Working Paper | 489

CMICRO - Nº37
Working Paper Series
Setembro de 2018

FGV | SAO PAULO SCHOOL OF ECONOMICS

No Impact of Rural Development Policies? No Synergies with Conditional Cash Transfers? An Investigation of the IFAD-Supported Gavião Project in Brazil

Lorena Vieira Costa
Steven M. Helfand
André Portela Souza

No Impact of Rural Development Policies? No Synergies with Conditional Cash Transfers? An Investigation of the IFAD-Supported Gavião Project in Brazil[1]

Lorena Vieira Costa
Federal University of Viçosa
lorenavclelis@gmail.com

Steven M. Helfand
University of California, Riverside
steven.helfand@ucr.edu

André Portela Souza
Getulio Vargas Foundation, São Paulo
andre.portela.souza@fgv.br

9/18/18

Abstract: Public policies frequently are implemented simultaneously rather than in isolation. We estimate the impacts—and possible synergies—of a rural development project (Pro-Gavião) and the Brazilian conditional cash transfer program (Bolsa Família). In partnership with the State Government of Bahia, Pro-Gavião was an IFAD-supported 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 of impacts by initial level of poverty. 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.

Key words*:* Rural development projects, Conditional Cash Transfers, IFAD, Synergies, Brazil.
JEL Codes: O13, Q1.

---

[1] This research was supported in part by a grant from the International Fund for Agricultural Development (IFAD) to the University of the Andes for a project on conditional cash transfers and rural development in Latin America. We thank them for this support. We also thank the Brazilian CNPq and Capes for financial support of Costa's dissertation research, Fernanda de Alcântara Viana who was responsible for the field work in Bahia, and IBGE for access to the Agricultural Census microdata. The views expressed here are solely those of the authors.

1. Introduction

Rural development interventions in developing countries are extremely heterogeneous. The menu of interventions includes infrastructure projects, credit, technical assistance, and 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" (IFAD, 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 development projects that have reached close to 500 million people (IFAD, 2018). Yet there is little rigorous empirical evidence of IFAD program impacts. Brown and 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, 2008; de 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, they 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 (de Janvry and Sadoulet, 2005; Singh *et al.*, 1986). 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, 2009). 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 (Zeng *et al.*, 2017). 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; de Brauw, 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 *et al.*, 2017; Carter *et al.*, 2014; 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 since 2013 has released several reports that conduct *ex post* impact evaluations of recent projects. These efforts, however, have been hampered by a lack of baseline data (IFAD, 2016b; IFAD, 2015; IFAD, 2013).

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 agricultural 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 and a family farmer credit program 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 be the channel for the negative effect. One experimental study that attempts to measure synergies comes from Macours *et al.* (2012) on Nicaragua. They show positive synergies between CCTs and either an investment grant or a training program on the ability of households to cope with drought shocks two years after the intervention.

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 Brazilian CCT Bolsa Família. 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 (IPEA, 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 (UNDP, 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 individual 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 in Brazil. The analysis focuses on land productivity, agricultural income, and child labor as outcomes, and credit, investment and access to 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 robust to alternative approaches to identifying the treated census tracts, matching techniques, and heterogeneity of impacts by initial level of poverty. We also rule out adverse rainfall in the

treated communities, and the influence of other 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 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

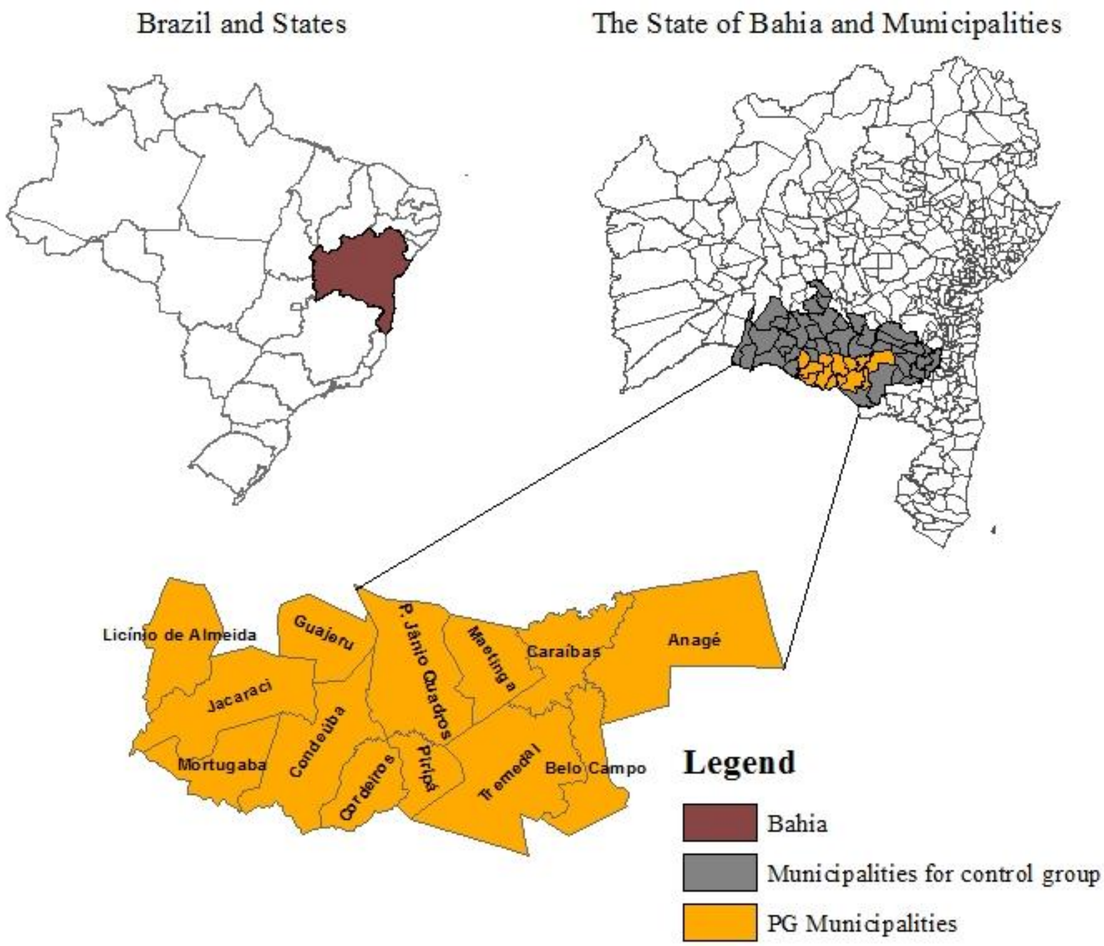
Conditional cash transfer and rural development policies grew rapidly in Brazil since the mid-1990s. The first conditional cash transfer policy—Bolsa Escola—was introduced in the municipality of Campinas in 1995, and by 2002 had become a federal program operating in nearly all Brazilian municipalities. The program was modified, unified with other smaller policies, and expanded in 2004 with the creation of Bolsa Família (BF). In its current form, poor families are eligible if there are children, or pregnant or nursing women in the household, while those families considered extremely poor receive a basic transfer regardless of the composition of their family. BF currently reaches about 13 million families. Many studies have provided evidence of the positive effects of the BF (or Bolsa Escola) on outcomes such as poverty, income inequality, education and child labor (Chitolina *et al.* (2016); de Brauw et al (2015); Glewwe and Kassouf (2012); Barros *et al.* (2010); Cardoso and Souza (2009); Hoffmann (2007)).

At the same time as the Brazilian government was expanding Bolsa Família, 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 2018, IFAD supported 13 projects (of which 8 are on-going or approved), providing a total of US$ 299.4 million in finance and benefiting over 425,000 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 (IFAD, 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.[2] The project spanned 13 municipalities in the southern part of the state (Figure 1), 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).

[2] 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 restrict the analysis to projects that began after 1996. We also require that the projects were in operation for a sufficient number of years in order to generate observable impacts by 2006.

Figure 1. Brazil, State of Bahia, Pro-Gavião Municipalities and Municipalities for Control Group



The Gavião river region was chosen for the project because of its extensive rural poverty and the adverse environmental conditions for agricultural production (BAHIA, 2006). The target population were 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 appears to have been considerable discretion involved on the part of the program administrators in determining the final list of communities to include.[3]

[3] This view is supported by interviews conducted in 2013 with former PG officials.

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.[4] There was, however, no rigorous evaluation of the program impacts on the beneficiaries using an RCT, or based on a methodology that at least relied on treatment and control groups, and baseline and follow-up data.

3. Empirical Strategy 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. In order to estimate the model, we construct a panel of census tracts for 1995-96 and 2006. In cases where the census tract changed, we built consistent geographical units called minimum comparable areas (AMCs).[5] Although we also control for a set of time varying observables, it is possible that there are some sources of selection bias related to unobserved factors that vary over time. For instance, our estimates would be biased upward if policy makers chose communities that were more likely to have faster growth in the outcomes of interest. While we think policy makers chose locations based on levels, not expected growth, this is nonetheless a possibility.

[4] 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" (IFAD, 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).

[5] AMCs were constructed based on digital maps using the ArcGis software. The AMCs contain an average of 1.8 census tracts each. For simplicity, in the current discussion we refer to census tracts.

Construction of the Control Group

We used propensity score matching to identify a control group that is similar to the treated census tracts based on observable pre-treatment characteristics. The propensity score is the conditional probability of receiving the treatment, given a vector of observed pre-treatment variables (Rosenbaum and 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 those related to participation in the project (such as the poverty incidence and gap, access to electricity, and agricultural practices) and the 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 and Kopeinig, 2005).[6]

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 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 and Ichino, 2002).

Estimating Program Impacts and their Synergies

Once the control group is formed, 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 Pro-Gavião, Bolsa Familia, 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. 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 indicates the presence of Pro-Gavião in 2006; BF_{st} refers to the percentage of farm

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

establishments that are beneficiaries of Bolsa Família 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.[7] 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 (Pro-Gavião) and α_2 (Bolsa Família). In order to allow for the fact that 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 Pro-Gavião could have had more or less success in census tracts with different levels of initial poverty.[8] To examine the possibility of heterogeneous effects of PG, a second equation is estimated:

$$Y_{st} = \beta_s + \beta_1 BF_{st} + \beta_2 BF_{st} * PG_{st} + \beta_3 PG_{st} * D_s^{40-80\%} + \beta_4 PG_{st} * D_s^{80-90\%} + \beta_5 PG_{st} * D_s^{>90\%} + X_{st}'\varphi + \gamma_t + \varepsilon_{st} \qquad (2)$$

where the dummies that indicate the presence of Pro-Gavião (PG_{st}) in census tract *s* are interacted with dummies that represent the incidence of extremely poor farm establishments in each census tract (greater than or equal to 40% but less than 80%, greater than or equal to 80% but less than 90%, and greater than or equal to 90%). Everything else is as defined in equation (1). With this specification, it is possible to check whether census tracts with a high share of extremely poor farms were impacted differently than those where the incidence was less than 40% (the excluded category). We also estimate a variant of equation (2) where instead of interacting the extreme poverty dummies with PG_{st}, we interact them with ($BF_{st} * PG_{st}$). This allows us to examine

[7] Because *Pro-Gavião* and *Bolsa Família* did not exist in 1996, the variables PG and BF are both zero in this year. Thus, for simplicity, we don't interact these variable with the time dummy as is standard in a DD model. Also, we do not need to include a separate intercept for the PG locations due to the inclusion of census tract fixed effects.
[8] While there are many dimensions of heterogeneity, we expect differences in poverty to be one of the most important. The level of poverty reflects the resource that households have at their disposable. Differences in resources can influence households' decisions to participate in a program, by affecting the costs and benefits. These differences could also influence the program's effectiveness.

whether the synergies had heterogeneous effects across census tracts with different levels of extreme poverty.

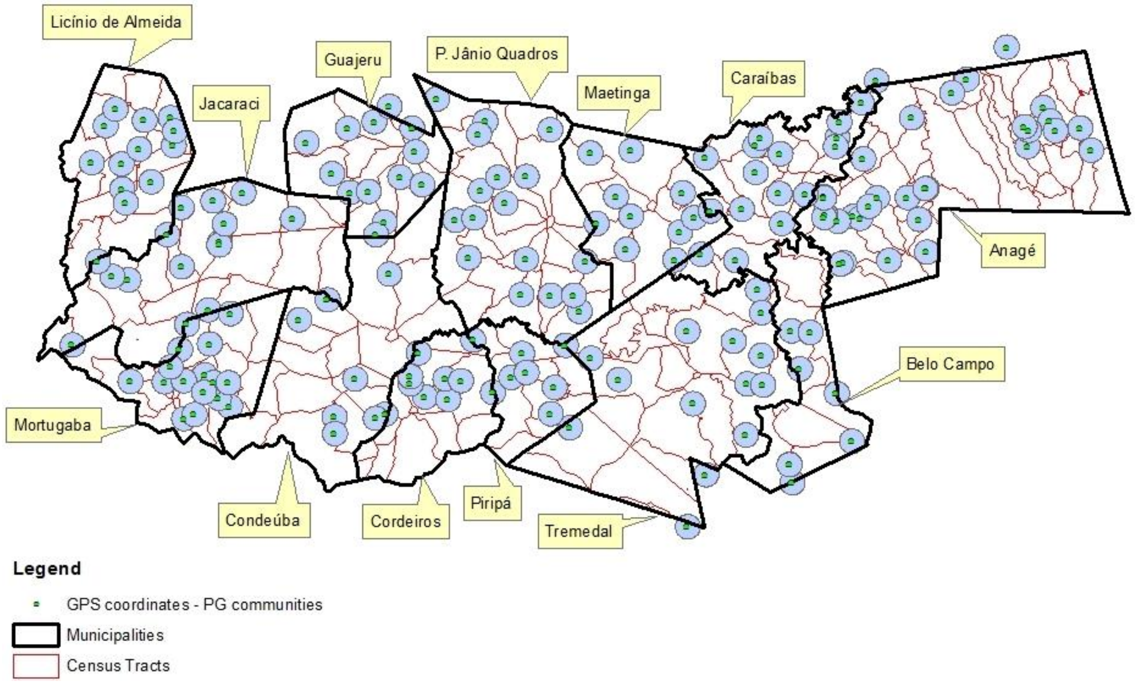
Data, Variables and Definitions

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 municipality is the lowest level of geographical aggregation at which the Census data are publicly available, we submitted a special request to the Brazilian Institute of Geography and Statistics (IBGE) to extract the data at the level of census tracts. This level of aggregation coincides more closely with the geographical level at which the IFAD project was implemented. 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. Our expectation is that rural communities and IBGE census tracts will be highly correlated.

Each community participating in PG was represented by a single geographical coordinate that we collected during our fieldwork. Whenever possible, this point represented 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.[9] In practice, 95% of the census tracts in the 13 PG municipalities were defined as treated with this approach. As a robustness check, we also estimated the effects considering as treated only those census tracts where the exact coordinates were located. Figure 2 shows the 13 municipalities, their divisions into census tracts, and the 2.5 km radius around each point representing a treated community. 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.

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

Figure 2. Pro-Gavião Municipalities, Census Tracts and Treated Communities (with a 2.5km radius)



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 Figure 1). Although we are not able to provide evidence concerning the parallel trends assumption, since we do not have data at the census tract level prior to 1995-96, it is important to consider a group of untreated units as similar as possible to those that were treated. Thus, the initial pool of 334 AMCs from which a control group could be selected was drawn 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 borders with the treated ones. In the end, our control group was drawn from a pool of 288 AMCs, none of which share a border with the treated 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 output (whether consumed on farm or sold), divided by the total area of the establishment. 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 in the establishment. Child labor measures the share of 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 the log of on-farm investment. Access to credit is defined as the share of establishments that had some form of formal financing in the AMC, whether from private or public banks, while access to electricity is the share 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 rely solely on agricultural income, rather than total income, because total income cannot be measured with the agricultural census data. They also refer to poverty among agricultural producers, not poverty among all rural households. In essence, they measure the extent to which on-farm income by itself can lift the 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.[10]

A final point of clarification relates to the Bolsa Família 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

[10] 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 were in the neighborhood of .80, suggesting that our measure is informative.

distinguishes these form social security and pension income. Because BF is the largest social program in Brazil, 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

Tables 1, 2 and 3 show, respectively, descriptive statistics on the main variables, the probit results, and the balance tests of means after matching. 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 the farm was only around R$553 in constant reais, which converts to less than one dollar per day. In this region, land productivity was considerably below the average for the state of Bahia, which in turn had only about half the national average land productivity. The average farm size in the treated AMCs was 17 hectares, and the output of these farms 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.

Table 1
Descriptive Statistics (farms under 50ha).

	Treated AMCs (N=99)		Non-treated AMCs (N=288)		
	Mean	SE	Mean	SE	p-value
Number of establishments	182.84	188.38	185.43	180.03	0.90
Farm size (ha)	17.02	4.71	13.34	4.80	0.00***
Land productivity (R$)	130.09	88.77	272.32	461.46	0.00***
Income per adult (R$)	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 (R$)	241.71	322.66	275.78	1120.96	0.77
Electricity (share)	0.15	0.19	0.13	0.20	0.39
Value of output per estab. (R$)	2082.26	1275.83	3755.94	10274.43	0.11
Expenditure per estab. (R$)	437.55	305.23	1122.55	3152.46	0.03**
Animal production (share)	0.44	0.17	0.34	0.21	0.00***
Crop production (share)	0.41	0.16	0.49	0.22	0.00***
Extractive crops (share)	0.11	0.10	0.04	0.09	0.00***
Perennial crops (share)	0.02	0.05	0.08	0.18	0.00***
Annual 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***
Machines (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	0.89	0.11	0.87	0.13	0.05**
Extreme poverty incidence	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).

Table 2 reports the probit results where the dependent variable is equal to one if the AMC participated in PG and zero otherwise.[11] All explanatory variables are observed in the baseline period. The model was estimated with 99 treated AMCs and 288 non-treated AMCs. Among the variables that are statistically significant for matching, we find farm size, land productivity and the extreme poverty gap, as well as the use of electricity, machines, and irrigation. 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.

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

Table 2
Probit Results for Participation in Pro-Gavião (1996).

Variables	
Farm size	0.057***
	(0.020)
Land productivity	-0.002**
	(0.001)
Access to credit	-16.686
	(14.681)
Investments per establishment	0.001***
	(0.0002)
Animal production	1.575***
	(0.499)
Extractive crops	3.384***
	(0.810)
Perennial crops	-0.926
	(1.277)
Technical assistance	-0.219
	(0.882)
Cooperatives	2.697
	(2.229)
Electricity	1.674***
	(0.470)
Machines	-3.628***
	(1.183)
Irrigation	-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.

Table 3 shows the difference in means for the matched samples. The Table also shows the standardized bias between the two 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 (Table 1), all of these differences disappear in the matched sample that restricts observations to the region of common support and uses the five nearest neighbors for matching.

Table 3
Tests of Means Between Treated AMCs and Non-treated AMCs After Matching.

Variables		Mean		% bias	% reduction |bias|	p-value
		Treated	Controls			
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		0.00	0.00	-1.60	95.00	0.54
Investments per establishment		237.10	211.69	3.10	25.40	0.55
Animal production		0.44	0.41	19.50	64.60	0.14
Extractive crops		0.11	0.13	-21.50	72.20	0.34
Perennial crops		0.02	0.01	4.50	90.40	0.37
Technical assistance		0.03	0.02	13.10	12.10	0.26
Cooperatives		0.01	0.01	6.20	74.90	0.50
Electricity		0.14	0.15	-8.00	20.70	0.60
Machines		0.04	0.05	-7.30	88.40	0.21
Irrigation		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	0.10				
	Matched	1.04				

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 Pro-Gavião, Bolsa Família, 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. Tables 4 and 5 show 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 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 decided to present these results anyway 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.

The most important finding to be highlighted in Table 4 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.

Table 4
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)
Pro-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)
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.

Table 5 shows a similar absence of any significant impacts of Pro-Gavião on the amount invested, and on access to credit and electricity. Access to credit, in contrast, was significantly affected by the incidence of social programs, and the interaction between the programs had a significant effect on the share of establishments with electricity. The estimate in column (1) for

credit suggest that a one percentage point (p.p.) increase in the farms that receive social programs is associated with a 0.21 p.p. increase in access to credit, all else constant. This effect remains significant (albeit smaller) even when we include additional controls, such as average farm size, the use of technical assistance, and participation in cooperatives.

Table 5
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.

If social programs were concentrated precisely on those AMCs where growth in access to credit was higher, then the estimated effect would not reflect a causal impact. However, government programs, such as Bolsa Familia, are not based on criteria related to the growth in variables, and even less so on access to credit. Although access to credit is not an explicit objective of social programs, this effect may be due to the fact that being a beneficiary makes it easier to provide collateral. The mere fact that a household participates in a social program can lead to greater reliability, stability and the possibility of contact in the case of credit arrears. This is consistent with the notion that regular and predictable transfers help to overcome liquidity constraints (Sabates-Wheeler *et al.*, 2009). In addition to facilitating investment in production, credit can help to smooth consumption in the face of income shocks, which are so frequent in rural environments.

In terms of access to electricity, the results show that the interaction between PG and social programs had a positive and statistically significant effect, although only at the 10% level of significance in the first specification. The increase in access to electricity in the region was substantial between 1996 and 2006. The share of farms with electricity increased from under 15% to around 60% in both treated and control AMCs. This increase reflects the priority given to certain public policies—such as Light for Everybody—and the general expansion of electrical power networks in this period. In this case, we suspect that the estimated effect may indicate an association, but not necessarily a causal impact.

The results presented so far refer to the average impact of the programs on the treated AMCs. However, an important question concerns the possibility that the impacts differ depending on the initial level of poverty of each location. It might be, for example, that it is more difficult for PG to succeed in locations with a higher prevalence of extreme poverty. The estimation of equation (2) allows us to test for the existence of heterogeneous impacts of Pro-Gavião. A slight modification of equation (2) allows for the possibility that there is heterogeneity in the impact of the interaction between policies. The results are shown in Table 6.[12] Similar to the average effects in Tables 4 and 5, these results provide no evidence of a policy impact on the PG AMCs, even when allowing for heterogeneity by the level of extreme poverty. Similarly, we find no evidence of positive impacts of the interaction between programs that differ by the level of poverty. The one coefficient that is significant at the 10% level—for land productivity—is negative. If causal, this indicates that the combined effects of the policies was harmful in the poorest AMCs. If, as is more likely, this is an association, it suggests that the poorest PG AMCs—that were also targeted more intensely by BF—had slower growth in land productivity than the others.

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. AMCs that benefited from PG, or both programs, do not seem to have had superior outcomes related to the growth of land productivity and income, or the reduction of child labor. The findings were unaltered when we allowed for the possibility of heterogeneous impacts that varied based on the initial level of extreme poverty in each AMC. The interaction

[12] No AMC treated by Pro-Gavião had an incidence of extreme poverty equal to or less than 40% in 2006. The dummies, therefore, compare to extreme poverty less than 40% in control AMCs.

Table 6

Heterogeneous Program Effects by Level of Extreme Poverty.

	Land productivity		Income per adult		Child labor		Investment		Credit		Electricity	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Pro-Gavião	-	0.25	-	0.23	-	11.81	-	0.18	-	-2.00	-	-4.43
		(0.32)		(0.42)		(13.26)		(0.46)		(3.33)		(13.14)
Social programs incidence	0.00	0.00	-0.01	-0.01	0.13	0.08	-0.01	-0.01	0.21***	0.18***	-0.09	0.06
	(0.004)	(0.003)	(0.01)	(0.01)	(0.18)	(0.13)	(0.02)	(0.01)	(0.06)	(0.04)	(0.17)	(0.12)
Pro-Gavião and ext. poverty 40%-80%	0.39	-	0.45	-	2.20	-	0.21	-	1.47	-	-14.94	-
	(0.31)		(0.41)		(10.93)		(0.91)		(3.06)		(10.80)	
Pro-Gavião and ext. poverty 80%-90%	0.29	-	-0.07	-	11.28	-	-0.32	-	-0.57	-	-8.99	-
	(0.30)		(0.40)		(9.50)		(0.82)		(2.91)		(10.01)	
Pro-Gavião and ext. poverty over 90%	-0.10	-	-0.27	-	14.72	-	0.23	-	0.03	-	0.70	-
	(0.31)		(0.45)		(10.71)		(0.90)		(2.87)		(10.14)	
Interaction between the programs	-0.01	-	0.00	-	-0.21	-	0.00	-	-0.06	-	0.45*	-
	(0.01)		(0.01)		(0.24)		(0.02)		(0.08)		(0.24)	
Interaction and ext. poverty 40%-80%	-	-0.06	-	-0.01	-	-17.90	-	-0.02	-	2.87	-	5.22
		(0.33)		(0.47)		(14.16)		(0.51)		(3.51)		(13.62)
Interaction and ext. poverty 80%-90%	-	-0.23	-	-0.54	-	-6.31	-	-0.55	-	-1.06	-	13.71
		(0.33)		(0.45)		(13.03)		(0.45)		(3.35)		(13.19)
Interaction and ext. poverty over 90%	-	-0.62*	-	-0.61	-	-7.09	-		-	-0.16	-	19.02
		(0.36)		(0.54)		(13.26)				(3.53)		(13.64)
Agricultural controls	N	N	N	N	N	N	N	N	N	N	N	N
Time dummy	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
R2	0.05	0.05	0.05	0.05	0.19	0.19	0.10	0.10	0.67	0.67	0.53	0.52
N	426	426	388	388	426	426	299	299	426	426	426	426

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

between the programs may have had an effect on access to electricity, but this was statistically significant only at the 10% level. We found, in contrast, evidence of a positive effect of social programs on access to credit.

Robustness Checks

In order to evaluate the robustness of the findings, in this section we present estimates of the impacts of each program and their interaction from a set of tests that we conducted. In Table 7 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). In the discussion that follows this section, we explore alternative explanations for the null results found thus far.

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 (see Figure 2). It is possible that the treatment had different intensities across households in a community associated with their exact location, and that using a 2.5km radius may be too noisy. As a robustness test, Panel A of Table 7 presents the results obtained from equation (1) when 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, all of the results remain qualitatively similar. In particular, there is no evidence that PG has favorably impacted land productivity, child labor, or the other channels considered, and the prior results for credit and electricity are still found. In the case of income, however, both PG and social programs now have negative and significant coefficients. This suggests that the AMCs surrounding the AMC that was defined as the center of the community may have performed better than the community center, and when these other AMCs are excluded the estimated coefficients become negative. 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.

Table 7
Robustness Checks.

	Land Productivity	Income per adult	Child labor	Investment	Credit	Electricity
Panel A: Exact coordinates						
Pro-Gavião	-0.05	-0.65**	4.96	0.63	-0.59	-1.62
	(0.27)	(0.32)	(5.92)	(0.57)	(2.01)	(5.66)
Social programs incidence	0.00	-0.01**	-0.01	0.00	0.20***	-0.11
	(0.004)	(0.01)	(0.12)	(0.02)	(0.05)	(0.13)
Interaction between the programs	0.00	0.01	-0.06	-0.02	-0.06	0.30**
	(0.01)	(0.01)	(0.15)	(0.01)	(0.06)	(0.15)
N	396	359	396	276	396	396
Panel B: Kernel matching						
Pro-Gavião	-0.05	-0.22	11.64	-0.44	-0.81	-5.56
	(0.27)	(0.38)	(8.35)	(0.85)	(2.46)	(8.62)
Social programs incidence	0.00	-0.01*	0.18	-0.02	0.21***	-0.08
	(0.004)	(0.01)	(0.13)	(0.02)	(0.05)	(0.14)
Interaction between the programs	0.00	0.00	-0.26	0.01	-0.05	0.41*
	(0.01)	(0.01)	(0.21)	(0.02)	(0.07)	(0.22)
N	768	704	768	509	768	768
Panel C: IBGE AMCs						
Pro-Gavião	-0.09	-0.41	9.85	-0.14	-3.95	4.42
	(0.23)	(0.33)	(6.41)	(0.70)	(3.28)	(7.15)
Social programs incidence	0.00	-0.01**	0.11	0.00	0.18**	0.05
	(0.003)	(0.01)	(0.12)	(0.01)	(0.07)	(0.14)
Interaction between the programs	0.00	0.01	-0.31*	-0.01	-0.05	0.06
	(0.01)	(0.01)	(0.18)	(0.02)	(0.08)	(0.19)
N	576	517	576	383	576	576
Time dummy	Y	Y	Y	Y	Y	Y
Fixed effects	Y	Y	Y	Y	Y	Y

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

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 7 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 4 and 5. 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 poorer AMCs where social programs had more penetration may have experienced slightly

slower income growth. In general, we conclude that—with the exception of some uncertainty surrounding the significance of the coefficient on income—the results are robust to the alternative matching procedure.

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 8 shows the estimated coefficients when using AMCs defined on the basis of an aggregation routine supplied by IBGE.[13] We note that this approach results in a larger number of AMCs, which could contribute to the precision of the estimates. As before, the sample excludes neighboring AMCs from the group of potential controls and 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 electricity that was significant at 10% and no longer is, 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 with the possible exception of access to electricity. Social programs do seem to have improved access to credit.

5. Discussion of null results

The finding that PG and the interaction between the programs had no statistically significant impact on the main outcomes studied in this paper 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.

[13] The main results in the paper use our own aggregation, which we think is preferred, because we detected a number of inconsistencies in IBGE's routine.

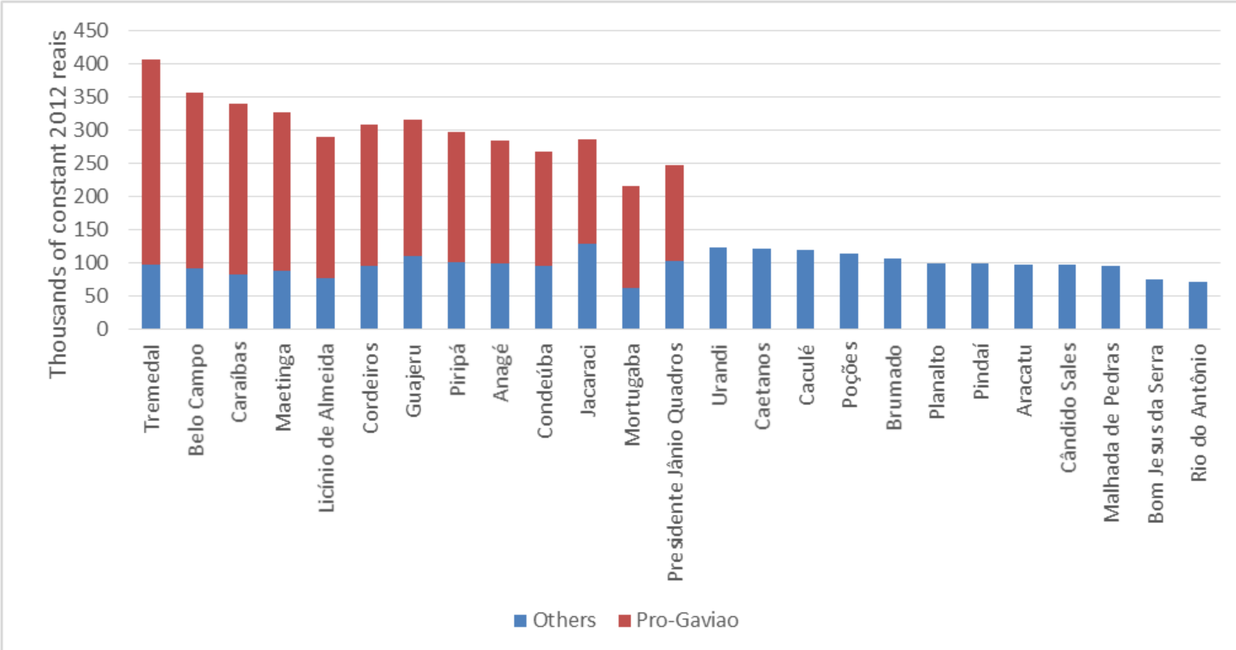
The Influence of Other Programs

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 affected 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 based on non-neighbors would not have represented the counterfactual of zero program intervention, but rather 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 the complete set of 54 municipalities analyzed, we did succeed in obtaining information from 25 municipalities in the River Gavião region (13 PG municipalities and 12 of the closest neighbors). This can help to provide evidence about how important PG was in relation to the other programs.

There were four different programs in the region that targeted rural areas: PRODUZIR, PRODUZIR II, PRODUZIR III and PRODECAR.[14] Figure 3 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.

[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.

Figure 3. Average Spending per Community by Pro-Gavião and Other Rural Programs (1996-2006)



Adverse Rainfall Shocks

A second possible explanation for a lack of positive impacts is that 1995-96 or 2006 could have been years of drought 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 Wilmott and 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. Thus, only 20% of the observations were in the upper or lower tails. 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, without much difference between the two 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.

Lack of Power

A third potential explanation relates to a lack of statistical power. It is possible that there were in fact impacts, but our tests do not have enough statistical power to capture them. Indeed, considering the entire sample of 288 non-treated AMCs and 99 treated AMCs, the minimum detectable effects for the main outcomes are well above some of the effects that we estimate. Using an 80% level of statistical power and a significance level of 5%, the minimum detectable effects correspond to about 0.33 standard deviations of the variables in the main sample. For both land productivity and income, this translates into 0.27 log points. Note that these are above the point estimates of our main results in Table 4 (shown in column 1 for each variable). In the case of land productivity, for example, we cannot be certain if there are positive effects of the PG program—estimated at around 0.16 in Table 4—that we are unable to detect due to a lack of power, or if there are no effects at all. In the case of agricultural income, in contrast, the estimated coefficient in Table 4 is only 0.001. Even if it were statistically significant, this would not be economically meaningful. Regarding child labor, the minimum detectable effect is 6.4 percentage points. The point estimate from Table 4 is around 10 percentage points. Thus, it is very likely that we would have captured a significant effect if there were one. In this case we can be more certain about the conclusion that there are no effects on child labor. Finally, regarding investment, access to credit and electricity, the minimum detectable effects are close to or above the point estimates of Table 5, suggesting that it is possible that there are some effects that we do not have enough power to

detect. In sum, we reach mixed conclusions regarding a lack of power. This is certainly a concern for some, but not all of our estimates. In other cases, the estimates are too small to be economically meaningful even if they were statistically significant.

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 can 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. An analysis of a reduction in risk would require data from more than one follow-up period, and this was not feasible with the data at our disposable. It is also possible that other dimensions of well-being may have been affected that we were unable to measure. 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.

The Setting

A fifth possibility relates to the harsh environmental and economic setting of the Gavião region. Favareto and 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 that necessitate the use of new technologies, 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. The soil and weather characteristics of the semi-arid region do, according to Bahia (2006), represent significant obstacles to agricultural production. Consequently, although PG may have built important items of infrastructure, and disseminated knowledge through extension activities, it may have been insufficient to improve the average situation of the beneficiaries. Market failures also create obstacles, and these may be responsible for a lack of response by households to public policies. De Janvry and Sadoulet (2005) suggest that even if certain public policies relax constraints in particular markets, the ability of

agricultural households to change their behavior may continue to 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 more solid evidence of program impacts, and how the relaxation of specific constraints can contribute to program success, it is difficult to differentiate between these competing conclusions.

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. For example, Directors of rural development programs did not have access to information from the Cadastro Único[15] that could be extremely valuable for targeting poor households and vulnerable locations, or to avoid duplication of efforts. 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, the heterogeneous governing coalitions that are often formed, or the archaic practices of traditional elites in the Northeast of Brazil.

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)

[15] This is the database on poor families that have applied to or received benefits from the Bolsa Família Program.

do find evidence of synergies between CCTs and the family farm credit program (Pronaf) in Brazil, and Macours *et al.* (2012) finds 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. Thus, we view this explanation as possible, but not likely.

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 impact 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 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 lack 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 Pro-Gavião or of synergies between the two programs. The presence of Bolsa Família 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 robust to different matching techniques, ways of defining the treated locations, and heterogeneity by 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, and in some cases a lack of power was a concern. 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 a creative approach to estimating impacts *ex post*, rural development interventions should build impact evaluations in from the start so that 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.

7. References

Aldana, U., Vásquez,T., & Yancari, J. (2016). Sierra Sur y Juntos: un análisis a partir de la implementación conjunta de estos dos programas en Chumbivilcas (Cusco, Perú). In: Maldonado, J. H., Moreno-Sánchez, R., Gómez, J. A. & Jurado, V. L. (Eds.). *Protección, producción, promoción: Explorando sinergias entre protección social y fomento productivo rural en América Latina*. Bogotá: Universidad de los Andes.

Bahia. (2006). Secretaria do Planejamento - SEPLAN e Companhia de Desenvolvimento e Ação Regional. Projeto Gavião. Relatório de Finalização do Projeto**.**

Barros, R., de Carvalho, M., Franco, S., & Mendonça, R. (2010). Markets, the State and the Dynamics of Inequality in Brazil. In: López-Calva, L. F. & Lustig, N. C. (Eds.) *Declining Inequality in Latin America: A Decade of Progress?* Washington, D.C.: Brookings and UNDP.

Becker, S. O., & Ichino, A. (2002). Estimation of average treatment effects based on propensity score. *Stata Journal*, 2(4), 358-377.

Brown, C. G., & Longworth, J. W. (1992). Multilateral Assistance and Sustainable Development: The Case of an IFAD Project in the Pastoral Region of China. *World Development,* 20(11), 1663-1674.

Caliendo, M., & Kopeinig, S. (2005). Some Practical Guidance for the Implementation of Propensity Score Matching. Institute for the Study of Labor (IZA)*: Discussion Paper 1588*.

Cardoso, E., & Souza, A. P. (2009). The Impact of Cash Transfers on Child Labor and School Enrollment in Brazil. In: Orazem, P. F., Sedlacek, G., & Tzannatos, Z. (Eds.). *Child Labor and Education in Latin America: An Economic perspective.* New York: Palgrave Macmillan.

Carter, M. R., Laajaj, R., & Yang, D. (2014). Subsidies and the persistence of technology adoption: Field Experimental evidence from Mozambique. *Working Paper 20465.* Available at: *http://www.nber.org/papers/w20465*

Chitolina, L., Foguel, M. N., & Menezes-Filho, N. A. (2016). The Impact of the Expansion of the *Bolsa Família* Program on the Time Allocation of Youths and Their Parents. *Revista Brasileira de Economia,* 70(2), 183-202.

De Brauw, A., Gilligan, D. O., Hoddinott, J. & Roy, S. (2015). The Impact of Bolsa Família on Schooling. *World Development*, 70, 303-316.

de Janvry, A., & Sadoulet, E. (2005). Progress in the Modeling of Rural Households' Behavior Under Market Failures. In: de Janvry, A., & Kanbur, R. (Eds.). *Poverty, Inequality and Development: Essays in Honor of Erik Thorbecke.* New York: Springer Publishers.

de Janvry, A., Sadoulet, E., & Murgai, R. (2002) Rural Development and Rural Policy. In: Gardner, B., & Rausser, G. (Eds.). *Handbook of Agricultural Economics,* vol. 2, Amsterdam, London and New York: Elsevier Science.

Del Carpio, X. V., Loayza, N. V., & Wada, T. (2016). The Impact of Conditional Cash Transfers on the Amount and Type of Child Labor. *World Development*, 80, 33–47.

Devereux, S. (2009). Social protection for agricultural growth in Africa. FAC Working Paper No. SP06. Brighton, UK, IDS.

Devereux, S. (2016). Social protection for enhanced food security in sub-Saharan Africa. *Food Policy*, 60, 52-62.

Duflo, E., Kremer, M., & Robinson, J. (2011). Nudging Farmers to Use Fertilizer: Theory and Experimental Evidence from Kenya. *American Economic Review*, 101(6), 2350-2390.

Favareto, A. P., & Seifer, P. (2013). Fatores de sucesso e lições para o futuro: o caso dos projetos apoiados pelo FIDA no Brasil. In: IICA. *Práticas de desenvolvimento no Nordeste do Brasil: experiências dos projetos apoiados pelo FIDA* (pp.31-68). Brasília: IICA.

Fiszbein, A. N., Schady, N., Ferreira, F. H. G., Grosh, M., Kelleher, N., Olinto, P., & Skoufias, E. (2009). *Conditional Cash Transfers: Reducing present and future poverty*. Washington, DC: World Bank.

Garcia, F., Helfand, S. M., & Souza, A. P. (2016). Transferencias monetarias condicionadas y políticas de desarrollo rural em Brasil: posibles sinergias entre Bolsa Família y el Pronaf). In: Maldonado, J. H., Moreno-Sánchez, R., Gómez, J. A. & Jurado, V. L. (Eds.). *Protección, producción, promoción: Explorando sinergias entre protección social y fomento productivo rural en América Latina*. Bogotá: Universidad de los Andes.

Glewwe, P., & Kassouf, A. L. (2012). The Impact of the Bolsa Escola/Família conditional cash transfer program on enrollment, dropout rates and grade promotion in Brazil. *Journal of Development Economics*, 97, 505-517.

Hoffmann, R. (2007). Transferências de Renda e Redução da Desigualdade no Brasil e em Cinco Regiões, entre 1997 e 2005. In: Barros, R. P., Foguel, M. N., & Ulyssea, G. (Eds.). *Desigualdade de renda no Brasil: uma análise da queda recente* (pp. 17-40). Brasília: IPEA.

International Fund for Agricultural Development (IFAD) (2003). Community Development Project for the Rio Gavião Region (PROGAVIÃO): Interim evaluation. Available at: *https://www.ifad.org/en/web/ioe/evaluation/asset/39829997*.

---- (2013). Democratic Socialist Republic of Sri Lanka: Dry Zone Livelihood Support and Partnership Programme. Available at: *https://www.ifad.org/web/ioe/evaluation/asset/39823520*

---- (2015). India: Jharkhand Chhattisgarh Tribal Development Programme. Available at: *https://www.ifad.org/web/ioe/evaluation/asset/39823508*.

---- (2016a). *IFAD Strategic Framework 2016-2025: Enabling inclusive and sustainable rural transformation.* Available at: *https://www.ifad.org/web/knowledge/publication/asset/39369820.*

---- (2016b). Republic of Mozambique: Sofala Bank Artisanal Fisheries Project. Available at: *https://www.ifad.org/web/ioe/evaluation/asset/39824114*

---- (2018). Agency data. Available at: *http://www.ifad.org/governance/index.htm*.

Instituto de Pesquisa Econômica Aplicada (Ipeadata) (2018). Municipal poverty data. Available at: *http://www.ipeadata.gov.br/Default.aspx*.

Macours, K., Premand, P., & and Vakis, R. (2012). Transfers, Diversification and Household Risk Strategies: Experimental evidence with lessons for climate change adaptation. *World Bank. Policy Research Working Paper 6053*.

Maldonado, J. H., Gómez, J. A., & Jurado, V. L. (2016a). Sinergias entre políticas de protección social y proyectos de desarrollo rural: aproximación desde la literatura existente y la teoria. In: Maldonado, J. H., Moreno-Sánchez, R., Gómez, J. A. & Jurado, V. L. (Eds.). *Protección, producción, promoción: Explorando sinergias entre protección social y fomento productivo rural en América Latina*. Bogotá: Universidad de los Andes.

Maldonado, J. H., Moreno-Sánchez, R., Gómez, J. A. & Jurado, V. L. (Eds.) (2016b). *Protección, producción, promoción: Explorando sinergias entre protección social y fomento productivo rural en América Latina*. Bogotá: Universidad de los Andes.

Moya, A. (2016). Programas para la reducción de la pobreza rural en Colombia: uma evaluación de las sinergias entre Oportunidades Rurales e Familias em Acción. In: Maldonado, J. H., Moreno-Sánchez, R., Gómez, J. A. & Jurado, V. L. (Eds.). *Protección, producción, promoción: Explorando sinergias entre protección social y fomento productivo rural en América Latina*. Bogotá: Universidad de los Andes.

Ricker-Gilbert, J. & Jayne, T. S. (2017). Estimating the Enduring Effects of Fertiliser Subsidies on Commercial Fertiliser Demand and Maize Production: Panel Data Evidence from Malawi. *Journal of Agricultural Economics*, 68 (1), 70-97.

Rosenbaum, P. R., & Rubim, D. (1983) The central role of the propensity score in observational studies for casual effects. *Biometrika*, 70(1), 41-55.

Sabates-Wheeler, R., Devereux, S., & Guenter, B. (2009). Building synergies between social protection and smallholder agricultural policies. *FAC Working Paper SP01*.

Singh, I., Squire, L., & Strauss, J. (Eds.) (1986). *Agricultural Household Models*. Baltimore: Johns Hopkins University Press.

Soares, F. V., Knowles, M., Daidone, S., & Tirivayi, N. (2017). *Combined effects and synergies between agricultural and social protection interventions: What is the evidence so far?* Rome: Food and Agriculture Organization of the United Nations.

Tirivayi, N., Knowles, M., & Davis, B. (2013). The interaction between social protection and agriculture: a review of evidence. Available at: www.fao.org/3/a-i3563e.pdf.

United Nations Development Program (UNDP) (2018). Municipal HDI ranking 2000. Available at: *http://www.br.undp.org/content/brazil/pt/home/idh0/rankings/idhm-municipios-2000.html*.

Willmot, C., & Matsuura, K. (2001). Terrestrial air temperature and precipitation: Monthly and annual time series (1950-1999), version 1.02. Center for Climatic Research, Department of Geography, University of Delaware. Available at: *http://climate.geog.udel.edu/~climate/html_pages/README.ghcn_ts2.html*.

World Bank (2007). *World Development Report 2008: Agriculture for Development*. Washington, D.C.: World Bank.

Zeng, D., Alwang, J., Norton, G. W., Shiferaw, B., Jaleta, M., & Yirga, C. (2017). Agricultural technology adoption and child nutrition enhancement: improved maize varieties in rural Ethiopia. *Agricultural Economics*, 48(5), 573–586.