Local Socioeconomic Impacts of Brazilian Hydroelectric Power Plants

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Abstract

This paper estimates the short- and medium-run effects of the construction of large hydroelectric power plants (HPPs) on the economic development of Brazilian municipalities. We use the synthetic control method to perform one case study for each of the 82 municipalities affected by a HPP between 2002 and 2011. Two main findings emerge. First, the median impact of the construction of HPPs on the local economy is modest and follows an inverted U-shape over a five-year horizon. Second, the estimated effects display a lot of dispersion. These results do not provide support for the view that large construction works can unequivocally spur local development.

Keywords: Hydro-power, Brazil

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1 Introduction

Since year 2000, more than one thousand hydroelectric power plants (HPPs) were built around the world (ICOLD, 2016), expanding electricity generation capacity by 427 GW, the equivalent to France and India's energy production capacity combined (EIA, 2012). While investments in renewable energy are positive in several ways, the construction process of new large HPPs often causes intense debates. On the one hand, these large infrastructure projects are often believed to promote local development, leading to an increase in income, jobs, and tax revenues for local governments (Bourguignon et al., 2008). Indeed, Severnini (2014) suggests the hydroelectric dams built in the US in the middle of the 20th had long-run agglomeration impacts. The dams by themselves might have impacts on productivity as shown by Duflo and Pande (2007) and Strobl and Strobl (2011). But, on the other hand, critics point to big social and environmental costs caused by these projects, including displacement of local population, and large in-migration influxes followed by poorly planned urban expansion (**?**; Fearnside and Pueyo (2012); Benchimol and Peres (2015)).

This paper evaluates the local socioeconomic effects of the construction of new HPPs built in 82 municipalities in Brazil between 2002 and 2011. We allow those effects to vary, treating each HPP construction as a specific event – each one of these cases has its own characteristics in terms of location, size and project design. In Brazil, where the system is centralized and energy is dispatched through a large transmission network, electricity access by itself might have limited impact in host municipalities, while the construction process and increased local public revenues can have larger consequences, creating heterogeneity on the impact. Our analysis can be viewed as a comprehensive collection of case studies.

We use the synthetic control method (Abadie et al., 2010; Abadie and Gardeazabal, 2003) to estimate the local effects of the construction of each HPP over time. This method allows us to estimate the dynamic effects from the beginning of the HPP construction for each one of the 82 municipalities that received new HPPs, effectively constructing 82 comparable case studies. By doing this, we can estimate the average (or, median) effect of HPP construction by year, as well as the distribution of these effects, even in the medium-term. This approach has been employed to analyze the consequences of a variety of events and policies, such as forest conservation policies (Sills et al., 2015), crime reduction policies (Saunders et al., 2014), natural disasters (Cavallo et al., 2013), and even the German reunification (Abadie et al., 2015).d

Our results reveal two key patterns. First, we find that the median impact of the construction of HPPs on the local economy follows an inverted U-shape over time. These findings are aligned with those obtained by **?**, on a recently published and independent paper that studied similar issues in Brazil, using an event-study empirical strategy (CONFIRMAR SE É UMA DESCRIÇÃO

PRECISA). The economy of the median municipality is stimulated only in the short-term: one year after the beginning of the construction, the GDP per capita is 7% higher than it would be in the absence of the construction and formal employment is 10% higher than its counterfactual, for example. However, five years after the construction (the average construction time is four years), we find that the median effect on a series of outcome measures is close to zero, such as in the GDP growth rate, GDP per capita, number of formal enterprises, and in the population size. The only medium-term economic effect we observe is an 8% increase in formal employment in the median municipality five years after the start of construction. We find the same pattern when we look at the average effects and perform statistical inference as in Cavallo et al. (2013).

Second, we find that the impacts of the construction of HPPs on the local economies are very heterogeneous. For all indicators studied, despite the median impacts being typically small and following similar patterns, the effects on some municipalities are much more severe than in others – both positively and negatively. For example, while the median impact of the HPP construction on GDP per capita is close to zero five years after its construction, the same estimated effect is actually a 12% increase for 20 of those municipalities; while for another 20, the HPP causes a 13% decrease.

The main contribution of this paper is to estimate the distribution of the short- and medium-run economic impacts from new hidroeletric powerplants constructed using current technology and under present day regulatory standards in a middle-income country context. In various ways, our findings are in line with most of the literature (e.g., Cavallo et al. (2013), Miguel and Roland (2011), Allcott and Keniston (2014)). More broadly, this paper relates to the literature on socioeconomic impact of large infrastructure (e.g., Feyrer et al. (2015), Davis (2011), Aragón and Rud (2015)), and the geography of economic activity literature (e.g., Kline and Moretti (2013), Moretti (2010), Davis and Weinstein (2002)).

This paper proceeds as follows: Section **??** provides background information and presents our data. Section 4 details our empirical strategy. We discuss the results in Section 5 and briefly conclude in Section 6.

2 Background

Brazil heavily relies in hydroelectric power. Since the 1960s, the electrification of the country was largely based on the construction of large HPPs projects such as the Itaipu Dam, which is the HPP that produce the most energy in the world after the Three Georges Dam in China. Brazil largely electrified the country in the following four decades, which contributed to the de-

velopment of the interior of the country Lipscomb et al. (2013). By 2003, Brazil had 139 medium and large HPPs, which represented about 70 GW of installed capability to generate hydroelectric power. According to the Demographic Census, in 2000, 93.5 percent of the households in the country were connected to electricity.

Following the energy crisis in 2001 (Costa and Gerard, 2015) and to unleash the economic development, the government invested in expanding its generation capacity and transmissions line. In 2003, the Workers' Party with the presidential election with an agenda of reducing inequality and promoting economic growth. The new government fomented large infrastructure projects and, since then, 67 new medium and large HPPs started being constructed, what could add 21 GW of capacity to the national electric system – a 30 percent increase (ANEEL, 2016). Despite potential environmental concerns surrounding these projects[1] the development and industrialization agenda of the government pushed this projects forward.It is important to highlight the the main objective of these new HPPs was to increase the generation capacity of the national grid, not to electrify new regions.[2]

Even though the main objective was not to develop the regions around the HPPs, the construction process on itself could have large socio economic impacts locally. HPPs are typically built in rural areas, sparsely populated, which suitable hydrological conditions. The construction process attracts investments and people to the surrounding municipalities. In 2000 values, the average construction costs $650 million dollars and creates 3,700 direct jobs in a 4 year window (average construction time).[3] To give the scale of these enterprises, in 2000, the average GDP of the municipalities that received a new HPP in the following decade was %56 million dollars, and the number of workers in formal jobs was 3,368. This may have both a direct effect on the economy and may have an indirect effect by increasing the tax collection of the local governments.[4]

In sum, this influx of people and resources may create a boom in the local economy during the construction period. Depending on the magnitude of these short-run effects, the construction may lead to urbanization and structural transformation in the local economy after the HPP starts operating. Our goal in this project is to provide some estimates of the socio economic effects of the construction of new HPPs in the short- and medium-run.

[1]See, e.g., Fearnside and Pueyo (2012); Benchimol and Peres (2015); Assunção et al. (2017).

[2]Since most of the households were already connected to grid, the unelectrified households were usually in remote and poor areas where the electricity distribution network would not arrive. The government created a program to make this "last mile" connections called Light For All (*Luz Para Todos*) focused on the poorest families.

[3]Values deflated using Consumer Price Index (IPCA).

[4]For more detail about the construction and licensing processes see Assunção et al. (2017).

3 Data

3.0.1 Universe of Analysis

Our unit of analysis is a Brazilian municipality. We restrict attention to municipalities that (i) have an area flooded by a HPP whose construction initiated between 2002 and 2011, and (ii) are not subject to floods by more than one dam. As of 2013, there were 718 municipalities directly affected by HPPs; among them, 599 municipalities were impacted by dams constructed before 2002 or after 2011 and are excluded from our analysis. Out of the 119 municipalities left, 37 have more than one HPP flooding their areas, violating condition (ii). Hence, we end up analyzing the impacts of HPPs on 82 municipalities, distributed in 13 states all throughout Brazil, and add up to 4 thousand km^2 of flooded area. Figures A1, A3, A4 and A5 show these municipalities, as well as the municipalities we exclude. The municipalities that we study contain areas that are flooded by 29 different HPPs, adding up to four thousand km^2 of flooded area.

3.0.2 Data sources and variable definition

Hydroelectric Power Plants (HPPs) We use administrative data regarding all HPPs from the Brazilian Electricity Regulatory Agency (ANEEL – *Agência Nacional de Energia Elétrica*). For each HPP, ANEEL provides the list of municipalities that were directly affected, i.e that had an area flooded by the hydroelectric power plant's reservoir.

Municipal GDP Our first measure of economic activity at the local level is the Gross Internal Product of Brazilian municipalities, from IBGE (*Instituto Brasileiro de Geografia e Estatística*), which is available for the period 1999–2012. The series also includes data on the value added by the agriculture, manufacturing and services sectors, as well as total taxes collected.

Population We use annual population data at the municipality level for the years 1999–2013 from IBGE. These are projections made by IBGE considering not only the Demographic Census, but also data on civil and obituary registration at the local level. Gender classification was performed by the Ministry of Health, and is available for the period 1999-2012.

Formal employment and Firm Creation We use aggregated data from RAIS (*Relação Anual de Informações Sociais*) provided by the Ministério do Trabalho. RAIS can be seen as a census tracking information on all Brazilian firms. For each year, all formal companies that employed at least one person are obliged to provide data concerning their workforce. We exclude legal

entities classified as NGOs and public administration institutions from the analysis due to the poor quality of data for these fields, thus focusing exclusively on for-profit juridical entities. With this scope, we construct the series of (i) formal employment (measured at the end of each year) and (ii) the number of registered firms that provide information to RAIS, annually, for each municipality.

Land Prices We use annual land value data from Informa Economics FNP in their Agri-Annual series. This database provide prices for multiple land types in such a way that, in many instances, the same municipality has more then one land price for each year. Because of this, or interest variable is the annual variation of a price index[5], $\frac{I_t - I_{t-1}}{I_{t-1}}$. However, as there is no data for 90% of Brazilian municipalities and the number of treated units in this dataset is just five(in contrast to 82 in the other databases) we assign a lower confidence to these results and present them in the appendix.

4 Empirical Strategy

Assessing the impact of the construction of hydroelectric powerplants on local economies poses empirical challenges. The main challenge is to build a valid counterfactual for what would have happened to the local economy had the powerplant not been constructed. The decision to build a HPP in a specific place is based on economic, environmental and political cost-benefit calculations undertaken by the regulator and private parties. Therefore, municipalities that are affected by an HPP are likely to be different from non-affected municipalities in various dimensions, and empirical strategies that rely in simple comparisons of outcomes between these two groups to identify HPP' causal effects may lead to biased results. Furthermore, the number of affected municipalities we study is relatively small, 82, due to data and events limitations.

We employ the synthetic control method (Abadie et al., 2010; Abadie and Gardeazabal, 2003) to overcome this challenge. The main idea is to use a weighted average of municipalities without a HPP to construct a virtual, or *synthetic*, municipality that serves as the best counterfactual for the municipality that received a new HPP. Within this method, the effect of the HPP construction will be simple difference between the realized output of the studied municipality's and the realised output of its counterfactual.

In this work, we investigate not only one case of HPP construction, but 82 of them. In this section, we first explain how we estimate each individual effect using the synthetic control

[5] For each observation i, we created a price index $P_i = price_{it} / price_{it_0}$, where t_0 is the first year of the dataset: 2002. The price index for a given municipality is given by $\prod_{i=1}^{n} P_{it}^{1/n}$

method and, then, we discuss how we use their results to estimate the average treatment effect. After this, the inference methods are presented. Again, the case with only one treated unit is presented first, and the more general case for multiple units is presented later.

4.1 The synthetic control method for case studies

Suppose that, we observe one treated unit together with other J municipalities that have no HPP constructed and could serve as potential controls – the donor pool. Without loss of generality, let $j = 1$ be the municipality that was affected by the HPP, henceforth the treated unit. Suppose also that we observe these units for T periods starting in t=1 and let $T_0 < T$ be the period of HPP's construction beginning in municipality 1, henceforth the treatment period.

Let y_{jt} be the realized outcome of interest at municipality j and time t. Say that in the absence of the treatment – i.e. if the HPP had not been constructed –, the counterfactual outcome of j in periods $t \leq T_0$ would be y^C_{jt}. We define the difference between the realized outcome and the potential outcome in the absence of the treatment as the causal effect of the HPP construction in municipality j in period $t \leq T_0$:

$$\alpha_{jt} = y_{jt} - y^C_{jt} \ , \ \forall t \leq T_0. \tag{1}$$

Since we do not observe y^C_{1t}, we estimate it using synthetic control. Intuitively, our empirical strategy consists in using the observations before the treatment, i.e $t < T_0$, to construct a synthetic municipality that will provide us a counterfactual for $j = 1$. This allows us to calculate how the treated municipality's output of interest would have evolved in the absence of the HPP construction. We estimate $\hat{y}^C_{1t}$ using a weighed average of the J non-affected municipalities. Mathematically, a potential synthetic control is defined by a weight vector $W = (w_1, w_2, ..., w_{J_i})'$, such that

$$\begin{gathered}\sum_{j=1}^{J} w_j = 1 \\ w_j \geq 0 \ , \ \forall j \in J.\end{gathered} \tag{2}$$

As such, for a given weight vector W, the outcome of the synthetic municipality is:

$$\hat{y}^C_{1t} = \sum_{j=2}^{J+1} y_{jt} w_j \tag{3}$$

where y_{jt} is the outcome of interest at control municipality $j \in J$ and period t.

A given synthetic municipality provides a credible counterfactual for the HPP construction in j if, before the construction start in T, the treated and the synthetic municipalities follow similar dynamics in the outcome of interest:

$$\hat{y_{jt}}^C = y_{jt} \ , \ \forall t < T_0. \tag{4}$$

We calculate the optimal weights such that the weight vector W^* satisfies equations (2) and (4) approximately. Specifically, we choose W^* that minimizes

$$||y_i - \hat{y}_i^C|| = \sum_{t=1}^{T_0} (y_{it} - \hat{y}_{it}^C)^2 \tag{5}$$

subject to (2).

The key identification assumption is that, in the absence of the HPP construction, the synthetic municipality would continue to reproduce the unobserved trends of the treated one. Under this assumption, any divergence posterior to the construction can be attributed to the HPP construction, and the estimated effect of the HPP construction is $\hat{\alpha}_{jt} = y_{1t} - \hat{y}_{1t}^C$.

A possible source of errors with this approach concern interpolation biases. Suppose, for example, that we study a treated locality with GDP per capita of R$15000. The simple average of two localities, one with R$5000 and the other with R$25000, will apparently be a good match to the treated locality. However, it is possible that municipalities with such different GDPs per capita may face different shocks after the intervention. Therefore, in order to make our approach more reliable, we limit our donor pool to units similar to the treated one.

More specifically, for estimating treatment's effect for each treated municipality, its donor pool J consists of all other municipalities that comply with the following criteria: (i) not having area flooded by HPP in any point of the period we study; (ii) is located in the same state, or if the treated unit is located in Brazil's North region, is in the same region ; (iii) is, at most, one quintile away of the treated unit in respect to the variable of interest in the treated municipality's state[6]

The interpretation that the HPP causes all the difference between each treated unit and its synthetic counterpart is not free from criticism. In particular, one can argue that other factors unrelated to the plant caused the divergence between treated and control units. This is justifiable only if there is an idiosyncratic factor affecting only the treated municipality and not its synthetic counterparts at the specific time the HPP was constructed. Otherwise, the synthetic control would continue to follow the treat unit and would not show any divergence with it. In

[6] Or region, if the municipalities are in Brazil's North region. For some treated municipalities, this rule resulted in no convergence of the optimization algorithm. We opted by not including these treated units in our main results. However, by not adopting the exclusion rule we can include these municipalities without relevant changes in the main results. This third rule was not applied in the study of land prices in the appendix.

our case, the concern with idiosyncratic shocks is diminished because we repeat this exercise for each of the 82 affected municipalities on separate.

Figure 1 illustrates our use of the synthetic control method for the municipality of Filadélfia (in the state of Tocantins) and its (log) GDP per capita. In 2007, the construction of the HPP of Estreito was initiated and flooded $114km^2$, approximately 6% of Filadélfia's area. In the figure, it is possible to note that synthetic Filadélfia matches well the trends of Filadélfia before intervention – with t normalized to $T_0 = 0$. We find that the construction of HPP in Filadélfia caused a brief cycle of growth in the first years, where Filadélfia had a GDP per capita 13% higher then its synthetic counterpart. However, five years after the construction, the GDP per capita was 3% smaller then what it would be if the HPP had not been built.

4.2 Compiling the individual estimates

In this paper we study 82 municipalities affected by the construction of an HPP between 2002 and 2011, as explained in the previous section. As we estimate the treatments' effect separately, we index each separate event by $g = 1, 2, ...G$, with $G = 82$. In this subsection, we explain how all estimated results are compiled.

First, let T_{0g} be the year in which the treatment was enacted in event g. In order to better compare treatment occurred in different years, we normalize years by $\tau = t - T_{0g}$ such that $\tau = 0$ is the year of HPP construction.

Second, let $\hat{\alpha}_{j\tau g} = y_{j\tau g} - \hat{y}^C_{j\tau g}$ be the estimated effect of the HPP construction for municipality j, at period τ in event $g \in G$. We merge the results of the different treated (i.e with $j = 1$) municipalities by year τ to obtain an empirical distribution of effects.

We estimate the average effect of G treatments as:

$$\overline{\alpha}_\tau = \frac{\sum_{g=1}^{G} \hat{\alpha}_{g1\tau}}{G} = \frac{\sum_{g=1}^{G} (y_{g1\tau} - \hat{y}_{g1\tau})}{G} \tag{6}$$

Analogously, we estimate the 25^{th}, the 50^{th} and the 75^{th} percentile of the effects as

$$P_x(\alpha_\tau) = P_x(\hat{\alpha}_{g1\tau}) \tag{7}$$

, where P_x is the x^{th} percentile of the effect.

4.3 Inference and synthetic control, general remarks and the case of one treated unit

The particular nature of synthetic control estimators require the use of particular inference techniques. In this subsection, we explain the main problems posed for inference and how the literature has dealt with them. As the synthetic control was initially created for studies with only one treated unit, we start by discussing this particular case as a way to better introduce the subject. In the next subsection, we use the ideas developed here to explain how the inference for average effects is made.

In the context of synthetic control, the uncertainty about the value of the estimated effect come primarily from the uncertainty about how well the control group can reproduce the counterfactual. Idiosyncratic shocks in the treated unit, for example, cannot be reproduced by any weighted average of donor units. More formally, we can make:

$$y_{1t}^{C} = \sum_{2}^{J+1} (w_j y_{jt}^{C}) + \epsilon_{jt} \tag{8}$$

where ϵ_{jt} is a random shock that only occurs at unit j. As such, we can express the error in treatment effect estimation as:

$$\hat{\alpha}_{1t} - \alpha_{1t} = (y_{1t} - \hat{y}_{1t}^{C}) - (y_{1t} - y_{1t}^{C}) \tag{9}$$

thus,

$$\alpha_{1t} - \hat{\alpha_{1t}} = \epsilon_{jt} \tag{10}$$

As the distribution of these errors is unknown there is no way to estimate the distribution of measured treatment effects under the null hypothesis of no effect or to know if this distribution converge to a normal. As a way to overcome this problem, we rely in permutation tests to conduct inference. The use of permutation tests for synthetic control inference was first proposed by Abadie et al. (2010). In their paper, the objective was to estimate and do inference about the effect of a intervention in a single unit and permutation tests were considered attractive because they do not rely in large sample properties.

The main idea behind permutation tests is to measure the outcomes of units with and without treatment and compare them. If the outcomes of treated units are extreme, then we have evidence that the treatment has an effect.[7]

[7] For a good review of permutation tests, see Ernst et al. (2004)

For example, in order to conduct inference for just one treatment, we could use the synthetic control method for both treated and non treated units. Then, compute $\hat{\alpha}_{jt}$ for each one of them as our test statistic. For a bilateral test,[8] our p-value for the effect in period t would thus be calculated as:

$$\frac{\sum_2^J I(|\alpha_{jt}| \geq |\alpha_{1t}|)}{J} \tag{11}$$

This formula's value is the probability of estimating a result as extreme as the treated unit's result if we would randomly reassign the "treatment label" between the donor units and is useful as evidence about the null hypothesis of no treatment effect. If the treatment was randomly assigned, this probability has the interpretation of traditional bilateral p-value, i.e the probability of the measured result be obtained if the treatment has no effect.

When the treatment is not randomly assigned, two main problems threat the validity of the inference method previously proposed. The first is the existence of bias. Suppose, for example, that some unit's outcome of interest is persistently above (or bellow) its synthetic counterpart even before the intervention. If this bias continue after the intervention the probability of the unit present a "extreme" result will be inflated, becoming higher then its p-value. The second problem is heteroscedasticity of idiosyncratic shocks. For example, if the unit's expected value of the outcome of interest is equal to its synthetic counterpart, there is still a possibility that they will often diverge. This will happen if this unit is subject to constant shocks. As we take the divergence between $y_{jt} - \hat{y}_{jt}^C$ as our measure of how extreme a result is, if the treated unit has shocks with higher variance then the not treated units, we will underestimate the p-value. If its shocks have lower variance, we will overestimate the p-value.

Although the two problems are distinct in nature, both can be diagnosed by a poor pre-intervention fit. Because of this two types of corrections have been proposed (see Abadie et al. (2010) and Ferman and Pinto (2015)). The first is simply to exclude from the suggested comparisons units that have poor pre-intervention fit.[9] The second is to use as a test statistic an adjusted effect that accounts for different predictive precisions.The goodness of fit can be measured by the following variable:

$$RMSPE_j = \sqrt{\frac{\sum_{t=0}^{T_0-1}(y_{jt} - \hat{y}_{jt}^C)^2}{T_0 - 1}} \tag{12}$$

[8] Naturally, one can make unilateral tests by simple ranking the actual value of the tets statistics instead of its absolute value.

[9] In this case, we would have $\frac{\sum_2^J I(|\alpha_{jt}| \geq |\alpha_{1t}|) I(j\_not\_excluded)}{\sum_2^J I(j\_not\_excluded)}$

where $RMSPE_j$ is unit j's square root of the mean square prediction error, our measure of fit. Having $RMSPE_j$, we can implement the first suggestion by excluding units whose $RMSPE_j$ is a number of times bigger then the treated unit's RMSPE, $RMSPE_1$. Alternatively, we can obviates the need to exclude some units by applying the second suggestion. We can follow it by substituting α_{jt} as a test statistic by the adjusted effect:

$$t_{jt} = \frac{\alpha_{jt}}{RMSPE_j} \tag{13}$$

If we follow this approach, the p-value will be:

$$p = \frac{\sum_2^J I(|t_{jt}| \geq |t_{1t}|)}{J} \tag{14}$$

4.4 Inference for average treatment effect on multiple treated units

Cavallo et al. (2013) extends the methods outlined in the previous section, allowing inference for synthetic control to be used when there are more than one treated unit. In their framework, the method estimates the treatment's average impact and its p-value. In order to compute a p-value, Cavallo et al. (2013) propose the creation of placebo averages and permutation tests analogous to the used in the case of one treated unit. We follow their procedure, that consists in four steps:

1. Compute the average of the chosen test statistic of all treated units. Following Cavallo et al. (2013) we use $\hat{\alpha}_{jt}$. Thus, we calculate:

$$\overline{\alpha}_\tau = \frac{\sum_{g=1}^G t_{1\tau g}}{G} \tag{15}$$

2. Apply the synthetic control to no-treated units of all events g as if they were treated and compute the value of their test statistic. For the next item, we use only placebos that have RMSPE less then ten times higher then the treated unit's RMSPE.

3. Compute possible placebo averages of the chosen test statistic by picking a single placebo estimate corresponding to each disaster g; and then taking the average across the G placebos. Thus, each combination will have the following test statistic:

$$\overline{\alpha}_\tau^{PL(i)} = \frac{\sum_{g=1}^G \alpha_{j\tau g}}{G} \tag{16}$$

in which α_{jtg} is the value of the test statistic α, at event g, period τ and unit j. PL(i) is

simply a index of the placebos. Cavallo et al. (2013) suggest the computation of all possible averages. However, as the number of possible combinations in our study is much large (at the order of 10^{179}), we instead choose a random sample of them with size N=1000.

4. Rank the chosen test statistic and compute the lead τ specific p-value as:

$$p - value_\tau = \frac{\sum_{i=1}^{N} I(|\overline{\alpha}_\tau^{PL(i)}| \geq |\overline{\alpha}_\tau|)}{N} \tag{17}$$

5 Results

In this section, we present the results obtained for an array of studied variables aimed at measureing economic activity in each municipality potentially affected by the hydroelectric power plants (HPPs). We present our findings in two distinct sets of results. Building a HPP costs on average R$3.5 billion, the equivalent of 162.4% percent of the gross domestic product of the average municipality. Therefore, we first investigate whether the HPP has direct effects on local economic activity – subsection 4.1. Furthermore, the inflow of workers and capital to these remote areas, as well as the introduction of a new economic activity, may change the composition of the local economy with sustained impacts (Severnini, 2014; Bustos et al., 2016). Subsection 4.2 analyzes HPP impacts on short- and medium-run local structural transformation.

As explained in the previous section, we obtain one estimate for each of the 82 studied municipalities and present the results graphically by plotting the 25th, 50th, and 75th percentiles, as well as the average of the distribution of effects for each year τ. We normalize $\tau = 0$ to equal the year that the HPP construction is initiated. These effects are presented, as defined in the previous section, as the difference of the value of the variable of interest in the affected municipalities and their respective synthetic counterparts.

Due to data limitation, and to maintain the comparability of the results by not excluding many observations, we only present the results for 6 years prior and until 6 years after the beginning of the construction of the HPPs. The exception to this rule are the results for GDP and population, which we present only for 5 years prior and until 5 years after, due to data limitation.

5.1 Local economic activity

One first direct impact that the new HPP may have on the local economy is to change its population expressively. Since the construction of a HPP is a large engineering project, often implemented in areas with small population density, it has been documented that these projects

attract a mass of workers which may lead to agglomeration (Severnini, 2014). Therefore, we first look at the effects of the construction of a HPP on the municipalities' *population size*, shown in Figure 2d. This figure shows, for every year τ since the start of the construction of the HPP, the average (green solid line), the median (red dashed line) and the 25th and the 75th percentiles (the vertical bars) of the 82 estimated impacts $\hat{\alpha}_{i\tau}$ over time. We can observe that the magnitude of the median effects are small: the bigger effect takes place five years after the beginning of the construction, when the median municipality has a population 1 percent bigger than it would have had without the construction.

The variance of this effect, however, is large – and some localities appear to experience substantially larger population movement than others. This makes the average effect, which is around a 3% increase, bigger than the median in the short- and medium-run. We see that five years after the construction, the 25th percentile of the estimated impact is just below a 5% reduction in population size, while the 75th percentile of the effect is close to a 10% increase in population size. In sum, we observe that HPPs lead to local population growth at the average and the median of the distribution of effects, but effects are smaller and more heterogeneous than they are usually discussed as.

We now turn to look at the effects of HPP construction on different measures of local economic activity. The effect of the construction of HPPs on the *growth rate* of the municipal gross domestic product (GDPs) is shown in Figure 2a. The results point toward an inverted U-cycle of growth-retraction-normalization: during the first two years after the HPP's construction, the affected municipalities grow more than their respective control groups. The peak of this difference is a 3 percentage point increase in the growth of the median municipality. Like the results on population, the dispersion of this effect is substantial. At its highest point after a year of construction, the average effect, hitting a 7 percentage point increase, is higher than the median. After this, there is a reversion effect: three years after construction, the median municipality (affected by a HPP) grows at exactly the same rate as the ones in its comparison group. Four years after the treatment, the average construction time in our sample, the GDP of the local economy grows 5 percentage points less than its synthetic counterpart. This may capture exactly the end of the construction cycle. Figure 2a suggests that, in the medium term, the GDP growth rate of treated municipalities is similar to their comparison group, both at the mean and at the median.

Figure 2b presents the effects on the evolution of the *municipal GDP* at log-level, giving us a better picture of the accumulated effects of the changes in growth rates. The figure also shows a pattern of growth-reversion: after four years, the estimated median effect is zero. It's easier to see through this graph that heterogeneous effects on GDP make the average effect be consistently larger than the median effect. We will observe this pattern in many other indicators.

Since Figure 2d suggests that local population is not homegeneously affected by the HPP's con-

struction, this heterogeneous impact on GDP may be related to simple agglomeration. Figure 2c shows the impacts of HPP on *GDP per capita*. The growth-retraction-normalization pattern and the magnitudes of the effects are very similar to the ones depicted in figure 2b. The more striking difference is that the median effect is a 2.5% *reduction* in GDP per capita five years after the construction.

Table 2 present the results of conducting statistical inference on the average effect by year for each of these variables and figures. We see that the effect five years after the construction started is statistically equal to zero for all these four variables discussed. Table 3 presents the estimates of a simple difference in difference exercise similar to Duflo (2001). We also find no effect in the medium-run.

We observe the same pattern when we look at different indicators of economic activity. First we look at revenues of the municipal government. Figure 3a shows the impacts of HPP construction on current revenues, these are tax revenues plus transfers from the federal government and royalties. We see that the median current revenues increase, at growing rate, until the end of the contructions and stabilizes after that. This seems to be the effect of the royalties and direct transfers the operating HPP pays to the local government. Although this represent an increase in revenues, it does not reflect an increase in economic activity necessarily Caselli and Michaels (2013). When looking at municipal tax revenues, in Figure 3b, we see the same inverted-U pattern as with GDP or HDP per capita. Although the local tax revenues of the median municipality increases up to 35 percent two years after the construction, in the medium term the median effect is only 2.5%.

Figure 3d shows the evolution of the effects of HPP construction on the number of formally employed workers. In contrast to the cyclical pattern presented by GDP, the median effect on employment is sustained throughout the studied period, similarly to the pattern on local population size. After fast growth of this variable until two years after the construction, the effect oscillates around 6% and 11% for the remaining interval. That said, there is a large number of municipalities that have larger and more unstable effects: this leads to a cyclical behavior of the average effect. Moreover, the average effects are generally higher than the median ones, reaching a peak of 37% one year after the beginning of construction. This suggests that the HPP boots formalization of the workforce.[10]

All these results are based on official statistics from the government or its statistical bureau (IBGE). We also measure economic ativity using satellite data on night lights as an alternative indicator Henderson et al. (2012). We construct the log of the light intensity in each municipality at night. Figure 3c shows the results. We observe a similar pattern as before: a peak

[10]We find no effects on the number of formal firms, as defined by the number firm tax IDs (CNPJ) in the municipality, results in Table A6 in the appendix.

of light intensity during the construction of the HPP, but with effects reverting over time. The medium-run effect in the median municipality is stable and positive, similar to the results using population and formal employment.

Statistical inference based on the synthetic control exercise and difference in difference regressions presented in Tables 4 and 5 corroborate findings of Figure 3.

5.2 Structural transformation

The short-run economic boost created by contruction of the HPP and the medium-run effect on formalization can shift workers across sectors, potentially boosting a new set of economic activities to emerge. In this section we analyze the share of formal workers employed in each sector of the economy.

The first sector that should be directly affected is by the new HPP is the construction sector. In fact, we see in Figure 4a that the number of formal workers employed in the construction sector increase by more than 60% during the first three years, on average. As the construction unfold, we see that the average effect start to fade away and by the sixth year it is close to zero. We observe the same inversted-U pattern of average effect in employment in services and manufacturing sectors, but impacts are one order of magnitude smaller than the ones observed in the construction sector.

Surprisingly, the median effect of HPP of the construction of the new HPP on the size of the construction secto is flat close to zero during most of the period. This means that the construction sector in the affected municipalities evolved similarly to their synthetic counterparts. This could be due to outsourcing to contractors that bring workers from other construction sites in such a way that these are linked to firms registered in other municipalities.

In general, we observe small median effects in all sectors, except in agriculture. We observe that the construction process reduce the size of the agriculture sector, but this seems to revert in the end of the construction cycle. Tables 6 and 7 present the results of the statistical inference and difference in difference excercises.

These evidence corroborates the intuition derived from the aggregate economic measures. The HPP construction seem to trigger a short-lived economic prosperity on the average municipality, but without creating enought mommentum to led to lasting effects or structural change on the local economy more broadly.

5.3 Robustness

We perform several robustness and placebo tests to the main specification presented in section 5. In the first robustness test, we restrict the donor pool by excluding municipalities that are direct neighbors of treated units. This is to alleviate the concern that municipalities in the donor pool are somehow contaminated by the treatment. In a more extreme version of this robustness test, we exclude all the municipalities within the same micro-region of a treated unit.

In a third robustness test, we perform a placebo test, by anticipating the date of the intervention by two years. That is, for a municipality first affected by a HPP in 2007, we artificially change the treatment date to 2005, and recalculate the synthetic control municipality.[11] We hope that this lag give us a sense of how is the out-of-sample fit of synthetic control estimatives when there is no treatment yet.

Tables A1-A9 in the appendix show the results of these three robustness tests. As a general rule, there is not much difference between them and the main estimates. For example, the impact of HPP construction on GDP growth, one year after the treatment, is 6.5% and is significant at $\alpha = 1\%$ in the main estimates. When we exclude all municipalities neighboring treated units from the donor pool, the point estimate changes to 6.6% and remains significant at $\alpha = 1\%$. Exceptions to this rule are rare and generally concern effects for which either the main estimate or the robust one are not significant.

6 Conclusion

This paper analyzes the impact of the construction of hydroelectric power plants with dams for a large group of socioeconomic indicators. Contrary to more extreme reports – from both HPP enthusiasts and critics – we find that, in the medium-term, the majority of indicators were not impacted significantly by HPP construction.

Despite the typical medium-run effects being moderate, the same cannot be said about the short-run. For the three year period immediately after the beginning of the construction, the GDP growth accelerates. Furthermore, formal employment (especially in the construction sector), the municipal tax revenue and the night light measure from the space all increase significantly.

This study also concludes that the variation of HPP construction effects is very high, for all variables. Despite the typical municipality suffering only small or no effects at all from the

[11] However, when we show the results in tables A6-A9, we correct the treatment dates. This is, the $\tau - 0$ coefficient shows the estimated impact in the real treatment year.

treatment, there is a significant number of cases with large effects. Due to this, we conclude that the most famous cases of hydroelectric power plants constructions – when the effect was either greatly beneficial or disproportionately bad for the local population – should not be viewed as representative examples of this type of intervention.

This study focuses on documenting the typical effects stemming from the construction of hydroelectric power plants. However, we consider that it is essential to understand the mechanisms underlying these heterogeneous impacts in order to take advantage of the good opportunities and to avoid the risks of the economic costly ones.

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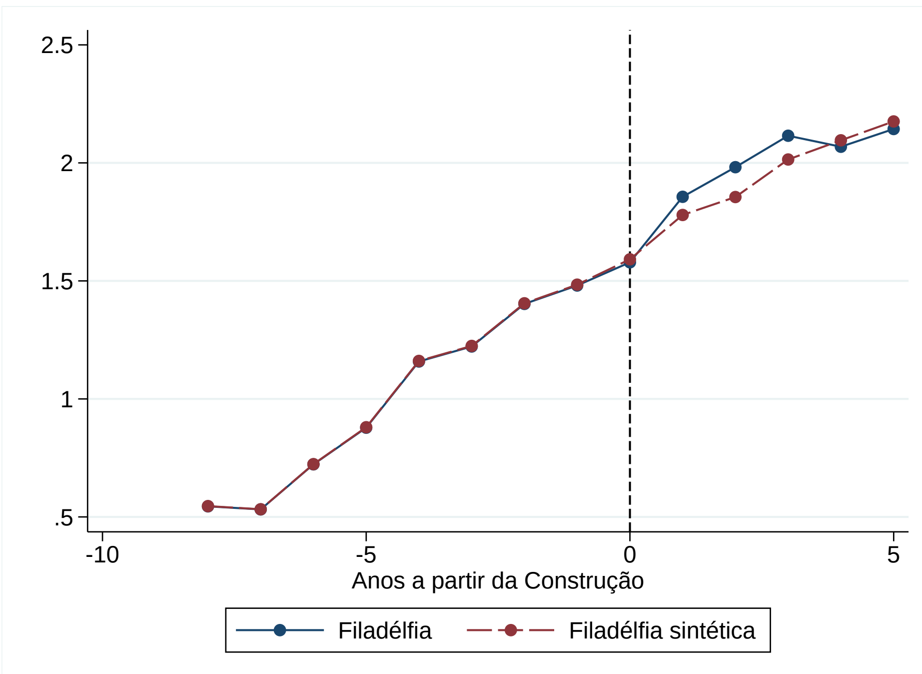

Figure 1: Filadélfia vs. *Synthetic* Filadélfia

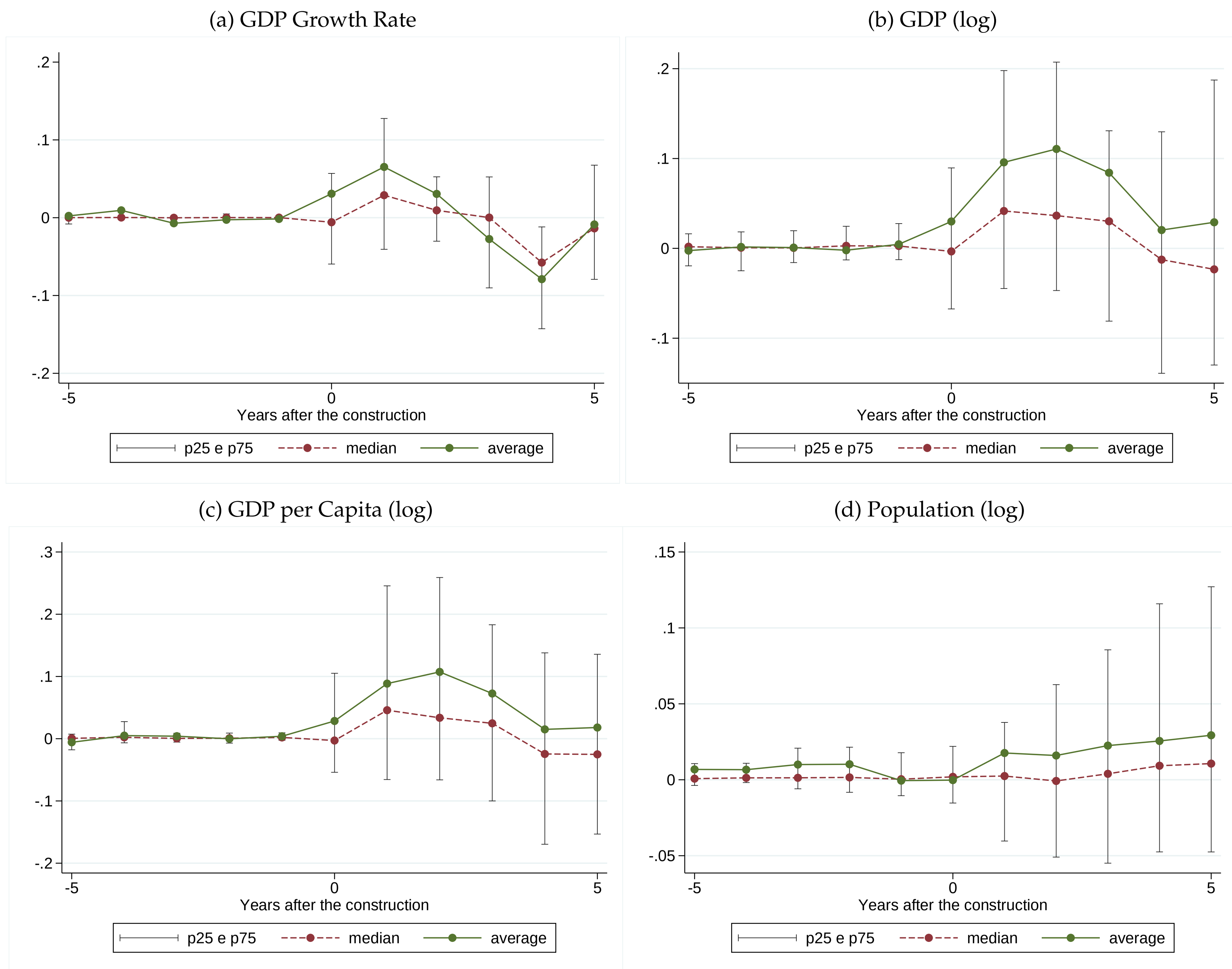

Figure 2: Effects on GDP and Population

τ=0 is normalized to equal the year of the HPP construction initiation

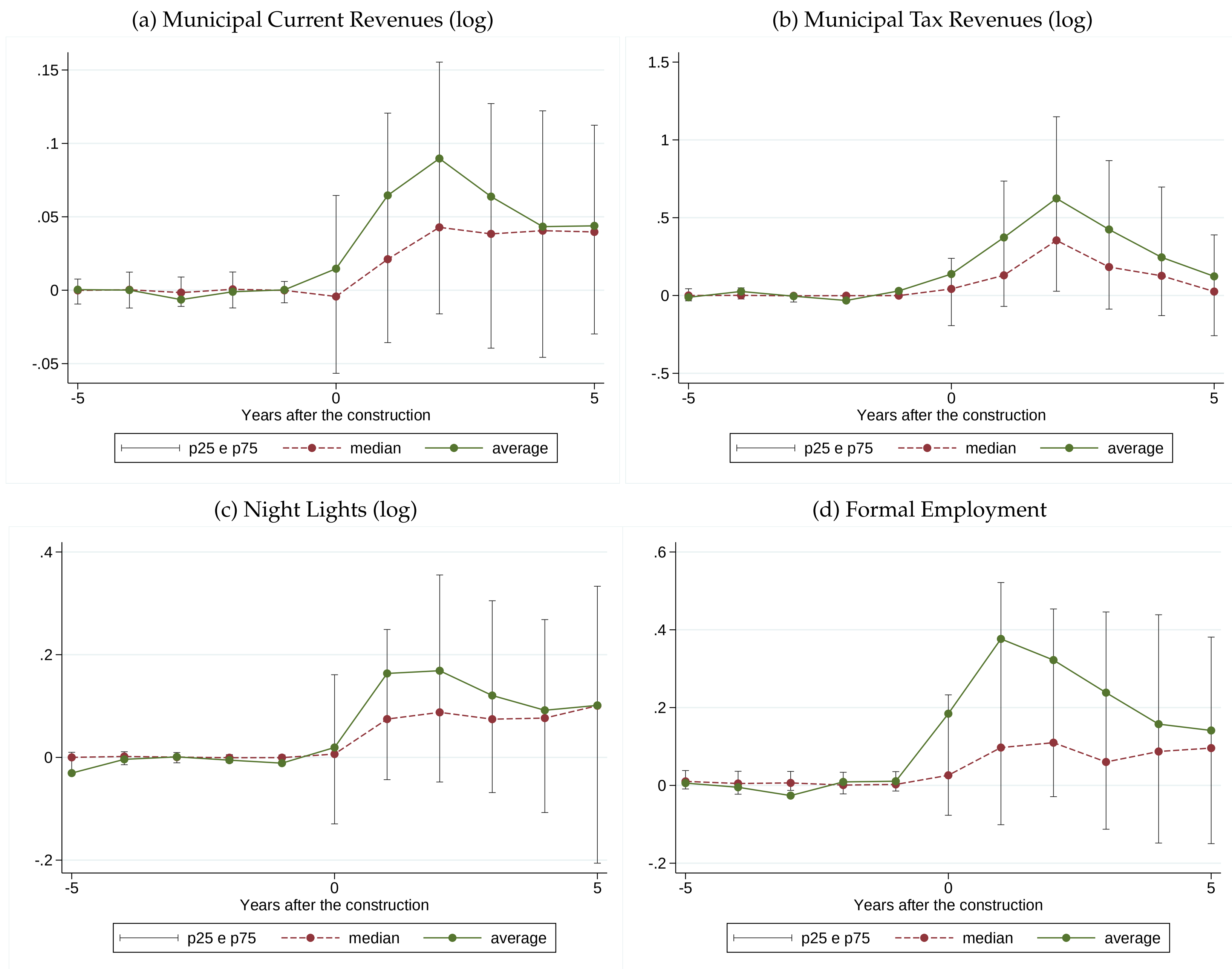

Figure 3: Effects on Revenues of Municipal Governments, Formal Employment and Nightlights

τ=0 is normalized to equal the year of the HPP construction initiation

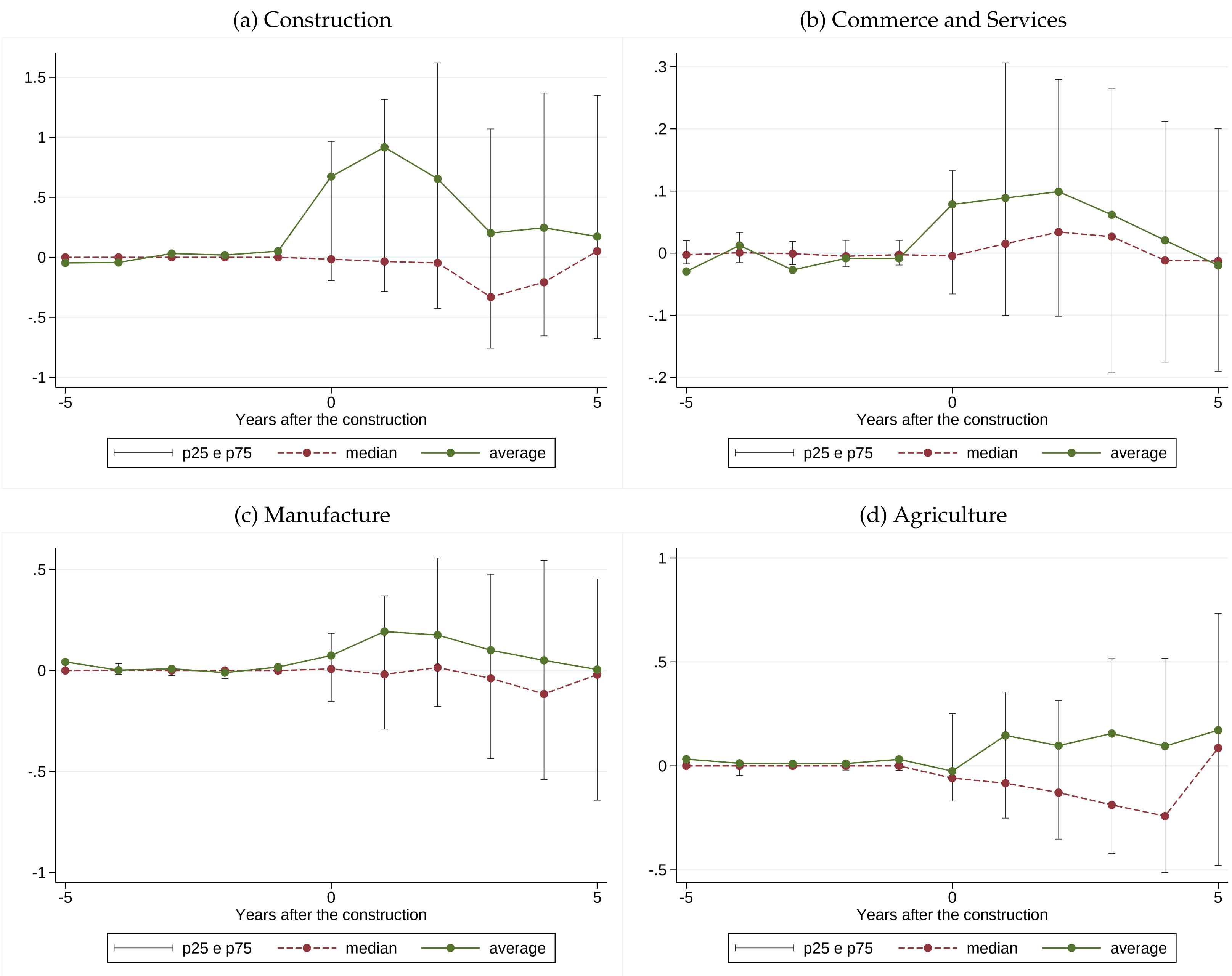

Figure 4: Effects on Employment: Structural Transformation

τ=0 is normalized to equal the year of the HPP construction initiation

Table 1: Summary statistics in 2000

	Donor Pool		Treated Municipalities	
	Average	Standart Deviation	Average	Standart Deviation
Municipal GDP				
Total (milions of BRL)	122.1	688.4	173.3	467.4
Per Capita (BRL)	3,959.3	4,511.1	3,990.2	2,662.9
Groth (%)	12.0	15.4	10.2	9.5
Agro GDP/GDP	0.3	0.2	0.3	0.2
Population				
Total Population[a]	23.0	88.0	36.2	85.3
Men per 100 Women[b]	104.5	6.0	104.4	7.8
Number of formal companies	207.5	1,352.7	395.8	1,193.8
Formal Employment	1,900.3	14,887.0	3,367.7	10,045.2
Revenues (milions of R$ (Ago/2014))				
Total Revenue	30.6	164.9	40.6	98.4
Total Tax Revenue	3.2	37.7	4.3	17.8
Education and Culture	8.6	30.6	10.7	20.2
Health and Sanitation	6.7	54.1	9.0	29.4
Mortality[c]				
Deaths per 100 Thousand inhabitants	323.4	196.0	342.9	190.1
Homicides per 100 housand inhabitants	7.7	14.0	8.4	13.2
Hospital Admissions per 100 mil inhabitants[c]				
by Mosquito-Borne Diseases	58.0	308.7	80.3	194.5
por Sexually Transmitted Diseases	19.9	54.8	23.3	54.7
Sanitation Infraestructure				
Density of the Water Network (100 km/km^2)	0.1	0.6	0.1	0.2
Density of the Sewage Network (100 km/km^2)	0.0	0.3	0.0	0.1
Number of Municipalities	2,923		82	

Notas:

Table 2: Mean Effects. Synthetic Control

	Population(log)	GDP growth	GDP (log)	GDP per capita (log)
$\tau - 4$	0.007	0.009	0.002	0.005
	(0.96)	(0.11)	(0.94)	(0.45)
$\tau - 3$	0.010	-0.007	0.001	0.004
	(0.81)	(0.23)	(0.96)	(0.43)
$\tau - 2$	0.010	-0.003	-0.002	-0.000
	(0.79)	(0.54)	(0.91)	(0.98)
$\tau - 1$	-0.001	-0.002	0.005	0.004
	(1.00)	(0.72)	(0.75)	(0.47)
$\tau + 0$	-0.000	0.031^{*}	0.030^{*}	0.028^{**}
	(1.00)	(0.09)	(0.06)	(0.03)
$\tau + 1$	0.018	0.065^{***}	0.096^{***}	0.089^{***}
	(0.46)	(0.00)	(0.00)	(0.00)
$\tau + 2$	0.016	0.031^{*}	0.111^{***}	0.107^{***}
	(0.58)	(0.07)	(0.00)	(0.00)
$\tau + 3$	0.023	-0.027^{*}	0.084^{***}	0.073^{***}
	(0.50)	(0.06)	(0.00)	(0.00)
$\tau + 4$	0.026	-0.079^{***}	0.020	0.015
	(0.43)	(0.00)	(0.41)	(0.56)
$\tau + 5$	0.029	-0.009	0.029	0.018
	(0.35)	(0.65)	(0.33)	(0.56)

Notes: This table shows mean effects of the construction of HPPs on each outome, as estimated by the synthetic control method. The method is described in section 3. The synthetic control point estimates are identical to the ones showed by Figure 2. p-values calculated using permutation tests, as decribed in section 3.3, are shown in paretheses.

Table 3: Mean Effects, Differences-in-Differencess

	Population(log)	GDP growth	GDP (log)	GDP per capita (log)
$\tau - 4$	-0.0049**	0.027	0.015	0.020
	[0.049]	[0.36]	[0.37]	[0.24]
$\tau - 3$	-0.0028	0.028	0.012	0.014
	[0.75]	[0.23]	[0.58]	[0.48]
$\tau - 2$	-0.0055	0.013	0.015	0.021
	[0.59]	[0.60]	[0.62]	[0.49]
$\tau - 1$	-0.026	-0.021	-0.0071	0.019
	[0.12]	[0.34]	[0.82]	[0.53]
$\tau + 0$	-0.029	0.047**	0.025	0.053*
	[0.10]	[0.031]	[0.45]	[0.093]
$\tau + 1$	-0.0091	0.082***	0.087**	0.096***
	[0.59]	[0.0015]	[0.021]	[0.0075]
$\tau + 2$	0.0020	0.028	0.10**	0.10**
	[0.91]	[0.24]	[0.022]	[0.014]
$\tau + 3$	0.0078	-0.018	0.082*	0.073*
	[0.66]	[0.49]	[0.070]	[0.088]
$\tau + 4$	0.0019	-0.069***	0.015	0.013
	[0.92]	[0.0031]	[0.73]	[0.75]
$\tau + 5$	0.0010	0.011	0.018	0.019
	[0.96]	[0.57]	[0.67]	[0.64]
Observations	46,132	40,055	43,097	43,097
Mean dep. var.	9.304	0.135	11.079	1.780

Notes: This table shows mean effects of the construction of HPPs on each outome, as estimated by differences-in-differences. P-values are shown in paretheses.

Table 4: Mean Effects. Synthetic Control.

	Tax Revenue (log)	Current Revenue (log)	Lights (log)	Formal Employment (ihs)
$\tau - 4$	0.026	0.000	-0.004	-0.005
	(0.22)	(0.99)	(0.78)	(0.79)
$\tau - 3$	-0.004	-0.006	0.001	-0.026^{*}
	(0.80)	(0.18)	(0.92)	(0.06)
$\tau - 2$	-0.032^{**}	-0.001	-0.005	0.009
	(0.05)	(0.83)	(0.62)	(0.51)
$\tau - 1$	0.029	0.000	-0.011	0.011
	(0.12)	(0.96)	(0.29)	(0.46)
$\tau + 0$	0.138^{***}	0.015	0.019	0.184^{***}
	(0.00)	(0.26)	(0.51)	(0.00)
$\tau + 1$	0.373^{***}	0.065^{***}	0.164^{***}	0.377^{***}
	(0.00)	(0.00)	(0.00)	(0.00)
$\tau + 2$	0.624^{***}	0.090^{***}	0.169^{***}	0.322^{***}
	(0.00)	(0.00)	(0.00)	(0.00)
$\tau + 3$	0.424^{***}	0.064^{***}	0.121^{**}	0.238^{***}
	(0.00)	(0.00)	(0.02)	(0.00)
$\tau + 4$	0.245^{***}	0.043^{*}	0.092	0.158^{**}
	(0.00)	(0.09)	(0.19)	(0.02)
$\tau + 5$	0.123^{*}	0.044^{**}	0.101	0.141^{**}
	(0.06)	(0.02)	(0.19)	(0.04)

Notes: This table shows mean effects of the construction of HPPs on each outome, as estimated by the synthetic control method and differences-in-differences. The method is described in section 3. The synthetic control point estimates are identical to the ones showed by Figure 4. p-values calculated using permutation tests, as decribed in section 3.3, are shown in paretheses.

Table 5: Mean Effects, Differences-in-Differences

	Tax Revenue (log)	Current Revenue (log)	Lights (log)	Formal Employment (ihs)
$\tau - 4$	0.068	-0.0013	0.069	0.056
	[0.25]	[0.90]	[0.13]	[0.34]
$\tau - 3$	0.067	-0.016	0.088*	-0.046
	[0.34]	[0.28]	[0.062]	[0.57]
$\tau - 2$	0.048	-0.0060	0.038	0.031
	[0.49]	[0.68]	[0.42]	[0.73]
$\tau - 1$	0.10	0.00088	0.031	0.032
	[0.19]	[0.95]	[0.54]	[0.72]
$\tau + 0$	0.24**	0.0079	0.071	0.19
	[0.032]	[0.71]	[0.24]	[0.10]
$\tau + 1$	0.50***	0.047	0.21***	0.33***
	[0.000096]	[0.11]	[0.0063]	[0.0095]
$\tau + 2$	0.70***	0.069**	0.24***	0.28**
	[0.000000100]	[0.035]	[0.0027]	[0.026]
$\tau + 3$	0.56***	0.046*	0.23***	0.16
	[0.000010]	[0.066]	[0.0030]	[0.19]
$\tau + 4$	0.34***	0.019	0.24***	0.046
	[0.00051]	[0.40]	[0.0020]	[0.67]
$\tau + 5$	0.21**	0.019	0.24***	0.0083
	[0.033]	[0.43]	[0.0044]	[0.94]
Observations	44,468	45,648	42,614	43,083
Mean dep. var.	-0.513	2.946	-0.434	6.107

Notes: This table shows mean effects of the construction of HPPs on each outome, as estimated by differences-in-differences. P-values are shown in paretheses.

Table 6: Mean Effects. Synthetic Control

	Employment in Construction (ihs)	Employment in Commerce and services(ihs)	Employment in Manufacture(ihs)	Employment in Agriculture(ihs)
$\tau - 4$	-0.043	0.012	0.002	0.012
	(0.18)	(0.43)	(0.95)	(0.63)
$\tau - 3$	0.032	-0.027	0.009	0.010
	(0.22)	(0.06)	(0.67)	(0.64)
$\tau - 2$	0.019	-0.008	-0.010	0.011
	(0.44)	(0.51)	(0.57)	(0.58)
$\tau - 1$	0.051^{*}	-0.009	0.017	0.031
	(0.06)	(0.47)	(0.34)	(0.17)
$\tau + 0$	0.673^{***}	0.078^{**}	0.074	-0.025
	(0.00)	(0.04)	(0.32)	(0.75)
$\tau + 1$	0.916^{***}	0.089^{*}	0.193^{**}	0.146
	(0.00)	(0.09)	(0.03)	(0.14)
$\tau + 2$	0.654^{***}	0.099^{**}	0.176^{*}	0.097
	(0.00)	(0.03)	(0.09)	(0.41)
$\tau + 3$	0.202	0.062	0.100	0.156
	(0.21)	(0.14)	(0.38)	(0.27)
$\tau + 4$	0.246	0.021	0.050	0.095
	(0.12)	(0.68)	(0.69)	(0.55)
$\tau + 5$	0.173	-0.020	0.005	0.172
	(0.29)	(0.69)	(0.97)	(0.31)

Notes: This table shows mean effects of the construction of HPPs on each outome, as estimated by the synthetic control method. The method is described in section 3. The synthetic control point estimates are identical to the ones showed by Figure 4. p-values calculated using permutation tests, as decribed in section 3.3, are shown in paretheses.

Table 7: Mean Effects. Difference-in-Differences

	Construction	Commerce and services	Manufacture	Agriculture
$\tau - 4$	0.15	0.069	0.078	0.029
	[0.12]	[0.19]	[0.41]	[0.78]
$\tau - 3$	0.18	-0.024	-0.094	0.022
	[0.25]	[0.77]	[0.41]	[0.87]
$\tau - 2$	0.27*	0.092	-0.20	0.15
	[0.095]	[0.26]	[0.11]	[0.32]
$\tau - 1$	0.24	0.069	-0.15	0.17
	[0.21]	[0.42]	[0.26]	[0.22]
$\tau + 0$	0.85***	0.11	-0.096	0.080
	[0.00056]	[0.19]	[0.50]	[0.60]
$\tau + 1$	1.02***	0.13	0.015	0.24
	[0.00019]	[0.16]	[0.91]	[0.18]
$\tau + 2$	0.77***	0.14	0.035	0.21
	[0.0054]	[0.16]	[0.80]	[0.29]
$\tau + 3$	0.40	0.044	-0.038	0.25
	[0.12]	[0.65]	[0.78]	[0.23]
$\tau + 4$	0.36	0.00092	-0.078	0.19
	[0.12]	[0.99]	[0.57]	[0.39]
$\tau + 5$	0.28	-0.062	-0.12	0.25
	[0.25]	[0.46]	[0.42]	[0.24]
Observations	43,083	43,083	43,083	43,083
Mean dep. var.	2.075	5.360	4.231	2.234

Notes: This table shows mean effects of the construction of HPPs on each outome, as estimated by differences-in-differences. P-values are shown in paretheses.

Table A1: Mean Effects - no neighbors

	Population(log)	GDP growth	GDP (log)	GDP per capita (log)
$\tau - 4$	0.009	0.010	0.001	0.004
	(0.38)	(0.22)	(0.88)	(0.64)
$\tau - 3$	0.010	−0.005	−0.004	0.004
	(0.15)	(0.50)	(0.67)	(0.56)
$\tau - 2$	0.009	0.002	−0.002	0.002
	(0.24)	(0.74)	(0.80)	(0.72)
$\tau - 1$	−0.004	0.002	0.003	0.006
	(0.69)	(0.74)	(0.67)	(0.31)
$\tau + 0$	−0.004	0.033 **	0.028 **	0.033 **
	(0.84)	(0.04)	(0.05)	(0.02)
$\tau + 1$	0.015	0.066 ***	0.101 ***	0.090 ***
	(0.22)	(0.00)	(0.00)	(0.00)
$\tau + 2$	0.015	0.007	0.122 ***	0.095 ***
	(0.32)	(0.67)	(0.00)	(0.00)
$\tau + 3$	0.023	−0.028*	0.089 ***	0.070 ***
	(0.28)	(0.07)	(0.00)	(0.01)
$\tau + 4$	0.023	−0.081 ***	0.015	0.014
	(0.31)	(0.00)	(0.52)	(0.56)
$\tau + 5$	0.026	−0.012	0.007	0.015
	(0.27)	(0.61)	(0.79)	(0.61)

Notes: This table shows mean effects of the construction of HPPs on each outome, as estimated by the synthetic control method. The method is described in section 4 and 5.3. Differently from the main estimates, for this table, municipalities that border affected units were excluded from the donor pool. p-values calculated using permutation tests, as decribed in section 4, are shown in paretheses.

7 Apêndice

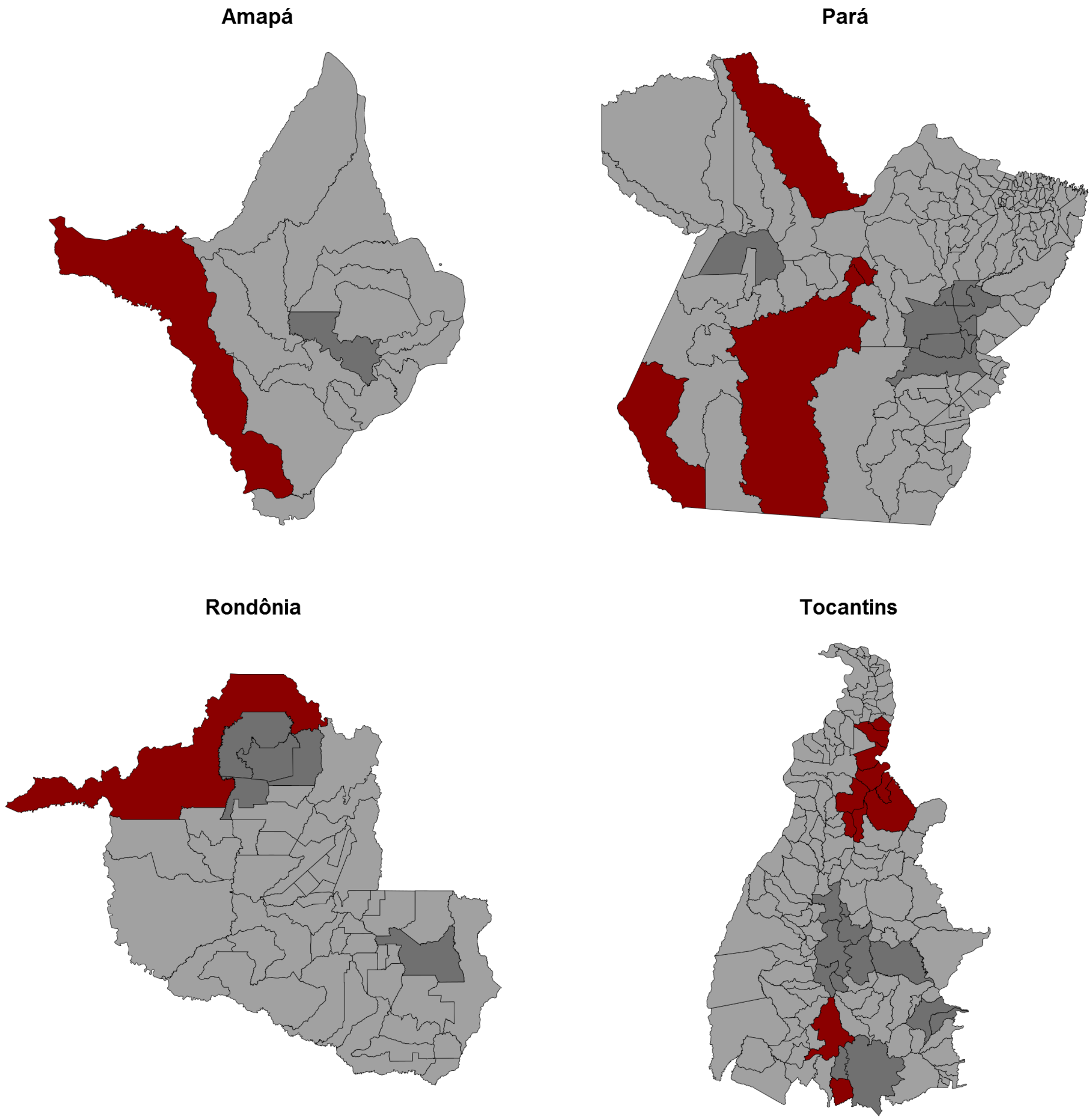

Figure A1: States of the "Norte" Region

Notes: The analyzed municipalities are in red; the excluded municipalities (because they affected by more than one dam, or by a dam constructed before 2002 or after 2011) are in dark gray. The other municipalities form the donor pool.

Affected municipalities: Amapá – The Santo Antônio do Jari HPP affects the municipality of Laranjal do Jari. Pará – The Belo Monte HPP affects the municipalities of Altamira and Vitória do Xingu. The Santo Antônio do Jari HPP affects the municipality of Almeirim. The Teles Pires affects the municipality of Jacareacanga. Rondônia – The Madeira (Santo Antônio + Jirau) affects the municipality of Porto Velho. Tocantins – The Estreito HPP affects the municipalities of Babaçulândia, Barra do Ouro, Darcinópolis, Filadélfia, Goiatins, Itapiratins, Palmeiras do Tocantins, Palmeirante and Tupiratins. The Peixe Angical HPP affects the municipalities of Peixe. The São Salvador affects the municipality of Palmeirópolis.

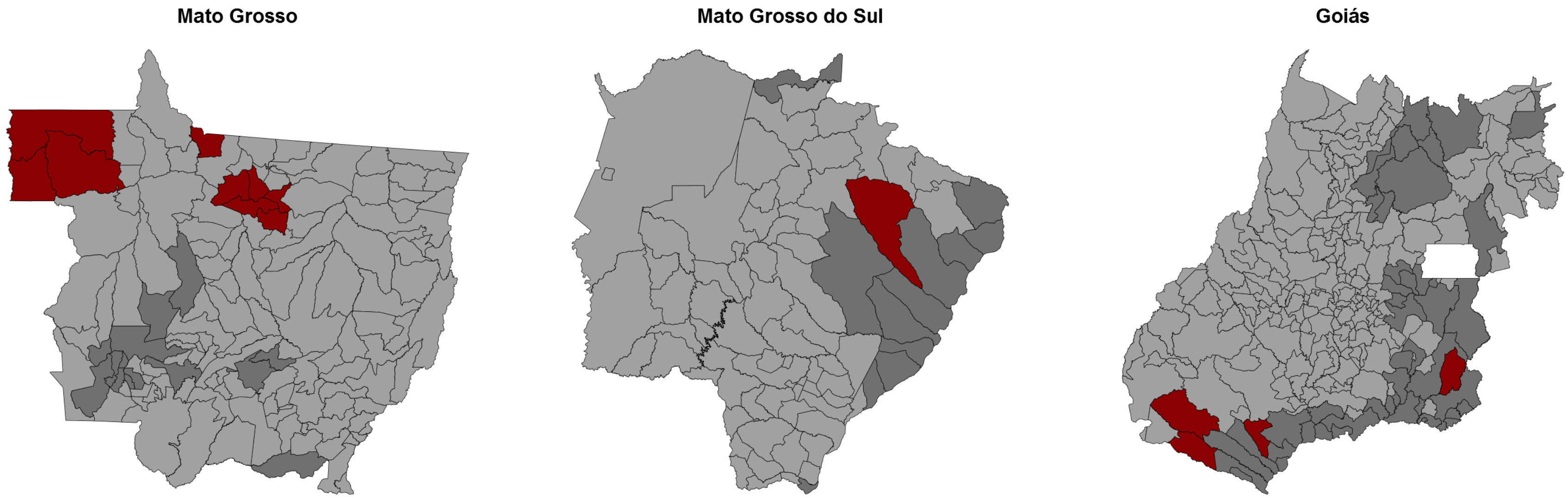

Figure A3: States of the "Centro-Oeste" Region

Notes: The analyzed municipalities are in red; the excluded municipalities (because they affected by more than one dam, or by a dam constructed before 2002 or after 2011) are in dark gray. The other municipalities form the donor pool.
Municípios afetados: Mato Grosso – The Colider HPP affects the municipalities of Cláudia, Colíder, Itaúba, Nova Santa Helena and Nova Canaã do Norte. The Dardanelos HPP affects the municipalities of Aripuanã, Colniza and Rondolândia. The Teles Pires HPP affects the municipalities Paranaíta. Mato Grosso Do Sul – The São Domingos HPP affects the municipality of Água Clara. Goiás – The Caçu e Barra dos Coqueiros HPP affects the municipality of Cachoeira Alta. The Espora affects the municipalities of Aporé and Serranópolis. The Serra Do Facão HPP affects the municipality of Campo Alegre de Goiás.

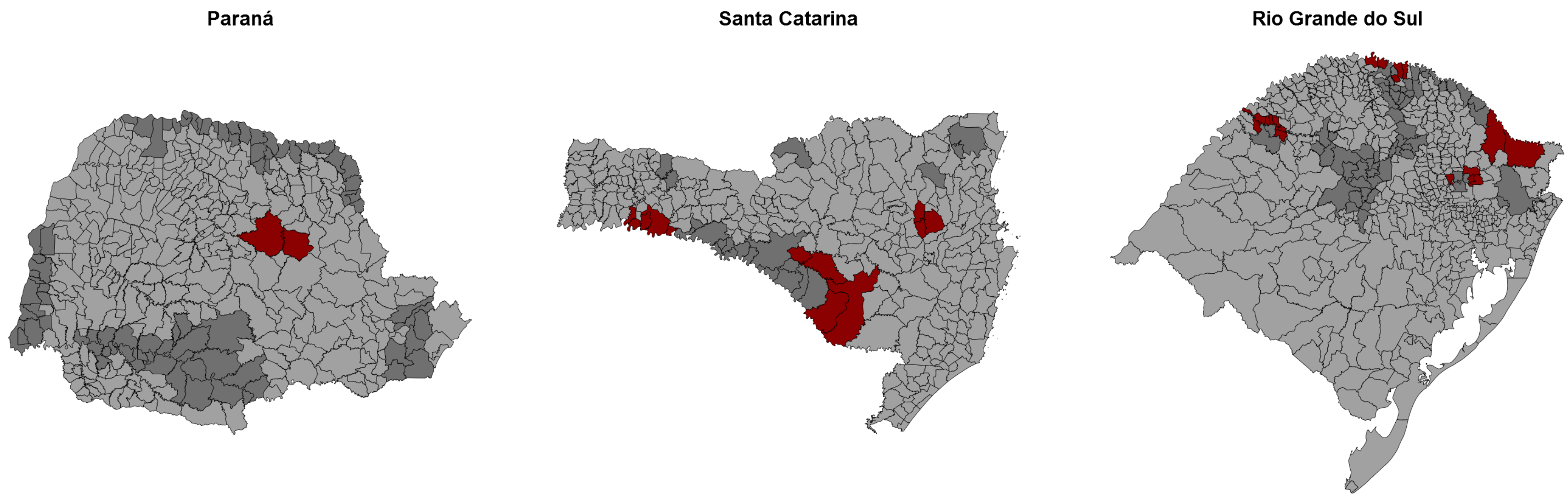

Figure A4: States of the "Sul" Region

Notes: The analyzed municipalities are in red; the excluded municipalities (because they affected by more than one dam, or by a dam constructed before 2002 or after 2011) are in dark gray. The other municipalities form the donor pool.

Affected municipalities: Paraná – The Mauá HPP affects the municipalities of Ortigueira and Telêmaco Borba. Santa Catarina – The Barra Grande affects the municipalities of Capão Alto and Lages. The Foz Chapecó affects the municipalities of Águas de Chapecó, Caxambu do Sul, Chapecó, Guatambú and Paial. The Garibaldi dam affects the municipalities of Vargem and São José do Cerrito. The Salto do Pilão HPP affects the municipalities of Apiúna, Ibirama and Lontras. Rio Grande do Sul – The 14 de Julho HPP affects the municipality of Cotiporã. The Barra Grande HPP affects the municipality of Bom Jesus and Vacaria. The Castro Alves HPP affects the municipalities of Antônio Prado, Flores da Cunha and Nova Pádua. The Foz Chapecó HPP affects the municipalities of Alpestre, Erval Grande, Itatiba do Sul and Rio dos Índios. The Monjolinho HPP affects the municipality of Benjamin Constant do Sul. The Passo Sao João HPP affects the municipalities of Dezesseis de Novembro, Roque Gonzales and São Pedro do Butiá. The São José affects the municipalities of Caibaté, Cerro Largo, Mato Queimado and Salvador das Missões.

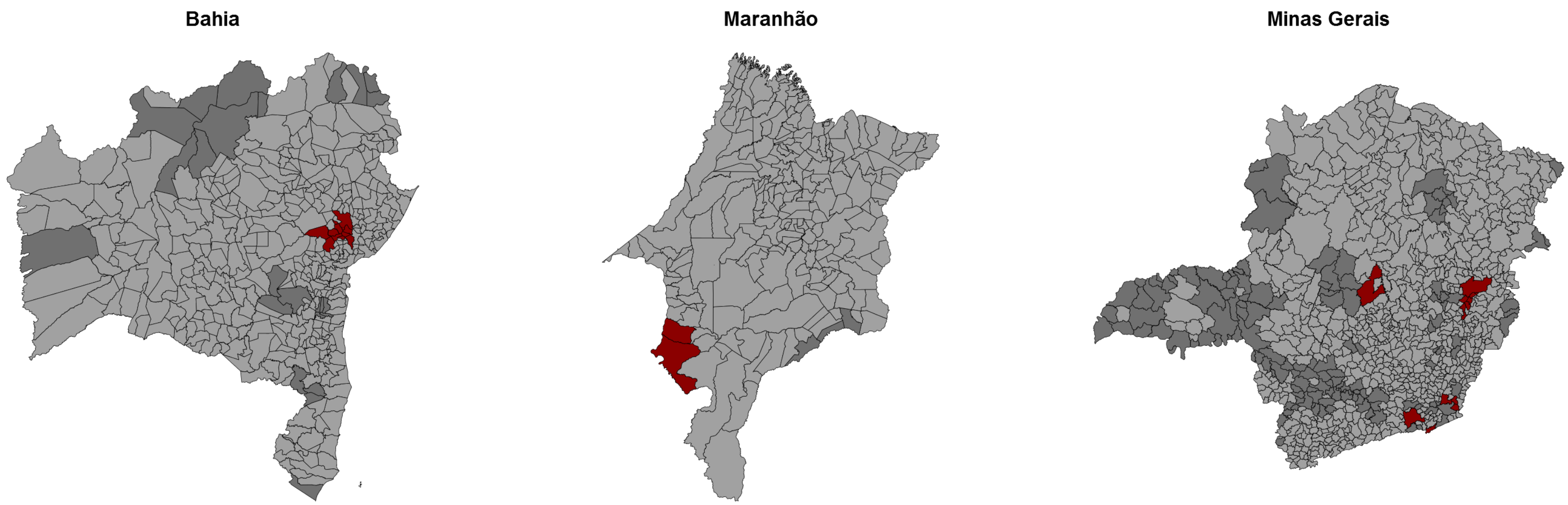

Figure A5: States of Bahia, Maranhão and Minas Gerais

Notes: The analyzed municipalities are in red; the excluded municipalities (because they affected by more than one dam, or by a dam constructed before 2002 or after 2011) are in dark gray. The other municipalities form the donor pool.

Affected Municipalities: Bahia – The Pedra Cavalo HPP affects the municipalities of Antônio Cardoso, Cabaceiras do Paraguaçu, Cachoeira, Castro Alves, Conceição da Feira, Feira de Santana, Governador Mangabeira, Rafael Jambeiro, Santo Estêvão e São Gonçalo dos Campos. Maranhão – The Estreito HPP affects the municipalities of Carolina and Estreito. Minas Gerais – The Baguari HPP affects the municipalities of Alpercata, Fernandes Tourinho, Governador Valadares, Iapu, Periquito and Sobrália. The Barra do Brauna HPP affects the municipalities of Cataguases, Laranjal and Recreio. The Picada HPP affects the municipality of Juiz de Fora. The Retiro Baixo HPP affects the municipality of Curvelo. The Simplicio HPP affects the municipality of Chiador.

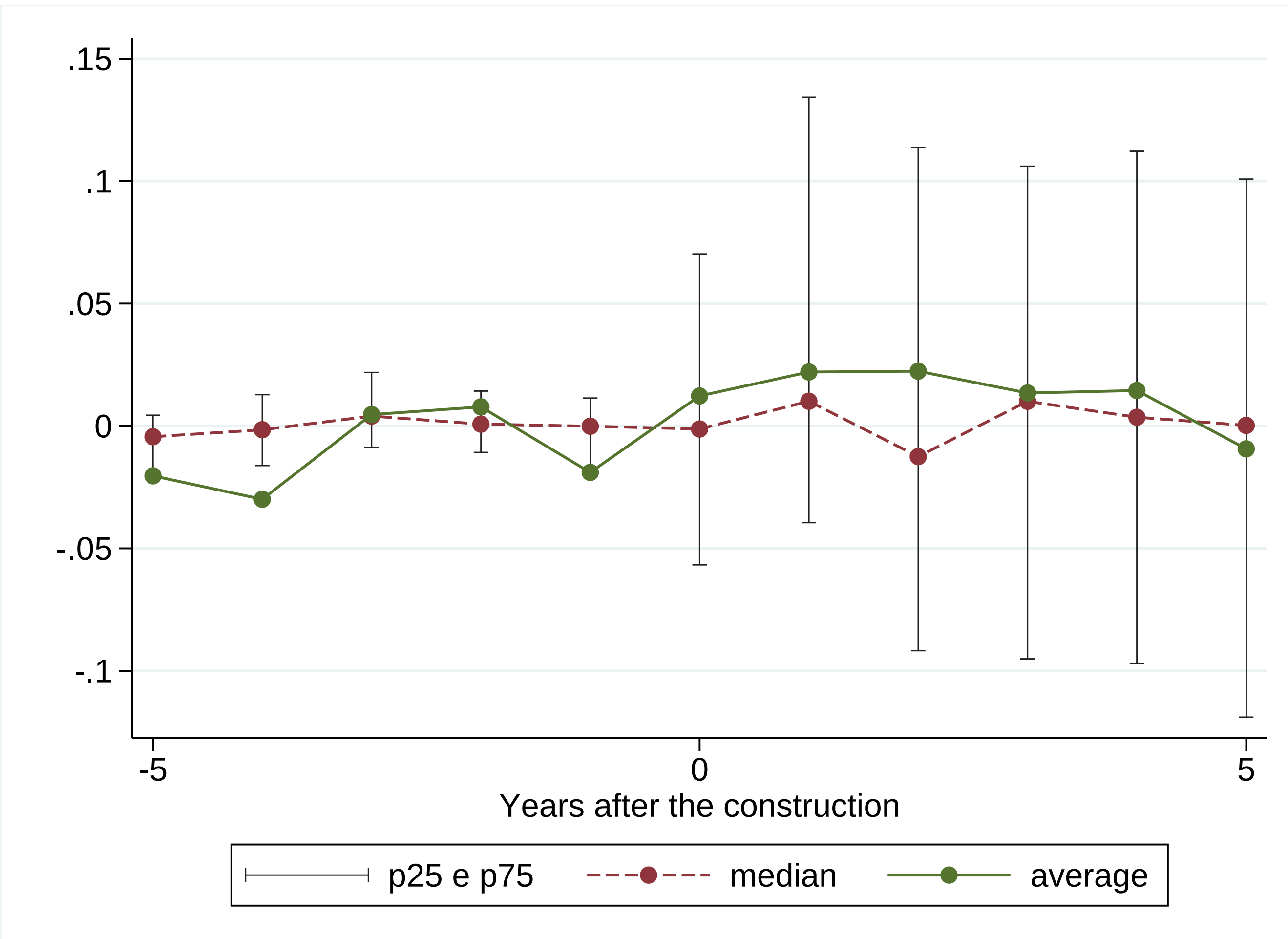

Figure A6: Effects on Number of Registered Formal Companies(log)

τ=0 is normalized to equal the year of the HPP construction initiation

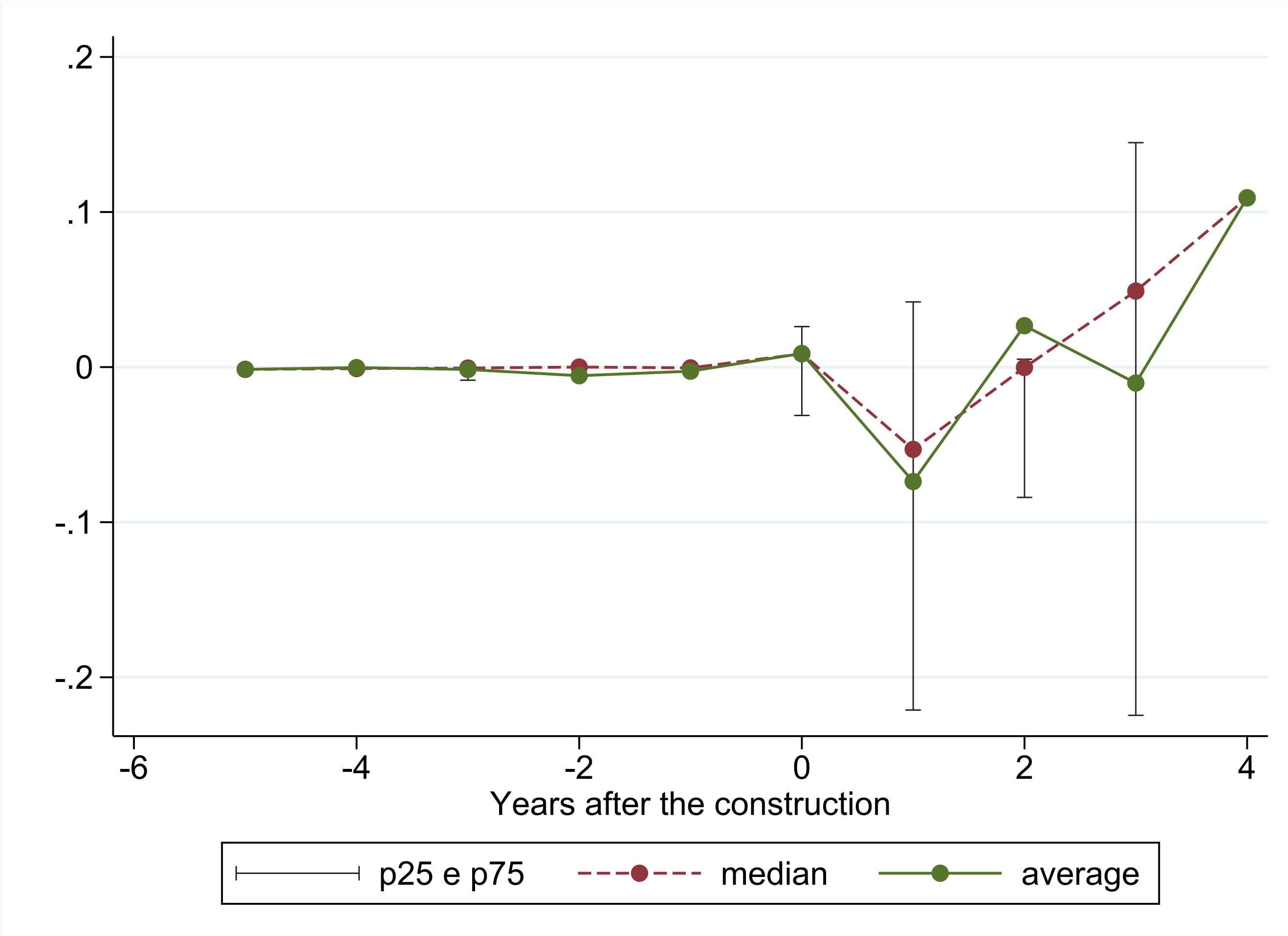

Figure A7: Effects on Price Index Variation

τ=0 is normalized to equal the year of the HPP construction initiation. There is data for only five treated units and 368 control units. Because of the lower number of available control units, we did not exclude controls that are more than one quintile away of the treated unit in respect to the variable of interest in the treated municipality's state

Table A2: Mean Effects - no neighbors

	Tax Revenue (log)	Current Revenue (log)	Lights (log)	Formal Employment (ihs)
$\tau - 4$	0.033	−0.001	0.006	−0.012
	(0.20)	(0.92)	(0.75)	(0.51)
$\tau - 3$	0.008	−0.005	−0.005	−0.027*
	(0.71)	(0.38)	(0.71)	(0.08)
$\tau - 2$	−0.015	0.002	−0.021	−0.004
	(0.46)	(0.69)	(0.15)	(0.82)
$\tau - 1$	0.032	0.003	−0.018	0.012
	(0.19)	(0.59)	(0.21)	(0.47)
$\tau + 0$	0.159 ***	0.019	0.038	0.166 ***
	(0.00)	(0.23)	(0.20)	(0.00)
$\tau + 1$	0.409 ***	0.061 ***	0.185 ***	0.323 ***
	(0.00)	(0.00)	(0.00)	(0.00)
$\tau + 2$	0.651 ***	0.085 ***	0.209 ***	0.271 ***
	(0.00)	(0.00)	(0.00)	(0.00)
$\tau + 3$	0.441 ***	0.057 ***	0.162 ***	0.180 ***
	(0.00)	(0.00)	(0.00)	(0.00)
$\tau + 4$	0.289 ***	0.038	0.115 **	0.087
	(0.00)	(0.23)	(0.03)	(0.12)
$\tau + 5$	0.175 ***	0.037*	0.127 **	0.062
	(0.01)	(0.08)	(0.03)	(0.31)

Notes: This table shows mean effects of the construction of HPPs on each outome, as estimated by the synthetic control method. The method is described in section 4 and 5.3. Differently from the main estimates, for this table, municipalities that border affected units were excluded from the donor pool. p-values calculated using permutation tests, as decribed in section 4, are shown in paretheses.

Table A3: Mean Effects - no neighbors

	Formal employment in Construction(ihs)	Formal employment in Commerce and services(ihs)	Formal employment in Manufacture(ihs)	Formal employment in Agriculture(ihs)
$\tau - 4$	-0.032	0.002	-0.017	-0.040
	(0.69)	(0.91)	(0.63)	(0.51)
$\tau - 3$	0.063	-0.005	0.014	-0.011
	(0.27)	(0.71)	(0.62)	(0.81)
$\tau - 2$	-0.022	-0.020	-0.018	0.045
	(0.66)	(0.18)	(0.57)	(0.33)
$\tau - 1$	0.055	-0.015	0.029	0.070
	(0.36)	(0.27)	(0.35)	(0.16)
$\tau + 0$	$0.540***$	0.047	0.067	0.067
	(0.00)	(0.20)	(0.37)	(0.57)
$\tau + 1$	$0.501***$	$0.086*$	0.143	0.079
	(0.01)	(0.08)	(0.12)	(0.53)
$\tau + 2$	0.172	0.075	$0.197*$	0.133
	(0.44)	(0.11)	(0.07)	(0.38)
$\tau + 3$	-0.063	0.041	$0.212*$	0.123
	(0.78)	(0.36)	(0.09)	(0.50)
$\tau + 4$	-0.170	0.023	0.104	0.186
	(0.44)	(0.65)	(0.41)	(0.35)
$\tau + 5$	-0.407	-0.011	0.108	0.184
	(0.11)	(0.83)	(0.43)	(0.44)

Notes: This table shows mean effects of the construction of HPPs on each outome, as estimated by the synthetic control method. The method is described in section 4 and 5.3. Differently from the main estimates, for this table, municipalities that border affected units were excluded from the donor pool. p-values calculated using permutation tests, as decribed in section 4, are shown in paretheses.

Table A4: Mean Effects - no contamined microrregions

	Population(log)	GDP growth	GDP (log)	GDP per capita (log)
$\tau-4$	0.007	0.007	0.005	0.018 **
	(0.51)	(0.34)	(0.54)	(0.04)
$\tau-3$	0.008	−0.002	−0.004	0.009*
	(0.33)	(0.76)	(0.56)	(0.09)
$\tau-2$	0.007	0.002	0.00	0.012*
	(0.48)	(0.74)	(0.82)	(0.08)
$\tau-1$	−0.007	−0.012 **	0.003	0.010
	(0.47)	(0.05)	(0.65)	(0.19)
$\tau+0$	−0.009	0.032*	0.030 **	0.041 ***
	(0.29)	(0.08)	(0.03)	(0.01)
$\tau+1$	0.015	0.082 ***	0.108 ***	0.101 ***
	(0.13)	(0.00)	(0.00)	(0.00)
$\tau+2$	0.014	0.026	0.135 ***	0.120 ***
	(0.22)	(0.14)	(0.00)	(0.00)
$\tau+3$	0.025	−0.034 **	0.099 ***	0.093 ***
	(0.12)	(0.06)	(0.00)	(0.00)
$\tau+4$	0.025	−0.084 ***	0.024	0.025
	(0.11)	(0.00)	(0.36)	(0.33)
$\tau+5$	0.027*	−0.001	0.012	0.016
	(0.10)	(0.96)	(0.69)	(0.60)

Notes: This table shows mean effects of the construction of HPPs on each outome, as estimated by the synthetic control method. The method is described in section 4 and 5.3. . Differently from the main estimates, for this table, municipalities within the same microrregion of affected units were excluded from the donor pool. p-values calculated using permutation tests, as decribed in section 4, are shown in paretheses.

Table A5: Mean Effects - no contamined microrregions

	Tax Revenue (log)	Current Revenue (log)	Lights (log)	Formal Employment (ihs)
$\tau - 4$	0.038	−0.003	0.010	−0.021
	(0.17)	(0.62)	(0.57)	(0.31)
$\tau - 3$	0.007	−0.008	−0.012	−0.034*
	(0.74)	(0.16)	(0.39)	(0.06)
$\tau - 2$	−0.024	0.002	−0.037**	0.001
	(0.21)	(0.71)	(0.02)	(0.94)
$\tau - 1$	0.014	0.001	−0.027*	0.015
	(0.59)	(0.87)	(0.09)	(0.43)
$\tau + 0$	0.137***	0.011	−0.005	0.166***
	(0.01)	(0.50)	(0.87)	(0.00)
$\tau + 1$	0.391***	0.061***	0.141***	0.365***
	(0.00)	(0.00)	(0.00)	(0.00)
$\tau + 2$	0.633***	0.080***	0.162***	0.283***
	(0.00)	(0.00)	(0.00)	(0.00)
$\tau + 3$	0.423***	0.052***	0.103**	0.192***
	(0.00)	(0.01)	(0.02)	(0.00)
$\tau + 4$	0.264***	0.033	0.081*	0.108*
	(0.00)	(0.25)	(0.07)	(0.07)
$\tau + 5$	0.118	0.031	0.111**	0.056
	(0.11)	(0.25)	(0.02)	(0.36)

Notes: This table shows mean effects of the construction of HPPs on each outome, as estimated by the synthetic control method. The method is described in section 4 and 5.3. . Differently from the main estimates, for this table, municipalities within the same microrregion of affected units were excluded from the donor pool. p-values calculated using permutation tests, as decribed in section 4, are shown in paretheses.

Table A6: Mean Effects - no contamined microrregions

	Employment in Construction (ihs)	Employment in Commerce and services(ihs)	Employment in Manufacture(ihs)	Employment in Agriculture(ihs)
$\tau - 4$	-0.025	0.006	-0.019	-0.052
	(0.75)	(0.73)	(0.61)	(0.46)
$\tau - 3$	0.033	$-0.045***$	0.009	0.008
	(0.65)	(0.01)	(0.81)	(0.85)
$\tau - 2$	-0.013	0.010	-0.038	0.038
	(0.82)	(0.50)	(0.24)	(0.39)
$\tau - 1$	0.053	0.001	0.003	$0.102**$
	(0.42)	(0.94)	(0.92)	(0.05)
$\tau + 0$	$0.563***$	$0.065*$	0.019	0.073
	(0.00)	(0.09)	(0.80)	(0.57)
$\tau + 1$	$0.498***$	$0.134***$	$0.149*$	0.099
	(0.01)	(0.00)	(0.10)	(0.51)
$\tau + 2$	0.126	$0.086*$	0.174	0.131
	(0.57)	(0.10)	(0.11)	(0.44)
$\tau + 3$	-0.042	0.024	0.186	0.035
	(0.85)	(0.63)	(0.12)	(0.89)
$\tau + 4$	-0.120	-0.013	0.052	0.033
	(0.56)	(0.79)	(0.68)	(0.90)
$\tau + 5$	-0.224	-0.083	0.083	0.045
	(0.33)	(0.12)	(0.53)	(0.90)

Notes: This table shows mean effects of the construction of HPPs on each outome, as estimated by the synthetic control method. The method is described in section 4 and 5.3. . Differently from the main estimates, for this table, municipalities within the same microrregion of affected units were excluded from the donor pool. p-values calculated using permutation tests, as decribed in section 4, are shown in paretheses.

Table A7: Mean Effects, synthetic control - two years lag

	Population(log)	GDP growth	GDP (log)	GDP per capita (log)
$\tau-4$	-0.001	0.006	-0.002	0.004
	(0.97)	(0.18)	(0.88)	(0.51)
$\tau-3$	-0.001	$-0.006*$	-0.002	0.002
	(0.95)	(0.10)	(0.89)	(0.60)
$\tau-2$	0.000	0.022	0.025	0.015
	(0.99)	(0.28)	(0.18)	(0.28)
$\tau-1$	-0.017	$-0.039**$	0.013	0.017
	(0.21)	(0.03)	(0.65)	(0.40)
$\tau+0$	-0.018	0.023	0.032	$0.038*$
	(0.20)	(0.20)	(0.22)	(0.09)
$\tau+1$	-0.006	$0.082***$	$0.098***$	$0.092***$
	(0.66)	(0.00)	(0.00)	(0.00)
$\tau+2$	-0.014	$0.034*$	$0.116***$	$0.107***$
	(0.34)	(0.10)	(0.00)	(0.00)
$\tau+3$	-0.004	$-0.056***$	$0.095***$	$0.068***$
	(0.84)	(0.01)	(0.00)	(0.01)
$\tau+4$	-0.004	$-0.090***$	0.026	0.005
	(0.82)	(0.00)	(0.40)	(0.91)
$\tau+5$	-0.005	0.017	0.025	0.016
	(0.80)	(0.32)	(0.49)	(0.70)

Notes: This table shows mean effects of the construction of HPPs on each outome, as estimated by the synthetic control method. The method is described in section 4 and 5.3. Differently from the main estimates, the ones shown in this table do not use all pre-intervention outcomes to choose the best synthetic control. In this case, the two last pre-intervention years are excluded. p-values calculated using permutation tests, as decribed in section 4, are shown in paretheses.

Table A8: Mean Effects, synthetic control - two years lag

	Tax Revenue (log)	Current Revenue (log)	Lights (log)	Formal Employment (ihs)
$\tau - 4$	0.031*	−0.000	−0.001	−0.007
	(0.09)	(0.98)	(0.94)	(0.60)
$\tau - 3$	0.002	−0.001	0.001	−0.022*
	(0.89)	(0.90)	(0.87)	(0.06)
$\tau - 2$	−0.035	0.001	−0.092**	0.012
	(0.69)	(0.92)	(0.02)	(0.76)
$\tau - 1$	0.096	0.011	−0.066*	0.006
	(0.16)	(0.48)	(0.10)	(0.93)
$\tau + 0$	0.216***	0.024	−0.016	0.193***
	(0.00)	(0.19)	(0.72)	(0.00)
$\tau + 1$	0.462***	0.071***	0.130***	0.363***
	(0.00)	(0.00)	(0.00)	(0.00)
$\tau + 2$	0.721***	0.100***	0.153***	0.314***
	(0.00)	(0.00)	(0.00)	(0.00)
$\tau + 3$	0.524***	0.074***	0.110*	0.230***
	(0.00)	(0.00)	(0.06)	(0.00)
$\tau + 4$	0.327***	0.058**	0.062	0.128***
	(0.00)	(0.02)	(0.47)	(0.08)
$\tau + 5$	0.177**	0.054**	0.071	0.101
	(0.02)	(0.02)	(0.49)	(0.23)

Notes: This table shows mean effects of the construction of HPPs on each outome, as estimated by the synthetic control method. The method is described in section 4 and 5.3. Differently from the main estimates, the ones shown in this table do not use all pre-intervention outcomes to choose the best synthetic control. In this case, the two last pre-intervention years are excluded. p-values calculated using permutation tests, as decribed in section 4, are shown in paretheses.

Table A9: Mean Effects, synthetic control - two years lag

	Employment in Construction (ihs)	Employment in Commerce and services(ihs)	Employment in Manufacture(ihs)	Employment in Agriculture(ihs)
$\tau - 4$	-0.028	-0.001	-0.013	0.017
	(0.28)	(0.94)	(0.48)	(0.46)
$\tau - 3$	0.036*	$-0.027**$	0.011	0.017
	(0.09)	(0.05)	(0.45)	(0.38)
$\tau - 2$	0.023	0.005	-0.068	0.047
	(0.81)	(0.91)	(0.38)	(0.57)
$\tau - 1$	0.049	0.004	0.006	0.088
	(0.67)	(0.92)	(0.96)	(0.48)
$\tau + 0$	0.693***	0.069	0.118	0.042
	(0.00)	(0.25)	(0.32)	(0.73)
$\tau + 1$	0.879***	0.108	0.286***	0.201
	(0.00)	(0.16)	(0.01)	(0.13)
$\tau + 2$	0.627***	0.131**	0.308***	0.210
	(0.00)	(0.04)	(0.01)	(0.15)
$\tau + 3$	0.181	0.103*	0.244*	0.256
	(0.28)	(0.10)	(0.08)	(0.11)
$\tau + 4$	0.175	0.043	0.181	0.205
	(0.29)	(0.55)	(0.26)	(0.24)
$\tau + 5$	0.132	0.003	0.062	0.259
	(0.44)	(0.97)	(0.73)	(0.19)

Notes: This table shows mean effects of the construction of HPPs on each outome, as estimated by the synthetic control method. The method is described in section 4 and 5.3. Differently from the main estimates, the ones shown in this table do not use all pre-intervention outcomes to choose the best synthetic control. In this case, the two last pre-intervention years are excluded. p-values calculated using permutation tests, as decribed in section 4, are shown in paretheses.

Table A10: Mean Effects, synthetic control

	Price Index Variation
$\tau - 4$	-0.000
	(0.93)
$\tau - 3$	-0.002
	(0.82)
$\tau - 2$	-0.006
	(0.51)
$\tau - 1$	-0.003
	(0.73)
$\tau + 0$	0.009
	(0.89)
$\tau + 1$	-0.074
	(0.40)
$\tau + 2$	0.027
	(0.80)
$\tau + 3$	-0.010
	(0.88)
$\tau + 4$	0.109
	(0.14)

Notes: This table shows mean effects of the construction of HPPs on each outome, as estimated by the synthetic control method and differences-in-differences. The method is described in section 4. The synthetic control point estimates are identical to the ones showed by FigureA7. p-values calculated using permutation tests, as decribed in section 4, are shown in paretheses.