How much green for the buck ? Estimating additional and windfall effects of the french agro-environmental schemes by DID-Matching

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The supplementary material mentioned in the footnotes can be found as a web appendix:

- The list of control variables mentioned in footnote 21 on page 22 can be found at

  `http://web.supagro.inra.fr/partage/subervie/MAE/controls.pdf`

- The results of the probit estimations mentioned in footnote 22 on page 23 can be found at `http://web.supagro.inra.fr/partage/subervie/MAE/probit_results.pdf`

- The figures representing the common support mentioned in footnote 25 on page 24 can be found at `http://web.supagro.inra.fr/partage/subervie/MAE/graphs_common_support.pdf`

- The results of the balancing tests mentioned in footnote 27 on page 25 can be found at `http://web.supagro.inra.fr/partage/subervie/MAE/matching_results.pdf`.

How Much Green for the Buck? Estimating Additional and Windfall Effects of the French Agro-Environmental Schemes by DID-Matching

Abstract

Cost-benefit analysis of agro-environmental schemes (AES) critically hinges on the extent of additional *vs* windfall effects. We use treatment effects methods to estimate these effects. We make three contributions to the literature. First, we derive the statistical assumptions underlying Difference-in-Difference matching as restrictions on an economic model. These restrictions are consistent with what we know about the practical implementation of the program. Second, we present the first disaggregated estimation of the additionality and windfall effects of a nationwide AES program on environmentally-relevant practices for a nationally representative sample of farmers. Third, we test the robustness of our findings confronting them with a credible alternative identification strategy. Our results suggest that voluntary AES programs that seek to reduce nitrogen use and encourage crop diversification may have large windfall effects. In contrast, more ambitious AESs such as conversion to organic farming, which combine strong requirements with large payments, seem to have achieved their goals.

1   Introduction

Payments for environmental services are widely used to improve environmental outcomes. Agro-environmental schemes (AES), consisting in paying farmers for adopting practices more favorable to the environment, are increasingly important components of environmental and agricultural policies both in the US and the EU. Any cost-benefit analysis of these programs critically hinges on being able to measure the increase in the level of practices that has occurred thanks to their implementation (additionality) and the extent to which they subsidize practices that would have been adopted in their absence (windfall effect). The aim of this paper is to estimate the extent of additionality and windfall effects of the French AES program, a major component of the EU program.

Estimating the additionality of an AES amounts to estimating its average treatment effect on the treated (ATT). As Greenstone and Gayer [17] argue, more work applying modern methods of causal inference to environmental economics is needed. The evaluation of the impact of AESs is a case in point. There have been a few empirical studies seeking to uncover the causal effect of AESs on farmers' practices. Early works include Lynch and Liu [34] and Lynch, Gray, and Geoghegan [33], who focus on a very specific US example, looking at the impact of AES on land prices. Pufahl and Weiss [36] apply the same estimator we use in this paper to estimate the causal effect of benefiting from at least one AES on some agricultural practices measured from bookkeeping records, using a non-representative sample of German farms.

We contribute to this literature in three ways. First, contrary to the widespread practice of stating both the evaluation problem and the identification strategy in statistical terms, we frame the assumptions as restrictions on an agricultural household model, emulating Heckman, LaLonde, and Smith [21]'s approach in the case of job training programs. Second, we present the first disaggregated estimation of the effects of a nationwide AES program on environmentally-relevant practices for a nationally representative sample of farmers. Third, we test the robustness of our findings confronting them with a credible alternative identification strategy.

First, framing the identification strategy as restrictions on an economic model pro-

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vides a better understanding of the sources of selection bias and of the implications of the chosen identification strategy. Voluntary participation in an AES generates a selection bias because farmers with environmentally-friendlier practices incur lower costs for complying with AES requirements. Consequently, comparing their practices to those of non-participants would overestimate the effect of the program. The model helps to identify both observed and unobserved determinants of the household's decision to enter the program that are potential sources of bias. We refer to selection on observables and on unobservables, according to the terminology of Heckman and Robb [22]. Moreover, the model highlights the correlation between the size of the treatment effect and selection into the program - a case of essential heterogeneity, according to the terminology of Heckman, Urzua, and Vytlacil [24].

The model also allows for the relevance of the chosen identification strategy to be discussed. We argue that the identification strategy underpinning Difference-In-Difference (DID) matching imposes acceptable restrictions on our theoretical model. DID-matching has been introduced by Heckman, Ichimura, and Todd [20] as an extension to matching that is robust to selection on unobservables. It consists in first-differencing outcomes with respect to a pre-program period, to get rid of selection on unobservables, and in comparing these first-differentiated outcomes for participants to those for observationally identical non-participants, in order to get rid of selection on observables. DID-matching fits neatly with the theoretical model because it imposes credible restrictions on it and accommodates essential heterogeneity. We are indeed able to state the restrictions on the theoretical model that imply the three statistical assumptions necessary for the validity of DID-matching. The usual Stable Unit Treatment Value Assumption (SUTVA), which requires the program not to have any effects on non-participants, implies that the AES does not generate any variation in prices of inputs or agricultural products. This assumption is credible, as the AESs that we study have a low take-up rate and potentially alter the quantities of products whose prices are fixed on world markets. The assumption of conditional independence of increments requires that, in the absence of the program, average variations in practices from pre-program levels be identical among

participants and non-participants. This implies that farmers' participation depends on observed and time-invariant unobserved characteristics only. It can safely be assumed that the time-varying unobserved determinant of practices such as profit and weather shocks are unknown to the farmers when they decide to enter the program because outcomes are observed two to five years after entry. Conditional independence of increments also requires some additive separability in the effect of the general price and unobserved fixed effects. This assumption cannot be justified *a priori* but can be tested with data on two pre-treatment years. The assumption of common support, which requires that there be observationally identical non-participants for each participant, implies the existence of a sufficiently large heterogeneity in the gains associated with participation in the AES program and/or the existence of unobserved variables shifting participatory costs. The disparity in the level of state assistance received by farmers makes this assumption credible. Finally, the model provides guidance for choosing control variables. We argue that potentially observed instruments should not be included in the set of control variables in order to increase the representativeness of the results, and we show some evidence of this phenomenon in the empirical application. We also choose to control for characteristics of the household that affect consumption (e.g. number and age of children) because our theoretical model leaves room to take a taste for working on the farm into account, which implies that production decisions are not separable from consumption decisions.

Second, as a contribution to the empirical literature, we use DID-matching to estimate the average treatment effect of the French AES program, implemented between 2000 and 2007, on the environmentally-relevant agricultural practices of a nationally representative panel of French farmers observed in 2000, 2003 and 2005. We focus on outcomes such as crop diversity, area planted with cover crops to limit nitrogen leakage, area covered by grass buffer-strips and area converted to organic farming.[1] By linking these surveys to the Agricultural Census of 2000, we are able to use a rich list of control variables, including namely factors of production (equipment, buildings, herd, household labor, education, etc.), previous experience with AESs, and quality labels. By linking the farmers' surveys

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[1]In an earlier version of this paper, we presented results on the use of pesticides and nitrogen, but because of the limited sample size, the results lacked precision.

to administrative records, we are also able to single out the effects of various AESs on each agricultural practice, which is particularly useful when there is a need to disentangle individual impacts of AESs with similar objectives.

Our results suggest that voluntary AES programs tend to have large windfall effects because entering an AES is generally less costly for those who choose to do so, and the effect of the program on them is thus lower. For example, we estimate that the AES aiming to plant cover-crops to curb nitrogen leakage increased the areas of cover-crops by 94,000 hectares (ha) in 2005, while more than 200,000 ha of cover-crops were subsidized that year. It thus appears that more than half of the subsidized area benefited from a windfall effect, implying that the estimated cost of this AES per additional hectare (170 €) was twice as high as the cost per subsidized hectare (88 €). We obtain similar results for the AES aimed at increasing crop diversity. We estimate that these measures triggered the planting of .65 to .85 new species on treated farms, but on a very limited share of the total farmland, resulting in a small decrease in the share of the area of farmland covered by the main crop (-3 %), as well as in a slight increase in the crop diversity index. The modest aims of the AES, only requiring farmers to add one crop to the rotation, might explain the very limited effects measured. In contrast, those AESs that combine strong requirements with large payments, such as the one involving a conversion to organic farming, seem to have achieved their goals. According to our estimates, this AES would be responsible for 90 % of the increase in areas converted to organic farming between 2000 and 2005.

Third, we check the robustness of our identification strategy by testing its implications and comparing its results to lower bounds obtained from a credible alternative model of the diffusion of practices. Under the restrictions justifying DID-matching, our model implies that the rate of adoption of practices is the same among participants and observationally identical non-participants. As a "placebo" test, we check whether the difference in the evolution of practices remains the same for both groups between two pre-treatment periods (2000 and 2003). In practice, the interpretation of the results was unfortunately disturbed by the fact that the newly elected government put the French AES program

on hold from early 2003 to late 2004 because of skyrocketing costs. In conjunction with the one-year lag due to the normal administrative process, this means that a household that applied to an AES in early 2002 may have received its first payment in late 2004 or early 2005. This household may nevertheless have altered its practices as early as 2003, because it was uncertain whether it had to comply with the restrictions of the program once its application had been submitted. This anticipation effect may thus lead to spurious rejections of the validity of DID-matching. To cope with this problem, we apply placebo tests to groups of participants entering the program after September 2003 and up to September 2005. The results point towards anticipation effects that generally fade out for participants entering after 2004, thereby giving credit to our identification strategy.

Because results from the placebo tests potentially leave room for time-varying selection on unobservables if anticipation effects are thought to be small, we implement as a last robustness test an alternative identification strategy leading to a lower bound on the treatment effects. The alternative estimator we use is a matching version of the so-called triple-differences (DDD) estimator proposed by Heckman and Hotz [19]. According to this approach, all the differences in practices observed in 2003 between future participants and their matched counterparts are due to time-varying selection bias. As anticipation effects were undoubtedly at play during this period, by doing so we overestimate selection bias and thereby obtain lower bounds on treatment effects. We make the credible assumption that the rate of adoption of new practices is proportional to time, i.e. that the differences observed in 2003 between future participants and their matched counterparts would have increased at the same rate even in the absence of the program. Results of DDD-matching confirm the positive effects of the AES aiming for the planting of cover-crops as well as the limited but positive effects of some AESs aiming for crop diversification, and fail to confirm the large effects of the AES encouraging conversion to organic farming. This result contrasts with the fact that participants in this AES exhibit the highest level of anticipation effects. The lower bound thus fails to be informative but does not rule out a large positive effect.

This paper is organized as follows: the implementation of AES in France is presented

in section 2; the theoretical model and identification strategy are discussed in section 3; the data used in the paper are presented in section 4; results of estimations by DID-matching and robustness checks are presented in section 5; and section 6 concludes.

2   Agro-Environmental Schemes in France

Rural development policies accounted for 22 % of public spending for the Common Agricultural Policy of the European Union in 2006, and AESs accounted for 37 % of rural development spending [36]. In France, these figures are lower (resp. 17 % and 25 %), because of a lower use of these schemes in public policy and historically high levels of direct support.[2] French AESs are nevertheless worth assessing for two reasons: first, their share of total public expenditure on agriculture has steadily increased since 1992, when they were first introduced (for example, public spending for AESs nearly doubled between 1999 and 2006). Second, France being the main beneficiary of agricultural policies in the EU, even a small proportion of the total budget represents a large amount of money. In 2006, 521 million Euros were spent on AESs in France, accounting for roughly 1 % of total CAP expenditures for the EU as a whole [7]. Finally, if AES expenditures in France per hectare of usable agricultural area (UAA) are lower than in most European countries,[3] it is mainly because the area affected by AESs is smaller than in other countries, and not because payments per hectare in an AES are small. Estimating the impact of AESs in France is therefore an important step towards measuring the effectiveness of agro-environmental spending in the EU.

In France, AESs were implemented between 2000 and 2006 as part of the National Plan for Rural Development (*Plan de Développement Rural National* (PDRN)). This plan contained a very thorough description of the different AESs that farmers could apply for, with some adjustments at regional level (mainly on payments, but regional variation of payments remained low [10]). AESs were referred to with a seven digit code: the first two digits referred to the general category of the AES, the following three referred

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[2]According to the French Ministry of Agriculture's website.
[3]According to the European Environment Agency's website.

to the particular requirements the farmer had to meet to enter the AES and, finally, the last two digits referred to the regional variation in the AES. The two-digit codes of particular relevance for our study are AES 02 (diversification of crop rotation), 03 (sowing of cover crops), 04 (planting of grass buffer strips), 08 (reduction in the use of pesticides), 09 (reduction in the use of fertilizers) and 21 (conversion to organic farming). Taken together, these AESs accounted for 22 % of total spending on AES in 2006 in France.[4] We usually stick to the 2-digit level, with the exception of measures 0201A, 0205A and 0301A. Measures 0201A and 0205A both aim to increase the diversity of crop rotation, but the former requires the addition of one crop to the rotation whereas the latter simply requires that at least four different crops be grown on the farm. Among the 03 measures, we focus on those requiring the sowing of cover crops during winter (0301A), since they are the most widely chosen. Measures 0302A and 0303A (respectively replacing spring crops by winter crops and mowing residues) have a very low take-up rate. There is more variation within measures 08 and 09 with respect to the requirements: measures 0801A and 0903A, which have the highest take-up rate within their respective 2-digit categories, have low requirements (mainly recording practices and choosing the frequency of pesticide interventions and the quantity of fertilizer spread with respect to analysis or yield expectation), while measures with more drastic requirements like the 0901A (reduction of 20 % of nitrogen use with respect to the local level) have lower take-up rates.

AESs are five-year contracts, with yearly payments and possible control of how well the requirements are met. The main way for farmers to benefit from an AES during this period was to submit a written application containing an environmental diagnosis of their farm and the particular measures they were applying for. An administrative authority then had to approve or refuse the application. When the application was approved, a contract was signed, stipulating the farmer's commitments and a schedule of annual payments. The time between a farmer's application and the signing of the contract was

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[4]Subsidies for extensive farming of meadows accounted for 60 % of total spending for the AES in France in 2006. As described in Chabé-Ferret and Subervie [11], the methods applied in this paper cannot be used to estimate the impact of these subsidies because most of the eligible population benefits from them, so that they tend to affect non-participants as well, mainly through the land market.

at least a year. In order to submit a valid application, most of the farmers benefited from the assistance of union-run local public administrations called *Chambres départementales d'Agriculture* (CA). The amount of help given to individual farmers by each CA varied widely across France, because right-wing CAs opposed the implementation of these contracts, as they came under a policy introduced by a left-wing government. In 2003, all applications were temporarily frozen by the newly elected government because of an unexpected surge in the number of applications. Contracts were gradually reinstated with an informal restriction on the total payments that an individual farmer could receive. This delay had not been anticipated by farmers who had applied to the AES program; as a result they altered their practices before being recorded as beneficiaries in the administrative files.

3   An economic model to justify DID-matching

This section seeks to state the identification assumptions underlying DID-matching in economic terms. We develop a model of an agricultural household taking part in an AES program and choosing its level of input. Identification assumptions are then presented as restrictions on this model. Contrary to the usual practice of stating statistical identification assumptions unrelated to any theoretical framework, this approach enables a theoretically-grounded definition of potential outcomes, causal effects and selection bias. Moreover, it generates as a by-product the set of candidate control variables. Stating the identification assumptions in economic terms provides a useful basis for discussing their validity by comparing them to what we know about agro-environmental program implementation and input choices. This section is organized as follows. We first state a model of input choice and entry into the AES. We then define the parameter of interest and present the identification assumptions as restrictions on this model. Moreover, we develop tests for the validity of the identification strategy and suggest a credible alternative identification strategy leading to a lower bound on the estimated treatment effects.

3.1   Modeling farmers' participation in an AES

We model a household making two sequential decisions. First, it decides whether or not to enter an AES, knowing the level of payments P it would receive, the type of constraints it would face, and further information, noted $\mathcal{I}$, which is of particular importance for the chosen identification strategy as we show in this section. Second, uncertain outcomes are revealed and the household chooses the level of inputs that maximizes its utility, while having to cope with the AES constraints in the event that it has chosen to enter the scheme. We solve this problem with backward induction, so that we first focus on production decisions both under the AES and outside of it, and then consider the household's decision to enter the scheme.

Input choices with and without the AES

The household produces only one agricultural good, whose price is p^Q, in quantity Q, by combining a variable input Y whose price is p^Y with household labor (H) and other factors of production. These consist of the fixed factors that the household possesses, like physical and human capital and land, stored into the vector $\mathbf{I}$ and unobserved factors like managerial ability, land quality and weather shocks, gathered into the vector $\boldsymbol{\epsilon}$. The production function F is such that: $Q = F(Y, H, \boldsymbol{I}, \boldsymbol{\epsilon})$. Among the unobserved factors $\boldsymbol{\epsilon}$, we distinguish between factors fixed through time (like managerial ability and land quality, noted $\boldsymbol{\mu}$) and those that vary through time (like weather shocks, noted $\boldsymbol{e}$). We thus have $\boldsymbol{\epsilon} = (\boldsymbol{\mu}, \boldsymbol{e})$.[5] When a household has entered an AES ($D = 1$) it receives payments P as a compensation for making a restricted use of inputs Y, so that $Y \leq \bar{Y}$. The household derives income from farming but also from working H^{off} hours off the farm for a wage w. It derives utility from consumption C and leisure L. Since Fall and Magnac [14] have empirically shown that French farmers strongly exhibit a

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[5]Note that we do not need to assume that prices are common to all households. Our settings make it possible for prices to also have household-specific components correlated with entrance into the AES: the fixed-through-time idiosyncratic profit opportunities (specific contract for high-quality crops, for example) can be modeled as components of $\boldsymbol{\mu}$ while the time-varying idiosyncratic profit opportunities are part of $\boldsymbol{e}$. Prices in our model account for general variations in price levels that are common to all households (time effects). We also suppose that households know future prices when entering the AES. This could be relaxed to households forecasting future prices based on current prices, without changing the main results.

particular preference for on-farm work, we add this feature *à la* Lopez [32] to our model, along with the possibility that farmers have a particular distaste for some inputs, due for example to ecological preferences.[6] Heterogeneity of tastes is described by two vectors: $\boldsymbol{S}$, containing observed consumption shifters (family size, age of children, etc.) and $\boldsymbol{\eta}$, which accounts for unobserved taste shifters. Here again we make a distinction between unobserved shifters that are fixed through time (like ecological preferences, taste for work on the farm, noted $\boldsymbol{\delta}$) and time-varying idiosyncratic taste shifters (like non-farm profit opportunities, noted $\boldsymbol{n}$). We thus have $\boldsymbol{\eta} = (\boldsymbol{\delta}, \boldsymbol{n})$.[7] The problem the household faces is:

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

subject to:

$$C = p^Q Q - p^Y Y + w H^{\text{off}} + DP \tag{2}$$

$$Q = F(Y, H, \mathbf{I}, \boldsymbol{\epsilon}) \tag{3}$$

$$D(Y - \bar{Y}) \leq 0 \tag{4}$$

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

where T is the total time available to the household.

The first order condition for the input level is the following (with λ^Y the Lagrange multiplier associated to the input constraint):[8]

$$\frac{\partial U}{\partial C}\left(p^Q \frac{\partial F}{\partial Y} - p^Y\right) + \frac{\partial U}{\partial Y} - \lambda^Y D = 0. \tag{6}$$

From Equation (6) we can define the so-called individual causal effect, which is the basis of our evaluation problem. Without the AES (i.e. when $D = 0$ in equation (6)), the household chooses the input level Y^0 that equalizes the marginal increase in utility, due to a marginal increase in agricultural profits, with the marginal disutility of using inputs.

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[6]Note that if the household has no particular taste for working on the farm or for using inputs, the production decision is fully separable from the consumption decision [42], a special case nested in our model.

[7]Factors stored in $\boldsymbol{n}$ can also reflect idiosyncratic variations in the wage or in the unemployment probability.

[8]A similar condition holds for labor on the farm and leisure.

This level depends on all the exogenous variables of the problem, including the household characteristics $\boldsymbol{S}$ and $\boldsymbol{\eta}$, as production decisions are not separable from consumption:[9]

$$Y^0 = g_0(p^Q, p^Y, w, T, \boldsymbol{I}, \boldsymbol{S}, \boldsymbol{\epsilon}, \boldsymbol{\eta}). \tag{7}$$

In an AES (i.e. when $D = 1$ in equation (6)), either the input constraint is binding, so that $Y^1 = \bar{Y}$, or the input constraint is not binding ($\lambda^Y = 0$), and $Y^1 \leq \bar{Y}$. Generally, we have:

$$Y^1 = g_1(P, \bar{Y}, p^Q, p^Y, w, T, \boldsymbol{I}, \boldsymbol{S}, \boldsymbol{\epsilon}, \boldsymbol{\eta}). \tag{8}$$

Note that the potential outcomes Y^1 and Y^0 have an economic meaning here: they are input demands and the set of variables that determine their level is known *a priori*.

The individual-level causal effect of the AES (Δ_Y) is the difference between the input level chosen by the household if it enters the AES and the input level it chooses if it does not enter the AES: $\Delta_Y = Y^1 - Y^0$. The observed input choice Y depends on whether or not the farmer has entered the AES: $Y = Y^1 D + Y^0(1 - D)$. The individual-level causal effect of the AES is thus not observable, since only one of the two potential input choices is observed. This is an instance of the fundamental problem of causal inference [27]. Because of this problem of missing data, researchers usually try to recover some averages of treatment effects on various subpopulations, such as the average treatment effect on the treated (ATT), which is the average effect of the AES on those who have chosen to enter it: $ATT = \mathbb{E}\left[Y^1 - Y^0 | D = 1\right]$. The value of this parameter is the one we try to recover here.

At this stage, it is worth mentioning that the theoretical model also allows for the various scenarios for the expected causal effect to be reviewed, by considering whether the input constraint is binding or not. Indeed, constrained households (for which $\lambda^Y > 0$) have to decrease their level of inputs in order to cope with the AES constraints. Thus for these households, we will have $\Delta_Y < 0$. Unconstrained households (for which $\lambda^Y = 0$)

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[9]This equation is a solution to the set of first-order conditions of the household's problem, including those related to labor that are not shown here. We assume properties of the problem so that such a solution exists.

could enter the AES at no cost, i.e. without modifying their agricultural practices. This is the case of households that do not have a specific taste for working on the farm or for using inputs. Thus for these households, we will have $\Delta_Y = 0$ meaning that the program has no effect. In other words, these households benefit from a pure windfall effect, as they receive a subsidy but do not change their practices at all. Unconstrained households may also have a particular taste for working on the farm or for using inputs. They do change their practices when they have opted for an AES, because of an income effect due to the monetary transfer. For these particular households, the sign of Δ_Y is unknown *a priori*.[10]

The sign and magnitude of the ATT will depend on the relative proportions of constrained and unconstrained households in the pool of participants. Note that, as constrained households bear a larger cost of entry than unconstrained households, the latter are likely to be more represented in the pool of participants than in the whole population. It is thus unsure whether the ATT is strictly positive. In the extreme case of a program attracting only unconstrained households, the ATT may be null.

Farmers' decision to enter the AES

We note V^1 and V^0 the utility of the household when it is respectively in or out of the AES program. V_1 and V_0 are the indirect utility functions defined by equations (1), (2), (3), (4) and (5). They depend on the same variables as Y^1 and Y^0. We note V the disutility of applying to the AES program. It depends on the time spent preparing the application, which may vary depending on the level of education, participation in past programs and possible assistance provided by agricultural unions. The household decides to enter the AES only if the expected utility gain is higher than the application costs:

$$D = \mathbb{1}\left[\mathbb{E}\left[V_1 - V_0 | \mathcal{I}\right] - V \geq 0\right], \tag{9}$$

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[10]For these households, the budget constraint (2) is altered when the household enters the program. The marginal utilities of consumption, leisure, work on the farm and input use move in an *a priori* unknown direction. Depending on whether labor on the farm and input use are inferior or normal goods, and on the complementarity between labor on the farm and input use, the effect on these households of entering the AES may go either way.

where $\mathcal{I}$ denotes the information set of the agents when deciding whether to participate in the AES or not. As the household has some information on the actual determinants of V_1 and V_0 when deciding to participate in the AES, a comparison of the input demand of participants with that of non-participants would lead to selection bias. Selection bias is due to the fact that some determinants of farmers' participation stored in $\mathcal{I}$ are also determinants of future input demands. Typically, fixed factors of production ($\boldsymbol{I}$), land quality and managerial ability ($\boldsymbol{\mu}$), consumption shifters ($\boldsymbol{S}$) and ecological preferences ($\boldsymbol{\delta}$) are known to the farmers when they decide to enter the AES. Farmers who choose to participate in an AES are thus also more likely to have lower input demands (which is consistent with our interpretation of the various values of λ^Y).

A simple comparison of the practices of participants and non-participants would thus overstate the effects of the program, since in the absence of the program participants would have used less input on average than non-participants:

$$\mathbb{E}\left[Y|D=1\right] - \mathbb{E}\left[Y|D=0\right] = ATT$$
$$+ \underbrace{\mathbb{E}\left[Y^0|\mathbb{E}\left[V_1-V_0|\mathcal{I}\right] \geq V\right] - \mathbb{E}\left[Y^0|\mathbb{E}\left[V_1-V_0|\mathcal{I}\right] < V\right]}_{\text{selection bias}}. \quad (10)$$

In order to control accurately for selection bias, all the factors mentioned above must be taken into account when choosing the identification strategy. This theoretical framework further shows that additional assumptions need to be made concerning time-varying idiosyncratic taste shifters like non-farm profit opportunities ($\boldsymbol{n}$) and transitory components like weather shocks ($\boldsymbol{e}$).

3.2   Identification assumptions as restrictions on an economic model

In this section, we derive the assumptions needed to identify ATT as restrictions on the economic model using DID-matching, as described in the previous section. We discuss the validity of these conditions and offer ways to test their implications. To enable a comparison with traditional statistical assumptions, this section is structured along the usual statistical framework: we first deal with the Stable Unit Treatment Value Assump-

tion (SUTVA), then with the assumption of conditional independence of increments and, finally, with the common support assumption.

The Stable Unit Treatment Value Assumption (SUTVA)

Rubin [38]'s Stable Unit Treatment Value Assumption (SUTVA) restricts the impact of the program on non-participants to null. It requires that, irrespective of how the treatment (here, the input constraint and associated payment) is allocated among farmers, each farmer's input level does not depend on whether the other farmers are being treated. In our model, this is implied by the following restriction:

Assumption 1. *The level of prices* (p^Q, p^Y, w)*, the distribution of observed and unobserved determinants of input use* $(T, \boldsymbol{I}, \boldsymbol{S}, \boldsymbol{\epsilon}, \boldsymbol{\eta})$ *and the function* g_0 *remain the same whether the AES is implemented or not.*

This assumption implies that observed and unobserved fixed production factors are the same whether the AES is implemented or not, thus ruling out anticipation effects. It also requires the absence of imitation effects or diffusion of practices, as the function g^0 does not depend on other farmers' practices. It also requires the AES not to have any effects on input and output prices. This assumption is far more likely to hold for AESs with an associated low take-up rate, and if prices of inputs and outputs are determined on a large market. The AESs that we study in this paper fall into this category. As a matter of fact, AESs requiring reduced input use are mainly chosen by cereal growers, with a low take-up rate in this population. Moreover, the price of pesticides, fertilizers and cereals are mainly determined on the world market.[11,12]

Assumption 1 implies that the effect of implementing the voluntary AES on those who have not entered is null. Under this assumption, ATT is thus the policy-relevant

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[11]By contrast, measures favoring extensive management of meadows are chosen by almost the entire eligible population, and the price of land is largely determined at a local level. Being able to consider the impact of different measures separately enables us to focus only on the measures for which assumption 1 is most likely to hold.

[12]Assumption 1 would also be far less likely to hold if we were to compare the no-AES situation to a situation where the AES would have been extended to every farmer. The difference between these two situations is measured by the Average Treatment Effect (ATE). This is also why we focus on the ATT.

parameter that enables us to compare the agricultural practices observed after the program has been implemented to a counterfactual situation where the AES program would not have existed and would not have been replaced by any other program of the same type [23], thereby estimating the level of additionality of the AES program.

The assumption of conditional independence of increments

The crucial identification assumption in DID-matching is the conditional independence of increments [20, 1, 36]. It states that the average increment in input use relative to the pre-program period among participants is equal to the average increment in input use among observationally equivalent non-participants. In our economic model, the validity of the assumption of conditional independence of increments requires the three following restrictions to hold simultaneously:

Assumption 2. *The three following conditions must hold simultaneously:*

(i) $\mathcal{I} = \left\{ P, \bar{Y}, p^Q, p^Y, w, T, \boldsymbol{I}, \boldsymbol{S}, \boldsymbol{\mu}, \boldsymbol{\delta} \right\}$,

(ii) $(V, \boldsymbol{\mu}, \boldsymbol{\delta}) \perp\!\!\!\perp (\boldsymbol{e}, \boldsymbol{n}) \,|\, (T, \boldsymbol{I}, \boldsymbol{S})$ *and* $(\boldsymbol{e}, \boldsymbol{n}) \,|\, (T, \boldsymbol{I}, \boldsymbol{S})$ *i.i.d,*

(iii) $Y^0 = l_0(T, \boldsymbol{I}, \boldsymbol{S}, \boldsymbol{\mu}, \boldsymbol{\delta}, \boldsymbol{e}, \boldsymbol{n}) + m_0(p^Q, p^Y, w, T, \boldsymbol{I}, \boldsymbol{S}, \boldsymbol{e}, \boldsymbol{n})$, *for some functions* l_0 *and* m_0.

Part (i) of assumption 2 states that a farmer's decision to enter an AES depends on prices (p^Q, p^Y, w), the level of the input constraint $\bar{Y}$ and of the associated payment P, the time dotation T, the level of fixed factors of production $\boldsymbol{I}$, the level of consumption shifters $\boldsymbol{S}$, and the level of unobserved factors fixed over time $\boldsymbol{\mu}$ and $\boldsymbol{\delta}$. However it does not depend on time-varying unobserved factors $\boldsymbol{e}$ (weather shocks) and $\boldsymbol{n}$ (idiosyncratic wage shocks). This ensures that selection for the program is based either on observed variables or on unobserved variables fixed through time. This assumption is realistic because participation in AESs is decided two to five years before practices are observed. This lag between entry into the program and the decision about input use means that transitory determinants of input use $\boldsymbol{e}$ and $\boldsymbol{n}$ cannot be forecasted at the time when the decision to enter the program is made.

Part (ii) of assumption 2 implies that the disutility of applying for an AES is independent of idiosyncratic unobserved shocks conditional on observed covariates, so that all the dependence between V and Y^0 is due either to observed covariates or to unobserved time-constant shifters ($\boldsymbol{\mu}$ and $\boldsymbol{\delta}$). It also means that transitory productivity shocks cannot be correlated to long-term determinants of productivity or tastes. Such assumptions can reasonably hold, as knowing the long-term mean climate does not help to forecast the climatic anomalies around this long run level for a given year. Finally, part (ii) also requires time-varying idiosyncratic shocks not to be auto-correlated. This could be restrictive with regard to other time-varying idiosyncratic profit shocks, such as idiosyncratic wage or price shocks. However, as we control for non-agricultural activities or specific contracts for quality products, this assumption seems reasonable.

Parts (i) and (ii) imply that the household can act upon information unobserved by us ($\boldsymbol{\mu}$ and $\boldsymbol{\delta}$). Participants and non-participants with the same value for the observed variables (T, $\boldsymbol{I}$, $\boldsymbol{S}$) may thus differ in unobserved dimensions, which results in selection bias even when conditioning on observed covariates, *i.e.* selection on unobservables [22]. Part (iii) of assumption 2 is a way to deal with this bias. It requires that the effect of the unobserved time-constant shifters on input demand be additively separable from the effect of time-varying covariates (e.g. prices). Observationally identical households must thus respond identically to variations in prices, even if they differ in unobserved dimensions. As a consequence, the average difference in practices between participants and observationally identical non-participants must be constant through time. Non-participants may nevertheless adopt practices more favorable to the environment because of changes in prices or in other policies.[13] Under assumption 2, we have:

$$\mathbb{E}[Y_{it}^0|D_i = 1, T_i, \boldsymbol{I}_i, \boldsymbol{S}_i] - \mathbb{E}\left[Y_{it}^0|D_i = 0, T_i, \boldsymbol{I}_i, \boldsymbol{S}_i\right]$$

$$= \mathbb{E}\left[l_0(T_i, \boldsymbol{I}_i, \boldsymbol{S}_i, \boldsymbol{\mu}_i, \boldsymbol{\delta}_i, \boldsymbol{e}_{it}, \boldsymbol{n}_{it})|D_i = 1, T_i, \boldsymbol{I}_i, \boldsymbol{S}_i\right]$$

$$- \mathbb{E}\left[l_0(T_i, \boldsymbol{I}_i, \boldsymbol{S}_i, \boldsymbol{\mu}_i, \boldsymbol{\delta}_i, \boldsymbol{e}_{it}, \boldsymbol{n}_{it})|D_i = 0, T_i, \boldsymbol{I}_i, \boldsymbol{S}_i\right] \tag{11}$$

$$= \mathbb{E}\left[Y_{it'}^0|D_i = 1, T_i, \boldsymbol{I}_i, \boldsymbol{S}_i\right] - \mathbb{E}\left[Y_{it'}^0|D_i = 0, T_i, \boldsymbol{I}_i, \boldsymbol{S}_i\right], \tag{12}$$

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[13]The conditionality of direct subsidies to the implantation of grass buffer-strips is a case in point.

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where t' refers to a pre-treatment period. The first equality is a consequence of parts (i) and (ii) of assumption 2 ($(\boldsymbol{e}_{it}, \boldsymbol{n}_{it})$ do not depend on the decision to participate). The second equality stems from the fact that $(\boldsymbol{e}_{it}, \boldsymbol{n}_{it})$ are i.i.d., so that their distribution at period t can be replaced by their distribution at period t'. Note that by rearranging equation (12), we get the standard assumption of conditional independence of increments, which is commonly used when applying DID-matching estimators: $\mathbb{E}\left[Y_{it}^0 - Y_{it'}^0 | D_i = 1, T_i, \boldsymbol{I}_i, \boldsymbol{S}_i\right] = \mathbb{E}\left[Y_{it}^0 - Y_{it'}^0 | D_i = 0, T_i, \boldsymbol{I}_i, \boldsymbol{S}_i\right]$. Though it seems difficult to justify on theoretical grounds, assumption 2 is fortunately testable. We use placebo test that consists in applying the identification strategy in pre-treatment years, where no effect should be detected.[14]

Assumption 2 also implies that the rates of adoption of practices are the same for the participants and their observationally identical counterparts. A reasonable alternative assumption would therefore imply that participants adopt practices at a quicker pace than non-participants. This amounts to replacing part (iii) of assumption 2 by $Y_{it}^0 = l_0(T_i, \boldsymbol{I}_i, \boldsymbol{S}_i, \boldsymbol{\mu}_i, \boldsymbol{\delta}_i, \boldsymbol{e}_{it}, \boldsymbol{n}_{it}) + m_0(p_t^Q, p_t^Y, w_t, T_i, \boldsymbol{I}_i, \boldsymbol{S}_i, \boldsymbol{e}_{it}, \boldsymbol{n}_{it}) + t k_0(T_i, \boldsymbol{I}_i, \boldsymbol{S}_i, \boldsymbol{\mu}_i, \boldsymbol{\delta}_i, \boldsymbol{e}_{it}, \boldsymbol{n}_{it})$, for functions l_0, m_0 and k_0. Under assumption 2, the matching version of the triple-differences (DDD) estimator of Heckman and Hotz [19] offers an unbiased estimation of the treatment effect:

$$\mathbb{E}\left[\frac{Y_{it}^0 - Y_{it'}^0}{t - t'} - \frac{Y_{it}^0 - Y_{it''}^0}{t - t''} | D_i = 1, T_i, \boldsymbol{I}_i, \boldsymbol{S}_i\right]$$
$$= \mathbb{E}\left[\frac{Y_{it}^0 - Y_{it'}^0}{t - t'} - \frac{Y_{it}^0 - Y_{it''}^0}{t - t''} | D_i = 0, T_i, \boldsymbol{I}_i, \boldsymbol{S}_i\right], \quad (13)$$

with t' and t'' two pre-treatment periods. We implement this estimator as an additional robustness check.

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[14]Placebo tests were first implemented by Heckman and Hotz [19] in the context of the evaluation of job training programs. They have become widely-used robustness tests for the validity of a DID design (see for example Duflo [13]) and part of what Angrist and Krueger [8] call refutability tests.

The common support assumption

Finally, in order to apply the DID-matching estimator, non-participants with the same observed characteristics T, $\boldsymbol{I}$ and $\boldsymbol{S}$ as participants must exist, meaning that the probability of not entering an AES must be strictly positive for all values of the observed characteristics. From the theoretical model we can write a sufficient condition for it:

Assumption 3. $\Pr(V > \mathbb{E}\left[V_1 - V_0|\mathcal{I}\right]|T, \boldsymbol{I}, \boldsymbol{S}) > 0$.

Assumption 3 states that there is a non-null probability that participation costs are higher than the expected utility of entering the AES program, for each level of the observed variables. When this assumption is not fulfilled for every value of $\boldsymbol{I}$ and $\boldsymbol{S}$, the set of values of these variables for which it is satisfied is called the zone of common support [20]. Two features of the model ensure that the zone of common support is wide enough. First, among households with the same expected utility gain from entering the AES, some have relatively higher participation costs V because of relatively less substantial assistance from public administrations at the local level. This means that variations in participation costs ensure that non-participants will exist, even among households with high gains associated with participation. V acts thus as an unobserved instrumental variable: it determines treatment intake but is uncorrelated to time-varying determinants of potential outcomes.[15] Second, among observationally identical households incurring the same costs for entering the program (V), variations in unobserved profit shifters $\boldsymbol{\mu}$ and $\boldsymbol{\delta}$ ensure that non-participants will exist, even among people incurring low costs for entering the program.[16] In a DID-matching design, the existence of an unobserved instrumental variable is thus not mandatory for the identification of an ATT. But if such an instrument exists, it should be kept unobserved in order to extend the width of the zone of common support.

To provide some evidence of the importance of keeping potential instruments unobserved, we study how the width of the zone of common support varies, depending on

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[15]Note that in our framework, we do not need V to be independent of (μ, δ): it is thus not an instrument for the level of agricultural practices but for their increments.

[16]This is not the case in a simple matching design: the existence of a common support in that case relies only on the existence of an unobserved instrument independent of (μ, δ).

whether we control for a candidate instrumental variable or not. This candidate instrumental variable is directly related to the institutional features of the implementation of the AES program in France. In each of the 95 French *départements*, there exists a *Chambre d'Agriculture* (CA) representing local farmers' unions. One of the missions of the CAs is to provide assistance to farmers willing to enter an AES. For political reasons, some CAs have chosen to support the AES program while others have not. This has resulted in wide variations in the cost of applying to the AES program over the period studied, which have translated into different take-up rates across *départements*. The main reasons for CA motivation relate to the relative political influence of cattle and crop farmers at the *département* level [9].

As a conclusion to this section, under assumptions 1, 2 and 3, DID-matching identifies the average effect of the treatment on the treated (*ATT*):

$$ATT = \mathbb{E}\left[\mathbb{E}\left[Y_{it}^0 - Y_{it'}^0 | D_i = 1, T_i, \boldsymbol{I}_i, \boldsymbol{S}_i\right] - \mathbb{E}\left[Y_{it}^0 - Y_{it'}^0 | D_i = 0, T_i, \boldsymbol{I}_i, \boldsymbol{S}_i\right]\right]. \qquad (14)$$

Note finally that the assumptions made so far allow for individual treatment effects to be correlated to participation in the program, i.e. for what Heckman, Urzua, and Vytlacil [24] call essential heterogeneity. The fact that DID-matching is robust to essential heterogeneity has not previously been emphasized in the literature.[17]

4 Data

The empirical analysis is based on a longitudinal data set constructed from a statistical survey on agricultural practices conducted in 2003 and 2005 by the statistical services of

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[17]This is why we choose DID-matching instead of an instrumental variables approach. In our model participating in an AES has heterogeneous effects, given observed covariates, and farmers act upon this unobserved (to us) information. Imbens and Angrist [29] have shown that in this case, using instrumental variables to estimate treatment effects does not recover *ATT*, our parameter of interest, but an average of causal effects on the subpopulation of households that would change their participation decision if they faced the administrative procedures of another *département*. Such a parameter is called a local average treatment effect (LATE), and does not answer the policy-relevant question we are trying to solve in this paper. Heckman and Vytlacil [25, 26] propose an estimator of *ATT* by means of local instrumental variables in the case where the instrument is continuous. We cannot use this approach here because our candidate instrument is discrete.

the French ministry of Agriculture (named "STRU" [18]) paired to both the 2000 Census of Agriculture ("CA-2000") and several administrative files recording information on the participation in the AES between 2000 and 2006. The data in "STRU-2005" are used to measure post-treatment outcomes, those in "CA-2000" are used to build both pre-treatment outcomes and control variables, and the data in "STRU-2003" serves for the robustness tests. This is an original database built especially for this work. Its construction involved a pairing procedure based on several steps because of the scattering of data. The sample extracted from "STRU" is representative of French farmers.

4.1 Definition of the participation variables

For each AES, participation is a binary variable taking a value of one when the surveyed farmer appears in administrative files as receiving subsidies compensating him for coping with the requirements of the AES between 2001 and 2005, and a value of zero when the surveyed farmer does not appear in the administrative between 2000 and 2005. The few farmers receiving an AES before 2001 are excluded from the sample, because no pre-treatment observation exists for them. Because farmers may benefit from several AES, the participation variables partially overlap. This is generally not a problem because the AES that are correlated with each other aim at influencing different practices. When two AES may have an impact on the same outcome variable, we study their effect separately by focusing on the sets of participants that only benefit from each one of them. Table 1 reports the sample size and the number of participants for the AES we study in this paper. The sample contains between 400 to 3,000 participants depending on the AES, which represents between 2,000 and 14,000 participant farmers nationwide. We also have access to almost 60,000 non-participants, representing 540,000 farmers nationwide.

4.2 Definition of the outcome variables

The average treatment effect on the treated is estimated for five AES. Several outcome variables are associated with each AES. Two outcome variables allow us to estimate

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[18]The extensive name of this survey is: *Enquête sur la Structure des Exploitations.*

Table 1: Samples size and AES participation

AES	Restriction imposed	Treated	CS[a]	Non treated	Sample
0301	Implanting cover crops	1,811	1,617	58,951	60,568
09	Reduction of fertilizer use	3,173	2,824	58,951	61,775
08	Reduction of pesticides use	3,197	2,849	58,951	61,800
04	Implanting grass buffer strips	1,532	1,356	58,951	60,307
0201	Adding one more crop to the rotation	446	382	58,951	59,333
0205	Having at least 4 crops in the rotation	1,844	1,635	58,951	60,586
21	Conversion to organic farming	720	536	58,951	59,487

Notes : (a) CS refers to the estimated number of treated on the common support, i.e. effectively used in the estimations. Details of its calculation can be found in appendix B.

the impact of the measures 03 and 04 which aim at reducing nitrogen carrying by rain drainage: the land area dedicated to cover crops for soil nitrate recovery and the length of fertilizer-free grass buffer strips located at the edge of agricultural fields which attenuate nitrate lixiviation. As cover crops may be a way to retain nitrogen during winter, we study whether farmers participating in AES 09 aimed at curbing the use of nitrogen fertilizers have an increased use of cover-crops, even when they are not participating in AES 03. The impact of the AES 02 encouraging crop diversification is measured on three outcome variables: the proportion of the total land area dedicated to the main crop, the number of crops, and a crop diversity index.[19] Finally, we use two outcome variables to estimate the impact of the measures, which aim at encouraging conversion to organic agriculture: the land area dedicated to organic farming and the land area under conversion. All areas are measured in hectares. Pre-treatment outcomes are extracted from "CA-2000" and "STRU-2003", the main exceptions being the area cultivated under organic farming and the area covered by grass buffer-strips. The former has not been measured in 2000 while the latter has only been measured in 2005. As a consequence, the effect of AES 04 and 21 on these two variables is estimated by simple matching. Validity of treatment effect estimates for these two AES thus relies on the assumption of no selection on unobservables. This is likely to be a minor problem because the eligibility to the AES 21 was conditional on not having any area cultivated under organic farming in

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[19]We use a regularity index, which is an evenness measure of crop diversity, independent of the number of crops and dependent solely on the distribution of land area among the crops.

2000, so that non-participants had higher areas under organic farming in 2000: matching gives thus a lower bound on the effect of the treatment. We perform a placebo test of this assumption by applying the identification strategy in the pre-treatment year 2003.

4.3 Definition of control variables

Crucial for the relevance of both matching and DID-matching identification strategies is the set of pre-treatment observed variables we use to select non-participants observationally identical to participants. The richness of the information in our database enables us to control for most of the important determinants of input choices and selection into the program listed in our theoretical model. On the production side, we have access to a very detailed description of the equipment (tractors, harvesters, etc.), buildings, herd size and composition, land area, slope, altitude and type of land at the level of the *commune*[20] (Jones et al. 2005, Metzger 2005, Hazeu 2006), size of the labor force, age and education level of farm associates, etc. On the consumption side, we have data on the composition of the household, the main non-farm activity of the farmer and his spouse, etc. The dataset also includes measures of technical orientation of the farm, labels of quality, past experience with the previous AES (1993-1999) and other agricultural policies.[21] The main unobserved variables are thus managerial ability, ecological preferences and prices.

5 Results

In this section, we first present the practical implementation of DID-matching, and then present and discuss the results of this estimation procedure. We finally present the results of the robustness checks based on placebo tests and DDD estimates.

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[20]A French *commune* is roughly equivalent to a US county. There are 36,000 *communes* in France.

[21]The extensive list of the variables is not presented but can be found on the web appendix.

5.1 Practical implementation of DID-matching, with an emphasis on the role of unobserved instruments

The procedure we use is in line with the most recent developments in the literature on program evaluation as they are presented in Todd [43]. As they are not a genuine contribution of this paper, the econometric methods used are presented in appendix B. The first step of the estimation procedure is an estimation of a probit participation model for each AES, where control variables are included as explanatory variables.[22,23] We generally find that participants are indeed different from non-participants: they are younger, more educated, work longer hours on larger farms, and are more likely to have had a previous experience with an AES. Whereas previous experience with quality labels tend to increase participation in AES 21, technical orientation toward growing cereals increases participation in all the AESs studied in this paper except AES 21. Overall, these results suggest an important selection on observables and are coherent with previous empirical studies of the determinants of participation in these AESs [12].

We check whether the political process may translate into disparities in take-up rates at the local level, by testing the joint significance of *départements* dummies in the participation equations. Results of the F-test are displayed in table 2. They show that the null of no-significance can be rejected at the significance level of 1% for each AES. Moreover, the results show that introducing the *départements* dummies increases the predictive power of the probit regression for almost all AES. These results show that the probability of entering an AES varies across *départements*, which corroborates the hypothesis of variable administrative costs.

We also estimate the probability of participating in a given AES, conditional on the control variables (i.e. the propensity score). Following Smith and Todd [41], we define the zone of common support as the set of participants for which there exists a sufficient density

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[22]The extensive results are not presented but they can be found on the web appendix.

[23]As the validity of our estimates depends on our correct specification of the participation model, we test our parametric specifications of against a nonparametric alternative using the specification test proposed by Shaikh, Simonsen, Vytlacil, and Yildiz [40]. Results do not reject the null that the model is correctly specified.

Table 2: Tests of significance of the *départements* into the participation

AES	Sample (1)	CS (1)	Correctly classified (1)	Sample (2)	CS (2)	Correctly classified (2)	F-stat
08	60,665	2,849	86.25	57,726	2,840	87.38	11.16
09	60,383	2,824	85.93	56,106	2,800	86.90	13.47
0201	49,079	382	85.97	30,299	372	85.48	6.03
0205	43,575	1,635	88.37	20,014	1,639	78.17	109.61
0301	58,005	1,617	85.79	40,420	1,601	86.95	9.72
04	54,855	1,356	87.14	49,541	1,347	88.38	9.46
21	53,196	536	89.14	52,097	520	89.55	2.23

Note : CS refers to the number of treated on the common support. Details on its calculation are presented in appendix B. "Correctly classified" refers to the predictive power of the probit regression (expressed in %); (1) (resp. (2)) refers to the probit regression without (resp. with) the *départements* dummies. The F-statistics measure the joint significance of the *départements* dummies.

of non-participants with the same value for the propensity score.[24],[25] As is clearly shown in table 2, including the *départements* dummies in the set of control variables shrinks the size of the zone of common support, limiting by the same amount the representativeness of the estimated treatment effects. It also divides the sample size by half in the case of AES 0201 and 0205 due to the absence of variation of participation status within *départements*. These results therefore strongly suggest that candidate instrumental variables should not be part of the control variable set when implementing DID-matching.

5.2 Average treatment effect on the treated estimated by DID-matching

DID-matching amounts to applying the matching procedure to outcome variables, which are first differentiated. We thus apply various matching methods,[26] which consists in predicting the counterfactual level of outcome of participants, from the level of outcomes of non-participants who have similar levels of the control variables. We assess the quality of the matching procedure by comparing the mean level of the control variables for the participants to that of their matched counterparts. Results show that differences of covariates among participants and non-participants are largely removed, meaning that the

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[24]The definition of the zone of common support is provided in more detail in appendix B.

[25]The graphs presenting the zone of common support for each AES are not shown but they can be found on the web appendix.

[26]See Imbens [28] for a detailed presentation of the various matching methods.

matching can be considered successful.[27] Results from three DID-matching estimators are presented: the nearest-neighbor estimator based on a multivariate matching (NNM[(1)]); the nearest-neighbor estimator based on a univariate matching on the propensity score (NNM[(2)]); and the local linear matching estimator based on the propensity score (LLM). The details of the estimation procedures are presented in appendix B. In cases when all estimators do not lead to the same result, the local linear estimator, known as the most efficient, must be considered first.

Effect of AES aiming at reducing nitrogen carrying

Two AES are likely to affect the land area dedicated to cover crops: the AES 0301, which implies introduction of cover crops in the UAA, and the AES 09, which consists in reducing the quantity of nitrogen fertilizer spread.[28] Many farmers also choose both AES 08 and AES 0301, despite the fact that there is no direct link between the practices associated to each of these AES. As many farmers choose both measures 08 and 09 along with measure 0301,[29] the impact of each AES is estimated separately.[30] Results are presented in table 3. All DID estimators suggest that participants in AES 0301, chosen with or without AES 08 and 09 (rows 1 and 2), have increased their area planted with cover crops, whereas participants in AES 08 and 09 only (without AES 0301) have not. Thus, cover crops are not a way for AES 09 participants to reduce fertilizer quantities. This result underlines the importance of having access to detailed data on specific AES, notably when there is a need to disentangle individual impacts of AES with similar objectives. For AES 0301, the average treatment effect on the treated is around 10 ha. It is different from zero at the 1 per cent level of significance. Table 4 displays the corresponding changes in the outcome variables for the treatment and control groups

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[27]The extensive results of the balancing tests are not presented but they can be found on the web appendix.

[28]One of the primary uses of cover crops is to increase soil fertility. Although the way in which organic nitrogen, captured by cover crops, can be transformed into mineral nitrogen fertilizer is both complex and uncertain, participants in AES 09 may be interested in planting cover crops in order to spread less nitrogen fertilizer after the winter.

[29]Half the beneficiaries of AES 0301 have also chosen AES 09.

[30]This does not need to be done for other AES, as the outcomes considered are not likely to be influenced by more than one AE measure.

(computed using multivariate matching) that lead to this result. Over the 2000-2005 period, cover crop areas of farmers participating in AES 0301 had increased by 13 ha, while cover crop areas of matched non-participants had increased only by 3 ha.

Table 3: Average treatment effect on the treated for AES in 2005 ("STRU-2005")

Outcome	AES	NNM[1]		NNM[2]		LLM	
Cover crops (hectares)	0301	10.66 (0.18)	***	10.62 (0.27)	***	10.66 (1.32)	***
Cover crops (hectares)	w/o 09/08	9.58 (0.29)	***	11.76 (0.53)	***	10.23 (2.35)	***
Cover crops (hectares)	09	3.44 (0.11)	***	3.35 (0.24)	***	3.38 (0.79)	***
Cover crops (hectares)	09 w/o 03	0.60 (0.10)	***	-0.17 (0.21)		0.20 (0.60)	
Cover crops (hectares)	08	3.16 (0.10)	***	2.69 (0.20)	***	3.10 (0.76)	***
Cover crops (hectares)	08 w/o 03	0.04 (0.10)		-0.61 (0.22)	***	-0.01 (0.54)	
Grass Buffer Strips (meters)	04	4.24 (0.24)	***	1.23 (0.40)	***	2.44 (1.49)	
Main crop (% UAA)	0201	-0.03 (0.00)	***	-0.04 (0.00)	***	-0.03 (0.03)	
Main crop (% UAA)	0205	-0.04 (0.00)	***	-0.04 (0.00)	***	-0.03 (0.01)	***
Crop diversity index	0201	0.05 (0.01)	***	0.05 (0.01)	***	0.05 (0.03)	*
Crop diversity index	0205	0.03 (0.00)	***	0.04 (0.00)	***	0.03 (0.02)	
Number of crops	0201	0.77 (0.08)	***	0.87 (0.08)	***	0.85 (0.36)	**
Number of crops	0205	0.69 (0.04)	***	0.70 (0.04)	***	0.65 (0.23)	***
Organic land area (hectares)	21	47.17 (0.60)	***	46.01 (0.83)	***	46.41 (0.13)	***
Under conversion (hectares)	21	4.46 (0.04)	***	4.26 (0.10)	***	4.41 (2.52)	*

Note : NNM[1] refers to the multivariate NNM estimator, NNM[2] refers to the univariate NNM estimator, and LLM refers to the local linear matching estimator. Standard errors are in parentheses. Details on their estimation are provided in appendix B. UAA refers to Usable Agricultural Area.

The positive effect of AES 0301 was a success for the French agri-environmental programme. Nevertheless, this success was moderated by a rather important windfall effect. Results indicate that AES 0301 induced the planting of nearly 94,000 ha of cover

crops in France in 2005 (obtained by multiplying the ATT by the number of participants) while 230,000 ha were subsidized in the same year [7]. This windfall effect translates into a larger cost per planted area than per subsidized area: 16 millions euros were spent in 2005 on AES 0301 [7], which means a cost of 170 euros per additional hectare of cover crops, while the mean premium for such AES did not exceed 88 euros per hectare. Such results suggest that around one out of two hectares of cover crops would in any case have been sown by participants, even in the absence of AES 0301.

The ATT for AES 04, which consists in planting fertilizer-free grass buffer strips at the edge of an agricultural field, has not been estimated using DID-estimators, the outcome variable being unobserved in 2000. The ATT varies across estimators. The local linear estimator suggests that participants in AES 04 have 240 more meters of grass buffer strips than their matched counterparts (table 3), although this is estimated with a lack of precision. Results presented in table 4 show that such a difference results from the fact that participants' strips are twice as long as those of non-participants. Anyway, such impacts do not appear to be large, compared to the total of all grass buffer strips in France counted in 2005 (around 20,000 km), largely due to the eco-conditionality of Common Agricultural Policy direct subsidies.

Effect of AES aiming at encouraging crop diversification

Two AES are likely to affect crop diversification: the AES 0201, which consists in introducing one new crop in the rotation, and the AES 0205, which implies having at least four different crops in the rotation. Unlike the case above, participants in AES 0201 are different from participants in the less ambitious AES 0205. As a matter of fact, results suggest that AES 0201 has generally had a stronger impact on outcome variables than AES 0205 (table 3), although there are fewer participants in AES 0201 (table 1). Such impacts are generally estimated precisely (ATTs are different from zero at the 1 per cent level of significance). Results suggest that AES 0201 (resp. 0205) has increased the crop diversity index by .05 (resp. .03), which is not a high effect, as the diversity index varies from 0 to 1. On the contrary, these same AESs have larger effects on the number of crops

in the rotation: they are responsible for the addition of a little less than one crop to the rotation (.85 for 0201 and .65 for 0205). These contrasting results can be reconciled by noting that these AESs have had a very limited effect on the area covered by the main crop as a proportion of UAA: it has decreased by only 3 %, meaning that most of the rotation has remained unchanged and that the additional crop has been planted on a limited area. Table 4 further shows that the difference in the crop diversity index between groups is mainly due to a decrease in the crop diversity index for matched non-participants.

Table 4: Unadjusted means of outcome variables in differences ("STRU-2005")

Outcome	AES	Treated	Controls	ATT	StdE
Cover crops (hectares)	0301	13.89	2.84	10.66 ***	0.18
Cover crops (hectares)	w/o 09-08	13.33	3.20	9.59 ***	0.29
Cover crops (hectares)	09	5.41	1.96	3.44 ***	0.11
Cover crops (hectares)	09 w/o 03	1.99	1.51	0.60 ***	0.10
Cover crops (hectares)	08	4.89	1.74	3.18 ***	0.10
Cover crops (hectares)	08 w/o 03	1.42	1.47	0.04	0.10
Grass Buffer Strips (meters)	04	1018.40	553.68	423.64 ***	24.14
Main crop (% UAA)	0201	-0.02	0.00	-0.03 ***	0.00
Main crop (% UAA)	0205	-0.03	0.00	-0.04 ***	0.00
Crop diversity index	0201	0.01	-0.05	0.05 ***	0.01
Crop diversity index	0205	-0.01	-0.04	0.03 ***	0.00
Number of crops	0201	0.55	-0.31	0.77 ***	0.08
Number of crops	0205	0.49	-0.25	0.69 ***	0.04
Organic land area (hectares)	21	50.10	3.54	47.18 ***	0.60
Under conversion (hectares)	21	4.48	0.01	4.47 ***	0.04

Note: The ATT is estimated using the nearest neighbor estimator NNM[1] based on multivariate matching. The difference of means of outcome variables between treated and control groups does not correspond precisely to the estimated ATT displayed in column 5, as the nearest neighbor procedure involved a bias-corrected step. UAA refers to Usable Agricultural Area.

Effect of AES aiming at encouraging conversion to organic farming

As in the case of the AES 04, the ATT for the AES 21, which consists in encouraging the adoption of organic farming practices, has not been estimated using DID estimators. This was because the outcome variables were unobserved in 2000. As already argued, this is not likely to lead to a large bias since farmers entering this AES where required to have no area cultivated in organic farming. If anything, matching estimates should thus lead to a lower bound on the treatment effect. Results suggest a rather important

impact of AES 21 on the land area dedicated to organic farming and the land area under conversion. Table 3 shows a difference between the treated and control groups close to 46 ha in the area fully converted to organic agriculture, and a difference of 4.5 ha in the area in the process of conversion. Table 4 further shows that such a gap is mainly due to the land area under organic farming being much larger for participants than for their matched counterparts. In view of these results, the AES 21 appears to be the cause of almost all the additional land devoted to organic farming since 2000.

5.3 Robustness checks: placebo tests and DDD estimates

Placebo tests consist in applying the DID-matching estimator to post-2003 participants outcomes. Indeed, no effect should be detected for these treated groups. However, these tests are disrupted by anticipation effects due to the unusually long period of time taken to process administrative applications in 2003. That is why we perform these tests on groups of future participants that enter the program at dates progressively farther away from September 2003. If our interpretation of anticipation effects is correct, and if the identification assumptions behind DID-matching are fulfilled, we should observe a progressive decrease in the placebo effect the further away participation takes place, and we should obtain a zero effect after some time. Results are presented in table 5.

For the AES 0301, the average treatment effect on the cover crop area that we estimate in 2003 on the post-September 2003 group of participants remains around 3 ha until we apply the estimator to the post-September 2005 group of participants. The average treatment effect then falls to 1 ha, without being statistically different from zero. Such results corroborate the idea of anticipatory behavior due to administrative delays. Results are similar for AES 09. For the AES 21, results conform to the same profile, except that anticipation is very high but drops more rapidly: it is halved between March and September 2004. Results for participants who enter the AES later become imprecise due to smaller sample size.

For AES 0201, the average treatment effects on the number of crops, on the main crop area, and on the crop diversity index cannot be estimated with a high level of

precision but overall the estimated average treatment effects appear to be small. On the contrary, for AES 0205, the number of crops exhibits a decreasing time trend coherent with anticipation behavior.

Table 5: Results of the placebo tests

		Sample								
Outcome	AES	post-Sept03		post-Mar04		post-Sept04		post-Mar05		post-Sept05
Cover crops (ha)	0301	3.52 (0.60)	***	3.60 (0.60)	***	3.14 (0.69)	***	3.34 (0.80)	***	1.32 (1.02)
Cover crops (ha)	09	2.85 (0.70)	***	2.82 (0.74)	***	2.64 (0.95)	***	2.53 (1.04)	**	1.86 (1.44)
Cover crops (ha)	08	1.13 (0.48)	**	0.91 (0.47)	*	0.85 (0.58)		0.90 (0.66)		1.93 (2.07)
Main crop (% UAA)	0201	-0.02 (0.02)		-0.01 (0.02)		-0.02 (0.01)		-0.03 (0.02)	*	n.a. (n.a.)
Main crop (% UAA)	0205	-0.01 (0.00)	***	-0.01 (0.00)	***	-0.01 (0.00)	***	-0.01 (0.01)	*	n.a. (n.a.)
Crop diversity index	0201	0.03 (0.02)	**	0.02 (0.02)		0.03 (0.02)		0.03 (0.04)		n.a. (n.a.)
Crop diversity index	0205	0.02 (0.01)	***	0.02 (0.01)	***	0.01 (0.01)	***	0.02 (0.01)	*	n.a. (n.a.)
Number of crops	0201	0.21 (0.19)		0.09 (0.19)		0.21 (0.28)		-0.12 (0.31)		n.a. (n.a.)
Number of crops	0205	0.33 (0.09)	***	0.33 (0.09)	***	0.35 (0.09)	***	0.19 (0.19)		n.a. (n.a.)
Organic land area (ha)	21	6.71 (2.53)	***	4.91 (2.35)	**	5.90 (2.65)	**	5.58 (4.13)		n.a. (n.a.)
Conversion to organic (ha)	21	13.96 (4.39)	***	15.58 (4.52)	***	4.05 (2.51)		4.81 (4.02)		n.a. (n.a.)

Note : The ATT are estimated using the local linear matching estimator. Asterisks denote statistical significance at 1 % (***), 5 % (**) or 10 % (*) level. Standard errors are in parentheses. Details of their estimation are presented in the appendix. Average treatment effects are estimated successively on the post-September 2003 participants' group, the post-March 2004 participants' group, the post-September 2004 participants' group, the post-March 2005 participants' group, and the post-September 2005 participants' group. For AES 04 only, placebo-tests can not be applied because the associated outcomes are not observed in 2003. UAA refers to Usable Agricultural Area.

Overall, results of the placebo tests confirm the importance of anticipation effects and suggest small or null time-varying selection bias. These results are consistent with our knowledge of the administrative procedure underlying the farmers' participation in the scheme and thus tend to support the chosen identification strategy based on DID-matching. However, insofar as we cannot totally reject the hypothesis of a divergence

between the two groups, in addition to the anticipation effect, we also turn to the triple-difference matching estimator with a view to determining the lower bound of the effect that we try to recover.

Table 6: Average treatment effect on the treated for AES in 2005 using DDD-matching

Outcome	AES	DDD Sep03-Mar05 ATT$^{(1)}$		DID Sep03-Mar05 ATT$^{(2)}$		DID whole sample ATT$^{(3)}$	
Cover crops (ha)	0301	4.87 (1.26)	***	10.46 (0.97)	***	10.66 (1.32)	***
Cover crops (ha)	09	-0.03 (1.22)		5.04 (1.02)	***	3.38 (0.79)	***
Cover crops (ha)	08	0.11 (0.85)		2.80 (0.78)	***	3.10 (0.76)	***
Main crop (% UAA)	0201	-0.04 (0.02)	***	-0.05 (0.02)	***	-0.03 (0.03)	
Main crop (% UAA)	0205	-0.01 (0.01)		-0.03 (0.00)	***	-0.03 (0.01)	***
Crop diversity index	0201	-0.02 (0.03)		0.03 (0.02)		0.05 (0.03)	*
Crop diversity index	0205	0.00 (0.01)		0.03 (0.01)	***	0.03 (0.02)	
Number of crops	0201	0.79 (0.38)	**	1.05 (0.37)	***	0.85 (0.36)	**
Number of crops	0205	0.07 (0.12)		0.67 (0.10)	***	0.65 (0.23)	***
Organic land area	21	14.07 (10.11)		45.01 (6.98)	***	50.82 (2.79)	***

Note : ATT$^{(1)}$ refers to the triple-difference estimates, ATT$^{(2)}$ refers to the DID-matching estimates on the same sample (farmers who have entered the AES between September 2003 and March 2005), and ATT$^{(3)}$ refers to the DID-matching estimates on the whole sample (farmers who have entered the AES before March 2005). StdE$^{(1)}$, StdE$^{(2)}$, and StdE$^{(3)}$ are the associated standard errors. UAA refers to Usable Agricultural Area.

We apply the triple-difference estimator, which consists in correcting the DID-matching estimates in 2005 by taking into account the divergence estimated in 2003 between the participants and their matched counterparts. Note that the triple-difference estimator then leads to a lower bound on the treatment effect, since it assumes that all the divergence detected in 2003 is due to selection bias, which is not true. Results of the triple-difference estimator are displayed in table 6. As we apply this estimator to a subset of the data (only participants entering the scheme between September 2003 and March

2005 are included in the sample), it could be that the *ATT* estimated on this subpopulation is not representative of the treatment effect on the overall population of participants. In order to have an indication on the severity of this problem, we re-estimate the *ATT* by DID-matching on this subpopulation. Results are in general very close to the ones obtained on the overall population.

For AES 0301, DDD-matching gives an average treatment effect on the treated of around 5 ha, while it is around 10 ha when estimated by applying the DID-matching estimator. Although placebo tests clearly suggest that DID-matching should be preferred, 5 ha is a lower bound on the treatment effect, thereby confirming that this AES exhibits significantly positive additionality effects. For AES 0201, the average treatment effect on the main crop area is a reduction of 4%, whereas it is a reduction of 5% when estimated by applying DID-matching. Moreover, the average treatment effect on the number of crops is an increase of 0.8, whereas it is an increase of 1.05 when estimated by applying DID-matching. Such results thus indicate that the lower bound for these effects remain very close to the DID-matching results. For AES 0205, the triple-difference estimates suffer from a lack of precision. In any case, this does not modify our conclusions on DID-matching estimates: the DID-matching estimates being already very low, we actually expected very similar results from the triple-difference estimator. Finally, for AES 21, the triple-difference results do not allow for a lower bound to be provided with precision. However, here again, in accordance with the placebo test results, we can reasonably suppose that DID-matching results must be preferred and we cannot exclude a large effect of this AES.

6 Conclusion

In this paper, we extend the literature applying modern treatment effect methods to the evaluation of the AESs. Contrary to the widespread practice of stating both the evaluation problem and the identification strategy in statistical terms, we frame them as restrictions on an economic model of an agricultural household deciding to enter an

AES. This approach provides a better understanding of the implications of the chosen identification strategy, allows for the implied restrictions of what we know about actual program implementation to be compared, and provides guidance for selecting control variables. We argue that several features of the program are compatible with the identifying assumptions of DID matching: a low take-up rate that ensures that the effects of these programs on non-participants are negligible, a lag between entry and production decisions which implies ignorance of time-varying profit opportunities when deciding to enter the program, and geographic variations in administrative costs of the application that translate into variable probability of participating in the AES *ceteris paribus*. Moreover, our theoretical approach enables us to check the robustness of our results. As the assumptions required to implement DID-matching imply that the rate of adoption of practices be the same among participants and observationally identical non-participants in the absence of the program, we test this implication on two pre-treatment years. We also apply an alternative estimator assuming divergent rates of adoption of new practices. In the particular context of our case study, this approach has the advantage of giving a lower bound on the treatment effect.

Estimates using our original database linking administrative records to census and survey data show that the French AESs have contrasting effects, depending on their prerequisites and the level of payment they offer. The most successful AESs have concerned organic farming: large payments seem to have given high incentives to farmers to switch to organic farming, and the AES program is responsible for almost 90 % of the increase in areas under organic farming between 2000 and 2005. AESs that provide incentives to sow intermediate crops to reduce nitrogen transfer and erosion have also had a favorable effect: they are responsible for 12 % of the increase in this type of crop between 2000 and 2005. Large windfall effects are nevertheless detected for this AES, since more than half of the subsidized areas would have been sown without the AES, which translates into a cost per additional sown area that is more than double the cost per subsidized area. AESs that provide incentives to plant more diversified crops have had limited impacts: participants have generally added one crop to their rotation, but on a very limited area,

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which translates into very low impacts on diversity indexes. We also show that AESs requiring restricted input use and AESs requiring an intermediate crop both seem to increase the area under intermediate crops when considered jointly, but that the former has no effect when considered separately from the latter.

Follow-up to this work could go in three directions. First, we could get a better sense of the environmental consequences of the AES by studying the distribution of their causal effects. Distribution could be estimated by focusing on quantile treatment effects [16] or by the spatial distribution of the effects. Second, the complete evaluation of the program critically hinges on the estimation of the monetary value of the causal effect of the AES. This involves translating mean or distribution of causal effects of the AES in monetary terms. Third, at the methodological level, comparison of the ATT estimated in this paper, with the effect that would be estimated by using instrumental variables (and their respective quantile treatment effects [2]), would give a more thorough understanding of the heterogeneous impacts of the program and of how the two estimators weigh them.

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A   Sample size and AES participation

AES		Treated	CS[a]	Non treated	Sample
	Panel B: used for placebo tests on the post-sep03 treated				
0301	Implanting cover crops	741	655	58,586	59,241
09	Reduction of fertilizer use	467	405	58,586	58,991
08	Reduction of pesticides use	579	506	58,586	59,092
04	Implanting grass buffer strips	382	334	58,586	58,920
0201	Adding one more crop to the rotation	135	101	58,586	58,687
0205	Having at least 4 crops in the rotation	740	632	58,586	59,218
21	Conversion to organic farming	182	101	58,586	58,687
	Panel C: used for placebo tests on the post-mar04 treated				
0301	Implanting cover crops	727	641	58,586	59,227

Notes: (a) CS refers to the calculated number of treated observations lying on the common support. Details on its calculation are given in appendix B.

AES		Treated	CS[a]	Non treated	Sample
09	Reduction of fertilizer use	448	387	58,586	58,973
08	Reduction of pesticides use	552	484	58,586	59,070
04	Implanting grass buffer strips	365	322	58,586	58,908
0201	Adding one more crop to the rotation	132	95	58,586	58,681
0205	Having at least 4 crops in the rotation	740	632	58,586	59,218
21	Conversion to organic farming	173	98	58,586	58,684
Panel D: used for placebo tests on the post-sep04 treated					
0301	Implanting cover crops	543	472	58,586	59,058,
09	Reduction of fertilizer use	331	277	58,586	58,863
08	Reduction of pesticides use	418	366	58,586	58,952
04	Implanting grass buffer strips	239	203	58,586	58,789
0201	Adding one more crop to the rotation	88	53	58,586	58,639
0205	Having at least 4 crops in the rotation	736	627	58,586	59,213
21	Conversion to organic farming	106	54	58,586	58,640
Panel E: used for placebo tests on the post-mar05 treated					
0301	Implanting cover crops	387	329	58,586	58,915
09	Reduction of fertilizer use	251	212	58,586	58,798
08	Reduction of pesticides use	338	291	58,586	58,877
04	Implanting grass buffer strips	170	140	58,586	58,726
0201	Adding one more crop to the rotation	68	30	58,586	58,616
0205	Having at least 4 crops in the rotation	164	120	58,586	58,706
21	Conversion to organic farming	71	29	58,586	58,615
Panel F: used for placebo tests on the post-sep05 treated					
0301	Implanting cover crops	163	130	58,586	58,716
09	Reduction of fertilizer use	103	64	58,586	58,650
08	Reduction of pesticides use	118	80	58,586	58,666
04	Implanting grass buffer strips	57	16	58,586	58,602
0201	Adding one more crop to the rotation	21	0	58,586	58,586
0205	Having at least 4 crops in the rotation	36	0	58,586	58,586
21	Conversion to organic farming	32	0	58,586	58,586
Panel G: used for triple-difference matching estimates (sep03-mar05)					
0301	Implanting cover crops	386	332	58,586	58,918
09	Reduction of fertilizer use	247	206	58,586	58,792
08	Reduction of pesticides use	270	223	58,586	58,809
04	Implanting grass buffer strips	239	199	58,586	58,785
0201	Adding one more crop to the rotation	79	48	58,586	58,634
0205	Having at least 4 crops in the rotation	775	661	58,586	59,247
21	Conversion to organic farming	130	63	58,586	58,649

Notes: (a) CS refers to the calculated number of treated observations lying on the common support. Details on its calculation are given in appendic B.

B   Matching procedure

Propensity score and common support

In a first step, we estimate the propensity score $P(X)$: the probability of benefiting from an AES conditional on control variables ($P(X) = \Pr(D = 1|X)$, where $X = (T, I, S)$).

Rosenbaum and Rubin [37] show that matching on the propensity score is equivalent to matching on all the observed covariates, thereby dramatically reducing the dimensionality of the matching problem. We also use the propensity score to estimate the zone of common support, defined as the set of participants for whom the density of non-participants having the same propensity score is higher than some cut-off level [41]. The cut-off is determined so that some overall trimming level is attained.[31] We estimate the propensity score by running separate probit regressions on samples containing non-participants (farmers without any AES) and farmers benefiting from the particular AES that we are studying. Lechner [31] shows that this simple procedure performs as well as estimating a multinomial probit.

Matching estimators

With panel data, a typical DID-matching estimator calculates the mean difference between participants' mean increments in agricultural practices between dates t' (before the treatment) and t (after the treatment), and the mean increments of their matched counterparts:

$$\widehat{\mathbb{E}}\left[Y^1 - Y^0 | D = 1\right] = \frac{1}{n1} \sum_{i \in I_1 \cap S_P} \left(Y_{it}^1 - Y_{it'}^0 - \widehat{\mathbb{E}}\left[Y_{it}^0 - Y_{it'}^0 | D = 1, X_i\right]\right) \tag{15}$$

with

$$\widehat{\mathbb{E}}\left[Y_{it}^0 - Y_{it'}^0 | D = 1, X_i\right] = \sum_{j \in I_0} W_{ij}(Y_{jt}^0 - Y_{jt'}^0) \tag{16}$$

where Y^0 denotes the potential input level (the potential outcome) in the untreated state (no AES), Y^1 denotes the potential input level (the potential outcome) in the treated state (with AES), I_1 is the group of participants, S_P denotes the common support, I_0 denotes the group of non-participants and n_1 is the number of participants in I_1.

In what follows, we use two matching estimators. They differ in how the matched non-participants are chosen and in how the weights W_{ij} are constructed [28]. The nearest-neighbour matching (NNM) used in the analysis is a multivariate matching based on the distance between vectors X_j and X_i.[32] Such estimator matches each participant i to its "closest" non-participant j. We use Sekhon [39]'s implementation of NNM in R.

We also use local linear matching (LLM) which is based on the propensity score $P_i = P(X_i) = \Pr(D_i = 1 | X_i)$. This estimator constructs a match for each participant i using a weighted average over all non-participants, where the weights depend on the

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[31]In practice, we first define the set of positive densities: $\widehat{S}_P = \left\{i : \widehat{f}(P(X_i)|D_i = 1) > 0 \text{ and } \widehat{f}(P(X_i)|D_i = 0) > 0\right\}$. The common support group is then the following set: $\widehat{S}_q = \left\{i \in I_1 \cap \widehat{S}_P : \widehat{f}(P(X_i)|D_i = 1) > c_q \text{ and } \widehat{f}(P(X_i)|D_i = 0) > c_q\right\}$, where the cutoff level c_q is chosen as the solution to the following problem: $\sup_{c_q} \frac{1}{2J} \sum_{i \in I_1 \cap \widehat{S}_P} \left(\mathbb{1}\left[\widehat{f}(P(X_i)|D_i = 1) < c_q\right] + \mathbb{1}\left[\widehat{f}(P(X_i)|D_i = 0) < c_q\right]\right) \leq q$, where I_1 is the group of participants and J is the number of participants in $\widehat{S}_P$. In our applications, we choose $q = 0.05$.

[32]Letting $\| X \| = (X'SX)^{(1/2)}$ be the vector norm with positive definite weight matrix S, we define $\| X_i - X_j \|$ to be the distance between the vectors X_i and X_j. S is the diagonal matrix constructed by putting the inverses of the variances of the covariates on the diagonal [3].

distance between propensity scores. The weighting function for LLM is given by:

$$W_{ij} = \frac{G_{ij} \sum_{k \in I_0} G_{ik}(P_k - P_i)^2 - [G_{ij}(P_j - P_i)][\sum_{k \in I_0} G_{ik}(P_k - P_i)]}{\sum_{j \in I_0} G_{ij} \sum_{k \in I_0} G_{ik}(P_k - P_i)^2 - [\sum_{k \in I_0} G_{ik}(P_k - P_i)]^2} \qquad (17)$$

with $G_{ij} = G(\frac{P_i - P_j}{h})$, where G is a kernel function and h a bandwidth parameter [43].[33] Heckman, Ichimura, and Todd [20] advocate the use of this local linear regression version of the non-parametric kernel matching estimator because it has better performances at boundary points and adapts better to different data densities [15]. We programmed this estimator in R.

Bias-corrected matching estimator

Abadie and Imbens [5] show that matching estimators are biased in finite samples when there is more than one continuous covariate because of inexact matching. We thus use the bias-corrected matching estimator proposed by Abadie and Imbens [4], which uses linear regression within the matches to adjust for the remaining differences in their continuous covariates.[34] Such bias-adjustment thus affects the value of the estimator (but not its variance).

Estimator of the variances of matching estimators

Until recently, the properties of the NNM estimator have not been established because standard asymptotical analysis does not apply to matching estimators using a finite number of matches. Moreover, Abadie and Imbens [6] have shown that the bootstrap method fails for NNM but is valid for LLM. Abadie and Imbens [5] propose an asymptotically-consistent estimator of the variance of the NNM estimator for the population average treatment effect on the treated:

$$\widehat{V} = \frac{1}{N_1^2} \sum_{i=1}^{N} \left[D_i(Y_i^1 - \widehat{Y}_i^0 - \widehat{\tau})^2 + (1 - D_i)(K_M^2(i) - K_M'(i))\widehat{\sigma}_{D_i}^2(X_i) \right] \qquad (18)$$

where $\widehat{\tau}$ is the estimated ATT ($\widehat{\mathbb{E}}[(Y^1 - Y^0|D = 1, X, P])$, $K_M(j)$ is the number of times j is used as a match and $\widehat{\sigma}_{D_i}^2(X_i)$ is an estimator of the conditional outcome variance. As an estimator of the variance of the LLM estimator we implement a bootstrap procedure.

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[33]In practice, LLM estimation of $W_{ij}(Y_{jt}^0 - Y_{jt'}^0)$ simply amounts to estimating a in the following weighted least squares problem: $\min_{a,b} \sum_{j \in I_0} \left((Y_{jt}^0 - Y_{jt'}^0) - a - b(P_i - P_j)\right)^2 G_{ij}$.

[34]The detail procedure is given by Abadie, Drukker, Herr, and Imbens [3]. One must estimate the regression functions for the controls: $E(Y^0|X = x) = \widehat{\beta}_0 + \widehat{\beta}_1' x = \widehat{\mu}_0(x)$ with $(\widehat{\beta}_0, \widehat{\beta}_1) = \arg\min \sum_{i:D_i=0} K_M(i)(Y_i - \beta_0 - \beta_1' X_i)^2$ where $K_M(j)$ is the number of times j is used as a match. Then, given the estimated regression functions, one can predict the missing potential outcomes as: $\widehat{Y}_i^0 = \frac{1}{\#J_M(i)} \sum_{j \in J_M(i)}(Y_j + \widehat{\mu}_0(X_i) - \widehat{\mu}_0(X_j))$ where $\#J_M(i)$ is the number of units in the group of M matches $J_M(i)$.