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ARTICLE

No impact of rural development policies? No synergies with CCTs? The IFAD-supported Gavião Project in Brazil

Ausência de impactos de projetos de desenvolvimento rural? Ausência de sinergias com CCTs? O Pró-Gavião no Brasil

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Abstract: We estimate the impacts of an IFAD-supported rural development project (Pro-Gavião). Because public policies are frequently implemented simultaneously rather than in isolation, we also estimate the impacts of—and possible synergies with—the Brazilian conditional cash transfer (CCT) program Bolsa Família. Developed jointly by IFAD and the State Government of Bahia, Pro-Gavião was a rural development project in 13 contiguous municipalities between 1997 and 2005. Census tract level data were extracted for the analysis from the 1995-96 and 2006 Agricultural Censuses. The evaluation uses propensity score matching to construct a control group of untreated census tracts, and a difference-in-differences estimation to identify impacts. The outcomes analyzed include land productivity, agricultural income and child labor. Although Pro-Gavião involved significant investments in the region, the results suggest little if any program impact, or synergies between the two programs. The unexpected null findings are robust to alternative approaches to identifying the treated census tracts, matching techniques, and heterogeneity in several dimensions. We show that the lack of impacts is not driven by adverse rainfall in the treated communities, or the influence of other programs in the control communities. Alternative explanations for the null results are explored.

Keywords: rural development projects, conditional cash transfers, IFAD, synergies, Brazil.

Resumo: Estimamos os impactos de um projeto de desenvolvimento rural financiado pelo FIDA (Pró-Gavião). Uma vez que políticas públicas são frequentemente implementadas conjuntamente, também estimamos os impactos de, e as possíveis sinergias com, o programa brasileiro de transferência condicionada de renda, Bolsa Família. Desenvolvido conjuntamente pelo FIDA e pelo Governo da Bahia, o Pró-Gavião foi um projeto de desenvolvimento rural ocorrido em 13 municípios entre 1997 e 2005. Extraímos dados em nível de setor censitário para a análise dos Censos Agropecuários de 1995-96 e 2006. A avaliação usa Propensity Score Matching para a obtenção de um grupo de controle e diferença-em-diferenças para estimar os impactos. As variáveis de resultado incluem produtividade da terra, renda agrícola e trabalho infantil. Embora o Pró-Gavião tenha envolvido investimentos significativos na região, os resultados sugerem ausência de impactos do projeto, bem como de sinergias entre os dois programas. Esses resultados inesperados são robustos a diferentes abordagens para identificar os setores censitários tratados, a técnicas de pareamento e à consideração de heterogeneidades em diversas dimensões. Mostramos que a ausência de impactos não é resultado de condições pluviométricas adversas ou da influência de outros programas. Explicações alternativas para a ausência de resultados são exploradas.

Palavras-chave: projetos de desenvolvimento rural, transferências condicionadas de renda, FIDA, sinergias, Brasil.

1. Introduction

Rural development policies in developing countries are extremely heterogeneous. The menu of interventions includes infrastructure projects, credit and technical assistance, insurance policies, market access support, and initiatives to build human and social capital. Some policies focus on a single issue—such as credit for family farmers—while others are multi-faceted and complex. Projects supported by the International Fund for Agricultural Development (IFAD) fall into the latter category. Through a host of interrelated interventions, developed in consultation with the participating countries and with input from the targeted communities, IFAD's mission is "focused exclusively on reducing poverty and food insecurity in rural areas through agriculture and rural development" (International Fund for Agricultural Development, 2016a, p. 13). It has supported a wide variety of rural development projects around the world since its inception in 1977, and has provided US$18.5 billion in grants and low-interest loans to projects that have reached close to 500 million people (International Fund for Agricultural Development, 2018). Yet there is little rigorous empirical evidence of IFAD program impacts. Brown & Longworth (1992) is perhaps the only published article in economics that offers an evaluation of an IFAD project. We seek to address this gap in the literature by providing an evaluation of the IFAD-supported Gavião Project in Brazil.

Rural development projects and conditional cash transfer policies (CCTs) share the goal of reducing rural poverty, but their strategies differ. CCTs seek to alleviate current poverty and promote human capital investments that will improve the well-being of future generations (Fiszbein et al., 2009). Rural development projects aim to increase productivity, generate agricultural income and guarantee food security (World Bank, 2007; Janvry et al., 2002). Even though these policies have different target populations, designs, and actions, there are reasons to believe that policy synergies could exist between them (Maldonado et al., 2016a). By synergies we mean that the total combined impact of the policies is larger than the sum of their individual impacts, implying that there is a positive interaction effect. If synergies exist, designing policies to leverage them could contribute to the impact and cost effectiveness of anti-poverty policies in rural areas of developing countries.

The existence of synergies might be more likely in environments marked by significant market failures, such as those faced by many small farmers in developing countries (Janvry & Sadoulet, 2005). In these settings, social protection policies may help to relax liquidity constraints which could allow for greater investment in productive activities (Tirivayi et al., 2013). The impact on agricultural production is likely to be greater when cash transfers provide a predictable and stable stream of income (Sabates-Wheeler et al., 2009). Social protection policies can also contribute to the demand for food, which could enhance the incentives for investment and increased production (Devereux, 2016). Synergies could operate in the other direction as well, with increased agricultural production contributing to the nutritional well-being and long run human capital accumulation of children. There is no guarantee, however, that these policies will have enhanced impacts when executed simultaneously. This is especially true if there is no coordination in the design and implementation of the policies.

In recent years, there has been a dramatic increase in the number of impact evaluation studies in developing countries, and CCTs in particular have attracted considerable attention (Del Carpio et al., 2016; Brauw et al., 2015; Macours et al., 2012). While many specific agricultural policies in developing countries have been evaluated, such as subsidies designed to encourage the adoption of new technologies or the use of fertilizers (Ricker-Gilbert & Jayne, 2017; Duflo et al., 2011), there is little rigorous empirical evidence on the impacts of IFAD projects. To its credit, IFAD has recognized this limitation and has released several reports that conduct *ex post* evaluations of recent projects. These efforts, however, have been hampered by a lack of baseline data (International Fund for Agricultural Development, 2013, 2015, 2016b).

There is a similar limitation with regard to the evaluation of policy synergies. Despite growing recognition of the possibility of interactions between social and development programs, Maldonado et al. (2016a) stress that there is a dearth of empirical evidence on this topic in the international literature. In a broad review of the literature on the combined impacts of agricultural and social protection interventions, Soares et al. (2017, p. 10) note that the lack of evidence on synergies results from the fact that most "evaluations do not try to measure the interaction effects but focus solely on the overall impact." The chapters published in Maldonado et al. (2016b) represent an initial attempt to study synergies in six Latin American countries. The Peru and Colombia studies are particularly relevant as they test for synergies between CCTs and IFAD-supported projects. The results are mixed, however, with Moya (2016) finding negative synergies on production and assets in Colombia, and Aldana et al. (2016) finding positive synergies on certain intervening variables—like investment and the adoption of new practices—yet negative impacts of synergies on income. There is also a Brazil study in this book. Garcia et al. (2016) find positive synergies between the Brazilian CCT (Bolsa Família) and a family farmer credit program (Pronaf) on agricultural productivity and agricultural income, but in many cases the synergies only compensate for the negative effect of CCTs on these variables. The authors show a negative correlation between the growth of CCTs and the use of family labor, and hypothesize that this might explain the negative effect.

In this paper, we estimate the impacts of an IFAD-supported rural development project called the Community Development Project for the Rio Gavião Region (Pro-Gavião), and test for policy synergies with the Bolsa Família CCT. The thirteen municipalities where Pro-Gavião took place are among the least developed in Brazil. In the year 2000, these municipalities had an average of around 16,000 people each, 74% of which were rural. Over half of the population in these municipalities was extremely poor, and close to three quarters was poor (Instituto de Pesquisa Econômica Aplicada, 2018). The human development index in these locations was in the bottom third of all municipalities in Brazil in the year 2000, with half of them in the bottom ten percent (United Nations Development Program, 2018).

The empirical approach utilized provides a method for evaluating rural development programs *ex post*, even when baseline and follow-up data were not collected for this purpose at the time. The strategy to identify program impacts, and synergies, relies on a) field work conducted to gather GPS coordinates of the 210 treated communities so that they could be linked with census tracts, b) propensity score matching to create a control group of untreated census tracts, and c) a difference-in-differences estimation with census tract level fixed effects. The models are estimated with average census tract level data on farms under 50 hectares drawn from the 1995-96 and 2006 Agricultural Censuses. The analysis focuses on land productivity, agricultural income, and child labor as outcomes, and credit, investment and electricity as potential channels. These are the relevant variables that are available in the Censuses.

Taken as a whole, the results paint a picture of generally improving conditions in the decade under study, but with little evidence of program impacts or synergies. The unexpected null findings are largely unchanged when heterogeneity of impacts is permitted across differences in initial poverty or the intensity with which census tracks were treated. The results are robust to alternative approaches to identifying the treated census tracts and to different matching techniques. We also rule out adverse rainfall in the treated communities and the influence of other development programs in the control communities as potential explanations for the lack of impacts. While the limitations of our data and approach lead us to view these results as suggestive, albeit important, they are by no means the final word on this subject. We discuss a number of issues that could help to understand the absence of statistically significant results.

In addition to this introductory section, the paper is organized as follows: Section 2 provides an overview of Pro-Gavião and Bolsa Família. Section 3 describes the methodology and data. Section 4 presents the main results, heterogeneous results, and robustness tests. Section 5 discusses seven possible reasons for the null findings, and Section 6 concludes.

2. Background on Bolsa Família and Pro-Gavião

Conditional cash transfer (CCT) and rural development policies grew rapidly in Brazil since the mid-1990s. The first CCT—Bolsa Escola—was introduced in the municipality of Campinas in 1995, and by 2002 had become a federal program operating in nearly all municipalities. The program was modified, unified with other policies, and expanded in 2004 with the creation of Bolsa Família (BF). BF targeted poor families with children or pregnant or nursing women in the household, while those families considered extremely poor received a basic transfer regardless of the composition of their family. BF reached about 14 million families in 2020. Many studies have provided evidence of the positive effects of BF (or Bolsa Escola) on outcomes such as poverty, income inequality, education and child labor (Chitolina et al., 2016); Brauw et al. (2015); Glewwe & Kassouf (2012); Cardoso & Souza (2009); Hoffmann, 2007).

At the same time as the Brazilian government was expanding BF, IFAD was collaborating with the Federal and State governments on a number of rural development projects in the Northeast—the poorest region of the country. Between 1980 and 2021, IFAD supported 13 projects (of which 5 are on-going, planned or approved), providing a total of US$ 278.9 million in finance and benefiting over 615,400 families. The main goal of these interventions is to increase family farmers' production and income by promoting access to essential services such as training, credit and technical assistance, giving special attention to the importance of local organizations, community development, and participation in markets (International Fund for Agricultural Development, 2018).

In this paper, we focus on just one of IFAD's projects in Brazil—Pro-Gavião (PG)—that took place between 1997 and 2005 in the state of Bahia.[1] The project spanned 13 municipalities in the southern part of the state (see Appendix Figure A1), reaching 210 communities and over 17,000 beneficiaries. With a total cost of US$ 40.4 million, shared approximately equally between IFAD and the State Government of Bahia, PG emphasized two lines of action: one that focused on production and another on community development. The first line comprised the creation of producers' associations, agricultural extension, diffusion of technologies appropriate for the semi-arid region, access to credit, and training related to agricultural management, microenterprises, and the elaboration of business plans. Community development involved investments in individual and community infrastructure, such as wells and cisterns, bathrooms, community laundries, dams, expansion of the electrical grid, and other items. Different communities received different components, so some may have had more complete intervention packages than others (Bahia, 2006). We explore this issue empirically by testing for heterogeneity of impacts based on the intensity of treatment.

[1] Among the IFAD projects in Brazil, our choice to evaluate Pro-Gavião was based on data availability and its period of operation. As will be described below, we used Agricultural Census data from 1995-96 and 2006. In order to have baseline data prior to the existence of the IFAD project, we restricted the analysis to projects that began after 1996. We also required that the projects were in operation for a sufficient number of years so that they could generate impacts by 2006.

The Gavião river region was chosen for the project because of its extensive rural poverty (Bahia, 2006). The target population consisted of small agricultural producers, most of whom had incomes below the poverty line. There were, however, no clear criteria for the selection of communities. Field work was conducted in the municipalities to identify the most deprived communities for inclusion, often in terms of infrastructure, but there was considerable discretion involved on the part of program administrators in determining the final list of communities to include.[2]

According to the interim and final reports, both IFAD and the state government of Bahia considered PG to be a successful project. The reports cite considerable achievements on numerous fronts, including community organization, empowerment of women, infrastructure construction, the introduction of technology, facilitating access to credit, boosting the productivity of small herds of animals, and improvements in nutritional status.[3] There was, however, no rigorous evaluation of the program impacts on the beneficiaries using an RCT or based on a methodology that seeks to control for unobservables.

3. Methodology and Data

Our empirical strategy seeks to address the fact that the selection of communities to be included in Pro-Gavião was not random. We first conducted field work to obtain the GPS coordinates of the 210 communities that participated in PG. This allows us to identify the treated census tracts in the Agricultural Censuses. We then use a matching procedure to construct a control group based on observables that has similar pre-intervention characteristics associated with the policy makers' decisions. Because there might be unobservable characteristics that are jointly associated with treatment choice and the outcomes of interest, we also use a difference-in-differences approach. This allows us to remove the influence of unobservable characteristics that do not vary over time.

Construction of the Control Group

We used propensity score matching to identify a control group of census tracts that is similar to the treated census tracts based on observable pre-treatment characteristics. The propensity score is the conditional probability of receiving treatment given a vector of observed pre-treatment variables (Rosenbaum & Rubin, 1983). We estimated a probit model, with the dependent variable equal to one if the census tract participated in PG, and zero otherwise. The explanatory variables included the 1996 levels of variables related to participation in the project (such as the poverty incidence and gap, access to electricity, and agricultural practices) and the baseline outcome variables used in this study. The choice was also based on variable inclusion and exclusion exercises ("hit or miss") to improve the prediction and quality of the model and to ensure balance of the observables (Caliendo & Kopeinig, 2005).[4]

Different criteria can be used to match treated and control observations. We present results based on the five nearest neighbors, and in the section on robustness we show qualitatively similar results with kernel-based matching. With the nearest neighbor approach, each treated

[2] This view is supported by interviews conducted with former PG officials.

[3] An interim evaluation concluded: "Viewing all these elements together, it can be said that the project has had a promising and favourable impact on reducing rural poverty in the Gavião River region" (International Fund for Agricultural Development, 2003, p. xlix). And the final project report concluded: "...the result of Pro-Gavião is strongly positive. And replicating it in a new project called PRODECAR, in a vast region of the Bahian semi-arid, with the support of IFAD...is an unequivocal way to recognize its success" (Bahia, 2006, p. 48).

[4] Balance guarantees that units with identical propensity scores have the same distribution of observable characteristics, regardless of whether or not they are treated (Becker & Ichino, 2002).

unit is matched with the five units in the non-treated group that have the closest propensity scores, with replacement. With kernel-based matching, each treatment unit is matched with a weighted average of all control units, based on weights inversely proportional to the distance of their propensity scores (Becker & Ichino, 2002).

Estimating Program Impacts and their Synergies

We build a panel of census tracts for 1995-96 and 2006 and use a difference-in-differences (DD) estimation to identify the impacts of PG, BF, and their interaction. To control for additional confounders, we use a fixed effects estimator that addresses time invariant unobserved heterogeneity at the level of census tracts.[5] Our main estimating equation is:

$$Y_{st} = \alpha_s + \alpha_1 PG_{st} + \alpha_2 BF_{st} + \alpha_3 BF_{st} * PG_{st} + X_{st}'\varphi + \gamma_t + \varepsilon_{st} \quad (1)$$

where Y_{st} is the average result of interest in census tract s and period t; PG_{st} is a dummy variable that equals zero for all locations in 1996 and one for the PG census tracts in 2006; BF_{st} equals zero in the baseline and then measures the percentage of farm establishments that are beneficiaries of BF in 2006; the term $BF_{st} * PG_{st}$ represents the percentage of establishments that access BF in each census tract treated by PG in 2006; X_{st} refers to a vector of controls that change over time, given in terms of their mean values in census tract s and period t; α_s is the census tract fixed effect; γ_t is the year fixed effect; and ε_{st} is a random error.[6] Coefficient α_3 on the interaction term provides the impacts of the synergies between the two programs. If there are no synergies, then the marginal impact of each program is reduced to α_1 (PG) and α_2 (BF). Because matched control units have different degrees of similarity with the treated census tracts, weights were used that reflect the frequency with which each untreated observation was used as a match. Treated census tracts are unweighted.

The model specified in (1) provides an estimate of average impacts at the level of each census tract. It is quite possible, though, that PG could have generated heterogeneous impacts on census tracts with different characteristics. We hypothesize, for example, that census tracts that were treated more completely should exhibit larger impacts. We create two proxies (described below) for the intensity of treatment and test for heterogeneity. We also hypothesize that differences in the initial level of poverty could lead to significant differences in impacts. Differences in resources can influence households' decisions to participate in a program, by affecting the costs and benefits. These differences can also influence the program's effectiveness. To examine the possibility of heterogeneous effects of PG, for example due to differences in the intensity of program treatment, the following equation is estimated:

$$Y_{st} = \beta_s + \beta_1 PG_{st} + \beta_2 BF_{st} + \beta_3 BF_{st} * PG_{st} + \beta_4 PG_{st} * INT_s + \gamma_t + \varepsilon_{st} \quad (2)$$

Where the dummy that indicates the presence of Pro-Gavião (PG_{st}) in census tract s in 2006 is interacted with a dummy (INT) that represent those census tracts with intensity of treatment above the median. Everything else is as defined in Equation 1. With this specification, we check whether census tracts with greater intensity of treatment were impacted more than those

[5] In cases where the census tract changed, we construct consistent geographical units called minimum comparable areas (AMCs). AMCs were constructed based on digital maps using the ArcGis software. AMCs contain an average of 1.8 census tracts each. For simplicity, in the current discussion we refer to census tracts.

[6] Equation 1 is a standard DD model written in a slightly different form.

where the intensity was less than the median. We also estimate a variation of Equation 2 where instead of interacting *PG* with *INT*, we interact it with a dummy that indicates that the initial level of extreme poverty (*POV*) was above the median in 1996.

Data, Variables and Definitions

The analysis is conducted with data from the 1995-96 and 2006 Agricultural Censuses in Brazil. The sample is restricted to farms under 50 hectares in order to be more consistent with the IFAD target population. This threshold was determined based on an analysis of project documents, information collected in the field, and discussions with government officials in Bahia. Because the census tract coincides more closely with the geographical level at which the IFAD project was implemented, we submitted a special request to the Brazilian Institute of Geography and Statistics (IBGE) to extract the data at this level. In the sample that we extracted, each census tract had around 120 agricultural establishments in 1996. By way of comparison, the PG project had an average of 81 families per rural community.

Each community participating in PG was represented by a single geographical coordinate that we collected during our fieldwork. This point was intended to represent the community center (such as a church, school, association or soccer field). Since residents of rural communities tend to be dispersed, census tracts within a 2.5km radius around each geographical coordinate were considered to be treated by PG.[7] In practice, 95% of the census tracts in the 13 PG municipalities were defined as treated with this approach. As a robustness check, we considered as treated only those census tracts where the exact coordinates were located. The 210 communities were located in 156 rural census tracts that we transformed into 99 AMCs with the radius definition and into 75 AMCs using the point definition.

It is important to consider a group of untreated units as similar as possible to those that were treated. Thus, the 41 municipalities in Bahia located in the vicinity of the 13 PG municipalities provided census tracts that were candidates for matching and that could potentially be included in the control group (see Appendix Figure A1). An initial pool of 334 AMCs from which a control group could be selected was created from the untreated census tracts that belong to the 13 municipalities where PG was located as well as the census tracts from the other 41 nearby municipalities. The PG intervention, however, could have created spillovers to neighboring AMCs. This would be expected with the construction of roads and bridges, and might also happen with the spread of new technologies. If spillovers were important, they would generate a downward bias on the estimated impact of the program as the benefits of the program could leak into the control group. In order to minimize the potential for spillovers to bias the estimates, we excluded all non-treated AMCs from within the 13 PG municipalities and also those AMCs that shared a border with the treated ones. In the end, our control group was drawn from a pool of 288 AMCs.

The three outcome variables that could be constructed with the Agricultural Census data to measure the impact of the program were the log of land productivity, the log of income per adult family worker, and child labor. All variables represent AMC level averages of the farm level data, and all monetary variables are in constant 2006 reais. Land productivity is defined as the total value of all agricultural and livestock divided by the total area of establishments. Income per adult family worker—a measure of the returns to on-farm work—is the value of agricultural and livestock output, minus the value of variable expenditures, divided by the number of adult family members working on establishments. Child labor measures the percentage of

[7] This definition resulted from observations made during the fieldwork.

establishments that employed people under the age of 14 in each AMC. We examined three additional dependent variables that could represent channels through which the program achieved its impacts: access to credit, access to electricity, and on-farm investment. Access to credit is defined as the percentage of establishments that had some form of formal financing in the AMC, whether from private or public banks, while access to electricity is the percentage of establishments in the AMC with electricity on the farm. Investment is measured as the log of average investment per establishment in the AMC.

We began with a long list of potential variables that could be used for the matching or as time-varying controls. These included farm size in hectares, the shares of different types of products in the total value of output—such as livestock, or perennial and annual crops—the use of technical assistance, machines or irrigation, and the incidence of poverty and extreme poverty, as well as their gaps. The poverty measures refer to poverty among agricultural producers, not rural households, and rely solely on agricultural income because total income cannot be measured with the agricultural census data. In essence, they measure the extent to which on-farm income by itself can lift family workers above the poverty line. The poverty line was specified as half a minimum wage per adult equivalent family member, with the extreme poverty line set at one quarter of a minimum wage.[8]

A final point of clarification relates to the BF CCT program in 2006. The 2006 Agricultural Census does not specifically identify receipts from BF. It asks informants if they received transfers from federal, state or municipal government "social programs" and it distinguishes these from social security and pension income. Because BF was the largest social program at this time, it is reasonable to assume that most informants who receive transfers are referring to this program. However, there are other state and municipal programs that provide transfers. For this reason we talk about "social programs" rather than BF in the sections below.

4. Results

Descriptive Statistics and Matching

Table 1 shows descriptive statistics on the main variables used in the study. The PG region is among the poorest in Brazil. Table 1 shows that based solely on agricultural income, over 75% of the farms under 50 ha were extremely poor in 1996. The annual agricultural income generated per adult working on farms in treated AMCs was only around R$553 in constant reais, which converts to less than one dollar per day. Land productivity was considerably lower than the average for the state of Bahia, which in turn had only about half of the national land productivity. The average farm size in the treated AMCs was 17 hectares, and output came mostly from animal production (44%) and annual crops (28%). Agricultural practices and the use of technology were fairly rudimentary in the baseline, as only 15% of the farms had electricity, 4% used machines such as tractors in production, 3% accessed technical assistance, 2% used irrigation, and virtually none had access to credit. 30% of the treated farms, in contrast, used child labor in 1996. Prior to matching, the final column of Table 1 shows that the means of most variables were statistically different between the treatment and control groups.

We now briefly discuss the probit results and the balance tests of means after matching. The details can be found in the Appendix. The dependent variable in the probit equals one if

[8] The poverty lines were based on the minimum wage prevalent in August 2000 so that, as a validation exercise, the poverty measures could be compared to household level rural poverty measured with the 2000 Demographic Census. Municipal level correlations for all of Brazil were in the neighborhood of .80, suggesting that our measure is informative.

the AMC participated in PG and zero otherwise.[9] All explanatory variables are observed in the baseline period. The model was estimated with 99 treated and 288 non-treated AMCs. Among the variables that are statistically significant for matching are farm size, land productivity and the extreme poverty gap, and the use of electricity, machines, and irrigation (Appendix Table A1). The resulting matched sample consists of 96 treated AMCs and 117 controls. The matched sample comprises the most similar treated and control AMCs belonging to the region of common support of the propensity scores, using the five nearest neighbors. Appendix Table A2 shows the difference in means for the matched sample. It also shows the standardized bias between the groups, the percentage reduction in the absolute value of the bias, and p-values for the t-tests of the difference in means. The principal takeaway from this table is that there is a significant reduction in the bias after matching. While many variables exhibited statistically significant differences prior to matching, all of these differences disappear after matching.

Impacts of Pro-Gavião, Bolsa Família and their Synergies

Using the control group created above, we now present the main results for the impacts of PG, BF, and their interaction on three outcome variables: land productivity, income per adult family worker, and child labor. We also explore impacts on three potential channels: investment, credit and electricity. Table 2 shows these results based on the estimation of Equation 1, with and without additional controls. For each dependent variable, the specification in column (1) does not include any additional variables, while the one in column (2) includes time varying controls that are potentially endogenous. The controls include farm size, technical assistance, participation in cooperatives, use of animal traction and irrigation. Although potentially endogenous, we present these results in order to shed light on additional channels through which the main effects might operate. All variables measured in monetary units—productivity, income and investment—are in logs, while the others are in shares.

Table 1. Descriptive Statistics, 1996 (farms under 50ha)

N	Treated AMCs		Non-treated AMCs (non-neighbors)		p-value
	99		288		
	Mean	SE	Mean	SE	
Number of establishments	182.84	188.38	185.43	180.03	0.90
Farm size	17.02	4.71	13.34	4.80	0.00***
Land productivity	130.09	88.77	272.32	461.46	0.00***
Income per adult	553.65	393.74	748.14	1200.17	0.11
Child labor (share)	0.30	0.21	0.26	0.19	0.06*
Access to credit (share)	0.00	0.00	0.01	0.03	0.02**
Investments	241.71	322.66	275.78	1120.96	0.77
Electricity (share)	0.15	0.19	0.13	0.20	0.39
Value of output per estab.	2082.26	1275.83	3755.94	10274.43	0.11
Expenditure per estab.	437.55	305.23	1122.55	3152.46	0.03**
Livestock production (share)	0.44	0.17	0.34	0.21	0.00***
Vegetable production (share)	0.41	0.16	0.49	0.22	0.00***
Vegetable extraction (share)	0.11	0.10	0.04	0.09	0.00***

Note: All monetary values are in Reais of 2006 (R$ 1 = US$0.43). *p<0.10, **p<0.05, ***p<0.01.

[9] The propensity score matching procedure uses the psmatch2 command in Stata, with standard errors calculated with bootstrapping.

Table 1. Continued...

N	Treated AMCs		Non-treated AMCs (non-neighbors)		p-value
	99		288		
	Mean	SE	Mean	SE	
Permanent crops (share)	0.02	0.05	0.08	0.18	0.00***
Temporary crops (share)	0.28	0.12	0.36	0.21	0.00***
Technical assistance (share)	0.03	0.11	0.05	0.12	0.22
Cooperatives (share)	0.01	0.04	0.02	0.07	0.06*
Animal traction (share)	0.39	0.37	0.54	0.35	0.00***
Mechanical traction (share)	0.04	0.07	0.16	0.25	0.00***
Irrigation (share)	0.02	0.04	0.09	0.18	0.00***
Poverty Incidence (share)	0.89	0.11	0.87	0.13	0.05**
Extreme poverty incidence (share)	0.77	0.17	0.75	0.17	0.21
Poverty gap	0.70	0.15	0.68	0.16	0.46
Extreme poverty gap	0.55	0.18	0.55	0.18	0.93

Note: All monetary values are in Reais of 2006 (R$ 1 = US$0.43). *p<0.10, **p<0.05, ***p<0.01.

The most important finding to be highlighted in Table 2 is the absence of any statistically significant effect of PG on the growth of any of the variables. The inclusion of controls does not change any of these results. Land productivity, for example, rose by about 25% in this decade in the treated AMCs, but there is no statistically significant evidence that it rose more rapidly than in the control group. Neither PG nor social programs—whether in isolation or their interaction—significantly affected the average growth of land productivity, income per adult or the share of establishments using child labor. This is a surprising finding.

Because we did not find any statistically significant impacts on the three main outcome variables, we decided to only briefly present the analysis of channels in the appendix. Appendix Table A3 shows a similar absence of any significant impacts of PG on the amount invested or on access to credit and electricity. Access to credit, in contrast, was significantly affected by the incidence of social programs. This could be because participation in a social program leads to greater income stability, reliability for planning, and the possibility of contact in the case of credit arrears. The Table also shows that the interaction between PG and social programs had a positive and statistically significant effect on access to electricity. The increase in access to electricity was substantial in this period, as the share of farms with electricity increased from under 15% to around 60% in both treated and control AMCs. This reflects the priority given to certain policies—such as the Light for Everybody program—and the general expansion of electrical power networks in this period. We suspect that the estimated effect indicates an association, but not necessarily a causal impact.

Table 2. Effects of Pro-Gavião, Social Programs and their Interaction on Land Productivity, Income and Child Labor

	Land Productivity		Income per adult		Child labor	
	(1)	(2)	(1)	(2)	(1)	(2)
Pró-Gavião	0.16	0.30	0.001	0.22	10.41	10.70
	(0.28)	(0.28)	(0.38)	(0.38)	(9.73)	(9.19)

Notes: Agricultural controls include: farm size, technical assistance, participation in cooperatives, use of animal traction and irrigation. AMC level clustered standard errors in parentheses. *p<0.10, **p<0.05, ***p<0.01.

Table 2. Continued...

	Land Productivity		Income per adult		Child labor	
	(1)	(2)	(1)	(2)	(1)	(2)
Social programs incidence	-0.001	0.01	-0.01	0.00	0.13	0.10
	(0.004)	(0.004)	(0.01)	(0.01)	0.17	(0.17)
Interaction between the programs	-0.01	-0.01	-0.003	-0.01	-0.22	-0.19
	(0.01)	(0.01)	(0.01)	(0.01)	(0.24)	(0.24)
Agricultural controls	N	Y	N	Y	N	Y
Time dummy	Y	Y	Y	Y	Y	Y
Fixed effects	Y	Y	Y	Y	Y	Y
R2	0.00	0.12	0.01	0.01	0.19	0.17
N	426	426	388	388	426	426

Notes: Agricultural controls include: farm size, technical assistance, participation in cooperatives, use of animal traction and irrigation. AMC level clustered standard errors in parentheses. *p<0.10, **p<0.05, ***p<0.01.

Exploring Potential Heterogeneity

The results presented thus far refer to the average impact of the programs on the treated AMCs. However, an important question concerns the possibility that Pro-Gavião might have had differential impacts depending on the intensity of treatment or the initial level of poverty of each location. The estimation of Equation 2 allows us to test for the existence of heterogeneous impacts. The intensity of treatment variables (INT) equal one when an AMC is above the median for either access to technical assistance or the share of families that benefited from PG infrastructure and construction programs.[10] The poverty intensity variable (POV) equals one when extreme poverty in the AMC is above the median.

When intensity of treatment is measured by the share of families with access to technical assistance, Table 3 shows that we continue to find no evidence of positive treatment effects on land productivity, income or child labor. The results are similar when intensity of treatment is measured by the share of families that benefited from PG infrastructure and construction projects. There is only a single coefficient on the interaction term PG*INT that is statistically significant at the 10% level. But it is negative, suggesting that income may have grown more slowly in the locations that were treated with greater intensity. Finally, when we explore heterogeneity based on baseline extreme poverty, we find more evidence of heterogenous impacts, but continue to find no statistically significant evidence of *positive* program effects. The results suggest that child labor was rising relative to the control group in the treated locations with poverty below the median, but was no different than the control group in the high poverty AMCs.[11] We also find evidence of heterogeneity for the outcome variable income per adult, with income rising faster in the poorer treated locations than in the less poor treated AMCs. But an F-test of the sum of the coefficients fails to reject the null that this sum equals zero at a 5% level of significance, suggesting that even in the poorer AMCs income growth was no different than in the control locations.

[10] We also experimented with access to credit (rather than technical assistance), and the value of spending on infrastructure and construction (rather than their shares). The results were qualitatively similar because the correlation coefficient between the variables used and these alternatives were both above 0.75.

[11] The coefficient on PG is 21.33 and the coefficient on PG*POV is -20.96. Both are significant at least at the 5% level. But an F-test that the sum of the two coefficients equals zero is not rejected at the 5% level of significance.

Table 3. Heterogeneous Effects of Pro-Gavião

	Land Productivity			Income per adult			Child labor		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
Pro-Gavião	0.26	0.26	0.04	0.09	0.19	-0.47	12.84	10.36	21.33
	(0.30)	(0.30)	(0.30)	(0.40)	(0.41)	(0.40)	(9.86)	(9.78)	(9.99)
PG*technical assistance above the median	-0.21	-	-	-0.21	-	-	-5.66	-	-
	(0.17)			(0.25)			(5.37)		
PG*infrastructure and construction above the median	-	-0.25	-	-	-0.49*	-	-	0.14	-
		(0.16)			(0.25)			(5.34)	
Pro-Gavião* extreme poverty above the median			0.24			0.85***			-20.96***
			(0.18)			(0.24)			(4.70)
Social programs incidence	-0.00	-0.00	-0.00	-0.01	-0.01	-0.01	0.13	0.13	0.13
	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.18)	(0.18)	(0.18)
Interaction between the programs	-0.01	-0.01	-0.01	-0.00	-0.00	-0.00	-0.22	-0.22	-0.24
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.24)	(0.24)	(0.23)
Time dummy	Y	Y	Y	Y	Y	Y	Y	Y	Y
Fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
R2	0.03	0.03	426	0.07	0.09	0.14	0.42	0.41	0.47
N	426	426	0.04	388	388	388	426	426	426

Notes: Robust standard errors in parenthesis. *p<0.10, **p<0.05, ***p<0.01.

Regarding the channels, Appendix Table A4 shows that there is a single coefficient—significant at 10%—that suggests weak heterogeneity in the impact of PG on investment. However, an F-test of whether the sum of the coefficients on PG and PG*INT is different from zero fails to reject the null. Thus, even in the locations that were treated with greater intensity we are unable to conclude that the impact on investment was positive.

The empirical findings presented thus far suggest that there is no statistically significant evidence for positive impacts of PG, or of synergies between the two programs, on the main outcome variables studied. Thus, AMCs that benefited from PG, or both programs, do not appear to have had superior outcomes related to the growth of land productivity and income, or the reduction of child labor. Similarly, AMCs that were treated with greater intensity—measured either by technical assistance or infrastructure and construction projects—show no signs of performing better. We did find evidence of heterogeneity according to baseline poverty rates, but not by enough to produce statistically significant positive program impacts.

Robustness Checks

In order to evaluate the robustness of the main findings, we present estimates of the impacts of each program and their interaction from a set of tests that we conducted. Results from these robustness checks are shown in Table 4 where we explore a) the identification of treated AMCs based on the exact coordinates rather than the 2.5km radius (Panel A); b) the use of kernel matching rather than five nearest neighbors (Panel B); and c) an alternative approach to aggregating census tracts in order to construct AMCs (Panel C).

The main estimates provided in this paper consider as treated by PG the AMCs located within a 2.5km radius around the geographical coordinates of the communities. It is possible that using a 2.5km radius may be too noisy. As a robustness test, in Panel A of Table 4 we define treated AMCs based on their exact coordinates. When we do this, we end up with 75 treated AMCs rather than 99. With the exception of income, the results remain qualitatively similar. In particular, there is no evidence that PG has favorably impacted land productivity or child labor. In the case of income, both PG and social programs now have negative and significant coefficients. This suggests that the 24 AMCs that were included with the 2.5km approach may have performed better than 75 that are defined as the center of the community. As a result, we have less confidence in whether these coefficients are actually zero, or perhaps negative. Most importantly, we find no evidence that they are positive.

The choice of matching criterion could also influence the results. Choosing the five nearest neighbors may enhance the comparison of more similar census tracts, but reduces the matched sample size and thus affects the statistical power of the exercises. Panel B of Table 4 shows the estimated coefficients that result from using the kernel matching procedure with the entire sample rather than the five nearest neighbors. The procedure is implemented using our original definition of treated AMCs based on the 2.5km radius. Again, the estimated coefficients are qualitatively similar to what was presented in Tables 2 and 3. The only difference is the coefficient on social programs when income is the dependent variable. It continues to be negative and small (-.01), but it is now significant at the 10% level. As with the exact coordinates, this suggests the possibility that the AMCs where social programs had more penetration may have experienced slightly slower income growth.

Table 4. Robustness Checks

	Land Productivity	Income per adult	Child labor
Panel A: Exact coordinates			
Pro-Gavião	-0.05	-0.65**	4.96
	(0.27)	(0.32)	(5.92)
Social programs incidence	0.00	-0.01**	-0.01
	(0.004)	(0.01)	(0.12)
Interaction between the programs	0.00	0.01	-0.06
	(0.01)	(0.01)	(0.15)
N	396	359	396
Panel B: Kernel matching			
Pro-Gavião	-0.05	-0.22	11.64
	(0.27)	(0.38)	(8.35)
Social programs incidence	0.00	-0.01*	0.18
	(0.004)	(0.01)	(0.13)
Interaction between the programs	0.00	0.00	-0.26
	(0.01)	(0.01)	(0.21)
N	768	704	768
Panel C: IBGE AMCs			
Pro-Gavião	-0.09	-0.41	9.85
	(0.23)	(0.33)	(6.41)

Notes: AMC level clustered standard errors in parentheses. *p<0.10, **p<0.05, ***p<0.01.

Table 4. Continued...

	Land Productivity	Income per adult	Child labor
Social programs incidence	0.00	-0.01**	0.11
	(0.003)	(0.01)	(0.12)
Interaction between the programs	0.00	0.01	-0.31*
	(0.01)	(0.01)	(0.18)
N	576	517	576
Time dummy	Y	Y	Y
Fixed effects	Y	Y	Y

Notes: AMC level clustered standard errors in parentheses. *p<0.10, **p<0.05, ***p<0.01.

The construction of minimum comparable areas is based on the manipulation of digital maps, and it is possible that the process may be subject to small aggregation errors. In order to check the robustness of the results to the process that was used, an alternative way of constructing AMCs was considered. Panel C of Table 4 shows the estimated coefficients when using AMCs defined on the basis of an aggregation routine provided by IBGE.[12] We note that this approach results in a larger number of AMCs, which could contribute to the precision of the estimates. As before, the approach uses the five nearest neighbors for matching. Again, most of the results are robust. The few exceptions are the coefficient on the interaction term for child labor that becomes significant at 10%, and the coefficient on social programs in the income regression which is small, negative and significant as in Panels A and B.

Taken as a whole, the robustness checks confirm the results found previously. We conclude that there is no evidence of a positive impact of PG on land productivity, income or child labor relative to the control locations, and no evidence of a positive synergistic effect of the two policies on the main outcomes studied in this paper.[13]

5. Discussion

The finding that PG and the interaction between the programs had no statistically significant positive impact on the main outcomes studied in this paper—even in the models that allow for heterogeneity—represents an unexpected null result that raises a number of questions. In this section, we address seven possible explanations for these findings: i) the influence of other programs in control AMCs, ii) adverse rainfall shocks in treated AMCs, iii) a lack of power, iv) the data, v) the setting, vi) the design and implementation of the policies, and vii) the possibility that the findings are correct.

i) The Influence of Other Programs

One potential explanation for finding no impact of Pro-Gavião is that there were other rural development programs taking place in Bahia at the same time, and these might have differentially benefited the control AMCs. The World Bank, for example, invested heavily in rural poverty alleviation programs throughout the Northeast of Brazil in this period. Because IFAD was investing in these 13 municipalities, other programs might have left these locations alone and targeted other—almost as needy—municipalities. This would imply that our control group would not have represented the counterfactual of zero program intervention, but rather

[12] The main results in the paper use our own aggregation which we prefer because we detected a number of inconsistencies in IBGE's routine.

[13] The only possible exception is the interaction term on child labor which is significant at the 10% level in one of the three robustness exercises.

the counterfactual of no PG intervention. This would alter the interpretation of our results. To address this issue, we were able to gather administrative data from the state government of Bahia on spending in PG and neighboring locations. Although it was not possible to verify this hypothesis for all 54 municipalities analyzed, we did succeed in obtaining information from 13 PG municipalities and 12 of the closest neighbors.

There were four different programs in the region that targeted rural areas: PRODUZIR, PRODUZIR II, PRODUZIR III and PRODECAR.[14] Appendix Figure A2 shows the average spending per community by these programs, and by PG, in each municipality in the period between 1996 and 2006. The data show that the cumulative amount spent by other programs per community was roughly similar in the treated versus control municipalities. The average for the complete set of 25 municipalities was close to R$100,000 per community. Moreover, PG spent an additional R$208,000 per community on average in the treated municipalities. As a result, spending per rural community in the treated municipalities was triple what it was in the control municipalities. We conclude that the presence of other rural development programs was unlikely to be the reason for finding no impacts of PG.

ii) Adverse Rainfall Shocks

A second possible explanation for a lack of positive impacts is that 1995-96 or 2006 could have been years of adverse weather in the PG AMCs that did not occur in the control AMCs, thus biasing the estimated impact of the program. In order to test this hypothesis, we used monthly data described in Willmot & Matsuura (2001) to construct municipal level deviations from a 25-year moving average of quarterly rainfall. Because many crops are planted in the months prior to the harvest that is captured in the census, our data cover six quarters for each census, including the two quarters prior to the reference period of the census. The rainfall deviations reveal that 1995-96 was a relatively normal year for both the PG and control municipalities. For both groups, 80% of the deviations—measured by municipality and quarter—fell in the middle 80% of the historical distribution of deviations. Equally as important, in five of the six quarters there was no statistically significant difference in rainfall shocks between the two groups. Thus, the baseline data in our study appear to be drawn from a relatively normal year for rainfall in both groups.

The 2006 census data also seem to have been drawn from a relatively normal year for rainfall. 83% of the deviations for both groups fell in the middle 80% of the distribution of deviations, with only 17% of rainfall shocks in the top or bottom 10% of deviations. In contrast to the baseline, five of the six quarters exhibited statistically significant differences in rainfall deviations across groups, but in all cases PG had more rain rather than less, without being excessive. The one quarter where there might have been excessive rain—in the top 10% of the distribution of shocks—it affected both the PG and control municipalities equally, with no statistically significant difference between them. Thus, we conclude that both the baseline and follow-up periods were relatively normal years for rainfall, and if anything 2006 was a somewhat better year in the treated than the control locations. Differential rainfall does not appear to explain the lack of impact of the PG intervention.

iii) Lack of power

It is possible that low power due to the small size of the sample could be affecting our inference. To shed light on this issue, we conducted nonparametric permutation tests similar

[14] The Portuguese word produzir means “to produce.” Produzir, Produzir II and Produzir III were stages of a broad program for reducing rural poverty, which was the result of a partnership between the state government of Bahia and the World Bank. The program took place between 1995 and 2014.

to Chetty et al. (2009) and Dell & Querubin (2018) that allow us to calculate an empirical distribution of placebo effects. This was done by randomly assigning treatment (participation in PG) to 99 AMCs in order to estimate the impact of the program placebo on the dependent variables. Each time this was done, we first randomly assigned the 99 AMCs, then ran the propensity score matching model to create a control group, and finally estimated the fixed effects DD models described in (1) and (2). The exercise was repeated 1000 times, generating 1000 sets of placebo coefficients. The share of placebo coefficients that are larger in absolute value than what was estimated for a given coefficient provides the empirical p-value of the null hypothesis that the coefficient equals zero.

Table 5 shows the empirical p-values obtained from the permutation tests for the coefficients that measure the direct impact of PG. Four models were estimated.[15] Panel A shows the results from the model using Equation 1. The estimated coefficients from Table 2 are reproduced here, with the empirical p-values shown beneath them. Panels B, C and D show the results that were estimated with Equation 2, allowing for heterogeneity by baseline poverty or intensity of treatment. As in panel A, we reproduce the coefficients that were estimated above (Table 3) and show the empirical p-values beneath them.

Panel A shows that the empirical p-values lead to conclusions that are similar to what was obtained from Table 2 other than for one coefficient which is now statically different than zero at the 1% level. However, the coefficient suggests *negative* program effects. Child labor now appears to rise (or fall more slowly) in the treated AMCs. Thus, the main conclusion remains unchanged: we find no statistically significant evidence for positive program impacts of PG.

Table 5. Permutation tests

		Land Productivity	Income per adult	Child labor
		Panel a: Main model		
Pro-Gavião	Coeficient	0.16	0.00	10.41
	P-value	0.13	0.95	0.00
		Panel b: Heterogenous effects by initial level of extreme poverty		
Pro-Gavião	Coeficient	0.04	-0.47	21.33**
	P-value	0.82	0.04	0.00
PG*extreme poverty above the median	Coeficient	0.24	0.85***	-20.96***
	P-value	0.22	0.01	0.00
		Panel c: PG intensity (technical assistance)		
Pro-Gavião	Coeficient	0.26	0.09	12.84
	P-value	0.10	0.69	0.01
PG*technical assistance above the median	Coeficient	-0.21	-0.21	-5.66
	P-value	0.23	0.39	0.26
		Panel d: PG intensity (construction of infrastructure)		
Pro-Gavião	Coeficient	0.26	0.19	10.36
	P-value	0.10	0.42	0.02
PG*infrastructure above the median	Coeficient	-0.25	-0.49*	0.14
	P-value	0.10	0.02	0.97

Notes: Robust standard errors in parenthesis. Notes: Robust standard errors in parenthesis. *p<0.10, **p<0.05, ***p<0.01.

[15] Although we restrict attention to PG, the models include the same variables as in Tables 2 through 4.

The conclusions obtained from the p-values in panel B are also similar to what we found in Table 3. Child labor rises in the less poor AMCs relative to the control group, and this is offset in the poorer AMCs. The coefficients on income per adult are now both significant at least at the 5% level, and we are unable to conduct an F-test as we did previously. The point estimate of the sum of the two coefficients is positive for the poorer AMCs. Thus, we conclude that the evidence is suggestive of a positive impact for these AMCs.

Panels C and D suggest that we continue to find no positive impacts of PG, even in those AMCs that were treated with more intensity. When intensity is proxied for with access to technical assistance, none of the coefficients are significant on the interaction terms that captures greater intensity. When the proxy for intensity of treatment is the number of families benefited by infrastructure and construction projects, there is now a significant coefficient on income per adult in the AMCs treated more intensively. However, the coefficient is negative, suggesting that income grew more slowly in these AMCs. In sum, the permutation tests largely corroborate our main conclusion that we find no positive effects of PG on the six variables studied. The only possible exception is the growth of income in treated AMCs that had baseline extreme poverty above the median.

iv) Other Limitations of the Data

Another possibility is that there were in fact impacts, but they were on outcomes that we were not able to measure. It was only possible to evaluate outcomes that could be measured in the Agricultural Censuses, and even among these there were limitations. For example, PG may have helped farmers to cope better with the risks that they face by providing technical assistance and disseminating new technologies in the semi-arid region. Although we couldn't find any positive impacts on the growth of land productivity or income, it is possible that there was a reduction in the variance of agricultural production over time. With data solely on a single follow-up period, it was not feasible for us to study this issue. It is also possible that other dimensions of well-being may have been affected. The components of PG that encouraged participation in training events, or the creation of associations and common processing centers, may have been responsible for improvements in the human and social capital of the beneficiaries, or of non-agricultural sources of income. But these are not variables that could be measured with the Agricultural Censuses.

v) The Setting

A fifth possibility relates to the harsh environmental and economic setting of the Gavião region. Favareto & Seifer (2013) identify a number of structural factors that could limit the success of rural development programs in the semi-arid region. These relate to i) environmental restrictions, ii) unequal economic structures, including high land concentration, insecurity of the poor, and a lack of opportunities to participate in markets, and iii) cultural and political-institutional constraints. Market failures also create obstacles, and these may be responsible for a lack of response by households to public policies. Janvry & Sadoulet (2005) suggest that even if certain policies relax constraints in particular markets, the ability of agricultural households to change their behavior may be constrained by imperfections that remain in other markets. The difficulty with pointing to structural constraints—whether they derive from inequality, the environment, or market failures—is that it is not clear what this implies for policy. Some analysts might conclude that the interventions were appropriate, but insufficient, others might infer that they did not target the appropriate constraints, while yet others might suggest that the environment is simply to adverse for these programs to succeed. Without solid evidence of program impacts, and how the relaxation of specific constraints could contribute to program success, it is difficult to differentiate between these competing conclusions.

vi) The Design and Implementation of the Policies

According to Devereux (2016), potential synergies between social protection and rural development policies are limited by the fact that these are not well articulated conceptually, nor are they reflected clearly in policy agendas. In the case of PG, we believe that the absence of significant interaction effects could be a result of the way in which the policies were designed and implemented. We conducted interviews with approximately 30 officials involved in running BF, PG, and other rural development programs in the Northeast of Brazil in order to analyze their perceptions about the interaction between these programs. Although many respondents believe that synergistic effects are likely, they agreed that there was generally little or no coordination in the design and implementation of the policies. There may be legal or administrative restrictions that impede the sharing of information, but there are also political obstacles to policy coordination, with their roots in the individual logic of politicians and the heterogeneous governing coalitions that are often formed.

Another related explanation for the lack of synergies has to do with the sequencing of the policies and the duration of overlap. Bolsa Família was only created in 2004, although it consolidated and expanded pre-existing programs like Bolsa Escola which became a federal program in 2002. Thus, it is possible that synergistic effects were dampened because the CCT was only present during the second half of the PG project. While this is possible, Garcia et al. (2016) do find evidence of synergies between CCTs and the family farm credit program (Pronaf) in Brazil, and Macours et al. (2012) find positive synergies in Nicaragua from a pilot project that only lasted for one year. In our case, it seems likely that an overlap of at least three years should have been sufficient to generate impacts.

vii) The Findings Might be Correct

In spite of the many reasons why there might actually be an impact, even though we were unable to detect one, it is nonetheless a rather astonishing result to find zero positive impacts of the Pro-Gavião program on almost all outcomes that we were able to measure, and little robust evidence of policy synergies. While the null results estimated here are more suggestive than definitive, they underscore the need to plan well-designed impact evaluations—based on household level data—long before the rural development programs begin.

6. Conclusions

Despite having provided US$18.5 billion in grants and low-interest loans since 1977, there is little rigorous evidence on the impact of IFAD projects around the world. There is a similar dearth of evidence on synergies between rural development projects and conditional cash transfer policies. In an effort to address this gap in the literature, we explored the impacts of an IFAD-supported rural development project—Pro-Gavião—in 13 municipalities of Brazil, and possible synergies with the Bolsa Família conditional cash transfer program. The paper used a matching technique to create a control group of untreated census tracts, and a difference-in-differences estimation to identify policy impacts. The findings were unexpected. When examining the main outcomes of land productivity, agricultural income, and child labor—all available in the Agricultural Censuses—we found no statistically significant evidence to support a positive impact of PG or of synergies between the two programs. The presence of BF seems to have improved access to credit, and there was some evidence showing a likely association between the interaction of the policies and improved access to electricity. These results are mostly robust to different matching techniques, ways of defining the treated locations, and heterogeneity by intensity of PG treatment and the initial level of poverty.

The paper discussed possible explanations for these null results. These fell into four broad categories. First, it is possible that policies did in fact have impacts, but we were unable to measure them with the data and methods employed. A reduction of risk, for example, was not something that we could measure with a single year of post-intervention data. Second, it is possible that the soil, climate, and economic environments are so adverse in this region that it is extremely difficult for rural development interventions to succeed. Third, there could be omitted variables that confound program impacts. We were able to discard two potential candidates: adverse rainfall shocks in the treated communities, and superior access to other rural development programs in the control locations. Finally, because these policies were not designed to be complementary, and were implemented independently of each other, it is possible that the synergistic effects were dampened.

Two lessons from this study are clear. First, many policy makers, program administrators and researchers believe that conditional cash transfers and rural development interventions are likely to have enhanced impacts when implemented in tandem. As our results suggest, the evidence on this issue remains unclear. Nonetheless, it is likely that in order to fully exploit potential synergies—where they exist—policies need be designed and implemented with these complementarities in mind. Enhancing the coordination of policies would likely reduce duplication, align incentives, and increase impacts. Second, while we have devised an approach to estimating impacts *ex post* in this particular setting, rural development interventions should build in impact evaluations from the start so that a wide variety of outcomes can be measured at the household level and evaluated with a rigorous methodology. In this regard, although provocative, our results are more suggestive than definitive.

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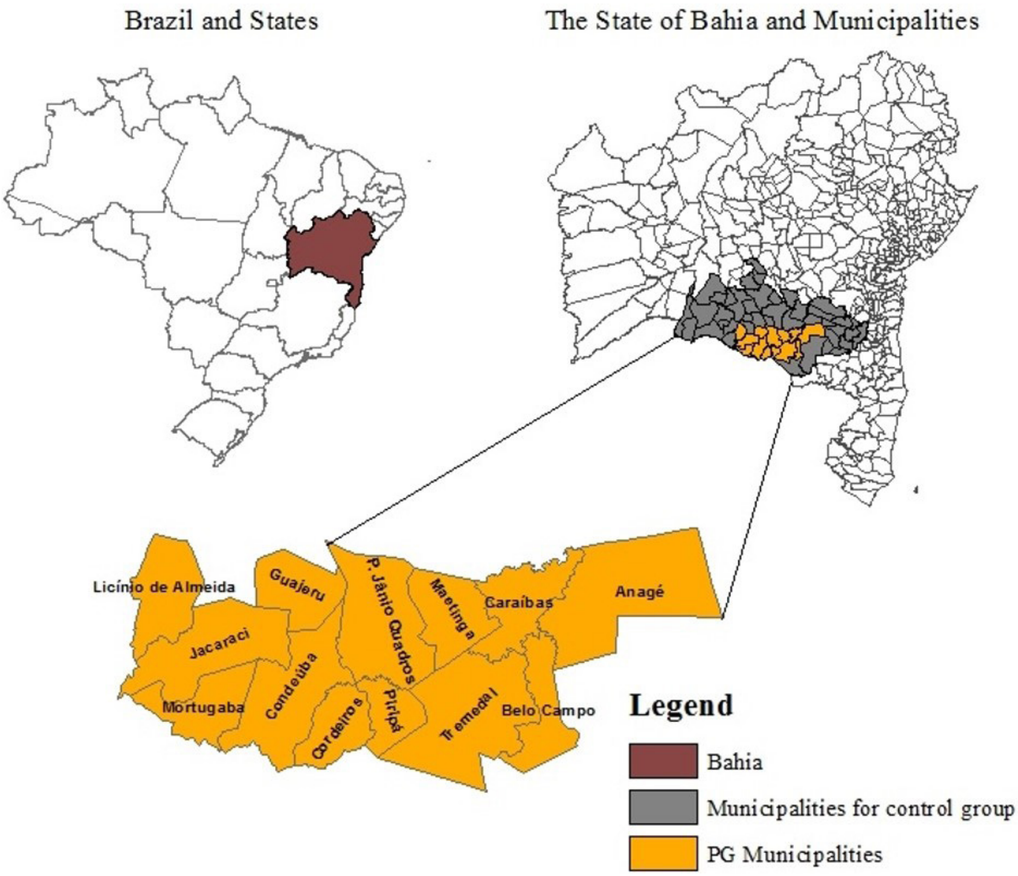
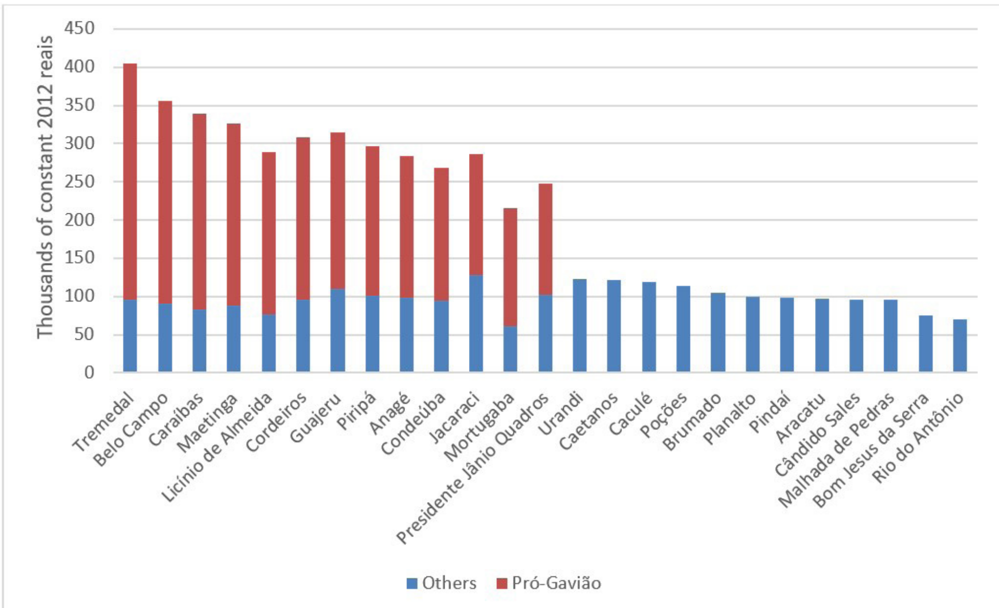


Appendix Figure A1. Brazil, State of Bahia, Pro-Gavião Municipalities and Municipalities for Control Group



Appendix Figure A2. Average Spending per Community by Pro-Gavião and Other Rural Programs (1996-2006)

Appendix Table A1. Probit Results for Participation in Pro-Gavião (1996)

Variables	
Farm size	0.057***
	(0.020)
Land productivity	-0.002**
	(0.001)
Access to credit (share)	-16.686
	(14.681)
Investments per establishment	0.001***
	(0.0002)
Livestock production (share)	1.575***
	(0.499)
Vegetable extraction (share)	3.384***
	(0.810)
Permanent crops (share)	-0.926
	(1.277)
Technical assistance (share)	-0.219
	(0.882)
Cooperatives (share)	2.697
	(2.229)
Electricity (share)	1.674***
	(0.470)
Mechanical traction (share)	-3.628***
	(1.183)
Irrigation (share)	-4.595***
	(1.704)
Extreme poverty gap	-1.690***
	(0.602)
Constant	-0.962
	(0.691)
N	387
LR chi2	144.870
Prob>chi2	0.000
Pseudo R2	0.329

Note: *p<0.10, **p<0.05, ***p<0.01.

Appendix Table A2. Tests of Means Between Treated AMCs and Non-treated AMCs After Matching

Variables	Mean		% bias	% reduction	p-value
	Treated	Controls		\bias\	
Farm size	16.92	16.94	-0.30	99.60	0.98
Land productivity	130.51	132.74	-0.70	98.40	0.86
Access to credit (share)	0.00	0.00	-1.60	95.00	0.54
Investments per establishment	237.10	211.69	3.10	25.40	0.55
Livestock production (share)	0.44	0.41	19.50	64.60	0.14
Vegetable extraction (share)	0.11	0.13	-21.50	72.20	0.34
Permanent crops (share)	0.02	0.01	4.50	90.40	0.37
Technical assistance (share)	0.03	0.02	13.10	12.10	0.26
Cooperatives (share)	0.01	0.01	6.20	74.90	0.50
Electricity (share)	0.14	0.15	-8.00	20.70	0.60
Mechanical traction (share)	0.04	0.05	-7.30	88.40	0.21
Irrigation (share)	0.02	0.01	2.40	95.50	0.57
Extreme poverty gap	0.55	0.54	6.70	-523.40	0.65
Rubin's R Unmatched	0.1				
Matched	1.04				

Appendix Table A3. Effects of Pro-Gavião, Social Programs and their Interaction on Investment, Credit and Electricity

	Investment		Access to credit		Access to electricity	
	(1)	(2)	(1)	(2)	(1)	(2)
Pró-Gavião	-0.13	0.19	0.15	0.12	-6.75	-11.27
	(0.84)	(0.80)	(2.69)	(2.51)	(9.67)	(8.37)
Social programs incidence	-0.01	-0.01	0.21***	0.15***	-0.09	-0.29*
	(0.02)	(0.02)	(0.06)	(0.05)	(0.17)	(0.15)
Interaction between the programs	0.00	0.00	-0.06	-0.03	0.43*	0.55**
	(0.02)	(0.02)	(0.08)	(0.07)	(0.24)	(0.22)
Agricultural controls	N	Y	N	Y	N	Y
Time dummy	Y	Y	Y	Y	Y	Y
Fixed effects	Y	Y	Y	Y	Y	Y
R2	0.11	0.10	0.67	0.68	0.54	0.48
N	299	299	426	426	426	426

Notes: Agricultural controls include: farm size, technical assistance, participation in cooperatives, use of animal traction and irrigation. AMC level clustered standard errors in parentheses. *p<0.10, **p<0.05, ***p<0.01.

Appendix Table A4. Heterogeneous Effects of Pro-Gavião on Investment, Credit and Electricity

	Investment			Credit			Electricity		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
Pro-Gavião	-0.29	-0.13	-0.14	-0.18	1.00	0.28	-8.45	-9.61	-8.85
	(0.85)	(0.84)	(0.85)	(2.72)	(2.72)	(2.67)	(10.05)	(9.72)	(9.91)
PG*technical assistance above the median	0.66*	-		0.76	-		3.97	-	
	(0.33)			(1.58)			(5.25)		
PG*infrastructure and construction above the median	-	0.21		-	-2.27		-	7.56	
		(0.35)			(1.60)			(5.32)	
Pro-Gavião* extreme poverty above the median			0.06			-0.27			4.03
			(0.35)			(1.52)			(5.14)
Social programs incidence	-0.01	-0.01	-0.01	0.21***	0.21***	0.21***	-0.09	-0.09	-0.09
	(0.02)	(0.02)	(0.02)	(0.06)	(0.06)	(0.06)	(0.17)	(0.17)	(0.17)
Interaction between the programs	-0.00	-0.00	-0.00	-0.06	-0.06	-0.06	0.43*	0.43*	0.43*
	(0.02)	(0.02)	(0.02)	(0.08)	(0.08)	(0.08)	(0.24)	(0.24)	(0.24)
Time dummy	Y	Y	Y	Y	Y	Y	Y	Y	Y
Fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
R2	0.19	0.17	0.17	0.80	0.80	0.80	0.76	0.76	0.76
N	299	299	299	426	426	426	426	426	426