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Office of Evaluation and Oversight

Inter-American Development Bank
Office of Evaluation and Oversight
Working Paper: OVE/WP-04/09
August, 2009

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* This paper is part of the project 'Ex-post Evaluation of the IDB Agricultural Technology Uptake Programs', by the Office of Evaluation and Oversight (OVE) of the Inter-American Development Bank (IDB). The authors are grateful to the Department of Agricultural Statistics (DIEA) of the Ministry of Agriculture (MGAP) of Uruguay and to Carlos Sammarco for providing the data used for this evaluation. The authors also owe special thanks to Camilo Saavedra, Carlos Paolino, Luis Macagno, Rodolfo Stucchi and Paul Winters for useful discussions. The findings and interpretations of the authors do not necessarily represent the views of the Inter-American Development Bank. The usual disclaimer applies. Correspondence to: Alessandro Maffioli, e-mail: alessandrom@iadb.org, Office of Strategic Planning and Development Effectiveness (SPD), Inter-American Development Bank, Stop E0805, 1300 New York Avenue, NW, Washington, D.C. 20577.

Abstract

This paper analyzes the impact of the ‘Farm Modernization and Development Program’ (PREDEG) on the technology uptake and productivity of Uruguayan farmers. Using a unique panel dataset, we combine propensity score matching techniques and fixed effects models to estimate the program’s impact. Although the results vary according to the crops, we find consistent evidence that the program increased the rate of adoption of certified varieties and the density of plantation. However, there is only limited evidence of its effects on productivity, mostly derived from helping producers to cope with an illness of peach orchards. Conversely, we find some indications of negative lagged productivity effects for apples, which might be related to a short term cost of transitioning to new varieties or technologies. While the evidence of the effects on yields is not definitive due to the limited timeframe of the evaluation, the results indicate that PREDEG services were useful for incentivizing the adoption of specific technologies, as well as for crop-specific technical assistance like plant health.

Keywords: PREDEG, Uruguay, Agricultural Technology Transfer, Panel Data, Policy Evaluation.

JEL Classification: Q12, Q16, H43

INTRODUCTION

The 'Farm Modernization and Development Program' (PREDEG for its name in Spanish) in Uruguay came about as a means to respond to increased competition in the agricultural sector at the end of the 1990 by increasing the productivity of the sector. When the Government and the IDB designed the PREDEG, the Uruguayan farm production was suffering major competitiveness limitations both at Latin American and at international level. These limitations were particularly concerning given the expectations of deeper trade integration, in particular with MERCOSUR. The expected implications of this integration were two-fold: first, Uruguayan farmers had to meet the quality standards required by the international markets in order to take advantage of the new commercial opportunities and, second, the Uruguayan farmers had to improve their efficiency in order to hold out the competition of the Argentinean farmers in the internal market. This required improving productivity and the quality of the Uruguayan products, in order to compete in international markets.

Although increased integration would ultimately be beneficial for a majority of farmers, leaving these necessary adjustments in technology and production methods to the individual decisions of farmers was thought to be ineffective. Improving productivity and quality requires a functioning system of technology generation and transfer and a means to implement these technologies. Extension services can provide the proper institutional system to deliver these trainings to farmers. However, the prevalence of private markets for extension services is restricted by the characteristics of the service itself and by the capacity and willingness of farmers to pay for these services. The benefits from implementing easy-to-copy changes in production are not easily appropriable, so individual farmers do not face the full incentives for payment of these services. As a consequence, some form of public intervention is generally justified, be it in the form of pure public provision, full public financing or cost-sharing mechanisms between farmers and the public sector. The latter was the system chosen by Uruguay to cope with the need for improvements in productivity and quality, in the form of PREDEG.

With the objective of contributing to the general literature on technology adoption in agriculture, this paper analyzes the effectiveness of the PREDEG, assessing whether the productivity of farmers and the quality of their products increased as a result of the services provided by the program. [1] The program was

[1] Although the PREDEG targeted farmers in different sectors, the paper considered only orchards producers because of data availability. According to a report of the National Institute for

not randomized, so we rely on panel data methods and propensity score matching to minimize existing biases arising from the method for selecting 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 for the beneficiary producers.

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

The paper is organized into five sections. Following this introduction, section II discusses the theory behind public intervention in extension services, and the existing evidence on the effectiveness of these types of programs. Section III focuses on the program, its objectives, the constraints the farming sector was experiencing at the time of program design, and how PREDEG sought to address them. Section IV describes the dataset used and some summary statistics. Section V discusses the methodology and hypothesis for the identification of the effects of the program. Section VI discusses the results on the effect of the program on technology uptake and productivity, and section VII summarizes the results, identifies policy recommendations and concludes.

AGRICULTURAL EXTENSION SERVICES, THEORY AND EVIDENCE

The provision of agricultural extension services has been justified in the literature on both equity and efficiency grounds. In the presence of market failures, for example externalities, limited access to credit or non-competitive market structures, producers will not face the correct incentives to produce certain varieties, use new production techniques or adopt new technologies, resulting in production levels that are not socially optimal[2]. In addition, if less advantaged farmers are more exposed to these failures because of their limited resources (lack of market power in an oligopsony, limited access to credit, low capacity to pay for extension services[3]), the justification for solving these market failures

Agricultural Research (INIA, 2006) the PREDEG was originally supposed to aim only at the horticultural and orchards sectors.

[2] See, for example, Hanson and Just (2001) and Feder et al. (2003).

[3] See the World Development Report 2006, for examples on how inequality might create poverty through limitations in market access.

through public intervention gains relevance under both equity and efficiency arguments.

Agricultural extension confronts some of these inefficiencies, through training and provision of technology for beneficiaries. The question is why if producers are inefficient and extension services are effective, a private market for extension services is not rampant in rural areas. The literature identifies two main reasons for the absence of such markets. First, services provided by these programs are non-rival, such that their use by one farmer does not preclude other farmers from using it. In addition, they are non-excludable, given the difficulty to limit the transfer of knowledge or technology. If services are non-rival and non-excludable, the appropriability of the benefits of such programs is low, since a successful technology would be immediately adopted (copied) by competitors, eliminating the profits for the first adopter. Thus, the incentives to demand these services for a positive price are low, limiting the development of these markets[4].

Under these circumstances, public intervention could generate large positive externalities if it directly targets these inefficiencies. However, even if public intervention is advisable, the question still remains of whether the government should provide the services itself or instead set the institutional structure to create a market and finance private providers for the services. In addition, despite the existence of appropriability issues and externalities, the services provided are expected to increase the value of production, and as such these services have a monetary value that could be charged to the beneficiaries. However, the incentives to set or accept market prices are complex, due to asymmetric knowledge of the value of the service between government (principal) and private provider (agent), the heterogeneity of beneficiaries and the uncertainty of agricultural activities, and, as a consequence, the difficulty to design outcome-based payments that create the right set of incentives between the government and the provider. Together with the high cost of monitoring private providers (remoteness & sparseness of services), the availability of information for effective targeting by the public sector, might justify public financing and provision of services in some settings.

Finally, if governments are generally concerned with equity considerations, even in the absence of coordination failures or other market imperfections, public intervention might be appropriate on equity grounds if targeted to low-income farmers. In addition, and since poor farmers tend to be, among other things, less educated, which combined with their lower capacity to pay for extension services would result in large productivity differences based on socioeconomic status, the

[4] See Hanson and Just (2001).

efficiency and equity arguments would both hold for support of low income farmers with the provision of extension services.

Evidence

Notwithstanding the theoretical considerations for public intervention through the financing and/or provision of extension services, the existing evidence of their effectiveness is scarce and inconclusive, partly due to the few rigorous impact evaluations undertaken until now. In addition, these evaluations fail to address questions on the effectiveness of new modalities of extension programs around the world (e.g. privatization, fee-for-service, decentralization, etc.). As Alex and Rivera (2005) point out after reviewing 44 case studies, reforms in extension services include decentralization, privatization, demand-driven approaches, new approaches for public services and national strategies. In addition, Hanson and Just (2001) delimit five categories of extension under which those reforms can be understood, including: i) public extension with public funding and traditional delivery, ii) paid public extension with public provision, with a fee-for-service funding, iii) partially public-funded private extension, delivered by private firms and financed in part by public budgets and in part by user fees, iv) policy-supported private extension, provided by firms and financed by users, but with government subsidies or taxes for specific production techniques, and v) private extension, provided by private firms that charge fees for their services. In this section we review the evolution of the reforms and analyze their logic on the basis of the arguments developed in the previous section. Unfortunately, the evidence on the comparable effectiveness of these five models is non-existent.

The main body of research on the effect of extension services on producers is based on production functions using the extension variable as one of the inputs, showing large positive rates of return to extension services (Birkhaeuser et al., 1991). However, in the absence of random assignment to treatment and control groups, this methodology is likely to provide biased estimates of causal effects, due to endogeneity of program participation and the presence of unobservable characteristics that might determine participation and be correlated with the outcome variable. In addition, as pointed out in Dinar et al. (2007), this approach assumes that farms operate at technically efficient levels, an assumption that is disputed by evidence of inefficiencies in production that serve as a justification for the provision of public extension services. This led to a more sophisticated method, based on stochastic frontier models, which relax the efficiency assumption by defining a "best practice" frontier production function and measuring the distance from this frontier for each producer[5]. However, stochastic frontier models suffer from the same identification problems if treatment is not random and includes unobservable characteristics.

In demand driven programs, as is the case of PREDEG, when the selection of farmers depends at least partly on the desire of farmers on the basis of their own personal characteristics and necessities, the treatment coefficient in production function estimations will include unobservable characteristics that determine both participation and the outcome of interest. The sign and importance of the bias will depend on these unobservable characteristics. For example, if farmers who demand extension services were more motivated, we would expect the production function regression to overestimate the returns to extension services. Other examples of selection bias might come from program implementation, such as when extension agents are more prone to contact more educated productive farmers, or if the programs are devoted to regions where there is more responsiveness to extension services.

The few pseudo-experimental evaluations generally show positive results, though not uniform. Duflo & Kremer (2003)[6] in the only randomized experiment to date show that farmers do not use the optimal amount of fertilizer due to risk aversion (hyperbolic discounters). Godtland et al. (2004) estimate the effect of a farmer field school program and a traditional extension program on farmers' knowledge of integrated pest management practices, using both a regression with controls and matching techniques, and show significant positive effect of both programs, but of higher value for the farmer field school program.

With regards to the farmer field school approach, Feder et al. (2003) use a modified differences-in-differences model and find no impact on yields or on the reduction of pesticide use for Indonesia. Their modified model accounts for the fact that the program was introduced at different times across villages, taking into account that the diffusion of innovation would reduce the impact of extension with the years. On the other hand, Praneetvatakul and Waibel (2006) claim that two time-point observations are not enough to estimate the impact of this kind of programs. They use a panel of four years that comprises eight rice-growing seasons in Thailand and find a positive impact on knowledge and pest management practices both on the short and long run. Nevertheless, in a companion paper they do not find any impact of the program on rice production yield.

[5] See Aigner et al. (1977) for the seminal paper on the literature. Coelli et al. (1998), Kumbhakar and Lovell (2003) can also be consulted for a theoretical discussion of its options. For empirical applications see, among others, Bravo-Ureta and Evenson (1994) and O'Neill et al. (1999).
[6] See also Duflo et al. (2004)

Several works using panel data have been carried out since then, but the number is limited when compared to other areas of development economics. With fixed effect or difference-in-difference models, they control for farms' and farmers' time-invariant unobservable characteristics. Owens et al. (2003) and Romani (2003) estimate the impact of traditional extension services using panels of farmers for Zimbabwe and Ivory Coast respectively[7]. Both studies find a positive impact of extension services on productivity and yields. However, they note that this impact is neither present for all the years nor for all the crops studied.

Another body of literature has used instrumental variables to identify program effects, finding generally positive though small and heterogeneous program effects. However, the instruments and assumptions are at least questionable. As Romani (2003) and Feder et al. (2003) point out, finding a variable correlated with the participation in extension programs but not with the studied outcome is not an easy task since by program design the criteria used to select farmers for extension services are usually correlated with the outcome. They argue that the only option is to rely on strong distributional assumptions such as the joint normal distribution of errors[8]. Akobundu et al. (2004), using as instruments the distance from the extension office, whether an individual was rejected a loan, total farm debt, and the previous visit of an extension agent (not of the program) find a positive impact on farm income only for individuals with a high number of visits. The heterogeneity of results is also significant: farmers with better education, more skills and wealthier are more likely to adopt certain kind of innovations that are dependent on knowledge. Wealth and size of the farm can have similar effects on extension through the adoption rate. This is in accordance to the results of Godtland et al. (2004) who find that the farmer field school approach is effective for wealthier farmers.

In summary, the evidence on the impact of extension services on farmers' productivity and technological adoption is generally positive, though it tends to suffer from biases resulting from endogenous placement and omitted variable bias. The results of pseudo-experimental studies highlight the heterogeneity of program impacts based on farmer's characteristics, such as education, experience and wealth. Alston et al. (2000) review an important number of evaluations, with the majority dedicated to agricultural research, and find a median rate of return of 58 percent. Evenson (2001) describes rates of return between 5 and 50 percent for developing countries, but also notes that impacts vary widely. These results

[7] Owens et al. (2003) also control for time effects, use clustered standard errors at the village level and count with a measure of farmers' skills.
[8] With several periods of data a dynamic panel data estimator could be used, and further differences or the moments of the distribution could act as appropriate instruments.

point to the specificity of impacts depending on the particular design of the program, especially as it relates to the methodology for selecting beneficiaries.

THE PREDEG PROGRAM

During the early years of this decade the Uruguayan agricultural sector started to face stronger international competition. The conditions were not favorable: the productivity of the sector was low and the quality of the products was not up to international standards. In addition there was a 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 expected during the period.

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 percent of the total agricultural sector and 33 percent of the fruit sector (DIEA 2003). The production chain was particularly disarticulated and the adopted varieties were not in line with the international quality standard. The sector was composed by small and scattered producers who were mainly oriented towards the internal market.

In addition, some orchards experienced severe exogenous shocks. Between 1990 and 2001, the peach production was strongly affected by root asphyxia, which reduced the plant stock to one-half. In the same period, the quinces production suffered the contraction of the demand from the national food industry, which was its main commercial destination.

On the basis of the diagnostic, PREDEG was designed to “increase the value of farm products and export through investment designed to improve the sub-sector competitiveness” (IDB 1997). Concretely, the program aimed at boosting productivity, increasing net income for producers and operators, raising the quality and health standards to the standards required by the international trade, identifying commercially viable projects and securing access to international markets. The proposed 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 almost half of the program’s financial resources and included four areas of involvement: a) introduction of improved plant varieties, b) validation and adaptation of new technology, c) training and d) technical assistance.

The program adopted a 'partially public-funded private services' approach,[9] requiring beneficiaries to finance part of the cost of the training and technical assistance (TTA) services. The services would be delivered by private companies and Non-Governmental Organizations (NGO). The program targeted mainly small and medium producers, although it offered services to agribusiness under a specific sub-component. The services offered for these groups, however, were different and during implementation a bigger emphasis was put in small and medium producers (almost 75 percent of beneficiaries).

According to a previous qualitative evaluation carried out by the IDB,[10] the selection of beneficiaries was, for the most part, producer driven, resulting in some heterogeneity in the participating producers. Producers applied for the program, and the restrictions for participation were very few.[11] However, the nature of the program, which provided co-financing and required some financial capacity on the part of the farmer and placed some limits on the size of the farms, restricted participation for both very small producers with limited resources and large producers with excess resources. As a consequence, while some heterogeneity was inevitable, these indirect restrictions narrowed the pool of participants, resulting in beneficiaries who tended to be slightly richer, younger and more prone to use technical assistance.

In terms of the services provided, the program offered some services which were common to all the agricultural products, as well as some product-specific services. The main activity financed by the program for fruit orchards and grapes producers was switching production to certified varieties meeting international quality standards, which received 40 percent 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, 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 a fundamental objective of the services.

[9] For a complete discussion on different categories of extension services see Hanson and Just (2001).
[10] CINVE (2005).
[11] There were some specific restrictions on the total land size and the total amount of the subsidy. In addition, the need for co-payment and up-front investments further drove the type of producer that had access to the program.

The approach of the program was intended to be largely demand-driven, offering a series of services and allowing farmers to choose to participate. The idea was that by forcing producers to pay part of the cost of the extension services, they would have influence over the quality of the delivery. In practice, producers had very limited influence in the quality of the extension services or the specific types of services since, in general, producers had to pay only a small amount of the costs of the technician (in some cases the fees were waived).

The results from the qualitative assessment of the program justifies its success based on the high demand for participation in the program. However, its impact is not as clear. In addition to interviews, the evaluation included a survey of producers who participated in the program, which show high rates of technology adoption. However, by not attempting to control for selection bias, the evaluation fails to establish whether the observed differences come from the characteristics of the beneficiaries or from program participation. The same evaluation argues that the program was based on poor assumptions. The two basic assumptions of the PREDEG design, i.e. macroeconomic stability and intensification of the MERCORSUR agreement, proved to be wrong. On the one hand, between 1999 and 2002 Uruguay went trough one of its worst macroeconomic crisis ever. As a consequence, the collapse of the internal demand and the severe financial constraints significantly reduced the co-financing capacity of the potential beneficiaries. In addition, the fiscal restrictions delayed and reduced the budget allocations to the program and induced the authorities to modify the PREDEG strategy without a careful revision of the intervention model, which limited its impact. On the other hand, the failure of the MERCUSUR negotiations implied less potential competition in the internal market, allowing a slower adjustment than expected.

Regardless of whether the assumptions that justified the intervention were correct, there is no reason why we should not expect positive effects from the program. Farmers received training on the use of new improved technologies for production, and whether they faced increased competition from MERCOSUR is relevant for the evaluation to the extent that it may have affected participation. However, even though the adjustment was slower than expected, competition did increase eventually, so while farmers may have delayed their participation, the question remains whether, upon participation, the program (and more generally the model of intervention) was effective in encouraging technology adoption and stimulating the conversion to new varieties, and whether these improvements led to increases in the productivity. And these are the questions this paper attempts to address.

DATA AND BASIC STATISTICS

To evaluate the PREDEG impact, we gained access to a unique panel dataset, which we constructed using three existing sources of information (see Table 1). First, we use 2000 National Agricultural Census (NAC) as baseline data, since it includes a rich set of demographic and farm characteristics variables. In addition, we matched the census with the subsequent fruit surveys, which were carried out between 2002 and 2006. The result is a panel dataset of 385 producers for the period 2000-2006.[12] The program's administrative records allowed us to identify 125 treated farmers, the years of treatment and the particular orchards in which they received treatment[13]. 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 - Data sources

Source	Period		Unit	Number
	Collection	Harvest		of observations
PEU (Administrative)	1997-2005	1997-2005	Farmer	452
National Agricultural Census (NAC)	2000	1999/2000	Farmer	1,734†
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

Notes: † Only fruit producing farmers. The NAC included a total of 57,131 farmers. ‡ The FS prior to 2002, including the 2001 FS, were collected on sample designed on the basis of the National Agricultural Census collected in 1990. For this reason, we could not match this information with the other FS

The final panel includes 332 producers, 72 of which participated in PREDEG only between 1999 and 2005. The total sample represented the 19.4 percent of the Uruguayan orchard producers in 1999, while the treated sub-sample represented the 18 percent of the orchard producers who participated in the PREDEG between 1999 and 2005.

By specific orchards, the sample sizes in the final dataset are 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 (see Table 2).

[12] The panel did not include the year 2001. Henceforth, we defined the year included in the panel on the basis of the second year of the harvest period.
[13] We validated this last step on the basis of a question on the participation in PREDEG included in the 2005 FS.

Table 2- Number of producers receiving treatment associated with a particular fruit, 2000-2006

	2000	2002	2003	2004	2005
# Farmers Treated					
Peach					
Current year	16	31	25	20	23
Cummulative (since 1999)	27	46	52	53	55
Apple					
Current year	14	18	28	22	22
Cummulative (since 1999)	18	28	33	44	49
Apples and Peaches					
Current year	2	11	11	6	11
Cummulative (since 1999)	9	19	25	30	33
By # cummulative treatments					
Apples					
1	14	19	23	20	13
2	0	9	13	16	21
3	0	2	6	7	5
4	0	0	1	5	10
5	0	0	0	0	1
Peaches					
1	16	21	16	16	19
2	0	26	17	13	8
3	0	4	16	13	14
4	0	0	3	11	9
5	0	0	0	1	8

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.

In order to get an idea of the magnitude of importance of each of these fruits, Table 3 shows the evolution of exports in each of the fruits between 2000 and 2007, where we can see that exports of apples grew exponentially during the period representing the most exported fruit in Uruguay after citrus, while exports of peaches are very low in comparison. In fact, the two fruits chosen for the evaluation represent very different markets: apples could be characterized as mainly an export product, while peaches are mostly for local consumption.

Table 3- Exports by fruit, 2000-2007 (in tons)

Fruits	2000	2001	2002	2003	2004	2005	2006	2007
Apple	4,218	3,916	3,162	6,492	6,256	10,027	7,089	9,109
Pear	2,243	1,499	733	1,073	3,891	4,031	4,057	4,502
Peaches	88	0	41	86	176	45	205	121
Quince (membrillo)	113	58	0	6	54	70	128	67
Prunes	20	15	1	3	13	9	8	18
Grapes	201	95	5	14	24	34	5	14
Other	13	149	59	5	7	8	13	6

Source: MGAP-DGSA-DIEA, Boletín de Importación-Exportación de productos Hortifrutícolas.

Treatment

Taking advantage of the large variation in both the number and the timing of treatments for farmers, we use the number of treatments a farmer has received in previous years to estimate program effects.

Summary Statistics

The combination of datasets also leads to different degree of availability of certain types of information. The information available for all years of the panel includes the number hectares cultivated, number of plants in production, yields, sub-varieties of orchards and percentage of irrigated land. Prices, employment, health practices, market of destination, intention of planting are available in the panel, but only for some years. Finally, we have information at baseline only on a rich set of personal characteristics of the producers and the technical characteristics of the farm. The combination is a dataset that allows us to specify a participation model to identify a proper comparison group, and exploit panel data methods to control for time-invariant individual effects that might drive both participation and the outcomes of interest.

At the baseline, the PREDEG beneficiaries were on average younger, more educated and more likely to adopt a corporate legal form. The treated farms were more concentrated in the medium size segments, were endowed with specific equipment, such as the vaporizers and cold chambers, and adopted more technological and managerial practices, such as contracting technical assistance services, maintaining irrigation systems and registering economic activities. The beneficiaries were also more focused on the production of orchards, while the non-beneficiaries were farming other crops or raising livestock.

Table 4 - Characteristics of the Farms and Farmers (Baseline Data, year 2000)

	Treated					Control						
Variable	Obs	Mean	St. Dev.	Min	Max	Obs	Mean	St. Dev.	Min	Max	Mean diff.	
Characteristics of the producers												
Age	71	47.14	11.52	25	72	257	50.66	12.99	19	82	-3.52	**
Gender	71	0.97	0.17	0	1	257	0.91	0.28	0	1	0.06	*
Education	71	11.42	3.74	7	20	258	10.46	3.73	0	20	0.96	**
Individual	71	0.8	0.4	0	1	257	0.89	0.32	0	1	-0.08	*
Company	71	0.15	0.36	0	1	257	0.06	0.24	0	1	0.09	**
Characteristics of the farms												
Size (Plants)												
Micro	72	0.08	0.28	0	1	260	0.29	0.45	0	1	-0.21	***
Small	72	0.38	0.49	0	1	260	0.22	0.41	0	1	0.16	***
Small-Medium	72	0.24	0.43	0	1	260	0.12	0.32	0	1	0.12	***
Medium	72	0.15	0.36	0	1	260	0.04	0.2	0	1	0.11	***
Large	72	0.06	0.23	0	1	260	0.03	0.17	0	1	0.02	ns
Total land	71	38.44	51.98	3	390	258	57.8	204.69	1	2066	-19.37	ns
Employment												
Total Employment	71	5.69	5.58	1	36	258	5.36	15.08	0	193	0.33	ns
Residents	71	0.76	4.51	0	29	258	0.76	7.61	0	100	0	ns
Skilled labor	71	0.34	0.61	0	2	258	0.97	5.89	0	78	-0.63	ns
Machine and equipment												
New tractors	72	0.13	0.33	0	1	260	0.12	0.44	0	4	0	ns
Other machinery	72	7.89	4.78	0	20	260	5.93	4.4	0	29	1.96	***
Cold chamber	72	0.39	0.49	0	1	260	0.16	0.37	0	1	0.23	***
Wire fence	71	0.14	0.35	0	1	258	0.18	0.38	0	1	-0.04	ns
Technology and Management												
Administrator	71	0.08	0.28	0	1	258	0.1	0.3	0	1	-0.02	ns
Technical assistance	71	0.86	0.35	0	1	258	0.53	0.5	0	1	0.33	***
Registers	71	0.77	0.42	0	1	258	0.47	0.5	0	1	0.31	***
Undercover Sowing	71	0.04	0.2	0	1	258	0	0.06	0	1	0.04	***
Irrigation systems	71	0.79	0.41	0	1	258	0.53	0.5	0	1	0.26	***
Health treatment	71	0.01	0.12	0	1	258	0.04	0.19	0	1	-0.02	ns
Other uses of land												
Market garden	71	0.27	0.45	0	1	258	0.42	0.49	0	1	-0.15	**
Wood	71	0.14	0.35	0	1	258	0.22	0.42	0	1	-0.08	*
Pasture	71	0.01	0.12	0	1	258	0.08	0.27	0	1	-0.06	**
Livestock												
Bovine	71	0.13	0.34	0	1	258	0.34	0.48	0	1	-0.22	***
Sheep	71	0	0	0	0	258	0.04	0.19	0	1	-0.04	*
Access to road and utilities												
Permanent access	71	0.96	0.2	0	1	258	0.98	0.15	0	1	-0.02	ns
Phone	71	0.89	0.32	0	1	258	0.85	0.36	0	1	0.04	ns
Electricity	72	0.97	0.17	0	1	260	0.96	0.2	0	1	0.01	ns

*Notes: * significant at 10% level; ** significant at 5% level, significant at 1% level. [‡] We adopted the size classification used by the official Uruguayan statistics. See DIEA 2003.*

IDENTIFICATION STRATEGY

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

In order to assess whether the program had the intended impact for beneficiaries, we use two objective measures of technology adoption: the adoption of certified varieties and the density of plantation. For the adoption of certified varieties, which was the fundamental activity of the program, we use the percentage of production coming from certified varieties for both apples and peaches as an indicator. For density of plantation we use the number of trees per ha. Finally, we estimate the effects on productivity by measuring the yield as the total production per ha.

Identification Strategy

The fundamental challenge of the paper lies on the identification of a proper counterfactual scenario that allows us to estimate what would have happened to the beneficiaries in the absence of the program. Simple OLS specifications might lead to biased estimates, since beneficiary farmers might differ systematically from non-beneficiaries and, therefore, they might have achieved certain results because of their own characteristics, and note necessarily as a result of the program's support.

The literature documenting these possible biases is extensive. As Birkhaeuser et al. (1991) and Evenson (2001), among others, have noted, the problem of endogeneity is pervasive in the estimation of extension impacts. Following Romani (2003), we can distinguish three main problems. First of all, 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 necessities. For example, farmers can demand extension services when they are in particularly unfavorable conditions. Secondly, there will be a selection bias if the 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 is the programs are devoted to regions where there is more responsiveness to extension services.

This selection-bias problem has been widely analyzed by the literature on policy evaluation and the same analysis can be easily adapted to the PREDEG case. We can define Y_{it}^{T} the productivity of farm *i* in year *t* if the farmer participates in PREDEG and Y_{it}^{C} the productivity of the same farm if it does not participate in the program. We are interested in the difference $Y_{it}^{T} - Y_{it}^{C}$, which is the effect of having participated in the program for farm *i*. Computing $Y_{it}^{T} - Y_{it}^{C}$ requires knowledge of the counterfactual outcome Y_{it}^{C} that is not empirically observable since a farm can not be observed among participants and non-participants at the same time.

However, although we cannot compute the program impact for an individual farm, we can still evaluate an average effect of program participation, $E[Y_{it}^{T} - Y_{it}^{C}]$ comparing data on participating and non-participating farms. Defining D as the dummy variable for program participation, the average treatment effect will be given by:

$$\delta = E[Y_{it}^{T} \mid D = 1] - E[Y_{it}^{C} \mid D = 0] \qquad (1)$$

Subtracting and adding $E[Y_{it}^{C} \mid D = 1]$, we obtain:

$$\delta = E[Y_{it}^{T} \mid D = 1] - E[Y_{it}^{C} \mid D = 0] = E[Y_{it}^{T} \mid D = 1] - E[Y_{it}^{C} \mid D = 1] - E[Y_{it}^{C} \mid D = 0] + E[Y_{it}^{C} \mid D = 1] =$$

$$= E[Y_{it}^{T} - Y_{it}^{C} \mid D = 1] + E[Y_{it}^{C} \mid D = 1] - E[Y_{it}^{C} \mid D = 0] \qquad (2)$$

The term $E[Y_{it}^{T} - Y_{it}^{C} \mid D = 1]$ in (2) is the average effect of the treatment on the treated that we want to isolate. The difference $E[Y_{it}^{C} \mid D = 1] - E[Y_{it}^{C} \mid D = 0]$ is the selection bias: besides the effect of the program there may be systematic differences between participating and non-participating firms that may affect the impact of the program.

A simple estimator of δ using the sample analogue $E[Y_{it}^{T} \mid D = 1] - E[Y_{it}^{C} \mid D = 0]$ will give an unbiased estimate of the program impact only if there is no selection bias, that is only if $E[Y_{it}^{C} \mid D = 1] - E[Y_{it}^{C} \mid D = 0] = 0$. However, as seen in section II, participating and non-participating farms differ in a number of dimensions (e.g. size) that are likely to affect both their productivity and the probability being supported by PREDEG. Therefore, the simple difference in mean outcomes between participants and non-participants is capturing the effect of program participation together with the impact of third factors affecting both the capability of increasing the productivity and the probability of participating in the program.

A first solution to the selection bias problem would be to identify all appropriate covariates to control for omitted variables that could explain different patterns in innovation expenditures between beneficiaries and non-beneficiaries over and above program's participation. The missing counterfactual could then be consistently estimated by the mean innovation expenditures of non-participants.

However, in general, we do not have data on all relevant covariates. In particular, as noted by Blundell and Costa Dias (2002), there are unobservable characteristics that are not measurable and may lead to the failure of the "selection-on-observables" assumption.

In the presence of a panel data set we can impose some restrictions on the data generation process and use over time and between farms variation to deal with the problem of selection on unobservable characteristics and identify the impact of the program. A first restriction is to assume linearity of the conditional expectation function, $E[Y \mid x, D]$ with respect to the set of observable covariates x. A second restriction is to assume that the unobservable characteristics are constant over time.

Using a log linear specification, we can write:

$$\log(y)_{it} = \beta_2 * D_{it} + \beta_3 * \vartheta_t + \gamma_i + \eta_{it} \qquad (6)$$

where y_{it} is the farm productivity and i in year t, D_{it} is a dummy variable for program participation, ϑ_t denotes year dummies controlling for time effects affecting the farm performances; γ_i is a firm-specific component common to all firms and ε_{it} is an i.i.d. zero mean random variable assumed to be independent of x_{it}.

Under the assumption of no-correlation between ε_{it} and x_{it}, β_2 can be consistently identified using a fixed effects estimator that exploits within group time variation, that is changes in farms participation over time. The fixed effects model is a direct extension of the difference-in-difference (diff-in-diff) estimator where there are only two groups and two periods. As in the simple diff-in-diff model, under the assumption of time-invariant unobservable factors, fixed effects relax the assumption of no correlation between unobservable factors and program participation accounting for self-selection into the program on the basis of firm-specific characteristics that affect both the level of innovation effort and program participation.

The correction for individual fixed effects rests on the assumption that self-selection operates through farm characteristics that remain constant over time leaving out two potential sources of variation that could bias the estimated program impact. On one hand, fixed-effects cannot control for time varying unobservable characteristics that could drive program's participation. On the other, fixed effects 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.

RESULTS: EFFECTS OF THE PROGRAM ON TECHNOLOGY ADOPTION AND PRODUCTIVITY

As we saw in the data section, due to the small size of our samples for producers of certain orchards, we focus our analysis on apples and peaches. Using individual farmer's fixed effects and propensity score matching, we present specifications with different lags in the outcome variables, in order to identify effects that might take longer to manifest. Lastly, we present some results using the value of the yield with proxy prices.

The results from our different specifications suggest that the intervention was partially successful, since the program had positive and immediate effects on the adoption of improved techniques, such as the introduction of improved varieties and the density of plantation for participants. The effects on productivity, however, are less clear. The improvements in methods were associated with increases in productivity for peaches only two years after participation in the program, and there is no evidence of increases in productivity for apples. When aggregating both apples and peaches, considering the value of production computed with average prices and taking into account the destination of the products, the results show no effect of the program. To the extent that the quality of the production is reflected by prices, these results do not support the notion that the program had an effect on the quality of the products. However, the interpretation is not definitive, since due to the nature of agricultural sector, investments may need a longer time frame to yield productivity or quality increases.

Propensity Score Matching

First, in order to identify a comparison group that resembled the treatment group, we model the participation in the program based on observable characteristics of the farmers at baseline. With the resulting predicted probability of participating, we can construct a comparison group that was as likely to participate in the program as beneficiaries, but who did not receive the program. The model includes a set of personal and farm's characteristics at baseline (in Table 5).

Table 5 - The PREDEG Participation model (Baseline Data, year 2000)

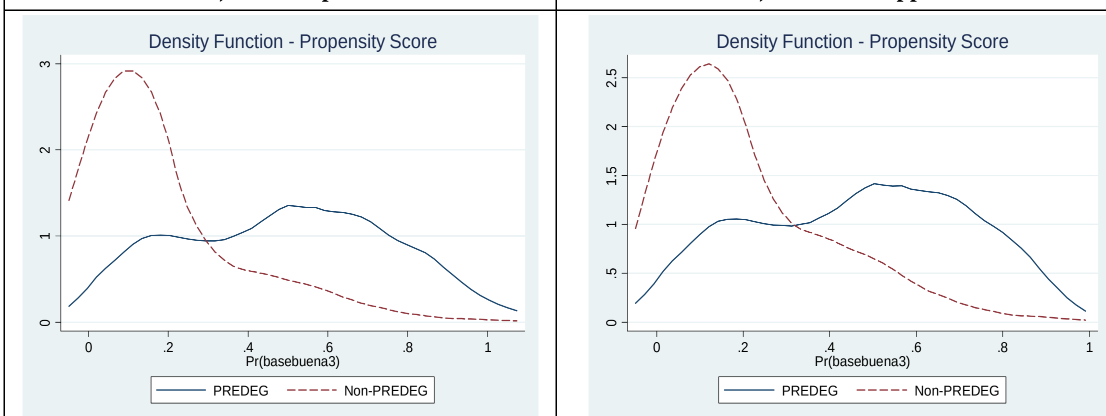
	Coeff.	Std. Error	Z	P>|Z|	[95% Conf. interval]	
Characteristics of the producers						
Age	-0.013	0.009	-1.46	0.144	-0.029	0.004
Gender	0.441	0.455	0.97	0.332	-0.45	1.333
Education	0.051	0.03	1.72	0.086	-0.007	0.11
Foreign	-0.063	0.466	-0.14	0.892	-0.977	0.85
Individual	0.319	0.491	0.65	0.516	-0.643	1.281
Company	0.471	0.564	0.84	0.403	-0.634	1.575
Characteristics of the farms						
Size (Plants)						
Micro	-0.426	0.363	-1.17	0.241	-1.138	0.286
Small	0.886	0.31	2.86	0.004	0.279	1.494
Small-Medium	0.83	0.338	2.46	0.014	0.168	1.492
Medium	1.504	0.434	3.46	0.001	0.653	2.355
Large	0.819	0.598	1.37	0.171	-0.352	1.99
Location	0.23	0.336	0.69	0.493	-0.428	0.888
Additional farm	-0.178	0.502	-0.36	0.722	-1.162	0.805
Employment						
Total Employment	0.053	0.028	1.88	0.06	-0.002	0.109
$(\text{Total Employment})^2$						
Skilled labor	-0.292	0.136	-2.15	0.032	-0.558	-0.025
Residents	0.004	0.051	0.07	0.943	-0.096	0.103
Machine and equipment						
Tractors	-0.232	0.282	-0.82	0.41	-0.784	0.32
Cold chamber	0.365	0.24	1.52	0.129	-0.106	0.836
Wire fence	-0.051	0.277	-0.18	0.855	-0.594	0.493
Technology and Management						
Administrator	-0.264	0.39	-0.68	0.499	-1.027	0.5
Technical assistance	0.662	0.257	2.58	0.01	0.158	1.166
Registers	0.437	0.231	1.89	0.059	-0.016	0.889
Irrigation systems	0.57	0.226	2.52	0.012	0.126	1.013
Health treatment	-0.204	0.623	-0.33	0.744	-1.426	1.018
Access to road and utilities						
Permanent access	-0.333	0.529	-0.63	0.529	-1.369	0.704
Phone	-0.492	0.335	-1.47	0.143	-1.149	0.166
Electricity	0.622	0.823	0.76	0.45	-0.992	2.235
Constant	-3.252	1.333	-2.44	0.015	-5.864	-0.64
Number of observation = 325						
LR chi2(28) = 113.11						
Prob > chi2 = 0.0000						
Log likelihood = -114.05296 Pseudo R2 = 0.3315						

According to the results from the participation model, the producer's and, especially, the farm's characteristics at baseline are strong determinants of participation, which may have led to biases if not properly controlled for. Beneficiary producers tend to be more educated, but we did not observe significant effects of age or gender. With regards to the farm characteristics, there are several factors that help predict participation. First, small and medium size farms were more likely to be treated, and the result in terms of total employment confirms that micro-farms did not participate in the program. Second, the technological and managerial development of the farms played a significant and positive role in determining the participation in the program, as shown by the positive coefficients of technical assistance, irrigation systems and registering economic activities. Finally, controlling for total employment, a higher share of qualified labor force seems to negatively affect the likelihood of participating in the program.[14]

Using the participation model, we predict the probability to participate in the program for both beneficiaries and non-beneficiaries and plot the resulting distributions of the propensity scores in Figure 1a. The wide distribution of the propensity score for treated individuals allows us to identify a wide common support, which is the intersection of the values of propensity score between treated and controls. In total, only 3 treated and 90 control farmers are outside of the common support.

Figure 1 - Propensity score distribution, full sample and common support

a) Full sample	**b) Common support**



The flatness of the distribution also reveals a rather low predictive capacity for beneficiaries, despite the strong determinants found in the participation model.

[14] We also estimated the same model including all the orchard producers included in the NAC, i.e. 1,754 producers of which 240. producers participated in PREDEG after 1999 at least in one fruit. The results did not change significantly, confirming that we properly identified the variables that have affected the participation into the program.

Many participating farmers have very low predicted probability of participating, which points to the possible existence of unobservable characteristics in the selection process that are not captured by this model. As a consequence, we have to rely on fixed effects estimations on the panel of producers to further reduce the biases in our estimation.

It is worth noticing the importance of including fixed effects in our regressions, since, in the absence of a powerful participation model, individual characteristics might still drive the results of OLS regressions on the common support. By including fixed effects, we eliminate biases coming from time-invariant characteristics that are correlated with program participation, both observable and unobservable. To the extent that participation in the program is driven by characteristics that tend to stay constant overtime (either observable, such as size of the farm, age, education, or unobservable characteristics such as ambition), fixed effects should greatly reduce selection bias.

However, fixed-effects leave time variant characteristics that are correlated with both participation and outcomes as the sole source of bias. If farmers choose to participate in exceptional years (good or bad), the coefficient of program participation will capture these effects, resulting in biased estimates. One way to check the presence of this bias is by measuring the correlation between participation and changes in productivity. If farmers decide to participate in exceptionally good years, the correlation should be positive and high. This is not the case, however, as the correlation between changes in productivity from the previous year and the decision to participate is only 0.01 for apples and 0.007 for apples. Furthermore, our methodology uses the large variation in the number of treatments, and different lags of the dependent variable to estimate medium term impacts, which should be less affected by the correlation between temporary shocks and participation.

Results

We focus our analysis on two measures that should capture the adoption of the main technology change supported by the program, which is switching production to certified varieties, as well as productivity. The conversion of varieties was the main activity financed by the project for peaches and apples[15], so we would expect that beneficiaries converted to certified varieties faster than non-beneficiaries. In order to measure this effect we will use the share of production in certified varieties and the density of plantation as indicators. Both indicators should be positively affected by the program, which should, under

[15] The program also targeted other fruits, like pears and prunes.

certain conditions, result in increased productivity. The results are presented below separately for apples and peaches.

Note that we use the number of cumulative treatments a farmer receives as our variable of interest, so the coefficient presented in the tables estimates the marginal effect of participating in the program. The implicit assumption is that an additional year of the program has the same effect regardless of whether the farmer participated previously in PREDEG. While one might expect the marginal effect to vary depending on the type of intervention, in the context of PREDEG it seems a plausible assumption. Most of the program's resources for peaches and apples were devoted to the conversion of varieties, which farmers usually carried out in different phases in different plots of land. As a consequence, one would expect that the conversion and density should be affected marginally by subsequent participations in the program. [16]

Finally, we present results for 1 year, 2 year and 3 year lags in the treatment variable in order to explore both the persistency and the delays of the program's effect.

Adoption of improved plant varieties[17]

The program had an immediate, positive and significant impact on the adoption of improved plant varieties for both peaches and apples. The effect on the percentage of production coming from certified varieties is large for the 1 year lag regressions, with estimated coefficients of 7 and 6.8 percentage points for apples and peaches respectively. For apples, the coefficient is robust to restricting the sample to the common support and to weighting observations by the propensity score. However, the effect seems to disappear 2 years after participation. For peaches, the effect is also significant for 1 year lag, and, although smaller, also significant for the 2 year lags.

[16] We relax this assumption in some specifications by introducing quadratic forms and separating the number of treatments into categories, but we found no evidence of diminishing returns to additional treatments in the variables of interest (density and percentage of production of new varieties). Interestingly, the exception is the adoption of improved plant varieties, which exhibits negative returns. The results for the quadratic form regressions are not shown, but are available upon request.

[17] In the second half of the 90's there was a pronounced change in the composition of produced varieties. This change was in part a response to the new demands for better quality and taste in fruits by more demanding domestic and external markets. (DIEA, 2003). In the case of apples, the recommended varieties comprised initially (1998) one third of the total number of varieties, increasing to almost 50% of the varieties in 2002. In the case of peaches, 68% were recommended varieties in 1998 and the percentage increased to 75% of the existing varieties in 2002 (DIEA 2003).

Table 6 - The PREDEG Impact on the Adoption of Improved Plant Varieties

	Fixed effects			Fixed effects common support			Weighted Fixed Effects		
	1 year lag	2 year lag	3 year lag	1 year lag	2 year lag	3 year lag	1 year lag	2 year lag	3 year lag
Apples									
Coef.	7.709***	1.681	0.189	7.064***	1.534	0.506	7.112***	1.783	-0.641
Std.error	(1.40)	(1.40)	(1.34)	(1.81)	(1.52)	(1.69)	(1.53)	(1.24)	(1.39)
# obs	929	754	603	740	604	485	907	736	589
# farms	195	176	172	147	136	135			
Peaches									
Coef.	6.645***	2.410*	1.24	6.811***	2.642**	1.286	3.222	2.997**	1.807
Std.error	(1.44)	(1.26)	(1.62)	(1.97)	(1.22)	(1.57)	(4.21)	(1.33)	(1.40)
# obs	1080	838	712	812	637	543	1062	824	701
# farms	267	222	214	193	166	160			

Notes: * significant at 10% level; ** significant at 5% level, significant at 1% level

The contemporaneous effect on adoption of improved varieties suggests that the program accelerated the adoption of improved plant varieties, both for peaches and apples. The impact is also consistent with the fact that the PREDEG centered its services of technical assistance and funding on producers of improved varieties, so the participation in the program incentivized their adoption.

The interpretation of the results needs to take into account the process by which conversion takes place. At first sight, since newly planted trees usually take between 3 and 4 years to become fully productive, it might be somewhat counterintuitive that a higher share of production is coming from new varieties. However, since conversion takes place also by replacing old trees of noncertified varieties, changes in the production mix and in the overall land productivity can occur in the short run. This makes a progressive conversion of the productive land across several years more efficient, partly explaining the pattern of participation in the program. Since we do not observe large increases in the adoption of certified varieties for non-beneficiaries, it may also explain the lack of persistency we observe when using lagged results, since the conversion is usually associated with participation in the previous year, regardless of previous participations.

Density of Plantation

An additional objective of the program, which was associated with the conversion, was to increase the density of plantation to maximize productivity, and the results show very robust, persistent effects of the program on density, measured by the number of trees per ha. In both apples and peaches, this effect is consistent across models, and seems persistent across time, as shown by strong lagged effects. The lagged effect increases for apples, indicating that the density increases further in later years after participation and suggesting an important growth in density for beneficiaries when adding up the effects across the years, In the case of peaches, the effect is decreasing, but still positive and significant.

Table 7 - The PREDEG Impact on the density of plantation

	Fixed effects			Fixed effects common support			Weighted Fixed Effects		
	1 year lag	2 year lag	3 year lag	1 year lag	2 year lag	3 year lag	1 year lag	2 year lag	3 year lag
Apples									
Coef.	50.099***	51.558***	62.156***	39.366***	42.923***	57.411***	34.352***	42.497***	57.582***
Std.error	(12.74)	(12.86)	(11.58)	(12.96)	(9.31)	(14.49)	(12.55)	(8.72)	(10.89)
# obs	754	754	603	604	604	485	736	736	589
# farms	176	176	172	136	136	135			
Peaches									
Coef.	46.572***	39.328***	29.303***	42.697**	38.352***	30.194***	-16.465	28.239***	25.141***
Std.error	(15.52)	(10.47)	(7.68)	(17.92)	(11.62)	(10.93)	(38.22)	(10.71)	(8.61)
# obs	838	838	712	637	637	543	824	824	701
# farms	222	222	214	166	166	160			

Notes: * significant at 10% level; ** significant at 5% level, significant at 1% level

An increase in the density of plantation, however, 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[18] 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). This threshold, which corresponds to the optimal density of plantation, depends on the characteristics of the soil, the availability of inputs and the variety of fruit.

Unfortunately, we do not have information on the implementation of complementary changes in production methods or on the optimal density for each producer. However, we can use the end outcome, productivity, to test whether these improvements in adoption of new technologies resulted in increases in productivity for producers.

Productivity

Despite the increases in the density of plantation and the change in varieties for beneficiaries, the evidence of increases in productivity is scarce. The program increased the productivity for peach producers, but not for apple producers. Concretely, there is no increase in the productivity of apples in one and two year lags, and a significant negative effect the third year after participation. Conversely, there is a positive effect for peaches one and two years after participation.

[18] See for example Vig and Kallay (1983) Ng, Leong and Yoon (1983).

Table 8- The PREDEG Impact on Productivity

	Fixed effects			Fixed effects common support			Weighted Fixed Effects		
	1 year lag	2 year lag	3 year lag	1 year lag	2 year lag	3 year lag	1 year lag	2 year lag	3 year lag
Apples									
Coef.	-0.238	0.021	-2.532***	-0.205	-0.021	-2.773**	-0.326	0.155	-2.541***
Std.error	(0.82)	(0.71)	(0.96)	(0.95)	(0.68)	(1.30)	(0.89)	(0.64)	(0.78)
# obs	788	788	619	630	630	498	770	770	605
# farms	182	182	175	140	140	137			
Peaches									
Coef.	0.803*	0.726**	0.506	0.917**	0.833**	0.435	1.111	0.129	0.138
Std.error	(0.44)	(0.35)	(0.40)	(0.36)	(0.35)	(0.40)	(0.86)	(0.53)	(0.50)
# obs	888	888	732	668	668	554	874	874	721
# farms	228	228	218	170	170	162			

Notes: * significant at 10% level; ** significant at 5% level, significant at 1% level

The difference in the productivity effects of the program for apples and peaches seems somewhat counterintuitive at first. Beneficiaries experienced faster rates of conversion and increased density of plantation for both products, so we would expect similar effects on productivity from these modifications. As is evident from the table this was not the case.

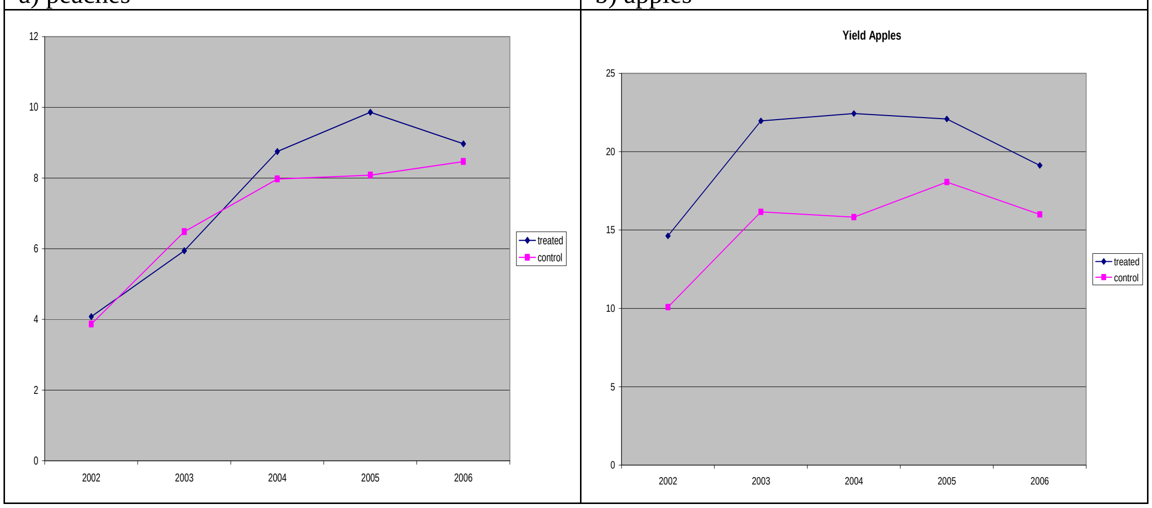
A possible explanation for these differences springs from the fact that the density of apple plantations grew faster than for peaches. Increasing the density of plantation implies planting new trees, which combined with the change in varieties, might lower the average productivity in the short run. If this were the only explanation, however, we would expect the negative effects on productivity to be evident from the first year after participation. We would also expect the coefficients of lagged variables to grow with longer lags, as newly planted trees regain productivity. In fact, the coefficients show the opposite trend, with productivity decreasing progressively after participation.

A more plausible explanation has to do with the pace of conversion and the density. Farmers convert their land to the new varieties in phases, as we saw above. If the pace of conversion is faster than the rate of growth of the production of newly planted trees, we would expect larger decreases in productivity arising from higher conversion rates. Combined with the large growing increases in density, this may yield negative effects on production. Since the density increased notably faster than the conversion, especially for apples, it is possible that the new trees have a negative effect on existing trees by competing for nutrients and sunlight. Given the time frame of the evaluation, it is impossible to discern whether this effect dissipates in the medium run.

In the presence of these short term adjustment costs, the immediate positive effect of the program on the productivity for peaches, which is persistent for two-year lags, requires further analysis. If peaches orchards were also converted at a high rate for beneficiaries and their density increased, as we saw in the previous results, the productivity adjustment should be similar. However, we observe a positive one and two-year lagged effect on productivity. The slower pace of increase in density for peaches is unlikely to explain the differences between the two fruits. A more plausible explanation has to do with the high prevalence of a peach-specific illness during program implementation. In the 2000 and 2001, peach trees were severely affected by root asphyxia, which reduced the peach production significantly for the next three years[19]. This condition, caused by unusually high levels of rain, can be diminished through better conduction methods and water management, which were part of the technical assistance provided by the program. In fact, dealing with pests was one of the main topics the technical assistance, so participants might have been able to cope better with the pest or ease their recovery from the illness, because of the training. Consistent with this, figure 2 shows how the yields for peaches increased more steadily for participants than non participants, which was not the case for apples.

Figure 2 - Yield per ha, peaches and apples 2002-2006

a) peaches | b) apples



Even if the program helped participants with the recovery of productivity levels in peaches, the question on whether the introduction of these technologies result in positive impacts on productivity still remains. There is no evidence that yields increased more for beneficiaries, except in the case of peaches, which is likely to

[19] DIEA (2003).

be driven more by the effect of the root asphyxia than by the conversion to improved varieties.

It is possible that the program still met its objective to increase the value of production, however, even in the absence of productivity increases. The objective of the change in variety might have been to improve the quality of the production, not necessarily to increase more kilograms per hectare of fruit. If these were the case, the higher price should compensate the absence of increased yields, if the program did in fact increase the value of the overall production for beneficiaries. We explore this next.

Value of production

In order to test that hypothesis we introduce prices into our analysis. Unfortunately, we do not have individual prices for all years, so we use the 2002 prices for each variety reported by DIEA to calculate the value of production per hectare for each producer[20]. In order to incorporate as well the additional factor that some varieties are more attractive for the external sector (and precisely because of this they were recommended), we calculated an average of the price of each variety in the domestic market, the price to the industry and the export price. Finally, we added the value of the production of each variety of apple and peach and divide it by the total surface with crops for this two fruits.[21] The results are presented in Table 9.

Table 9 - The PREDEG Impact on the aggregated value of yields

		Fixed effects			Fixed effects common support			Fixed effects common support		
		Level	1 year lag	2 year lag	Level	1 year lag	2 year lag	Level	1 year lag	2 year lag
Aggregate Value	Coef.	2.827	2.501	2.86	1.073	3.733	3.992	-1.626	-0.085	0.432
	Std.error	(7.25)	(9.22)	(7.46)	(7.37)	(7.25)	(9.29)	(7.30)	(7.50)	(8.85)
	# obs	1354	809	818	1101	997	603	1085	997	603

Notes: * significant at 10% level; ** significant at 5% level, significant at 1% level

We do not find any effect of the program on the value of production per hectare, not even when we consider the lagged effect, in none of the three specifications. This is somewhat surprising, since one of the objectives of the program was to increase the value of production through improvements in productivity and quality. This lack of significance, however, might be partially due to the

[20] We also run the regressions using the prices that each individual reported to have received for his/her fruits in the year 2004 and results were similar.
[21] We also tried adding the value of the varieties of all fruits: apples, peaches, pears, nectarines, quinces and plumbs, the results were again qualitatively similar.

relatively short time horizon of the evaluation, in which the density of plantation and the switch in varieties drove down yields for apples and peaches were affected by a debilitating illness.

Conclusions and Recommendations

Although the results vary according to the crops, we find consistent evidence that the PREDEG program increased the rate of adoption of certified varieties and the density of plantation. However, we find only limited evidence of its effects on productivity. We show evidence that the program assisted peach producers to cope with the severe root asphyxia that affected the sector in the early years of the decade. Conversely, we find some indications of negative lagged productivity effects for apples, which might be related to a short term cost of transitioning to new varieties or technologies. While the evidence of the effects on yields is not definitive due to the limited timeframe of the evaluation, the results indicate that PREDEG services were useful for incentivizing the adoption of specific technologies, as well as for crop-specific technical assistance like plant health.

These results suggest that public intervention in extension services can result in quicker technology adoption and can be used to incentivize the adoption of necessary changes in production methods that might be stalled due to poor individual incentives to pay for these services, or to the absence of supply of these services.

The results are also relevant for the particular method of intervention. Co-financing of the services by producers and combined with private provision might be an effective tool to target a certain type of producer, notably those with resources that might not implement reforms due to lack of incentives. This method, however, has limited reach for low income producers, since the co-financing and credit restrictions limit their ability to participate. As a consequence, other types of institutional settings might be advisable to complement such schemes.

Finally, while this paper contributes to the literature on the effectiveness of such policies, there is a large range of topics for future research on this area. There is a need to balance demand-driven mechanisms to ensure the appropriateness of the trainings and effective targeting of these services to producers whose behavior can be modified, in order to maximize the effectiveness of the program. The existing evidence on these programs points to the fundamental role of targeting to maximize the effectiveness. However, the definition of appropriate targeting rules is not decided. Other topics, such as the method of provision (groups vs.

individual, for example), or the effectiveness of these programs in improving managerial techniques or market access need to be explored more carefully in the future.

The application of rigorous impact evaluation techniques to measure the effects of agricultural programs is becoming more common in the literature. However, the absence of randomized evaluations in these programs should be overcome since, when properly designed, they can provide the answers not only to the question of effectiveness of the overall intervention, but to the particulars of different implementation models.

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