al. (2013).

After each state received its fixed proportion of the totally annually fund, each local jurisdiction receive its share from their corresponding state according to its national level determined coefficient, which in turn, depends on its last year estimated population size. Note that in Census years, every ten years in general, the coefficient depends on the IBGE‘s counted population instead of IBGE‘s population estimations as in the remaining years. Note also that for all Brazilian local jurisdictions inside the same population bracket, the FPM coefficient will be the same. The coefficient determines the share received by a jurisdiction inside of a specific state. In the gathered data used in this paper, the FPM revenue of jurisdiction corresponds to an average about to 35% of total jurisdiction' revenues.

In the gathered data, the average jurisdiction own taxes revenues in terms of total revenues correspond about to 4.7%. However, if local jurisdictions are willing to increase or decrease the 4.7% share, they have instruments to do so. For example, there are three main local taxes attributed to local jurisdictions: they can raise or decrease the IPTU rate (Land and Urban Property Tax), the ITBI rate (Tax on the transfer of real estate), and the ISS (Tax on Services of any kind) or choose to be more or less lenient in these taxes collection. There are also many fees that they can create and/or be more or less lenient with its collection as well. Although it goes beyond this paper scope to estimate by how much they can change the 4.7% rate, it seems to be the case that their public expenditures don‘t react to neighbors‘ public expenditures‘ levels.

It is also important to bear in mind that local revenues are mostly composed of state and of central government's transfers. It is also folk wisdom that Brazilian local jurisdictions has limited ways to get credit. In a limited way, they receive credit for specific projects only upon request to their states, to the central government, to the BNDES (National Development Bank), and/or to other national or international development bank/fund. Furthermore, they can't issue bonds or create new forms of taxation. In this way, the credit restriction tightness depends upon on higher governments‘ spheres discretion.

The variation that made all this and many of other research cited in the Introduction and in Literature Review possible consists of jurisdictions coefficients‘ discontinuities along FPM thresholds across population. In this way, coefficients are a step function $k(.)$ of the estimated population of jurisdictions. The following Equation 1, based on Corbi et al. (2019), shows the fund value received by a jurisdiction i in state k.

$$FPM_i^k = \frac{FPM^k \lambda_i}{\sum_{i \in K} \lambda_i} \tag{1}$$

Where k index state and i local jurisdiction; λ's are local jurisdictions' coefficient based on its population bracket. It can be seen in the Equation 1 that if one jurisdiction has an increase in its population between two years such that it crosses a FPM population threshold, it implies an increase of its coefficient, then its FPM revenue automatically increases. Note, however, that, *ceteris paribus*, the others jurisdiction in the same state will have its FPM revenue marginally decreased.

A relevant question to this work is: where exact those thresholds come from? They are are: 10,188, 13,584, 16,980, and so forth. According to **?**, the initial rules are from 1967, when they created thresholds at multiples of 2,000 up to 10,000, then every 4,000 up to 30,000, and so forth. The rules also stipulated that these cutoffs should be updated proportionally with population increments along the time. Litschig and Morrison (2013) claims that the thresholds were updated twice, once with the census of 1970 and then with the census of 1980, which explains the "odd" numbers. It is noteworthy that the thresholds are still equidistant from one another, the distance being 6,792 for the first 7 cutoffs, except for the second cutoff, which lies exactly halfway in between the first and the third cutoffs.

In order to illustrate the FPM mechanism, the Figure 1 in the Introduction plots jurisdiction's FPM revenues in 10,000 BRL for all jurisdictions with less than 50,000 inhabitants over the estimated population in the previous period. Black lines are running-means over population bins of 200 inhabitants. Red vertical lines are transfers‘ coefficients population thresholds. Note that when the population crosses the red lines (thresholds), discontinuities between FPM revenues emerges. They are lower in the left hand side bracket and higher in right hand side bracket. Furthermore, notice that the discontinuity is visible at a national level even-though FPM revenues differs between jurisdictions with same coefficient in different states.

4 Data and descriptive statistics

In the gathered data[1], the unit of observation is a pair of all jurisdictions up to 30 km radius distance between each other. The Appendix reproduces the main results for 20 and 10km distance between jurisdictions. The data is a panel covering years from 1999 to 2014. In the data, lines are units of observation in a given year and the columns are their variables.

The source of variables are from Brazilian institutions. The public budget and finance variables were retrieved from Financas do Brasil - FINBRA - (Finances from Brazil). The FPM transfers variable was retrieved from the Secretaria do Tesouro Nacional - STN - National Treasury. The population estimates are from the IBGE. The variables related to local labor market are from the Relação Anual de Informações Sociais - RAIS - (Ministry of Labor Administrative Dataset). The data set containing the pair of municipalities with the radius distance between them was provided by IDados[2] a for-profit Brazilian company. These data are high-quality survey and administrative data.

In the gathered data, there are 10,686 pairs of jurisdiction and 2,634 neighbors-focal jurisdictions. The jurisdictions population growth 10th percentile is about to -2%, the median is around to 0.8% and the 90th percentile is around 4%. In other words, the majority of the jurisdictions have positive growth. The population growth distribution is skewed to the right of the zero growth mark. This means that there are more jurisdiction crossing from the left lower bracket to the right higher bracket of the FPM population threshold than jurisdiction crossing from the right higher bracket to the left lower bracket of the threshold, see Figure 1.

Table 1 summarize key variables‘ statistics for the whole gathered data. Notice that there are only jurisdictions with less than 50.000 inhabitants in the sample. The average jurisdiction has almost 18.000 inhabitants growing at a 1.02% rate. 32% of its revenue consists of FPM transfers, and only 5.8% of the revenues consists of local taxation. The FPM per capita is about to 162 BRL. Total revenue and expenditure per capita are 566 and 551 BRL respectively. Health and Education expenditures are 125 and 180 BRL, respectively. The average jurisdiction has 562 public employees and 1569 private employees with almost 200 firms.

In what refers to revenues, a variation in a focal jurisdiction's revenue might have effects that go beyond this jurisdiction borders and affect neighboring jurisdictions, this is what this paper refers as spillovers.

[1] This papers merges Corbi et al. (2019)‘s data base with a data containing the distance between each jurisdiction and all other jurisdictions

[2] https://idados.id/

Table 1: Summary statistics whole sample

Variable	Obs	Mean	Std. Dev.
Population	119122	17866.63	9438.2
Pop. growth %	119122	1.02	4.23
FPM revenue %	119122	32.33	11.29
Own local revenue %	119075	5.79	11.14
FPM p.c.	119122	162.9	46.66
Total Revenues p.c.	119122	566.18	304
Expenditure	119122	550.96	278.01
Health expenditures p.c.	118506	125.37	71.65
Education expenditures p.c.	118759	180.18	85.9
Pub. wage bill p.c.	119031	262.07	130.79
Public Sector Employment	118125	562.14	329.48
Private Sector Employment	118984	1569.46	2139.36
Number of Firms	118984	196.16	229.7

This paper consider the case in which a focal jurisdiction receive 20% more revenue transfers from an external revenue source without having to directly tax its inhabitants. In this context, if the focal jurisdiction has a population near a randomly[3] chosen threshold and crosses it between two years, its FPM transfers are very likely to abruptly change. Theoretically, these FPM revenue transfers can have positive, negative or null spillover effects on the neighboring jurisdiction.

This paper is an Event Study that builds on Siegloch et al. (2021) model. A neighbor jurisdiction can have many focal jurisdictions and a positive event occurs only when one of them stay at the left lower bracket for at least three years, crosses the population threshold from the lower left bracket to the right higher bracket, staying at least three years there. Note that none of the other focal jurisdictions can move to other bracket during the whole event time span: seven years. A negative event happens in the reverse order, the neighbor jurisdiction has only one focal jurisdiction that stays in the higher right bracket for at least three years, move to the lower left bracket, and stays in it for at least three more years. Similarly to the positive event, in the negative event, the neighbor jurisdiction can have many focal jurisdictions, but none of them can change its bracket during the whole event time span: seven years. Events potentially increases (decreases) the focal jurisdiction's FPM coefficient, which potentially increase (decrease) its FPM's revenues. The central idea is to exploit these potential FPM revenue shocks' positive, negative or null spillovers effects from focal jurisdiction to neighbors jurisdiction.

In this context, the gathered data is separated in three groups of sub-samples. The first sub-sample,

[3]The thresholds were randomly chosen in 1967 and updated twice according to Brazilian population growth (1970 and 1980) **?**

denoted as comparison group, consists of pairs of jurisdictions in which all focal jurisdictions didn't cross a threshold for the whole event time span. In the second sub-sample, denoted as positive event group, the neighbors jurisdictions might have many focal jurisdiction, but only one focal jurisdiction incurring in a positive event and none of them incurs in a negative event to be defined. Meanwhile, the third sub-sample, denoted as negative event group, consists of neighbors jurisdictions that might have many focal jurisdiction, but only one has a negative event and none of them has a positive event. Note that, in the analysis to be done, the positive and negative groups are compared to the same comparison group.

The following Table 2 show the main variables averages and standard deviation for comparison, positive and negative events groups. In the following analysis, Table 2's columns 2a and 2b content refers to the comparison group and can be analysed in terms of columns 3 (positive event) and 4 (negative event) groups. The first noticeable difference between the samples is the number of observations. The comparison group has roughly 11000 neighbors jurisdictions, the positive event group has about to 8500 neighbors observations and the negative event group has roughly 1100 observations. It means that for a given neighbor jurisdiction, it is more likely having all focal jurisdictions in the same bracket during the whole time event span than having a positive event, and these both types of events are more likely to happen than a rare negative event.

Table 2: Neighbor's summary statistics for all sub-samples

	Comparison. N ∼ 11k		Positive N ∼ 8k		Negative N ∼ 1k	
Variables (1)	Mean (2a)	Std (2b)	Mean (3a)	Std (3b)	Mean (4a)	Std (4b)
Population	17042	9449	18102	9586	16600	9435
Pop. growth %	0.8	4.4	1.0	4.1	0.4	6.3
FPM revenue %	32	10	32	10	33	9
Own revenue %	5	4	6	5	5	4
FPM R$ p.c.	163	48	161	47	167	49
Revenues R$ p.c.	570	283	569	319	567	440
Expenditure R$ p.c.	553	255	554	304	539	373
Health R$ p.c.	129	74	128	80	120	76
Education R$ p.c.	176	80	175	86	167	87
Pub. wage bill R$ p.c.	255	121	258	138	243	131
Public Employ.	511	306	558	343	479	279
Private Employ.	1423	1958	1568	2052	1218	1733
Number of Firms	204	221	209	217	184	196

Table 2 shows neighbor jurisdiction's summary statistics of main variables. Column 1 refers to the variable's name, columns 2a and 2b refer to the comparison sub-sample means and standard deviation respectively, columns 3a and 3b refer to the positive event sub-sample, column 4a and 4b refers to negative event sub-sample. Variables number of observation marginally varies.

Table 2 shows in columns 2 and 3 that comparison and positive event groups are similar in many dimensions. For example, comparison and positive event neighbors have roughly the same percentage of FPM transfers in terms of total revenues: 32%. They also have (roughly) the same total revenue per capita: around 570 BRL. Note also that the total public expenditure per capita are roughly the same: 553 BRL. The same is valid for expenditures in education, health and personnel (wage bill). Finally, both groups have the same[4] mean for number of firms, which is around 205. As a matter of fact, all the standard tests conjecturing the null hypothesis that these variables means' differences between groups are equal to zero cannot be rejected within a 95% confidence interval. However, assuming both groups are from the same sample, the standard tests rejects this same null hypothesis for all other variables.

Although the tests reject the null hypothesis of equal means for all other variables, the averages differences between the positive and comparison groups have small magnitudes. For example, the average comparison neighbor has FPM revenue per capita of 163 BRL and the average positive event neighbor has FPM revenue per capita of 161 BRL. The average comparison and positive event neighbors have their own revenues in terms of total revenues around 5.49% and 5.93% respectively, which are very close as well. Similarly, as can be seen in Table 2's columns 2a and 3a, the average comparison neighbor has 17000 inhabitants growing at roughly 0.8% rate per year. Meanwhile, the average positive event neighbor has about to 18100 inhabitants growing at 1.0% rate per year. There is also a small difference in the number of private and public employees for the average positive event and comparison neighbors. The average positive event neighbor has 510 public employees, while the average comparison neighbor has 557. The average positive event and comparison neighbor have 1420 and 1560 private employees, respectively.

In what refers to the negative event and comparison groups, when assuming they are from the same sample, standard mean differences tests can not reject the null hypothesis of equality for many variables. They are: population, own revenues in terms of total revenues, total revenues per capita and total expenditure per capita.

Although it is not possible to reject the null hypothesis of means equality for all the other variables, the average comparison neighbor and the average negative event neighbor have small magnitude differences for many dimensions. For example, total FPM revenues are 31% and 32% for the average comparison and for the average negative event neighbor, respectively. As previously stated, the average comparison neighbor has FPM transfers per capita of 163 BRL, while the average negative neighbor has FPM transfers per capita of 167 BRL. The average comparison and negative event neighbors have total expenditures per capita in health and education differences by 8 BRL each, a very small magnitude.

On the other hand, the average comparison and negative event have dimensions that are economic relevant. For example, the average comparison neighbor population grows at a rate of 0.8%, this is twice the rate of the average negative neighbor: 0.4%. The average comparison and negative neighbors have also relevant differences in what refers to the labor market variables. The average comparison neighbor has 31 public employees more than the 479 public employees in the average negative neighbor and 204 more pri-

[4]not statistically different within an interval of 95%

vate employees out of the 1218 private employees in the average negative event neighbor. The comparison group has 20 more firms than the 184 firms in the average negative event neighbor.

This section has explained the Brazilian local jurisdictions public budget context, chiefly in what refers to the FPM. The FPM institutional setting was also presented. The FPM institutional design created the main source of variation that allowed the Event Study possible. This paper selects three different subsamples of the whole gathered data, this section summarized their characteristics through the main variables' averages and standard deviation. No major difference between positive and comparison group was found. Although labor market variables between negative and comparison groups were remarkable different.

5 Empirical strategy

This section explains the Event Study method used, which is based in Siegloch et al. (2021). It also describes the identifying assumption used to identify the spillover causal effects. The equation of the regressing model to be estimated is also specified, which not only identify the spillover effect, but also the direct effects, which consists of the FPM revenues shocks effects on its own jurisdiction's outcomes. Finally, internal validity tests are verified.

In order to estimate causal spillover effects, the ideal experiment would randomly assign the three subsamples definitions to jurisdiction in Brazil. The sample would consist of randomly neighbor jurisdictions from the Brazilian pairs of jurisdictions up to 30km radius distance between each other and with less than 50,000 inhabitants. In order to circumvent the impossibility of such an experiment, it is assumed that the event of a focal jurisdiction crossing the threshold is exogenous to their neighbors' outcomes.

A threshold crossing occurs when the estimated or counted population has an increase or decrease great enough for it being able to cross the threshold. The origin of this population change is very important for the study event identification assumption. For example, a possible threat the identification assumption would occur if neighbor's decision makers manipulate the population size of the focal jurisdiction so it has a crossing threshold event. However, it is hard to imagine that decision makers would intentionally influence population growth in other jurisdiction to make it incurring in an event. Furthermore, the FPM's coefficients are based upon population estimations by the IBGE, taking into account national and state population growth and migration. The population estimation procedure is not directly manipulated by the IBGE.

It is important to note that Castro and Mattos (2021) estimate spillovers effects in total expenditures as well, in their fuzzy RD analysis, regarding the running variable (population) manipulation near the threshold, the authors argue that even if this is the case in a specific year, decision makers cannot directly influence population growth along the years. As in Castro and Mattos (2021) setting, this papers consider a neighbor jurisdiction along time.

Notwithstanding, Castro and Mattos (2021) present empirical evidence that local jurisdictions‘ population growth is continuously distributed across the thresholds. Therefore an event of a focal jurisdictions‘ population crossing the FPM's thresholds is likely to be orthogonal to any fiscal, demographic, or even electoral preference of a neighbor jurisdiction. Finally, Castro and Mattos (2021) also show empirical evidence that bordering jurisdictions population growth is not affected, on average, by the jurisdiction's position along the fixed population threshold, whether on the right higher brackets or in the lower left brackets. Finally, they also show empirical evidence that population growth has a continuous distribution near the thresholds.

In this context, the following regression specification allows the estimation of the first stage and the spillover effects:

$$Y_{ijt} = \sum_{l=-4}^{4} \beta_l D_{ijl} + \delta_i + \gamma_t + \epsilon_{ijt} \tag{2}$$

Where i indexes neighbor, j indexes focal jurisdiction and t indexes time. Y_{ijt} denotes the outcome of neighbor jurisdiction i, pair of the focal jurisdiction j, at period t. D_{ijl} consists of dummies assuming value one for periods within the event time span happening for neighbor i pair of focal jurisdiction j, assuming value zero otherwise. It can be lag periods $4 < l < 0$, lead periods $0 < l < 4$ or crossing period: $l = 0$. Note that Equation 2 is a two-way fixed effects regression. The first fixed effect refers to δ_i, which controls for neighbor jurisdiction fixed characteristics across time. The second fixed effects refers to γ_t, which controls for time specific variations correlated to the event assignment along time. ϵ_{ijt} is assumed to be an independent and identically distributed (i.i.d.) error. The identification assumption is that $E[\epsilon_{itj}|D_{itj}, \delta_i, \gamma_t] = 0 \quad \forall \quad t \in \{-12, ...12\}$ holds.

The coefficients of interest are β_l for all $l \in \{-4, ..., 4\}$ such that $l \neq -1$. Note that the one year after crossing threshold period refers to $l = 0$. β_l such that $l \geq 0$ refer to the causal spillover effects of the focal jurisdiction crossing the threshold at $t = l$ in terms of the year previous to the threshold crossing $l = -1$ joint with the comparison group.

By omitting β_{-1} and inputting -1 to the comparison group observations, the coefficient β_{-4} refers to a cyclical component in the outcome at four or more years before the focal jurisdiction crossing the threshold in terms of the year previous to the threshold crossing and in terms of the comparison group. β_{-3} and β_{-2} refer to the outcome variation inside the window of periods that the focal jurisdiction remained in the bracket before crossing at periods $t = \{-2, -3\}$. Meanwhile, β_0, β_1, β_2 and β_3 refer the spillover effects one, two, three, and four or more years after the crossing period in terms of the year before the threshold crossing and comparison group, respectively. The appendix reproduces main results for when the comparison group is not imputed within the focal jurisdiction one year before the threshold crossing.

Equation 2 estimation method is Ordinary Least Squares (OLS) with standard errors clustered at the micro-region level. This procedure follows Corbi et al. (2019), that uses standard errors robust to heteroskedasticity clustered at the micro-region level. IBGE defines micro-region as: *groups of economically* *integrated municipalities sharing borders and structure of production* BRASIL (1990). In the population of interest, from which the neighbor jurisdictions are sampled, there are 460 micro regions and 60% of the pair of neighbor and focal jurisdictions are in the same micro-region.

Note that the leads and lags in dummies D_{ijt}'s are binned up to an absolute value of four. That is, neighbor jurisdictions with focal jurisdictions that stayed more than four periods in the same bracket before crossing the threshold have its lag period ($t < -4$) binned up to minus four ($t = -4$). Conversely, neighbor jurisdictions with focal jurisdictions that remained more than four periods at the same bracket after crossing the threshold have its period $t > 4$ truncated to four ($t = 4$). Remembering that the comparison group is included in the regression estimation by imputing them to the dummy D_{ijj} at $t = -1$, one period before the focal jurisdiction crossing the threshold.

One possible threat to the identification assumption is the existence of systematic shocks causing population migration between focal and neighbor jurisdiction. For example, if systematic shocks causes neighbor‘s citizens to migrate to focal jurisdiction, then the focal can incur in a positive event due to these shocks violating the identification assumption. In this context, it is possible to test if focal events are correlated with neighbor population by running a regression of the specification in Equation 2 with logarithm of neighbor‘s population as an outcome. A null correlation supports the identification assumption. Figure 2 plot coefficients of a regression from Equation 2 with logarithm of neighbor‘s population as an outcome. Dots with lines are point estimations within confidence interval of 95% estimations, relative to the previous year of the event and comparison group observations.

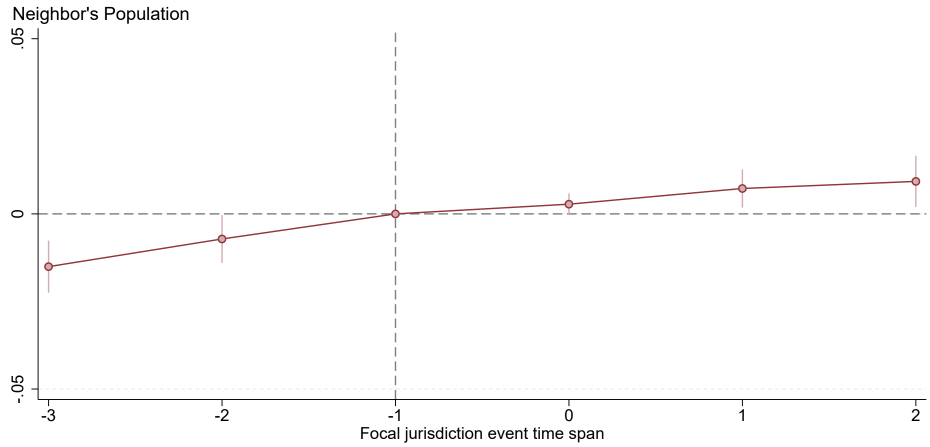
Figure 2a shows null correlations for almost all the time event span, except for four years or more. This is a very small positive correlation between positive event and logarithm of neighbor‘s population. Notice that the correlation sign is very small and can be neglected. This correlation might arise due to the small positive correlation between neighbor and focal populations and this is not a concern since it is easy to control for neighbor correlation in the analysis, as done in the Appendix. Figure 2b shows that null correlations cannot be ruled out for the first two years after the event, but very noisy negative and different from zero correlations are estimated for three years after the event. Again, this is not sufficient evidence against the identification assumption, since it very noisy and small. This correlation might arise due to the small positive correlation between neighbor and focal jurisdiction‘s population, which is addressed in the Appendix.

The following tables help to understand and visualize the event design setting. Table 3 tabulates variable D_{itj} values frequency in the positive event sub-sample. In other words, the frequency of neighbors inside the positive event sample along the time event span can be seen in the Table 3. Note that the Table 3 inputs the comparison group at $t = -1$ together with the neighbor's that are one period before, ($t = -1$), the crossing date, ($t = 0$).

First, Table 3 shows that D_{ijt}'s distribution has heavy tails at periods $t = 4$ and $t = -4$. This happens because of the binning up at periods $t \in \{-4, 4\}$: neighbor jurisdictions with focal jurisdictions that

Figure 2: Internal Validity Test: events correlation with neighbor's population.

(a) Positive event sample



(b) Negative event sample

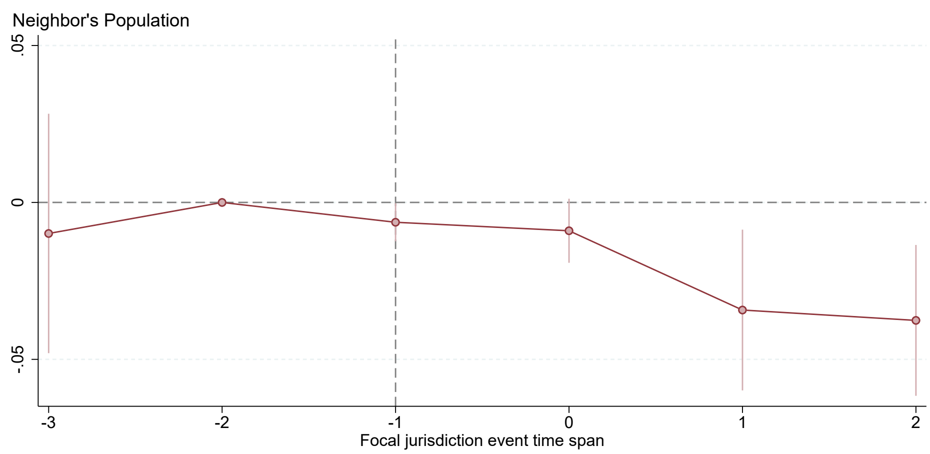


Figure 2 show estimates of positive and negative events' correlation with neighbor's population. This can be seen as an internal validity test, null correlation with neighbor's population favors the identification assumption. Figure 2a use positive events sample and shows a very weak positive correlation between the event and neighbor's population. Null correlation cannot be ruled out in most of the event time span. Figure 2 uses negative event sample and imprecisely estimates null correlation between focal's negative event and neighbor's FPM revenues. Dots with lines are point estimations within confidence interval of 95% estimations. The estimates are relative to the previous year of the event and comparison group variations.

stayed in the same bracket longer than four periods after or before crossing the threshold at $t = 0$ are normalized in a way such that $D_{ijt=4} = 1$ for all t such that $t \geq 4$, conversely, $D_{ijt=-4} = 1$ for all t such that $t \leq -4$. Secondly, it can be seen a discontinuity at period $t = -1$ before the crossing period $t = 0$. This fact is due to the normalization of the comparison group to this time period as explained above. Note that

Table 3: Frequency of positive D_{itj}

D	Freq.	Percent	Cum.
-4	2,240	6.46	6.46
-3	556	1.6	8.06
-2	561	1.62	9.67
-1	27,135	78.2	87.87
0	587	1.69	89.56
1	578	1.67	91.23
2	547	1.58	92.8
3	545	1.57	94.37
4	1,952	5.63	100
Total	34,701	100	-

Table 3 tabulates frequency of neighbors inside the positive event sample along the time event span. Note that the Table inputs the comparison group at $t = -1$ together with the neighbor's that are one period before the crossing date ($t = 0$)

in Equation 2, D_{ijt} is a dummy for each value of Table 3. If one ignores the above mentioned normalization, the distribution of jurisdiction across the whole event time: $t \in \{-12, .., 12\}$ seems to have a normal distribution with mean $t = 0$.

In what refers to group of neighbors incurring in a negative event, the frequency of jurisdictions pairs along the event time span can be seen in the Table 4 below.

Table 4: Frequency of negative D_{itj}

D	Freq.	Percent	Cum.
-4	199	0.72	0.72
-3	69	0.25	0.97
-2	76	0.27	1.24
-1	26,693	96.42	97.67
0	76	0.27	97.94
1	75	0.27	98.21
2	73	0.26	98.48
3	64	0.23	98.71
4	358	1.29	100
Total	27,683	100	

Table 4 tabulates frequency of neighbors inside the negative event sample along the time event span. Note that the Table inputs the comparison group at $t = -1$ together with the neighbor's that are one period before the crossing date ($t = 0$)

The same patterns observed in Table 3 can be seen in Table 4, except to some differences. For example, total frequency of pairs of jurisdictions in Table 3 is 34,701, while in Table 4, it is 27,683. It means that sampling a neighbor pair with positive events is more likely to occur than sampling a neighbor in which a negative event occurs. This is due to the fact that the majority of jurisdictions has a positive population growth.

Figure 3 shows the number of neighbors that incurred in positive events across years. The number of positive events in each year is reported in Figure 3a. Along 2002 and 2011, there are 587 events with an average of 37 events per year. It can be seen that 2002, 2008 and 2011 are outliers. Meanwhile, the number of negative events in each year is reported in Figure 3b. Negative events are rarer than positive events and less evenly spaced across years. There are 76 negative events with an average of 5 events per year.

This section explained the methodology that will be used to estimate the causal spillover effects of a FPM revenue shock in a focal jurisdiction on outcomes of a neighbor jurisdiction. The Equation 2 specified the regression specification and the coefficients of interest to be estimated are explained. Finally, the frequency of pairs of jurisdiction distribution through the event time span was tabulated in Tables 3 and 4. Figure 3 showed the events distribution along the years. The next section will present and interpret the estimation results.

6 Results

This section will show the results of the $\hat{\beta}_t$'s values and their 95% confidence interval, they are estimates of Equation 2 presented in a set of Figures and then summarized in a Table. Firstly, it will be presented estimated direct effects of a focal jurisdiction crossing a threshold on its own outcomes. Secondly, the spillover effects on neighbor's outcomes will be estimated in a reduced form way and presented in Figures. These spillover effects results' heterogeneity across neighbor jurisdiction's population size will be analysed after that. Finally. results will be summarized in a Table with a 2SLS regression outputs.

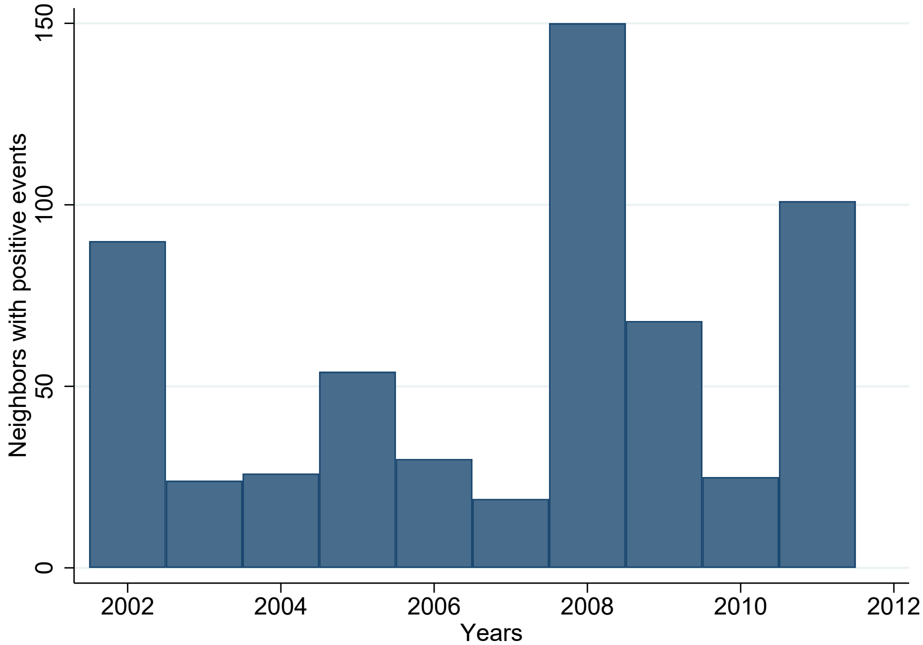
The first stage consists of estimating the crossing threshold effects on own jurisdiction's outcomes. For example, it will be estimated the event direct effects on jurisdictions own FPM revenues, total expenditures and public employment, all in logarithms. Therefore, the coefficients are the estimated percentage variation caused by the event, or in a reduced form way, by variations in FPM revenues.

Figure 4 shows the positive event effects on neighbor (blue line) and focal (red line) jurisdictions FPM revenues. Note that in the Figure 4, the comparison group and the jurisdiction in the year before crossing the threshold are normalized to zero $x = -1$. This happens because they are the omitted dummy in Equation 2 estimation. Dots with lines are point estimations within confidence interval of 95% estimations, relative to the previous year of the event and comparison group variations.

It can be seen that the one year after crossing the threshold, $x = 0$, the focal jurisdiction increases its FPM revenues by 20% relative to its FPM level at $t = -1$ together with the comparison group. It can also

Figure 3: Number of neighbors with positive and negative events across years.

(a) Positive event sample



(b) Negative event sample

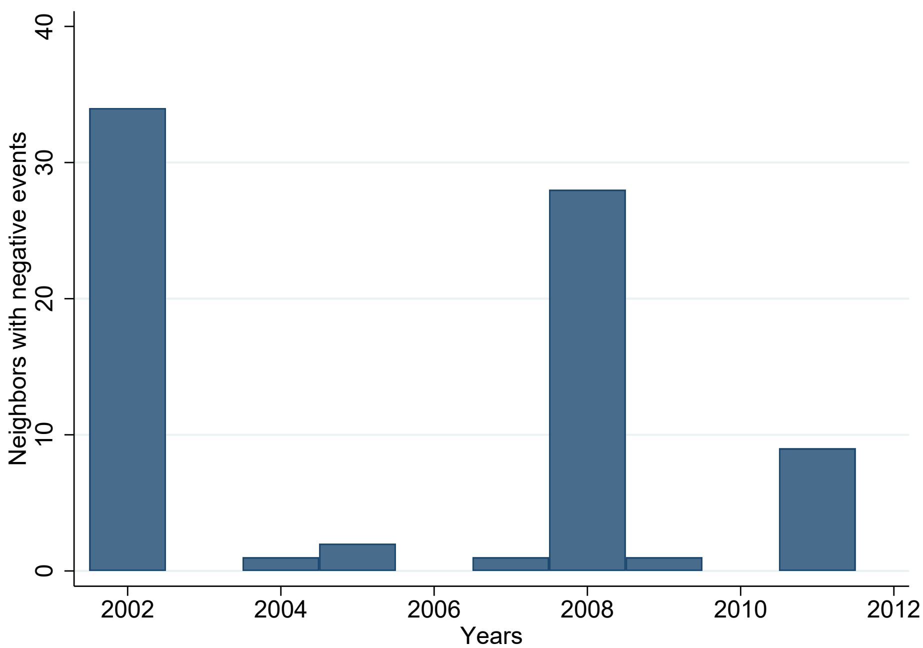


Figure 3 shows the number of neighbors that incurred in positive events across years. The number of positive events in each year is reported in Figure 3a. Meanwhile, the number of negative events in each year is reported in Figure 3b.

be seen in Figure 4 that the FPM relative variation remains around 20% in the years following the the first year after crossing e.g. at $x \in \{1, 2\}$.

Note that if a focal jurisdiction crosses the threshold and this has a relevant effect on the neighbor jurisdiction's FPM revenues, then it is very likely that the neighbor jurisdiction also has crossed the threshold. In turn, this would arise an endogenous relationship between neighbor and focal jurisdictions' FPM revenues and confound the spillover effect estimates. The blue line in Figure 4 shows the effect of a focal jurisdiction crossing the threshold on the log of neighbor jurisdiction's FPM revenues. The blue line in Figure **??** shows estimates that indicate that the positive event has null effects on neighbor‘s FPM revenues.

In summary, the red line in Figure 4 show estimates indicating that If a focal jurisdiction crosses a population threshold, then its own FPM revenues "permanently" increase by 20%. Furthermore, the blue line shows estimates indicating that events' effects are null on neighbor‘s FPM revenues.

Figure 4: Positive event direct effects FPM's revenues

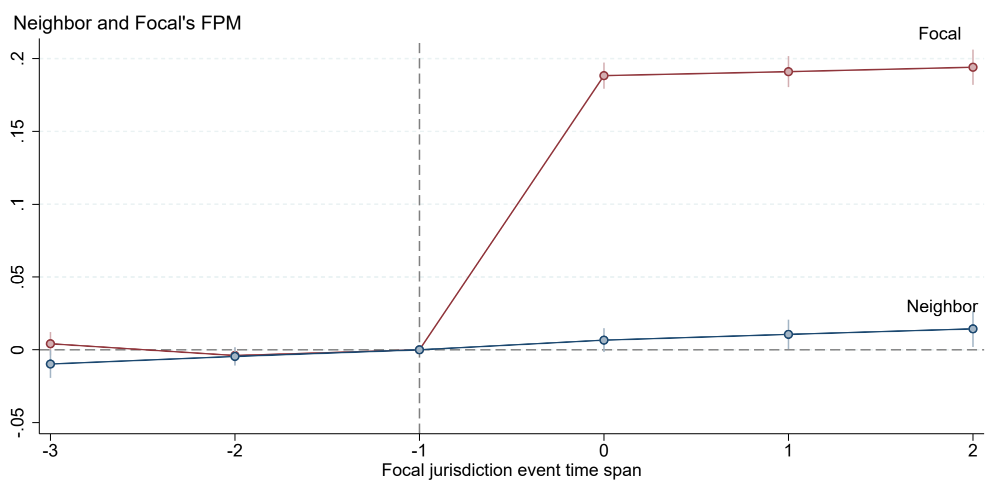


Figure 4 show effects estimates using positive event sample: if a focal jurisdiction crosses a population threshold then its own FPM's revenues increase by 20% (red line) and neighbor‘s FPM revenue remain constant (blue line). Dots with lines are point estimations within confidence interval of 95% estimations. The estimates are relative to the previous year of the event and comparison group variations.

Figure 5 shows the negative event first stage estimations. It can be seen that the FPM revenues vary inside a small confidence interval in years before the threshold crossing, $x < -1$. There is an exception, four years before the crossing date, a very imprecisely estimate of -10% variation on FPM revenues. A possible explanation is that jurisdictions for all periods before four years was truncated to -4 in estimations.

The variation in FPM revenues is a decreasing function of time after the crossing date, beggining with a point estimation of -15% in the first year after the crossing date and reaching -20% in the last years. Note that for a negative event, the causal estimates have high variance e.g. point estimates are less precise than the positive event. This is due to the fact that there are fewer jurisdiction in the negative event sample.

Figure 5: Negative Event direct effects on FPM's revenues

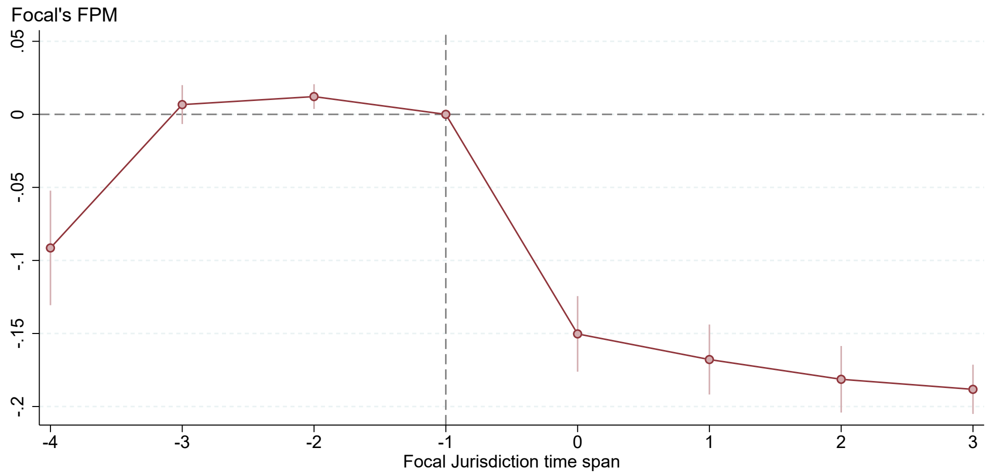


Figure 5 show direct effects estimates using negative event sample: if a focal jurisdiction crosses a population threshold then FPM's revenues increase by 15% in the first year, then 20$ four years or more after the event. Dots with lines are point estimations within confidence interval of 95% estimations. The estimates are relative to the previous year of the event and comparison group variations.

Figure 6 shows the event direct effects on focal jurisdiction total expenditure. In general, estimates of the event before crossing the threshold are not statistically different from zero. Although there is the four years before crossing the threshold exception.

Figure 6a shows the direct effects of the event for the positive event sample. Note that the positive event has a 20% increase on FPM revenues, while the effect on total expenditure estimation is 10%. By a back-of-the-envelope calculation, 50% of the extra FPM revenue goes to total expenditures. Figure 6b shows the direct effect of the event for the negative event sample. Note that the negative event has a -15% to -20% variation on FPM revenues along time, while this estimated effect is -5% to -10% on total expenditures. Using a back-of-the-envelope calculation, a -15% to -20% reduction in FPM revenues is related to -33% to -50% reduction in Expenditures.

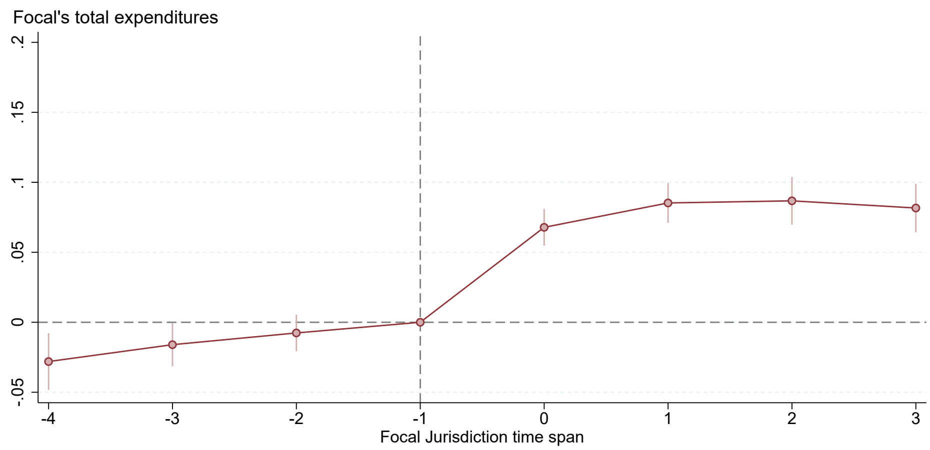
Figure 7 shows the event direct effects on focal jurisdiction local taxation. Estimates of the direct effects in both events' samples are smaller than zero, but very imprecise.

Figure 7a shows the direct effects estimates of the event for the positive event sample. The estimated direct effects has zero point estimates for all time span after the crossing date, but they lie inside confidence intervals between $[-5\%, 5\%]$. Considering results of Corbi et al. (2019) and the evidence pointed out by Figure 7a, it is very likely that the direct effect on local taxation is null, at least for the positive event sample. On the other hand, Figure 6b shows that the direct effect, for the negative event sample, have very imprecise estimates. It is not possible to make any proper inference from the Figure 6b.

Figure 8 shows the event direct effect on jurisdiction's public employees. Estimates before the crossing

Figure 6: First stage: direct effects on Focal's total expenditures

(a) Positive event sample



(b) Negative event sample

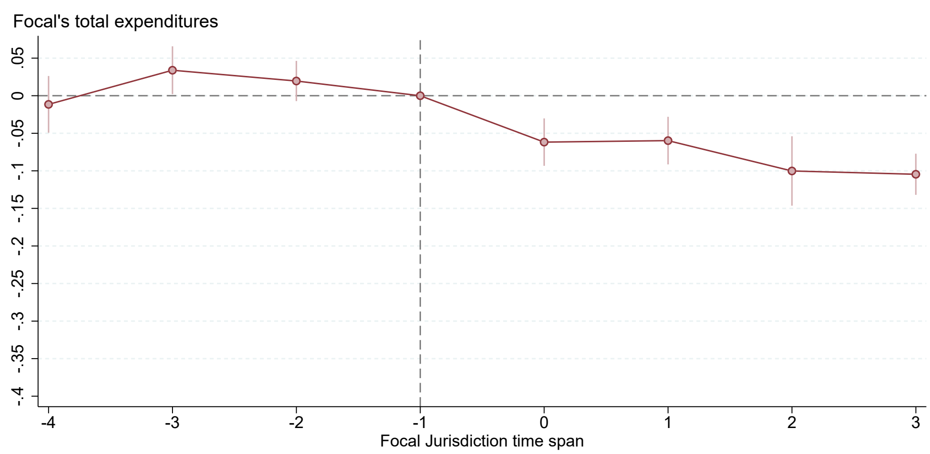
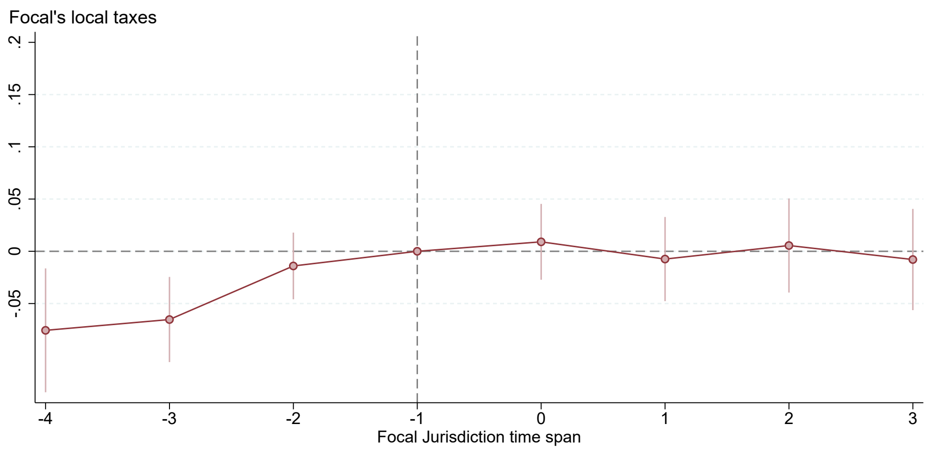


Figure 6 show direct effects on total public expenditures estimates using positive and then negative event sample. In the first year, the positive event direct effect is around 7.5% in the first year, then 10$ four years or more after the event. These variations for the negative event are 5% and then 10%. Dots with lines are point estimations within confidence interval of 95% estimations. The estimates are relative to the previous year of the event and comparison group variations.

Figure 7: First stage: direct effects on Focal's local taxation

(a) Positive event sample



(b) Negative event sample

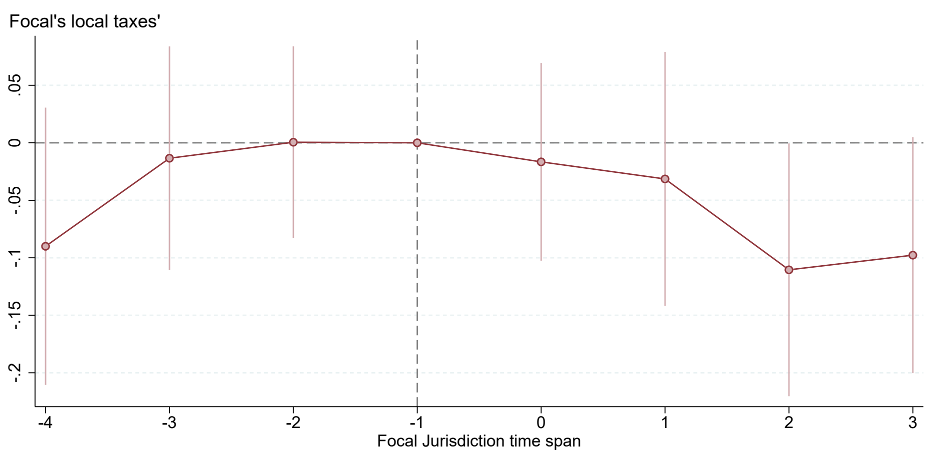


Figure **??** show direct effects on local taxes estimates using positive and then negative event sample. In the first year, the positive event estimates points out to null direct effect within a confidence interval of $[-5\%, 5\%]$ along all time event span. These variations for the negative event are very imprecise and inference is very difficult to be done. Dots with lines are point estimations within confidence interval of 95% estimations. The estimates are relative to the previous year of the event and comparison group variations.

threshold date are not statistically different from zero.

Figure 8a shows the direct effect on public employees for the positive event sample. Note that the positive event has a estimated effect of 1% ($x = 0$) increase on public employees, one year after the event, while this value estimation is 6% four years after the event ($x = 3$). Note that these are imprecise estimations, though two years after the crossing threshold date, estimated direct effects are all positive within a 95% confidence interval. In a reduced form estimation, a 20% positive variation in FPM revenues, increases the number of public employees about to 6% in four or more years after the event. Figure 8b shows the direct effect on public employees for the negative event sample. Note that the estimations are very imprecise. The only event's effect estimation in which zero is outside the 95% confidence interval is negative and occurs only four years or more after the event. The effect estimation varies between -2.5% and -15% within the confidence interval. This means that a -20% decrease in FPM revenues, in a reduced form estimation, has an effect inside a window of $[-15\%, -2.5\%]$ on public employees four years or more after the FPM revenue decreased.

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6.1 Second Stage

Now this paper estimates test spillover effects of focal's revenues' shocks on neighbor's total public expenditures. Figure 9 show estimates of Equation's 2 β_t coefficients as dots inside lines representing their confidence interval. Figure 9a uses the positive event sample, while Figure **??** uses the negative event sample.

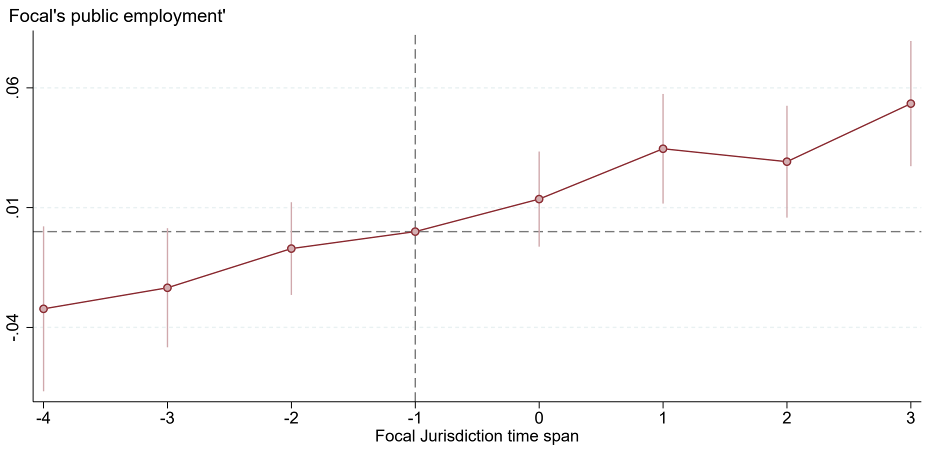
Figure 9 present very precise null spillovers effects' estimates. It seems to be the case that positive shocks on focal jurisdiction's FPM's revenues have no effects on neighbor jurisdiction's total expenditures. Note that 6 shows estimates of positive direct effects on total expenditures, in accordance with Corbi et al. (2019) and Castro and Mattos (2021). However, the estimates reveals null spillover effects from the direct effects. Note that confidence interval of 95% are very small. Figure 9b is analogous to Figure 5, but estimations use the negative event sample. Although the spillover effects estimates are still around zero, they are less precise, as in all negative events analysis. In this context, estimates of spillover effects are zeros inside confidence intervals around $\{-5\%, 5\%\}$. The precision of the null spillover effects' estimates highly increases when restricting the sample to neighbors that have smaller population size than the focal jurisdiction, as it will be shown in the next subsection.

Figure 10 shows estimates of the second stage for Total Public Employment. In other words, it estimate the spillovers effects on neighbor's public employment. Similarly to previously analysis, Figure 10a uses the positive event sample and Figure 10b uses the negative event sample.

Figure 10a presents precise null spillover effect on neighbor's public employment. For all years after the crossing threshold date, the estimated percentage change in public employment is very close to zero. They have not so large confidence intervals inside the window $[-3\%, 5\%]$. It is noteworthy that precision of estimates increases if the positive event sample is restricted to neighbors with smaller population than fo-

Figure 8: First stage: direct effects on Focal's public employees

(a) Positive event sample



(b) Negative event sample

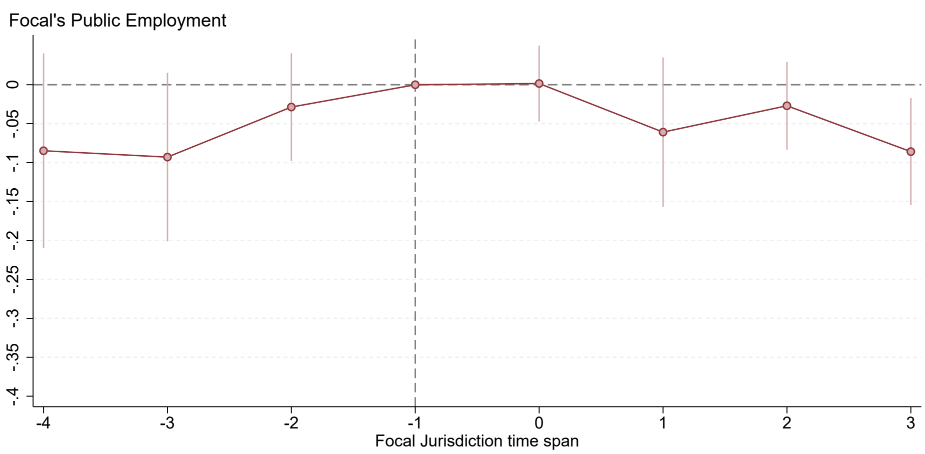
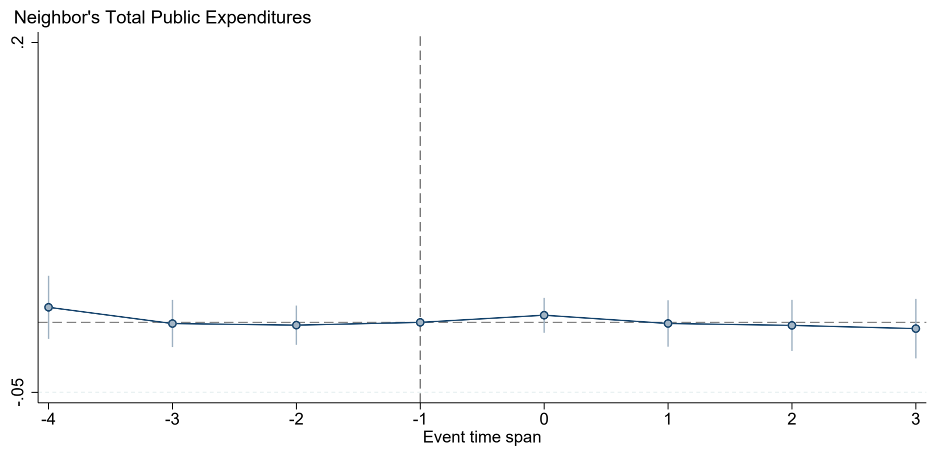


Figure 8 show direct effects on Public Employment estimates using positive and then negative event sample. In the first year, the positive event estimates points out to a positive variation between [0%,3%], four years or later after the event the estimates has a confidence interval within [3%,7%]. For the negative event, estimates are still imprecise, after the event, null direct effects can be ruled out only for four years or more after the event, the estimate's confidence interval is within [-15%,-1%]. Inference is very difficult to be done. Dots with lines are point estimations within confidence interval of 95% estimations. The estimates are relative to the previous year of the event and comparison group variations.

Figure 9: Second stage: Spillovers Effects Neighbors' Total Expenditures

(a) Positive event sample



(b) Negative event sample

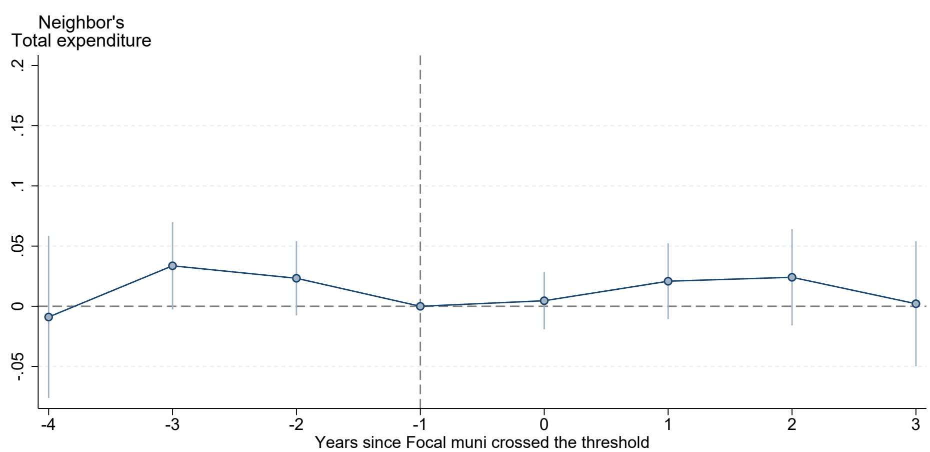


Figure 9 show estimates of spillover effects on neighbor's total public expenditure. Figure 9a use positive events sample, for all years, before and after the event, precise estimates indicate null spillover effects. Figure 9b uses the negative event sample and estimates are less precise, although null direct effect cannot be ruled out. Dots with lines are point estimations within confidence interval of 95% estimations. The estimates are relative to the previous year of the event and comparison group variations.

cal jurisdiction. Figure 10b presents imprecise negative spillover effects estimates on neighbor's public employment. For all years after the focal crosses the threshold, the percentage change in public employment is very close to zero. They have large confidence intervals inside the window $[-15\%, 0\%]$ for the whole event time span. Estimates don't change when restricting the sample to smaller neighbors, though precision increases.

Figure 11 shows estimates of spillover effects on neighbor's Total Private Employment. In other words, it estimate the focal's FPM revenue shock spillovers effects on neighbor's private employment. Similarly to previously analysis, Figure 11a uses the positive event sample and Figure 11b uses the negative event sample.

Figure 11a presents imprecise estimates of the focal event's spillover effect on neighbor's private employment. For all years after the focal jurisdiction crosses the threshold, the point estimates indicates a negative percentage change in private employment. However, the spillover effect is marginally statistically significant only in the second year after the focal's FPM's shock. The spillover effects estimates have large confidence intervals in side the window $[-5\%, 2\%]$ for the whole event time span. Due to the imprecision of the estimates, making inference is very difficult. Even though, it is not possible to rule out the null spillover effects hypothesis. Figure 11b also presents imprecise estimates. The spillover effects point estimates are closer to zero spillover though. For almost all years after the focal crosses the threshold, the percentage change in private employment is very close to zero. However, they have large confidence intervals inside the window $\{-15\%, 10\%\}$ for the whole event time span.

Figure 12 shows estimates of spillover effects on number of firms. In other words, it estimate, in a reduced form way, the focal's FPM's revenue shock effects on the neighbor's number of firms. Similarly to previously analysis, Figure 12a uses the positive event sample and Figure 12b uses the negative event sample.

Figure 12a presents precise estimates of spillover effect on number of firms. For all years after the crossing threshold date, the point estimates indicate null spillover effects within a very small confidence interval. However, Figure 12b presents imprecise estimates. For all years after the crossing threshold date, the spillover effect on the number of firms is negative. However, null spillover effects cannot be ruled out within a 95% confidence interval. Note, however, that estimates have large confidence intervals inside the window $[-8\%, 2\%]$.

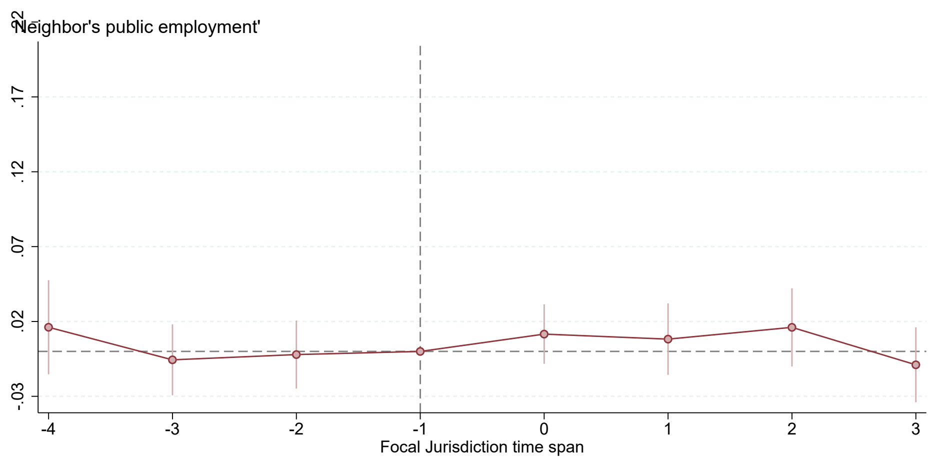
6.2 Heterogeneity

In this subsection, the main results of spillover effects on neighbor's total expenditures, total public employment and number of firms are estimated again, but only for neighbors that have a smaller population size than its respectively focal jurisdiction.

The rationale for checking this heterogeneity is that spillover effects are likely to be stronger for neighbor jurisdictions that are smaller than the focal jurisdictions. This would be the case if smaller nearby ju-

Figure 10: Second stage: Spillovers Effects on Neighbors' Total Public Employment

(a) Positive event sample



(b) Negative event sample

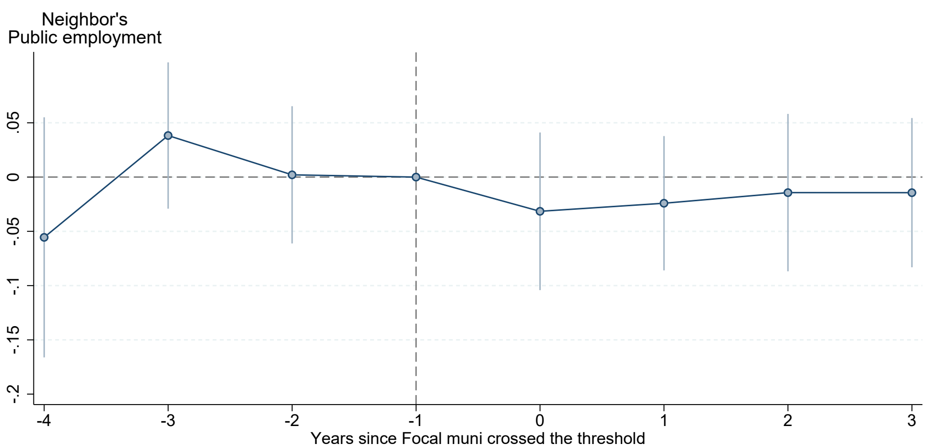
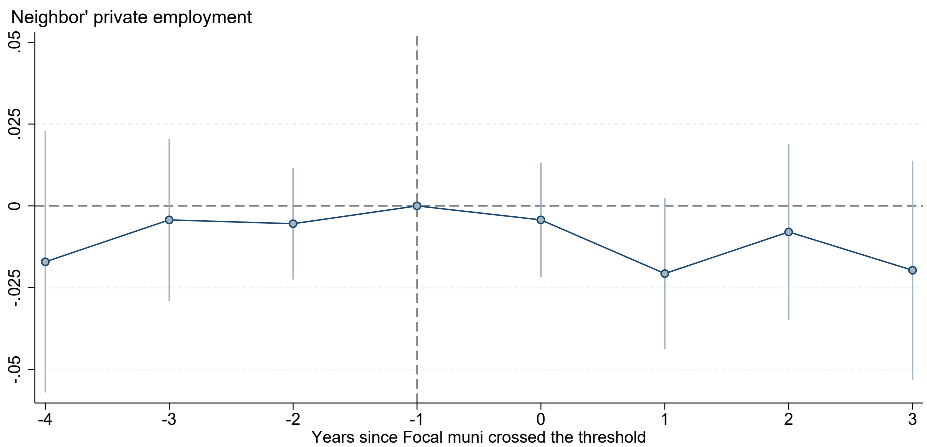


Figure 10 show estimates of spillover effects on neighbor's total public employment. Figure 10a use positive events sample, for all years, before and after the event, estimates indicate null spillover effects. Figure 10b uses the negative event sample and estimates are imprecise, although null direct effect cannot be ruled out. Dots with lines are point estimations within confidence interval of 95% estimations. The estimates are relative to the previous year of the event and comparison group variations.

Figure 11: Second stage: Spillovers Effects on Neighbors' Total Private Employment

(a) Positive event sample



(b) Negative event sample

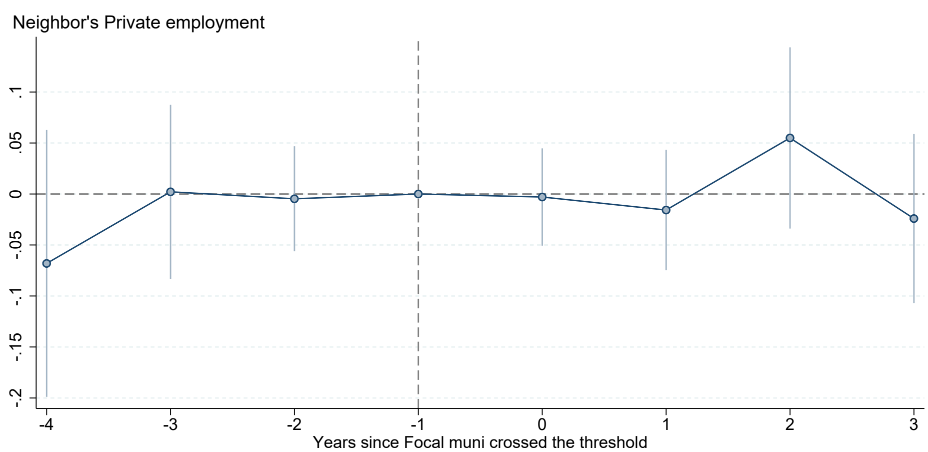
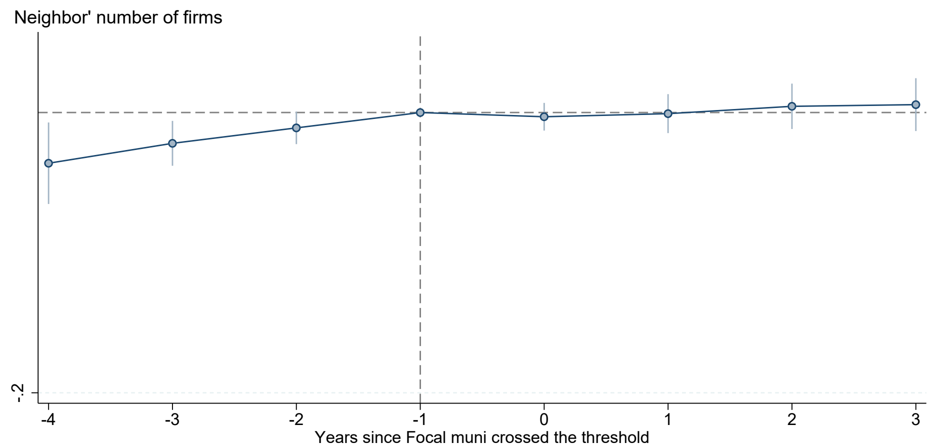


Figure 11 show estimates of spillover effects on neighbor's formal private employment. Figure 11a use positive events sample, for all years, before and after the event, it is very difficult to make inference due to the high estimates imprecision. Figure 11b uses the negative event sample and estimates are still very imprecise. Dots with lines are point estimations within confidence interval of 95% estimations. The estimates are relative to the previous year of the event and comparison group variations.

Figure 12: Second stage: Spillovers Effects on Neighbors' Number of firms

(a) Positive event sample



(b) Negative event sample

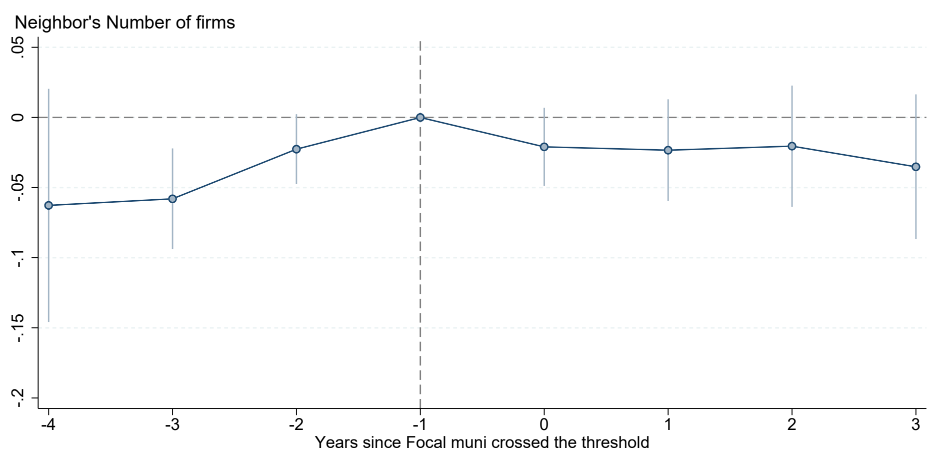


Figure 12 show estimates of spillover effects on neighbor's total number of firms. Figure 12a use positive events sample, for all years after the event, it presents highly precise null spillover effects estimates. Figure 12b uses the negative event sample and estimates are very imprecise, which makes difficult to do inferences. Dots with lines are point estimations within confidence interval of 95% estimations. The estimates are relative to the previous year of the event and comparison group variations.

risdiction are more susceptible to the decision making process of central larger focal jurisdictions. In other words, due to economies of scale of big cities. These outcomes are specifically selected because Section 5.2 has shown clean evidences of null spillover effects on them. Since the main results are consistent with a null spillover effects, this exercise can also be seen as a robustness check.

Figure 13 is similar to Figure 9, the only difference is that the sample is restricted for neighbors with relative smaller population size in terms to the focal jurisdiction in the former Figure. Figure 13a reports estimated spillover effects on neighbor's total expenditure for the restricted positive event sample. They corroborate the previous evidence on null spillover effects with even more precisely estimates than when using the whole sample.

An interesting result is reported in Figure 13b. When using the whole negative event sample, results consists of imprecisely estimates of null spillover effects. However, using the restricted negative event sample, estimates off null spillover effects are very precise.

Figure 14 also restricts the sample to relative smaller neighbors. Figure 10 refers to the positive event sample. Figure 14a uses the negative event neighbor. They also are consistent with the null spillover effect hypothesis. However, both Figures have more precisely estimated spillover effects when compared to their respectively unrestricted samples. Note that Figure 14b shows results for the negative event sample, they are similar to the results reported by Figure 10b, very imprecise null spillover effects estimates. All in all, the focal's positive FPM's revenues shock seems to have null spillover effects on the neighbor's public employment, but the the evidence for the negative event is not so strong.

Figure 15 reports spillover effects estimates on number of firms. Figure 15a uses the positive event sample restricted to neighbors with a greater population size than the focal jurisdiction.

Figure 15a reports null spillover effects estimates, just as the ones shown in the Figure 12a, but with higher precision. Meanwhile, Figure 15b shows that restricting the negative event sample doesn't alter the inference. There are still high imprecise negative spill over effects. Although, null spillover effects hypothesis cannot be rejected.

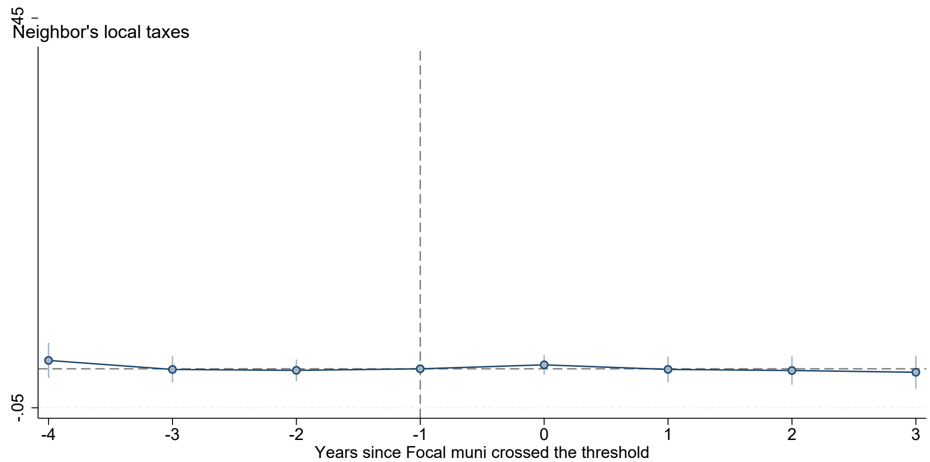
7 Conclusion

The current empirical literature has been finding positive expenditure fiscal spillover, as shown in the Literature Review section.This paper contributes with a novel result, it estimates null fiscal spillover between local jurisdictions. This is important because it casts doubts on a large theoretical literature of fiscal spillover between local jurisdiction, as there are spillover existence predictions, see Tait (1981),Oates and Schwab (1988),Wilson (1986).

Isen (2014) and this paper are two exceptions in what refers to finding null expenditures spillovers between local jurisdictions. However, in Isen (2014) context, the focal jurisdiction has its revenue and taxation source of variation simultaneously taxed and spend in the same focal jurisdiction, instead of just

Figure 13: Spillovers Effects on Expenditure of relative smaller Neighbor

(a) Positive event sample



(b) Negative event sample

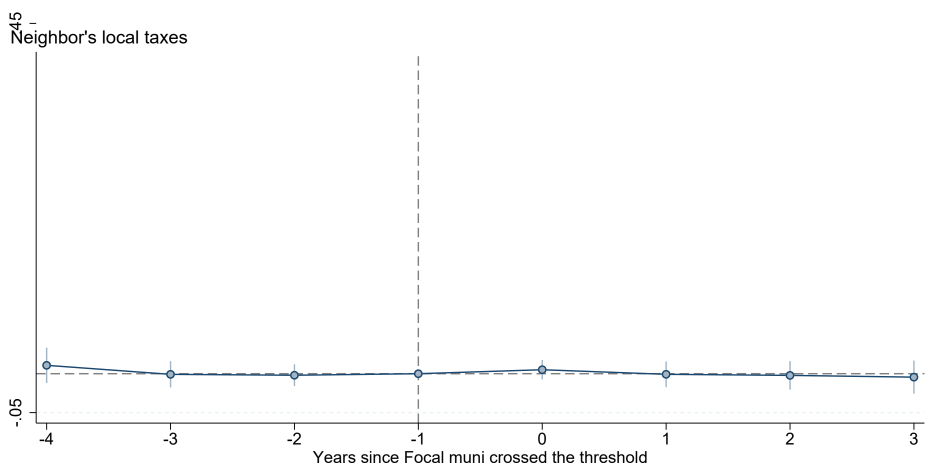
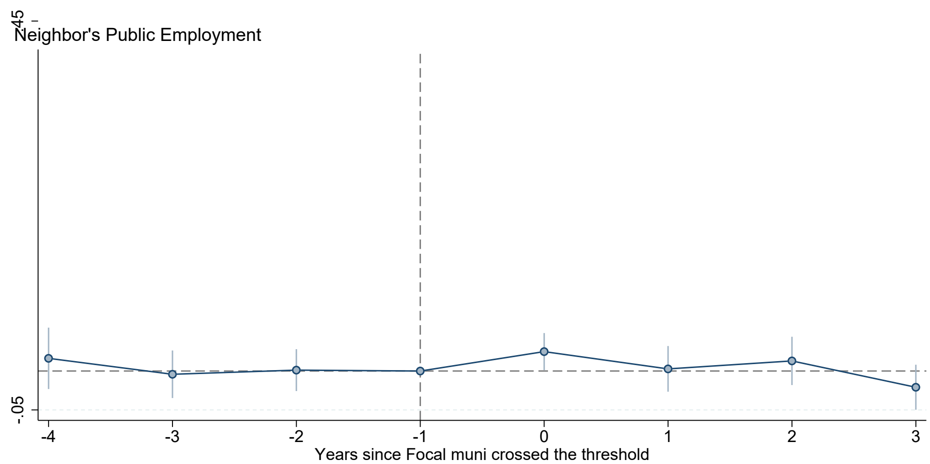


Figure 13 show estimates of spillover effects on neighbor's public total expenditure for samples restricted to neighbors that have smaller population size than the focal jurisdiction. Figure 13a use restricted positive events sample, for all years, before and after the event, it presents highly precise null spillover effects estimates. Figure 13b uses the restricted negative event sample and estimates are very precise as well, corroborating the result of null spillover effects. Dots with lines are point estimations within confidence interval of 95% estimations. The estimates are relative to the previous year of the event and comparison group variations.

Figure 14: Spillovers Effects on total public employment of relative smaller Neighbor

(a) Positive event sample



(b) Negative event sample

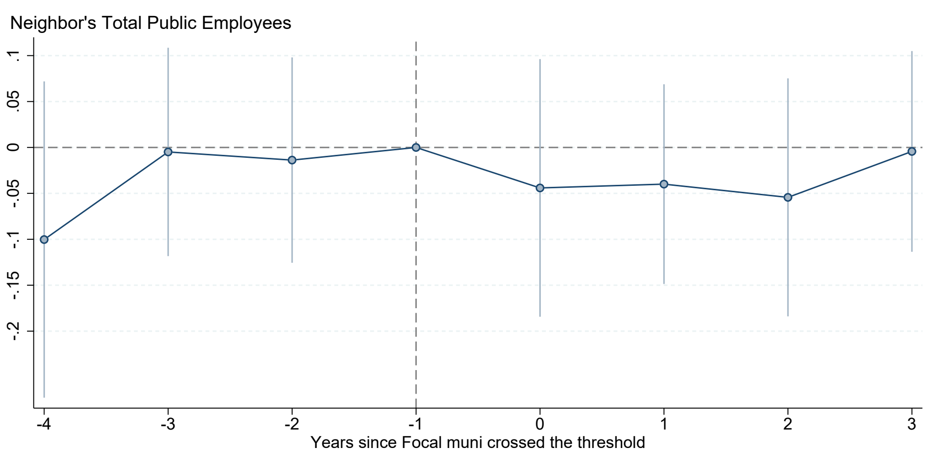
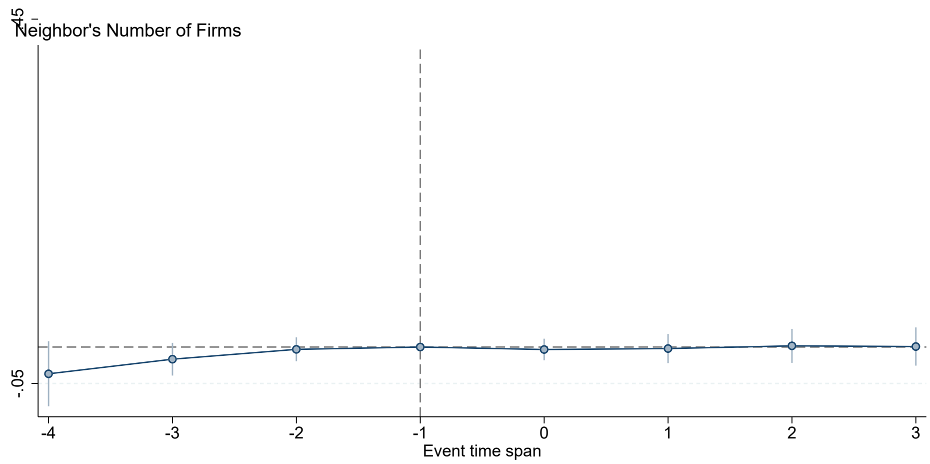


Figure 14 show estimates of spillover effects on neighbor's public total employment for samples restricted to neighbors that have smaller population size than the focal jurisdiction. Figure 14a use restricted positive events sample, for all years, before and after the event, it presents highly precise null spillover effects estimates. Figure 14b uses the restricted negative event sample, estimates are very imprecise, which makes any type of inference very difficult. Dots with lines are point estimations within confidence interval of 95% estimations. The estimates are relative to the previous year of the event and comparison group variations.

Figure 15: Spillovers Effects on number of firms of relative smaller Neighbor

(a) Positive event sample



(b) Negative event sample

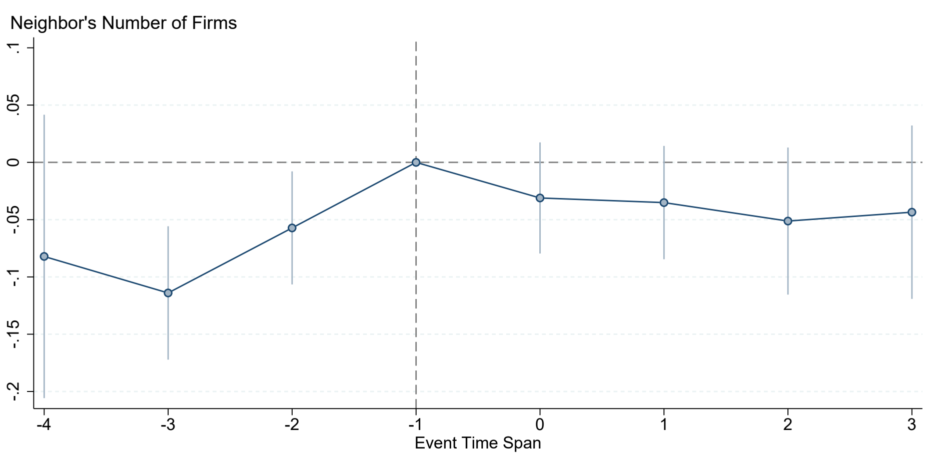


Figure 15 show estimates of spillover effects on neighbor's total number of firms for samples restricted to neighbors that have smaller population size than the focal jurisdiction. Figure 15a use restricted positive events sample, for all years, before and after the event, it presents highly precise null spillover effects estimates. Figure 15b uses the restricted negative event sample and estimates, but they are very imprecise, which makes any type of inference very difficult. Dots with lines are point estimations within confidence interval of 95% estimations. The estimates are relative to the previous year of the event and comparison group variations.

having FPM revenues transfers as in this paper context. In this paper, the taxation that is source to the revenues' variation are external to the local jurisdiction, it comes from central government to the local jurisdiction. In this sense, this paper not only corroborates Isen (2014) results, but it also expands the null expenditure spillover effects hypothesis to a setting in which revenues and taxation are not issued simultaneously in the same local jurisdiction.

This paper estimate that the studied event has a direct effect of 20% on FPM's revenues, which in turn, has a direct effect of 10% in total expenditures expenditures. By a back-of-the-envelope calculation, 1% variation in FPM' revenue is associated with a 0.5% variation in total public expenditures in the same local jurisdiction. On the other hand, this paper estimate that these variations in focal jurisdiction's FPM's revenues and expenditures causes very precise null spillover effects on neighbor total expenditures and other labor market outcome as well.

This paper has estimated that the event has a direct effect of 6% on public employment. On the other hand, it also estimates null spillover effects on neighbor's on public employment and number of firms outcomes. It means that this work presented evidences that an unconditional transfer shock in a focal jurisdiction has null effects on some of the neighbor jurisdiction's labor market outcomes, at least in the public sector employment and number of firms. Note that null spillover effects couldn't be ruled out on private employment as well.

When exploring the heterogeneity of spillover effect. The samples were restricted to focal jurisdiction with greater population size than the neighbor jurisdiction. In this way, it is reasonable to think that the spillover effect would be stronger. The results indicated that the null spillover effects were even more precisely estimated.

The results found in this paper contributes to the current Brazilian debate on fiscal decentralization. It also contributes to the fiscal federalism literature, see Oates (1999). As future research, this paper left open questions through what mechanisms null spillovers can operate. Maybe answers might come back to the old Tiebout (1956)'s model.

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