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The impact of extension services on farming households in Western Kenya

A propensity score approach[1]

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Abstract

The aim of this paper is to assess the impact of the adoption of technological packages in agriculture Kenya on the farming households, as promoted by the National Agriculture and Livestock Extension Programme (NALEP), a program run by the Government of Kenya. To this end, we collected data on beneficiaries through a survey of 1000 households in the district of Lugari, in Western Kenya. We use propensity score matching to compute the average treatment effect on the treated. We find evidence that: I) program beneficiaries changed their crop rotation practices; II) treated households increased their fertilizer dosage by 23.8%; IV) productivity per acre is not affected by the treatment; V) treated households also were less likely to store their surplus maize.

JEL Classification: Q16, Q13, Q12

Keywords: Agricultural Extension, Kenya, Propensity Score Matching, Maize, Fertilizer, Crop Rotation, Productivity

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1. Introduction

Poverty reduction is core to the field of development economics. With 75% of the world's poor living in rural areas, the topic of improved agriculture is viewed as central to poverty reduction (Thirtle, Lin, & Piesse, 2003; de Janvry & Sadoulet, 2010).

Extensive research on agricultural growth since India's green revolution in the 1960s has provided evidence on ways to uplift production, livelihoods and food security for the rural poor. Agricultural productivity gains in India, mainly through the adoption of high yield seeds varieties (HYV), brought both absolute and relative gains to poor rural households, although those gains took time to manifest themselves (Ravallion & Datt, 1998). Agricultural extension services are one of the most common forms of public-sector support for knowledge diffusion and learning. Extension has the potential of bridging discoveries and mitigation methods from research laboratories and the in-field practices of individual farmers. In addition to information about cropping techniques, optimal inputs use, high-yield varieties and prices, extension frontline agents can improve the managerial skills of farmers by diffusing information on record keeping, further improving the commercial potential of agricultural production (Birkhaeuser & Evenson, 1991).

In Kenya, as of 2005, 61% of the population was employed within the agriculture sector (World Bank, 2013). At the same time, climate change is believed to affect adversely the highly-productive lands[3], representing only 16% of the territory, that are subject to high and medium rainfalls. Those factors conjugated threaten rural household's livelihoods, income and food security. Kenya has suffered from 28 droughts over the last hundred years, four of which have occurred during the last ten years. Those climatic events threaten the livelihoods and incomes of the people who depend on agriculture, especially for the country's poor, who represent a little over half of the population. In order to act upon the situation, the Government of Kenya has

[3] Jones and Thorton (2003) use spatial simulations to predict the effect of climate change on maize production in Africa and Latin America. They find for Kenya a decrease in maize yields of 6% by 2055, compared with the yields for the year 2000.

introduced the Swedish International Development Agency (Sida) funded[4] National Agriculture and Livestock Extension Programme (NALEP) in the year 2000, which lasted until December 2011. It aimed at uplifting productivity, encouraging commercialization and enhancing resilience through the increased use of agricultural technologies and improved inputs, using demand driven and participatory agricultural extension approaches. Whether extension services contributed to affect technology adoption, crops productivity and incomes in Kenya is the empirical question studied in this paper. We analyse the average treatment effect on the treated on outcomes separated in 4 main groups: adoption of extension methods, farm output, revenue, and basic household welfare. To this end, we use propensity score matching (PSM) to evaluate the impact of the programme, as first proposed by Rosenbaum and Rubin (1983), using a unique data set collected for the purpose of this paper.

In terms of relevance, the quantitative impact of technological packages adoption has been seldom studied, since most of the literature focuses on the impact of the adoption of specific technologies (Becerril & Abdulai, 2010; Dercon, 2009). This paper distinguishes itself by the use of data that has been collected in a unique setting in Kenya, by cooperating closely with the local government. Also, this paper provides a framework for policymakers that enables a refinement of program evaluation practices.

In a review, Birkhaeuser and Evenson (1991) draw a portrait of agricultural extension since WWII, underlining the different strengths and weaknesses of the systems. They discuss how the low level of skills of extension agents sometimes hampered the potential of the different extension programmes. Other reasons for the failure of extension to enhance productivity include the lack of understanding of governments on the incentives to adopt new technologies and whether they suit the socioeconomic and agroecological circumstances of the service recipients (Anderson & Feder, 2004).

[4] The programme benefited from a support of 508M SEK (~80M USD) from Sida, for the whole length of the programme.

Using propensity score matching, as we do in this paper, in order to evaluate program outcomes is a popular way to proceed, since it offers an alternative for ex-post evaluation, as well as ways to overcome selection bias. Propensity scores were first used more than three decades ago as a method to control bias (Cochrane & Rubin, 1973; Rubin, 1973; Rubin, 1979; Bassi, 1984; Rosenbaum & Rubin, 1985). More recently, its use has been seen a regain in popularity following works by James Heckman as well as Rajeev Dehejia and Sadek Wahba on the impact of training programs (Friedlander, Greenberg, & Robins, 1997; Heckman, Ichimura, & Todd, 1997; Heckman, Lalonde, & Smith, 1999; Dehejia & Wahba, 2002; Heckman & Navarro-Lozano, 2004).

NALEP is seen as a leader in Sub-Saharan Africa (SSA) in terms of coverage and participatory methods, yet the programme has not generated a great deal of academic research. In 2006, a Sida report by Cuellar et al. (2006) claimed that 80% of the households part of the program that formed a producer group – called *Common Interest Group* (CIG) – stated that the introduction of the programme has offered new opportunities for men, women and youth in agriculture. The study revealed that more than 70% of the farmers interviewed claimed that they now regard agriculture as an enterprise rather than a mean of subsistence. However, the report was mostly aimed at evaluating the implementation of the program rather than the impact on the livelihoods and production of program takers and it did, furthermore, not use a formal statistical analysis. A paper by Richard Githaiga (2007) looked at the impact of CIGs under NALEP. It found that the CIG approach had a significant impact on farmers' access to extension services but no significant impact on farmers' access to agricultural credit and marketing. In addition, the study found that CIGs had a significant impact on the agricultural productivity of group members. Githaiga's paper suffered from several problems, though. The survey instruments used to collect the information seem to have been strongly biased towards positive outcomes.

This paper is organized as follows: Section 2 describes the program in length, with information about the intervention and a specific attention to the types of institutions that were formed under the program; Section 3 provides information on the data that was collected and a general description of the variables of interest; Section 4 presents the empirical framework on propensity score matching and the different matching methods employed; in Section 5 we provide the results and a discussion about them; we then conclude in section 6.

2. The program

The implementation of the National Agriculture and Livestock Extension Programme (NALEP) started in 2000 and was coordinated jointly by the Ministry of Agriculture and the Ministry of Livestock Development of Kenya. The programme sought to enhance social economic development and poverty alleviation through agriculture and livestock development. The programme generally aimed at providing and facilitating pluralistic and efficient extension services for increased production, food security, higher incomes and improved environment. The programme targeted rural populations engaged in agriculture, livestock and fisheries, with a specific focus on pro-poorness and non-discriminatory access to the program[5]. NALEP covered first the high-potential agroecological zones and expanded its coverage in 2007-08 to all districts in Kenya. NALEP strived to support initiatives at different levels: supporting institutional set-up (setting up local institutions for improved marketing, lobbying and decision making, which we describe later), enhancing the use of extension approaches, promoting technical packages, promoting collaboration and networking with other actors (NGOs, Private sector, Other Ministries, etc.), mitigating problems associated with gender and other cross-cutting issues (HIV and AIDS, drugs, alcohol and other substance abuse). This paper focuses on the direct effects on crops of the adoption of technical packages.

NALEP's targeting approach was focused on vulnerability and pro-poorness using participatory methods to identify the needs of beneficiaries. One of the tools used was the Participatory Analysis of Poverty and Livelihood Dynamics (PAPOLD), a community-driven survey tool used to identify potential beneficiaries. PAPOLD surveys have been used in several agricultural and forestry projects that used participatory approaches in Burkina Faso, Vietnam and Kenya

[5] By this, we mean that the program took affirmative action in selection poor, vulnerable and excluded individuals.

(Hoang & Nguyen, 2011). The survey was performed at the beginning of the implementation period in each sub-location[6] where NALEP was about to deliver services. It included a census of the sub-location's dwellings and an asset-scale wealth chart[7], and other location specific agricultural information on soils, production, etc. The PAPOLD survey's goal was twofold: identifying the needs of vulnerable households to accessing resources for productivity, and assisting the farmers[8] to commercialize their products. NALEP's mandate was to deliver services to all divisions in Kenya, even though it has not been achieved fully by the end of the programme. Hence, there was no formal "selection mechanism" that assigned treatment to a sub-location, but rather a progressive roll-out of the programme to the whole country's divisions. The decision making process that determined which division received the programme's support first is formally stated in the programme procedure and could not be determined accurately.

NALEP was operationalized through a structure composed of grass-root institutions. The highest level of institution that was created through NALEP was the stakeholders' forum (SHF), with representation from both the public and private sectors, formed the entry point for NALEP in a new treatment area[9], called *focal area* by the programme. The stakeholders included private extension service providers, input suppliers, marketing agents, NGOs, community based organizations (CBOs), government ministries and departments, local councils and other development structures. The SHF was responsible for conducting a Broad Based Survey (BBS)

[6] The lowest agglomeration is called the Sub-location, which is the equivalent of a small village. Municipalities often encompass several locations. The sub-location is superseded by the Location, the Division, the District and the Province.

[7] The community, through communal meetings (*barazas*), had to decide on the asset scale and the relevant thresholds for poverty. E.g. If a household owned kitchen utensils, it is not considered very poor; if a household owned a bicycle, it is not considered poor. Then, using the list of households that were considered poor and very poor, extension services were designed according to the needs of the sub-location.

[8] NALEP did not provide assistance to large scale farmers or to the ones possessing modern technologies, such as mechanic farming tools.

[9] Typically 300 ha and 2000 households per focal area per year. The focal area covered a subset of a location. Those focal areas shall in principle be interacting with NALEP for 3 years, but in practice, due to logistical constraints, the time spent in a focal area was a year.

– a sort of baseline[10], in the focal area with the assistance of NALEP technical personnel ending with the production of a Community Action Plan (CAP) defining the community's own projects – a type of "community business plan". Rather than imposing solutions to the households, NALEP mobilised communities to generate their own projects and to link them with development agencies to facilitate implementation of the projects. The community was in charge of project cycle management and ownership of all community development projects, in order to facilitate the phasing out process and avoid the "aid void".

One level below the SHF, NALEP helped developing the Focal Area Development Committee (FADC), which was a committee formed for the purposes of steering and coordinating collaborative activities of the focal area. It played the role of an indigenous commerce chamber. Among the FADC, NALEP encouraged individuals to work together and to form Common Interest Groups (CIGs). A CIG is a group of individuals that have come together to develop a commodity (in either livestock or crop production) or activity into a commercial enterprise with marketing as a major thrust. It is a kind of cooperative, although lacking the level of formality of a proper cooperative. Through these groups, NALEP contributed in building local capacities in various technical areas, rights of farmers, pastoralists, fisher folk, and other clients and mainstreaming gender and other cross-cutting[11] issues.

CIGs provided a platform for bargaining, as well as for extension service provision in general. Although the type of extension programme called "training and visit" (T&V) programs – usually one-on-one sessions – have met quite a degree of success elsewhere, showing high returns on investment from the point of view of the program (Bindlish & Evenson, 1997), NALEP found it more cost effective to collaborate with groups rather than with individuals. The group approach also allows for the infusion of leadership capabilities among members of the community, as they rotate group leadership positions.

[10] The BBS was not performed with the aim of evaluating the program. The quality of the data was poor and its availability was scarce.
[11] In aid, cross-cutting issues are viewed as supporting conditions for development. They include mitigating problems linked to substance abuse, health, violence, etc.

The decline of central planning, combined with a growing concern for sustainability and equity, has resulted in participatory methods gradually replacing top-down approaches in extension that were used in former programmes.

3. Data

In February 2012, we sampled 1000 household using a household survey in the district of Lugari over 25 consecutive days. We designed the survey based on publicly available documentation and the specific nature of the programme. 10 enumerators have been trained for the purpose of the data collection, which we supervised directly with the support of the NALEP staff based in Lugari. That region has been selected with the support of various agronomic experts and managers at NALEP headquarters in Nairobi, in order to represent a core region where NALEP was active, the high-potential region. Within the Lugari district, a treatment and control group have been selected, upon analysis of internal documentation of the programme. The control group needed to exhibit the same characteristics (ethnic composition, soil quality, average education level, rainfall, etc.) as the treatment group, upon assessment of the programme staff and data from the Kenya National Bureau of Statistics. The data has been collected in 2 distinct locations, 500 surveys by location, but all within the Lugari district. The treated region was represented by the Lugari sub-location part of the Lugari location, while the control region was surveyed in the Chekalini Location, divided between the Musembe and Koromaiti sub-locations. Figure A.**3** and Figure A.**4** in appendix detail the specific location of the survey. Each survey location was assigned to a sub-group of enumerators who divided the territory in quadrants and proceeded to a random walk survey selection process. The control and the treatment survey areas are adjacent to each other, yet the villages used for the sampling are located at the extreme south border of the Chekalini location, in order to try to avoid spill-overs. The border between the two locations is delimited by the administrative town of Lugari, meaning that both sampled and control regions are located at equal distance from the main market of the whole district. A small river had confluents that spread equally to both location and road access to Lugari town was of similar (low) quality among the sampled locations.

After cleaning the dataset, as certain households interviewed were not farming or only raising livestock (27 households), or that the surveys were not fully completed or the quality of certain enumerator's work was not acceptable[12], a total of 665 survey remained. Further analysis of the data led to the conclusion that either a small part of the respondents exaggerated certain reported values or that mistakes in the data input led to faulty numbers. Therefore, there was a need to adjust for outliers. We decided to proceed to trimming the yields and prices at 5%, which led to reasonable upper and lower boundaries, given publicly available data on crops yields and prices.

Ethnic diversity was not a suitable piece of information[13] to be collected, and information on soil quality could not be collected in this study, due to logistical limitations[14]. The survey was composed of 115 questions and was divided in 5 categories: demographics, household characteristics (assets, energy, expenditures, water, sanitation and hygiene), health (access, HIV/AIDS, prevalence of diseases, mortality), agriculture and livestock (productivity, production, use of inputs, prices, income and extension services and other income generating activities) and food security,. Table 1 below presents the results of the test for the difference in means between treated and non-treated households[15], over a set of control variables. In general, we observe that the control and treatment are rather similar, with education years of the head of the household being slightly higher for the treated group (+0.5 year). Table 2 also presents the unmatched effect of the program on a set of outcome variables.

[12] One enumerator was suspected to have falsified the data; hence all of his work was discarded (100 surveys).

[13] The information was deemed too sensitive to be collected, as Kenya has a long history of social tensions between ethnic groups. Moreover, the very concept of tribal ethnicity as a binary variable can be seen as a social construction.

[14] Otherwise available data was not detailed enough to include soil quality as a control variable.

[15] The variables presented in Table 1 are used later in this paper in order to construct the propensity scores. The t-test results are presented here in order to describe the differences between the treated and control group on variables that are assumed not to be affected by treatment.

Table 1: t-test for equality of means between groups[16]

	Observations control	Observations treated	Mean control	Mean treated	Difference in mean	SE
Total Farm Acreage	376	289	2.09	2.42	-0.32	0.20
Male headed household	376	289	0.74	0.80	-0.05	0.03
Number of HH members	376	289	6.28	6.35	-0.07	0.20
Total Farm Assets Value	376	289	46708.78	18866.78	27841.99	15558.66
Total Household Assets Value	376	289	61663.16	71105.54	-9442.37	10142.67
Age of the head of household	376	289	50.55	51.87	-1.31	1.06
Education level of the head of the household (years)	376	289	9.50	9.99	-0.49*	0.21
Number of living rooms in the main dwelling house	376	289	3.75	3.82	-0.07	0.13
Household has electricity	376	289	0.11	0.11	0.00	0.03
Cultivates maize	376	289	0.99	0.99	0.00	0.01

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

In the Lugari district, implementation of NALEP has started in early phases of the program (2001-02). There are naturally pros and cons about performing a study more than ten years after the initial treatment. Such a design can help us understand the long-term effect of the programme, which is fundamentally more meaningful than a short-term response to a treatment. In theory, effects of publicly funded agriculture research take 30 years to be captured (Chavas & Cox, 1992), which is of relevance here given the role of research in extension and the public nature of the provision. Also, assessment from the staff involved in delivering the extension trainings revealed that the NALEP recipients were first wary and did not show a high level of interest in a “dry” program (NALEP did not effectively proceed to any hand-out to the farmers in kind or cash), but that after a certain time, the individuals would opt-in to a greater extent. The problem with surveying a region for which the involvement of NALEP has been over ten years was not only the difficulty to construct a control group (avoiding spillovers), but also to assess the programme’s specific effects. Knowledge networks are unavoidable[17], meaning that spillovers are a real potential source of downward bias, and was somehow wished by the

[16] Some balancing covariates show missing values, explaining the varying number of observation accross the variables in the table. Among the litterature, only Rosenbaum and Rubin (1984) make explicit reference to this issue. They show in a theorem and a corollary that a propensity score composed of covariates that feature a pattern of missing data has similar features of a balancing score that shows no missing data.

[17] On this topic, Foster and Rosenzweig (1996) show the positive role of neighbour’s knowledge in the profitability of using high yielding varieties (HYV) seeds.

program. The obvious disadvantage of such a setting is that other factors might have influenced welfare and productivity over the course of the period, other than the treatment, since the treatment is clustered at the sub-location level. This nonetheless remains a potential source of bias that appears unavoidable in our setting, which we discuss later in this paper. The advantage of investigating the long-run effect is that it addresses a key objective of the program: improving livelihoods through demand-led increase in sustainable production. The goal of the program was indeed not to improve livelihoods and productions for the time of the intervention, but rather to create a long-lasting effect[18].

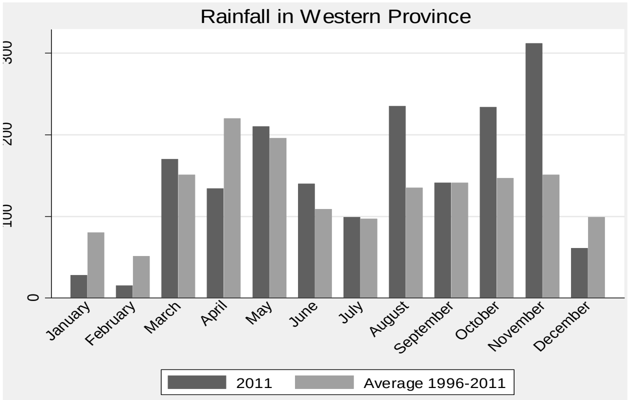
In order to interpret the results carefully, it is useful to have a look at the elements that inform us on the representativeness of the reference year[19] (2011). Figure 1 informs us on the rainfall of 2011 in the Western province. Overall, the province where Lugari is located, Western, benefited from higher rains than normal (12.8% higher rainfall than the 1996-2011 average), especially in the months of August to December[20] (+46%). This information is useful when analysing the results, as NALEP household located in regions where rains were abundant ought to benefit more than their non-NALEP counterparts, since NALEP was pushing for improved water catchment technologies. Similarly, NALEP households located in provinces (not covered by this survey) where rainfall levels were low ought to be better off than their non-NALEP counterparts, as the program was encouraging the use of drought resistant crops and other techniques aiming at improved resilience.

Another important element about the reference year is that Kenya, and more specifically the Western province, has been affected has been affected by two viruses, the maize chlorotic mottle virus and the maize dwarf mosaic virus, which induced a synergism referred to as the corn lethal necrosis. The viruses lead the leafs to dry up, and eventually to plant death. Certain

[18] On this topic, Barnerjee et al. (2007) in their study on education in urban India, provide evidence that initial significant improvement in test score decreased to much lower levels one year after the intervention.
[19] The reference year means that the survey inquired about crop production in 2011.
[20] 5 dekads (periods of 10 days, in climatology) spread over the months of June, August, October and November showed rains that were classified as “much above normal”, that is, above the 8th decile of recent monthly rainfall data (Kenya Meteorological Department, 2013).

districts, such as Bomet (located 180km from Lugari), have seen their maize yields reduced by 80% in 2011. According to the Ministry of Agriculture, the surveyed district (Lugari) has not been as hardly by the viruses, as they were expecting the maize yields to be 10% lower for that district. As a mitigation method, the Ministry of Agriculture had been educating maize farmers on the importance of crop rotation, and more specifically to avoid planting maize during the short rains season (September to November) in order to restrict the breeding ground of the virus, and opting for leguminous crops instead. (Ochieng, et al., 2012).

Figure 1: Rainfall in Western province



Data: (Famine Early Warning Systems Network, 2013)

Table 2: t-test for equality of means between groups, outcome variables

	Observations control	Observations treated	Mean control	Mean treated	Difference in mean	SE
Extension						
Intercropped maize‡	358	285	0.86	0.91	-0.05	0.02
Use of fertilizer‡	343	287	0.89	0.90	-0.01	0.02
Fertilizer dosage per acre (Kg/a)	288	203	40.81	50.70	-9.89*	3.98
Use of farm yard manure‡	340	279	0.64	0.70	-0.06	0.04
Manure dosage per acre (Kg/a)	340	279	0.47	0.55	-0.08	0.06
Use of hybrid/OPV seeds‡	344	281	0.93	0.90	0.03	0.04
Surplus maize was stored‡	343	285	0.77	0.62	0.14***	0.04
Maize for HH consumption‡	364	289	0.24	0.26	-0.02	0.03
Use of retention ditches‡	354	288	0.19	0.20	-0.02	0.03
Use of water pans‡	354	288	0.03	0.08	-0.05**	0.02
Use of cut-off drains‡	353	288	0.29	0.24	0.05	0.03
Use of dams‡	354	287	0.01	0.00	0.01	0.00
Use of waterholes‡	354	288	0.11	0.11	-0.00	0.03
Use of irrigation canals‡	354	288	0.13	0.13	-0.00	0.03
Use of roof catchments‡	353	288	0.55	0.49	0.06	0.04
Output						
Total farmed acreage	364	289	3.60	5.25	-1.64***	0.45
Total nominal yield (kg)	364	289	1080.15	1250.11	-169.95	105.23
Total yield per acre (kg/a)	362	285	358.18	328.52	29.66	38.88
Revenue						
Total gross revenue (Ksh)	364	289	29253.08	33393.44	-4140.36	2503.79
Total revenue per acre (Ksh/a)	362	285	9883.29	8570.27	1313.02	760.87
Welfare						
Monthly HH expenditure (Ksh)	198	189	14739.38	19060.02	-4320.64**	1620.14
HH Expenditure per capita (Ksh)	198	189	2598.72	3093.39	-494.67	271.81
Below the extreme poverty line‡	364	289	0.44	0.38	0.06	0.04
Below the extreme poverty line‡	364	289	0.26	0.21	0.06	0.03
Faced hunger in 2011‡	362	287	0.73	0.74	-0.01	0.04
Hungry spell length	254	206	4.76	4.22	0.53***	0.15

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$, ‡ Yes = 1

We present here an overview of the “raw” effect of the programme, with a specific focus on the control group[21]. Those values later serve as benchmarks for our treatment effect estimates.

Control group respondents reported high rates of usage of certain technologies, such as intercropped maize (86%) and fertilizer use (89%). The fertilizer dosage is around 20% below the recommended 50kg/acre, but it still represents a good performance in the Kenyan context. It might be explained by the aggressive nation-wide fertilizer price subsidization policy of the Kenyan Government[22]. Animal manure can also be used as organic fertilizer, but since it was not bought[23], farmers often did not know the quantity that was applied (as it was not traded). Nonetheless, the Kenyan Agriculture Research Institute (KARI) recommends dosage of 4 tons of farm yard manure per acre, no way near the 0.64 kg per acre reported here. The control group also shows a high usage rate of high yield hybrid and open pollinated seeds varieties (OPV) at 93%. We were interested in evaluating the storage capacity of households, whether on farm or off-farm, since NALEP contributed to promote such infrastructures in order to foster resilience and income smoothing when drought or bumper harvest occur. In this context, 77% of the households in the control group reported storing surplus maize. We also looked at the probability that the maize was solely grown for household consumption, since that crop is a major staple in Kenya and especially in Western Kenya. 24% of the control households answered that it was mostly for household consumption. We then look at an array of water harvesting technologies – which are crucial inputs even in rain fed regions; we find that adoption rates are generally low, except for roof catchments, retention ditches and cut-off drains, which consists in a rudimentary methods.

We present in Table **2** only aggregated output data, in an effort of concision. We investigate the characteristics of farmed acreage (as opposed to total land owned), since a plot that performs

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[21] We use control group average values as a beachmark for interpreting the scalle of the average treatement effect on the treated.

[22] The nation wide subsidy (about 30% of the market price) is operationalized trough the National Produce and Cereal Board, using its wide range of facilities. In the case of Lugari, the main depot is located in Lugari town, rendering a similar access to the policy to both control and treated sampled households.

[23] 90% of the households held some amount of small livestock.

crop rotation will show a higher yearly acreage. Control households use on a yearly basis 3.60 acre, which generate 1.08 ton of aggregate crop output[24]. The unit-level yield per acre average corresponds to 358 kg per acre. Those crops generated a gross revenue for the control group of 29 253 Ksh per year, which relates to an average unti-level gross crop revenue per acre of 9 883 Ksh.

Using an aggregation of 19 expenditure item categories[25], we evaluated monthly household expenditures. It is common practice to use expenditures to evaluate income using expenditure, since it has a tendency to reflect a smooth income and avoids problems associated with recall bias (Atkinson & Bourguignon, 2000). That monthly figure amounted to roughly 14 700 Ksh (176 USD)[26], which corresponds to around 2 600 Ksh per capita (31 USD). Using the national poverty and extreme poverty lines[27], we created binary variables for poverty. The poverty rate in the control group (44%) corresponds roughly to the rate for Lugari made available by the Government of Kenya (47%). We have also included a more subjective outcome variable on food security, where the respondents were asked whether they faced hunger in the year previous to the survey. If so, they were then asked on the duration of that spell. Those last two outcome variables are more problematic to use, since they are highly subjective[28]. It is also probable that the treatment influences the way households perceive and define food insecurity. Also, assessment from the survey enumerators revealed that households were sometimes expecting interventions in their communities if their needs appeared urgent. Nonetheless, we decided to include those variables. Figures on food insecurity are much higher for 2011 at 73%, a potential

[24] The aggregate comprises output of the most common crops in Lugari : Maize, beans, sweet potatoes, sorghum, millet, kales, grain amaranth and sunflower.

[25] Those items include: Rent, school fees, food items, health care, energy (coal, wood, electricity, etc.), water, clothing, transport, telecommunication, domestic workers, bank repayments, savings payments, transfers, loans, purchases of land and other contributions. Figures were sometimes provided in yearly figures (i.e. school fees) and were adjusted to a monthly basis.

[26] 1 USD = 83.40 KSH, rate of February 1st 2012

[27] The indexed rural extreme poverty line corresponds to the theoretical rural extreme poverty threshold of Ksh 1 228 per month per adult equivalent. The rural poverty line is set at Ksh 1942 per month per adult equivalent. The national poverty line was last made available in 2005, we indexed if to February 2012 using CPI data from the Kenya National Bureau of Statistics.

[28] One could consider, for instance, that the treatment influences the way households perceive the very definition of food insecurity.

consequence of the drought and the destroyed crops in the second half of 2010, whereas the length of that spell was on average of 4.76 months, or 148 days.

4. Empirical approach

This study uses non-parametric propensity score matching (PSM) methods in order to estimate the effect of the treatment, which here refers to participating in the programme. This technique first introduced by Rosenbaum and Rubin (1983), involves pairing individuals among two groups, treated and control, using a large set of information on those individuals[29]. PSM is a popular method to evaluate the impact of economic policies on individuals or households (Lechner, 2002; Jalan & Ravallion, 2003).

An important issue that needs to be overcome using this strategy is how to ensure that selection problems do not bias the result. The challenge here is to reconstruct a control group that has the same observable characteristics as the treated group. PSM pairs individuals in a treated group (like households participating in NALEP) to individuals in an untreated group (like households not participating in NALEP) using a large set of observable information and assuming that the outcomes are independent of assignment to treatment, conditional on pre-treatment covariates, the method can lead to unbiased estimators of the treatment impact (Dehijia & Wahba, 2002). This technique is widely acknowledged (Heckman, Ichimura, & Todd, 1997; Dehejia & Wahba, 2002; Heckman & Navarro-Lozano, 2004) to produce unbiased results when the assumptions are respected.

Since no consistent baseline information was collected by the program, longitudinal techniques, such as Difference-in-Difference could not be used. The assignment rule of the treatment was not strict – it was based on an asset-scale that was locally established in each PAPOLD, and the roll-out assignment of the extension treatment has not been randomly assigned. Clearly, those issues restricted the analytical tools available to assess impacts of the programme. One option would have been to proceed with a simple linear regression with treatment as a control, but

[29] For an overview, Caliendo & Kopeinig (2005) provide a survey of the methodologies.

obvious problems related to selection would have arisen. Instead, we opted for the method of propensity score matching. Angrist and Pischke (2008) also argue that matching and regression are not that different, after all, up until the point when we specify a model for the score. That specification is, according to them, analogous to the problem of parametrization of the control variables in regression settings. They also add that PSM focuses researcher attention on models for treatment assignment, instead of the typically more complex and mysterious process determining outcomes, which is well suited for cases where treatment assignment is the product of human institutions or government regulations.

The impact of the intervention needs to be separated from what would have happened anyway to the individuals without the presence of the intervention, the so called counter factual. In order to do this, the observed outcomes need to be differentiated from individuals who were affected by the treatment and the counterfactual potential outcome, had the same individual been observable with and without treatment. This is obviously impossible, but the potential outcome $Y_i(D_i)$ for each individual i, where $i = 1, \ldots, N$ and N denotes the total population, is represented below in Eq. (1), where $Y_i(1)$ is the outcome for the treated individual i and $Y_i(0)$ is the outcome for the non-treated individual.

$$\tau_i = Y_i(1) - Y_i(0) \tag{1}$$

Clearly, τ_i cannot be observed, since an individual is either treated or not, so we focus instead on average population effects.

The parameter of interest is the average treatment effect on the treated (ATT)[30], defined as:

$$\tau_{ATT} = E[\tau|D = 1] = E[Y(1)|D = 1] - E[Y(0)|D = 1] \tag{2}$$

[30] Another parameter of interest could have been the average treatment effect (ATE), $\tau_{ATE} = E[Y(1) - Y(0)]$, but this parameter causes problems, since both counterfactuals have to be reconstructed, that is, $E[Y(1)|D = 0]$ and $E[Y(0)|D = 0]$. Most of the literature focuses on ATT.

The last term on the RHS in (2), $E[Y(0)|D=1]$, is obviously not observed. Using (2), if we add and subtract a term representing the unobserved effect on the treatment group, had they not be treated, we obtain the following:

$$E[Y(1)|D=1] - E[Y(0)|D=0] = \tau_{ATT} + E[Y(0)|D=1] - E[Y(0)|D=0] \quad (3)$$

The first term in (3) represents the effect we are trying to identify. The two last terms on the RHS, represent the *selection bias*. It captures the concepts that the treated group might have fared better than the control, even without treatment. The true parameter τ_{ATT} is only identified if:

$$E[Y(0)|D=1] - E[Y(0)|D=0] = 0 \quad (4)$$

It is usually argued that a randomization of the treatment assignment, thereafter "randomization", usually solves selection issues. In experimental economics, randomization is a popular way to address the issue of selection, but in an *ex-post* setting, such randomization is not possible. Instead, we need to add identifying assumptions to solve the selection problem.

If one can control for observable differences in characteristics between the treated and non-treated group, the outcome that would result in the absence of treatment is the same in both cases. This is to say, we re-create the counterfactual outcome of non-treatment on the treated. Following Rosenbaum & Rubin (1983), this assumption, called the conditional independence assumption (CIA), can hold under a specific setting: I) A very rich set of information on the households needs to be collected[31]. II) This is an untestable assumption (since one cannot observe the counterfactual status for each individual, only its "twin"). One possible identification strategy is to assume, that given a set of observable covariates, which are not affected by treatment, potential outcomes are independent of treatment assignment. Following

[31] Heckman et al. (1997) are specific about the criteria for the data to be of sufficient quality: (i) the same data sources (i.e., the same surveys or the same type of administrative data or both) are used for participants and nonparticipants, so that earnings and other characteristics are measured in an analogous way, (ii) participants and nonparticipants reside in the same local labour markets, and (iii) the data contain a rich set of variables that affect both program participation and labour market outcomes. They note that failure to meet those criteria diminishes the performance of the estimators greatly.

Rosenbaum and Rubin (1983), if $P(X) = P(D = 1|X)$ is the propensity score, then the balancing on pre-treatment given the propensity score is

Lemma 1: (Balancing) $$D \perp X|P(X) \tag{5}$$

Suppose that the assignment to treatment is unconfounded, thus

Lemma 2: (Unconfoundedness) $$Y(0), Y(1) \perp D|X, \qquad \forall X \tag{6}$$

This means that the selection is only based on observable characteristics and that all variables that influence treatment assignment and potential outcomes simultaneously are observed by the researcher.

On top of the lemmas, we need to add a condition in order to rule out the perfect predictability of D given X. This is called the common support condition and it is stated[32] as

$$0 < Pr(D = 1|X) < 1 \tag{7}$$

The last equation expresses the idea that two individuals with the same set of covariate X have a positive probability of being both treated and non-treated (Heckman, Lalonde, & Smith, 1999).

Assuming that the CIA and the common support condition hold, the PSM estimator for ATT can be generalized as:

$$\tau_{ATT}^{PSM} = E_{P(X)|D=1}\{E[Y(1)|D = 1, P(X)] - E[Y(0)|D = 0, P(X)]\} \tag{8}$$

where τ_{ATT}^{PSM} is, conditional on the common support condition, the mean difference in outcomes, weighted by the propensity score of the treated individuals.

Given the setting of our study, we identify several sources of potential bias when using PSM:

[32] Smith and Todd (2005) add that if the ATT is the parameter of interest, the required condition is in fact $Pr(D = 1|X) < 1$, because (7) only guarantees the possibility of a participant analogue for each nonparticipant.

Bias 1. Spillovers. A major problem with development programs is the "aid void": once the involvement of the donor/government/NGO is over, the initiatives die out. NALEP has deployed extensive efforts to enhance leadership capacities in the communities, for them to maintain the extension technologies and to share them among producers. Such "knowledge networks" are not necessarily community bound, meaning that with better roads and telecommunication, knowledge is likely to spread further than ever. This represents a great achievement of development projects – to enhance indigenous institutions and to foster "organic" development, yet it is a source of concern for our methodological purposes. If knowledge on agricultural technologies is likely to spread, how can we be certain that the control group has not been influenced in any way by NALEP? We cannot be completely certain about this. We have purposefully selected a control region that was separated by geographical barriers to avoid too close networks. Nonetheless, we cannot rule out completely the possibility that spillovers bring a downwards bias to the effect that we are attempting to measure, since knowledge spillovers to the control region would influence positively their yields.

Bias 2. Selection on unobservables. Another source of potential bias is that among our treated sample, unobservable characteristics (talent, motivation, etc.) might influence the participation of the households in NALEP, resulting in a treated sample that differs from the control, since it potentially includes a sample of different characteristics. In the absence of a good instrument for the treatment, we face a potential problem. To address this issue, we have assigned a treated status to whoever was located in a sub-location that was treated by NALEP. This constitutes a bold way to proceed, since we cannot be absolutely certain that the sampled households actually received the treatment[33]. We opted for this strategy

[33] We decided to proceed this way for several reasons. I) Opt-in rates were high according to the program: close to 90% of the households residing in a given treated sub-location actually took part in the programme, in a way or another. This rules out major issues regarding non-takers. II) Recall bias over 10 years means that the respondents might not remember whether the program was active in their location or whether they received extension services, but the programme documentation showed evidence that they had been treated. This decision nonetheless can generate concern regarding the common support condition, as it is meant to rule out perfect predictability of treatment. For that reason, we have selected, with the help of NALEP staff, households in the control group that would have been eligible (no large

because of the problems associated with recall bias, since the survey respondent might not remember about the specific programme. Assigning a treatment status to households, if it includes non-takers, has the potential to bias downwards the results. Nonetheless, participation rates in NALEP were usually quite high, meaning that the potential non-treated included in the treatment group should not be a source of major bias. It is nonetheless impossible to track perfectly who actually took part in the programme, among the treated sample.

If Lemma 1 is respected[34], observations with the same propensity score show the same distribution of observable characteristics, irrespective of their treatment status (Becker & Ichino, 2002). That is, we have a random exposure to treatment and treated and untreated observations are statistically identical when it comes to the covariates X used for the creation of the PS. From there, we can choose a standard probability model to estimate the propensity score. We have chosen to use the standard logistic probability model, expressed by equation (9).

$$\Pr\{D_i = 1 | X_i\} = \phi(h(X_i)) \quad (9)$$

Where ϕ denotes the logistic continuous distribution function and $h(X_i)$ is a function that includes all the covariates used to compose the score.

We make an assumption that an individual's programme participation decision does not depend on the decisions of others. This assumption would be violated if peer effects influenced participation. As discussed earlier, we assigned the treatment status to anyone who was located within the treatment region; hence this should not cause concern.

We formulate a final assumption, being that the treatment for all units is comparable (no variation in treatment). Are the treatments really comparable across the treated individuals?

scale farmers, similar socioeconomic conditions, etc.), in order to recreate a standard setting. Technically, this also means that our parameter of interest, ATT, actually corresponds to the intention to treatment effect (ITT), which does not differ from our actual interpretation.

[34] Lemma 2 is untestable. (Rosenbaum & Rubin, 1983)

Treatments are likely homogeneous within the locations, since the same officers are in charge of delivering extension services. Yet, as officers become more experienced, there is still a possibility that year over year the quality of treatment improved marginally.

4.1. Matching methods

Assuming that the assumptions above are respected, we can proceed further with the matching techniques. The matching approach, roughly speaking, aims at recreating the randomized setting of an experiment from a non-random sample. In an ideal setting, we would match treated and control households on exact covariates. This is unfortunately not possible in our sampling design[35] and we need to turn to various matching techniques that match our sampling groups using different criteria to find the degree of similarity in the probability in receiving the treatment. In this paper, we use stratification, kernel and nearest neighbour matching, and then compare the results across the methods.

4.2. Estimation of the propensity score

In principle, any discrete choice model would be suitable for determining which functional form to use. In practice though, logistic approaches are preferred, given the appeal of constraining predictions between zero and one. The model should only include variables that influence simultaneously the participation decision and the outcome variable. Obviously, the variables should be unaffected by treatment, or the anticipation of it. In the best case scenario, we would have measured those variables ex-ante, but this was not possible. Instead, we have opted for variables that are likely to be stable over-time. It is worth noting that some authors have pointed out that over-parameterized models should be avoided (Augurzky & Schmidt, 2001; Bryson, Dorsett, & Purdon, 2002), since the inclusion of additional variables in the model will increase the variance of the estimates. Table 3 presents the results of a logistic (logit model) for different specifications of the propensity scores. Those specifications inform us on the probability of participation in the extension program, given a set of covariates The first score (psm1) is a

[35] In cases, as the current one, where X is a set of various continuous variables, we would need an infinitely large set of data in order to match on exact covariates.

simple specification that includes only basic regressors. We present 6 different models, using different specifications in each to address the robustness of the chosen specification. All specifications respect the balancing condition. Models *psm1* and *psm5* show overall lower explanatory power, and both show the unattractive feature of only 4 strata. Figure A.5 in appendix plots the density function of the different propensity scores specifications. We can observe there that *psm5* produces the best distribution in terms of smoothness, while *psm6* has also a relatively smooth distribution.

Additionally, Table A.1 in appendix presents the comparison of overall significance level and overall mean standardized[36] bias, pre- and post-matching. In theory, after matching, we should observe a low significance level, an insignificant p-value and a decrease in overall mean standardized bias. Rosenbaum and Rubin (1985) note that the focus should be on individual covariate bias exceeding 20% Our most exhaustive models, *psm5* and *psm6*, show little significant bias[37] on individual covariates with 6.8% on the variable "Number of Household Members" and -10.6% for "Household has electricity" for *psm5*, while *psm6* shows 6.8% bias for the variable "Number of Household Members". An examination reveals that *psm5* minimizes the overall mean standardized bias by 51% (to 6.4%), and *psm6* reduces the bias by 50% (to 5.7%).

[36] Following (Rosenbaum & Rubin, 1985), mean standardized bias is the difference in marginal distributions of the control covariates (X). Formally, the pre-matching mean standardized bias is defined as $SB_{before} = 100 \times \frac{\overline{X_1} - \overline{X_0}}{\sqrt{0.5 \times (V_1(X) + V_0(X))}}$ and the post-matching mean standardized bias is defined as $SB_{after} = 100 \times \frac{\overline{X_{1M}} - \overline{X_{0M}}}{\sqrt{0.5 \times (V_{1M}(X) + V_{0M}(X))}}$, where $\overline{X_1}$ and $\overline{X_0}$ are the sample means in the treated and control group and $V_1(X)$ and $V_0(X)$ are the sample variances in the treated and control group.
[37] The results of covariate imbalances are not reported exhaustively in the paper.

Table 3 Logistic regression estimation of the propensity scores

	(1) psm1	(2) psm2	(3) psm3	(4) psm4	(5) psm5	(6) psm6
Education level of the head of the household (years)	0.0413*	0.0405*	0.0402*		0.0469*	0.180*
	(2.25)	(2.16)	(2.12)		(2.34)	(2.23)
Total Farm Acreage	0.0321	0.0198	0.0185	0.0153	0.0268	0.0312
	(1.60)	(0.95)	(0.86)	(0.70)	(1.28)	(1.45)
Roof of the main dwelling house is made of plain tin sheets		2.006***	2.011***	2.006***		
		(4.49)	(4.50)	(4.39)		
Household has electricity		-0.0658	-0.0636	-0.0722		-0.0424
		(-0.55)	(-0.53)	(-0.60)		(-0.34)
Number of living rooms in the main dwelling house			0.00605	-0.00350		-0.0146
			(0.19)	(-0.11)		(-0.45)
Cultivates maize			0.0826	0.0376		-0.129
			(0.18)	(0.08)		(-0.30)
Age of the head of household				0.0388	0.00587	0.0515*
				(1.71)	(1.53)	(2.15)
Squared age of the head of household				-0.000318		-0.000428
				(-1.50)		(-1.91)
Squared education of the head of household				0.00184		-0.00775
				(1.69)		(-1.82)
Male headed household					0.164	0.175
					(1.35)	(1.43)
Number of HH members					-0.00348	-0.0156
					(-0.17)	(-0.74)
Total Farm Assets Value					-0.000000438	-0.000000396
					(-1.54)	(-1.39)
Total Household Assets Value					2.06e-08	0.000000234
					(0.05)	(0.55)
Constant	-0.639***	-0.657***	-0.756	-1.554*	-1.075**	-2.492**
	(-3.36)	(-3.40)	(-1.52)	(-2.13)	(-3.23)	(-3.04)
Balanced	Yes	Yes	Yes	Yes	Yes	Yes
Number of strata	4	6	6	5	4	6
Observations	665	665	665	665	665	665
Pseudo R^2	0.009	0.051	0.051	0.054	0.017	0.025

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: *t* statistics in parentheses. The common support condition has been imposed to all regression

5. Results

We have chosen specific outcomes[38] that the NALEP program was targeting, in order to assess the long-term adoption rate of the program takers, vis-à-vis their control group peers. We basically see the outcomes separated in 4 main groups: adoption of extension methods, farm output, revenue, and basic household welfare.

Table 5 below presents the ATT estimates for our set of outcome variables, using the most exhaustive score specification (*psm6*). We also present in appendix, under Table A.2, the ATT estimates using an alternate score specification (*psm5*), for good measure. Each line corresponds to a separate estimation, where we used the three different matching methods defined previously, in order to assess the robustness of our estimation. The common support condition was also imposed on all estimations. As far as we know, there is no test for overall significance in a PSM setting, to handle problems related to multiple comparisons. Several estimations use an aggregate covariate as outcome variable (i.e. revenue, expenditures), while it was not possible to build a sensitive aggregate function for most technology adoption factors (since they are often dichotomous). Aggregate covariates serve as an index function, in order to tackle the issue of spurious relationships.

We see that the overall results are robust across the matching methods, with similar estimates and standard errors. Comparing the results between the use of *psm5* and *psm6*, we see little deviation on individual ATT estimates with the stratification and kernel matching method; significant ATT estimates vary on average[39] by 6.2% and 2.2% respectively, while nearest neighbour matching exhibits more sensitivity towards the score specification, as the estimates vary by 17.5% between the specifications.

[38] We perform 29 estimations, each over a key outcome. This can be seen as a large number of estimations, compared to the standard economic literature, yet among the research of agricultural programme evaluation, it is a rather common procedure. Also, it is worth reminding that we analyse technological packages that are likely to influence a set of outcomes.

[39] Taking the average absolute deviation of significant matched ATT estimates between psm5 and psm6.

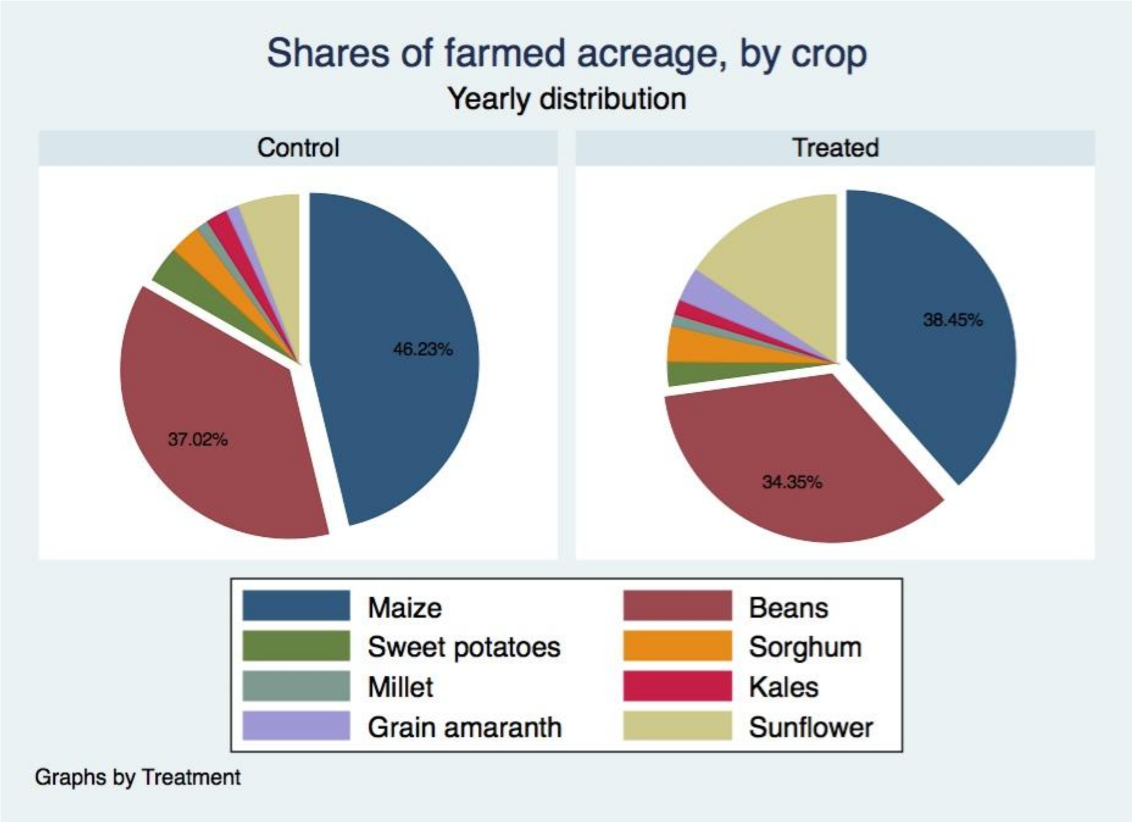
In terms of adoption of extensions methods, the ATT estimates[40] show that treated households use between 7.30 and 11.38 kg/acre of commercial fertilizer more than their control counterparts, although the significance varies across the methods. Control households use on average 40.79 kg/acre of commercial fertilizer, meaning that treated households reach on average the recommended fertilizer dosage (50 kg/acre). This represents an increase in fertilizer use of 23.78% on average. We did not otherwise find evidence that treated households were more likely to use other inputs such as high yielding seeds and open pollinated seeds varieties (OPV), probably because the usage rate of the control group is already very high (89%). On the topic of technology adoption, there are no sign showing that water harvesting technologies have been more adopted by the treated households.

In terms of post-harvest handling, we find that the households in the treated group were between 13% and 15% less likely to store the maize surplus. It is not clear what this last finding implies, as it could on one hand imply good marketing efforts to increase stock turn-over and reduce inventories, while it can on the other hand mean that treated households do not seem to respond to storage methods promoted by the program.

In terms of output, there is no sign that the treated households were more productive than the control households. The key element behind this farming might lay in the fact that Kenya was affected by the so-called corn lethal necrosis, as explained in introduction. A mitigation strategy promoted by the Ministry of Agriculture field staff was to rotate crops, more specifically to avoid planting maize during the 2nd harvest, as to eliminate the breeding ground for the virus. The outcome "total acreage farmed" translates the fact that treated households have farmed a larger number of acres, by 1.46 acre on average. Figure **2** below shows the composition of farm crop distribution on farms for the whole 2011 year (for both harvests). Even if we cannot draw any causal conclusion using such representation, we have evidence that the treated group seemed to have moved away from the traditional heavy reliance on maize. Instead we observe a substitution in production towards usually more marginal crops.

[40] All the following results are based on psm6.

Figure 2: Share of farmed acreage



We have also analysed the individual effect in price obtained from farmed crops. The results are presented in Table 4. It appears that for treated households, only maize has obtained a statistically significant different price at the market, at a premium of about 2.25 ksh/kg (+7.29% higher than the control value). It is unclear whether that difference in price compared to the control sample is due to increased bargaining power from the side of producers, or simply from scarcity of maize being produced, since a shift in production is likely to reduce the supply to local markets for the second harvest[41].

[41] There does exist any grading rating for crops in Kenya, hence the price difference cannot be attributed to difference in quality.

Table 4: ATT for prices using psm6

	Stratification			Kernel			Nearest Neighbour		
	ATT	SE		ATT	SE		ATT	SE	
Maize price per kg	2.44	0.51	***	2.41	0.72	***	1.89	0.61	**
Bean price per kg	2.17			2.61	1.46		3.35	1.94	
Sweet potatoes price per kg				2.08	3.35		1.83	3.01	
Sorghum price per kg				-0.73	3.66		-0.76	6.37	
Millet price per kg				3.64	10.29		9.12	7.07	
Kales price per kg				4.94	3.09		3.14	3.35	
Grain amaranth price per kg				-5.82	8.85		-1.27	3.36	
Sunflower price per kg	1.62			1.86	1.25		2.85	1.62	

The last topic we analyse is the basic household welfare. The first component we look at is the total monthly household expenditures. ATT estimates are somewhat stable across the methods and specifications, from 4481Ksh to 6617 Ksh per month. That value is also significant when decomposed at the per capita level, 568Ksh to 812ksh per month per person. This result, combined with the earlier rather inconclusive results on farm output and revenue, suggests that the increased revenues for the treated households lay outside the spectrum of farm productivity, most likely in off-farm revenues. It is worth noting that the CIG structures and the management skills part of the curriculum of the NALEP trainings are likely to have affected the entrepreneurial skills of the treated individuals, hence at least partly explaining this large difference in off-farm revenue for the treated group. Also, one shortcoming identified by the program in terms of design is that the NALEP beneficiaries did not have access to proper credit services, as current microfinance schemes were aimed mostly at small shopkeepers and classic banking services to wealthier individual. This means that the large difference in monthly expenditure is unlikely to be explained by extensive borrowing by the treated households.

Finally, we find that treated households were as likely to declare that they were food insecure in 2011 as the control group, in terms of prevalence. That said, ATT estimates show that treated food insecure households' hungry spell length in 2011 was shorter than control households, by between 16 and 20 days a year[42], with significant results. Again, we cannot formally identify the

[42] The results in the table are shown in *months*, and they have been transposed in the text into *days*.

mechanism behind those dynamics in food security. Moreover, as noted in the data section, that variable is potentially problematic because of the potential influence of the programme on how households define "hunger".

Table 5 ATT matching estimates using psm6

	Stratification		Kernel		Nearest Neighbour	
	ATT	SE	ATT†	SE‡	ATT	SE
Extension						
Intercropped maize	0.04	0.02	0.04	0.03	0.00	0.03
Fertilizer use	0.01	0.02	0.01	0.03	-0.02	0.03
Fertilizer dosage	11.38		10.42	3.31 **	7.30	5.14
Manure use	0.04	0.04	0.05	0.04	0.09	0.05
Manure dosage	0.08	0.05	0.08	0.05	0.06	0.07
Use of hybrid/OPV seeds	-0.04	0.04	-0.02	0.04	-0.17	0.08 *
Surplus maize is stored	-0.15	0.04 ***	-0.14	0.03 ***	-0.13	0.05 **
Maize for HH consumption	0.02	0.03	0.02	0.03	0.01	0.05
Use of water retention ditches	0.03	0.03	0.02	0.03	0.03	0.04
Use of water dams	-0.01	0.00	-0.01	0.01 *	-0.01	0.01
Use of waterholes	0.00	0.03	0.00	0.03	-0.01	0.04
Use of irrigation canals	0.00	0.03	0.00	0.03	0.00	0.04
Use of roof catchments	-0.05	0.04	-0.06	0.04	-0.02	0.05
Output						
Total acreage farmed	1.42	0.39 ***	1.57	0.48 **	1.40	0.43 ***
Total production	140.25	105.10	158.63	108.75	101.03	154.94
Total yield per acre	-37.51		-33.36	36.74	-30.77	51.76
Gross revenue						
Total crop revenue	2836.17	2519.94	3785.90	2688.58	3339.14	3306.32
Total crop revenue per acre	-1641.93		-1386.98	712.01	-793.98	1032.56
Welfare						
Total household expenditure	4999.45		4481.12	1507.32 **	6617.27	1789.35 ***
Per cap HH expenditure	568.39		569.50	239.49 *	812.02	315.61 *
Below poverty line	-0.05	0.04	-0.06	0.04	-0.07	0.05
Below extreme poverty line	-0.05	0.03	-0.05	0.03	-0.06	0.05
HH experienced hunger in 2011	0.02	0.04	0.00	0.03	0.08	0.05
Length of hungry spell in 2011			-0.52	0.16 ***	-0.66	0.22 **

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: The stratification matching method sometimes led to inconclusive results (unavailable ATT, SE, or both). This is due to the absence of sufficient observations in certain strata.
†: Kernel matching computed using a bandwidth of 0.06
‡: Bootstrapped kernel matching standard errors computed using 50 replications.

5.1. Sensitivity analysis

Following Ichino, Mealli and Nannicini (2008), we tested our results for their sensitivity to the failure of the CIA. The technique they propose aims at evaluating the sensitivity of point estimates of the ATT under various scenarios of deviation of the CIA. The scenarios are based on different values of a simulated confounder U as a matching parameter, generated using different parameters defining the distribution of *U*. Given those parameters, we predict a value of the confounding factor for treated and control households and then re-estimate the ATT including the confounder *U*. Following Ichino, Mealli and Nannicini (Ibid), we characterize a binary confounder by the parameters

$$Pr(U = 1|D = i, Y = j, W) = Pr(U = 1|D = i, Y = j) \equiv p_{ij}$$

where $i, j \in \{0,1\}$ and W a set of observable covariates. The arbitrary values of p_{ij} are based either on calibrated values or on simulated values based on the distribution of observable binary covariates. Using those different values, we repeat the estimation 100 times and we average out the ATTs obtained in the simulation. This in turn leads to a point estimate of the ATT given a deviation of the CIA under specific parameters. In order to evaluate the size of the effect of *U* on the relative probability to have a positive outcome in case of no treatment, the authors propose to evaluate a logit model of $Pr(Y = 1|D = 0, U, W)$. They define it the "outcome effect" and it is characterized as the average estimated odds ratio of the variable *U*:

$$\frac{\frac{P(Y = 1|D = 0, U = 1, W)}{P(Y = 0|D = 0, U = 1, W)}}{\frac{P(Y = 1|D = 0, U = 0, W)}{P(Y = 0|D = 0, U = 0, W)}} = \Gamma$$

The authors propose to evaluate the so-called "selection effect" by evaluating the logit model $Pr(D = 1|U, W)$, where the odds ratio of *U* would measure the effect of the confounding factor on the relative probability of treatment. It is expressed by:

$$\frac{\frac{P(D = 1|U = 1, W)}{P(D = 0|U = 1, W)}}{\frac{P(D = 1|U = 0, W)}{P(D = 0|U = 0, W)}} = \Lambda$$

Table 6.A to 6.C below show the results of sensitivity analyses using the nearest neighbour matching method for the outcome variables *total farmed acreage*, *stored surplus maize* and *total crop revenue per acre* respectively. We chose the 2 first outcome variables as they represented key findings, while the third outcome variable was tested as placebo, since no effect was found on the latter. The first line of each table shows the baseline ATT and standard error, in order to facilitate the comparison with the ATTs for the simulated confounders. In all cases, the neutral confounder has practically no effect on Γ and Λ , as well on the ATT. The confounder based on the distribution of the binary variable "gender of the head of the

household” in Table 6.A has a p_{11} value of 0.87, meaning that 87% of treated households are headed by male, we impose an identical fraction of households advantaged – say by a skill (the unobservable confounder we are trying to simulate) – and headed by a male (while that is not necessarily the case) and are assigned a value U of 1. The same logic applies for all p_{ij} values in the *confounder like* sub-table. In Table 6.A, we see that in the instance of the simulated confounder based on the ownership of a mobile phone there is no strong effect on the probability of positive total acreage farmed in the case of no treatment ($\Gamma \approx 1$), as well as the probability of being treated ($\Lambda \approx 1$). Ichino et al. note that both the outcome and selection effects need to be strong in order to influence the ATT and standard errors. This represents a key finding, since the total acreage farmed is attributed to government policies and the diffusion of information related to those policies, and it does not seem that the access to means of communication has an effect on the use of government policies. Therefore, the outcome can be attributed to the NALEP treatment. The ATT also does not differ from the baseline scenario under that confounding factor. The confounder based on the binary variable “bicycle ownership” generates higher values for Γ and Λ, yet the ATT also remains constant with respect to the baseline scenario. Overall, we observe strong robustness in the ATT point estimates, as they do not vary across the various simulated confounder. The same conclusions apply to the simulated confounders included in matching methods with the outcome variable “surplus maize is stored” and the placebo test on the variable “total crop revenue per acre”, whereas the simulated deviation of the CIA does not influence the point estimates for ATT.

Table 6.A Sensitivity analysis on the variable "total acreage farmed"

	Fraction U=1 by treatment/outcome				Outcome effect	Selection Effect	ATT	SE
	p_{11}	p_{10}	p_{01}	p_{00}	Γ	Λ		
No confounder	0.00	0.00	0.00	0.00	-	-	1.40	0.43
Neutral confounder	0.50	0.50	0.50	0.50	1.02	1.03	1.40	0.43
Confounder like:								
Head of HH Gender	0.87	0.75	0.76	0.74	1.17	1.37	1.40	0.43
Owns mobile phone	0.86	0.88	0.87	0.88	1.05	0.98	1.40	0.43
Owns bicycle	0.61	0.48	0.49	0.38	1.73	1.68	1.40	0.43

Table 6.B Sensitivity analysis on the variable "Surplus maize stored"

	Fraction U=1 by treatment/outcome				Outcome effect	Selection Effect	ATT	SE
	p_{11}	p_{10}	p_{01}	p_{00}	Γ	Λ		
No confounder	0.00	0.00	0.00	0.00	-	-	-0.13	0.05
Neutral confounder	0.50	0.50	0.50	0.50	1.00	0.99	-0.13	0.05
Confounder like:								
Head of HH Gender	0.80	0.81	0.74	0.73	1.05	1.55	-0.13	0.05
Owns mobile phone	0.92	0.79	0.89	0.81	2.13	1.05	-0.13	0.05
Owns bicycle	0.50	0.59	0.38	0.40	1.00	1.89	-0.13	0.05

Table 6.C Sensitivity analysis on the variable "total revenue per acre"

	Fraction U=1 by treatment/outcome				Outcome effect	Selection Effect	ATT	SE
	p_{11}	p_{10}	p_{01}	p_{00}	Γ	Λ		
No confounder	0.00	0.00	0.00	0.00	-	-	-793.98	1032.56
Neutral confounder	0.50	0.50	0.50	0.50	1.02	1.04	-793.98	1032.56
Confounder like:								
Head of HH Gender	0.84	0.78	0.76	0.73	1.24	1.39	-793.98	1032.56
Owns mobile phone	0.90	0.86	0.94	0.82	3.97	0.98	-793.98	1032.56
Owns bicycle	0.47	0.57	0.49	0.33	2.02	1.64	-793.98	1032.56

6. Conclusion

This paper was taking on a complex challenge; to evaluate the long-run effect of a government run extension program on households. Kenya, as many other sub-Saharan countries, has the potential to uplift millions of households through increased farm productivity and marketing. This paper emphasized the crucial methodological aspects required for this type of *ex-post* analysis, which is a setting that is less resource intensive than randomized experiments.

According to the PSM estimation performed in this paper, there are clear signs that the program has had impacts on the treated households. The increased quantity of fertilizer applied, corresponding to the recommended dosage, is one of the positive outcome of the program. The response of treated households in terms of pest mitigation and crop rotation is also crucial and an important finding. While this is the case, the scope of those impacts are clearly not corresponding to the general objectives the program had set. We could not find evidence of improve productivity or improve irrigation, for instance. Overall, the program was aiming at uplifting agriculture in terms of productivity and to fight poverty. From our perspective, structural obstacles – such as land rights, access to credit, plot sizes, integrated value chains and specialization of farmers – restricted what the program could achieve in practice. We believe that the program was conscious of those constrains, as many of the aforementioned constrains are addressed by the program that followed NALEP, the Agriculture Sector Development Support Program (ASDSP). Nonetheless, the results show that the treated households spend more every month than the control households, while production and productivity is unchanged, suggesting that off-farm incomes are greater for the NALEP beneficiaries.

The question is then: Why didn't the program lead to better results? There is much room for speculation, but certain answers can be sketched. One explanation would be that behavioural economists point to the works of Dupas (2009) and Ashraf, Berry, & Shapiro (2007) that suggest that the price of government provided services might affect the way beneficiaries value the services, and make use of them. As NALEP provided the service for free, as opposed to the recent Payment for Ecological Services (PES) approach to extension in Uganda, it is possible that the NALEP beneficiaries did not engage with the treatment to a sufficient extent. Another explanation of the limited impact of NALEP is the very nature of the beneficiaries. For instance, the Government of Kenya has laid down plans to modernize the country by 2030 and has set goals of increased food production, as well as improving the country's food security status. The "low hanging fruit solution" for this master plan would for instance involve working closely with large-scale farmers. NALEP targeted the very opposite side of the spectrum of agriculture workers: the poor, small landholder and vulnerable. With such a selection of beneficiaries, it is arguably understandable that the returns to the program are low, since the beneficiaries are generally less endowed. Another explanation, more classical in the context of publicly provided services, is the limited resources of the program. As stated earlier, the program was supposed to spend 3 years in a focal area, but in practice the involvement was only for one year. This reduces drastically the potential for learning agricultural techniques that require timing and happen only once a year (i.e. water harvesting, top-dressing). Hence, it is possible that the knowledge retention on timely, but crucial, activities has been limited due to limited resources.

In terms of bias of our results, it is also possible that unobservables played a role overall, even after testing for sensitivity to deviation of the CIA, as we could not match households on soil quality. Our setting is also prone to problems related to aggregate shocks at the sub-location level, which is an issue that is yet little understood.

The impact of the adoption of technological packages has not received a lot of attention from researchers within the field of economics, and it deserves to change. Further applied research should include better information on the incidence of treatment and the extent of the knowledge that remains overtime. Godtland et al. (2004) investigated the effect of farmers field schools (FFS) on potato farmers in Peru, and more of such research should be conducted. Also, this paper is the result of a cooperation between the programme itself and academics, which has yielded positive externalities for the programme in developing new evaluation techniques for their programming purposes.

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APPENDIX

Figure A.3: Map of Kenya and location of Lugari district

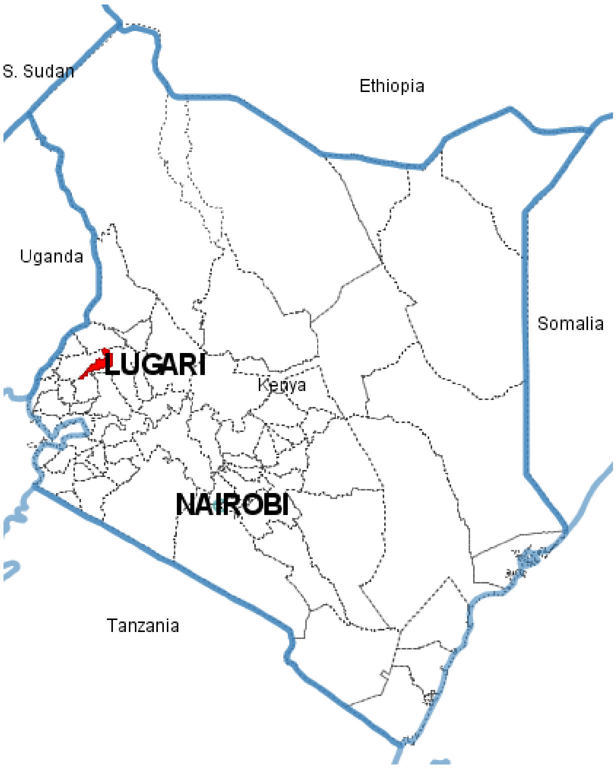

Figure A.4: Detail of Lugari District

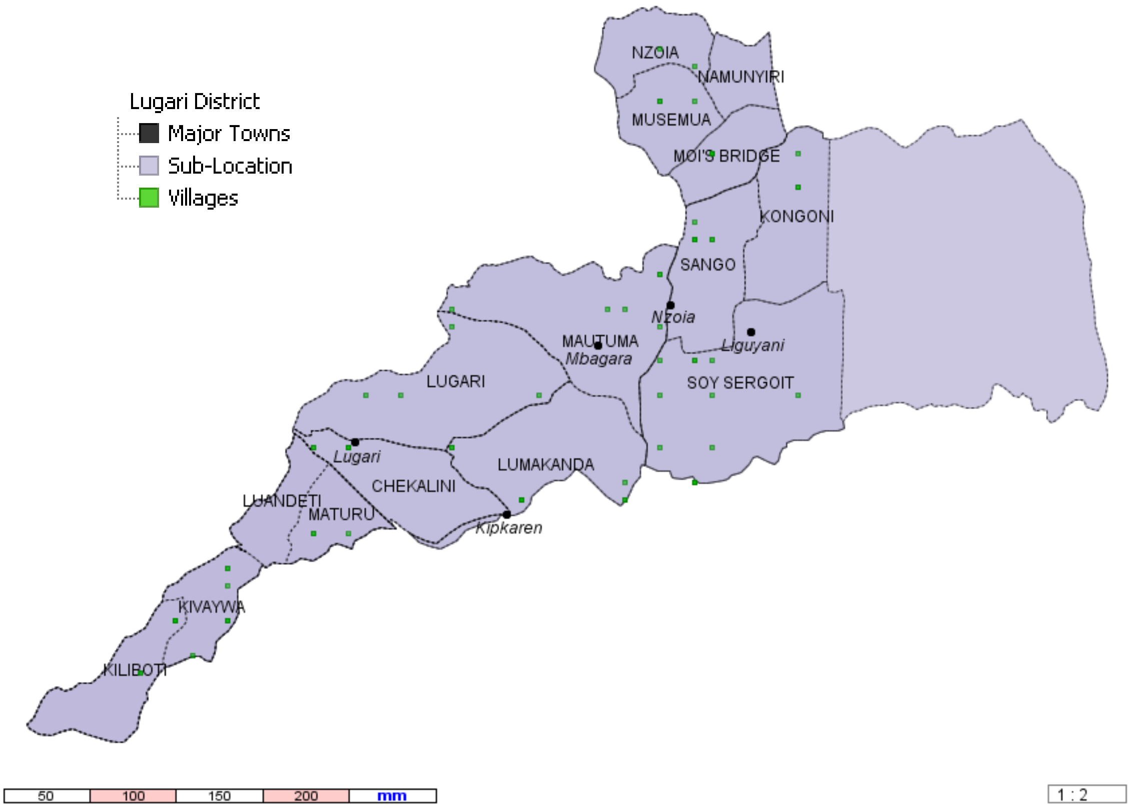

Table A.1 : Matching estimates bias

Score		pseudo-R^2	Chi-Square P	Mean Bias	Median bias
psm1	Before matching	0.004	0.122	8.443	8.443
	After matching	0.000	0.874	5.307	5.307
psm2	Before matching	0.048	0.000	15.454	8.443
	After matching	0.077	0.000	16.479	8.524
psm3	Before matching	0.048	0.000	12.745	7.661
	After matching	0.084	0.000	12.973	6.070
psm4	Before matching	0.051	0.000	12.003	9.603
	After matching	0.089	0.000	10.329	5.666
psm5	Before matching	0.018	0.017	9.659	10.802
	After matching	0.014	0.093	6.397	4.943
psm6	Before matching	0.029	0.008	8.591	10.385
	After matching	0.041	0.001	5.723	5.663

Figure A.5: Density distribution of the different score specifications

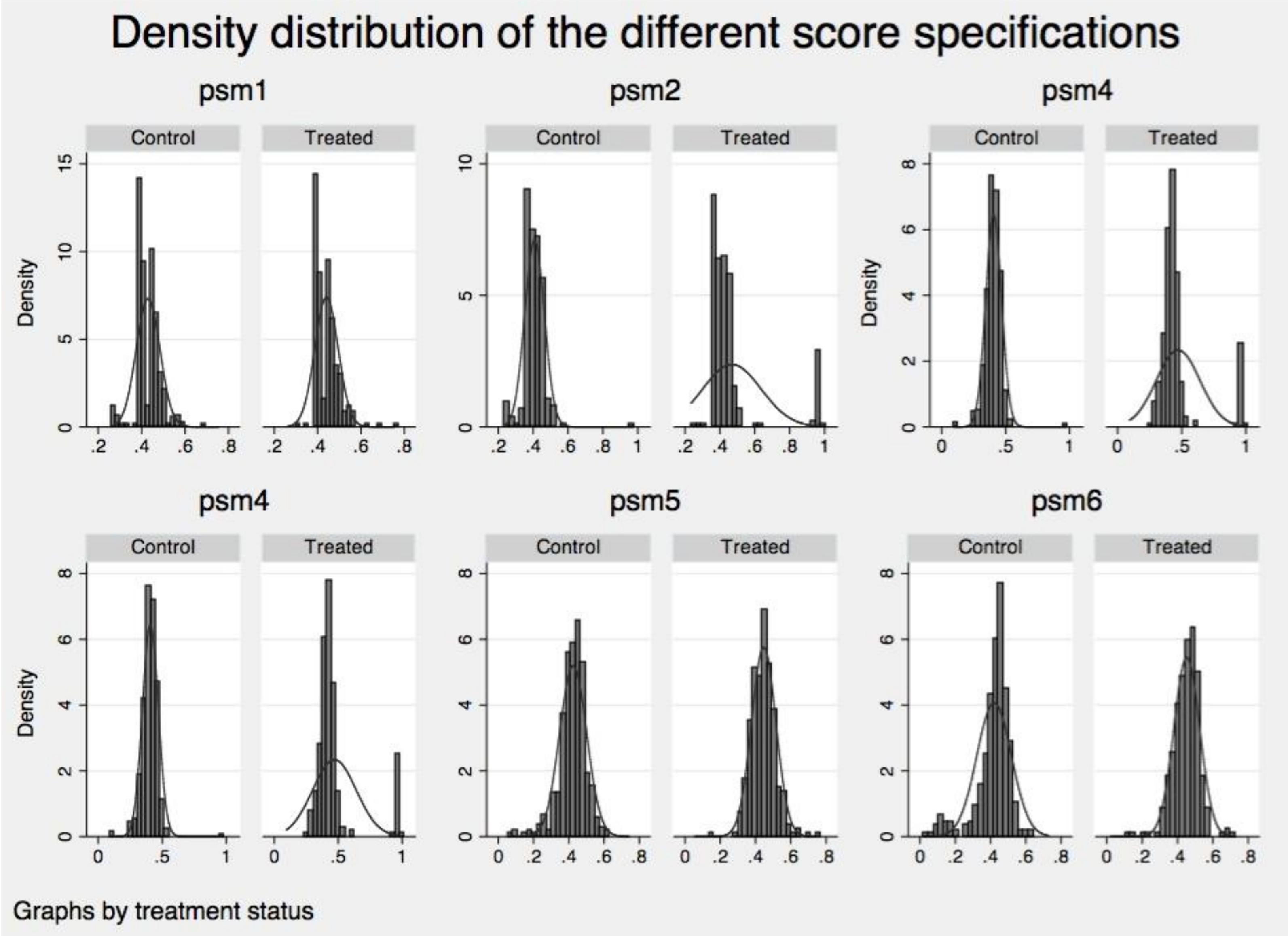

Table A.2 : ATT estimates for various outcomes variables, using psm5

	Stratification		Kernel		Nearest Neighbour	
	ATT	SE	ATT	SE	ATT	SE
Extension						
Intercropped maize	0.04	0.02	0.04	0.02	0.07	0.04 *
Fertilizer use	0.01	0.02	0.01	0.02	0.01	0.03
Fertilizer dosage	10.08		10.08	3.60 **	12.87	4.27 **
Manure use	0.06	0.04	0.06	0.04	0.03	0.05
Manure dosage	0.09	0.05	0.07	0.05	0.12	0.07
Use of hybrid/OPV seeds	-0.04	0.04	-0.04	0.04	-0.07	0.07
Surplus maize is stored	-0.13	0.04 ***	-0.13	0.04 **	-0.17	0.05 ***
Maize for HH consumption	0.01	0.03	0.02	0.03	-0.06	0.05
Use of water retention ditches	0.01	0.03	0.02	0.03	-0.02	0.04
Use of water dams	-0.01	0.00 *	-0.01	0.01	-0.01	0.01
Use of waterholes	0.00	0.03	0.00	0.02	-0.03	0.03
Use of irrigation canals	0.00	0.03	0.00	0.03	-0.06	0.04
Use of roof catchments	-0.05	0.04	-0.05	0.04	0.00	0.05
Output						
Total acreage farmed	1.14	0.41 **	1.52	0.40 ***	1.71	0.39 ***
Total production	306.27	209.67	299.70	222.78	129.21	230.72
Total yield per acre	-7.55	42.67	-16.76	48.87	-73.02	52.24
Gross revenue						
Total crop revenue	3369.34	2575.58	3584.02	2446.62	1085.75	3254.39
Total crop revenue per acre	-1124.41	770.05	-1301.66	843.21	-2364.83	998.95 *
Welfare						
Total household expenditure			4131.56	1996.16 *	3406.22	1949.83
Per cap HH expenditure			515.27	283.87	486.79	349.73
Below poverty line	-0.05	0.04	-0.06	0.03	-0.02	0.05
Below extreme poverty line	-0.05	0.03	-0.05	0.03	-0.02	0.05
HH experienced hunger in 2011	0.02	0.04	0.00	0.03	0.02	0.05
Length of hungry spell in 2011	-0.53	0.12 ***	-0.53	0.15 ***	-0.61	0.20 **
HH experienced hunger in 2010	0.06	0.02 ***	0.06	0.02 ***	0.06	0.02 **
Length of hungry spell in 2010			1.16	1.48	0.90	0.54

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: The stratification matching method sometimes led to inconclusive results (unavailable ATT, SE, or both). This is due to the absence of sufficient observations in certain strata.

†: Kernel matching computed using a bandwidth of 0.06

‡: Bootstrapped kernel matching standard errors computed using 50 replications.