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Local economic development through clean electricity generation – an analysis for Brazil and a staggered difference-in-difference approach

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Abstract

Adaptation of energy systems worldwide to move away from fossil fuels is widely accepted to be a key step in responding to the challenge of climate change. For developing countries and their development banks, this challenge is compounded by the need to ensure economic development, particularly to lift parts of the population out of poverty. In this article, we analyse the economic impacts of electricity generation projects of the Brazilian national development bank. We use a two-way fixed-effects (TWFE) estimator on a 15-year municipality-level panel with time-varying (or "staggered") treatment that accounts for recent findings in the panel data analysis literature. Our study finds that clean electricity generation has weaker economic effects compared to fossil electricity generation and compared to other projects of the development bank. This differentiated impact is particularly notable when it comes to the impact of investment on employment creation and wage levels. This is the first study that uses microdata to analyse the different economic impacts of clean electricity generation and fossil electricity generation at the local level. We posit that differences in labour intensities of clean electricity generation jobs and the jobs created by fossil electricity generation as well as other types of development bank investment account for these different impacts of project investments. We recommend that the cost of externalities of these projects be internalised in order for development banks and policymakers to get a fuller picture of the benefits brought about by them. Smaller economic impacts of certain development bank investments might also have negative implications for poverty reduction efforts in the country.

Keywords: energy; development bank; employment creation; microdata; staggered panel-data analysis; Brazil

JEL classification: C18, O22

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David Grover: Conceptualisation, Methodology, Validation, Writing – Review & Editing, Supervision.
Dorothée Charlier: Methodology, Writing – Review & Editing, Supervision.

Highlights

- Clean electricity generation projects in Brazil have smaller local economic benefits compared to fossil electricity generation projects
- Lower labour intensity for clean electricity generation projects is posited to explain these differences
- It is proposed that development bank projects be monitored more closely and their externalities measured more accurately
- Smaller economic impacts of certain development bank projects might also have implications for poverty reduction efforts in Brazil

Abstract (français)

L’adaptation des systèmes énergétiques mondiaux est considérée comme un élément clé face au défi du changement climatique. Pour les pays en développement et leurs banques de développement, ce défi s’ajoute au besoin du développement économique, particulièrement pour réduire les taux de pauvreté dans ces pays. Dans cet article, nous analysons les impacts économiques des projets de la banque nationale de développement brésilienne. Nous utilisons la méthode du « two-way fixed effects » (TWFE) pour analyser un panel de 15 ans au niveau communal avec un traitement qui varie avec le temps (un traitement « échelonné »), qui prend en compte certains développements récents dans la littérature d’analyse des données de panel. Notre étude montre que la production d'électricité propre a un effet économique plus faible par rapport à la production d'électricité à partir des sources fossiles. Cet impact différencié est particulièrement remarquable quant aux effets sur la création d’emploi et au niveau salarial. Cette étude est la première qui utilise des données micro pour analyser les impacts économiques différenciés de la production d'électricité propre et de la production d'électricité à partir des sources fossiles. Nous postulons que cet impact différencié est dû aux différences des intensités de main-d’œuvre entre les emplois créés par la production d'électricité propre et ceux créés par la production d'électricité à partir des sources fossiles. Nous recommandons que les coûts des externalités de ces projets soient internalisés afin de mieux informer les banques de développement et les décideurs politiques. Enfin, les impacts économiques plus faibles de certains investissements des banques de développement pourraient également avoir des conséquences négatives sur les efforts de réduction de la pauvreté dans le pays.

Mots-clés: énergie; banque de développement; création d’emploi; microdonnées; analyse de données de panel échelonnées; Brésil

1. Introduction

Ever since the adoption of the Kyoto protocol in 1997, and even more so since the adoption of the Paris accord in 2015, developing countries have had to formally deal with two challenges at the same time: economic development and greenhouse gas mitigation. Bringing about economic development is essential for these countries, particularly as a means to lift the population out of poverty (Dollar & Kraay, 2002), yet these countries also have a large stake in reducing CO_2 emissions, not only because their emissions are rising more rapidly than those of developed countries, but also because poorer populations are much more susceptible to the effects of climate change (Dell, Jones, & Olken, 2012). Do these two challenges form a synergy or a trade-off? There is a rich literature that uses models to explore both perspectives (see Köberle et al., 2021; Mathiesen, Lund, & Karlsson, 2011; Scherer et al., 2018), but there is a dearth of empirical studies that use microeconomic data to analyse the interactions between these two challenges. Using microeconomic data from Brazil, in this article, we present an econometric analysis of the local economic development impacts of clean electricity generation, a key element in climate change mitigation, and compare these impacts to those of fossil electricity generation, as well as other development projects.

In this article, we combine long-term data (2003 to 2017) of the investments of Brazil's national development bank (BNDES), with municipality-level data from the annual labour survey (RAIS) to analyse the broader impacts of clean electricity generation, fossil electricity generation, and non-electricity-generation projects on GDP per capita, employment levels, and wage levels in Brazilian municipalities.

We use a two-way fixed effects (TWFE) estimator on panel data with "staggered" treatment timing to estimate the aggregate and dynamic effects of clean electricity generation, fossil electricity generation, and other development projects on three indicators of economic development: GDP per capita levels, employment levels, and wage levels in the municipality where they are located. Since the potential of reverse causality in similar econometric models has been pointed out in the literature, we use a propensity score to compare only similar municipalities, to minimise time-dependent endogeneity that might lead to reverse causality concerns. Recent discussions around panel data analysis with varying treatment periods (also called "staggered" treatment panels) have been considered and methods suggested in the literature have been used as robustness checks (see Goodman-Bacon (2018)). To our knowledge, this is the first article in the literature on policy impact assessment to take into account these concerns relating to staggered treatments.

Empirically, we consider the case of Brazil, where its national development bank (Banco Nacional de Desenvolvimento Econômico e Social, BNDES) has played an important role in spurring infrastructure development projects in the absence of private capital investments (Hanley et al., 2016). In this article, we link the bank's activities from 2003 to 2017 with the agenda of sustainable development, looking into its electricity generation (clean and fossil) investments and their impacts on per capita GDP levels, employment levels, and wage levels. Current literature that has dealt with impacts of the bank's projects on employment creation has focused on its overall impact without distinguishing the types of projects (Pereira, 2007; Reiff, dos Santos, & Rocha, 2007; Torres Filho & Puga, 2006), or has dealt with productivity impacts (Coelho & De Negri, 2011), or has focused on impacts on investment (Barboza & Vasconcelos, 2019). However, with electricity generation projects accounting for nearly 20% of all BNDES investments in the period 2003-2017 (amounting to 91 billion Reais or 37.6 billion USD[1]), the impact of these projects on economic outcomes merit our attention.

As a signatory to both the Kyoto Protocol and the Paris Accord, the country has had to take steps to restructure its electricity generation and consumption patterns to reduce its overall carbon emissions (Santos et al., 2017). These environment-and-development debates in Brazil frequently focus on deforestation. The present research relates to deforestation in two ways. First, it assesses the economic impact of energy alternatives to potentially unnecessarily destructive methods of harvesting biomass for energy. Second, it considers the extent to which BNDES investments deliver wage and employment gains at all, which themselves should reduce the deforestation rate in forested municipalities to the extent that poverty drives many of the irregular economic activities that in turn drive deforestation. Over the 20th century, Brazil was known for chronically high levels of poverty and income inequality. By

[1] Since the Brazilian Real has been very volatile with respect to the US Dollar between 2003 and 2017, an average exchange rate of 1 USD = 2.4188 BRL has been used throughout this article wherever a conversion is presented.

integrating the narratives of economic development and the necessity of electricity generation projects to be more climate-conscious, this article also contributes to the literature of sustainable development impacts of clean electricity generation investments as well as those of development bank activities.

The remainder of the article is organised as follows: section 2 presents in more detail a literature review of the broader macroeconomic literature and the impact assessment literature, section 3 sets the empirical context, with a presentation of the data and descriptive statistics, section 4 describes the model and the approach used to treat econometric issues, and section 5 interprets the results. The article concludes with a discussion and a presentation of the conclusions in section 6.

2. Literature review

A broad literature in macroeconomics attests to the general positive relationship between infrastructure development and public spending with output (or GDP) elasticity and employment (Abiad, Furceri, & Topalova, 2016; Romp & de Haan, 2007), starting from the work of Aschauer (1989) (see Bom & Ligthart (2014) for a review). Some studies do not differentiate between types of infrastructure spending, such as Calderón & Servén (2008) who find a positive impact of infrastructure investment on GDP growth in sub-saharan Africa. Other studies using quasi-experimental methods have focused on evaluating the impacts of specific types of infrastructure spending (usually transport sector or telecommunications sector spending) and have looked at their impacts on various development-related outcome variables. Chandra & Thompson, (2000), for instance find a positive impact of interstate highway construction in the US on the level of economic activity at the county level. Similarly, Banerjee, Duflo, & Qian, (2012) find a moderate positive causal effect of access to transportation on GDP per capita levels. When it comes to analysing energy sector investments, a significant portion of the literature focuses on analysing the impact of rural electrification on development indicators in developing countries (see, for instance, Bensch, Kluve, & Peters (2011); Bernard (2012); van de Walle, Ravallion, Mendiratta, & Koolwal (2015), and a meta-evaluation of the World Bank's own rural electrification projects (IEG World Bank, 2008)). This literature, however, focuses on expanding electricity access, which is related to access to the electricity distribution network (and might not necessarily involve electricity generation investments). Our present study, however, while still positioned in the impact evaluation literature, looks at electricity generation projects and their impacts on GDP per capita levels, employment levels, and wage levels.

The studies closest to ours would have to be those that use quasi-experimental methods to analyse the impacts of infrastructure investments in Brazil (including other studies that analyse the impacts of BNDES investment). Some studies that have dealt with the impacts of Clean Development Mechanism (CDM) projects on sustainable development outcomes in Brazil have shown poverty-reducing and employment generation impacts of clean electricity generation investments, but do not make a comparison of those projects with fossil energy generation projects or non-electricity-generation projects (Grover & Rao, 2020; Mori-Clement, 2019; Mori-Clement & Bednar-Friedl, 2019).

Reiff, dos Santos, & Rocha (2007), publishing in the BNDES journal, use municipal level data and show that BNDES investment did generate employment between 2000 and 2005, in line with similar findings for investments before 2000. This finding should not be surprising, given the high volume of investment. Using data at the municipal level, and a fixed-effects estimator, they estimate around a 0.01 percent increase in formal employment levels for each percentage increase in bank investment. Torres Filho & Puga (2006) and Pereira (2007) show that those enterprises that received BNDES funding generated more employment than those that did not receive BNDES support, and with higher wages. Their article, however, lacked the presentation of a full model (including the specific methods used to control for selection bias), and their conclusions might need to be treated with some caution.

De Sousa (2009) finds no signs of impact of BNDES actions on firms' productivity. Coelho & De Negri (2011) observe that BNDES financing positively affected the growth rates of total factor productivity, labour productivity, the number of employees, and net sales. Moreover, the firms that most benefited from BNDES financing were those with high total factor productivity. Frischtak, Pazarbasioglu, Byskov, Hernandez Perez, & Carneiro (2017) also hold that increases in BNDES investment tend to boost economic output and contend that it lowers inequality in the medium term. In an evaluation of 31 large industrial projects of the BNDES on employment and GDP per capita and their agglomeration spillover effects, Sant'Anna, Martini, & Dias (2020), using a fixed effects model with synthetic controls, demonstrate that large projects did have a significant impact (of up to a 10 percent increase) on both these variables.

There has also been some uncertainty cast on previous results in the literature showing employment creation by BNDES investments, particularly those results that appear in BNDES-produced journals. Hanley et al., (2016) offer a sober and uncertain view of the future of the bank, in light of corruption scandals and the economic and political crisis in the country, and underscore the need for external analyses of BNDES projects and their impacts. Few econometric studies, with the exception of recent studies by Martini, Jordão, Grimaldi, & Dias (2019) and Sant'Anna et al. (2020) also take into account time-dependent endogeneity issues that might arise due to the projects' locations.

Missing from the literature is a specific insight into electricity generation projects of the Bank, as well as their impacts on employment and wage levels. To address this empirical gap, in this article we study the impact of BNDES investments in electricity generation projects on per-capita GDP levels, on employment levels, and on wage levels, specifically looking into the effects that clean electricity generation projects have compared to fossil electricity generation, and to other projects of the bank.

There is a part of the macroeconomics literature that has looked into the different economic impacts of clean electricity generation and fossil electricity generation. This literature suggests that while there is a correlation between energy consumption (regardless of type) and economic growth, there is also evidence to suggest that different types of energy sources might have different impacts (Apergis & Payne, 2012; Ohler & Fetters, 2014). It is also not clear whether investment in clean electricity generation results in net employment creation or destruction (or neither). While some studies report a net positive economic impact (Bulavskaya & Reynès, 2018; Caldés, Varela, Santamaría, & Sáez, 2009), other studies have been more circumspect (Frondel, Ritter, Schmidt, & Vance, 2010; Lehr, Lutz, & Edler, 2012). It has been noted that net employment benefits depend on factors such as origin of the equipment (domestically manufactured or imported) (Cai, Cusumano, Lorenzoni, & Pontoni, 2017), and that employment benefits differ between electricity generation technologies due to different labour requirements at different project phases (construction, operation and maintenance) (Tourkolias & Mirasgedis, 2011). There is therefore reason to hypothesise that the economic and employment creation impacts of clean electricity generation and fossil electricity generation might be different. We can expect this difference because these two types of investment might require different labour intensities that in turn affect the local labour markets differently.

3. Data and descriptive statistics

Project-level data were obtained from the BNDES for the period 2003 to 2019, with information about the nature of the project, the location of the project (at municipality-level), the funding instrument used, the date of commencement of the project, and the amount invested in the project as of 2019. There were 17,874 projects in the dataset out of which 2,822 projects were electricity generation projects. Out of these electricity generation projects, 140 projects were fossil electricity generation projects and 2,682 projects were clean electricity generation projects. Only projects that were classified under the subsector "Electric energy generation"[2] were included (breakdown of classification of projects shown in Table 10 in Appendix A). During the course of the organisation of the data, projects that were not associated with a municipality were dropped from the dataset. Out of the 17,874 projects in the original dataset, 6,050 projects were dropped due to this lack of location data. Projects contracted before 2003 and after 2017 were also dropped (411 projects). A further 709 projects were present in the dataset but did not receive any investment, and hence were dropped. Out of the 5,560 municipalities in the country, 991 received at least one project funded by the bank, with a total of 11,115 projects in consideration for the analysis. Table 1 presents the relevant project-level descriptive statistics, and Figure 1 presents the evolution of the yearly investment by project type (shown in percent of total), aggregated at the national level.

[2] « Geração de energia elétrica » in Portuguese.

The data were then collapsed to municipality-level data, and integrated with census data and national accounts data obtained from the Brazilian Geographic and Statistical Institute (Instituto Brasileiro de Geografia e Estatística, IBGE) as well as the *Bolsa Familia* social transfer programme data. The social transfer programme data were not available for the years 2014 and 2015.

As for employment and wage data, data from the annual labour survey (Relação Anual de Informações Sociais, RAIS) were obtained from the Brazilian Ministry of the Economy from 2003 to 2017, at the municipality level. After the integration of these data, the data was organised as a panel dataset (strongly balanced). The panel has a time-varying treatment structure (or “staggered” treatment) that presents some methodological challenges (discussed in section 4.1).

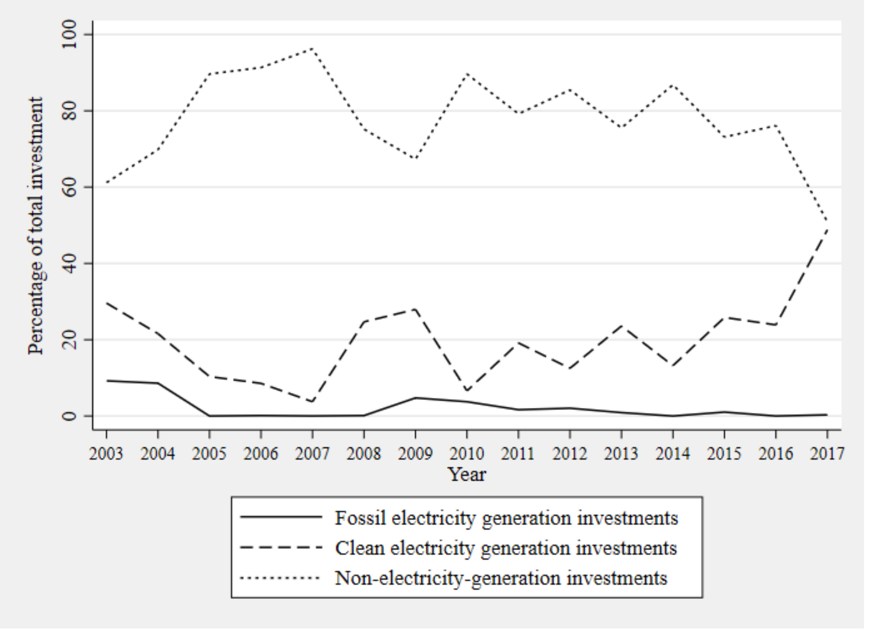
We consider the municipality as the unit of analysis in our models. This unit of analysis seems the most appropriate, given the nature of the projects that are in our dataset – the median project investment in a municipality is around 8 million BRL (around 3.3 million USD), compared to the median GDP of 43 million BRL (17.7 million USD).

Tables 2 and 3 present descriptive statistics of the panel variables at the municipality-year level, and Figure 2 presents the mean annual growth (of each of the dependent variables) of the municipalities that received each type of project. Table 4 presents the correlation coefficients between the dependent and independent variables of interest. The differences seen in Figure 2 between those municipalities that received a project and those municipalities that did not point us to the possible endogeneity problems that might need to be accounted for while estimating a regression model. This challenge is treated in detail in section 4.1.

Table 1: Project-level descriptive statistics

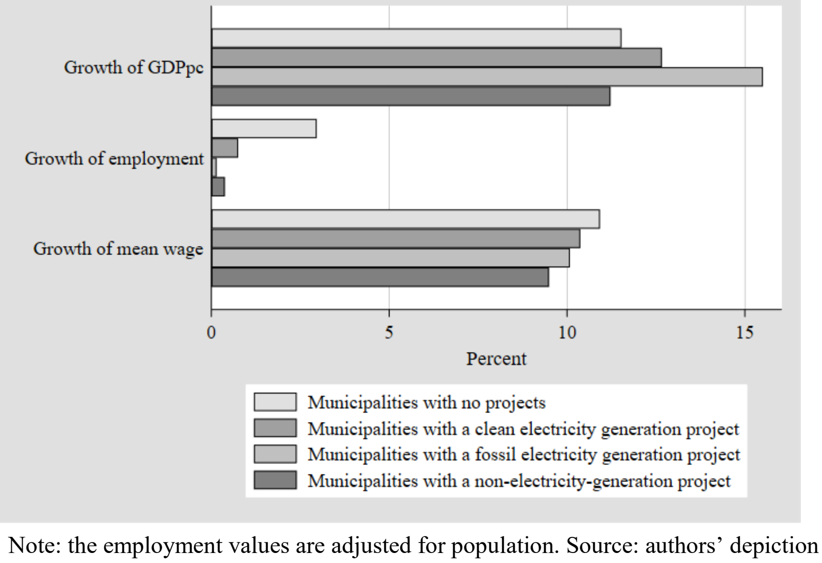
Project type	Description	N	Mean	SD	Min	Max	Total
Investment (all projects)	Investment in projects till 2017 (million BRL)	11,115	36.1	163.0	0.002	9,890	402,266
Investment (electricity generation)	Investment in electricity generation projects till 2017 (million BRL)	1,934	42.6	169.7	0.002	2,500	82,452
Investment (clean electricity generation)	Investment in clean electricity generation projects till 2017 (million BRL)	1819	41.1	124.6	0.04	2,500	74,759
Investment (fossil electricity generation)	Investment in fossil electricity generation projects till 2017 (million BRL)	115	66.9	158.0	0.30	1,250	7,692
Investment (non-electricity-generation)	Investment in non electricity-generation projects till 2017 (million BRL)	9,181	34.8	126.9	0.04	9,890	319,814

Source: authors' calculations, using data provided by BNDES



Source: authors depiction using data from BNDES

Figure 1: Evolution of total investment over time



Note: the employment values are adjusted for population. Source: authors' depiction using data from BNDES and the RAIS labour survey from the Brazilian ministry of the economy

Figure 2: Mean year-on-year growth of the three dependent variables, according to the type of project in that municipality

Table 2: Descriptive statistics of dependent variables used (municipality-year level)

Variable	Description	N	Mean	SD	Min	Max
$GDPpc_{it}$	GDP per capita (1000 BRL)	83,400	13.37	16.58	0.3	815.69
$Employment_{it}$	Total number of people employed in municipality	83,436	7,384.39	77,265	1	5,236,600
$Mean\_wage_{it}$	Mean monthly wage level (BRL)	83,436	1046.85	501.65	166.28	10,077.36

Source: authors' calculations, using data from BNDES, the RAIS labour survey from the Brazilian ministry of the economy, and from the IBGE.

Table 3: Descriptive statistics of independent variables (municipality-year level)

Variable	Description	N	Mean	SD	Min	Max
Investment (total) $_{it}$	Investment in all types of projects (million BRL)	83,399	4.70	116.45	0.0	13,454.58
Investment (electricity generation) $_{it}$	Investment in all electricity generation projects (million BRL)	83,399	0.97	52.60	0	13,337.74
Investment (clean electricity generation) $_{it}$	Investment in clean electricity generation projects (million BRL)	83,399	0.87	51.86	0.0	13,337.74
Investment (fossil electricity generation) $_{it}$	Investment in fossil electricity generation projects (million BRL)	83,399	0.09	7.98	0.0	1,402.83
Investment (non-electricity-generation) $_{it}$	Investment per capita to non-electricity-generation projects (million BRL)	83,399	3.74	100.86	0.0	13,392.22
$Population_{it}$	Population (1000 people)	83,399	34.71	205.27	0.80	12,106.92
Bolsa $Familia_{it}$	Bolsa Familia cash transfers (1000 BRL)	61,130	229.5	788.9	40	72,081.5

Source: authors' calculations, using data from BNDES, the RAIS labour survey from the Brazilian ministry of the economy, the IBGE, and Ipeadata.

Table 4: Correlation coefficients between the main dependent and independent variables

	GDPpc	**Employment per capita**	**Mean wage**
Investment (total)	0.37	0.35	0.29
Investment (electricity generation)	0.16	0.13	0.13
Investment (clean electricity generation)	0.15	0.12	0.12
Investment (fossil electricity generation)	0.06	0.06	0.06
Investment (non-electricity-generation)	0.36	0.34	0.27

Note: all variables in logs. Source: authors' calculations, using data from BNDES, the RAIS labour survey from the Brazilian ministry of the economy, and the IBGE.

Three dependent variables were identified as being of interest to the study, following the hypotheses presented. The first, *GDPpc* (GDP per capita), is an indicator of economic activity in a municipality-year, the second, *Employment* measures the total number of people employed in a municipality (as of December of a given year). The third, *Mean wage* measures the mean monthly wage in a municipality-year.

To construct the independent variable, we use two different (but clearly related) ways to measure the presence of a project in a given municipality-year. The simplest way is to use a binary dummy variable to denote the presence of at least one project in that municipality-year. We add detail to the independent variable by using the cumulative investment as the independent variable[3] (model explained in detail in section 4.2).

The binary dummy variable in question, d_{it}, was created to denote the presence of a project in a particular municipality *i* and in year *t*. A value of 1 for the d_{it} project dummy would indicate that the municipality is a "treated" municipality in that year i.e. at least one project was present in that municipality in that year. If a municipality is treated it remains treated for the rest of the panel. This would mean that a project and its investment are considered immobile – if a municipality receives a project in a particular year, the project and its associated capital remains in the municipality. This is a reasonable assumption to make, since investments through the development bank are largely in infrastructure development. This is also an important assumption to make to estimate the model (see (Callaway & Sant'Anna (2021)). The model is discussed in detail below in section 4.

[3] Descriptive statistics of the variable *Investment* are presented in Table 3. However, to aid interpretation, the variable used in the regressions is *Investment per capita*, denoted as $Investment_{pc}$.

4. Model

A panel data analysis using a two-way fixed effects (TWFE) estimator was used to analyse the principal research question: to investigate whether investment in the bank's projects (eventually separated into clean electricity generation, fossil electricity generation, and non-electricity-generation projects) results in an increase of per capita GDP, employment levels, and wage levels. The estimator makes use of panel data for the entire period of 2003-2017 (with 15 periods), allowing for time-invariant fixed effects and year fixed effects to explore the effects of the presence of a project (the independent variable) on GDP per capita, employment levels, and mean wage levels (the dependent variables).

4.1. Dealing with econometric issues

The structure of the panel and the choice of model (further detailed in section 4.2) present two econometric issues that might introduce bias in the estimation. The first is the endogeneity that might potentially be present in an OLS model due to non-random assignment of projects to municipalities. The second issue concerns the "staggered" nature of the treatments of the municipalities with respect to time, and the potential bias that might be introduced by estimating a model without considering that possibility (cf. Goodman-Bacon, 2018). Since the data and methods used here present a natural quasi-experimental scenario, there is the possibility of endogeneity that needs to be accounted for, which would otherwise provide biased results in an OLS model. There is the possibility that investment flows to municipalities not in a random manner (as is required for an experimental setup), but based on certain underlying characteristics of the municipality (i.e. based on some selection criteria). In other words, an inference based on OLS estimation might include some reverse causality, since it cannot be ruled out *a priori* that investment flows to municipalities that already are richer, or already have a strong industry presence, for instance. A naïve OLS estimation will also fail to take into account any pre-existing time-independent differences between municipalities, such as area, geography, or presence of existing industry, among others.

In an ideal experiment, the treatment (i.e. allocation of project) would be random, and any differences between the levels of the dependent variables between treated municipality-years and all municipality-years would purely be due to the treatment effect (see Table 5). However, the magnitude of the difference between the means of the two groups appears to be larger than what we would expect for a treatment effect, and leads us to suspect that some degree of endogeneity would be present in a pooled OLS estimation.

Two strategies are employed to reduce endogeneity in the model. The first is to include municipality fixed effects in the model that would account for time-independent differences between municipalities such as geography. The second strategy attempts to choose a subset of the treated municipalities that would more closely approximate a random allocation of treatment. This is achieved by generating a propensity score that estimates each municipality's propensity to be treated based on pre-treatment characteristics of the municipality, and then by choosing a subset of the municipalities comprising of those that have similar propensity scores (Khandker, Koolwal, & Samad, 2010)[4]. A similar approximation of a doubly robust estimator was used by Ravallion & Chen (2005). The propensity score is estimated using a probit model for the treated municipalities in 2003, based on pre-treatment characteristics of the municipalities (i.e., using covariates from 2002 or the closest available pre-treatment year). The results table of this probit model are presented in Table 11 in Appendix A. Following the probit estimation, 8 blocks were generated, with each block containing municipalities of comparable propensity scores, the balancing property of the covariates being satisfied (details of the different blocks are presented in Table 12 of Appendix A). The first block (which is also the largest) was retained to continue with the analysis, in order to have a sample of municipalities that are comparable to each other. The first block groups municipalities that have propensity scores between .0010111 and 0.025, and the block contains 3,919 municipalities. The panel ultimately used for subsequent analyses contains only these municipalities (for 15 time periods, this translates to 58,785 municipalities). Descriptive statistics of this restricted panel are presented in Tables 13 and 14 in

[4] This combination of a linear regression and propensity score matching is an attempt to imitate a doubly robust estimation, or an augmented inverse propensity weighted estimator (originally developed for cross sections by Robins, Rotnitzky, & Zhao (1994)). However, applying the same concept to a panel data TWFE estimation with unit fixed effects (in our case, municipality fixed effects) is not obvious (see Arkhangelsky, Imbens, Lei, & Luo (2021)), hence leading us to a compromise that retains (to the extent possible) external and internal validity while also retaining statistical power.

Appendix A. It is to be noted that while the means of most variables in restricted panel are similar to those of the full panel, the restricted panel seems to exclude some of the larger municipalities[5].

Comparing the means of the dependent variable of the treated municipality-years and all municipality-years in the panel (Table 5), it can be seen that the difference between the two is clearly lesser in the restricted panel, as evidenced by the reduced difference in means between the two groups, but there is still a statistically significant difference. Part of this difference could be ascribed to the true treatment effect (if any), part of it to time-independent differences between municipalities that persist after the propensity score matching, and part of it could be due to time-varying differences between treated and untreated municipalities.

[5] While the choice of this block among others may seem arbitrary, it is important to consider that these municipalities share a similar propensity score (and are hence "matched" and comparable to each other), and that this block also contains the majority of municipalities (hence retaining statistical power). Ravallion & Chen (2005) also recognize this tradeoff between reduction of bias and the exclusion of observations. A more elegant way to approach this problem without losing observations would be to use some version of inverse probability weighting, whose application to panel data, however, presents other difficulties (see Arkhangelsky et al. (2021) that attempts to resolve part of the issue, but does not allow for dynamic effects).

Table 5: Comparing the means of the dependent variables between treated municipality-years and all municipality-years before and after PSM

	GDPpc	**Employment levels**	**Mean wage levels**
Difference in means (treated vs untreated) before PSM (full panel)	0.889***	2.362***	0.395***
t-statistic	110 (df = 83096)	180 (df = 83082)	80.55 (df = 83082)
Difference in means (treated vs untreated) after PSM (restricted panel)	0.639***	0.778***	0.313***
t-statistic	45.42 (df = 58783)	47.88 (df = 58783)	35.89 (df = 58783)

Note: Variables are in logs. Degrees of freedom in parentheses. $^{*}p<0.1$, $^{**}p<0.05$, $^{***}p<0.01$.

A third source of bias may arise from the structure of the panel, specifically, due to the so-called "staggered" nature of the treatment of municipalities. Since municipalities in our panel are treated at different periods, the standard two-way fixed-effects model (TWFE) might be biased, as noted by an emerging econometrics literature (Callaway & Sant'Anna, 2021; Goodman-Bacon, 2018). This literature essentially considers the 2x2 difference-in-differences design (treated/untreated vs. pre/post-treatment) as the simplest way to ascertain the effect of an intervention, and Goodman-Bacon (2018) shows that a TWFE model can be decomposed into multiple 2x2 comparisons, and that the average treatment effect on the treated (the coefficient on the variable of interest in a TWFE model) is in fact a weighted average of these individual 2x2 combinations.

Among these individual 2x2 combinations, there might be some combinations that effectively compare the post-treatment period of some (treated) municipalities to the pre-treatment period of some municipalities that are already treated. This could amount to a violation of the parallel trends assumption that is needed to be made for a 2x2 difference-in-difference design, since the municipalities that are being treated as a control group are, in fact, not true controls. This could introduce a bias in the estimation, depending on the weight that these "spurious" comparisons have in the panel (Goodman-Bacon, 2018), and there might also be bias in the estimations of the dynamic effects of the treatment (Sun & Abraham, 2021). In theory, the bias should reduce when the number of "never-treated" municipalities is large, but the robustness of the TWFE model should be explicitly tested.

One way to test for the robustness of the TWFE model in a staggered panel would be to test for the effects of all the projects year-by-year, using a t-test to compare the treated municipalities before and after treatment. A robustness check to account for the staggered structure would be to apply the TWFE model to smaller subsets of the data, i.e., subsets defined by temporal or spatial restrictions. A difference-in-difference with multiple periods estimator has been used to perform these robustness checks as well as gather the dynamic effects (Callaway & Sant'Anna, 2021). The results of these robustness checks are mentioned in section 5.3.

4.2. Model specification

The fixed effects model used is as shown in equations 1.1 and equation 1.2:

$$Y_{it} = \beta_0 + \beta_1 \cdot d_{it} + \boldsymbol{\beta X_{it}} + \alpha_i + \Omega_t + \varepsilon_{it} \tag{1.1}$$

where Y_{it} is the dependent variable, $\mathbf{d_{it}}$ is a dummy variable that denotes the presence of a project in a municipality in a given year, $\mathbf{X_{it}}$ is a vector of time varying covariates, α_i denotes time invariant fixed effects, and Ω_t denotes year dummies. β_0 denotes the intercept, and $\beta_1, \beta_2, \ldots \beta_n$ denote the coefficients of the independent variables and covariates, and ε_{it} denotes the error term. A summary of these variables has been presented in the previous section.

In some ways, the equation 1.1 would capture the "pure" effect of the presence of project investment on the outcome variables (or in other words, the average treatment effect on the treated municipalities, ATT). In addition, the model used to estimate 1.1 lends itself more easily to robustness checks that are detailed in section 5.3.

We then interact the dummy variable $\mathbf{d_{it}}$ with the variable *Investment* to get the variable $Investment_{it}$, that is the amount of BNDES project investment present in that municipality-year.

$$Y_{it} = \beta_0' + \beta_1' \cdot Investment_{it} + \boldsymbol{\beta' X_{it}} + \alpha_i + \Omega_t + \varepsilon_{it} \tag{1.2}$$

As summarised in Tables 2 and 3, the three dependent variables are *GDPpc*, (the GDP per capita of a municipality-year), employment levels in a municipality-year, and the mean wage levels in a municipality-year. The independent variables of immediate interest are the investment amounts for projects per capita[6]. The total investment in projects is broken down into investments in electricity generation and non-electricity-generation projects. The dynamic effects of the model are also estimated, to follow the evolution of the impacts of the projects across time periods.

[6] The *Investment* variables are used as *Investment per capita* in the regressions to aid interpretation. The significance level is robust to using *Investment* instead of *Investment per capita*.

5. Results

Tables 6 and 7 show the results of the regression, with the GDPpc, employment levels, and mean wage levels as the dependent variables. Overall, the bank's projects have a positive effect per capita GDP and employment levels, with each additional percentage of investment per capita leading to a 0.023 percent increase of GDP per capita, a 0.018 percent increase in employment, and a corresponding 0.006 percent increase in mean wage levels. Electricity generation projects seem to have a less positive effect than non-electricity projects as can be seen from Table 7. To be able to compare the magnitude of the effect size of these projects, the coefficients presented in Tables 6 and 7 could be compared to the coefficients of the effects of primary, secondary, and tertiary sector investments on the same dependent variables (Table 15 in Appendix A).

Table 6: Effects of investments in all project types

	GDPpc		Employment per capita		Mean wage	
Investment$_{pc}$ (log)	0.023^{***}		0.018^{***}		0.006^{***}	
	(0.003)		(0.003)		(0.002)	
Project dummy		0.155^{***}		0.110^{***}		0.030^{***}
		(0.023)		(0.024)		(0.011)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	43091	43091	43091	43091	43091	43091
R2	0.86	0.86	0.24	0.24	0.91	0.91

Note: All dependent variables are in logs. All independent variables except project dummy are in logs. Fixed effects model estimation by OLS using the demeaning method (equivalent to including municipality fixed effects). Heteroskedasticity-robust standard errors in parentheses (adjusted for 3,919 clusters (corresponding to municipalities)). Constant and coefficients of population (log), Bolsa Familia spending (log), and of year dummies omitted for presentation. $^{*}p<0.1$, $^{**}p<0.05$, $^{***}p<0.01$.

5.1. Comparison of the impacts of electricity generation and non-electricity-generation projects

When projects are divided into electricity generation projects and non-electricity generation projects, a clear difference can be observed between the coefficients of each of these groups of projects on the dependent variables, as can be seen from Table 7. While electricity generation projects do have a positive and statistically significant impact on GDP per capita (a percent increase in electricity generation investment causing 0.017 percent increase in GDP per capita), it is clearly lesser than for non-electricity-generation projects (a percent increase in non-electricity-generation investment leading to 0.026 percent increase in GDP per capita). The economic impact of the bank's investments can be clearly seen, however with a difference between types of projects. When it comes to total employment as a dependent variable, the difference between electricity generation and non-electricity-generation projects is stark: electricity generation projects do not cause any statistically significant increase in total employment levels, in contrast to non-electricity-generation projects, which do cause significant increases in total employment levels. This result is surprising, since it suggests that electricity generation projects have a positive economic impact without having an effect on employment levels. This result also seems to be corroborated by the results of the model with mean wage levels as the dependent variable. The same pattern is observed of electricity generation projects not leading to increases in wage levels, whereas non-electricity-generation projects cause an increase in wage levels (0.008 percent increase in wage levels for every percent increase in investments in non-electricity-generation projects). A possible explanation is that an increase in employment levels might have lead to an upward pressure on the price of labour (i.e. wages), and this effect is observed here for non-electricity-generation projects but not for electricity generation projects.

These results would include only the downstream local impacts of construction and operation and maintenance of the project. Upstream benefits, including manufacturing or extraction would not typically be included, since the projects included are strictly electricity generation projects, and because we account for them starting from the moment they are implemented in the municipality. For instance, taking into account the manufacturing processes of wind turbine blades (which might take place elsewhere in the country or abroad) is not within the scope of this level of analysis.

Table 7: Effects of investments in electricity generation and non-electricity-generation projects

	GDPpc		**Employment per capita**		**Mean wage**	
$Investment_{pc}$ (electricity generation)	0.017***		0.005		0.003	
	(0.005)		(0.004)		(0.003)	
$Investment_{pc}$ (non-electricity-generation)	0.026***		0.029***		0.008***	
	(0.004)		(0.005)		(0.002)	
Project dummy (electricity generation)		0.137***		0.040		0.027
		(0.045)		(0.036)		(0.022)
Project dummy (non-electricity-generation)		0.158***		0.154***		0.033***
		(0.024)		(0.030)		(0.012)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	43091	43091	43091	43091	43091	43091
R2	0.86	0.86	0.24	0.24	0.91	0.91

Note: All dependent variables are in logs. All independent variables except project dummy are in logs. Fixed effects model estimation by OLS using the demeaning method (equivalent to including municipality fixed effects). Heteroskedasticity-robust standard errors in parentheses (adjusted for 3,919 clusters (corresponding to municipalities)). Constant and coefficients of population (log), Bolsa Familia spending (log), and of year dummies omitted for presentation. $^{*}p<0.1$, $^{**}p<0.05$, $^{***}p<0.01$.

Table 8: Effects of investments in clean and fossil electricity generation projects

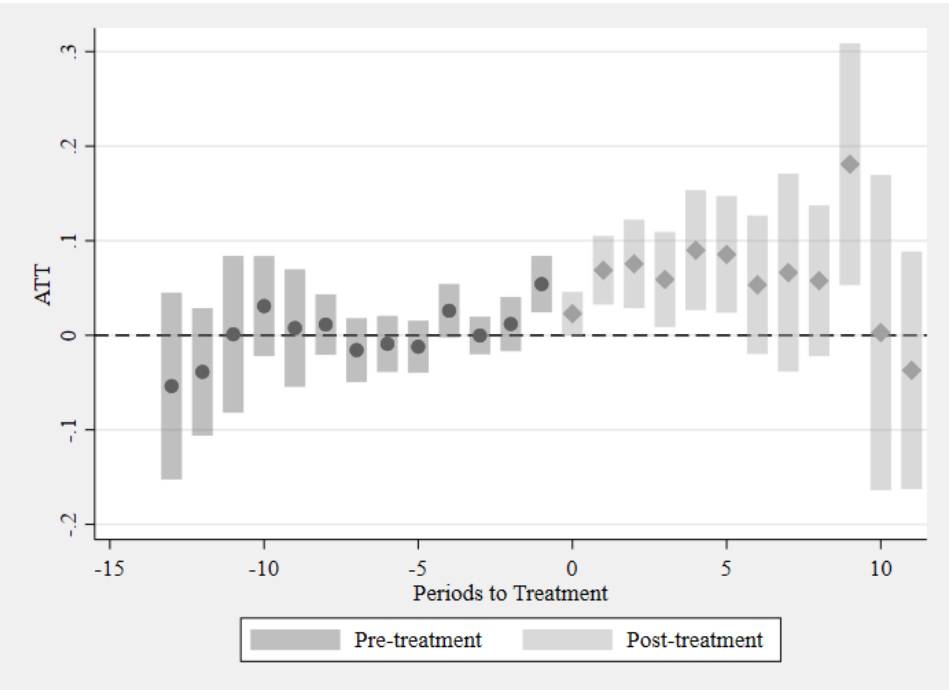
	GDPpc		Employment per capita		Mean wage	
Investment$_{pc}$ (clean electricity generation)	0.012**		0.004		0.000	
	(0.005)		(0.004)		(0.002)	
Investment$_{pc}$ (fossil electricity generation)	0.089**		0.031***		0.063**	
	(0.041)		(0.003)		(0.027)	
Investment$_{pc}$ (non-electricity-generation)	0.025***		0.029***		0.007**	
	(0.004)		(0.005)		(0.002)	
Project dummy (clean electricity generation)		0.103**		0.030		0.002
		(0.042)		(0.036)		(0.016)
Project dummy (fossil electricity generation)		0.977**		0.405***		0.686**
		(0.403)		(0.038)		(0.287)
Project dummy (non-electricity-generation)		0.151***		0.152***		0.028**
		(0.024)		(0.030)		(0.012)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	43091	43091	43091	43091	43091	43091
R2	0.86	0.86	0.24	0.24	0.91	0.91

Note: All dependent variables are in logs. All independent variables except project dummy are in logs. Fixed effects model estimation by OLS using the demeaning method (equivalent to including municipality fixed effects). Heteroskedasticity-robust standard errors in parentheses (adjusted for 3,919 clusters (corresponding to municipalities)). Constant and coefficients of population (log), Bolsa Familia spending (log), and of year dummies omitted for presentation. *p<0.1, **p<0.05, ***p<0.01.

This surprising result leads us to look deeper into the types of electricity generation projects that are being invested in (Table 8) – when clean electricity generation projects are considered in the regression analysis (i.e. electricity generation projects that do not involve fossil fuels) and are separated from fossil electricity generation projects, the results seem to suggest that the weaker (or inexistent) positive effects of electricity generation projects stem from clean electricity generation projects.

5.2. Dynamic treatment effects

The coefficients on the independent variables presented in Tables 6 to 8 are average treatment effects on the treated municipalities (ATT) over the entire time period covered by the panel. We break down the dynamic treatment effects for the post-treatment time periods (as well as pre-treatment periods). This is done using the data from the individual 2x2 difference-in-difference estimations obtained through the decompositions suggested by Callaway & Sant'Anna (2021), since including lags or leads of the primary independent variable in the model might lead to some biased estimations due to staggered treatment as described in section 4.1. Figure 4 shows the dynamic effects of the projects on GDPpc.



Note: ATT: Average treatment effect on treated. Figure generated using the *csdid* Stata package that implements a difference-in-difference with multiple periods estimator to decompose a TWFE model with staggered treatment to individual 2x2 difference-in-difference estimations. For the sake of brevity, only one dependent variable-independent variable pair was considered (GDPpc – Investment (total)). Source: authors' calculations.

Figure 4: Dynamic treatment effects, with the dependent variable as GDPpc (log)

5.3. *Robustness checks*

To verify that the model is robust to different specifications, particularly to the staggered treatment problem mentioned in section 4.1, three different models were created as robustness checks. The first uses a method recently proposed by Callaway & Sant'Anna (2021) to decompose a TWFE model and subsequently re-compose the coefficients. In addition, we use two complementary checks to manually verify the model's robustness to different specifications. We do this by i) using a broad t-test before and after treatment to discern that a treatment effect actually exists (this could be roughly compared to the "parallel trends test" frequently used in 2x2 difference-in-difference designs), and ii) applying the same TWFE model (presented in equation 1.1) to subsets of the dataset, to ensure that the effect observed in the model results is also present when applying the model to subsets of the data. The manual robustness checks are explained and presented in Appendix B.

Callaway & Sant'Anna (2021), in the difference-in-difference with multiple periods estimator that they develop offer a way to obtain a decomposition of the staggered TWFE model into 2x2 difference-in-difference estimations (which have been used to obtain the dynamic effects presented in Figure 4). The coefficients of the individual 2x2 estimations can be used to "re-compose" the average treatment effect on the treated (ATT) using appropriate weights (Goodman-Bacon, 2018). The specification corresponding to equation (1.1) was re-estimated using the difference-in-difference with multiple periods decomposition (using the same covariates). The ATT effects are presented in Table 9 below, and can be directly compared to the coefficients on the "project dummy" variable in Table 6. The "never-treated" municipalities are used as controls, but using only the "not-yet-treated" municipalities as controls do not change the coefficients or their significance radically. The improved doubly robust DiD estimator based on inverse probability of tilting and weighted least squares proposed by Sant'Anna & Zhao (2020) is used to gather the ATT estimates. The estimates are also robust to other methods used to compute the ATT estimates.

The ATT estimates presented in Table 8 below indicate that the coefficients presented in the main results table (Table 6) were probably biased upward. However, the significance levels remain comparable.

Table 9: Comparison of the ATT estimates using the difference-in-difference with multiple periods estimator to the TWFE model coefficients

	GDPpc (log)	**Employment per capita (log)**	**Mean wage (log)**
ATT computed through the difference-in-difference with multiple periods estimator	0.063*** (0.017)	0.056** (0.025)	0.009 (0.010)
Coefficients from Table 6	0.155*** (0.023)	0.110*** (0.024)	0.030* (0.010)

Note: for the purposes of the robustness check to ensure that the basic model holds, all projects were considered, without distinguishing between types of projects.

The difference-in-difference with multiple periods estimator method to compute the ATT estimates has its advantages in correcting for biased estimates of the TWFE model, but it can only consider a binary treatment as in equation 1.1 and cannot consider varying levels of treatment as can be done with the *Investment* variable in equation 1.2. We therefore maintain the results presented in Table 6 – in particular, the coefficients of the *Investment* variable lend themselves more easily to policy interpretation. Also, a single ATT estimate (or indeed a TWFE coefficient presented in Tables 6 to 8) might be inadequate to capture the dynamics of the model, so it is important to take the dynamic effects presented in Figure 4 into consideration while assessing the model, and not merely the TWFE coefficients which only shows the presence of an aggregate effect.

6. Discussion and conclusions

The results of the present analysis highlight the difficulties that policymakers and development project investors might have to face when trying to tackle the double problem of needing to have positive environmental and economic outcomes on their investment (in addition to financial returns on investment). The results seem to narrate a cautionary tale about clean electricity generation investments by providing empirical evidence about the differentiated impacts of electricity generation projects on economic indicators, namely GDP per capita, employment levels, and wage levels in Brazilian municipalities. The Bank's projects in general have a positive impact on GDP per capita, employment levels, and on wage levels, a result that should be unsurprising, given the large amounts of investment. It is of more interest, however, to look at the difference between different types of projects when considering their impacts. While the contributions of electricity generation projects to positive economic outcomes are present on a superficial level (when looking at GDP per capita), a deeper look into the impacts on employment and wage levels are more telling.

The difference might be due to the difference in labour intensities between jobs created in clean energy and fossil energy sectors, since there is evidence to suggest that "green" jobs are more skill intensive and less labour intensive than "non-green" jobs (Consoli, Marin, Marzucchi, & Vona, 2016). The effects of these investments on employment and wage levels seem to lend support to this explanation, given that while an economic impact of clean electricity generation projects is felt, mean wage levels and employment levels seem to be unaffected. This perhaps suggests that the economic benefits of these electricity generation investments might have been felt more by only a small proportion of the population, without affecting economic indicators at an aggregate level. More data on the labour intensities of these projects would be required in order to specifically locate the flows of labour in response to investments.

When it comes to the implications of these results, they affirm Ravallion & Datt (1996) and Loayza & Raddatz (2010)'s assertions that not all growth is equal – GDP growth per capita due to investments in clean electricity generation is lesser than GDP growth due to investments in fossil electricity generation in our sample. This might have an effect on poverty alleviation efforts in the country.

Is clean electricity generation then unequivocally bad for poverty and socio-economic outcomes? Our results show a reduced impact of clean electricity generation projects on economic output, wages, and employment, but it is important to consider that negative externalities of fossil fuel use (pollution, health, resource depletion, etc.) are not entirely internalised in the national accounts data that we use for analysis. Investing in environmental capital (in addition to "traditional" physical capital) might have positive socio-economic outcomes both in the short term (pollution reduction) and in the long term (increased labour productivity, increased resilience to climate uncertainties), but these would not be captured in national accounts data. Hallegatte et al. (2012) propose an analytical framework that includes environmental capital as an input in their production function that might serve to internalise these aforementioned externalities. They discuss the potential trade-off between environmental outcomes and economic outcomes that might arise out of "clean" investments, and identify the costs that might arise out of implementation of pro-environmental policies. They argue that these perceived costs depend on how we measure economic output. When negative externalities are internalised, some of these costs might not arise at all, or might be compensated by (relatively) smaller environmental externalities. Internalising negative externalities might also help in realising the real value of damage done by activities such as deforestation that is particularly relevant in a Brazilian context. Steps toward such an internalisation could potentially be made by correctly valuing environmental capital and socioeconomic outcomes.

However, we should be careful in contenting ourselves with rationalising a reduced effect size (as we see in our results) by merely identifying a lack of measurement of returns on investment in environmental capital. A reduced effect size can also be due to inefficient allocation of project capital, or it might still lie in the nature of employment created by these investments.

In the data considered in our article, we estimate that an inefficient allocation of project capital would be minimal. This is because BNDES project capital is not "allocated" by any central allocating mechanism, it is rather demand-led – requests for funding of projects come from where an investor is in need of it. We therefore have reason to believe that the reduced impact of clean electricity generation projects on GDP per capita, wages, and employment might be due to the lower requirement of labour that clean energy investment might require, compared to projects in other sectors.

Our results seem to indicate that for development banks, investing in non-electricity-generation projects might be a more efficient pathway to better economic outcomes. However, such a conclusion would be incomplete without taking into account the reduction of negative externalities and creation of positive externalities that clean energy projects ostensibly contribute to. One of those positive externalities would also be the development of competences in innovative technologies and the development of new business models (Fankhauser & Jotzo, 2018). The present article also aims to offer an insight into how the Brazilian national development Bank might need to reorient its strategy in response to the dual challenge of positive environmental outcomes and positive socioeconomic outcomes. We also echo the call of Frischtak et al. (2017) for the Bank to adopt specific and targeted programs to those sectors that have the potential to generate larger positive externalities (in this case, clean electricity generation projects). While there is a clear positive sign for the Bank's projects due to visibly positive effects of investments in general on economic outcomes, more precise monitoring of the projects that are funded, along with more detailed information on the types of employment opportunities that are created thanks to the Bank's investment might enable the bank to (better) target their lines of financing towards to counteract the weaker socioeconomic impacts of certain types of projects such as energy projects.

Clearer accounting procedures to better monitor the economic impacts of projects would also assist future analyses of these investments, with a view to increase efficiency in distribution of capital and aid.

7. References

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Appendix A

Table 10: Details of classification of projects into clean and fossil electricity generation projects

Classification	Name of CNAE subsector (Portuguese/original)	Name of CNAE subsector (English/translation)	Number of projects	Remarks
Clean electricity generation	Geração de energia elétrica – eólica	Generation of electric energy – wind	1367	
	Geração de energia elétrica – co-geração cana de açúcar	Generation of electric energy – cogeneration - sugarcane	269	
	Geração de energia elétrica – hidrelétrica	Generation of electric energy – hydroelectric	516	
	Geração de energia elétrica – PCH	Generation of electric energy – small hydroelectric	400	
	Geração de energia elétrica – nuclear	Generation of electric energy – nuclear	4	Category contains 4 projects. Classified as “clean energy”
	Geração de energia elétrica – solar	Generation of electric energy – solar	34	
	Geração de energia elétrica – outras fontes alternativas	Generation of electric energy – other alternative sources	6	Category contains 6 projects of power generation using biogas
	Geração de energia elétrica	Generation of electric energy	74	Category manually checked – most projects are biomass projects
	Geração, transmissão e distribuição de energia elétrica	Generation, transmission, and distribution of electric energy	12	Diverse small projects, but most get eventually excluded because they do not fit panel criteria
Fossil electricity generation	Geração de energia elétrica – térmica	Generation of electric energy – thermal	83	
	Geração de energia elétrica – co-geração exclusive c	Generation of electric energy – “exclusive cogeneration”	43	Project details manually checked – most projects use fossil energy
	Geração de energia elétrica – co-geração gas	Generation of electric energy – cogeneration gas	14	

Table 11: Probit model used to calculate the propensity score

	Project dummy (2003)
GDP per capita (2002)	0.384^{***}
	(0.096)
Population (2002)	0.415^{***}
	(0.043)
Number of branches of rural bank	0.366^{*}
	(0.210)
Primary sector share of GDP (2002)	-0.376
	(0.365)
Secondary sector share of GDP (2002)	-0.292
	(0.383)
Wage inequality (Gini) (2003)	0.824
	(0.592)
Latitude	-0.003
	(0.007)
Longitude	-0.006
	(0.007)
Obs.	5555
LL	-476.4

Note: All dependent variables are in logs. GDP per capita and population are in logs. Estimation by MLE. Standard errors in parentheses. Constant omitted for presentation. $^{*}p<0.1$, $^{**}p<0.05$, $^{***}p<0.01$.

Table 12: Lower and upper bounds of blocks of propensity score

Block number	**Inferior of block of pscore**	**Untreated**	**Treated**	**Total**
1	.0010111	3,893	26	3919
2	.025	458	14	472
3	.05	302	26	328
4	.1	153	35	188
5	.2	72	20	92
6	.4	22	16	38
7	.6	2	8	10
8	.8	0	3	3
Total		4,902	148	5050

Note: The balancing property is satisfied. The number of blocks selected was such that the mean propensity score for treated and untreated units is not different within each block. The total number of municipalities in the table is lesser than the total in the dataset since they fall out of the common support range.

Table 13: Descriptive statistics of dependent variables used in the restricted panel (municipality-year level)

Variable	Description	N	Mean	SD	Min	Max
$GDPpc_{it}$	GDP per capita (1000 BRL)	58,785	11.48	13.56	0.99	815.70
$Employment\_level_{it}$	Total number of people employed in municipality	58,785	1,139.70	1,099.89	1	36,088
$Mean\_wage\_level_{it}$	Mean monthly wage (BRL)	58,785	1,006.33	465.83	166.28	6,968.07

Source: authors' calculations, using data from BNDES, the RAIS labour survey from the Brazilian ministry of the economy, and from the IBGE

Table 14: Descriptive statistics of independent variables in the restricted panel (municipality-year level)

Variable	Description	N	Mean	SD	Min	Max
Investment (total) $_{it}$	Investment in all types of projects (million BRL)	58,785	0.87	25.70	0.0	2,664.83
Investment (electricity generation) $_{it}$	Investment in all electricity generation of projects (million BRL)					
Investment (clean electricity generation) $_{it}$	Investment in clean electricity generation projects (million BRL)	58,785	0.48	7.46	0.0	2,664.83
Investment (fossil electricity generation) $_{it}$	Investment in fossil electricity generation projects (million BRL)	58,785	0.06	16.86	0.0	1,402.83
Investment (non-electricity-generation) $_{it}$	Investment per capita in non-electricity-generation projects (million BRL)	58,785	0.33	16.91	0.0	2,422.76
$Population_{it}$	Population (1000 people)	58,785	12.45	10.32	0.90	122.42
Bolsa $Familia_{it}$	Bolsa Familia cash transfers (1000 BRL)	43,091	166.0	522.5	40	72,081.5

Source: authors' calculations, using data from BNDES, the RAIS labour survey from the Brazilian ministry of the economy, the IBGE, and Ipeadata.

Table 15: The effects of bank investment by sector on the three dependent variables

	GDPpc	**Employment per capita**	**Mean wage**
$Investment_{pc}$ (primary sector)	0.033^{***}	0.031^{***}	0.013^{***}
	(0.008)	(0.006)	(0.003)
$Investment_{pc}$ (secondary sector)	0.027^{***}	0.033^{***}	0.007^{***}
	(0.004)	(0.006)	(0.002)
$Investment_{pc}$ (tertiary sector)	0.017^{***}	0.008^{**}	0.004^{*}
	(0.005)	(0.004)	(0.002)
Year fixed effects	Yes	Yes	Yes
Obs.	43091	43091	43091
R2	0.86	0.28	0.91

Note: All dependent variables are in logs. All independent variables are in logs. Fixed effects model estimation by OLS using the demeaning method (equivalent to including municipality fixed effects). Heteroskedasticity-robust standard errors in parentheses (adjusted for 3,919 clusters (corresponding to municipalities)). Constant and coefficients of population (log), Bolsa Familia spending (log), and of year dummies omitted for presentation. $^{*}p<0.1$, $^{**}p<0.05$, $^{***}p<0.01$.

Appendix B

In addition to the robustness checks using the difference-in-difference with multiple periods estimator, to manually check the robustness of the model, a "short-term event study" design was implemented by choosing an arbitrary year 't' in the panel and constructing a window of +/- 3 years around the year. Any projects that were assigned to the municipality in year 't' were treated as "events", and the levels of the dependent variables before and after the "event" were compared using an independent samples t-test. The main goal was to be able to discern and isolate the impact of an event (in this case the arrival of project investment in a particular year) on the dependent variables. Municipalities that received projects in any other year than year 't' were excluded from the analysis, in order not to confound the analysis. Two control groups were used – a) the municipality-years of treated municipalities before the 'event' year (i.e. the pre-treatment periods of the treated municipalities), and b) the pre-treatment years of the treated municipalities and all the never-treated municipality-years. A positive difference in means between the two comparison groups would indicate that the levels of the dependent variables were higher in the treated municipality-years compared to the control group. While this test cannot be used to infer causality (due to the lack of control for covariates), it nevertheless suggests that a difference exists between the treated and untreated municipalities that needs further analysis. The results of the independent samples t-tests using this event study design with a +/- 3 year window suggest that for most years; there is a statistically significant positive effect of the presence of a project on GDP per capita in the treated municipalities. Results of the event study are presented in Table 16.

The second robustness check would be to apply the same regression as presented in equation 1.1 to subsets of the panel, the expectation being that the model applied to subsets of the panel should also present similar results as the model applied to the whole panel. The subsets considered might be based on temporal restrictions on the original panel, or it can be based on spatial restrictions on the original panel. Temporal restrictions could be, for instance a selection of any seven consecutive years in the panel and hence creating a subset of the panel. Spatial restrictions could involve creating subsets of the panel based on certain administrative subregions of Brazil.

Results of estimating the model for temporally- and spatially-restricted subsets of the panel are presented in Tables 17 to 19 (for GDP per capita as the dependent variable). The coefficients are in most cases of the same sign and statistical significance as the coefficients presented in Tables 6 and 7, although for a minority of the subsets of the panel, the coefficients are not significant.

Table 16: Robustness check with a constructed event study design

	Year	2006		2007		2008		2009		2010		2011		2012		2013		2014	
	Control group	UT +PT	PT	UT +PT	PT	UT +PT	PT	UT +PT	PT	UT +PT	PT	UT +PT	PT	UT +PT	PT	UT +PT	PT	UT +PT	PT
T-test	Diff in means	0.549	0.433	0.690	0.552	0.532	0.423	0.66	0.524	0.718	0.598	0.722	0.605	0.555	0.493	0.531	0.498	0.374	0.430
	Test statistic	t(21593) =5.652, p=0.00	t(341)= 3.78, p=0.00	t(21586) =6.81, p=0.00	t(334)= 4.40, p=0.00	t(21670) =7.79, p=0.00	t(418)= 4.80, p=0.00	t(21614) =7.81, p=0.00	t(362)= 5.01, p=0.00	t(21572) =7.25, p=0.00	t(320)= 4.95, p=0.00	t(21635) =11.16, p=0.00	t(383)=7. 38, p=0.00	t(21614) =9.34, p=0.00	t(362)=6 .19, p=0.00	t(21516) =7.86, p=0.00	t(264)= 5.40, p=0.00	t(21446)= 5.02, p=0.00	t(194)=4 .45, p=0.00
Number of treated municipalities		11		10		22		14		18		40		51		37		32	

Note: dependent variable is GDPpc (log). Control groups - UT + PT: Untreated (i.e. never-treated) and pre-treatment period of treated municipalities, PT: pre-treatment period only

Table 17: Robustness check with temporally restricted subsets

	Year	2006		2007		2008		2009		2010		2011		2012		2013		2014	
	Control group	UT + PT	PT	UT + PT	PT	UT + PT	PT	UT + PT	PT	UT + PT	PT	UT + PT	PT	UT + PT	PT	UT + PT	PT	UT + PT	PT
FE model	Coefficient	0.144	0.105	0.177	0.124	0.157	0.110	0.139	0.086	0.153	0.081	0.157	0.090	0.060	0.019	0.154	0.075	0.308	0.207
	p-score	0.00	0.00	0.00	0.002	0.00	0.001	0.001	0.042	0.003	0.091	0.007	0.108	0.089	0.682	0.031	0.479	0.002	0.288
Number of treated municipalities		87		82		84		73		64		69		51		37		32	

Note: dependent variable is GDPpc (log). Control groups: UT – Untreated municipalities (i.e. never-treated), PT – pre-treatment period of treated municipalities (i.e. not-yet treated)

Table 18: Robustness check using regression by macroregions (all project types)

	GDPpc				
Macroregion	**Centre West**	**North**	**North East**	**South**	**South East**
$Investment_{pc}$ (all projects)	0.021***	0.031***	0.044***	0.008**	0.025***
	(0.006)	(0.009)	(0.012)	(0.004)	(0.005)
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Obs.	3576	3672	13860	9756	12227
R2	0.84	0.88	0.89	0.89	0.82

Note: Dependent variable is in logs. All independent variables are in logs. Fixed effects model estimation by OLS using the demeaning method (equivalent to including municipality fixed effects). Heteroskedasticity-robust standard errors in parentheses. Constant and coefficients of population (log), Bolsa Familia spending (log), and of year dummies omitted for presentation. *p<0.1, **p<0.05, ***p<0.01.

Table 19: Robustness check using regression by macroregions (electricity generation and non-electricity-generation projects)

	GDPpc				
Macroregion	**Centre West**	**North**	**North East**	**South**	**South East**
$Investment_{pc}$(electricity generation projects)	0.016*	0.031***	0.041***	-0.001	0.009
	(0.009)	(0.008)	(0.014)	(0.004)	(0.007)
$Investment_{pc}$(non- electricity-generation projects)	0.023***	0.031**	0.039**	0.014***	0.034***
	(0.008)	(0.013)	(0.016)	(0.005)	(0.006)
Year fixed effects	Yes	Yes	Yes	Yes	Yes

Note: Dependent variable is in logs. All independent variables are in logs. Fixed effects model estimation by OLS using the demeaning method (equivalent to including municipality fixed effects). Heteroskedasticity-robust standard errors in parentheses. Constant and coefficients of population (log), Bolsa Familia spending (log), and of year dummies omitted for presentation. *p<0.1, **p<0.05, ***p<0.01.