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Do agri-environmental schemes help reduce herbicide use? Evidence from a natural experiment in France

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Abstract

Agri-environmental schemes (AES) are a central component of the environmental policy of the European Union. Despite widespread interest and investment in AESs, few of these programs have been carefully evaluated and doubts are often expressed about the effectiveness of voluntary programs. The purpose of this article is to estimate the additional effects of AESs targeting nonpoint source pollution from pesticides, focusing on one emblematic case study: herbicide use in vineyards. We use original data collected from winegrowers participating in AESs in the south of France, and we use exogenous variation in the timing of the implementation of the AESs as a natural experiment. We show that the quantity of herbicides used by participants in the program in 2012 was around 30% below what they would have used without the program, while the impact was significantly higher in 2011 - around 50% - presumably because of higher weed pressure. Although significant, these impacts remain smaller than what had been expected by policy makers. Focusing on the "zero herbicide between the vine rows" option, which is both the most often chosen as well as the least stringent among the measures, we moreover show the presence of windfall effects. Simple extrapolation of these results suggests that this level of effectiveness may not be sufficient to ensure water quality in the watersheds targeted by the AES.

Key words: Agri-environmental scheme, water quality, nonpoint source pollution, herbicides, pesticides, natural experiment.
JEL: Q15, Q18, Q25, Q28, Q53.

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1 Introduction

Increasing concern over the environmental impact of agriculture led to the introduction of agri-environmental schemes (AESs) in the European Community in the mid-1980s. AESs are voluntary contracts under which farmers are offered payments for reducing the negative externalities of agricultural production, and constitute a central component of the environmental policy of the European Union.[1] However, doubts are often expressed about the effectiveness of these programs. Because AESs are voluntary programs whose requirements and per-hectare payments are generally constant for all farmers, the potential for adverse selection is high (Fraser, 2009; Chabé-Ferret and Subervie, 2013). Indeed, farmers receive payments in exchange for adopting certain agricultural practices, ones that they may well have adopted even in the absence of these payments. In the extreme case in which an AES attracts only farmers who would have behaved the same way in the absence of payment, the additional effect of the AES is null, while the windfall effect is maximum.

Despite widespread interest and investment in AESs (Uthes and Matzdorf, 2013; Udagawa, Hodge and Reader, 2014), few programs have been thoroughly evaluated. Chabé-Ferret and Subervie (2013) show that French AESs that impose strong requirements, such as the AES subsidizing conversion to organic farming, had large additional effects and almost non-existent windfall effects. On the contrary, the authors find that AESs with modest aims, such as the AES only requiring farmers to add one crop to the rotation, have generated very limited additional effects. Pufahl and Weiss (2009) moreover show, from a non-representative sample of German farms, that benefiting from AESs may significantly reduce the purchase of farm chemicals. To our knowledge, there has been no evaluation of AESs specifically targeting the use of pesticides to date.

Contamination by pesticides from agriculture is a source of water quality degradation in several countries in the European Union. This occurs when pesticides used in fields are picked up and carried away by runoff and deposited into lakes, rivers, wetlands, coastal waters, and underground sources of drinking water. These pollutants are of increasing concern because of their potential impacts on the environment, wildlife, and human health. Within the context of the European Union water framework directive, French AESs that aim to reduce pesticide runoff from fields have been implemented in watersheds where water quality improvement has been identified as a priority. Despite their importance, these AESs have received little attention with respect to their impacts on agricultural practices. The purpose of this article is to estimate the additional and windfall effects of AESs targeting nonpoint source pollution from pesticides, focusing on one emblematic case study: herbicide use in French vineyards.

Of all the cropping systems in France, wine growing uses the most pesticides, with an average application of 16 phytosanitary treatments per hectare in 2010. Indeed, growing wine grapes requires high levels of protection against bio-aggressors and competitive weeds in order to ensure adequate levels of production (Agreste, 2012). Among the chemicals used by winegrowers, herbicides are the most commonly detected in the ground and surface waters. Given the extent of winegrowing and its heavily reliance on herbicides, incentivizing winegrowers to reduce their use

[1] The European Agricultural Fund for Rural Development (EAFRD) has been allocated a budget of EUR 96.3 billion for the period 2007-2013 (20% of the funds dedicated to the CAP), of which EUR 1.8 billion has been allocated to French AESs. Figures are available here: http://agriculture.gouv.fr/pac-developpement-rural-feader.

of herbicides is a major challenge. Languedoc-Roussillon contains the most vineyards of any region in France, covering 236,500 ha and constituting 30% of the nation's vineyards. Two out of every three farms in the region grow wine grapes (Agreste, 2011). The AESs implemented here are area-specific and designed to address specific environmental issues, including water pollution. AESs targeting pesticide use were introduced in 2007 and include a major innovation compared to previous AESs: they target the most environmentally-sensitive sites and are implemented by local operators (hereafter referred to as site operators). These AESs are currently implemented in 29 watersheds in the Languedoc-Roussillon region, most of which exhibit levels of herbicide residues exceeding the regulatory limit.

The main alternatives to using herbicides for weed control are mechanical methods such as tillage, controlled grassing, and mowing. However, mechanized weeding under vine rows requires specific investments, and farmers willing to reduce their use of herbicides without these further investments usually combine the use of mechanical alternatives between the rows with chemical weeding underneath the rows. Because these alternative techniques have been increasingly implemented by farmers (19 percent of vineyards using no herbicides before the AESs were launched (Agreste, 2012)), the additional impact of the monetary incentives offered to farmers for reducing their use of herbicides deserves evaluation.

Previous work on the evaluation of AESs relies on non-experimental data and has relied on identification strategies that address the issue of self-selection bias (Chabé-Ferret et Subervie, 2013; Pufahl et al., 2009; Udagawa et al., 2014). To do so, these studies employed DID-matching methods, which eliminate selection bias by comparing participants in AESs to observationally-identical non-participants, assuming that farmers' self-selection into AESs is due to both observable and unobservable factors that are constant through time (Heckman, Ichimura, and Todd, 1997). We depart from the approach used in these studies by exploiting exogenous variation in the timing of the implementation of the AESs. Because the process governing the eligibility of winegrowers for an AES arguably resembles random assignment, we are able to circumvent the empirical issue of self-selection and to estimate the causal effect of the AES on pesticides use. We are moreover able to assess this effect over time because our dataset includes almost all participants in the AES under study in two consecutive years: 2011 and 2012. We show that the quantity of herbicides used by participants in the program in 2012 was around 30% below what they would have used without the program, while this impact was significantly higher in 2011 - around 50% - presumably because of higher weed pressure. Our results moreover show that the farmers who engaged in the "zero herbicide between the rows" option, both the most often chosen as well as the least stringent measure, would not have applied as much herbicides as expected in the absence of the AES, which indicates the presence of a windfall effect. Although significant overall, the specific impacts we find remain smaller than those initially expected by policy makers. Simple extrapolation of these results moreover suggests that these AESs may not be sufficient to ensure improved water quality in the watersheds targeted by the AES.

The remainder of this article is organized as follows. We first present the theoretical framework, which allows us to define the parameter we aim to recover. We then present the data and the identification strategy used. Thereafter we present and discuss the results of the evaluation of the overall program and of one specific measure - the so-called “zero herbicides between the vine rows”. We present results that are based on the natural experiment assumption, as well as those we obtain from various matching estimators. We also present the results of several robustness checks and of a sensitivity test. Finally, we present some illustrative figures in order to discuss the likely impact of the AES on water quality.

2 Theoretical framework

In order to define the parameter that we aim to recover in the empirical analysis, we use the framework provided by Chabé-Ferret and Subervie (2013). We model a winegrower who decides to participate or not participate in the AES and chooses the level of herbicide use that maximizes his utility given the AES requirements. Equation (1) assumes that the winegrower derives his utility U from consumption C, leisure L, and on-farm work H. His utility also includes his taste or distaste for herbicide use Y, as well as consumption shifters S (such as family size) and others that are generally unobservable, such as ecological preferences (δ). The production function presented in Equation (2) shows that the winegrower produces the quantity Q, whose price is p^Q, using a quantity of herbicides that we denote Y, labor (H), physical and human capital and land (I), as well as other unobservable factors, such as managerial ability, land quality, and climate conditions (ϵ). If the winegrower enters the AES (AES = 1), he receives financial compensation P and must restrict his use of herbicide Y, so that $Y \leq \bar{Y}$ (Equation 3). The winegrower derives income from farming, from the AES if enrolled, and also from working H^{off} hours off of the farm for a wage w (Equation 4). Equation (5) describes the time constraint.

$$\max_{C,L,H,H^{\text{off}},Y} U(C,L,H,Y,S,\delta) \tag{1}$$

$$\text{subject to:}$$

$$Q = F(Y,H,I,\epsilon) \tag{2}$$

$$\text{AES}.(Y - \bar{Y}) \leq 0 \tag{3}$$

$$C = p^Q Q - p^Y Y + wH^{\text{off}} + \text{AES}.P \tag{4}$$

$$L + H + H^{\text{off}} = J \tag{5}$$

The first order condition for the input level is:

$$\frac{\partial U}{\partial C}\left(p^Q \frac{\partial F}{\partial Y} - p^Y\right) + \frac{\partial U}{\partial Y} - \lambda^Y \text{AES} = 0$$

where λ^Y is the Lagrange multiplier. When AES = 0, the winegrower uses the quantity of herbicide Y^0 that equalizes the marginal increase in utility, due to a marginal increase in agricultural profits, with the marginal disutility of using inputs:

$$Y^0 = g_0(p^Q, p^Y, w, J, I, S, \epsilon, \delta)$$

When AES = 1, the winegrower uses the quantity of herbicide Y^1:

$$Y^1 = g_1(P, \bar{Y}, p^Q, p^Y, w, J, I, S, \epsilon, \delta)$$

The individual-level causal effect of the AES is thus equal to $(Y^1 - Y^0)$ and is by definition unobservable. The parameter of interest that we aim to estimate is the average causal effect of the AES on the amount of herbicides used by those who actually participated in the AES, the so-called Average Treatment effect on the Treated (ATT):

$$\text{ATT} = E[Y^1 - Y^0|\text{AES} = 1] = \underbrace{E(Y^1|\text{AES} = 1)}_{\text{observable}} - \underbrace{E(Y^0|\text{AES} = 1)}_{\text{unobservable}}$$

As in most impact analyses, the average level of herbicide that would have been used by participants, had they not participated in the AES, $E(Y^0|\text{AES} = 1)$, is unobservable in our data. The purpose of the empirical analysis is precisely to estimate this level.

3 Sample and Data

Our sample includes winegrowers located in three counties of the Languedoc-Roussillon region in the south of France - watersheds where water quality improvement has been identified as a priority by public authorities. Between 2007 and 2013, 414 farmers in this area engaged a total number of 8,672 hectares of land in the AES pertaining to water quality. This amounted to a budget of 8.8 millions €.[2] Winegrowers were given the opportunity to participate in AESs aiming to reduce herbicide use, the main pesticide responsible for nonpoint source water pollution in the area. They were able to choose one or more of four possible options: convert to organic wine growing in exchange for 350 €/ha, eliminate all herbicide use for 243 €/ha, reduce herbicides use by 40% of the regional standard for 141 €/ha, and eliminate herbicide use between vine rows only for 165 €/ha. Of the 4,268 ha of vineyards engaged in the scheme between 2007 and 2012, our database includes 3,390 ha, or approximately 80% of the total area under contract in the region.[3] Table 1 shows that the most frequently chosen option in our sample was eliminating herbicide use between the vine rows only, chosen by 79% of farmers in the engaged areas. This proportion is very close to the actual regional take-up of 72%.

The sample used for analysis includes exclusively participants in the AES (Table 2). These winegrowers are expected to differ from other winegrowers in various dimensions. Indeed, many individual-specific factors may determine participation in AESs that target pollution from herbicides. For example, the size of the labor force working on the farm can influence the adoption of alternatives to herbicide use, as these practices are more time-consuming. The availability of water for irrigation can also facilitate weed control through the practice of grassing, as this reduces competition for water in the soil under the vines. In contrast, steep slopes in the vineyard could present

[2] Data are available here: http://draaf.languedoc-roussillon.agriculture.gouv.fr/Commissions-regionales-agro

[3] Our data also include an additional 238 hectares engaged in 2013. Unfortunately, we were not able to collect all necessary data regarding the areas under contract in 2013.

an obstacle to implementing mechanized weeding practices. Additionally, the type of grape produced and the wine-making process involved can influence the sale price of the production, and indirectly the motivation and ability of farmers to begin reducing their use of pesticides. Finally, the degree of a household's reliance on farm income is likely to decrease the probability of participation in an AES, as a reduction in pesticide use is expected to increase yield variability.

In order to study to what extent winegrowers from our sample differ from neighboring non-participants, we estimate a logistic regression where the participation in the AES depends on a variety of individual-specific factors. To do so, we matched our data to the French Agricultural Census that was conducted in 2010 by the Department of Statistics of the French Ministry of Agriculture. This database contains a detailed description of every farm during the farming year 2009-2010, i.e. before the first wave of participation in the AES in 2011. In order for this comparison to make sense, we focus on a subset of winegrowers from this database who were eligible for the AES.[4] Results are displayed in Table 3. The results suggest that winegrowers who had received agricultural education or training and whose spouse also works on the farm are two times more likely to participate in the AES. They also indicate that those who produce wine under geographically protected appellations, which guarantees higher sale prices, are more likely to participate in the AES. In our data, the geographical conditions faced by participants do not differ from those faced by non-participants. Interestingly, the proportion of Utilized Agricultural Area (UAA) cultivated without herbicides is not significantly higher among participants, which suggests moderate windfall effects of the AES.

The quantity of herbicides used by winegrowers is private data, and even if most of the winegrowers maintain records of the treatments that they apply, they are under no legal obligation to provide this information. However, data of this sort are routinely solicited of winegrowers participating in AESs. Indeed, every farmer willing to participate to the scheme was required to undergo a diagnosis of his farm by a certified technician who was frequently the site operator himself. As part of this diagnosis, information on the quantity of herbicides used during the previous farming season was collected for each plot on the farm. This appraisal was conducted every year during the entire period of the AES through an annual follow-up. We collected both the initial diagnosis as well as the follow-up documents held by the site operators for almost every farmer participating in the scheme. Our sample includes farmers who entered the scheme in 2010, in 2011, in 2012, or in 2013. For farmers who entered the scheme in 2010 (resp. 2011; 2012; 2013), we were able to collect data on the quantity of herbicides used in 2009, 2010, 2011 and 2012 (resp. 2010, 2011 and 2012; 2011 and 2012; 2012). From these documents, we were able to calculate the quantity of herbicides used by winegrowers on the plots under contract, as measured through the Treatment Frequency Index (TFI). This index represents the number of so-called reference doses of herbicides applied during a farming year (Pingault et al., 2009). The reference dose is often considered the normal

[4] As no listing of the eligible farmers was available, we focused on farmers located in the same municipalities as participating farmers, assuming that they were eligible, as well. Indeed, the main criterion for eligibility was the location of the vineyard within one of the areas targeted for water quality recovery.

dose, as it corresponds to the efficient dose of a product for a specific culture and pest:

$$\text{TFI} = \frac{\text{treated area}}{\text{total area}} * \frac{\text{dose used}}{\text{reference dose}}$$

For example, if the reference dose of an herbicide is spread over the entire area of a plot, then the TFI of the plot equals one. If the herbicide is spread at its reference dose but only under the vine rows, the TFI of the plot equals 1/3 (because the space between vine rows is roughly twice as wide as the vine row).

4 Identification Strategy

Our sample includes three groups of winegrowers: those who entered the AES in May 2010, those who entered the AES in May 2011, and those who entered the AES in May 2012. These data allow us to estimate two impacts: the impact of the AES on herbicide use in 2011 (that we note ATT_{2011}) as well as the impact of the AES on herbicide use in 2012 (that we note ATT_{2012}). The definition of the identification strategy for the impact in 2011 is:

$$\text{ATT}_{2011} = E(\text{TFI}^1_{2011}|\text{AES}_{2011} = 1) - E(\text{TFI}^0_{2011}|\text{AES}_{2011} = 1)$$

where $E(\text{TFI}^1_{2011}|\text{AES}_{2011} = 1)$ measures the amount of herbicides actually used in 2011 by those who were participants in the AES in 2011 and $E(\text{TFI}^0_{2011}|\text{AES}_{2011} = 1)$ measures the amount of herbicides that would have been used in 2011 by the same group had they not participated in the AES. From the group of winegrowers who entered the AES in May 2010, we are able to estimate $E(\text{TFI}^1_{2011}|\text{AES}_{2011} = 1)$ directly using the mean value of the TFI. Obviously, we are not able to estimate $E(\text{TFI}^0_{2011}|\text{AES}_{2011} = 1)$ from the same group. However, we can estimate this from the group of individuals who entered the scheme in May 2012, provided the following assumption holds:

$$E(\text{TFI}^0_{2011}|Z, \text{AES}_{2011} = 1) = E(\text{TFI}^0_{2011}|Z, \text{AES}_{2012} = 1)$$

where Z denotes the set of information available to the winegrower when deciding to participate in the AES. This assumption means that the mean value of the TFI that we observe in 2011 among those who entered the AES in 2012 equals the mean value of the TFI that we would have observed in 2011 among those who entered the AES in 2010, had they not participated in the AES.

In order to discuss to what extent such an assumption is likely to hold, we must examine a farmers decision to enter the AES. Returning to the theoretical framework, we model the winegrowers' decision to enter the AES in the following way:

$$\text{AES} = 1\{E(V_1 - V_0|Z) - W \geq 0\}$$

where W is the disutility of applying for the AES (due to various transaction costs, for example), V_1 is the indirect utility of the individual when he participates, and V_0 is the indirect utility when he does not. The selection bias problem occurs when some factors stored in Z are also determinants of herbicide demand, which is likely to be the case in our framework:

$$Z = (P, \bar{Y}, p^Q, p^Y, w, J, I, F, \epsilon, \delta)$$

For example, one can reasonably suppose that winegrowers who feel concerned about environmental issues (those who have a high δ) are simultaneously more likely to participate in AESs and less likely to use herbicide even in the absence of any AES (they have a low TFI^0). Because the level of δ determines the level of TFI^0, the participant group and the comparison group must have the same δ on average when deciding to enter the AES in order for the comparison we propose to be valid.

Areas under study were eligible to participate in the AES at different dates due to administrative delays, which we believe to be exogenous to winegrowers' practices. In practice, AES implementation by local operators in priority areas required many administrative procedures that ended up delaying the availability of contracts to farmers. As a first requirement, each priority watershed underwent a hydro-geological diagnostic in order to assess its vulnerability and accurately delineate the limits of the area targeted for the AES. An official decree then had to validate this delimitation. Next, a local operator was nominated to design an agri-environmental project on the basis of a second diagnostic. This second diagnostic aimed to identify the current farming practices in use at the site in order to best adapt the proposed options to the needs of the local farmers. The operator was required to choose from a national menu of options, two options that were to be offered to the farmers at his site for each farming activity. As the last requirement, the final scheme had to be validated by a regional committee that meets once per year. After this process was complete, the operator was then in charge of introducing the AES to the farmers at his site. Farmers willing to participate then had to send in an application form by the 15th of May. The time needed to implement these steps was highly heterogeneous between sites, which contributed to the incremental availability of the AES to farmers in different areas. We make use of this exogenous variation in the timing of AES implementation in order to estimate its causal effect on the quantity of herbicides used by participating farmers.

Because our sample includes only farmers who engaged in the AES as soon as they were offered the opportunity to do so,[5] we assume that the individuals who entered the scheme in May 2010 and the individuals who entered the scheme in May 2012 had the same probability of entering the scheme in 2010, given Z. If we accept the qualitative information available regarding the timing of the program's implementation, then participants and future-participants differed only in their eligibility, which is exogenous to their practices. Under this assumption, the impact of the AES in 2011 can then be properly identified through a comparison of the average use of herbicides of participants located on early-approved sites (eligible in 2010) with the average use of herbicides of future participants located on late-approved sites (eligible in 2012). While we are not able to test the natural experiment assumption directly, we are able to test whether both groups have similar observable characteristics in 2009, i.e. before the first wave of participation in the AES, using data

[5] Eligibility and participation indeed coincide for most individuals in the sample. We intentionally drop the small number of those who did not choose to enter the scheme as soon as it was available because we doubt that those farmers have similar environmental concern and motivation as those who chose to enter without delay.

obtained from the French Agricultural Census. In addition, we can test the sensitivity of the identification strategy in order to determine how strongly an unmeasured variable must influence the selection process such that it undermines the effectiveness of the identification strategy. Results of those tests are presented in Section 5.

Finally, even if the natural experiment assumption holds, we must make a second assumption, known as the Stable Unit Treatment Value Assumption (SUTVA) for the identification to be valid (Rubin, 1978). SUTVA requires that the treatment received by a subject does not alter the outcome for other subjects. The SUTVA assumption fails if, for example, the quantity of herbicides used by farmers in late approved sites had been influenced by the implementation of the scheme in early approved sites. It is very unlikely that such a phenomenon occurred in our framework, as groups are usually located on sites distant from each other. Participation of first-wave farmers may have encouraged the participation of second-wave farmers to some extent, but this does not invalidate the identification strategy as long as second-wave participants did not change their practices before they entered the scheme. We test the SUTVA in Section 5.

5 Results

In this section, we present and discuss the results of the estimations based on the natural experiment assumption, as well as those we obtain from various matching estimators. We also present the results of several robustness checks and a sensitivity test. Finally, we present some illustrative figures in order to discuss the likely impact of the AES on water quality.

5.1 Overall impact of the AESs

In order to properly estimate the impact of the AES on farmers, we compare current participants to future participants in the AES. Based on the previous section, we argue that current participants and future participants had similar probabilities to enter the AES in 2010 given their characteristics Z. Under this assumption, we can recover the average treatment effect of the AES by comparing them directly without controlling for their characteristics. Nevertheless, we present both estimators (the direct comparison and the comparison conditioned on covariates) as a robustness check.

In order to estimate the impact of the AES in 2011, we first compare the TFI between the group of individuals who engaged in the AES in 2010 and the group of those who engaged in 2012. In order to obtain a standard error, we simply regress the outcome on the treatment variable, which is a dummy variable that takes on the value of one when treated and zero otherwise. The results are displayed in Table 4 (Panel A). They show that the AESs have a significant and large overall impact on the TFI in 2011: the quantity of herbicides used by participants in the program was 50% below the quantity that would have been used without the program. We run the same regression excluding the number of farmers who engaged in organic farming schemes because we suspect that they may drive this estimate (Panel B). The result does not change.

We then run the regression on Panel C, in which individuals in the treatment group are those who engaged in 2010 or 2011, while individuals in the control group are those who engaged in

2013. Results show that the quantity of herbicides used by participants in the program in 2012 was 0.45, i.e. 38% below the quantity used in the control group. Unfortunately, this result lacks precision (we reject the null hypothesis at the 10% significance level only). In order to increase the sample size, we test the hypothesis that farmers who engaged in the AES in May 2012 can be considered as controls in 2012. It is indeed reasonable to believe that farmers who engaged in the AES in May 2012 were not able to meet the contractual commitments before the next grape harvest (September 2012) due to the fact that a large portion of herbicide applications occur before May. We thus compare the mean value of the TFI in the subgroup of farmers who engaged in the AES in May 2012 to the mean value of the TFI in the subgroup of farmers who engaged in May 2013. The result shows that the TFI does not differ significantly across these groups.[6] We conclude that SUTVA holds in our data (see Section 4) and consequently that farmers who engaged in the AES in 2012 can be considered as untreated in 2012. Results of the estimation from the sample in which the individuals used as controls are those who engaged in 2012 or 2013 are displayed in the lower part of Table 4 (Panel D). As expected, the estimate appears to be more precise (we now reject the null hypothesis at the 5% significance level). The impact is now slightly smaller:[7] the quantity of herbicides used by participants in the program in 2012 was 0.6, i.e. 27% below the quantity used in the control group.

Taken together, the results from Table 4 suggest that the impact of the AESs in 2011 was larger than in 2012. In order to test the assumption that these impacts actually differ, we estimate a panel-data model focusing on the subgroup of individuals for whom we have data on TFI in both years 2011 and 2012:

$$\mathrm{TFI}_{it} = \alpha + \beta_0 \mathrm{AES}_i + \beta_1 \mathrm{AES}_i * T_t + \beta_2 T_t + \epsilon_{it}$$

where the variable AES takes on the value of one when the farmer is treated and zero otherwise. Individuals used as treated are those who engaged in 2010, and individuals used as controls are those who engaged in 2012.[8] The variable T takes on the value of one in 2011 and zero in 2012, the variable *AES*T* is an interactive term, and α refers to the constant term of the model. In this regression model, the estimate of the impact in 2011 equals $\beta_0+\beta_1$, while the estimate of the impact in 2012 equals β_0. Results are displayed in Table 5. Columns (1)-(2) display results from the entire sample. Taking the average effect in both years, our results suggest that the AESs had a significant impact on TFI - specifically a 45% decrease compared to the counterfactual situation (Column 1). Regarding the heterogeneity of the impact across years, results in Column 2 confirm that the impact in 2011 indeed differs from the impact in 2012 (we reject the null hypothesis that β_1 equals zero at the 1% significance level).

Results in Column 2 moreover show that the effect of "being in 2011", as measured through the variable T, is zero for individuals who participated in the AES ($0.21 - 0.22 \approx 0$), while it is significantly different from zero and positive for individuals who did not participate in the AES ($0.21 \neq 0$).

[6]Results of the regression are available from the authors upon request.

[7]Compared to Panel C, Panel D includes a larger number of individuals but excludes farmers engaged in organic farming schemes

[8]As previously, we assume that SUTVA holds, i.e. that farmers who engaged in 2012 are considered as untreated in 2012.

In other words, the mean value of the TFI in the control group was significantly higher in 2011 than in 2012, which explains why the impact in 2011 appears larger than the impact in 2012. This result is in line with rainfall data for these years, which indicates higher weed pressure in 2011 (Direction régionale de l'Environnement, de l'Aménagement et du Logement, 2011, 2012). Columns (3)-(4) display results from a sample that excludes farmers engaged in organic farming schemes. The results hold in this case, as well.

5.2 Additional and windfall effects of the "zero herbicide between the rows" option

Next, we turn to the impact analysis of one specific AES, the so-called "zero herbicide between the rows" option, for which the sample size is large enough. Results are displayed in Columns (5)-(6) of Table 5. This AES option is interesting because it is both the most often chosen as well as the least stringent among the measures that target herbicide use - characteristics which are probably related. Results displayed in Column (5) show that the average effect of this AES, taking all years together, is significantly different from zero at the standard level of significance. The TFI in the treated group was only 0.21 points below the TFI in the control group (0.95), which corresponds to a 22% decrease compared to the counterfactual situation. This result calls for two comments.

Firstly, this impact seems small. Given that farmers who commit to not applying herbicides between vine rows are expected to have a TFI that equals 0.32, i.e. 1/3 of the counterfactual level (0.95/3 = 0.32),[9] one would expect an impact of -0.63 (0.32-0.95=-0.63) rather than our estimated impact of -0.21. Note that this result does not imply that participants in the AES did not meet the contractual commitment. Rather, it suggests that those farmers would not have applied as much herbicides as expected in the absence of the AES. Put differently, in order for the impact of the AES to be $-2/3$ of the counterfactual level of herbicide use, the counterfactual level would have to exceed 2 ($0.75 * 3 = 2.2$). It appears that in the absence of the AES, winegrowers would not have applied such a high level of herbicides. This suggests the presence of some windfall effect.

Secondly, results in Column (6) show that the impact of this AES varies across years (we reject the null hypothesis that β_1 equals zero at the 5% significance level). Specifically, the impact appears significantly different from zero in 2011, but not in 2012. In 2011, the average TFI in the control group was equal to 1.03 ($0.84 + 0.19$), while the average TFI in the treated group was 0.29 points below ($1.03 - 0.29 = 0.74$). The story is slightly different in 2012, when the average TFI in the control group reached 0.84 only (which corresponds with lower weed pressure in 2012 due to a drought in the region), while the average TFI in the treated group was only 0.06 points below the counterfactual level ($0.84 - 0.06 = 0.78$). This result suggests that farmers complied with the contractual commitment during the first year of their agreement (with a TFI close to 0.7) and maintained these practices afterwards, and that the use of herbicides in the control group fluctuated between the two years (with a TFI close to 1 in 2011 and close to 0.8 in 2012). As the mean TFI in the control group approaches the mean TFI in the treated group in 2012, the impact of the AES on herbicide use becomes statistically null.

[9] Public authorities indeed expected that winegrowers would divide the quantity used by 3, as the space between vine rows is roughly twice as wide as the vine row.

5.3 Discussion on spatial variability

As noted in the theoretical framework, the amount of herbicides farmers apply depends not only on individual-specific factors (such as physical and human capital) but also on area-specific factors pertaining to geographical constraints (e.g. soil type, topography, and climatic conditions like temperature and rainfall). For example, the quantity of herbicides used during rainy years is expected to be higher than during dryer years, all other determinants being equal, because of the higher weed pressure that rainfall generates. Our sample includes farmers who are spread over a fairly wide area (Figure 1). Notably, when comparing average levels of TFI among participants and future participants, to some extent we compare farmers located in the east zone of the region under study to farmers located in the west zone. In order to avoid potential bias in the estimate of the impact of the AES that could arise due to spatial variability in geographical characteristics, we perform the estimations by focusing on a subset of farmers who are very close to each other geographically and who constitute a large enough sample to ensure accurate results.[10] In doing so, we drop 23% of the observations from the initial sample. Results are displayed in Table 6. As in Table 5, columns (1)-(2) display results from the entire sample; columns (3)-(4) display results from a sample that excludes farmers engaged in organic farming schemes; and columns (5)-(6) display results from a sample that includes only farmers engaged in the so-called between-the-rows option. Results appear very similar to those obtained from the initial sample. We conclude that the data we use from western areas do not bias the estimates.

5.4 Alternative identification strategy

In relying on the natural experiment assumption, we believe that current participants and future participants did not differ on average in their likelihood to participate in the AES in 2010. Alternatively, if we assume that current participants differ from future participants in some factors X that are observable to us before the AES starts, we can use a quasi-experimental approach in order to estimate the ATT. The idea behind this is the following: if current participants and future participants are similar on average in all of their characteristics (the natural experiment assumption), then they are similar a fortiori in characteristics that are observable to us. Thus, employing a quasi-experimental approach should provide the same results as the simple comparison of TFI between groups.

The validity of matching estimators of the AES impact in 2011 relies on the following assumption:

$$E\left(\mathrm{TFI}^0_{2011}|X_{2010},\mathrm{AES}_{2011}=1\right)=E\left(\mathrm{TFI}^0_{2011}|X_{2010},\mathrm{AES}_{2011}=0\right)$$

In practice, matching estimators eliminate the selection bias caused by observable characteristics X by comparing the TFI of current participants with those of observationally-identical matched future-participants (Imbens, 2004). There are a variety of matching estimators. We use the nearest-neighbor matching estimator (Abadie et al., 2004), the kernel-based matching estimator, and the

[10] Further east, in Gard county, are the territories of Malaven, Camp de Cesar, and Briançon. Farmers in these areas are not separated by more than 40 km.

local linear matching estimator (Leuven and Sianesi, 2003). The general form of the matching estimators is:

$$E(\mathrm{TFI}^1 - \mathrm{TFI}^0|\mathrm{AES}=1) = \frac{1}{n_1}\sum_{i\in I_1\cap S_P}\left(\mathrm{TFI}_i^1 - E(\mathrm{TFI}_i^0|\mathrm{AES}=1, X_i)\right)$$

with

$$E(\mathrm{TFI}_i^0|\mathrm{AES}=1, X_i) = \sum_{j\in I_0}\lambda_{ij}\mathrm{TFI}_j^0$$

where I_1 denotes the group of treated farmers, I_0 denotes the group of untreated farmers, and n_1 is the number of treated farmers in I_1. S_P denotes the common support, the subset of treated farmers for whom the density of observationally-identical untreated farmers is higher than some cut-off level (Todd, 2008).

Matching estimators differ in how matched untreated farmers are selected through the matching procedure. This is driven by the weights λ_{ij} that we assign to potential matches given their characteristics X. The nearest-neighbour matching estimator matches each participating farmer to the one or two closest future-participants (closest in terms of vector X). It is important that the covariates X are not affected by the treatment (Imbens, 2004), which is why we utilize 2010 values from the French Agricultural Census. Moreover, we also apply the matching procedure to the summary statistic $\Pr(\mathrm{AES}_i = 1|X_i)$, also called the propensity score (Rosenbaum and Rubin, 1983). We obtain the individual propensity scores by estimating the probability of participating in the AES, conditional on the control variables X, from a sample that includes participants, future-participants and non-participants of the neighbouring areas (see Section 3). In this model, the dependent variable takes on the value of one when the individual is a participant or future-participant and zero when the individual never participated in the AES. As expected, given their characteristics, participants and future participants have similar likelihoods of participating in the AES in 2010, as shown in Figure 2 and Figure 3. Finally, we use the asymptotically-consistent estimator of the variance of the nearest-neighbour matching estimator provided by Abadie and Imbens (2006), and we implement a bootstrap procedure of 500 repetitions in order to generate an estimator of the variance of the kernel and local linear matching estimators.

Another, computationally easier, way to obtain an estimate of the ATT is to run an ordinary least squares regression of the following model:

$$\mathrm{TFI}_i = \gamma_0 + \gamma_1\mathrm{AES}_i + \gamma_2 X_i + \mu_i$$

where γ_1 is the impact that we seek to estimate. However, in addition to the assumption of linearity, doing so would require supposing that γ_1 is constant across X, meaning that the impact of the AES is the same for all participants. Without any evidence for such an assertion, we present the results of the matching approach, which does not require specifying the functional form of the outcome equation and relaxes the assumption of constant additive treatment effects across individuals. We also present the results of linear regressions as a robustness check.

Table 7 displays the mean level of covariates X from the French Agricultural Census for farmers in each group. Columns 1 and 2 refer to current participants and future participants respectively, who are used for the direct comparison of the mean TFI in 2011. Column 3 refers to the subset of future participants who ended up in the control group following the matching procedure.[11] These figures show that even before we apply the matching procedure, the treated and untreated groups do not differ in most features. This result suggests that there was no selection of a specific type of winegrowers into the first-wave implementation of the AES. Importantly, they did not differ in terms of herbicide use, as the area with zero herbicide use was close to 0.25 in both groups. This result is in line with the natural experiment assumption that both groups should not differ in any dimension before the launch of the program. The matching procedure performed quite well in reducing some differences between covariates through the delimitation of the new control group; although these differences remain statistically significant because of the sample size (36 observations only in each group).

Table 8 gives the estimated ATT in 2011 from the direct comparison between groups (first row of the table) and from the matching estimators. The results appear remarkably stable, with a TFI gap between groups of nearly -0.5 points in all cases. We turn next to the estimated ATT in 2012 (Table 9). Here again our estimates are significant and very similar to our main result, with a TFI gap between groups close to -0.3 points, although this result is slightly more scattered and sometimes less precise. These results provide evidence that our main findings based on the natural experiment assumption do not suffer from any selection biases that are due to observable factors.

5.5 Sensitivity test

Although our empirical strategy is likely to perform well, there remains the possibility of a selection bias due to unobservable factors. Rosenbaum (2002) uses an approach that determines how strongly an unmeasured variable must influence the selection process in order to undermine the results of the matching analysis. Two farmers with the same observed characteristics may differ in the odds of participating in the AES by at most a factor of Γ. We thus search the critical levels of Γ at which the estimated ATT would become insignificant. This search indicates that the critical value for Γ is greater than 3. This means that two farmers who have the same observable characteristics X would have to differ in their odds of program participation by a factor of 3 (200%) in order to render the ATT estimated from the matching procedure insignificant. We can thus conclude that, even though unobservable factors may play a role in the decision to enter the AES, it is very unlikely that they would influence the odds of participation to such a large extent. We are thus confident that our identification strategy performs well.

5.6 What is the impact on water quality?

We estimate that the AES leads to a 0.5 point reduction of the TFI in 2011, which means that the participating farmers applied about half the quantity of herbicides that they would have applied in the absence of the scheme. Such an evaluation is an important step toward the assessment of the cost-effectiveness of the AES in improving water quality in French watersheds. Because we do not know the proportion of winegrowers who participate in the scheme in each watershed, the

[11] Because the number of current participants is quite small, we did not apply the common support procedure, which would have further reduced the sample size.

quantity of herbicides used by all non-participants in each watershed, the exact location of each participant, nor the contribution of their lands to global water pollution levels, we are not able to conduct a complete cost-benefit analysis of the studied AES. Nonetheless, we are able to provide some insights regarding the impact of the AES on water quality through some illustrative numbers.

Let us focus on a specific, commonly used herbicide, glyphosate, which has a reference dose of 1,440 g/ha. Depending on soil characteristics, 0.1 to 5% of the applied product ends up in surface or groundwater. This means that at least 1.44 g/ha are likely to be carried away by runoff. An estimated 1,500 cubic meters of water exits each hectare per year as runoff.[12] As a result, assuming that polluted water will replace all clean water in the long term, the concentration of glyphosate in the water would be 0.96 μg/l, which is much higher than the legal limit for drinking water (0.5 μg/l).[13] Supposing now that all winegrowers in a watershed chose to participate in the AES and that our estimate of the ATT would be the same for all of them, the concentration of glyphosate in the water would reach only 0.48 μg/l, which is below the legal limit for the level of pesticides in drinking water. Under this scenario, the AES would have reduced the level of herbicides in regional water sources just below the cumulated threshold for drinking water. However, under the assumption that the maximum of 5% of applied herbicides are carried away by runoff, the AES would have lead to a concentration in herbicides in the water exiting the plots equal to 24 μg/l instead of 0.48 μg/l, which is much higher than the legal threshold. These simple calculations suggest that the success of this AES in ensuring water quality is conditional on many factors.

6 Conclusions

The main results of our analysis suggest that the AESs targeting the reduction of herbicide use in French vineyards had a significant impact on participants' practices. We show that the quantity of herbicides used by participants in the AESs in 2012 was around 30% below what they would have used without the program, and that this impact was significantly higher in 2011 (around 50%). These results are robust to various estimators, robustness checks, and a sensitivity analysis.

We moreover show that variation in the impact of the AES over time can be explained by seasonal differences in weed pressure: while participants comply with the contractual commitment during the first year of their agreement and maintained these practices afterwards, winegrowers in the control group adjust the quantity of herbicides they use according to the weed pressure they face in a given year. The mean value of the TFI in the control group is significantly higher in 2011 (a rainy year) than in 2012 (a dry year). Consequently, the ATT in 2011 appears larger than the ATT in 2012.

Analysis of the least demanding but most adopted AES option - "zero herbicide between the vine rows" - also shows that winegrowers who chose this option would not have applied as much herbicide as expected in the absence of the AES. Put differently, in order for the impact of the AES on herbicide use to reach the expected level, the counterfactual TFI would have to exceed 2. It

[12]Runoff can be roughly estimated as the difference between annual rainfalls, approximatively 650 mm in the region under study, and annual evapotranspiration, which is on average 500 mm for vineyards.

[13]The legal limit for the level of pesticides in drinking water is 0.1 μg/l for each molecule of pesticide, with a maximum cumulated level of 0.5 μg/l for all types of pesticides.

appears that even in the absence of the AES, winegrowers would not apply such a high level of herbicides. This suggests the presence of some windfall effect.

Results also indicate that the least stringent AES option ("zero herbicide between the rows") has a significant impact on herbicide use in 2011, but no significant impact in 2012, even though the AESs taken together do have a significant impact in 2012. This suggests that least demanding AES options are effective in avoiding pollution peaks when weed pressure is high (as in 2011), whereas more demanding AES options guarantee an overall reduction in herbicide use even during easy farming years in which less weed pressure is experienced (as in 2012).

Though additional work is necessary to measure the impact of the scheme on overall water quality within the affected watersheds, our analysis constitutes a first step in this direction by showing that, under certain conditions, these AESs are likely to reduce the level of herbicides in water below the cumulated threshold for drinking water. This result is more likely to hold when the quantity of herbicides carried away by runoff is small.

In conclusion, it is worth-mentioning that all of our estimates rely on existing data. Though these data were available to us, their consistency was less than ideal because they were based on sometimes sporadic reports submitted by the local site operators themselves. Future AES evaluation would benefit from more complete data, the collection of which could be facilitated by greater oversight of the reporting process on a national level.

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Table 1: Areas under contract in the sample

AES	2010	2011	2012	2013	Total	%
phyto02	92.1	2.9	12.4	41.1	148.6	4%
phyto04	176.4	3.5	0.0	0.0	179.9	5%
phyto10	562.9	272.3	1840.8	197.7	2873.7	79%
couver03	0.0	0.0	17.8	0.0	17.8	0%
bioconv	119.1	149.2	103.7	0.0	372.0	10%
biomaint	0.0	0.0	37.7	0.0	37.7	1%
Total	950.5	427.9	2012.4	238.9	3629.7	100%

Note: Figures are expressed in hectares. AES phyto02 refers to the suppression of herbicide use; AES phyto04 refers to the reduction in herbicides use by 40% of the regional standard; AES phyto10 refers to the suppression of herbicide use between the vine rows; AES bioconv (resp. biomaint) refers to the conversion to (resp. maintaining of) organic wine growing.

Table 2: AES participation in the sample

AES	2010	2011	2012	2013	Total
phyto02	10	1	2	7	20
phyto04	6	1	0	0	7
phyto10	29	11	77	14	131
couver03	0	0	4	0	4
bioconv	5	7	7	0	19
biomaint	0	0	2	0	2
Total	50	20	92	21	183

Note: AES phyto02 refers to the suppression of herbicide use; AES phyto04 refers to the reduction in herbicides use by 40% of the regional standard; AES phyto10 refers to the suppression of herbicide use between the vine rows; AES bioconv (resp. biomaint) refers to the conversion to (resp. maintaining of) organic wine growing.

Table 3: Determinants of participation in AES

	Odds Ratio	Std. Err.	z	P>z	
Date of birth	1.03	0.01	2.97	0.00	***
Agricultural education: less than baccalaureate (0/1)	2.00	0.48	2.91	0.00	***
Agricultural education: more than baccalaureate (0/1)	2.56	0.87	2.75	0.01	***
Agricultural training: less than baccalaureate (0/1)	2.27	0.65	2.87	0.00	***
Agricultural training: more than baccalaureate (0/1)	2.11	1.21	1.30	0.20	
General education: less than baccalaureate (0/1)	1.31	0.37	0.96	0.34	
General education: more than baccalaureate (0/1)	1.47	0.52	1.09	0.28	
Spouse's main activity: agricultural activity (0/1)	2.38	0.67	3.09	0.00	***
Spouse's main activity: non-agricultural activity (0/1)	1.14	0.29	0.52	0.60	
Spouse's main activity: none (0/1)	1.42	0.44	1.12	0.26	
Vineyard surface area (ha)	1.00	0.00	2.82	0.01	***
Vineyard surface area (%UAA)	1.85	1.26	0.90	0.37	
Labor (annual work unit)	1.00	0.00	-2.22	0.03	**
Production (hl)	1.00	0.00	0.34	0.73	
Surface area without herbicide (%UAA)	0.81	0.23	0.76	0.45	
AOP (%production)	2.90	0.89	3.48	0.00	***
Vinification in particular cellar (%production)	0.68	0.18	-1.45	0.15	
Irrigation (%UAA)	0.84	0.91	-0.16	0.87	
Property (%UAA)	0.43	0.12	-3.09	0.00	***
Slope (degrees)	1.05	0.06	0.84	0.40	

Note: Three asterisks *** (resp. **, *, °) denote rejection of the null hypothesis at the 1% (resp. 5%, 10%, 15%) significance level. The sample includes a total number of 1,562 winegrowers: 139 participants in AES and 1,423 winegrowers who were eligible but did not participate in the AES. We created dummy variables for the categories of the variables "Agricultural education", "Agricultural training", and "General education" (the reference category is "no baccalaureate"). The reference category of the variable "Spouse's main activity" is "no spouse".

Table 4: Average treatment effects in 2011 and 2012

	Mean value						
	Control	Treated	ATT	s.e.	n_0	n_1	N
TFI 2011 - Panel A	0.98	0.48	-0.50***	0.09	83	38	121
TFI 2011 - Panel B	1.04	0.55	-0.49***	0.09	76	31	107
TFI 2012 - Panel C	0.74	0.45	-0.28*	0.16	15	48	63
TFI 2012 - Panel D	0.86	0.62	-0.23**	0.09	83	34	117

Note: Column 1 displays the mean value of TFI in the treated group. Column 2 displays the mean value of TFI in the control group. ATT refers to the Average Treatment Effect; $s.e.$ refers to the standard error; n_0 (resp. n_1) refers to the number of farmers in the control (resp. treated) group. N is the sample size. In Panel A, individuals in the treatment group are those who engaged in 2010 and individuals in the control group are those who engaged in 2012. In Panel B, individuals in the treatment group are those who engaged in 2010 and individuals in the control group are those who engaged in 2012 but farmers engaged in organic farming schemes are excluded. In Panel C, individuals in the treatment group are those who engaged in 2010 or 2011 and individuals in the control group are those who engaged in 2013. In Panel D, individuals in the treatment group are those who engaged in 2010 or 2011 and individuals in the control group are those who engaged in 2012 or 2013. Panel D excludes farmers engaged in organic farming schemes. Three asterisks *** (resp. **, *, °) denote rejection of the null hypothesis at the 1% (resp. 5%, 10%, 15%) significance level.

Table 5: Random-effects regressions

	organic included		organic excluded		between-the-rows	
	(1)	(2)	(3)	(4)	(5)	(6)
AES	-0.40***	-0.28***	-0.38***	-0.25***	-0.21**	-0.06
	0.08	*0.09*	*0.08*	*0.09*	*0.10*	*0.11*
AES*T		-0.22***		-0.25***		-0.29**
		0.08		*0.09*		*0.11*
T		0.21***		0.19***		0.19***
		0.04		*0.05*		*0.05*
cons	0.89***	0.77***	0.95***	0.85***	0.95***	0.84***
	0.05	*0.05*	*0.04*	*0.05*	*0.04*	*0.05*
Nb. Obs.	233	233	204	204	164	164
Nb. Farmers	123	123	108	108	86	86

Note: The variable *AES* takes on value 1 when the farmer is treated and zero elsewhere; the variable *T* takes on value 1 in 2011 and zero in 2012; the variable *AES*T* is an interactive term; *cons* refers to the constant term of the model. In all regressions individuals used as treated are those who engaged in 2010 and individuals used as controls are those who engaged in 2012. Columns (1)-(2) display results from the whole sample. Columns (3)-(4) display results from a sample which excludes farmers engaged in organic farming schemes. Columns (5)-(6) display results from a sample which includes only farmers engaged in the so-called between-the-rows scheme. Standard errors appear in italics below the coefficient estimates. Three asterisks *** (resp. **, *, °) denote rejection of the null hypothesis at the 1% (resp. 5%, 10%, 15%) significance level.

Table 6: Random-effects regressions - Eastern region only

	organic included		organic excluded		between-the-row	
	(1)	(2)	(3)	(4)	(5)	(6)
AES	-0.42***	-0.29***	-0.30***	-0.15°	-0.27**	-0.13
	0.09	*0.10*	*0.09*	*0.11*	*0.11*	*0.13*
AES*2011		-0.23**		-0.29**		-0.26**
		0.10		*0.11*		*0.13*
2011		0.21***		0.19***		0.19***
		0.05		*0.05*		*0.05*
cons	0.93***	0.81***	0.97***	0.87***	0.97***	0.86***
	0.04	*0.05*	*0.04*	*0.05*	*0.04*	*0.05*
Nb. Obs.	193	193	170	170	149	149
Nb. Farmers	100	100	88	88	77	77

Note: The variable *AES* takes on value 1 when the farmer is treated and zero elsewhere; the variable *T* takes on value 1 in 2011 and zero in 2012; the variable *AES*T* is an interactive term; *cons* refers to the constant term of the model. In all regressions individuals used as treated are those who engaged in 2010 and individuals used as controls are those who engaged in 2012. Columns (1)-(2) display results from the whole sample. Columns (3)-(4) display results from a sample which excludes farmers engaged in organic farming schemes. Columns (5)-(6) display results from a sample which includes only farmers engaged in the so-called between-the-rows scheme. Standard errors appear in italics below the coefficient estimates. Three asterisks *** (resp. **, *, °) denote rejection of the null hypothesis at the 1% (resp. 5%, 10%, 15%) significance level.

Table 7: Balancing tests on pre-treatment variables

Variables X	(1) treated	(2) untreated	stat		(3) matched	stat	
Date of birth	1966	1964	1.39		1963	3.12	***
Agricultural education: less than baccalaureate	0.67	0.35	9.89	***	0.53	7.90	**
Agricultural education: more than baccalaureate	0.17	0.26	1.20		0.17	4.00	
Agricultural training: less than baccalaureate	0.06	0.16	2.27		0.19	0.35	
Agricultural training: more than baccalaureate	0.03	0.04	0.09		0.03	0.00	
General education: less than baccalaureate	0.53	0.71	3.78	*	0.61	11.76	***
General education: more than baccalaureate	0.25	0.16	1.44		0.13	5.14	*
Spouse's main activity: agricultural activity	0.42	0.17	8.08	***	0.19	21.78	***
Spouse's main activity: non-agricultural activity	0.17	0.30	2.24		0.20	4.47	
Spouse's main activity: none	0.06	0.18	3.22	*	0.03	1.01	
Vineyard surface area (ha)	2811	3221	0.98		2592	0.94	
Vineyard surface area (%UAA)	0.91	0.90	0.70		0.93	-0.87	
Labor (annual work unit)	2332	2774	0.91		2103	0.94	
Production (hl)	1103	1246	0.66		941	1.42	
Surface area without herbicide (%UAA)	0.29	0.21	1.00		0.22	1.57	
AOP (%production)	0.76	0.81	0.89		0.83	-1.84	*
Vinification in particular cellar (%production)	0.43	0.22	-2.30	**	0.22	2.71	**
Irrigation (%UAA)	0.02	0.03	0.54		0.01	1.11	
Property (%UAA)	0.38	0.32	0.75		0.40	-0.32	
Slope (degrees)	4.28	3.34	2.67	***	3.47	1.84	*

Note: *stat* is the statistics of the test that tests the null hypothesis that the means for both groups are equal (t-test for continuous variables and chi2 test for categorical variables). Two asterisks ** (resp. *) denote rejection of the null hypothesis at the 1% (resp. 5%) significance level. The sample includes 37 treated who engaged in 2010 and 84 untreated who engaged in 2012 (see Panel A from Table 4). We created dummy variables for the categories of the variables "Agricultural education", "Agricultural training", and "General education" (the reference category is "no baccalaureate"). The reference category of the variable "Spouse's main activity" is "no spouse".

Table 8: Treatment effects in 2011 - Matching estimators

	ATT	s.e.	stat	
OLS	-0.53	0.09	-5.74	***
OLS (X)	-0.52	0.11	-4.86	***
OLS (pscore)	-0.53	0.09	-5.80	***
One Nearest Neighbour (X)	-0.46	0.13	-3.58	***
One Nearest Neighbour (pscore)	-0.53	0.13	-3.93	***
Two Nearest Neighbour (X)	-0.49	0.12	-4.00	***
Two Nearest Neighbour (pscore)	-0.54	0.10	-5.24	***
Kernel regression	-0.52	0.09	-5.51	***
Local linear regression	-0.47	0.14	-3.49	***

Note: ATT refers to the Average Treatment Effect; *s.e.* refers to the standard error; *stat* refers to the test statistic. Three asterisks *** (resp. **, *, °) denote rejection of the null hypothesis ($ATT = 0$) at the 1% (resp. 5%, 10%, 15%) significance level. We use the asymptotically-consistent estimator of the variance of the nearest-neighbor matching estimator and we implement a bootstrap procedure (500 repetitions) to get an estimator of the variance of the kernel and the local linear matching estimators. The sample size is 113, which is smaller than sample size in Table 4. This is because some participants in the AES have not been found in the French Agricultural Census.

Table 9: Treatment effects in 2012 - Matching estimators

	ATT	s.e.	stat	
OLS	-0.23	0.10	-2.45	**
OLS (X)	-0.29	0.12	-2.38	**
OLS (pscore)	-0.22	0.10	-2.29	**
One Nearest Neighbour (X)	-0.22	0.11	-1.88	*
One Nearest Neighbour (pscore)	-0.18	0.11	-1.59	°
Two Nearest Neighbour (X)	-0.29	0.14	-2.10	**
Two Nearest Neighbour (pscore)	-0.19	0.10	-1.87	*
Kernel regression	-0.22	0.09	-2.40	**
Local linear regression	-0.17	0.13	-1.29	

Note: ATT refers to the Average Treatment Effect; *s.e.* refers to the standard error; *stat* refers to the test statistic. Three asterisks *** (resp. **, *, °) denote rejection of the null hypothesis ($ATT = 0$) at the 1% (resp. 5%, 10%, 15%) significance level. We use the asymptotically-consistent estimator of the variance of the nearest-neighbor matching estimator and we implement a bootstrap procedure (500 repetitions) to get an estimator of the variance of the kernel and the local linear matching estimators. The sample size is 108, which is smaller than sample size in Table 4. This is because some participants in the AES have not been found in the French Agricultural Census.

Figure 1: Location of surveyed farmers (areas in blue)

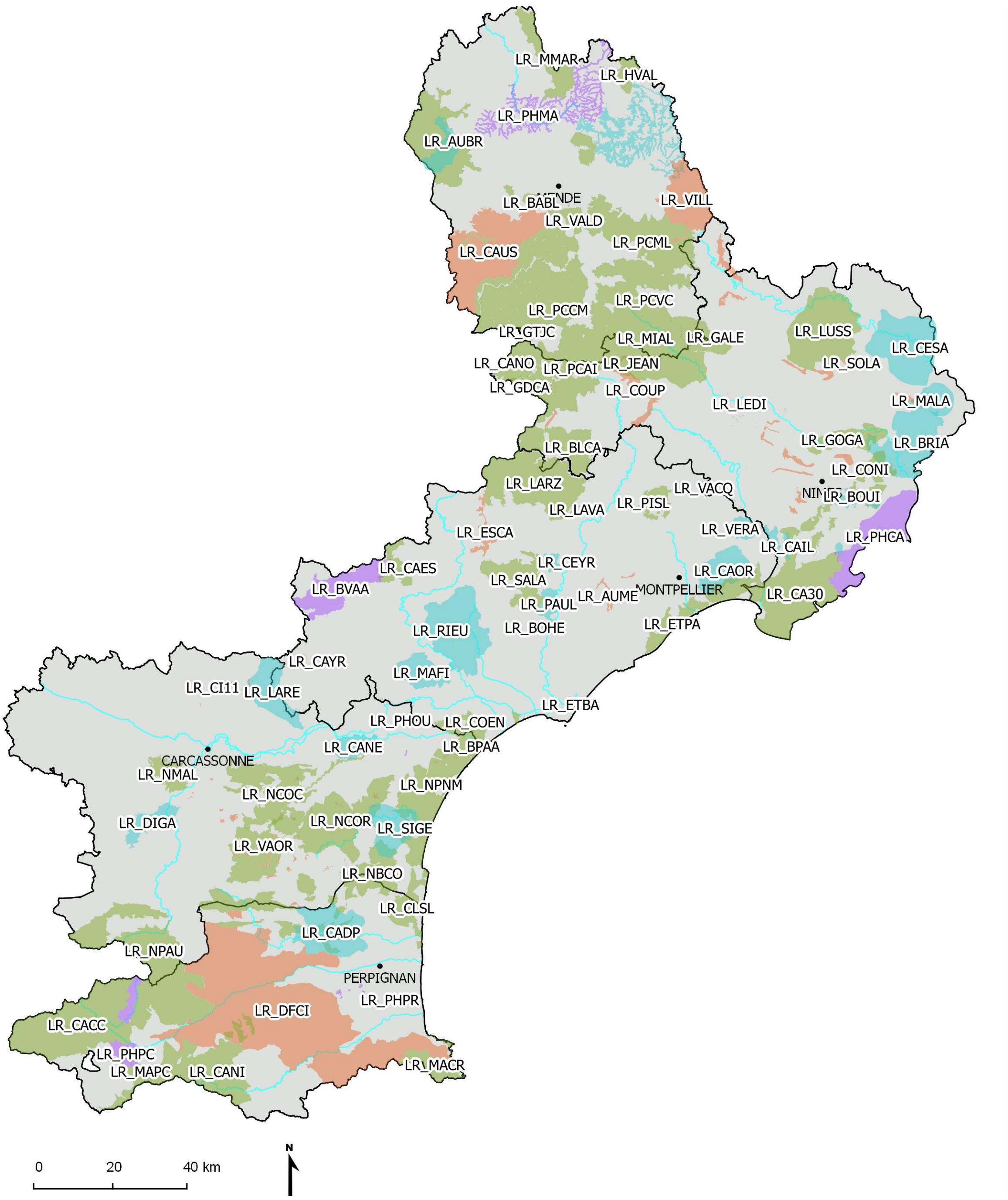


Figure 2: Density of propensity scores by group - ATT in 2011

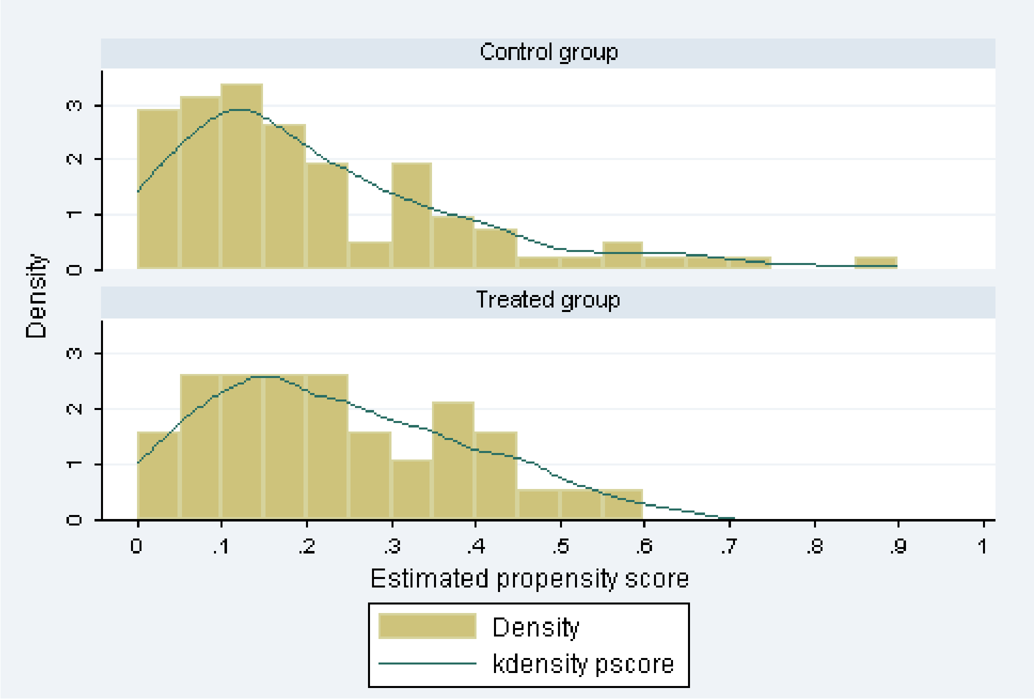
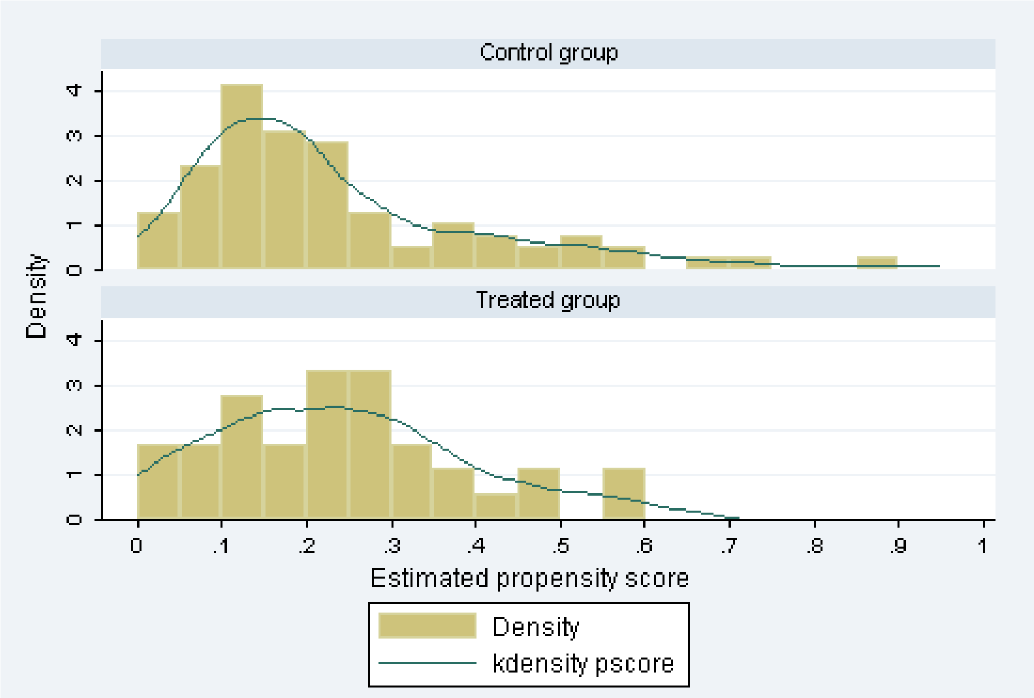


Figure 3: Density of propensity scores by group - ATT in 2012



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