

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

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

## Abstract

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. In this paper, we study the French implementation of the EU AES program. We estimate additional and windfall effects of five AESs, for a representative sample of individual farmers using Difference-In-Difference (DID) matching. We test for cross-effects, examine the implications of the identifying assumptions by implementing a placebo test, provide a lower bound using DDD-matching, and insert our estimates of both additionality and windfall effects into a cost-benefit framework. We find that the AESs promoting crop diversity have succeeded in inserting one new crop to the rotation, but on a very small part of the cropped area. We also find that the AES subsidizing the planting of cover crops has led to the planting of 10 additional hectares of cover crops on the average recipient farm, at the expense of almost 7 hectares of windfall effect, and that this AES does not appear to be socially efficient. On the contrary, we find that the AES subsidizing grass buffer strips could very well be socially efficient, despite very large windfall effects. We finally estimate that the AES subsidizing conversion to organic farming has very low windfall effects and very high additionality.

**Keywords:** Agro-environmental Schemes - Additionality - Windfall Effects - Treatment Effects - Difference in Difference Matching - Agricultural Practices - Crop Diversity - Cover Crops - Grass Buffer Strips - Organic Farming.

# 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. In this paper, we study the French implementation of the EU AES program. The AESs that we study aim at altering agricultural practices in order to improve the environment. Two AESs aim at increasing crop diversity, which in turn may increase the diversity of habitats, and thus biodiversity. Increased crop diversity may also reduce weeds' resistance to pesticides by diversifying rotations on the same field. Another AES that we study subsidizes the planting of cover crops during the winter, which curbs erosion and prevents nitrogen leaching into groundwater by storing it during the winter. We also study an AES that subsidizes the planting of grass buffer strips, mainly along rivers and streams. Grass buffer strips are known to be very efficient at preventing nitrogen, phosphorus and pesticide runoff from fields. They thus contribute to the improvement of surface water quality. Finally, we study an AES that subsidizes conversion to organic farming. Organic farming bans the use of chemical fertilizers and pesticides, thereby reducing the emission of pollutants into ground and surface water.

The aim of this paper is to estimate the additional and windfall effects of these AESs on agricultural practices in order to conduct a cost-benefit analysis of each one of them.<sup>1</sup> Cost-benefit analysis of these programs indeed critically hinges on the relative extent of their additional and windfall effects. An AES has an additional effect if it encourages farmers to adopt environmentally greener practices, *i.e.* if it has a positive causal effect on practices that favor the environment. An AES suffers from windfall effects if it pays for practices that would have been adopted in its absence. Higher additionality improves the efficiency of the program and thus increases the benefit/cost ratio. Higher windfall effects, on the contrary, tend to decrease the efficiency of the program by using resources

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<sup>1</sup>We thank the editor and three anonymous referees for suggesting that we insert our estimates into a cost-benefit framework.

to pay for practices that would have been adopted anyway, and thus deteriorates the benefit/cost ratio. The very nature of the AESs that we study opens up the possibility for large windfall effects. Indeed, these AESs are voluntary programs in which farmers receive a payment per hectare concerned by the practice. As both the AES requirements and the per hectare payments are constant for all farmers, the potential for adverse selection is very high: farmers with the lowest costs of complying with the requirements of a given AES are the most likely to enter it. Thus, it is very likely that farmers who self-selected into an AES would in any case have adopted a higher level of the practice than farmers not entering the AES, had the AES not been implemented.

We estimate additional and windfall effects of the five AESs described above for a representative sample of French farmers. We use a detailed sample of individual farmers for whom we have data on practices related to the AESs under study (crops planted, area under cover crops, grass buffer strip and organic farming) recorded in 2005, five years after the beginning of the program. We also have data on practices and farms' and farmers' characteristics before the program started. Finally, we have detailed and disaggregated information from administrative sources on the AESs that each farmer has entered. We show that, under some assumptions, the relevant causal effect measuring additionality is the so-called average treatment effect on the treated (ATT). ATT is the difference between the average level of a practice in the presence of an AES and the average level of the same practice had the AES not been implemented, among farmers benefiting from an AES. Windfall effects can easily be estimated from average practices observed among recipients of an AES and the ATT. Estimating the ATT is more complicated since we cannot observe the average level of a given practice for recipient farmers had the AES not existed. This is an instance of the fundamental problem of causal inference [25], due to the impossibility of observing the counterfactual situation. If we try to approximate this counterfactual quantity among recipient farmers by using the average level of the practice among non-recipient farmers, our estimates of the ATT will most likely be plagued by selection bias. Indeed, profit-maximizing farmers self-selecting into an AES have lower costs of complying with the AES requirements. It is therefore likely that farmers who

chose to enter an AES would in any case have adopted greener practices than farmers not entering it, had the AES not been implemented. So a rough comparison of the practices of recipient farmers to those of non-recipient farmers is likely to overstate the true level of additionality of an AES.

We use Difference-In-Difference (DID) matching [2, 22] to get rid of selection bias and to estimate the ATT. DID-matching combines a non-parametric matching procedure with first-differencing with respect to a pre-treatment period. Matching gets rid of selection bias due to observed covariates by comparing recipient farmers to similar non-recipient. First differencing gets rid of selection bias due to time-invariant unobservables. The validity of DID-matching relies on three assumptions. First, the absence of diffusion effects of the AESs on non-recipient farmers. Second, the existence of non-recipient farmers similar to recipient farmers in terms of observed covariates. Third, in the absence of any AES, the difference in practices between recipient and similar non-recipient farmers is constant over time. We argue that the economics of the program under study make it very likely that these assumptions hold in our application. Moreover, we test the validity of various implications of these assumptions and find evidence in their favor. We test for the presence of diffusion effects<sup>2</sup> by inserting the initial average level of a given practice among neighboring farmers as a control variable. We find no difference in estimated treatment effects with or without this additional control variable suggesting that diffusion effects are absent. We test for the existence of similar farmers by using Smith and Todd [45]’s common support estimation procedure. We generally find that non-recipient farmers do exist for most of our treated farmers. Finally, we test for the constancy of the average difference in practices between recipients and non-recipients in the absence of the program by implementing a placebo test. We estimate the effect of a placebo treatment by comparing future recipients to future non-recipients at two different dates. We find effects of smaller magnitude than the treatment effects, and evidence that these effects are anticipation effects: because the date at which the requirements are really binding is uncertain, farmers start complying with the requirements early on. Indeed,

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<sup>2</sup>We thank an anonymous referee for suggesting that we try to test this assumption.

these anticipation effects vanish when we look at recipients who will enter an AES at a later stage. We nevertheless provide a lower bound on the treatment effect by providing estimates from triple-difference (DDD) matching. Finally, because farmers can enter multiple AESs and we want to perform a separate cost-benefit analysis for each AES under study, we show that we can conduct a separate cost-benefit analysis for each AES if there are no cross-effects among the AESs:<sup>3</sup> an AES aimed at altering a given practice (say cover crops) must not alter any other practice (e.g. conversion to organic farming). We develop tests of this assumption and find strong support for the absence of cross effects for most AESs under study.

We find that the AESs subsidizing the planting of cover crops have led to the planting of 10 additional hectares of cover crops on the average recipient farm, at the expense of almost 7 hectares of windfall effect. Because the per hectare payment of this AES is quite high, and because the social value of cover crops is limited, this AES does not appear to be socially efficient. On the contrary, we find that the AES subsidizing grass buffer strips could very well be socially efficient, despite very large windfall effects, because grass buffer strips are very efficient at curbing the runoff of pollutants. We finally estimate that the AES subsidizing conversion to organic farming has very low windfall effects and very high additionality. According to our estimates, this AES is responsible for 90 % of the increase in areas converted to organic farming between 2000 and 2005. We estimate that it costs 151€ to convert one additional hectare to organic farming, compared to an average estimated social benefit from organic farming of 540€/ha. We cannot apply a complete cost-benefit analysis to the AESs aiming at increasing crop diversity because payments were not directly tied to a practice that we can observe. We nevertheless 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.

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<sup>3</sup>We thank an anonymous referee for pointing this out.

Overall, we find strong evidence of adverse selection, which induces large windfall effects. We find that the AESs combining strong requirements in order to curb adverse selection with large payments, such as the one subsidizing conversion to organic farming, are the most efficient schemes.

Our paper is not the first attempt at measuring the effects of AESs. The AESs in the EU are similar to the Conservation Reserve Program (CRP) in the US, in the sense that the government offers individual farmers or firms temporary subsidies in exchange for voluntary changes in agricultural practices that are expected to generate environmental benefits - to reduce crop acreage in this case. Early works include Lynch and Liu [34] and Lynch, Gray, and Geoghegan [33], who focus on the impact of these AESs on land prices. Wu [50] and Roberts and Bucholtz [36] run OLS and 2SLS regressions to test the hypothesis that acreage reductions due to CRP have been offset by increases in cropland in other areas. Smith and Goodwin [46] estimate a five-equation structural model of CRP participation, soil erosion, crop insurance participation, conservation, and fertilizer usage, using a 2SLS procedure, to determine the impact of CRP on soil erosion. Wu, Adams, Kling, and Tanaka [51] jointly estimate crop choice and the decision to use conservation tillage and simulate the effects of CRP on erosion and nitrogen leaching and runoff. Roberts and Lubowski [37] model the decision to establish crops using a binomial probit regression to predict the likelihood that each CRP contract will return to crop production if the program were to expire once and for all. Most if not all econometric studies of CRP are based on a county level database from the United States Department of Agriculture's National Resources Inventory, although econometric models are based on individual farmers' decisions to enroll land in CRP and change land use. In addition to the empirical literature on AES evaluation, a growing number of empirical works aim at estimating the effects of the US Environmental Protection Agency (EPA) voluntary programs or voluntary international standards like ISO14001 on firms' environmental performances. They run a linear 2SLS regression on micro-data to recover the impact of voluntary programs on the toxic releases and economic performance of firms in the US [10, 28] and in developing countries [16]. Arimura, Hibiki, and Katayama [11] use

the Bayesian approach (maximum simulated likelihood along with the GHK simulator) to estimate the impact of the implementation of ISO14001 and publication of environmental reports on environmental performance of Japanese facilities.

Perhaps the closest paper to our own is the study by Pufahl and Weiss [35] of the effect of benefiting from at least one AES on farm sales, fertilizer expenditures and cattle livestock density measured from bookkeeping records of a non-representative sample of German farms. We focus on different outcomes and implement a separate analysis for each AES. We test for cross-effects, examine the implications of the identifying assumptions, provide a lower bound using DDD-matching, and insert our estimates of additionality and windfall effects into a cost-benefit framework.

This paper is organized as follows: the implementation of AESs 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; the cost-benefit analysis is presented in Section 6 and Section 7 concludes.

## 2 Agro-Environmental Schemes in France

Rural development policies accounted for 22 % of public spending for the Common Agricultural Policy (CAP) of the European Union in 2006, and AESs accounted for 37 % of rural development spending [35]. 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.<sup>4</sup> AES expenditures in France per hectare of usable agricultural area (UAA) are lower than in most European countries,<sup>5</sup> but it is mainly because the area covered by AESs is smaller than in other countries, and not because payments per hectare are small. French AESs are nevertheless worth assessing for three 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

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<sup>4</sup>According to the French Ministry of Agriculture's website.

<sup>5</sup>According to the European Environment Agency's website.

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 [8]. Finally, the future reform of the CAP is going to involve a major “greening” of all subsidies. As a result, a growing share of CAP spending is going to take the form of AESs.

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 [13]). AESs were referred to with a seven digit code: the first two digits referred to the general category of the AES, the following two referred to the particular requirements the farmer had to meet to enter the AES, the fifth digit coded for even more detailed requirements, and, finally, the last two digits referred to the regional variation in the AES. These AESs aim at improving the environment by altering farmers’ practices. AES 02 encourages crop diversification, which is likely to increase biodiversity, directly by increasing cropped biodiversity, but also indirectly by enhancing non-cropped biodiversity. AESs 03 (resp. 04) subsidizes the planting of cover crops (resp. grass buffer strips) and thus contributes to the reduction of nitrogen, phosphorus and pesticides leakage (resp. runoff) from the field. This in turn decreases the concentrations of pollutants in surface and ground waters. AESs 08 and 09 aim at decreasing the levels of pesticides and nitrogen applications on the fields, which also might decrease leakage and runoff. AES 21 encourages diversification toward organic farming, a practice that has been shown to be friendlier to the environment than conventional farming.

Taken together, these AESs accounted for 22 % of total spending on AES in 2006 in France.<sup>6</sup> We usually stick to the 2-digit level, with the exception of measures 0201, 0205 and 0301. Measures 0201 and 0205 both aim to increase the diversity of crop

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<sup>6</sup>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 [15], 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.

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 (0301), since they are the most widely chosen. Measures 0302 and 0303 (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 0801 and 0903, 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 0901 (reduction of 20 % of nitrogen use with respect to a local baseline) have lower take-up rates.

AESs are five-year contracts, with yearly payments and possible control of how well the requirements are met. Farmers can enroll only part of their farm under an AES, and combine different AESs on the same part of their farm or on different ones. Farmers receive the same payments per hectare for a given AES. These payments have been calculated so as to compensate an average farmer for the profit loss following the adoption of the practice. Total payments are proportional to the area on which the farmer declares to implement the requirements. 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. Almost all applications were approved. A contract was then 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 of 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 assistance 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 Theoretical model and identification strategy

In this section, we develop a model of an agricultural household deciding whether or not taking part in a unique AES program and then choosing its level of input. Identification assumptions are then presented as restrictions on this model. We finally deal with the complexities of the real world scenario in which farmers can simultaneously choose multiple AESs.

#### 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 information at hand on profit and utility determinants noted  $\mathcal{I}$ . Second, all 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 and how the AES impact them, and then consider the household's decision to enter the scheme.<sup>7</sup>

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<sup>7</sup>We do not explicitly model the dynamic behavior of farmers. Dynamics could play an important role if there are large learning effects of entering a scheme and if the sunk costs for changing practices are large. We do not think that there are large sunk costs for most of the practices we study, with the exception of organic farming. Farmers wishing to convert to organic farming may have delayed their decision in order to benefit from AES 21. For our estimates to be correct, we have to assume that the costs of entering the schemes were not anticipated by the farmers, so that some of the delayers actually could not enter the scheme at a reasonable cost. This is an application of the general result of Abbring and Heckman [7] that a structural dynamic model with the assumptions in Rust [41] implies conditional exogeneity assumptions that resemble matching in a dynamic framework. To our knowledge, there is no such result relating structural dynamic models allowing for unobserved fixed effects *à la* Keane and Wolpin [27] to

## 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:  $Q = F(Y, H, \mathbf{I}, \boldsymbol{\epsilon})$ . 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  $\boldsymbol{\epsilon} = (\boldsymbol{\mu}, \boldsymbol{e})$ . 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  $Y \leq \bar{Y}$ .<sup>8</sup> The household derives income from farming but also from working  $H^{\text{off}}$  hours off the farm for a wage  $w$ . It derives utility from consumption  $C$  and leisure  $L$ . Since Fall and Magnac [19] have empirically shown that French farmers strongly exhibit a 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.<sup>9</sup> 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

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the assumptions behind DID-matching. This goes beyond the scope of our paper. With DID-matching, we assume the additive separability of the fixed effects, whereas a structural dynamic model would make stronger functional form assumptions and would restrict the distribution of the unobservable to have a (low) finite number of points of support. Such a model would help measuring expectation effects, but we do not expect them to be very large. We view DID-matching as imposing the minimum set of reasonable assumptions that allows us to estimate the effect of the program we study.

<sup>8</sup>This setting is closer to AESs 08 and 09 that encourage farmers to use less inputs. The discussion of our identification strategy derived from this special case extends to the other AESs we study.

<sup>9</sup>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 [47], a special case nested in our model.

$\mathbf{n}$ ). We thus have  $\boldsymbol{\eta} = (\boldsymbol{\delta}, \mathbf{n})$ .<sup>10</sup> The problem the household faces is:

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

subject to:

$$C = p^Q Q - p^Y Y + wH^{\text{off}} + DP \quad (2)$$

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

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

$$L + H + H^{\text{off}} = T \quad (5)$$

where  $T$  is the total time available to the household.

The first order condition for the input level is the following (with  $\lambda^Y$  the Lagrange multiplier associated to the input constraint):<sup>11</sup>

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

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. This level depends on all the exogenous variables of the problem, including the household characteristics  $\mathbf{S}$  and  $\boldsymbol{\eta}$ , as production decisions are not separable from consumption:<sup>12</sup>

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

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,

<sup>10</sup>Factors stored in  $\mathbf{n}$  can also reflect idiosyncratic variations in the wage or in the unemployment probability.

<sup>11</sup>A similar condition holds for labor on the farm and leisure.

<sup>12</sup>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.

we have:

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

$Y^1$  and  $Y^0$  are usually called potential outcomes. The individual-level causal effect of the AES ( $\Delta_Y$ ) 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 [25]. 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:  $ATT = \mathbb{E}[Y^1 - Y^0 | D = 1]$ . The value of this parameter is the one we try to recover here.

The causal effect might vary across the population, depending on 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  $\lambda^Y = 0$ ) could enter the AES at no cost, i.e. without modifying their agricultural practices, so that the the program has no effect on them ( $\Delta_Y = 0$ ).<sup>13</sup> These households would thus benefit from a pure windfall effect: they receive a subsidy but do not change their practices at all.

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 very well be null.

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<sup>13</sup>Unconstrained households may also have a particular taste for working on the farm or for using inputs so that they may 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  $\Delta_Y$  is unknown *a priori*.

## 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:

$$D = \mathbf{1} [\mathbb{E} [V_1 - V_0 | \mathcal{I}] - V \geq 0], \quad (9)$$

where  $\mathcal{I}$  denotes the information set of the agents when deciding whether to participate in the AES or not. Selection bias arises because some determinants of farmers' participation stored in  $\mathcal{I}$  are also determinants of input demands. Typically, fixed factors of production ( $\mathbf{I}$ ), land quality and managerial ability ( $\boldsymbol{\mu}$ ), consumption shifters ( $\mathbf{S}$ ) 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.

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:

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

## 3.2 Identification strategy

In this section, we derive the assumptions needed to identify  $ATT$  using DID-matching as restrictions on the economic model presented in the previous section. We discuss the validity of these conditions and offer ways to test their implications. Matching estimators assume that after conditioning on a set of observable characteristics ( $Z$ ), outcomes are

conditionally mean independent of program participation. The conditional mean independence assumption on  $Y^0$  can be written:  $\mathbb{E}[Y^0|D = 1, Z] = \mathbb{E}[Y^0|D = 0, Z]$ . However, for a variety of reasons there may be systematic differences between participant and nonparticipant outcomes, even after conditioning on observables. This could lead to a violation of the identification conditions required for matching. A DID-matching strategy, as defined in Heckman, Ichimura, and Todd [22], allows for temporally invariant unobserved differences in outcomes between participants and nonparticipants that closely resemble fixed effects in a panel data setting. Differencing the outcomes gets rid of the selection bias due to these unobservables. The conditional mean independence assumption on increments of  $Y^0$  that underlies DID-matching is:  $\mathbb{E}[Y_t^0 - Y_{t'}^0|D = 1, Z] = \mathbb{E}[Y_t^0 - Y_{t'}^0|D = 0, Z]$ , with  $t$  (resp.  $t'$ ) a post- (resp. pre-) treatment date. Under this assumption, DID-matching estimates are obtained by applying matching to the outcomes differenced with respect to a pre-treatment period. Two additional assumptions are needed to ensure that matching and DID-matching both recover an average causal effect on the treated. First, for each treated observation, there has to exist some untreated units having the same level of the observed covariates, *i.e.* we require that there exist common support for the observed covariates. Second, we need that untreated units' outcomes are not altered by the presence of the treatment among treated units, an assumption usually called SUTVA. We first deal with the Stable Unit Treatment Value Assumption (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 [40]'s 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 order to recover the ATT, we require the following restriction to hold in our model:

**Assumption 1.** *The level of prices  $(p^Q, p^Y, w)$ , the distribution of observed and un-*

*observed determinants of input use ( $T, \mathbf{I}, \mathbf{S}, \boldsymbol{\epsilon}, \boldsymbol{\eta}$ ) and the function  $g_0$  remain the same whether the AES is implemented or not.*

This assumption requires 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 AES not to have any effects on input and output prices. Assumption 1 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.<sup>14</sup>

This assumption also rules out imitation effects or increasing returns (due for example to several farmers creating a co-op to sell their organic products, while an isolated farmer cannot and may find it difficult to sell her products).<sup>15</sup> In that case, note that the estimated treatment effects will be biased downward, as farmers not entering the scheme may adopt a given practice because their neighbors have entered the scheme, and thus our estimates of treatment effects can be interpreted as lower bounds on the true treatment effect. Note however that conversion to organic farming following entry into an AES may decrease the prices of organic products. In that case, we expect entry in the AES by some farmers to deter other farmers from organic farming and DID-matching estimates would be biased upward.

Assumption 1 assumes away price and imitation effects and increasing returns due to neighboring farmers entering an AES. We set up a test of the validity of SUTVA based on the proportion of farmers adopting a given practice before anyone enters a scheme. Farmers having converted to organic farming before 2000 also generate imitation effects and/or increasing returns that make their neighbors more likely to go organic, and also to enter the scheme paying for this conversion. That means that if there are imitation effects,

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<sup>14</sup>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.

<sup>15</sup>We thank an anonymous referee for pointing this out.

our estimates suffer from omitted variables bias: the initial proportion of a farmer’s neighbors that has adopted the practice of interest (organic farming, cover crops) may at the same time determine selection into the corresponding scheme and outcomes in the absence of the treatment. The same is true for price effects: the larger the initial number of competitors, the less profitable the conversion to organic farming and thus entry into the corresponding scheme. At the end of the day, if there are imitation effects, price effects or increasing returns, controlling for the amount of neighbors initially adopting the practice would change our estimated treatment effects with respect to a baseline estimate not controlling for this variable. If the estimate increases (resp. decreases), then imitation (resp. price) effects dominate.

Assumption 1 implies that the effect of implementing the voluntary AES on those who have not entered it is null. Under this assumption,  $ATT$  is thus the policy-relevant 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 [24], 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 [2, 22, 35]. It states that, in the absence of the program, the average increment in input use 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:*

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

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

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

Part (i) of assumption 2 states that a farmer's decision to enter an AES does not depend on time-varying unobserved factors  $\mathbf{e}$  (weather shocks) and  $\mathbf{n}$  (idiosyncratic wage shocks). This ensures that selection in the program is based either on observed variables or on unobserved variables fixed through time. This assumption seems 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 farmers may not be able to forecast the level of the transitory determinants of input use  $\mathbf{e}$  and  $\mathbf{n}$  when deciding to enter the program.

Part (ii) of assumption 2 implies 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 autocorrelated. The main autocorrelated profit shocks may transit through prices, wages or activities. As we control for non-agricultural activities or specific contracts for quality products, it seems reasonable that we control for all autocorrelated idiosyncratic profit shocks.

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 ( $T, \mathbf{I}, \mathbf{S}$ ) may thus differ in unobserved dimensions, which results in selection bias even when conditioning on observed covariates, *i.e.* selection on unobservables [23]. 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.<sup>16</sup> Under assumption 2, we have:

$$\begin{aligned} & \mathbb{E}[Y_{it}^0 | D_i = 1, T_i, \mathbf{I}_i, \mathbf{S}_i] - \mathbb{E}[Y_{it}^0 | D_i = 0, T_i, \mathbf{I}_i, \mathbf{S}_i] \\ &= \mathbb{E}[l_0(T_i, \mathbf{I}_i, \mathbf{S}_i, \boldsymbol{\mu}_i, \boldsymbol{\delta}_i, \mathbf{e}_{it}, \mathbf{n}_{it}) | D_i = 1, T_i, \mathbf{I}_i, \mathbf{S}_i] \\ &\quad - \mathbb{E}[l_0(T_i, \mathbf{I}_i, \mathbf{S}_i, \boldsymbol{\mu}_i, \boldsymbol{\delta}_i, \mathbf{e}_{it}, \mathbf{n}_{it}) | D_i = 0, T_i, \mathbf{I}_i, \mathbf{S}_i] \end{aligned} \quad (11)$$

$$= \mathbb{E}[Y_{it'}^0 | D_i = 1, T_i, \mathbf{I}_i, \mathbf{S}_i] - \mathbb{E}[Y_{it'}^0 | D_i = 0, T_i, \mathbf{I}_i, \mathbf{S}_i], \quad (12)$$

where  $t'$  refers to a pre-treatment period. The first equality is a consequence of parts (i) and (ii) of assumption 2 ( $(\mathbf{e}_{it}, \mathbf{n}_{it})$  do not depend on the decision to participate). The second equality stems from the fact that  $(\mathbf{e}_{it}, \mathbf{n}_{it})$  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:

$$\mathbb{E}[Y_{it}^0 - Y_{it'}^0 | D_i = 1, T_i, \mathbf{I}_i, \mathbf{S}_i] = \mathbb{E}[Y_{it}^0 - Y_{it'}^0 | D_i = 0, T_i, \mathbf{I}_i, \mathbf{S}_i]. \quad (13)$$

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 between two pre-treatment years,  $t'$  and  $t''$ , where no effect should be detected.<sup>17</sup> Indeed, assumption 2 implies that equation (13) holds with  $t$  replaced by  $t''$ . We implement this test by setting  $t'' = 2003$ : the program is already in place at that date, but not all farmers have entered it. We use the farmers entering after 2003 to build the placebo test.

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

<sup>16</sup>The conditionality of direct subsidies to the implantation of grass buffer-strips is a case in point.

<sup>17</sup>Placebo tests were first implemented by Heckman and Hotz [21] 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 [18]) and part of what Angrist and Krueger [9] call refutability tests.

than non-participants. One way to do this is to replace part (iii) of assumption 2 by  $Y_{it}^0 = l_0(T_i, \mathbf{I}_i, \mathbf{S}_i, \boldsymbol{\mu}_i, \boldsymbol{\delta}_i, \mathbf{e}_{it}, \mathbf{n}_{it}) + m_0(p_t^Q, p_t^Y, w_t, T_i, \mathbf{I}_i, \mathbf{S}_i, \mathbf{e}_{it}, \mathbf{n}_{it}) + tk_0(T_i, \mathbf{I}_i, \mathbf{S}_i, \boldsymbol{\mu}_i, \boldsymbol{\delta}_i, \mathbf{e}_{it}, \mathbf{n}_{it})$ , for functions  $l_0$ ,  $m_0$  and  $k_0$ . Assumption 2 implies conditional independence in the rate of increase of the practices:

$$\begin{aligned} \mathbb{E} \left[ \frac{Y_{it}^0 - Y_{it'}^0}{t - t'} - \frac{Y_{it}^0 - Y_{it''}^0}{t - t''} \middle| D_i = 1, T_i, \mathbf{I}_i, \mathbf{S}_i \right] \\ = \mathbb{E} \left[ \frac{Y_{it}^0 - Y_{it'}^0}{t - t'} - \frac{Y_{it}^0 - Y_{it''}^0}{t - t''} \middle| D_i = 0, T_i, \mathbf{I}_i, \mathbf{S}_i \right], \quad (14) \end{aligned}$$

Under this assumption, the matching version of the triple-differences (DDD) estimator of Heckman and Hotz [21] yields an unbiased estimate of the treatment effect. We implement this estimator as an additional robustness check.

### The common support assumption

Finally, in order to apply the DID-matching estimator, there must exist non-participants having the same observed characteristics  $T$ ,  $\mathbf{I}$  and  $\mathbf{S}$  for each participant. The probability of not entering an AES must be strictly positive for all values of the observed characteristics. A sufficient condition for this to hold is:

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

Assumption 3 states that, for each level of the observed variables, some farmers have participation costs higher than the expected utility of entering the AES program. The set of values of  $\mathbf{I}$  and  $\mathbf{S}$  for which this assumption is satisfied is called the zone of common support [22].

This assumption has empirical content because 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. 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 [12].  $V$  acts thus as an unobserved instrumental variable: it determines treatment intake but is uncorrelated to time-varying determinants of potential outcomes.<sup>18,19</sup>

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

$$ATT = \mathbb{E} \left[ \mathbb{E} \left[ Y_{it}^1 - Y_{it'}^0 \mid D_i = 1, T_i, \mathbf{I}_i, \mathbf{S}_i \right] \right] - \mathbb{E} \left[ Y_{it}^0 - Y_{it'}^0 \mid D_i = 0, T_i, \mathbf{I}_i, \mathbf{S}_i \right]. \quad (15)$$

### 3.3 Definition of treatment effects with multiple treatments

In practice, farmers can choose among several AESs and may combine some of them. This makes no difference with respect to the way we have encoded our identification assumptions, but it requires some care in defining treatment effects. Let's suppose that there are two AESs,  $a$  and  $b$ , that farmers can enter either separately or jointly. AES  $a$  (resp.  $b$ ) is designed by assumption to alter practice  $Y_a$  (resp.  $b$ ).  $D_a$  (resp.  $D_b$ ) is a random variable equal to one when a farmer chooses to enter AES  $a$  (resp.  $b$ ) and zero otherwise. We can define four potential outcomes for each practice  $j \in \{a, b\}$ :

$$Y_j = \begin{cases} Y_j^{11} & \text{if } D_j = 1 \text{ and } D_{-j} = 1 \\ Y_j^{10} & \text{if } D_j = 1 \text{ and } D_{-j} = 0 \\ Y_j^{01} & \text{if } D_j = 0 \text{ and } D_{-j} = 1 \\ Y_j^{00} & \text{if } D_j = 0 \text{ and } D_{-j} = 0, \end{cases} \quad (16)$$

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<sup>18</sup>Note that in our framework, we do not need  $V$  to be independent of  $(\mu, \delta)$ : it is thus not an instrument for the level of agricultural practices but for their increments.

<sup>19</sup>In our setting, a zone of common support may exist even in the absence of an unobserved exogenous instrument. Variation in the expected unobserved net returns to the program, due to the unobserved (by us) fixed through time determinants of profits (productivity, tastes) is enough to generate variation in participation behavior. This variation is endogenous in the sense that it is correlated with the potential outcomes, but only through the unobserved fixed through time determinants. Because under the assumption of independence of increments, we can difference out the effect of these determinants, participation is not correlated to the increments in potential outcomes, thereby generating common support.

where  $D_{-j}$  refers to the AES that is not  $j$  (*i.e.*  $-j = b$  when  $j = a$ ).

Because we have access to data on individual AESs, we want to decompose the effect of each AES taken separately, with the aim of doing a cost-benefit analysis for each AES. We also want to focus on the impact of a given AES on the practice it aimed at altering. But farmers generally enter various AESs at the same time, making it difficult to separate out the effect of various AESs and to define a given treatment effect.

In practice, we say that a farmer benefits from AES  $a$  if she receives payments at least for this AES (she may also receive payments for other AESs). We define a farmer as being untreated if she receives no payment at all for any AES. So rigorously, the treatment effect we estimate is the average effect of taking AES  $a$  (and any other AES that in practice has been associated to it) on the practice it was meant to alter relative to taking no AES at all, for the farmers that take AES  $a$ :

$$ATT_a = \mathbb{E} [Y_a - Y_a^{00} | D_a = 1] \quad (17)$$

$$= \mathbb{E} [Y_a^{11} D_b + Y_a^{10} (1 - D_b) - Y_a^{00} | D_a = 1] \quad (18)$$

$$= \mathbb{E} [Y_a^{11} - Y_a^{00} | D_b = 1, D_a = 1] \Pr(D_b = 1 | D_a = 1) \\ + \mathbb{E} [Y_a^{10} - Y_a^{00} | D_b = 0, D_a = 1] \Pr(D_b = 0 | D_a = 1). \quad (19)$$

This parameter is a weighted average of the treatment effect of AES  $a$  and  $b$  taken together and of AES  $a$  taken alone on the respective subpopulations. In order to use this parameter in cost-benefit analysis, we make the assumption that only AES  $a$  (resp.  $b$ ) matters for practice  $Y_a$  (resp.  $Y_b$ ):

**Assumption 4** (No cross-effects). *For  $j \in \{a, b\}$ ,  $Y_j^{10} = Y_j^{11} = Y_j^1$  and  $Y_j^{00} = Y_j^{01} = Y_j^0$ .*

Under this assumption, there is no indirect effect of AES  $b$  on  $Y_a$ , and thus there are no complementarities between AESs  $a$  and  $b$ . We can thus proceed to a separate cost-benefit analysis for each AES because we have:

$$ATT_a = \mathbb{E} [Y_a^1 - Y_a^0 | D_a = 1]. \quad (20)$$

This assumption has some empirical content, so it can be tested:

- First, we can test whether there is a direct effect of AES  $b$  on  $Y_a$  by estimating whether  $\mathbb{E}[Y_a^{01} - Y_a^{00} | D_b = 1, D_a = 0]$  is equal to zero.
- Second, we can test whether there is any additional effect of AES  $b$  on top of AES  $a$  by estimating  $\mathbb{E}[Y_a^{11} - Y_a^{10} | D_b = 1, D_a = 1]$ .

## 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 the French ministry of Agriculture (named “STRU”<sup>20</sup>) 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.<sup>21</sup> 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

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<sup>20</sup>The extensive name of this survey is: *Enquête sur la Structure des Exploitations*.

<sup>21</sup>The construction of the database is extensively described in appendix C in the supplementary material. We pair administrative data on AES recipients to farm surveys. The pairing algorithm is extremely accurate (98% of the treated farmers are correctly identified). There nevertheless are a few recipient farms that we cannot identify and we may wrongly attribute an AES to a few non-recipient farms. This measurement error may yield some attenuation bias, so that our estimates are lower bounds on treatment effects.

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. Table 1 shows that roughly half the farmers

Table 1: Overlap between the AES studied in the paper

	0301	09	08	04	0201	0205	21	PHAE
09	903							
08	770	1 980						
04	591	775	797					
0201	203	201	217	206				
0205	53	119	130	125	9			
21	77	118	94	64	45	9		
PHAE	46	114	92	11	10	57	65	
AESs 19 and 20	33	133	106	16	8	1	87	308

taking AES 0301 also take AES 09, 40 % of the farmers taking AES 04 also contract AES 0301, 60 % of the farmers entering AES 08 also take AES 09 and 50 % of the farmers taking AES 0201 also take AES 0301. AES 0205 and 21 on the contrary are not generally highly associated with other AESs. 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.<sup>22</sup>

Table 2 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.

<sup>22</sup>There are also three different kinds of widely adopted treatments that may confound the effect of our AES. The first is the first component of the CAP, composed of direct subsidies. Every farmer is entitled to these payments, and they only depend on farm structure, so that by controlling on farm structure, we compare farmers having the same level of these payments. A second problem is the introduction of an indemnity for covering the soil in winter in one region in France during the period under study. We exclude farmers benefiting from this subsidy from our sample. Finally, the AES subsidizing extensive herding (AES 19, 20 and PHAE) have been very widely adopted. Notice from table 1 that these AESs do not overlap with the ones we study (that mostly concern cereal growers). We nevertheless re-estimate treatment effects after excluding those benefiting from AESs 19 and 20 and PHAE, and find no difference in treatment effects. We thank an anonymous referee for inviting us to clarify this point.

Table 2: Sample sizes and AES participation

AES	Restriction imposed	Treated	CS <sup>(a)</sup>	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.

## 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 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 AES 02 encouraging crop diversification is measured on four outcome variables: the area dedicated to the main crop and the proportion of the total usable arable area (UAA) it covers, the number of crops, and a crop diversity index.<sup>23</sup> 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 AESs 04 and 21 on these two variables is estimated by simple

<sup>23</sup>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.

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 AES 21 was conditional on not having any area cultivated under organic farming in 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. We have data on production factors (equipment, buildings, herd size and composition, composition and size of UAA, size of the labor force, age and education level of farm associates, etc.), and on the consumption side (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 AESs (1993-1999) and other agricultural policies.<sup>24</sup> The main unobserved variables are thus managerial ability, ecological preferences and prices.

Almost all our control variables are measured at the farm level. The only exceptions are the variables measuring altitude, slope, agro-environmental zone and soil carbon content, that are measured at the commune level,<sup>25</sup> for want of measures at the farm level. We use mean altitude and slope at the commune level to approximate these variables at the farm level. Communes are sufficiently small entities that this approximation does not entail a lot of error.

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<sup>24</sup>The extensive list of the variables is not presented but can be found in appendix D in the supplementary material.

<sup>25</sup>A French *commune* is roughly equivalent to a US county. There are approximately 36,000 *communes* in France.

## 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 testing for SUTVA, placebo tests and DDD estimates.

### 5.1 Practical implementation of DID-matching

The procedure we use is in line with the most recent developments in the literature on program evaluation as they are presented in Todd [48]. 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.<sup>26,27</sup> 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 [17].

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 [45], we define the zone of common support as the set of participants for which there exists a sufficient density of non-participants with the same value for the propensity score.<sup>28,29</sup> As shown in table 2, restriction to the zone of common support generally reduces the number of recipient farms by 10%. The maximum is reached with AES 21, for which a quarter of the

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<sup>26</sup>The extensive results are not presented but they can be found in appendix E in the supplementary material.

<sup>27</sup>As the validity of our estimates depends on our correct specification of the participation model, we test our parametric specifications against a nonparametric alternative using the specification test proposed by Shaikh, Simonsen, Vytlačil, and Yildiz [44]. Results do not reject the null that the model is correctly specified.

<sup>28</sup>The definition of the zone of common support is provided in more detail in appendix B.

<sup>29</sup>The graphs presenting the zone of common support for each AES are not shown but they can be found in appendix F in the supplementary material.

recipient farms have no untreated counterpart. In order to understand how the farms on the common support differ from the average recipient farm, we run probit regressions for the presence on the common support. Results indicate that recipient farmers remaining on the common support are older and have smaller farms and a lower education level than recipient farmers that are not on the common support.<sup>30</sup>

DID-matching amounts to applying the matching procedure to first-differenced outcome variables: in our application, the change in outcomes between 2005 and 2000.<sup>31</sup> The matching estimator we use 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.<sup>32</sup> 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 matching can be considered successful.<sup>33</sup>

Results from three DID-matching estimators are presented: the nearest-neighbor estimator based on a multivariate matching (NNM<sup>(1)</sup>); the nearest-neighbor estimator based on a univariate matching on the propensity score (NNM<sup>(2)</sup>); and the local linear matching estimator based on the propensity score (LLM). The details of the estimation procedures are presented in appendix B.<sup>34</sup> We mainly report and comment the results from LLM, known as the most efficient, but both NNM estimators yield similar results. We estimate

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<sup>30</sup>We thank an anonymous referee for suggesting this test.

<sup>31</sup>Note that treatment effects may depend on the time elapsed since the farmer entered the AES. We do not expect these vintage effects to be important, though, because the farmers have to cope with all the requirements of the AES from the beginning, and there is thus not a lot of scope for learning. We test this possibility by estimating, in 2003 and 2005, the treatment effect of various AES on farmers having entered the schemes before 2003. As expected we do not find evidence of strong vintage or learning effects: treatment effects mostly remain constant through time. The results are available upon request. We thank an anonymous referee for suggesting that we study vintage effects.

<sup>32</sup>See Imbens [26] for a detailed presentation of the various matching methods.

<sup>33</sup>As suggested by Rosenbaum and Rubin [39], we use standardized differences to assess the quality of our adjustment. Before matching, there are around 80 variable that exhibit large differences, whereas there is at most one large difference after matching with LLR. On the basis of these results, we can accept the fact that our LLR matching procedure is precise. The extensive results of the balancing tests are not presented but they can be found in appendix G in the supplementary material. We thank an anonymous referee for suggesting we implement balancing tests.

<sup>34</sup>NNM procedures use the four closest non-recipient neighbors of each recipient farms to build the counterfactual level of the practice. We implement LLM using an Epanechnikov kernel and we set bandwidth to .05. Setting a lower bandwidth of .02 yields similar results.

standard errors for LLM by using a bootstrap procedure. Abadie and Imbens [6] having shown that bootstrapping fails for NNM, we report Abadie and Imbens [5]’s asymptotic standard errors.<sup>35</sup>

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

Table 3 reports the LLM estimates of direct and cross effects of each AES on agricultural practices. Cross effects are estimated by focusing on farmers not receiving the AES having a direct effect on the practice, *i.e.* they correspond to  $\mathbb{E}[Y_a^{01} - Y_a^{00} | D_b = 1, D_a = 0]$ .<sup>36</sup> Table 4 displays the corresponding changes in the outcome variables for the treatment and control groups computed using NNM<sup>1</sup>, only for direct effects. We first discuss the effects of the AESs on crop diversity, then on carrying of pollutants (cover crops and grass buffer strips) and on conversion to organic farming.

### Effects of the AESs on crop diversification

Two AES are likely to directly affect crop diversification: AES 0201, which consists in introducing one new crop in the rotation, and AES 0205, which requires having at least four different crops in the rotation. Unlike the case above, recipients of AES 0201 are different from recipients of 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 2). These 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). This is not a strong effect, the diversity index varying from 0 to 1. On the contrary, these AESs have larger effects on the number of crops in

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<sup>35</sup>Chabé-Ferret [14] shows that controlling on pre-treatment outcomes may bias DID-matching estimates. As a robustness check, we run DID-matching without controlling for pre-treatment outcomes and find similar results.

<sup>36</sup>We have also estimated  $\mathbb{E}[Y_a^{11} - Y_a^{10} | D_b = 1, D_a = 1]$ , but estimates are imprecise because of low sample size. We nevertheless have enough power to reject cross effects of AESs other than 0301 on the planting of cover crops.

the rotation: they are responsible for the addition of a almost 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: it has only decreased by approximately 2 ha, *i.e.* only 3 % of UAA. Most of the rotation has thus remained unchanged and 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.

Cross effects of other AESs are generally lower than direct effects. All AESs seem to slightly increase the number of crops on the farm. AES 21 promoting organic farming adds .58 crops to the farm. Other AESs increase the diversity index, but they do not decrease the area covered by the main crop.

Table 3: Direct and cross effects of various AESs

	0201	0205	0301	04	08	09	21
Eveness	<b>.05*</b> (.03)	<b>.03</b> (.02)	.02*** (.006)	.03*** (.006)	.02** (.006)	.02*** (.006)	.07*** (.02)
Number of crops	<b>.85**</b> (.36)	<b>.65***</b> (0.23)	.29*** (.07)	.37*** (.07)	.22*** (.04)	.20*** (.05)	.58*** (.14)
Area under main crop (%UAA)	<b>-.03</b> (.03)	<b>-.03***</b> (.01)	-.006* (.004)	-.01*** (.004)	.002 (.004)	-.0007 (.004)	-.01 (.01)
Area under main crop (ha)	<b>-2.30*</b> (1.34)	<b>-1.51***</b> (.58)	1.21*** (.41)	-.68 (.62)	2.23*** (.36)	2.20*** (.35)	2.71** (1.19)
Cover crops (ha)	1.04 (.79)	1.08*** (.34)	<b>10.66***</b> (1.32)	.23 (.38)	-.01 (.54)	.20 (.60)	.31 (.35)
Grass buffer strips (m)	-119.91* (68.30)	192.45*** (44.64)	-7.49 (38.96)	<b>243.61</b> (149.24)	13.54 (29.67)	30.60 (28.35)	-17.10 (40.51)
Organic farming (ha)	-13.39 (45.64)	-6.58 (15.09)	-5.13 (21.07)	11.12 (26.33)	-.50 (12.86)	7.49 (18.93)	<b>46.41***</b> (0.13)
Under conversion (ha)	.30 (2.57)	3.96 (10.67)	-3.31* (1.85)	.08 (2.80)	-1.46 (1.73)	1.66 (2.88)	<b>4.41*</b> (2.52)

Note: results in bold are the estimates of the direct effect of each AES on the practice it is meant to alter. Cross effects are estimated on the subgroup receiving AES  $b$  but not receiving AES  $a$ , the one aiming at directly altering practice  $Y^a$ . Estimations use LLM with an Epanechnikov kernel and bandwidth set to .05. Standard errors are in parentheses and are estimated by 500 bootstrapped replications for direct effects and 100 replications for cross-effects. Asterisks denote statistical significance at 1 % (\*\*\*) , 5 % (\*\*) or 10 % (\*) level. UAA refers to Usable Agricultural Area.

## Effects of the AESs on the planting of cover crops and grass buffer strips

AES 0301, which subsidizes the introduction of cover crops in the UAA, and AES 04, which subsidizes planting grass buffer strips, aim at decreasing the transfer of pollutants (mainly nitrogen) to ground and surface water. Results displayed in table 3 show that AES 0301 have increased the area planted in cover crops, the average treatment effect on the treated being around 10 ha. Table 4 shows that, between 2000 and 2005, the area planted in cover crop by farmers receiving AES 0301 has increased by 13 ha, while it has increased by only 3 ha among similar non recipients.

The ATT for AES 04 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.

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

Outcome	AES	Treated	Controls	ATT	StdE
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
Cover crops (hectares)	0301	13.89	2.84	10.66 ***	0.18
Grass Buffer Strips (meters)	04	1018.40	553.68	423.64 ***	24.14
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<sup>(1)</sup> 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. StdE stand for standard errors. Asterisks denote statistical significance at 1 % (\*\*\*) , 5 % (\*\*) or 10 % (\*) level. UAA refers to Usable Agricultural Area.

We find evidence that the assumption of no cross-effects is supported by the data in the case of cover crops. AESs other than 0301 have no effect on the implantation of cover crops. AESs 0201 and 0205 have the largest cross effect: a very narrow increase of 1 ha of cover crops. We also find that AESs other than 04, 0201 and 0205 do not have any significant effect on the planting of grass buffer strips, thereby largely confirming the absence of cross effects. The positive effects of AES 0205 may indicate that some farmers have used cover crops or grass buffer strips to increase crop diversity on their farms.

### **Effects of the AESs on the 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, because the outcome variable is unobserved in 2000. As already argued, this is not likely to lead to a large bias since farmers entering this AES were 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 area dedicated to organic farming and the 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. Furthermore, we do not detect significant cross effects of other AESs on organic farming. In view of these results, the AES 21 appears to be the cause of 90% of the increase in area devoted to organic farming between 2000 and 2005.

### **5.3 Robustness checks: diffusion effects, placebo tests and DDD estimates**

In this section, we present the results of the tests of the the validity of our identifying assumptions. We focus in turn on diffusion effects, placebo tests and DDD matching estimates.

### **Tests of the validity of Assumption 1 (no diffusion effects)**

We test for the validity of SUTVA by adding the initial proportion of organic farmers, and farmers implanting cover crops, in the farmer's *canton* as control variables. A *canton* is larger than a *commune* (it usually is made of 3 to 5 of them) and thus likely represents the extent of a farmer's zone of influence. Adding this control variable barely changes our estimated treatment effects for organic farming (45.5 ha) and implantation of cover crops (10.5 ha). We take this as evidence that SUTVA is not rejected by the data.

### **Tests of the validity of assumption 2: placebo tests**

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 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.

For 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.

Table 5: Results of the placebo tests

Outcome	AES	Sample								
		post-Sept03	post-Mar04	post-Sept04	post-Mar05	post-Sept05				
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 (0.01)	*	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.)
Cover crops (ha)	0301	3.52 (0.60)	***	3.60 (0.60)	***	3.14 (0.69)	***	3.34 (0.80)	***	1.32 (1.02)
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 : asterisks denote statistical significance at 1 % (\*\*\*) , 5 % (\*\*) or 10 % (\*) level. Standard errors are in parentheses. Estimations use LLM with an Epanechnikov kernel and bandwidth set to .05. Standard errors are in parentheses and are estimated by 500 bootstrapped replications. Details on the 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.

### A lower bound on treatment effects: results from triple difference estimates

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.

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

Outcome	AES	DDD		DID		DID	
		Sep03-Mar05		Sep03-Mar05		whole sample	
		ATT <sup>(1)</sup>		ATT <sup>(2)</sup>		ATT <sup>(3)</sup>	
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)	***
Cover crops (ha)	0301	4.87 (1.26)	***	10.46 (0.97)	***	10.66 (1.32)	***
Organic land area	21	14.07 (10.11)		45.01 (6.98)	***	50.82 (2.79)	***

Note : ATT<sup>(1)</sup> refers to the triple-difference estimates, ATT<sup>(2)</sup> refers to the DID-matching estimates on the same sample (farmers who have entered the AES between September 2003 and March 2005), and ATT<sup>(3)</sup> refers to the DID-matching estimates on the whole sample (farmers who have entered the AES before March 2005). UAA refers to Usable Agricultural Area. Estimations use LLM with an Epanechnikov kernel and bandwidth set to .05. Standard errors are in parentheses and are estimated by 500 bootstrapped replications.

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 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.

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. 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 How much green for the buck? A tentative cost-benefit analysis**

In this section, we insert our estimates of additionality and windfall effects into a cost-benefit framework. We analyze each AES separately, and we take into account direct effects only, which is reasonable in view of the limited extent of cross effects among AESs that we find. We first present a simple framework integrating *ATT* and windfall effects

into a cost-benefit framework. We define a threshold in the cost/benefit ratio such that the AES is efficient if the social benefit from the practice it promotes is superior to that threshold. Second, we calculate this cost/benefit ratio for each of the AESs under study. To do so, we combine our ATT estimates with data on costs extracted from the administrative files. Third, we compare the cost/benefit threshold to estimates taken from the literature of the social benefit generated by the various agricultural practices we study. The results from these calculations are presented in table 7.

### A framework for cost-benefit analysis

Under assumptions SUTVA and no cross-effects, the variation of social welfare due to the implementation of a given AES can be measured by the sum of the compensating variation of farmers and consumers. However, we do not study farmers' surplus in this paper, because we lack data on profits<sup>37</sup> and thus focus on consumer's surplus. We adopt a taxpayer's view on the program and we investigate sufficient conditions for the program social benefits to exceed its costs. We assume that the benefit from a practice is proportional to the average level of this practice.<sup>38</sup>

$B$  measures the social benefit from on unit of the practice  $Y$ . The total benefit generated under the scheme is thus:  $\mathbb{E}[Y^1|D = 1] B$ , where  $\mathbb{E}[Y^1|D = 1]$  is the area under the practice in the average treated farm when it receives treatment. We consider only the direct costs of the program, *i.e.* direct payments to farmers. We thus disregard administrative costs and deadweight loss due to taxation. Costs associated to the scheme are thus per hectare payments ( $C$ ) multiplied by total area for which the farmer receives payment:  $\mathbb{E}[Y^p|D = 1] C$ .  $Y^p$ , the area for which the farmer gets paid, can be different from  $Y^1$ . It can be lower if the farmer declares more than what she implants or higher if the total area under the practice is capped and there are increasing returns to the practice at the farm level. When the treatment is implemented, the net benefit is thus:  $\mathbb{E}[Y^1|D = 1] B - \mathbb{E}[Y^p|D = 1] C$ . This has to be compared with the benefit that

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<sup>37</sup>Because there is free entry into the program, we can reasonably suppose that the average farmer's surplus is positive. This is implied by our model, conditional on  $\mathcal{I}$ .

<sup>38</sup>We thus disregard the potentially important question of the spatial distribution of treatment effects. We leave this for further research.

would have been reached had the program not been implemented:  $\mathbb{E}[Y^0|D = 1] B$ , where  $\mathbb{E}[Y^0|D = 1]$  is the counterfactual level of practice  $Y$ . Consumer surplus from the AES is thus equal to:

$$CS = \mathbb{E}[Y^1|D = 1] B - \mathbb{E}[Y^p|D = 1] C - \mathbb{E}[Y^0|D = 1] B \quad (21)$$

$$= \mathbb{E}[Y^1|D = 1] B - \left( \mathbb{E}[Y^p - Y^1|D = 1] - \mathbb{E}[Y^1 - Y^0|D = 1] - \mathbb{E}[Y^0|D = 1] \right) C - \mathbb{E}[Y^0|D = 1] B \quad (22)$$

$$= \underbrace{\mathbb{E}[Y^1 - Y^0|D = 1]}_{ATT} (B - C) - \left( \underbrace{\mathbb{E}[Y^p - Y^1|D = 1]}_E + \underbrace{\mathbb{E}[Y^0|D = 1]}_W \right) C, \quad (23)$$

After rearranging, equation (23) shows that consumer surplus depends on the level of additionality of the program, measured by the  $ATT$ , and on the level of discrepancy or declarative error  $E$  and the windfall effect  $W$ . The AES is cost-effective whenever the social benefit  $B$  is superior to a threshold  $B^*$ , with:

$$B^* = \frac{ATT + W + E}{ATT} C \quad (24)$$

where  $B^*$  increases with  $W$  and  $E$  and decreases with  $ATT$ . This formulation emphasizes the mechanism by which the additional and windfall effects determine the cost-effectiveness of an AES. French AESs are not designed to avoid windfall effects. A farmer who chooses to participate in an AES commits to reach a certain level of the practice but this may not require that she actually alters the practice to achieve compliance. In extreme cases, the counterfactual level of practice meets the required level; it happens to be the same in the presence of the scheme as that in the absence of scheme. In that case, the  $ATT$  is null and the windfall effect is maximum. Note that  $B^*$  is also a measure of the cost/benefit ratio, *i.e.* the cost per hectare of additional treatment effect. Indeed, The numerator of  $B^*$  can be equal to total payments received by the average recipient of an AES:  $(ATT + W + E)C = \mathbb{E}[Y_1^p|D = 1] C = P$ .

## Measuring the cost per hectare of additional treatment effects

As a first step toward a cost-benefit analysis, we calculate the cost per hectare of additional treatment effect ( $B^*$ ) for the different AESs. We measure  $C$  directly by dividing total payments by the total area under contract for each farmer.  $ATT$  comes from the LLR estimates of the previous section.  $W$  is calculated as the difference between the observed level of the practice and the  $ATT$ .  $E$  is the difference between the level of the practice for which the farmer gets paid (*i.e.* the total area under the AES) and the level we measure in the 2005 farm survey. As an intermediate step, we also calculate the cost per hectare of observed area under the practice ( $C_2$ ), by dividing average payments by the average observed area under the practice.<sup>39</sup> As reported in table 7, in the case of AES 0301 (implanting cover crops), the average area under contract (21 ha) is slightly larger than what we actually measured from survey data (17 ha), which suggests that some farmers committed to implant more cover crops than what they actually did.<sup>40</sup> This translates in a higher cost per observed (81€) than per declared (68€) implanted area. Moreover, the additional treatment effect (11 ha) is equal to 60% of the implanted area under cover crops, so that the windfall effect (6.58 ha) is large. Thus, almost 40% of the implanted cover crops area would have been sown by participants, even in the absence of AES 0301. Mechanically, this windfall effect translates into a larger cost per implanted area than per subsidized area: we indeed estimate a cost of 131 € per additional hectare of cover crops, while the mean premium for such AES is only 68 € per hectare.

In the case of AES 21, farmers converted more area to organic farming than what they get paid for, so that  $E$  is negative. This is probably due to a combination of increasing returns with an informal cap on subsidized area. The cost per implanted area is thus lower than the cost per subsidized area. There is nevertheless a positive windfall effect: in the end, the cost per additional treatment effect is slightly lower than the cost per subsidized area. AES 21 is thus very cost-effective.

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<sup>39</sup>For the sake of consistency, we focus on units lying on the common support.

<sup>40</sup>This discrepancy could also stem from the slight remaining measurement error in the AES variable or from slightly different definitions of the area planted in cover crops between farm surveys and administrative data.

For grass buffer strips (AES 04), in order to compare our estimates to data from administrative records, we convert the *ATT* into hectares, under the assumption that a grass buffer strip is 10 meters wide. Surprisingly, the average area under contract appears five times larger than what data from survey suggest.<sup>41</sup> Moreover, there is a large windfall effect and thus a very small treatment effect. This translates into a cost of almost 1800 € per additional ha of grass buffer strips, while the mean premium for such AES is only 93 € per ha.

We cannot apply formula (23) to AESs 0201 and 0205 because payments are not tied to a given practice. We calculate the cost per additional area not under the main crop. AES 0201 (resp. 0205) reduces area under the main crop by 2.30 ha (resp. 1.51 ha) on average. This translates into a cost per additional area not under the main crop of 990€/ha (resp. 2900€/ha).

Table 7: Cost-benefit analysis of various AESs on the average treated farm

AES	0201	0205	0301	04	21
Payment (€) ( $P$ )	2 271	4 356	1 392	421	7 667
Area under contract (ha) ( $\mathbb{E}[Y^P D=1]$ )	13.50	124.75	20.54	4.51	47.20
Observed area under the practice (ha) ( $\mathbb{E}[Y^1 D=1]$ )			17.24	1.02	54.71
Declarative error (ha) ( $E$ )			3.30	3.49	-7.51
Additional treatment effect (ha) ( $ATT$ )	2.30	1.51	10.66	0.24	50.82
Windfall effect (ha) ( $W$ )			6.58	0.78	3.89
Cost per area under contract ( $C$ )	168	35	68	93	162
Cost per area under the practice ( $C_2$ )			81	413	140
Cost per unit of additional treatment effect ( $B^*$ )	987.37	2884.77	131	1 754	151
Social benefit per unit of additional treatment effect ( $B$ )			$0.7*N_a$	$1.6*N_b$	540

Note: we cannot apply formula (23) to AESs 0201 and 0205 because payments are not tied to a given practice. We calculate the cost per additional area not under the main crop, obtained from estimates not presented in the previous sections.  $N_a$  is the number of units of N-fertilizer whose leaching is prevented by one hectare of cover crops.  $N_b$  is the number of units of N-fertilizer whose runoff is prevented by one meter of grass buffer strips. Sample: treated farms on the common support.

<sup>41</sup>We do not think that this discrepancy can be explained by measurement error in the AES variable, because remaining measurement error is quite small. This discrepancy may of course be due to an underestimation of the strip width. And yet, we cannot reasonably suppose that grass buffer strips are 50 meters wide on average - although we are not able to check this from available data. People in charge of conducting the farm surveys acknowledged that there is a large measurement error in the measure of the length of grass buffer strips. This is the most likely explanation of the large discrepancy we find.

## Toward a cost-benefit analysis

In order to achieve a complete cost-benefit analysis, we have to compare the unit costs to estimates of the social benefit of the practices promoted by each AESs. In table 7, we provide estimates of the social benefit  $B$  taken from the literature. Subsidizing the conversion to organic farming appears highly cost effective: it costs 151 € per hectare, whereas average social benefit from organic farming is usually thought to be higher. Indeed, Sandhu, Wratten, Cullen, and Case [42] estimate the average benefit of organic farming relative to conventional farming to be of 540 € per hectare per year.<sup>42</sup> According to these figures, the benefits from implementing AES 21 would largely offset its cost.

On the contrary, the comparison of the costs of AES 0301 subsidizing the planting of cover crops cost (131 € per additional hectare) to an estimate of the social cost of one kilogram of N-fertilizer leaching from the field provided by van Grinsven, Rabl, and de Kok [49] - 0.7 € per kg - suggests a poor cost-efficiency of AES 0301. Indeed, achievement of cost-efficiency for this AES would require that one hectare of cover crops prevents the leaching of 187 kg of N-fertilizer, which seems highly unrealistic. However, taking into account the value of avoided phosphorus leaching and of increased biological and landscape diversity could improve the cost-effectiveness of this measure.

We calculate that subsidizing the implantation of grass buffer strips costs 1800 € per additional ha. Lankoski and Ollikainen [30] uses an estimate of 1.6 € per kg of N-fertilizer for the social cost from nitrogen runoff. To reach cost-effectiveness, buffer strips have thus to prevent the runoff of 1.1 kg of N-fertilizer per meter. Grass buffer strips have a very high reported efficiency at curbing nitrogen runoffs. Cost-effectiveness of this AES depends thus on the size of the watershed that leads to the buffer strip. For example, with an assumed 80% efficiency of the buffer strip and runoffs of 14 kgN/ha, one kilometer of a 10-m width buffer strip has to have a cropped watershed of 100 ha to ensure that social benefits from this AES exceed its costs. Moreover, we should account for reduced runoff of phosphorus and pesticides and for increased biodiversity. It thus seems that despite high associated windfall effects, AES 04 could very well be cost-effective.

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<sup>42</sup>We use an exchange rate of \$1.5 per €. Their estimate is \$810 per hectare and per year.

## 7 Conclusion

In this paper, we estimate the causal effects of various French AESs subsidizing the adoption of practices more favorable to the environment. We use DID-matching to estimate the causal effects of each separate AES. We argue that the economics of the program make it likely that the identifying assumptions of DID-matching hold in our data. We also test and find support for the validity of these assumptions. We also examine cross effects of the AESs on practices there were not meant to alter and find that these effects are smaller than the direct effects, and generally null.

Overall, we find that the French AESs are characterized by large windfall effects. The AESs subsidizing crop diversity are taken by farmers that have very diverse rotations at the beginning of the program. They add on average a little less than one more crop to the rotation thanks to the AES, but they do so on a very limited portion of their farm. Therefore, the effect of the AESs promoting crop diversity on a crop diversity index and on the area planted in the main crop is small. We estimate that the AES subsidizing the planting of cover crops suffers from a 40 % windfall effect.<sup>43</sup> The cost-effectiveness of this AES is thus low: we estimate that, with this AES, it costs 131 € to induce the planting of one hectare of cover crops, almost doubling the average cost per subsidized area planted in cover crops (68 €/ha). Compared with estimates of social benefits from avoided nitrogen leaching taken from the literature, the benefits from this AES does not appear to be higher than its costs. According to our estimates, the AES subsidizing the planting of grass buffer strips suffer from a 75 % windfall effect. Despite its very low cost-effectiveness at increasing the planting of grass buffer strips, the social benefit from this AES may very well be positive at least in some part of France because of the very high efficiency of grass buffer strips at curbing nitrogen runoffs. Finally, according to our results, the AES subsidizing the conversion to organic farming appears to be highly cost-effective and to increase social welfare. Indeed, this AES has low windfall effects. We estimate that the cost per additional hectare converted to of organic farming thanks to this AES is of 151 €/ha. This is much lower than the estimates of the social

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<sup>43</sup>Note that we do not include the declarative error in our measure of the windfall effect.

benefits from organic farming estimated by Sandhu, Wratten, Cullen, and Case [42]: 540 €/ha. Apart from the large social benefits from organic farming, the source of the high cost-effectiveness of this AES resides in the stringency of its eligibility conditions. Indeed, only non organic farmers could benefit from this AES. According to our estimates, conversions to organic farming due to this AES account for 90 % of the almost doubling of the area farmed organically in France between 2000 and 2005. The French government has recently increased the total amount dedicated to these subsidies. At the same time, eligibility to these subsidies has been extended to all organic farmers, whatever their date of conversion. This may jeopardize the reported high efficiency of this AES by opening up the possibility for windfall effects. This new policy could nevertheless have a positive effect on the duration of the conversion to organic farming.

We have not been able to estimate whether farmers went back to conventional farming at the end of their 5-year contract because of the insufficient time scale of our data. We leave this for further research. Another research direction we are currently exploring is using variations in the level of assistance received by farmers across *départements* in an instrumental variable strategy.<sup>44</sup> Much remains to be done to improve the insertion of treatment effects estimates into a fully-fledged cost-benefit framework. Estimating farmers's surplus from the AES would be a first step. More importantly, the environmental effect of greener practices might vary through space. Estimating the spatial distribution of treatment effects is thus an interesting avenue for further research. Finally, estimating the treatment effects of the AESs directly on the environment remains an essential but very difficult undertaking. Kleijn, Baquero, Clough, Diaz, Esteban, Fernandez, Gabriel, Herzog, Holzschuh, Johl, Knop, Kruess, Marshall, Steffan-Dewenter, Tschardtke, Verhulst, West, and Yela [29] provide evidence that AESs in the EU enhance common biodiversity. To our knowledge, we lack the same type of evidence for the effects of AESs on water quality. Recent work on very detailed Finnish data is a step in this direction [1].

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<sup>44</sup>We thank an anonymous referee for suggesting we pursue in this direction.

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## A Sample size and AES participation

AES		Treated	CS <sup>(a)</sup>	Non treated	Sample
<i>Panel B: used for placebo tests on the post-sep03 treated</i>					
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
<i>Panel C: used for placebo tests on the post-mar04 treated</i>					
0301	Implanting cover crops	727	641	58,586	59,227
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
<i>Panel D: used for placebo tests on the post-sep04 treated</i>					
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
<i>Panel E: used for placebo tests on the post-mar05 treated</i>					
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
<i>Panel F: used for placebo tests on the post-sep05 treated</i>					
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
<i>Panel G: used for triple-difference matching estimates (sep03-mar05)</i>					
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

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 <sup>(a)</sup>	Non treated	Sample
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 appendix 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 [38] 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 [45]. The cut-off is determined so that some overall trimming level is attained.<sup>45</sup> 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:

$$\widehat{\mathbb{E}}[Y^1 - Y^0|D = 1] = \frac{1}{n_1} \sum_{i \in I_1 \cap S_P} (Y_{it}^1 - Y_{it'}^0 - \widehat{\mathbb{E}}[Y_{it}^0 - Y_{it'}^0|D = 1, X_i]) \quad (25)$$

with

$$\widehat{\mathbb{E}}[Y_{it}^0 - Y_{it'}^0|D = 1, X_i] = \sum_{j \in I_0} W_{ij} (Y_{jt}^0 - Y_{jt'}^0) \quad (26)$$

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  $I_1$ .

<sup>45</sup>In practice, we first define the set of positive densities:  $\widehat{S}_P = \{i : \widehat{f}(P(X_i)|D_i = 1) > 0 \text{ and } \widehat{f}(P(X_i)|D_i = 0) > 0\}$ . The common support group is then the following set:  $\widehat{S}_q = \{i \in I_1 \cap \widehat{S}_P : \widehat{f}(P(X_i)|D_i = 1) > c_q \text{ and } \widehat{f}(P(X_i)|D_i = 0) > c_q\}$ , where the cutoff level  $c_q$  is chosen as the solution to the following problem:  $\sup_{c_q} \frac{1}{2J} \sum_{i \in I_1 \cap \widehat{S}_P} (\mathbb{1}[\widehat{f}(P(X_i)|D_i = 1) < c_q] + \mathbb{1}[\widehat{f}(P(X_i)|D_i = 0) < c_q]) \leq q$ , where  $I_1$  is the group of participants and  $J$  is the number of participants in  $\widehat{S}_P$ . In our applications, we choose  $q = 0.05$ .

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 [26]. The nearest-neighbour matching (NNM) used in the analysis is a multivariate matching based on the distance between vectors  $X_j$  and  $X_i$ .<sup>46</sup> Such estimator matches each participant  $i$  to its "closest" non-participants  $j$ . Both multivariate nearest neighbor matching estimator (matching on  $X$  covariates) and univariate NNM estimator (matching on propensity score) matches each participant to the four closest non-participants. We use Sekhon [43]'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 distance between propensity scores. The weighting function for LLM is given by:

$$W_{ij} = \frac{G_{ij} \sum_{k \in I_0} G_{ik} (P_k - P_i)^2 - [G_{ij} (P_j - P_i)] [\sum_{k \in I_0} G_{ik} (P_k - P_i)]}{\sum_{j \in I_0} G_{ij} \sum_{k \in I_0} G_{ik} (P_k - P_i)^2 - [\sum_{k \in I_0} G_{ik} (P_k - P_i)]^2} \quad (27)$$

with  $G_{ij} = G(\frac{P_i - P_j}{h})$ , where  $G$  is a kernel function and  $h$  a bandwidth parameter [48].<sup>47</sup> Heckman, Ichimura, and Todd [22] 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 [20]. We programmed this estimator in R. We set the bandwidth to .05. We experimented with a lower bandwidth of .02 without altering the results.

## 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.<sup>48</sup> 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

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<sup>46</sup>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].

<sup>47</sup>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:  $\min_{a,b} \sum_{j \in I_0} ((Y_{jt}^0 - Y_{jt'}^0) - a - b(P_i - P_j))^2 G_{ij}$ .

<sup>48</sup>The detail procedure is given by Abadie, Drukker, Herr, and Imbens [3]. One must estimate the regression functions for the controls:  $E(Y^0|X = x) = \hat{\beta}_0 + \hat{\beta}_1'x = \hat{\mu}_0(x)$  with  $(\hat{\beta}_0, \hat{\beta}_1) = \arg \min \sum_{i: D_i=0} K_M(i)(Y_i - \beta_0 - \beta_1'X_i)^2$  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:  $\hat{Y}_i^0 = \frac{1}{\#J_M(i)} \sum_{j \in J_M(i)} (Y_j + \hat{\mu}_0(X_i) - \hat{\mu}_0(X_j))$  where  $\#J_M(i)$  is the number of units in the group of  $M$  matches  $J_M(i)$ .

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:

$$\hat{V} = \frac{1}{N_1^2} \sum_{i=1}^N \left[ D_i (Y_i^1 - \hat{Y}_i^0 - \hat{\tau})^2 + (1 - D_i) (K_M^2(i) - K'_M(i)) \hat{\sigma}_{D_i}^2(X_i) \right] \quad (28)$$

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