IMPROVING TECHNOLOGY ADOPTION IN AGRICULTURE THROUGH EXTENSION SERVICES: EVIDENCE FROM URUGUAY

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Abstract

This paper evaluates whether cost-sharing public interventions are successful in promoting agricultural technology uptake by small and medium farmers, and whether these changes can affect yields. Our paper contributes to the debate by providing empirical evidence, which is scarce in the literature, from a program offering extension services to fruit producers in Uruguay. Using a unique panel dataset, we estimate a fixed effects model for the impact of extension services on technology adoption and yields. We find evidence that the program increased density of plantation. Once we address small sample issues, we also find some evidence of impact on the adoption of improved varieties. However, we find no evidence of impact on yields for the period under study. Although this lack of effects on yields could be due to the limited timeframe of the evaluation and does not rule out effects on other measures of productivity, it may also indicate that the practices promoted by the program are insufficient to induce a detectable impact on productivity and, consequently, sustainable benefits for the farmers. The study, therefore, confirms the need of including the design of impact evaluations in the policy design in order to properly consider the timing of all the potential effects and produce conclusive findings and precise recommendations.

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I. Introduction

With perfectly functioning markets that capture all costs and benefits, and in absence of informational or credit constraints, the necessary adjustments in technology and production methods to increase farms' efficiency could be left to farmers' individual decisions. In that ideal world, farmers would adopt those changes that are profitable for them without further need for intervention. Nevertheless, the situation of farmers in developing countries is typically far from the ideal one. Informational failures, credit constraints and economies of scale are widely spread and they justify the provision of extension services as a way to help farmers to adopt the required technologies. This is the rationale for many public programs aimed at fostering the adoption of new technologies.

In this framework, governments have developed a wide spectrum of possible policy solutions. Over time, the traditional model where the public sector was responsible for both financing and providing extension services has been substituted or complemented by demand-driven programs with different degrees of privatization and decentralization. This trend has been particularly important in Latin-America since the early 90s, when severe public budgetary constraints induced many governments to reduce the role of the public sector to the co-financing of extension services provided by the private sector.

Although the evaluation of this kind of interventions is extremely relevant for the design and implementation of public agricultural policies, the literature providing empirical evidence on their effectiveness is scarce. This paper aims at partially filling this gap by estimating the effect of public interventions based on the co-financing of private extension services. For this purpose, we focus on the Farm Modernization and Development Program (PREDEG for its name in Spanish), aimed at fostering technology adoption by fruit producers in Uruguay.

Because the program under study was not randomised, we rely on panel data methods to minimise existing biases arising from the selection of beneficiaries. The results point to positive effects of the program on improved production techniques, such as the density of plantation and the rate of adoption of certified varieties of apples and peaches. However, we do not find evidence of impact on yields, which raises some serious concerns about the ability of these techniques to produce real sustainable benefits for the adopting farmers.

This paper provides two contributions to the current discussion about technology transfer services in agriculture. First, it assesses the effectiveness of this particular form of cost-sharing intervention, shedding some light on the debate about alternative ways of financing and delivering technology transfer services. Second, it constitutes one of the first attempts to apply impact evaluation techniques to the assessment of agricultural technology transfer policy in Latin America.

The paper is organised into seven sections. After this introduction, section II outlines the rationale of public intervention in extension services, and the existing evidence on their effectiveness. Section III describes the program and the context of its application in relation to the Uruguayan fruit production sector. Section IV presents the dataset and summary statistics. Section V and VI discuss the methodology and results. Finally, section VII identifies policy recommendations and concludes.

II. Public intervention in extension services: rationale and effectiveness

Barriers to the adoption of agricultural technologies are widely diffused in developing countries and are often pointed out to justify public intervention in extension services. Jack (2009) groups these barriers in two categories: i) barriers that make a technology that is beneficial for society not beneficial for the individual farmer, and ii) barriers that discourage the adoption even when a technology is potentially profitable for the individual farmer. In the former case, extension services can be considered as an instrument to help farmers to adopt technologies while, in the latter, they can be seen as an additional agricultural input, whose supply and demand could be constrained by specific barriers.

The first group of barriers originates from the presence of externalities, non-competitive market structures and distorted prices, economies of scale or coordination problems. In these cases, farmers do not face the correct incentives to produce certain varieties, to use certain agricultural inputs or to adopt new techniques, resulting in production levels that are not socially optimal[1]. The second group of barriers usually derives from the lack of information on potential benefits, which restrict the demand for extension services, and credit constraints, which can prevent a farmer from purchasing them.

The public support of agricultural extension services has also been justified on equity grounds. If less advantaged farmers are also the most exposed to adoption barriers because of limited resources (lack of market power in an oligopsony, limited access to credit, low capacity to pay for extension services[2]), the justification for solving these market failures through public intervention gains even more relevance.

Although these arguments justify public intervention, the question remains of whether the government should provide the services itself or instead set the institutional structure to create a market and finance private providers. After all, despite the existence of appropriability issues and externalities, extension services are expected to increase the value of production, and have a monetary value that could be charged to the beneficiaries.

Nevertheless, incentives to set or to accept market prices are complex, due to asymmetric knowledge of the value of the service between government (principal) and private provider (agent), heterogeneity of beneficiaries and the uncertainty of agricultural activities. As a consequence, the design of outcome-based payments that create the right incentives between the government and the provider has proven to be difficult. In addition, the high cost of monitoring private providers and the availability of information for effective targeting by the public sector might still justify public financing and provision of services in some settings[3].

In general, the scarce existing empirical evidence on the effectiveness of this kind of programs points towards highly heterogeneous impacts of extension for different groups of agents. There is little evidence of comparative impacts of extension across different modalities of extension programs (such as privatization, fee-for-service, decentralization)[4], but a large heterogeneity of impacts is found even for a given modality of extension.

The main approach taken by the literature evaluating the impact of these programs includes extension services as an input in a production function. Findings reveal, in general, large positive rates of return to extension services (Birkhaeuser et al., 1991). However, a serious weakness of this approach is that in absence of an experimental design, estimates are likely to be biased due to non-random selection of participants[5].

This is particularly relevant in the case of demand-driven programs, such as the PREDEG, where farmers contact extension agents on the basis of their own personal characteristics. In this context, selection bias may be driven by self-selection (if for example participants differ from nonparticipants in their motivation, entrepreneurial behaviour, risk aversion, and so forth) or administrative bias (if for instance extension agents are more likely to contact more productive farmers). Since these factors are unobservable, they cannot be accounted for with a simple least squares regression approach.

Among the few studies that randomly allocate extension services, Duflo, Kremer and Robinson (2011) show that extension services significantly increase the use of fertilizers for farmers in Kenya, which in turn increases revenues. However, this study mainly focuses on fertilizer use and they do not measure the direct effect of extension on yields.

In absence of randomised assignment of extension services, an increasing number of studies try to control for selection bias with non-experimental methods. For example, using matching techniques, Godtland et al. (2004) find significant positive effects of a farmer field school program and a traditional extension program on farmer's knowledge of integrated pest management practices. Feder et al. (2003) find no impact on yields or on the reduction of pesticide use in Indonesia using a modified differences-in-differences model that accounts for the fact that the program was introduced at different times across villages. Praneetvatakul and Waibel (2006) use a four-year panel comprising eight rice-growing seasons in Thailand and detect a positive effect on knowledge and pest management practices in both the short and long run, although in a companion paper they find no evidence of impact on yields.

Some studies using instrumental variables find generally positive though small and heterogeneous effects. For instance, Akobundu et al. (2004) use the distance from the extension office, whether an individual was rejected a loan, total farm debt, and the previous visit of an extension agent as instruments for participation and find a positive impact on farm income only for individuals with a high number of visits. Furthermore, farmers with better education, more skills and wealthier are more likely to adopt certain kinds of innovations that are more dependent on knowledge. Wealth and size of the farm can have similar effects on extension through the adoption rate, in accordance to the results of Godtland et al. (2004) who find that the farmer field school approach is effective for wealthier farmers. However, the use of instrumental variables in this context is not widespread because of the difficulty to find a credible instrument, since the criteria used to select farmers are usually correlated with the outcome; in general, this can only be addressed by relying on strong distributional assumptions such as the joint normal distribution of errors (Romani, 2003; Feder et al., 2003).

A quick overview of the current literature on the impact of extension services reveals that the evidence is scarce and tends to find a positive effect, which varies significantly according to farmers' characteristics such as education, experience and wealth. As noted by Evenson (2001), the estimated rates of return in developing countries vary widely, between 5 per cent and 50 per cent, which points to the specificity of impacts depending on the particular design of the program, and on the characteristic of farmers that are receiving the extension services.

III. The PREDEG program

During the early years of the 1990s the Uruguayan agricultural sector started to face stronger international competition. The conditions were not favourable: the productivity of the sector was low and the quality of the products was not up to international standards. Additional problems were the lack of coordination between production, processing, and marketing activities, and very limited access to credit, especially for small producers. These factors were important constraints for the changing environment during the period and contributed to create many of the barriers to technology adoption mentioned in section II.

These problems were also severely affecting the orchard sector, which was relatively small at the time. In the year 2000, the total production of orchards amounted to US$43 million, which means 2.3 per cent of the total agricultural sector and 33 per cent of the fruit sector (DIEA 2003). The production chain was particularly disarticulated and adopted varieties were not in line with the international quality standard. The sector was composed of small and scattered producers who were mainly oriented towards the internal market.

In addition, some orchards experienced severe exogenous shocks. Between 1990 and 2001, peach production was strongly affected by root asphyxia, which reduced the plant stock to one-half.

We study the effect of PREDEG on farmers who were benefited by the program between 2000 and 2005. The program aimed at boosting productivity, at increasing net income for producers and at raising quality standards to the level required by international trade. The method of intervention included four components: i) technological development, ii) quality control, iii) marketing development, and iv) institutional strengthening.

The first component, which is the topic of this paper, was central to the program design. It received around US$ 22 millions, almost half of the program's financial resources, which represent approximately 18 per cent of the total production of the fruit sector in 2000. It included four areas of involvement: i) introduction of improved plant varieties, ii) validation and adaptation of new technology, iii) training and iv) technical assistance.

The program adopted a 'partially public-funded private services' approach[6], requiring beneficiaries to finance part of the cost of adopting new varieties and of the training and technical assistance (TTA) services, which would be delivered by private companies and NGOs. The program targeted mainly small-scale and medium-scale producers. The program financed between 50 and 70 per cent of the total cost; unfortunately, we do not count with information of the amount actually financed for each producer, which prevent us from analyzing the effect of the amount of financing on the outcomes of interest. Also we have limited access to the program administrative costs, a limitation that does not allow us to perform any ex-post cost-benefit exercise.

According to a previous qualitative evaluation[7], the selection of beneficiaries was producer-driven, resulting in some heterogeneity among users. The program had few official restrictions to participation.[8] However, the co-financing initial requirement turned out to be an entry barrier for very small producers with limited resources. As a consequence, the pool of participants resulted in beneficiaries who tended to be slightly richer, younger and more prone to use technical assistance than non-participants.

The program offered some services that were common to several agricultural products, as well as some product-specific services. The main financed activity for fruit orchards and grapes producers was a switch in production to certified varieties to meet international quality standards, which received 40 per cent of the program's resources. Technical assistance for incorporating new production techniques was the second activity in order of importance according to resource allocation. These included, for example, densification of plantation, and specific practices aimed at minimizing the negative effects of the root asphyxia suffered by peach trees in some years. Improving commercial development of products and increasing access to international markets was also an objective of the services.

The approach of the program was intended to be largely demand-driven, offering a series of services and allowing farmers to choose among them. The idea was that by forcing producers to pay part of the cost of the extension services, they would have had influence over the quality of the delivery. In practice, this did not happen, because producers had to pay only a limited amount of the costs.

IV. Data and basic statistics

As mentioned before, the program was not originally conceived for being subject to an impact evaluation. This not only implies that the program's benefits were not randomly allocated, assuming that randomization were feasible, but also that the program's monitoring mechanisms and information systems were not designed to produce data for an impact evaluation. In cases like this, where evaluators were not involved in the program design, the only available option is to use secondary sources of information that could serve the purposes of an impact evaluation. Obviously, as we will discuss in the following sections, this approach implies dealing with quite challenging data and sample issues.

For the purpose of this study, we gained access to a unique panel dataset, which we constructed using three existing sources of information (see table 1). First, we used the 2000 National Agricultural Census (NAC) as baseline data, because it includes data for 1999/2000 on a rich set of demographic and farm characteristic variables. In addition, we matched the census with the subsequent fruit surveys carried out by the Uruguayan Department of Agricultural Statistics (DIEA) between 2002 and 2006. The result is a panel dataset of 385 producers for the period 2000-2006.[9] The program's administrative records allowed us to identify 125 farmers who participated in PREDEG, the years of participation and the particular orchards in which they received treatment[10]. Finally, we excluded from the dataset 53 producers who participated in PREDEG before 1999 in order to use the NAC data as a baseline.

[Table 1]

The final panel includes 332 producers, 72 of which participated in PREDEG only between 1999 and 2005. The total sample represented 19.4 per cent of the Uruguayan orchard producers in 1999, while the treated sub-sample represented 18 per cent of the orchard producers who participated in the PREDEG between 1999 and 2005. When we divide farmers by orchards, sample sizes became relatively small, especially for treated producers. As a result, we have enough treated observations in apples and peaches, but we lack enough treated producers in other orchards, such as pears, plums and nectarines. Table 2 displays the sample sizes in our final database.

[Table 2]

It is worth noticing that apples are more oriented to the international market than peaches. While 4,200 tons of apples (7% of total production) was exported by Uruguay in the year 2000, only 88 tons of peaches was exported (0.36% of total production).[11] Furthermore, apple exports grew 117 per cent to 9,100 tons in 2007, while peaches exports only grew 37 per cent to 121 tons.

Due to the combination of different datasets, we cannot observe all relevant variables for all years in our sample. Information available for all years of the panel includes the number of hectares cultivated, the number of plants in production, yields, sub-varieties of orchards and percentage of irrigated land. Finally, we have information only at baseline on a rich set of personal characteristics of the producers and technical characteristics of the farm.

Table 3 shows significant differences between treated and non-treated farmers. At baseline, PREDEG beneficiaries were on average younger, more educated and more likely to have a corporate legal form. Treated farms were more concentrated in medium size segments; a higher fraction of them was endowed with specific equipment, such as vaporizers and cooling chambers, and used advanced technological and managerial practices, such as technical assistance services, irrigation systems and registry of economic activities. PREDEG farmers were also more specialised on the production of fruit, while the non-beneficiaries were more likely to be farming as well other crops or raising livestock.

[Table 3]

V. Identification strategy

The objective of this paper is to establish whether the program induced the adoption of new technologies of production, and whether these improvements led to increases in yields for beneficiaries, disentangling the program effect from potential differences in the characteristics of beneficiaries and non-beneficiaries.

As Birkhaeuser et al. (1991) and Evenson (2001) among others have noted, the problem of endogeneity is pervasive in the estimation of the impact of extension services. Following Romani (2003), we can identify three main problems. First, a simultaneity bias can arise when the farmers are the ones who initiate the contact with extension agents on the basis of their own personal characteristics and needs. For example, farmers can demand extension services when they are in particularly unfavourable conditions. Second, a selection bias may occur if more educated or experienced farmers are the ones demanding the service or if extension agents are more prone to contact this kind of farmers. Finally, there could also be an endogenous placement bias if the programs are targeted to regions where there is more responsiveness to extension services.

Although in a regression context unobserved heterogeneity may be a serious source of bias, if panel data are available we can use over-time and between-farms variation to deal with the problem of selection on unobservables and identify the impact of the program. Assuming that there are no time-varying unobservable variables correlated both with treatment and outcomes, we estimate an equation of the form:

$$Y_{it} = \alpha_i + \lambda_t + \beta D_{it} + \varepsilon_{it} \qquad (1)$$

where Y_{it} is the outcome of producer *i* in year *t*, α_i are producer-level fixed effects, λ_t are time-specific effects that are common to all producers, D_{it} is the treatment indicator and ε_{it} is a zero-mean random error term assumed to be independent of D_{it}. Under the assumption of time-invariant unobserved heterogeneity[12], β can be consistently identified using a fixed effects estimator that exploits within group time variation, that is, changes in farms participation over time[13].

More precisely, we use three different specifications. The first uses a binary treatment as shown in equation (1). In this case $D_{it} = I[t \geq \tau_i]$ where τ_i is the year in which the producer *i* enters the treatment and *I[.]* is an indicator function. Thus D_{it} is equal to one from the first year of participation until the end of the observation period.

To explore the possibility of a time-varying pattern in the treatment effect, we decompose D_{it} into separate dummy variables for each year of treatment (which is equivalent to interacting D_{it} with the time dummies). The equation in this case is:

$$Y_{it} = \alpha_i + \lambda_t + \sum_{t=1}^{T} \beta_t D_{it} \lambda_t + \varepsilon_{it} \qquad (2)$$

where λ_t is constructed as a set of dummy variables for the years 2001 (baseline omitted) to 2005.

Finally, since producers may enter the program more than once, we also analyse the effect of the treatment intensity, that is the number of times the producer is treated. This is estimated using the following specification:

$$Y_{it} = \alpha_i + \lambda_t + \sum_{k=1}^{K} \beta_k D_{kit} + \varepsilon_{it} \qquad (3)$$

where $D_{kit} = I[t \geq \tau_{ki}]$ with $k = 1, \dots, 5$ and where τ_{ki} indicates the year in which the producer received the *k*-th treatment. We consider five categories: D_{1it} is equal to 1 since the year in which the producer received the first treatment, D_{2it} is equal to 1 since the year in which she received the second treatment, and so on.

Finally, to check the robustness of the estimates, we also restrict the sample to a common support by estimating the propensity score at the baseline.

VI. Results

Due to small sample sizes for producers of certain orchards, we focus our analysis on apples and peaches and we analyse the impact of the program on three key expected outcomes: adoption of improved varieties (measured as the share of production of certified varieties on total production), density of plantation and yields (measured as output per hectare).

It is worth noticing that the three outcomes of interest may require different timing to materialise.[14] As it is often the case for technology adoption programs, the program's effects on direct measures of technology adoption – in our case adoption of improved varieties and density of plantation – can usually be observed in the short-medium run, while it takes more time for the effects on productivity to manifest.

Also, these effects may display rather different patterns over time, and not always monotonic patterns. Productivity measures, in particular, tend to not only show limited increases in the short-medium run, but even initial drops due to learning process related to the introduction of the new practices and techniques. These drops, if the practice and techniques are actually beneficial, are often overshot by increases in the long run (Maffioli et al. 2011).

VI.1. Adoption of improved plant varieties

Although almost all the coefficients shown in table 4 are not significant, they have the expected signs. The average effect of the program on the adoption of new varieties is around 6-7 percentage points for apples and 8 percentage points for peaches (which are very similar, considering the variance of the estimates).

[Table 4]

The second set of coefficients reported in table 4 (impact by year of treatment) reveals an interesting time pattern in the average effect of the program. Each coefficient represents the difference in means compared to a base category which in this case is the control group; thus, β_1 is the difference in adoption between treated and untreated individuals after one year of treatment; β_2 is the difference between treated and untreated after two years of treatment and so forth. The results show a generally increasing pattern of the impact for both fruits, although with a very high variance.

These results are consistent with the fact that newly planted trees usually take between three and four years to become fully productive. Because our measure of improved variety adoption is based on a share of production, this effect is expected to be low in the very short-run and to increase over time.

Although these findings are somehow consistent with the program's expected results, the small sample impede us to draw decisive conclusions on the impact of the program on adoption of improved varieties. Because of the small number of producers, the standard errors of the estimates are very large. We will further elaborate on this issue in the section VI.5.

VI.2. Density of plantation

An additional objective of the program, which was associated with conversion to improved variety, was to increase the density of plantation. Table 5 presents the results for this outcome. The first row shows some evidence of a positive and significant average effect for apples, and a positive but insignificant effect for peaches.

[Table 5]

The impact-by-years-treated estimates show that the impact of the program on density for both fruits appears approximately after two years. The effects over time seem to display an inverted U-shape pattern, although the standard deviations of the coefficients are high and the differences between them are only significant at 10 per cent level.

As previously mentioned, an increased density of plantation is a key outcome for the program. First, increased density was considered functional to the adoption of improved varieties. Second, higher density was also expected to lead to higher yields. However, an increase in the density of plantation does not guarantee an increase in productivity. In fact, above a certain threshold, productivity decreases if more trees are planted in the same plot of land, as trees compete for existing light and nutrients[15]. This is especially critical when the increase in density is not accompanied by complementary reforms, such as adequate conduction methods and treatment of the land (including the use of fertilizers). The threshold that corresponds to the optimal density of plantation depends on the characteristics of the soil, the availability of inputs and the variety of fruit.

VI.3. Yields

Ideally, we should explore whether the increase in intermediate outcomes such as adoption of improved varieties and density of plantation derives in higher productivity. Unfortunately, the lack of data on input prevents us from performing such analysis. However, we can measure the impact on yields, which is a measure of partial productivity with respect to land. The findings are presented in table 6.

[Table 6]

As in the case of adoption of improved variety, almost all the coefficients are not significant. However, in this case even the signs are not always consistent with the expected effect and differ between the two analysed orchards.

The interpretation of these results is not straightforward. The more plausible explanation, given the evidence of impact at least in terms of density of plantation, is that the period of time we observe is too short to detect a significant impact on yields.[16] The difference in signs between orchards, though its interpretation requires much more caution, can be due to the specific conditions to which the two orchards have been exposed before treatment, in particular the root asphyxia that affected the peach production up to 2001.

Although the lack of impact on yields is a rather discouraging indication of what may be the long term effects and sustainability of the program, we cannot completely rule out that the program had some effects on other measures of productivity. If the program affected the use of inputs other than land, it could have had a positive effect on technical efficiency and on total productivity that is not captured by yields. Unfortunately, we cannot test this hypothesis because of the lack of data on inputs.

VI.4. Robustness check: regressions on the common support

To check the robustness of our results, we restrict our estimations to a common support to eliminate those treated and untreated farmers in the tails of the distribution. We estimate a participation model using a probit regression:

$$P(D = 1|X) = F(X\beta)$$

where X is a set of observable characteristics measured at the baseline (using census data for the year 2000), including characteristics of the producers and the farms, and F is the Normal cumulative distribution function. We define the common support as the area of overlap between the predicted probabilities of participation for treated and untreated producers. This ensures that the regressions include producers that are similar in terms of observed variables at the baseline. The covariates contained in the vector X and the results of the participation model estimation can be seen in table 7.

[Table 7]

Tables 8 to 10 show the results of the previous fixed effects models, but restricting the sample to the common support. The findings of the previous subsections remain unchanged under this specification.

[Tables 8-10]

VI.5. Discussion: sample size and power issues

A very important factor to consider when analysing the results in the previous sections is that the estimations are based on a very small sample size, not only with respect to the total number of producers in the regressions but also (and especially) regarding the proportion of treated producers. As a consequence, the statistical power of our analysis may be considerably low, and thus the probability of not rejecting the null hypothesis of zero impact when in fact it is not true may be very high. This may be a minor concern for the case of density, for which we find significant impacts, and for yields, where the coefficients may be considered close enough to zero to argue that there is effectively no impact on this outcome. However, the lack of power seems to be a more serious problem in the case of adoption of new varieties, where the coefficients, although insignificant, show the expected signs and patterns. Thus, it can be argued that the lack of significance of these estimations may not be due to lack of impact but to low statistical power.

Unfortunately, the lack of power is a very difficult problem to solve, especially when, as in our case, it is impossible to get more data. However, a feasible exercise that can shed some light on this issue is to use the data on the group of producers who entered the program in 2001, a group that was originally excluded because the year 2001 is our baseline for the fixed effects model. To include this group in our analysis without losing the baseline for the regressions, we treat these producers as if they were not treated in the first year. In practice, this implies setting their treatment indicator to zero in the first year and one afterwards.

The expected effect of this exercise is to reduce the standard errors of the estimations. As to the magnitude of the coefficients, the inclusion of this group of producers could bias the impacts downwards since, assuming a positive impact, we would be taking the effect of the program on the first year for one group of producers as if it were their baseline value. However, as long as this bias is small enough relative to the decrease in standard errors, this exercise can give some insights to assess whether the lack of impact found on the previous sections is due to low statistical power or a true absence of effect.

The results are presented in table 11. We focus on adoption of new varieties[17]. While we find no further evidence of impact for peaches, the coefficients for apples change as expected. The magnitudes of the estimates are remarkably similar, although slightly lower. The standard errors show significant decreases ranging from 10 per cent to 28 per cent. As a result, the average impact of the program becomes significant at the 10 per cent level, while the impact over time starts being significant in the second year of treatment.

[Table 11]

These findings suggest that the lack of impact in the previous set of estimations is most likely due to low statistical power; when attempting to address the small sample problem, we find some evidence of positive impact of the program on adoption of new varieties.

VII. Conclusions

Although the results vary across crops, our findings point to a positive effect of the PREDEG program on the adoption of the promoted practices and technologies. In particular, we find consistent evidence that the program increased the density of plantation. In addition, once the issue of low statistical power is addressed, we also find some, certainly weaker, evidence of positive effects on the adoption of improved varieties.

While the positive impact on technology adoption supports the hypothesis of effectiveness of this particular form of intervention, the findings on yields are more discouraging. Although the lack of effects on yields could be due to the limited timeframe of the evaluation and does not completely rule out effects on other measures of productivity, it may also indicate that the practices promoted by the program were not sufficient to induce a detectable effect on productivity. Unfortunately our data do not allow us to fully address this quite relevant issue, which could be the focus of future research on this or similar programs.

Keeping in mind its assumptions and data limitations, this paper contributes to the existing literature by providing evidence that public intervention in agricultural extension services can foster technology adoption. In particular, it indicates that the public co-financing of services combined with private provision might be an effective tool to target farmers who lack incentives to implement reforms in spite of being endowed with a certain level of resources. This method, however, has limited reach for low income producers, since the co-financing requirement and credit restrictions limit their ability to participate. Therefore, other types of institutional settings might be advisable to complement such schemes.

In addition, this paper also points to the crucial need of properly considering the timing of the program effects in designing and implementing an impact evaluation. Failing to account for these issues may lead to inconclusive findings and misleading policy recommendations. In the particular case of the PREDEG, evaluators were involved neither in the design of the program nor in the definition of its monitoring mechanisms and information systems. Although ex-post exercises based on secondary source of information – such the one reported in this paper – can generate valuable knowledge on the effectiveness of public policies, complete assessments that cover the full range of a program's expected intermediate and final outcomes can be properly designed and planned only with the involvement of evaluators since the very beginning of the program design.

Finally, while this paper contributes to the literature on the effectiveness of publicly financed extension services, there is a large range of topics for future research on this area. Future studies should focus on whether the effectiveness of these programs could be maximised by a better targeting of the services. Both the content and scope of the services could be better adjusted to the particular needs of the farmers, if targeting is stricter. Other topics, such as the method of provision (groups vs. individual provision, for example), or the capacity of these programs to improve managerial techniques or market access also need to be explored more carefully in the future.

Tables

Table 1 - Data sources

Source	Period: Collection	Period: Harvest	Unit	Number of observations
PEU (Administrative)	1997-2005	1997-2005	Farmer	452
National Agricultural Census (NAC)	2000	1999/2000	Farmer	1734*
Fruit Surveys (FS)**	2002	2001/2002	Farmer	406
	2003	2002/2003	Farmer	408
	2004	2003/2004	Farmer	410
	2005	2004/2005	Farmer	412
	2006	2005/2006	Farmer	392

* Only fruit producing farmers. The NAC included a total of 57,131 farmers.

** The FS prior to 2002, including 2001, were collected on a sample designed on the basis of the National Agricultural Census collected in 1990. For this reason, we could not match it with the other FS

Table 2- Number of producers receiving treatment by fruit, 2001-2005

Cumulative Number of Treated Farmers

	Year				
	2001	**2002**	**2003**	**2004**	**2005**
Apples	0	5	8	18	23
Peaches	0	17	22	23	25

Number of farmers by times treated

	Times treated				
	1	**2**	**3**	**4**	**Total**
Apples	15	6	1	1	23
Peaches	7	6	6	6	25

Note: every farmer could produce more than one fruit, and could be treated more than once, either for the same fruit or for different fruits.

Table 3 - Characteristics of the Farms and Farmers (baseline data, year 2000)

Variables	Characteristics of the producers						
	Treated			Control			Difference
	Obs	Mean	S.D.	Obs	Mean	S.D.	
Age	71	47.14	11.52	257	50.66	12.99	-3.52**
Gender	71	0.97	0.17	257	0.91	0.28	0.06*
Education	71	11.42	3.74	258	10.46	3.73	0.96**
Foreign	72	0.06	0.23	260	0.06	0.24	-0.01
Individual	71	0.80	0.40	257	0.89	0.32	-0.08*
Company	71	0.15	0.36	257	0.06	0.24	0.09**

Variables	Characteristics of the farms						
	Treated			Control			Difference
Size (number of plants)	**Obs**	**Mean**	**S.D.**	**Obs**	**Mean**	**S.D.**	
Micro	72	0.08	0.28	260	0.29	0.45	-0.21***
Small	72	0.38	0.49	260	0.22	0.41	0.16***
Small-Medium	72	0.24	0.43	260	0.12	0.32	0.12***
Medium	72	0.15	0.36	260	0.04	0.20	0.11***
Large	72	0.06	0.23	260	0.03	0.17	0.02
Total land	71	38.44	51.98	258	57.80	204.69	-19.37
Employment							
Total Employment	71	5.69	5.58	258	5.36	15.08	0.33
Temporary employment	71	240.03	410.19	258	373.78	3026.21	-133.75
Skilled labor	71	0.34	0.61	258	0.97	5.89	-0.63
Skilled labor (%)	71	0.06	0.13	257	0.06	0.16	0.00
Residents	71	0.76	4.51	258	0.76	7.61	0.00
Machine and equipment							
Tractors	72	2.53	1.52	260	2.19	4.08	0.34
New tractors	72	0.13	0.33	260	0.12	0.44	0.00
FWD Tractors	72	0.29	0.59	260	0.33	2.06	-0.04
Other machinery	72	7.89	4.78	260	5.93	4.40	1.96***
Cold chamber	72	0.39	0.49	260	0.16	0.37	0.23***
Wire fence	71	0.14	0.35	258	0.18	0.38	-0.04
Technology and Management							
Administrator	71	0.08	0.28	258	0.10	0.30	-0.02
Technical assistance	71	0.86	0.35	258	0.53	0.50	0.33***
Registers	71	0.77	0.42	258	0.47	0.50	0.31***
Undercover Sowing	71	0.04	0.20	258	0.00	0.06	0.04***
Irrigation systems	71	0.79	0.41	258	0.53	0.50	0.26***
Health tretment	71	0.01	0.12	258	0.04	0.19	-0.02
Other uses of land							
Vineyard	71	1.00	0.00	258	1.00	0.00	0.00
Market garden	71	0.27	0.45	258	0.42	0.49	-0.15**
Cereals	71	0.06	0.23	258	0.07	0.26	-0.02
Meadow	71	0.07	0.26	258	0.09	0.29	-0.02
Wood	71	0.14	0.35	258	0.22	0.42	-0.08*
Pasture	71	0.01	0.12	258	0.08	0.27	-0.06**
Livestock							
Cows	71	0.13	0.34	258	0.34	0.48	-0.22***
Sheeps	71	0.00	0.00	258	0.04	0.19	-0.04*
Porks	71	0.20	0.65	258	1.33	7.75	-1.14
Access to road							
Motorized access	71	0.31	0.47	258	0.36	0.48	-0.05
Non motorized access	71	0.07	0.26	258	0.05	0.23	0.02
Improved access	71	0.62	0.49	258	0.58	0.49	0.04
Permanent access	71	0.96	0.20	258	0.98	0.15	-0.02
Other infrastructure							
Phone	71	0.89	0.32	258	0.85	0.36	0.04
Electricity	72	0.97	0.17	260	0.96	0.20	0.01

We adopted the size classification based on number of plants used by the official Uruguayan statistics. See DIEA 2003.

* significant at 10%, ** significant at 5%, *** significant at 1%

Table 4 – Impact on adoption of improved varieties

Adoption of improved varieties	Apples		Peaches	
Predeg	6.6	-	8.5	-
	(4.4)		(5.4)	
First year	-	4.9	-	5.5
		(4.7)		(4.4)
Second year	-	7.1	-	8.5
		(5.0)		(5.7)
Third year	-	14.0	-	9.5
		(7.1)**		(7.2)
Fourth year	-	8.3	-	12.1
		(7.9)		(7.3)*
Fixed effects	*Yes*	*Yes*	*Yes*	*Yes*
Time dummies	*Yes*	*Yes*	*Yes*	*Yes*
Observations	617	617	703	838
Number of producers	147	147	191	191

Robust standard errors clustered at producer level in parentheses

* significant at 10%, ** significant at 5%, *** significant at 1%

Table 5 – Impact on density

Density of plantation	Apples		Peaches	
Predeg	108.5	-	55.1	-
	(42.5)**		(34.8)	
First year	-	62.9	-	8.3
		(38.5)		(27.0)
Second year	-	159.4	-	53.0
		(56.3)***		(39.2)
Third year	-	160.9	-	76.1
		(51.2)***		(41.8)*
Fourth year	-	119.2	-	108.8
		(40.6)***		(51.2)**
Fixed effects	*Yes*	*Yes*	*Yes*	*Yes*
Time dummies	*Yes*	*Yes*	*Yes*	*Yes*
Observations	617	617	703	703
Number of producers	147	147	191	191

Robust standard errors clustered at producer level in parentheses

* significant at 10%, ** significant at 5%, *** significant at 1%

Table 6 – Impact on yields

Yield	**Apples**		**Peaches**	
Predeg	-1.3	-	1.1	-
	(1.4)		(0.9)	
First year	-	0.1	-	0.4
		(1.8)		(1.2)
Second year	-	-2.9	-	0.8
		(1.6)*		(1.0)
Third year	-	-2.6	-	2.3
		(1.8)		(1.1)**
Fourth year	-	-2.7	-	1.2
		(2.4)		(2.0)
Fixed effects	*Yes*	*Yes*	*Yes*	*Yes*
Time dummies	*Yes*	*Yes*	*Yes*	*Yes*
Observations	651	651	752	752
Number of producers	153	153	197	197

Robust standard errors clustered at producer level in parentheses

* significant at 10%, ** significant at 5%, *** significant at 1%

Table 7- Participation model

Variables	Coef.	Std. Err
Characteristics of the producers		
Age	-0.013	0.01
Gender	0.441	0.45
Education	0.051	0.03*
Foreign	-0.063	0.47
Individual	0.319	0.49
Company	0.471	0.56
Characteristics of the farms		
Size (Plants)		
Micro	-0.426	0.36
Small	0.886	0.31***
Small-Medium	0.830	0.34**
Medium	1.504	0.43***
Large	0.819	0.6
Location	0.230	0.34
Additional farm	-0.178	0.5
Employment		
Total Employment	0.053	0.03*
Skilled labour	-0.292	0.14**
Residents	0.004	0.05
Machine and equipment		
New tractors	-0.232	0.28
Cold chamber	0.365	0.24
Wire fence	-0.051	0.28
Technology and Management		
Administrator	-0.264	0.39
Technical assistance	0.662	0.26***
Registers	0.437	0.23*
Irrigation systems	0.570	0.23**
Health treatment	-0.204	0.62
Access to road and utilities		
Permanent access	-0.333	0.53
Phone	-0.492	0.34
Electricity	0.622	0.82
Constant	-3.252	1.33**
Observations	325	
Pseudo R2	0.3315	

* significant at 10%, ** significant at 5%, *** significant at 1%

Table 8 – Impact on adoption of improved varieties – common support

Adoption of improved varieties	Apples		Peaches	
Predeg	6.0	-	9.3	-
	(4.6)		(5.5)*	
First year	-	4.6	-	6.7
		(4.7)		(4.6)
Second year	-	6.4	-	9.1
		(5.3)		(5.8)
Third year	-	13.3	-	10.0
		(7.1)*		(7.2)
Fourth year	-	6.9	-	13.3
		(8.2)		(7.5)*
Fixed effects	*Yes*	*Yes*	*Yes*	*Yes*
Time dummies	*Yes*	*Yes*	*Yes*	*Yes*
Common support	*Yes*	*Yes*	*Yes*	*Yes*
Observations	481	481	515	515
Number of producers	110	110	137	137

Robust standard errors clustered at producer level in parentheses

* significant at 10%, ** significant at 5%, *** significant at 1%

Table 9 – Impact on density – common support

Density of plantation	**Apples**		**Peaches**	
Predeg	105.9	-	50.3	-
	(42.7)**		(34.8)	
First year	-	62.3	-	5.8
		(38.6)		(27.3)
Second year	-	156.7	-	48.0
		(56.8)***		(39.5)
Third year	-	157.9	-	68.6
		(50.8)***		(42.2)
Fourth year	-	112.1	-	109.5
		(43.2)**		(56.2)*
Fixed effects	*Yes*	*Yes*	*Yes*	*Yes*
Time dummies	*Yes*	*Yes*	*Yes*	*Yes*
Common support	*Yes*	*Yes*	*Yes*	*Yes*
Observations	481	481	515	515
Number of producers	110	110	137	137

Robust standard errors clustered at producer level in parentheses

* significant at 10%, ** significant at 5%, *** significant at 1%

Table 10 – Impact on yields – common support

Yield	**Apples**		**Peaches**	
Predeg	-1.6	-	1.4	-
	(1.4)		(0.9)	
First year	-	-0.2	-	0.4
		(1.8)		(1.2)
Second year	-	-3.2	-	1.1
		(1.7)*		(1.0)
Third year	-	-3.0	-	2.7
		(1.9)		(1.1)**
Fourth year	-	-3.3	-	1.6
		(2.5)		(2.1)
Fixed effects	*Yes*	*Yes*	*Yes*	*Yes*
Time dummies	*Yes*	*Yes*	*Yes*	*Yes*
Common support	*Yes*	*Yes*	*Yes*	*Yes*
Observations	505	505	544	544
Number of producers	114	114	141	141

Robust standard errors clustered at producer level in parentheses

* significant at 10%, ** significant at 5%, *** significant at 1%

Table 11 – Impact on adoption of improved varieties – extended sample

Adoption of improved varieties	**Apples**		**Peaches**	
Predeg	6.3	-	3.7	-
	(3.2)*		(3.9)	
First year	-	5.1	-	1.9
		(3.4)		(3.4)
Second year	-	6.7	-	4.1
		(3.8)*		(4.0)
Third year	-	10.7	-	4.5
		(5.2)**		(4.9)
Fourth year	-	5.8	-	5.3
		(7.1)		(5.3)
Fixed effects	*Yes*	*Yes*	*Yes*	*Yes*
Time dummies	*Yes*	*Yes*	*Yes*	*Yes*
Observations	681	681	764	764
Number of producers	160	160	205	205

Robust standard errors clustered at producer level in parentheses

* significant at 10%, ** significant at 5%, *** significant at 1%

ENDNOTES

[1] See, for example, Hanson and Just (2001) and Feder et al. (2003).
[2] See World Bank (2006) on how inequality creates poverty through limitations in market access.
[3] In practice, willingness to pay for extension services has been low to emerge in many settings (Anderson and Feder, 2007).
[4] See Rivera and Alex (2005) for a review of case studies on the reform of extension services and Hanson and Just (2001) for categories of extension services (ranging from traditional public extension services and partially public-funded extension to private extension).
[5] An additional limitation of this methodology pointed by Dinar et al. (2007) is the assumption that farms operate at technically efficient levels, which contradicts the idea of inefficiencies in production that justify the public provision of extension services. Although stochastic frontier models allow relaxing the efficiency assumption, they suffer from the same endogeneity problems that the production function approach does.
[6] For a complete discussion on different categories of extension services see Hanson and Just (2001).
[7] See CINVE (2005).
[8] There were some specific restrictions on farmers' land size and the total amount of the subsidy.
[9] The panel did not include the year 2001. Henceforth, we defined the years included in the panel on the basis of the second year of the harvest period.
[10] We validated this last step on the basis of a question on participation in PREDEG included in the 2005 Fruit Survey.
[11] Source: MGAP-DGSA-DIEA, Boletín de Importación-Exportación de productos Hortifrutícolas.
[12] Note that this specification is robust to fixed effects at any level, for example region / department fixed effects. As long as this heterogeneity is constant over time, it is captured by the fixed effects. This would be the case of any preprogram differences between control and treated groups that could be assumed to remain constant over time, including initial endowed of resources.
[13] It is important to note that, on one hand, fixed-effects cannot control for time varying unobservable characteristics that could drive program's participation. On the other hand, it does not reduce the bias induced by the potential correlation between program's modifications and the outcome of interest when policy interventions respond to changes in the outcome variable.
[14] For a complete discussion on the timing of the effects in the evaluation of technology promotion program see Crespi et al. 2011.
[15] See for example Vig and Kallay (1983) or Sansavini, S. and Corelli-Grappadelli (1997).
[16] It is worth noticing that both the measure of adoption of improved varieties and yields are based on fruit production, which may require some time to be fully affected by the technological changes promoted by the program, while the density of plantation is a measure based on number of trees in production, which could be modified in the short.-run.
[17] The results for the other two outcomes are consistent with the previous estimations. We do not present these results to save space, but the tables are available upon request.

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