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Local Economic Impacts of Hydroelectric Power Plants: Evidence from Brazil

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ABSTRACT

This paper evaluates the short- and medium-run effects of large hydroelectric power plant (HPP) construction on the economic development of Brazilian municipalities. Using the synthetic control method, we estimate the effects for 82 municipalities affected by HPP construction between 2002 and 2011. The average impact follows an inverted U-shape over a five-year horizon. The median impact in the medium run is positive but modest, with no effect on the composition of local economic activity. However, the estimated effects are highly heterogeneous. Five years after construction, the median impact on GDP growth rate is close to zero, while the 75th percentile of the treatment effects distribution is a 6.7 percentage points increase, and the 25th percentile is a 7.9 percentage points decrease. These results do not support the argument that large construction works unequivocally spur local development.

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1 INTRODUCTION

Since 2000, more than one thousand hydroelectric power plants (HPPs) have been built around the world (ICOLD 2016). This has expanded the capacity for electricity generation by 427 GW, equivalent to the combined energy production capacity of France and India (EIA 2012). Installed hydropower capacity is expected to double in the next three decades, with most of this expansion occurring in low- and middle-income countries that hold most of the world's untapped hydropower potential (EIA 2012).

The construction of large hydroelectric power plants (HPPs) has long been a subject of global controversy. Although proponents argue that HPPs provide renewable energy and stimulate economic development, critics highlight their social and environmental costs, uneven economic benefits, and limited long-term impact on local economies (WCD 2000; Ansar et al. 2014). For example, these concerns led to a shift in global financing policies, with the World Bank significantly scaling back HPP funding in developing countries in the late 1990s following the establishment of the World Commission on Dams in 1997.

Since then, ex-ante impact assessments that weigh the socioeconomic and environmental costs and benefits of building HPPs in specific locations have played an increasingly prominent role in infrastructure development (Laurance and Arrea 2017; Westin, dos Santos and Martins 2014; Fearnside 2014; Kirchherr, Ahrenshop and Charles 2019). In fact, more than 100 countries use some form of ex-ante impact assessment in their decision-making process (Jay et al. 2007). However, there is limited evidence documenting the realized external costs and benefits of building an HPP, mainly due to the challenge of constructing counterfactual scenarios showing what would have occurred if the HPP was not built.

Beyond energy generation, these large infrastructure projects are often believed to foster local development, contributing to increased income, jobs, and tax revenues for local governments (Bourguignon et al. 2008). A growing body of literature suggests that dams and infrastructure more broadly can have localized impacts by promoting agglomeration (Greenstone, Hornbeck and Moretti 2010; Severnini 2023), productivity gains (Duflo and Pande 2007), and urbanization (Strobl and Strobl 2011). The idea that public works can spur local economies dates back at least to the American New Deal of the 1930s (Billington and Jackson 2017).

In this paper, we explore the hypothesis that large infrastructure works can serve as a policy tool to stimulate local economic activity.[1] To do so, we assess the effects of the construction works of 29 HPPs between 2002 and 2011 on the economies of 82 Brazilian municipalities. Specifically, we utilize four key annual indicators from complementary data sources: employment, tax revenues, night lights measured by remote sensing, and municipal gross domestic product (GDP). These indicators capture different aspects of the local economies. We then employ the synthetic control method (Abadie, Diamond and Hainmueller 2010; Abadie and Gardeazabal 2003), a quasi-experimental approach increasingly popular among social and conservation scientists (Baylis et al. 2016; Sills et al. 2015; Cavallo et al. 2013; Saunders et al. 2015). This method helps estimate the dynamic effects from the beginning of HPP construction for each of the 82 affected municipalities over five years. In essence, our analysis constructs 82 comparable case studies to uncover the *distribution* of treatment effects over time.

It is important to emphasize that our analysis captures the local impacts of the construction process itself, distinct from the effects of electrification (Lipscomb, Mobarak and Barham 2013) or the combined effects of both (Severnini 2023; de Faria et al. 2017). The municipalities in our sample had high rates of electricity penetration even before the construction works started. Therefore, any improvements in the distribution network that might have accompanied the new HPPs would be marginal. Furthermore, investments in new HPPs and transmission lines did not extend the coverage of the electricity grid during our sample period. Instead, they augmented the generation

1 Our focus is on the economic impact of these new infrastructure projects. We acknowledge significant social and environmental costs associated with these projects, including displacement of the local population and poorly planned urban expansion (Kemenes, Forsberg and Melack 2011; Sovacool and Bulan 2011; Fearnside and Pueyo 2012; Benchimol and Peres 2015; Fearnside 2016; Costa, Szerman and Assunção 2025).

capacity and resilience of the interconnected transmission network. Consequently, proximity to a power plant does not necessarily augment energy availability. In contrast, municipalities hosting an HPP are significantly more affected by construction works than municipalities farther away from the site.

We find that the average effects of the construction of HPPs on the local economy follow an inverted U-shape over five years. For example, the average effect on local tax revenues is 62 percent two years after construction starts but falls 17 percentage points per year after that. By year five, municipalities that received an HPP have, on average, tax revenues 12 percent higher than their control counterparts. Median effects display less variation over time than average effects, and more modest than average for all years after construction starts.

To explore the possibility that the construction of an HPP has transformative (and thus potentially long-run) effects on the economy, we look at effects on employment composition across different sectors—construction, retail & services, manufacturing, and agriculture. Both average and median effects are essentially zero, indicating that, typically, such effects do not materialize. Considering that the construction period is typically four years, we conclude that the stimuli on local economies are short-lived.

However, we find that the impacts of the construction of HPPs on local economies are very heterogeneous. For all the indicators studied, the effects on some municipalities are much more severe than in others, both positively and negatively. For example, while the median impact of an HPP construction on GDP growth rate is close to zero five years after its construction, the 75th percentile of the treatment effects distribution is a *positive* 6.7 percentage points, while the 25th percentile is a *negative* 7.9 percentage points. In other words, for 20 of the municipalities in our sample frame, the respective HPPs had a strong and positive effect, while for another 20 municipalities, this effect was negative and large.

In an attempt to better understand the determinants of treatment heterogeneity, we performed a meta-analysis, correlating the estimated effects with the baseline characteristics of municipalities and HPPs. Although we find some evidence that the investment size of the HPP relates positively to the GDP and the GDP per capita effects, we do not find any conclusive evidence of baseline characteristics that can predict the local economic impact of HPP construction.

This paper complements the literature on the socioeconomic impact of large infrastructure projects (Greenstone, Hornbeck and Moretti 2010; Moretti 2010; Davis 2011; Kline and Moretti 2014; Ansar et al. 2014; Feyrer, Mansur and Sacerdote 2017) and local resources boom (e.g., Aragón and Rud 2016; Allcott and Keniston 2018). de Faria et al. (2017) estimates the average effects of electrification and hydropower projects on local socioeconomic indicators in Brazil between 1991 and 2010. They find that, on *average*, dam constructions lead to short-lived economic booms and negligible improvements in socio-economic conditions in local economies, which leads them to question the long-term justification for hydropower projects. In contrast, our main contribution is to estimate the *distribution* of medium-run economic impacts from recently constructed hydroelectric power plants using current technology and under present-day regulatory standards in a middle-income country context. Like de Faria et al. (2017), all our measures of economic activity indicate that the local economic effects of building an HPP are, at the mean and median, short-lived and ex-ante unknown. Thus, our findings also do not support the argument used by construction advocates that these projects will unequivocally spur a small economic miracle in the area. In contrast, our analysis highlights the variability in treatment effects among municipalities, suggesting that while the average impacts may be short-lived, some areas may experience significant positive (or negative) effects. This heterogeneity we document suggests that there are considerable opportunities and risks associated with new infrastructure concerning local economies. Case-by-case assessments are therefore fundamental to screening socially desirable projects.

This paper proceeds as follows. Sections 2 and 3 provide background information and present our data. Section 4 details our empirical strategy. We discuss the results in Section 5 and briefly conclude in Section 6.

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2 BACKGROUND

Brazil is heavily dependent on hydroelectric power. Since the 1960s, the country's electrification has been based mainly on the construction of large HPP projects such as the Itaipu Dam, which is the HPP that produces the most energy in the world after China's Three Georges Dam. Brazil largely electrified the country in the following four decades, which contributed to the development of the interior of the country (Lipscomb, Mobarak and Barham 2013). By 2003, Brazil had 139 medium and large HPPs, representing about 70 GW of installed capacity to generate electricity. According to the Demographic Census, in 2000, 93.5 percent of households in the country were connected to electricity.

Following the country's energy crisis in 2001 (Costa and Gerard 2021), the federal administration invested in expanding the country's generation capacity and transmission lines. Public subsidies targeted at large infrastructure projects led to 67 new medium and large HPPs, adding 21 GW of capacity to the national electric system—a 30 percent increase (ANEEL 2016). With an agenda centered on economic development and industrialization, the government pushed these projects forward despite potential environmental concerns and minorities' rights surrounding these projects (Fearnside and Pueyo 2012; Benchimol and Peres 2015; Costa, Szerman and Assunção 2025). It is important to highlight that at the outset of this expansion, most of the country's population was already under the grid and that the main objective of these new HPPs was to increase the generation capacity and resilience of the national grid.[2]

In addition to increasing generation capacity, the new HPPs can also have significant socioeconomic impacts on affected communities through their labor intensive construction process. HPPs are typically built in rural, sparsely populated areas suitable for hydrological conditions, and the construction process attracts investments and workers to surrounding municipalities. The average construction costs R$650 million (in constant 2000 prices) and creates 3,700 direct jobs during a four-year window (average construction time).[3] To give a sense of the scale of these enterprises, in 2000 the average GDP of municipalities eventually affected by an HPP during the next decade was R$56 million, and the number of (formal) workers in jobs was 3,368.

In sum, this influx of people and resources can create a boom in the local economy during the construction period. The onset of the construction of an HPP can therefore have large impacts on the economy in the short term. If the magnitudes of these short-run effects are large, construction may lead to urbanization and structural transformation in the local economy. In addition, HPPs can increase municipal tax revenues, which, in turn, can be used to support services that are important for business and economic growth. More long-lasting effects are also possible, and our goal in this study is to provide estimates of the local economic effects of the construction of new HPPs in the short and medium run.

3 SAMPLE AND DATA

3.1 SAMPLE FRAME

In Brazil, hydroelectric power plants are classified by installed capacity. According to the Brazilian Electricity Regulatory Agency (*Agência Nacional de Energia Elétrica*, ANEEL), medium-sized HPPs typically have an installed capacity between 30 MW and 300 MW, while large HPPs exceed 300 MW. This classification aligns with international standards, which define small HPPs as those below 30 MW (Bourguignon et al. 2008). In this study, we focus on medium and large HPPs constructed between 2002 and 2011.

Our unit of analysis is a Brazilian municipality (*município*). We define treated municipalities (i.e. those directly affected by an HPP construction) as those (i) with any part of their territory flooded by an HPP reservoir whose construction began between 2002 and 2011, and (ii) not subject to floods by

2 Bringing electricity to the minority of households without access to the grid was a distribution, or "last mile" problem. These households became the target of the Light For All program (*Programa Luz Para Todos*).

3 Values deflated using Consumer Price Index (IPCA).

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more than one dam. The first selection criterion is necessary because of data availability—we need data before and after the start of the construction process of each HPP. We exclude municipalities affected by more than one HPP or impacted outside of our study period to avoid confounding effects and ensure internal validity.

ANEEL provides, for each HPP, the list of municipalities that had an area flooded by the HPP's reservoir (ANEEL 2018). As of 2013, there were 718 municipalities affected by HPPs. Among them, 599 municipalities were affected outside of our sample period, with a further 37 municipalities affected by more than one HPP. We excluded these 636 municipalities from our sample, analyzing 82 municipalities. Hence, we end up analyzing 82 municipalities affected by 29 HPPs. These municipalities that are affected by 29 HPPs are distributed in 13 states throughout Brazil, with a total of 4,000 km^2 of flooded area. The 2,923 municipalities in the states that were never affected by an HPP form a pool of potential control cases, which will be used to construct a synthetic control case (see Section 4).

Our study focuses on constructions that started between 2002–2011, when Brazil rapidly expanded hydroelectric generation following the 2001 energy crisis (Costa and Gerard 2021). While these projects were crucial for stabilizing the national grid, they were often prioritized based on utility needs rather than local economic planning. This context may help explain why we find short-lived economic booms without sustained local development. We stopped our sample in 2011 to avoid the potential confounding effects of the 2012 New Forest Code, which introduced significant changes to environmental regulations affecting large infrastructure projects. Moreover, Brazil entered a period of economic and political turmoil for the rest of the decade, reducing the number of new projects after 2011, in particular, comparable new projects, for a post-2011 analysis.

3.2 DATA

We assembled a comprehensive and complementary set of yearly indicators to measure economic activity. Yearly data has advantages and limitations. The main advantage is that it can capture short-term, abrupt changes to economic activity. The main limitation is that the time-series dimension of these data is usually shorter than, say, that of decennial population censuses. Table 1 show the descriptive statistics.

Employment. We use confidential data from the *Relação Anual de Informações Sociais* (RAIS) provided by the Ministry of Labor (*Ministério do Trabalho*). RAIS can be seen as a yearly census tracking information on all Brazilian registered firms. Every year, all registered firms that employ at least one person are obliged to provide data concerning their workforce, creating an

	DONOR POOL		TREATED MUNICIPALITIES	
	AVERAGE	STANDARD DEVIATION	AVERAGE	STANDARD DEVIATION
Municipal GDP Growth (%)	12.0	15.4	10.2	9.5
Municipal Tax Revenues (million R$)	14.5	110.3	21.5	72.0
Average Pixel Luminosity (DMSP-OLS)	2.0	5.1	1.5	2.3
Employment (RAIS)				
Total Employment	1,900	14,887	3,367	10,045
Employment in Construction	130	1,584	175	613
Employment in Services	1,034	9,922	1,864	6,252
Employment in Manufacturing	566	2,722	988	2,593
Employment in Agriculture	46	170	136	459
Population (1,000 people)	23.0	88.0	36.2	85.3
Urban population (% of total population)	84.2	16.7	82.2	18.6
Households with Electricity (% of total households)	56.1	23.0	53.6	24.0
Number of Municipalities	2,923		82	

Table 1 Summary statistics in 2000.

Notes: Monetary values deflated to 2014 constant prices.

employer-employee-matched dataset. Importantly to our study, we have, for each firm, the number of employees on December 31st of each year, the firm's location, and its sector of activity. We focus exclusively on for-profit juridical entities. We aggregate these data to construct a time series of employment at the end of the calendar year for each municipality and sector (Construction, Retail & Services, Manufacturing, and Agriculture).

The main advantage of RAIS is that it provides high-frequency and accurate data on employment for all registered firms. The main limitation, however, is that it only captures employment at registered firms. Brazil, like many middle- and low-income countries, has a sizable informal sector, where firms and workers are not registered (Ulyssea 2018, 2020). One important implication is that results based on these data may capture either changes in employment or changes in the formal/informal composition of employment.

Tax revenues. The National Treasure Secretary (*Secretaria do Tesouro Nacional*) compiles annual data on municipal finances, known as FINBRA (Secretaria do Tesouro Nacional 2013). They include tax revenues collected by each municipality, for every year in the period 1989–2012. Brazilian municipalities collect several taxes, mainly property taxes and taxes on locally provided services. We deflate all monetary values using Brazil's general price index (IGP-M).

Night Lights. We also use measures of night lights as a proxy to economic activity, an increasingly popular approach employed by economists (Chen and Nordhaus 2011; Henderson, Storeygard and Weil 2011), for being able to capture both short-term economic fluctuations as well as long-run growth trends (Henderson, Storeygard and Weil 2012), making it especially appealing to our application. Specifically, we use data derived from the Operational Linescan System sensors of the United States Air Force Defense Meteorological Satellite Program (DMSP-OLS), Version 4. The National Oceanic and Atmospheric Administration (NOAA) processes the satellite images and publishes annual cloud-free composites in 30 arc second grids. We use the stable lights composites, which exclude background noise and ephemeral events, such as fires. The DMSP-OLS gives, for each pixel, data values ranging from 0 (no lights) to 63 (maximal luminosity). We aggregate pixels using geo-referenced municipality boundaries, taking the average pixel value.

Municipal GDP growth. The *Instituto Brasileiro de Geografia e Estatística* (IBGE), the country's official statistical agency, provides estimates of the Gross Internal Product of Brazilian municipalities for the period 1999–2012. We use these data to construct GDP growth for each municipality.

4 METHODS

Assessing the impact of the construction of hydroelectric power plants 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 power plant not been constructed. The decision to build an 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 on simple comparisons of outcomes between these two groups to identify HPPs' causal effects may lead to biased results.

We employ the synthetic control method (Abadie, Diamond and Hainmueller 2010; Abadie and Gardeazabal 2003) to overcome this challenge. The central idea of the method is to use a weighted average of municipalities without an HPP (the *donor pool*) to construct a *synthetic* municipality that serves as the best counterfactual for the municipality that received a new HPP (treated municipality). Weights are found using a data-driven approach that seeks to minimize any pre-HPP differences in the outcome variable of interest between the treated municipality and its synthetic counterpart. The ideal synthetic unit is identical to the treated unit in the pre-construction periods. The effect of the HPP construction will be the simple difference between the observed outcome of the studied municipality and the outcome of its synthetic counterfactual municipality.

When implementing this method, researchers face at least two important choices. The first choice concerns the selection of units to form the donor pool. For each treated municipality, we restrict

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the donor pool to municipalities that comply with the following criteria: (i) not having area flooded by HPP at any point in 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 from the treated unit with respect to the variable of interest in the treated municipality's state in year 2000.[4] These restrictions make the donor pool and treated municipalities more homogeneous and decrease the computational burden of having too large a donor pool.

Table S1 shows descriptive statistics on the number of eligible donor municipalities in the control pool after applying our selection criteria for each dependent variable. On average, there were 258 eligible donor municipalities in each analysis. We find that donor pool sizes vary across regions, with municipalities in Minas Gerais having more suitable controls than those in Mato Grosso. This suggests that synthetic control estimates for certain areas may rely on a smaller set of comparisons, potentially increasing variance in those cases.

The second important choice is to define the variables used to form the synthetic control unit. We use only the variable of interest (e.g., employment) in all periods of the pre-construction phase. There is a growing consensus (Botosaru and Ferman 2019) that this approach minimizes potential biases and increases the transparency of the method by reducing "cherry picking" (Ferman, Pinto and Possebom 2020). In any case, when one uses the pre-intervention data on the variable of interest, other matching variables have no practical effect on the construction of the synthetic unit. Finally, keeping implementation choices constant across all 82 case studies increases their comparability.

Next, we detail all technical specifications and necessary information for replication.

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—if the HPP had not been constructed—the counterfactual outcome of j in periods $t \leq T_0$ would be y_{jt}^C. 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 $t \leq T_0$:

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

Since we do not observe y_{1t}^C, we estimate it using synthetic control. Intuitively, our empirical strategy consists of 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 $\hat{y}_{1t}^C$ using a weighted average of the J non-affected municipalities. Formally, a potential synthetic control is defined by a weight vector $W = (w_1, w_2, \dots, w_{J_i})'$, 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:

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

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

4 For some treated municipalities, this rule resulted in no convergence of the optimization algorithm. In these cases, we relaxed this condition.

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 might be 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, municipalities with such different GDPs per capita may face different shocks after the intervention. Therefore, to make our approach more reliable, we limit our donor pool to units similar to the treated one.

More specifically, for estimating the 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 an area flooded by HPP at 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 from the treated unit in respect to the variable of interest in the treated municipality's state.[5]

The interpretation that the HPP causes all the differences 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 of HPP construction. Otherwise, the synthetic control would continue to follow the treated unit and would not show any divergence with it. In our case, the concern with idiosyncratic shocks is diminished because we repeat this exercise for each of the 82 affected municipalities.

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' effects separately, we index each separate event by $g = 1, 2, \dots G$, 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. To better compare the treatment that occurred in different years, we normalize years by $\tau = t - T_{0g}$ such that $\tau = 0$ is the year of HPP construction.

Second, let $\hat{\alpha}_{j\tau g} = y_{j\tau g} - \hat{y}_{j\tau g}^C$ 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.

5 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 to not include 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.

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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 nature of synthetic control estimators requires 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 comes 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 converges to a normal. To overcome this problem, we rely on permutation tests to conduct inference. The use of permutation tests for synthetic control inference was first proposed by Abadie, Diamond and Hainmueller (2010). In their paper, the objective was to estimate and make inferences about the effect of an intervention in a single unit. Permutation tests were considered attractive because they do not rely on 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.[6]

For example, to conduct inference for just one treatment, we could use the synthetic control method for both treated and non-treated units, j. Then, compute $\hat{\alpha}_{jt}$ for each one of them as our test statistic. For a bilateral test,[7] 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 randomly reassign the "treatment label" among the donor units. The value is useful as

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

7 Naturally, one can make unilateral tests by simply ranking the actual value of the test statistics instead of their absolute value.

evidence of the null hypothesis of no treatment effect. If the treatment was randomly assigned, this probability has the interpretation of traditional bilateral p-value, that is, the probability that the measured result would be obtained if the treatment has no effect.

When the treatment is not randomly assigned, two main problems threaten 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 below) its synthetic counterpart even before the intervention. If this bias continues after the intervention, the probability of the unit presenting an "extreme" result will be inflated, becoming higher than its p-value. The second problem is the 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 $y_{jt} - \hat{y}^C_{jt}$ as our measure of how extreme a result is, if the treated unit has shocks with higher variance than the not treated units, we will underestimate the p-value. If its shocks have a 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, Diamond and Hainmueller 2010; Ferman and Pinto 2019). The first is simply to exclude from the suggested comparisons units that have poor pre-intervention fit.[8] The second is to use an adjusted effect that accounts for different predictive precisions as a test statistic. The goodness of fit can be measured by the following variable:

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

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 several times bigger than the treated unit's RMSPE, $RMSPE_1$. Alternatively, we can obviate 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) extend the methods outlined in the previous section, allowing inference for synthetic control to be used when there is more than one treated unit. In their framework, the method estimates the treatment's average impact and its p-value. To compute a p-value, Cavallo et al. (2013) propose the creation of placebo averages and permutation tests analogous to those used in the case of one treated unit. We follow their procedure, which consists of 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 untreated 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 an RMSPE less than ten times higher than the treated unit's RMSPE.

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

3. Compute possible placebo averages of the chosen test statistic by picking a single placebo estimate corresponding to each disaster g. Then take 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 an 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 larger (at the order of 10^{179}), we instead choose a random sample of combinations 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

We now present and discuss the results obtained from applying the method detailed in section 4 to the data described in section 3. For each indicator, we center the results around the year that the HPP construction is initiated (τ = 0). Due to data limitation and to maintain the comparability of the results by not excluding many observations, we only present the results for 5 years prior to 5 years after the beginning of the construction of the HPPs. For each year, we plot average effects for the 82 municipalities, along with the 25th, 50th, and 75th percentiles of the effect distribution.

5.1 LOCAL ECONOMIC ACTIVITY

Figure 1a shows the evolution of the effects of HPP construction on the (log) number of employed workers, revealing three patterns. First, average effects display a pattern of growth reversion. Two

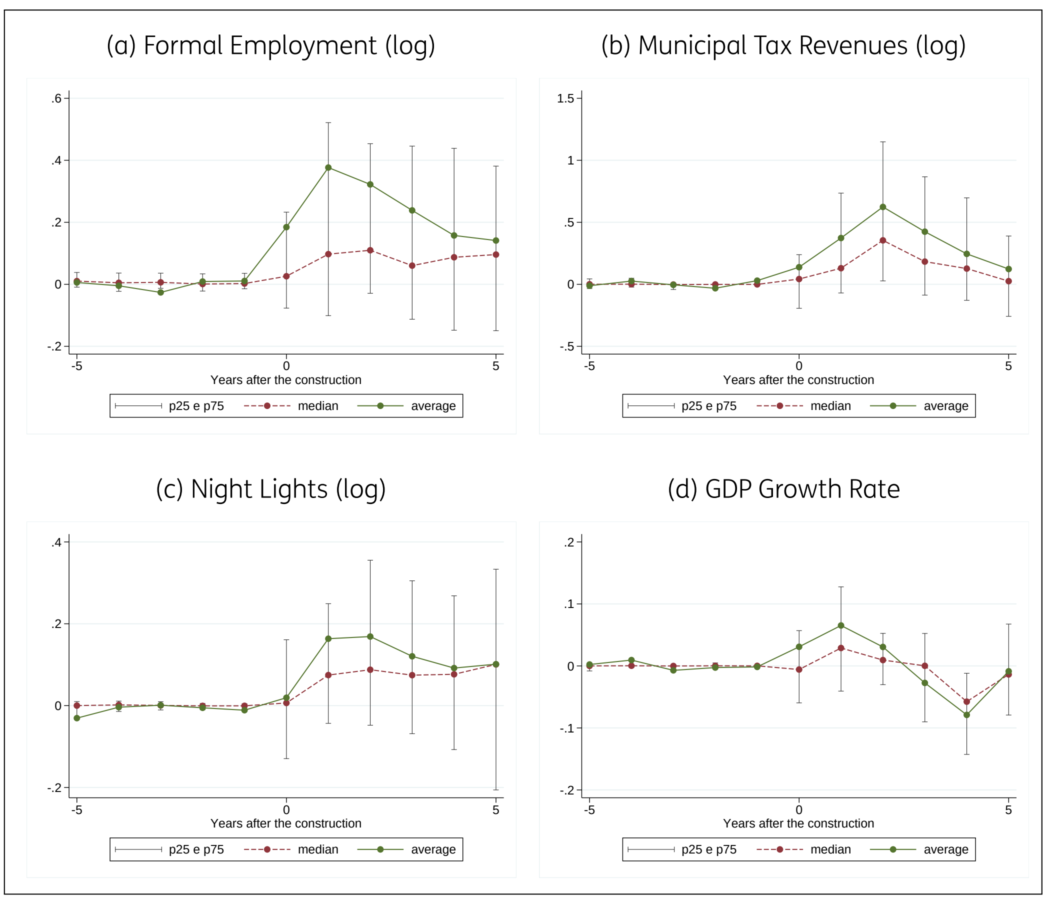

Figure 1 Effects on Indicators of Economic Activity.

Each panel shows the average, median, 25th and 75th percentiles of treatment effects. τ = 0 is normalized to equal the year of the HPP construction initiation. The number of treatment effects over which the average, median, and quartiles are calculated varies across normalized years, from a maximum of 82 to a minimum of 60.

years after construction starts, employment grows by 32 percent in comparison to the synthetic control units; but this figure drops to 14 percent five years after construction starts. Secondly, median effects are more stable and modest throughout the five years after the start of construction, oscillating around 10 percent. The contrast between average and median effects reflects a skewed distribution of effects, or the presence of "outliers", which raise the average. In fact, the third pattern in this figure is that the distribution of effects displays a considerable dispersion: while 25 percent of the effects are larger than 50 percent, another 25 percent are negative, implying that some municipalities grow less than their synthetic control counterparts as a result of an HPP being constructed.

Next, Figure 1b shows the results for municipal tax revenues. The inverted U pattern is even more pronounced than that found for employment and also applies to median effects. The average (resp. median) effect two years after construction starts is an increased 62 (resp. 45) percent. However, this figure drops to 12 (resp. 2.5) percent five years after construction starts. Moreover, at any given point in time, the effects are also widely dispersed. For example, one-quarter of municipalities experience more than 100 percent increases in their tax revenues two years after construction, while another quarter sees their tax revenues decrease when compared to the counterfactual scenario.

We also use remote sensing of night lights as an alternative measure (Henderson, Storeygard and Weil 2012) of economic activity. Figure 1c shows the results when we use the log of the average digital number (light intensity) in each municipality. We observe a similar pattern as before: the average effects are more pronounced in the first two years after construction and display some reversion on the five-year horizon. Median effects are stable and positive, similar to employment effects.

Finally, in Figure 1d we look at the effects of the construction of HPPs on the growth rate of the municipal GDP. Average effects indicate an inverted U-cycle of growth-retraction-normalization. Average effects peak one year after construction starts at 6.5 percentage points when they start declining to a negative eight percentage points at year four. Five years after construction, both the average and the median effects are virtually zero.

In general, these results can be summarized as follows. The construction of HPPs tends to boost local economies. The average effects on a comprehensive set of indicators that measure economic activity are positive between two and three years after construction begins. On average, there are modest increments in economic activity when construction ends (year five). Employment is greater, lights are brighter, and municipalities collect more taxes. However, these are changes in levels. The growth rate of local economies is essentially unchanged. For all indicators, the distribution of effects displays considerable dispersion, with some municipalities benefiting and some losing.

5.2 STRUCTURAL TRANSFORMATION

The construction of an HPP may have transformative effects on the economy, which can potentially translate into economic growth after the five-year period that we analyze. Specifically, the short-run effects on the labor market may be accompanied by changes in the employment composition across different sectors. In that case, the effects on economic growth could materialize in the longer run as a new set of economic activities emerge.

To explore this possibility, in this section, we analyze the breakdown of employment by sector. Figure 2 presents results for Construction, Manufacturing, Retail and Services, and Agriculture. The average effects on employment in the construction sector (Figure 2a) are sizable, reaching 91 percent one year after construction starts. However, these effects display the same inverted-U pattern found for overall employment, and on year five the average effect is 17 percent (statistically, the average effect on year five is non-significant, as discussed below in section 5.3). Perhaps surprisingly, median effects are remarkably flat at zero percent and are even negative for years three and four. The discrepancy between the average and median effects is the result of large effects only for some municipalities.

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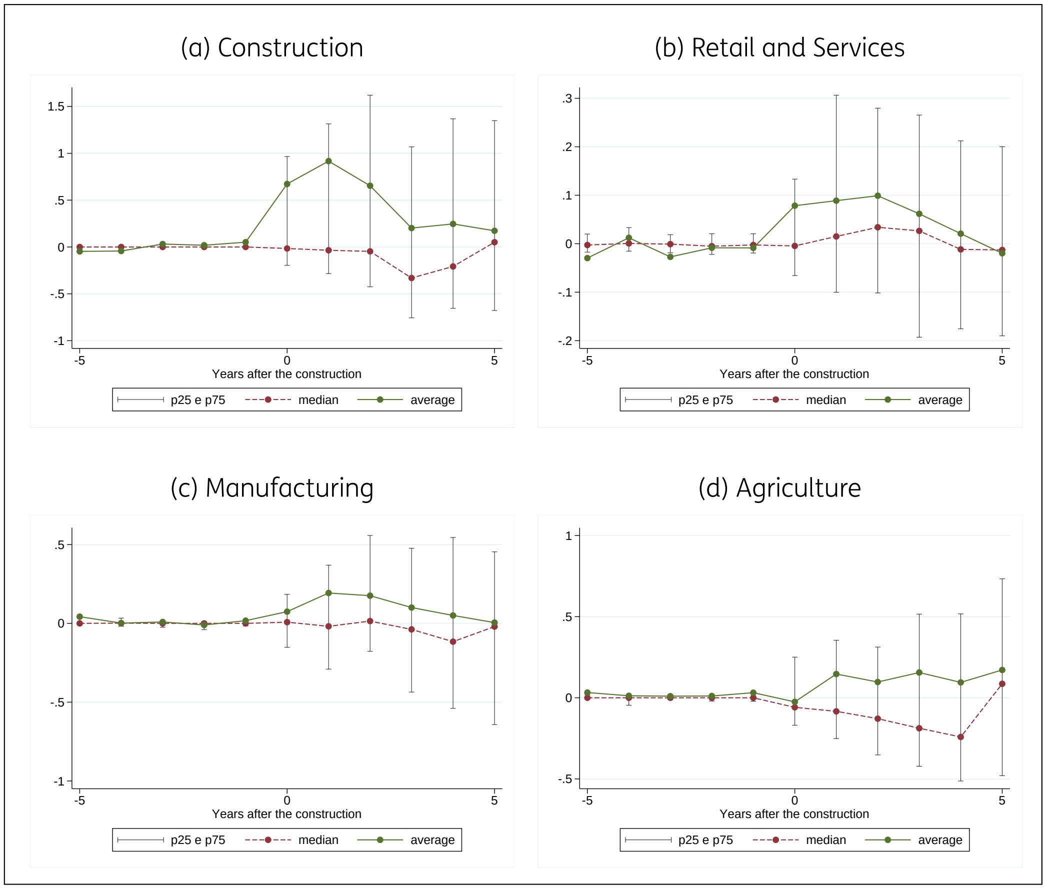

Figure 2 Effects on Employment Composition.

Each panel shows the average, median, 25th and 75th percentiles of treatment effects. $\tau = 0$ is normalized to equal the year of the HPP construction initiation. The number of treatment effects over which the average, median, and quartiles are calculated varies across normalized years, from a maximum of 82 to a minimum of 60.

Figures 2b–2d show the results for the remaining sectors. These figures show similar, but less pronounced, inverted-U patterns for average effects. The median effects are also flat at zero, except for the agricultural sector, where the median effects are declining with an abrupt reversion at year five. In general, we do not find any evidence that employment composition changes systematically. We conclude that the construction of an HPP triggers short-lived economic prosperity for the average municipality, but without creating enough momentum to lead to lasting effects or structural changes in the local economy more broadly.

5.3 STATISTICAL INFERENCE FOR AVERAGE EFFECTS

Using the methods discussed in the previous section, we perform statistical inference on average effects. Table 2 presents the results of conducting statistical inference on the average effect by year for each of the indicators studied in sections 5.1 and 5.2. The results confirm the overall conclusions of Figures 1 and 2.

5.4 SENSITIVITY TESTS

We perform sensitivity and placebo tests to the main specification presented in sections 5.1 and 5.2. As we show next, as a general rule, there is not much difference between them and the main estimates. Exceptions to this rule are rare and generally concern effects for which one of the estimates is not significant.

Local spillovers. HPP construction can generate spillovers in neighboring municipalities through labor migration and economic activity as workers commute and local businesses respond to increased demand for goods and services. We exclude neighboring municipalities from the donor pool in our synthetic control analysis to mitigate the potential bias from such effects. This is to alleviate concerns that such municipalities are contaminated by the construction of the HPP. Table S2 displays the results for the average effects. Our main results remain unchanged.

	FORMAL EMPLOYMENT (ihs)	TAX REVENUE (log)	LIGHTS (log)	GDP GROWTH	CONSTRUC-TION	RETAIL AND SERVICES	MANU-FACTURE	AGRI-CULTURE
$\tau - 4$	–0.005 (0.79)	0.026 (0.22)	–0.004 (0.78)	0.009 (0.11)	–0.043 (0.18)	0.012 (0.43)	0.002 (0.95)	0.012 (0.63)
$\tau - 3$	–0.026 (0.06)	–0.004 (0.80)	0.001 (0.92)	–0.007 (0.23)	0.032 (0.22)	–0.027 (0.06)	0.009 (0.67)	0.010 (0.64)
$\tau - 2$	0.009 (0.51)	–0.032 (0.05)	–0.005 (0.62)	–0.003 (0.54)	0.019 (0.44)	–0.008 (0.51)	–0.010 (0.57)	0.011 (0.58)
$\tau - 1$	0.011 (0.46)	0.029 (0.12)	–0.011 (0.29)	–0.002 (0.72)	0.051 (0.06)	–0.009 (0.47)	0.017 (0.34)	0.031 (0.17)
$\tau + 0$	0.184 (0.00)	0.138 (0.00)	0.019 (0.51)	0.031 (0.09)	0.673 (0.00)	0.078 (0.04)	0.074 (0.32)	–0.025 (0.75)
$\tau + 1$	0.377 (0.00)	0.373 (0.00)	0.164 (0.00)	0.065 (0.00)	0.916 (0.00)	0.089 (0.09)	0.193 (0.03)	0.146 (0.14)
$\tau + 2$	0.322 (0.00)	0.624 (0.00)	0.169 (0.00)	0.031 (0.07)	0.654 (0.00)	0.099 (0.03)	0.176 (0.09)	0.097 (0.41)
$\tau + 3$	0.238 (0.00)	0.424 (0.00)	0.121 (0.02)	–0.027 (0.06)	0.202 (0.21)	0.062 (0.14)	0.100 (0.38)	0.156 (0.27)
$\tau + 4$	0.158 (0.02)	0.245 (0.00)	0.092 (0.19)	–0.079 (0.00)	0.246 (0.12)	0.021 (0.68)	0.050 (0.69)	0.095 (0.55)
$\tau + 5$	0.141 (0.04)	0.123 (0.06)	0.101 (0.19)	–0.009 (0.65)	0.173 (0.29)	–0.020 (0.69)	0.005 (0.97)	0.172 (0.31)

Table 2 Average Treatment Effects with Inference.

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. The synthetic control point estimates are identical to the ones shown in Figures 1 and 2. p-values calculated using permutation tests are shown in parentheses.

In a more extreme version of this sensitivity test, we exclude all the municipalities within the same "micro-region" of a treated unit. Micro-regions are defined by IBGE, Brazil's official statistical office, and are composed, on average, of 10 contiguous municipalities that form a group that share common economic characteristics. Table S3 shows the results for the average effects when we exclude all municipalities within the same micro-region. Again, the main results remain unchanged.

Anticipatory effects. Large infrastructure projects often trigger economic changes before construction starts, as local businesses, labor markets, and municipal planning adjust in anticipation of future investments. Our analysis primarily focuses on the direct effects of construction, but we acknowledge that some municipalities may experience economic shifts before $\tau = 0$. We perform a placebo test to examine such potential anticipatory effects, which artificially brings forward the intervention date by two years. That is, for a municipality first affected by an HPP in 2007, we artificially changed the treatment date to 2005 and recalculated the synthetic control municipality. The aim is to see whether the synthetic control algorithm falsely captures treatment effects in the two years between the artificial and actual dates. We hope that this lag gives us a sense of the out-of-sample fit of synthetic control estimates when there is no treatment yet. Table S4 shows the results. Note that the table shows the "correct" treatment dates—that is, the $\tau - 0$ coefficient shows the estimated impact in the actual treatment year. We find no effect around $\tau - 2$ and $\tau - 1$, which suggests that while some municipalities experience early adjustments, there is no systematic pattern of pre-construction booms.

5.5 META ANALYSIS

Since we find that effects are very heterogeneous, with some localities being more affected than others, a natural question is whether one can identify ex-ante which regions would be more affected by the HPP construction. We try to provide some evidence in this regard by performing a meta-analysis. Given the limited number of municipalities affected by the construction of new HPPs, we lack statistical power, and the evidence in our exercises in this section remains suggestive.

To get as much variation as possible, we focus on the short-run effects of the first 3 years since the beginning of the construction of the HPP.[9] Since we have only 82 municipalities and potentially as many characteristics of the construction or municipality that could help predict the impact of the HPP construction on the local economy, we perform two exercises.

First, we run a Lasso selection model Belloni, Chernozhukov and Hansen (2014) to let the data indicate which covariates help explain our estimated treatment effect. We allow the model to select among 18 covariates, which are good predictors of the estimated effect for each outcome variable we investigate. We include variables measured at baseline (year 2000) capturing features of the HPP (total investment, flooded area, power capacity, and project estimates of number of direct jobs created), the local demographic (population, population density, and female to male ratio), the local economy (GDP per capita, municipal current revenue, municipal tax revenue, number of workers formally employed, and manufacturing share of the GDP), and dummy variables for each of the 5 country regions.

We show the results in Table 3 column 1. The Lasso model selected any of these variables for 9 of our outcome variables—"N.S." in the table. The only covariate selected to explain our HPP construction effects was the dummy for the Southeast region. According to this model, the construction of HPP in municipalities in the Southeast had a smaller effect on the growth of formal employment and the share of workers in the construction sector.

Second, we regress the average estimated effect of the HPP construction on each outcome studied on four covariates total investment, log GDP per capita, log population, and dummy for the Southeast region. Regressions include a constant. We show the results of this second exercise in Table 3 columns 2 to 5. Each line in the table corresponds to one regression on the four covariates in the columns. We

Table 3 Meta-Analiysis, 3-Years Average Effects.

This table displays the results of the meta-analysis based on our estimates from Section 5. The unit of observation is the municipality. Dependent variables indicated in each row are the average treatment effect in the first 3 years after the beginning of the construction. Column 1 presents the estimates of the regression using the covariate selected by the Lasso selection model (Belloni, Chernozhukov and Hansen 2014). "N.S." means no covariate was selected by the model. In the few cases a covariate was selected, the selected covariate was a dummy for the Southeast region. Results are presented in column 1 (N = 82). Columns 2 to 5 present the estimation results of regressing the outcome variable in each row in the four covariates in the columns and a constant (N = 79). Total investment in Million R$, GDP per capita, and Population in logs. Robust standard errors.
*** p < .01, ** p < .05, * p < .1.

	LASSO SELECTION SOUTHEAST DUMMY	COVARIATES			
		TOTAL INVESTMENT	GDP PER CAPITA	POPULATION	SOUTHEAST DUMMY
	(1)	(2)	(3)	(4)	(5)
GDP (Ln)	N.S.	.003** (.002)	–.067** (.03)	–.016 (.015)	–.089** (.04)
GDP p. c. (Ln)	N.S.	.004** (.002)	–.076*** (.027)	–.025* (.014)	–.113** (.051)
Population (Ln)	N.S.	0 (.001)	.006 (.014)	.006 (.007)	–.017 (.023)
Mun. Cur. Rev. (Ln)	N.S.	.01 (.008)	–.023 (.023)	–.015 (.013)	.017 (.032)
Mun. Tax Rev. (Ln)	N.S.	.043 (.027)	.056 (.126)	–.133* (.068)	.239 (.25)
Night Lights (Ln)	N.S.	.015 (.01)	–.079* (.041)	–.068** (.032)	.027 (.061)
Formal Empl.	–.423*** (.098)	.01 (.009)	–.254*** (.083)	–.023 (.049)	–.422*** (.118)
Empl. Construction (%)	–.108*** (.026)	.003 (.002)	–.016 (.032)	–.004 (.011)	–.098*** (.03)
Empl. Services (%)	N.S.	–.002 (.002)	.039 (.031)	–.01 (.013)	.093** (.041)
Empl. Manufacture (%)	N.S.	–.002* (.001)	–.035 (.026)	.012 (.011)	–.008 (.027)
Empl. Agriculture (%)	N.S.	.001 (.001)	–.02 (.018)	.002 (.006)	–.024 (.014)

9 In the Appendix we repeat the exercises using the 5-year window, but results are qualitatively the same.

observe that constructions with higher total investments seem to be positively related to effects on local GDP and GDP per capita (column 2, first two lines). Also, the HPPs' construction seems to have greater impacts in municipalities that are poorer at baseline, whenever we measure local impacts by GDP, GDP per capita, night lights, or formal employment (column 3). Despite these correlates, overall, we do not find any conclusive evidence of baseline characteristics that can predict the local economic impact of the construction of the HPPs.

6 DISCUSSION

We studied a type of construction project that is important in developing countries and that has raised a lot of public awareness: large hydroelectric power plants (HPPs). Evaluating the economic impacts of such projects is important for the understanding of their full costs and benefits. We find little support for the general argument that the construction of HPPs necessarily spurs local economic development. The effects on local economies are short-lived, and we find no evidence of structural transformation that could lead to qualitative changes in the economies in the long run, which is beyond the period we analyze.

Our findings align with global concerns about the economic impact of large hydroelectric projects. While HPP construction generates short-term economic booms, we find no evidence of long-term economic transformation in most affected municipalities. These results reinforce previous assessments by the World Commission on Dams (WCD 2000) and others (Ansar et al. 2014) that questioned whether large dams meaningfully benefit local communities. Given that our findings suggest that there are few overall economic benefits, policymakers should carefully weigh their expected benefits against potential long-term drawbacks.

It is important to note that our research design aims to isolate the effects of HPP construction. Thus, these conclusions may not apply to cases where HPPs increase the local provision of electricity or provide water regularization for downstream regions. Duflo and Pande (2007) document downstream agricultural productivity gains from water regularization after dam construction. However, these effects occur only after the reservoirs are filled and dams become operational—typically outside our study period, which focuses on the construction phase. Furthermore, unlike contexts where dams expand electricity access, our study examines regions where electrification was already near-universal (Severnini 2023), making energy access spillovers unlikely to drive our results.

However, the heterogeneity in treatment effects suggests considerable opportunities and risks associated with new infrastructure for local economies. Case-by-case assessments are therefore fundamental in the decision-making process. A better informed choice of sites could leverage the new energy infrastructure, which often has diffuse benefits, to benefit local economies. Furthermore, it remains an open question whether infrastructure built under different macroeconomic conditions—such as deliberate local economic planning—would yield stronger long-term benefits.

Building the right infrastructure in the right places (Laurance and Arrea 2017) is challenging given the difficulty of deriving accurate forecasts of these effects. In our exercise, the baseline characteristics of the affected communities seem to have little predictive power. Better understanding of the mechanisms underlying these heterogeneous impacts is, in our opinion, an open area for future research.

ADDITIONAL FILE

The additional file for this article can be found as follows:

- **Appendix Figures and Tables.** Figures S1 to S4 and Tables S1 to S4. DOI: https://doi.org/10.31389/eco.466.s1

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COMPETING INTERESTS

The authors have no competing interests to declare.

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