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

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ARTICLE INFO

ABSTRACT

Agro-environmental schemes (AES), which pay farmers to adopt greener practices, are increasingly important components of environmental and agricultural policies both in the US and the EU. Here 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 derive the statistical assumptions underlying DID-matching from a structural household model and we argue that the economics of the program make it likely that these assumptions hold in our data. We test the implications of the identifying assumptions, provide a lower bound using triple-difference matching, test for crossover effects and insert our estimates of both additionality and windfall effects into a cost-benefit framework. We find that the AESs promoting crop diversity have inserted one new crop into the rotation but on a small part of the cropped area. We also find that the AES subsidizing the planting of cover crops has increased cover crops by 10 ha on the average recipient farm at the expense of almost 7 ha of windfall effect. This AES does not appear to be cost effective. In contrast, we find that the AES subsidizing grass buffer strips could be socially efficient despite large windfall effects. We finally estimate that the AES subsidizing conversion to organic farming has low windfall effects and high additionality.

1. Introduction

Payments for environmental services are widely used to improve environmental outcomes. Agro-environmental schemes (AESs), which pay farmers for adopting greener practices, 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 to alter agricultural practices in order to improve the environment. Two of the AESs aim to increase crop diversity, which in turn may increase the diversity of habitats, and thus biodiversity. Increased crop diversity may also reduce the resistance of weeds 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. We also study an AES that subsidizes the planting of grass buffer strips along rivers and streams. Grass buffer strips contribute to the

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improvement of surface water quality by curbing nitrogen, phosphorus, and pesticide runoff from fields. Finally, we study an AES that subsidizes conversion to organic farming. Organic farming bans the use of chemical fertilizers and pesticides, thereby reducing the transmission of pollutants into ground and surface water.

Cost-benefit analysis of these programs hinges on the relative extent of their additional and windfall effects. An AES has an additional effect if it encourages farmers to adopt environmentally friendly 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 to pay for practices that would have been adopted anyway, and thus decreases the benefit/cost ratio. Because AESs are voluntary programs and requirements and 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-select into an AES would in any case have adopted the subsidized green practice to some extent had the AES not been implemented. In this paper, 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 strips, and organic farming) recorded in 2005, five years after the beginning of the program. We also have data on farm and farmers' characteristics and practices before the program started. Finally, we have detailed and disaggregated information from administrative sources on the AESs that each farmer has entered.

Determining the average level of a given practice for recipient farmers had the AES not existed, i.e. the counterfactual level, is key to the estimation of both additional and windfall effects. The windfall effect is identical to the counterfactual level. The average treatment effect on the treated (ATT) - the relevant causal effect measuring additionality - is the difference between the average level of a practice in the presence of the AES and the counterfactual level of the same practice. Unfortunately, we cannot observe the counterfactual situation. This is an instance of the fundamental problem of causal inference [16]. If we try to approximate the counterfactual level for recipient farmers by using non-recipient farmers, our estimates of the ATT are likely to be affected by selection bias. As a consequence, we may overstate the true level of additionality. Profit-maximizing farmers self-selecting into an AES indeed have lower costs of complying with the AES requirements. It is therefore likely that farmers who choose to enter an AES would in any case have adopted greener practices than farmers not entering it, had the AES not been implemented.

We use difference-in-difference (DID) matching [2,15] to eliminate 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 eliminates selection bias due to observed covariates by comparing recipient farmers to similar non-recipients. First-differencing eliminates selection bias due to time-invariant unobservable factors. 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 derive the statistical assumptions underlying DID-matching from a structural household model and we argue that the economics of the program make it likely that the identifying assumptions of DID-matching hold in our data. 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 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 [32]'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 compare future recipients to future non-recipients at two different dates. We find effects of smaller magnitude than the ATT, and evidence that these are anticipation effects: because the date at which the requirements will become really binding is uncertain, farmers start complying with the requirements early on. Indeed, these anticipation effects vanish when we look at recipients who 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, we test and find strong support for the absence of sizeable crossover effects for most AESs under study.

We find that the average recipient farm has planted 10 additional hectares of cover crops, at the expense of almost 7 ha of windfall effect. Because the per-hectare payment for this AES is quite high, and because the social value of cover crops is limited, this AES does not appear to be cost effective. On the contrary, we find that the AES subsidizing grass buffer strips could very well be cost effective, 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 0.65–0.85 new species on treated farms, but on a very limited proportion of the total farmland, resulting in a small decrease in the

proportion of farmland covered by the main crop (-3%), as well as a slight increase in the crop diversity index. The modest aims of these AESs, only requiring farmers to add one crop to the rotation, might explain the very limited effects measured. Overall, we find strong evidence of adverse selection, which induces large windfall effects. We find that the AESs combining restrictive requirements 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 [24] and Lynch et al. [23], who focus on the impact of these AESs on land prices. Wu [37] and Roberts and Bucholtz [26] 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 [33] 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 et al. [38] 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 [27] 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 was 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 to estimate the effects of the US Environmental Protection Agency (EPA) voluntary programs or voluntary international standards (e.g. ISO14001) on firms' environmental performances. They run a linear 2SLS regression on micro-data to estimate the impact of voluntary programs on the release of toxins and on the economic performance of firms in the US [4,18] and in developing countries [8]. Arimura et al. [5] use maximum simulated likelihood along with the GHK simulator to estimate the impact of the implementation of ISO14001 and publication of environmental reports on the environmental performance of Japanese facilities. The paper which is perhaps the closest to our own is the study by Pufahl and Weiss [25] of the effect of benefiting from at least one AES on farm sales, fertilizer expenditure, and cattle livestock density measured from the bookkeeping records of a non-representative sample of German farms. This study shows that AESs decreased the use of agrochemicals and increased grassland area.

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

AESs accounted for 37% of rural development spending for the common agricultural policy (CAP) of the European Union in 2006 [25]. The future reform of the CAP will involve a major "greening" of all subsidies. As a result, a growing share of CAP spending will take the form of AESs. Taken together, the AESs we study accounted for 22% of total spending on AES in France in 2006.

AESs are five-year contracts, with yearly payments and possible checks of how well the requirements are being met. Farmers may enroll only part of their farm under an AES, and combine different AESs on the same part of their farm or on different parts. Farmers receive the same payment 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 to which the farmer declares she will apply the scheme. In this paper, we focus on seven AESs. AES 0301 (resp. 04) subsidizes the planting of cover crops (resp. grass buffer strips) and thus contributes to the reduction of nitrogen, phosphorus, and pesticide leakage (resp. runoff) from fields. This in turn decreases the concentrations of pollutants in surface and ground waters. AESs 08 and 09 aim to decrease the levels of pesticides and nitrogen applied to the fields, which might also decrease leakage and runoff. AES 21 encourages conversion to organic farming, a practice that has been shown to be friendlier to the environment than conventional farming. AESs 0201 and 0205 both aim to increase the diversity of crop rotation, but the former requires the addition of one crop to the rotation whereas the latter requires that at least four different crops be grown on the farm.

Farmers who wanted to benefit from an AES during this period had to submit a written application containing an environmental diagnosis of their farm and the particular measures they were applying for. An administrative body 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 at least a year. In order to submit a valid application, farmers could obtain assistance from local union-run bodies 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, which formed part of a policy introduced by a left-wing government. In 2003, an unexpected surge in the number of applications led the newly elected government to temporarily freeze the scheme. 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 those farmers who had applied to the AES program; as a result they altered their practices before being officially recorded as beneficiaries.

3. Theoretical model and identification strategy

In this section, we model an agricultural household deciding whether or not to take part in a unique AES program and then choosing its level of input. Identification assumptions are then presented as restrictions on this model. Finally, we 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. Second, the household chooses the level of inputs that maximizes its utility in relation to the AES constraints. We solve this problem by backward induction, so that we first focus on production decisions and how the AES impacts them, and then consider the household's decision to enter the scheme.[1]

3.1.1. 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 possessed by the household, like physical and human capital and land, stored in the vector $\mathbf{I}$ and unobserved (by the evaluator but not by the farmer) factors like managerial ability, land quality, and climate variations, gathered in 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 over time (like managerial ability and land quality, noted as $\boldsymbol{\mu}$) and those that vary over time (like climate variations, noted as $\boldsymbol{e}$). We thus have $\boldsymbol{\epsilon} = (\boldsymbol{\mu}, \boldsymbol{e})$.

When a household has entered an AES ($D = 1$) it receives payments P as compensation for making restricted use of inputs Y, so that $Y \leq \overline{Y}$.[2] The household derives income from farming but also from working H^{off} hours off the farm for a wage w. It derives utility from consumption C, leisure L, on-farm work [22,10], and may exhibit a particular distaste for some inputs, due for example to ecological preferences. Heterogeneity in tastes is described by two vectors: observed consumption shifters (family size, age of children, etc.): $\boldsymbol{S}$ and unobserved taste shifters: $\boldsymbol{\eta}$. Here again we make a distinction between unobserved shifters that are fixed over time (like ecological preferences, taste for work on the farm, noted $\boldsymbol{\delta}$) and time-varying idiosyncratic taste shifters (like non-farm profit opportunities, noted $\boldsymbol{n}$). We thus have $\boldsymbol{\eta} = (\boldsymbol{\delta}, \boldsymbol{n})$. The problem the household faces is

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

subject to:

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

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

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

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

where T is the total time available to the household. The first order condition for the input level is (with λ^Y the Lagrange multiplier associated with the input constraint)

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

In the absence of the AES (i.e. when $D = 0$ in Eq. (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 household characteristics $\boldsymbol{S}$ and $\boldsymbol{\eta}$, as production decisions are not separable from consumption:[3]

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

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

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[1] We do not explicitly model the dynamic behavior of farmers. Dynamics could play an important role if there are large learning requirements for entering a scheme or if the sunk costs for changing practices are large. We do not think that this is the case 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 those who delayed could not enter the scheme at a reasonable cost. This is an application of the general result of Abbring and Heckman [3] that a structural dynamic model with the assumptions in Rust [30] implies conditional exogeneity that justifies matching in a dynamic framework.

[2] The discussion of our identification strategy derived from this special case extends to the other AESs we have studied.

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

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

Y^1 and Y^0 are called potential outcomes. The individual-level causal effect of the AES (Δ_Y) is the difference between the input level chosen by the household if it enters the AES and the input level it chooses if it does not enter the AES: $\Delta_Y = Y^1 - Y^0$. The observed input choice Y depends on whether or not the farmer has entered the AES: $Y = Y^1 D + Y^0(1-D)$. The individual-level causal effect of the AES is thus not observable, since only one of the two potential input choices is observed. This is an instance of the fundamental problem of causal inference [16].

The causal effect might vary across the population. Indeed, constrained households (for which $\lambda^Y > 0$) have to decrease their level of inputs in order to cope with the AES constraints ($\Delta_Y < 0$). Unconstrained households (for which $\lambda^Y = 0$) could enter the AES at no cost, i.e. without modifying their agricultural practices, so that the program has no effect on them ($\Delta_Y = 0$).[4] These households would thus benefit from a pure windfall effect; they receive a subsidy but do not have to change their practices at all in order to comply with the AES requirements.

In this paper, we try to recover the average treatment effect on the treated (ATT), which is the average effect of the AES on those who have chosen to enter it: $ATT = \mathbb{E}[\Delta_Y | D = 1]$. 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 entry cost than unconstrained households, the latter are likely to be more strongly 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.

3.1.2. Farmers' decision to enter the AES

Let V^1 and V^0 denote the indirect utility of the household when it is respectively in or out of the AES program, as defined by Eqs. (1)–(5). They depend on the same variables as Y^1 and Y^0. Let V denote the disutility of applying for the AES in the first period. 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 gain in utility is higher than the costs of application:

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

where $\mathcal{I}$ denotes the set of information available to the agents when deciding whether or not to participate in the AES. Selection bias arises because some determinants of farmers' participation stored in $\mathcal{I}$ are also determinants of input demands. As a consequence, participants and non participants will differ in terms of fixed factors of production ($\boldsymbol{I}$), land quality and managerial ability ($\boldsymbol{\mu}$), consumption shifters ($\boldsymbol{S}$), and ecological preferences ($\boldsymbol{\delta}$). Comparing them may thus overstate the causal effect of the program, as participants may use fewer inputs than non-participants in the absence of the program:

$$\mathbb{E}[Y|D=1] - \mathbb{E}[Y|D=0] = \mathbb{E}[Y^1|D=1] - \mathbb{E}[Y^0|D=1] + \mathbb{E}[Y^0|D=1] - \mathbb{E}[Y^0|D=0] \tag{10}$$

$$= ATT + \underbrace{\mathbb{E}[Y^0 | \mathbb{E}[V_1 - V_0 | \mathcal{I}] \geq V] - \mathbb{E}[Y^0 | \mathbb{E}[V_1 - V_0 | \mathcal{I}] < V]}_{\text{selection bias}}. \tag{11}$$

3.2. Identification strategy

Matching estimators assume that outcomes are mean independent of program participation conditional on a set of observable characteristics: $\mathbb{E}[Y^0|D=1,Z] = \mathbb{E}[Y^0|D=0,Z]$. However, for a variety of reasons there may be systematic differences between participants' and nonparticipants' outcomes in the absence of the program, even conditional on observables. This could lead to a violation of the identification conditions required for matching. A DID-matching strategy, as defined in Heckman et al. [15], allows for temporally invariant unobserved differences in outcomes between participants and nonparticipants that closely resemble fixed effects in panel data. Differencing the outcomes eliminates the selection bias due to these unobservable factors. The conditional parallel trend assumption that underlies DID-matching is: $\mathbb{E}[Y^0_t - Y^0_{t'} | D=1,Z] = \mathbb{E}[Y^0_t - Y^0_{t'} | D=0,Z]$, with t (resp. t') a post (resp. pre) treatment date. This means that observationally equivalent treated and non-treated individuals should exhibit the same change in input decisions in the absence of treatment, i.e. that their average difference in input use should be constant over time. DID-matching estimates are obtained by applying matching to the outcomes differenced with respect to a pre-treatment period. Three assumptions are needed to ensure that DID-matching recovers the ATT: the Stable Unit Treatment Value Assumption (SUTVA), the assumption of conditional parallel trends and the common support assumption. In what follows, we formulate these assumptions as restrictions on our model, discuss their relevance and propose tests of their implications.

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[4] Unconstrained households may also change their practices because of an income effect due to the payment P.

3.2.1. The stable unit treatment value assumption (SUTVA)

Rubin [29]'s SUTVA assumes that the program has no effect on non-participants. In our model, this is achieved through the following restriction:

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

Because the AESs that we study have a low take-up rate, and input and output prices are mainly determined on the world market, we do not expect the AESs to have any effects on input and output prices.[5] 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. Without any prior evidence for this assertion, we set up a test of the validity of SUTVA based on the proportion of neighboring farmers adopting a given practice before anyone enters a scheme.

3.2.2. The assumption of conditional parallel trends

A crucial identification assumption in DID-matching is that of parallel trends [2,15,25]. It states that, in the absence of the program, the average change in input use is the same among participants and observationally equivalent non-participants. In our economic model, the validity of this assumption requires the three following restrictions to hold:

Assumption 2 (*Conditional parallel trends*). The three following conditions must hold simultaneously:

(i) $\mathcal{I} = \{P, \overline{Y}, p^Q, p^Y, w, T, I, S, \mu, \delta\}$,
(ii) $(V, \mu, \delta) \perp\!\!\!\perp (e, n) | (T, I, S)$ and $(e, n) | (T, I, S)$ is identically distributed,
(iii) $\exists$ functions l_0 and m_0 such that: $Y^0 = l_0(T, I, S, \mu, \delta, e, n) + m_0(p^Q, p^Y, w, T, I, S, e, n)$.

Part (i) of Assumption 2 states that a farmer's decision to enter an AES does not depend on time-varying unobserved factors e (climate variations) or n (idiosyncratic wage variations). This ensures that selection for the program is based either on observed variables or on unobserved variables fixed over time. This assumption seems realistic because participation in AESs is decided two to five years before practices are observed, meaning that farmers may not be able to forecast the level of the transitory determinants of input use e and 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 (μ and δ). It also means that transitory variations in productivity 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 mean level for a given year. Part (ii) also requires time-varying idiosyncratic shocks to be identically distributed.

Part (iii) of Assumption 2 is a way to deal with the bias due to unobserved time-constant factors (μ and δ). 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). As a consequence, the average difference in practices between participants and observationally identical non-participants must be constant over time in the absence of treatment.

Under Assumption 2, in the absence of the AES, participants' and non participants' average practices follow parallel trends conditional on observed variables:

$$\mathbb{E}[Y_{it}^0 - Y_{it'}^0 | D_i = 1, T_i, I_i, S_i] = \mathbb{E}[Y_{it}^0 - Y_{it'}^0 | D_i = 0, T_i, I_i, S_i]. \tag{12}$$

Though it seems difficult to justify on theoretical grounds, Assumption 2 is fortunately testable. We use placebo tests that apply the identification strategy between two pre-treatment years, t' and t'', when no effect should be detected. We find some evidence that the common trend assumption may not hold in our data. We interpret this as anticipation effects. Another interpretation could be that farmers follow specific trends in the adoption of practices. If we weaken Assumption 2 and model input level as a linear random trend,[6] the matching version of the triple-differences (DDD) estimator of Heckman and Hotz [14] yields an unbiased estimate of the treatment effect. We implement this estimator as an additional check of robustness yielding a lower bound on the ATT.

3.2.3. The common support assumption

In order to apply the DID-matching estimator, non-participants having the same observed characteristics T, I, and S must exist for each participant. A sufficient condition for this to hold is:

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

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 T, I, and S for which this assumption is satisfied is called the zone of common support [15]. This assumption has empirical content because among households with the same

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[5] In 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.

[6] This alternative Assumption 2′(iii) is: $\exists$ functions l_0, m_0, and k_0 such that: $Y_t^0 = l_0(T, I, S, \mu, \delta, e, n) + tk_0(T, I, S, \mu, \delta, e, n) + m_0(p^Q, p^Y, w, T, I, S, e, n)$.

expected utility gain from entering the AES, some have relatively higher participation costs V because of relatively less substantial assistance from agricultural unions at the local level. V thus acts as an unobserved instrumental variable; it determines treatment intake but is uncorrelated to time-varying determinants of potential outcomes.

As a conclusion to this section, under Assumptions 1–3, DID-matching identifies the *ATT*:[7]

$$ATT = \mathbb{E}[\mathbb{E}[Y_{it} - Y_{it'} | D_i = 1, T_i, \boldsymbol{I}_i, \boldsymbol{S}_i] - \mathbb{E}[Y_{it} - Y_{it'} | D_i = 0, T_i, \boldsymbol{I}_i, \boldsymbol{S}_i] | D_i = 1]. \tag{13}$$

3.3. Definition of treatment effects with multiple treatments

In practice, farmers can choose from several AESs and may combine two or more 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 us suppose that there are two AESs, a and b and that farmers can enter either one or both. Let it be assumed that AES a (resp. b) is designed 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 $j \in \{a,b\}$

$$Y_j = \begin{cases} Y_j^{11} & \text{if } D_j = 1 \text{ and } D_{-j} = 1 \\ Y_j^{10} & \text{if } D_j = 1 \text{ and } D_{-j} = 0 \\ Y_j^{01} & \text{if } D_j = 0 \text{ and } D_{-j} = 1 \\ Y_j^{00} & \text{if } D_j = 0 \text{ and } D_{-j} = 0, \end{cases} \tag{14}$$

where D_{-j} refers to the AES that is not j (i.e. $-j = b$ when $j=a$). Given that farmers generally enter various AESs at the same time, 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 strictly speaking, for the farmers who take AES a, the treatment effect we estimate in this paper is the average effect of taking AES a (and any other AES that in practice has been associated with it) upon the practice it was meant to alter, relative to taking no AES at all:

$$ATT_a = \mathbb{E}[Y_a - Y_a^{00} | D_a = 1] \tag{15}$$

$$= \mathbb{E}[Y_a^{11} D_b + Y_a^{10}(1 - D_b) - Y_a^{00} | D_a = 1] \tag{16}$$

$$= \mathbb{E}[Y_a^{11} - Y_a^{00} | D_b = 1, D_a = 1] \Pr(D_b = 1 | D_a = 1) + \mathbb{E}[Y_a^{10} - Y_a^{00} | D_b = 0, D_a = 1] \Pr(D_b = 0 | D_a = 1). \tag{17}$$

This parameter is a weighted average of the treatment effect on the respective subpopulations of AES a and b taken together and of AES a taken alone. 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 crossover 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:

$$ATT_a = \mathbb{E}[Y_a^1 - Y_a^0 | D_a = 1]. \tag{18}$$

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 $\mathbb{E}[Y_a^{01} - Y_a^{00} | D_b = 1, D_a = 0]$.
- 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 database created especially for this study from a statistical survey of agricultural practices conducted in 2003 and 2005 by the statistical services of the French Ministry of Agriculture (named "STRU") paired to both the 2000 Agricultural Census ("CA-2000") and several administrative files recording information on the participation in each AES between 2000 and 2006. Creation of the database required a pairing procedure with several steps to deal with the scattering of the data. It is extensively described in an online appendix, which can be accessed via www.aere.org/journals. The sample extracted from "STRU" is representative of French farmers.

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[7] Alternatively, under Assumptions 1, 2′, and 3, DDD-matching identifies the *ATT*: $ATT = \mathbb{E}[\mathbb{E}[\Delta_{t,t',t''}^{Y_i} | D_i = 1, T_i, \boldsymbol{I}_i, \boldsymbol{S}_i] - \mathbb{E}[\Delta_{t,t',t''}^{Y_i} | D_i = 0, T_i, \boldsymbol{I}_i, \boldsymbol{S}_i] | D_i = 1]$ with $\Delta_{t,t',t''}^{Y_i} = Y_{it} - Y_{it'} - (t - t''/t' - t'')(Y_{it'} - Y_{it''})$.

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 meeting the requirements of the AES between 2000 and 2005, and a value of zero when the surveyed farmer does not appear in the administrative files between 2000 and 2005. Because farmers may benefit from several AESs, the participation variables partially overlap, as shown in the online appendix. The sample size and the number of participants for the AESs we study in this paper are reported in the online appendix. The sample contains between 400 and 3000 participants depending on the AES, which represents between 2000 and 14,000 participant farmers nationwide. We also have access to almost 60,000 non-participants, representing 540,000 farmers nationwide.

4.2. Definition of the outcome variables

Several outcome variables are associated with each AES under study. Two outcome variables allow us to estimate the impact of AESs 0301 and 04 which aim to reduce nitrogen carried by rainwater runoff: the surface area planted with 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 leaching. As cover crops may be a way to retain nitrogen during winter, we study whether farmers participating in AES 09, which aims to curb the use of nitrogen fertilizers, have planted more cover crops, even when not participating in AES 0301. The impact of AES 02, which aims to encourage crop diversification, is measured by 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 an index of evenness. Finally, we use two outcome variables to estimate the impact of the AES which aims to encourage conversion to organic farming; 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 (not measured in 2000) and the area covered by grass buffer-strips (not measured in 2000 nor 2003).

4.3. Definition of control variables

The richness of the information in our database enables us to control for most of the important determinants of input choice and of 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 the technical orientation of the farm, quality labels, past experience with previous AESs (1993–1999) and other agricultural policies.[8,9] The main unobserved variables are thus managerial ability, ecological preferences, and prices. All our control variables are measured at the farm level with the exception of altitude, slope, agro-environmental zone, and soil carbon content, which are measured at the commune level.[10]

5. Results

In this section, first we present the practical implementation of DID-matching; then we present and discuss the results of this estimation procedure. Finally, we 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 [34]. As they are not a genuine contribution of this paper, the econometric methods are presented in the online appendix. 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.[11] 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 previous experience with an AES. Whereas previous experience with quality labels tends to increase participation in AES 21, technical orientation toward growing cereals increases participation in all the AESs studied in this paper except

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[8] The full list of variables can be found in the online appendix. As Chabé-Ferret [7] shows that controlling for pre-treatment outcomes may bias DID-matching estimates, we also run DID-matching without controlling for pre-treatment outcomes and find similar results.

[9] Direct subsidies from the CAP are a function of farm structure, which we control for. We exclude from the sample farmers benefiting from a special indemnity for covering the soil in winter that is not a part of the AES program. Finally, our results are not sensitive to the exclusion of the small number of farmers also benefiting from AESs subsidizing extensive livestock rearing.

[10] There are approximately 36,000 communes in France. The average size of a French commune is around 7 sq.mile, which is a little less than half of the average size of a US Census Block Group. Using commune level data thereby provides a good enough approximation for individual farm characteristics like altitude and slope without generating large measurement errors.

[11] The full results can be found in the online appendix.

Table 1
Direct and cross effects of various AESs.

	0201	0205	0301	04	08	09	21
Eveness	**0.05***	**0.03**	0.02***	0.03***	0.02**	0.02***	0.07***
	(0.03)	**(0.02)**	(0.006)	(0.006)	(0.006)	(0.006)	(0.02)
Number of crops	**0.85***	**0.65***	0.29***	0.37 ***	0.22***	0.20***	0.58***
	(0.36)	**(0.23)**	(0.07)	(0.07)	(0.04)	(0.05)	(0.14)
Area under main crop (%UAA)	−0.03	−0.03***	−0.006*	−0.01***	0.002	−0.0007	−0.01
	(0.03)	(0.01)	(0.004)	(0.004)	(0.004)	(0.004)	(0.01)
Area under main crop (ha)	−2.30*	−1.51***	1.21***	−0.68	2.23***	2.20***	2.71**
	(1.34)	(0.58)	(0.41)	(0.62)	(0.36)	(0.35)	(1.19)
Cover crops (ha)	1.04	1.08***	**10.66***	0.23	−0.01	0.20	0.31
	(0.79)	(0.34)	**(1.32)**	(0.38)	(0.54)	(0.60)	(0.35)
Grass buffer strips (m)	−119.91*	192.45***	−7.49	**243.61**	13.54	30.60	−17.10
	(68.30)	(44.64)	(38.96)	**(149.24)**	(29.67)	(28.35)	(40.51)
Organic farming (ha)	−13.39	−6.58	−5.13	11.12	-0.50	7.49	**46.41***
	(45.64)	(15.09)	(21.07)	(26.33)	(12.86)	(18.93)	**(0.13)**
Under conversion (ha)	0.30	3.96	−3.31*	.08	−1.46	1.66	**4.41***
	(2.57)	(10.67)	(1.85)	(2.80)	(1.73)	(2.88)	**(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 bandwith set to 0.05. 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.

AES 21. Overall, these results suggest a significant selection on observables and they are coherent with previous empirical studies of the determinants of participation in these AESs [9].

We then estimate the probability of participating in a given AES, conditional on the control variables (i.e. the propensity score). Following Smith and Todd [32], we define the common support zone as the set of participants for which there exists a sufficient density of non-participants with the same value for the propensity score.[12] As shown in the online appendix, restriction to the common support zone generally reduces the number of recipient farms by 10%. The maximum is reached with AES 21, for which a quarter of the recipient farms have no untreated counterpart.[13]

Our main estimator is the local linear matching estimator based on the propensity score (LLM).[14] We estimate standard errors for LLM by using a bootstrap procedure.[15] We assess the quality of the matching procedure by comparing the mean level of the control variables of 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.[16]

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

Table 1 reports the LLM estimates of direct and crossover effects of each AES on agricultural practices. Crossover effects are estimated by focusing on farmers not receiving the AES that has a direct effect on the practice ($\mathbb{E}[Y_a^{01} - Y_a^{00}|D_b = 1, D_a = 0]$).[17,18]

5.2.1. Effects of the AESs on crop diversification

Two AESs are likely to directly affect crop diversification: AES 0201, which consists of introducing one new crop into the rotation, and AES 0205, which requires having at least four different crops in the rotation. The results suggest that AES 0201 has generally had a stronger impact on outcome variables than AES 0205 (Table 1), although there are fewer participants in AES 0201.

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[12] The construction of the common support zone is detailed in the online appendix.

[13] In order to understand how the farms on the common support differ from the average recipient farm, we run probit regressions for presence on the common support. Results indicate that recipient farmers on the common support are older and have smaller farms and a lower education level.

[14] See Imbens [17] for a detailed presentation of the various matching methods. We obtain similar results with two nearest-neighbor matching estimators: one using the propensity score only and the other using all the control variables simultaneously. The estimation procedures are detailed in the online appendix.

[15] We perform bootstrap at the farm level. The autocorrelation problems studied by Bertrand et al. [6] are less of an issue in our application: we only use two periods of data, and our sample is randomized at the farm level, thereby lowering the degree of spatial autocorrelation.

[16] As suggested by Rosenbaum and Rubin [28], we use standardized differences to assess the quality of our adjustment. Before matching, there are around 80 variables that exhibit large differences, whereas there is at most one large difference after matching with LLM. The full results of the balancing tests can be found in the online appendix.

[17] Estimates of $\mathbb{E}[Y_a^{11} - Y_a^{10}|D_b = 1, D_a = 1]$ are imprecise because of small sample size. We nevertheless have enough power to reject crossover effects on the planting of cover crops.

[18] In results not presented in this paper, we estimate the average causal effects of the AESs on practices measured in 2003 and 2005 for farmers that have entered before 2003 and find very similar results, thereby excluding learning or vintage effects.

These impacts are generally estimated precisely (ATTs are different from zero at the 1% level of significance). Results suggest that AES 0201 (resp. 0205) has increased the crop diversity index by 0.05 (resp. 0.03). This is not a strong effect; the diversity index varies from 0 to 1 and is equal to 0.77 (resp. 0.80) on the average recipient farm. On the contrary, these AESs have larger effects on the number of crops in the rotation: they are responsible for the addition of almost one crop to the rotation (0.85 for AES 0201 and 0.65 for AES 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. Crossover 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 0.58 crops to the farm. Other AESs increase the diversity index, but they do not decrease the area covered by the main crop.

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

AES 0301, which subsidizes the introduction of cover crops into the UAA, and AES 04, which subsidizes the planting of grass buffer strips, both aim to decrease the transfer of pollutants (mainly nitrogen) to ground and surface water. Results displayed in Table 1 show that AES 0301 has increased the area planted in cover crops, the average treatment effect on the treated being around 10 ha. This AES has thus triggered the planting of 94,000 ha (9400 recipients * 10 ha) of additional cover crops. This accounts for 12% of the 200% increase in the total area planted in cover crops between 2000 and 2005 in France. The ATT for AES 04 has not been estimated using DID-estimators, the outcome variable being unobserved in 2000. The LLM estimator suggests that participants in AES 04 have planted 240 more meters of grass buffer strips than their matched counterparts (Table 1), although this estimate lacks precision. This AES thus triggered the planting of 1440 km (=6000 recipients *240 m) of grass buffer strips in 2005, which is a low figure compared to a nationwide total of 20,000 km, largely due to the eco-conditionality of common agricultural policy direct subsidies. We find evidence that the assumption of no crossover effects is supported by the data in the case 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 crossover 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.

5.2.3. 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 aims to encourage the adoption of organic farming, has not been estimated using DID estimators, because the outcome variable is unobserved in 2000. This is not likely to lead to a large bias since farmers entering this AES were required to have no land cultivated by organic farming. Results suggest that the impact of AES 21 on the area dedicated to organic farming and the area under conversion is significant. Table 1 shows a difference approximating to 46 ha between the treated and control groups in the area fully converted to organic agriculture, and a difference of 4.5 ha in the area in the process of conversion. Furthermore, we do not detect significant crossover effects of other AESs on organic farming. These results suggest that AES 21 accounts for 90% of the almost 100% increase in the area devoted to organic farming between 2000 and 2005 in France.

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

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

5.3.1. Tests of the validity of Assumption 1 (no diffusion effects)

Farmers having converted to organic farming before 2000 may 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. This means that if there are imitation effects, our estimates suffer from omitted variables bias: the initial proportion of a farmer's neighbors that has adopted the practice concerned (organic farming, cover crops) simultaneously determines selection into the corresponding scheme and outcomes in the absence of the treatment. We test for the validity of SUTVA by adding the initial proportion of organic farmers, and farmers planting cover crops, in the farmer's *canton* as control variables. A *canton* is a larger administrative subdivision containing an average of 9 communes and is thus likely to represent 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 planting of cover crops (10.5 ha). We take this as evidence that SUTVA is not rejected by the data.

5.3.2. Tests of the validity of Assumption 2: placebo tests

Placebo tests consist of applying DID-matching to estimate the effect of receiving an AES after 2003 on the change of practices between 2000 and 2003. Theoretically, no effect should be detected for this "treated" group. However, these tests are disrupted by anticipation effects due to the unusually long period of time taken to process administrative applications in 2003. As a consequence, we have performed these tests on groups of future participants who entered the program at dates further and further removed 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 removed participation is from September 2003, and we should obtain a zero effect after some time. Results are presented in Table 2.

Table 2
Results of the placebo tests.

Outcome	Sample					
	AES	Post-Sept03	Post-Mar04	Post-Sept04	Post-Mar05	Post-Sept05
Main crop	0201	−0.02	−0.01	−0.02	−0.03*	n.a.
(% UAA)		(0.02)	(0.02)	(0.01)	(0.02)	(n.a.)
Main crop	0205	−0.01***	−0.01***	−0.01***	−0.01*	n.a.
(% UAA)		(0.00)	(0.00)	(0.00)	(0.01)	(n.a.)
Crop diversity	0201	0.03**	0.02	0.03	0.03	n.a.
index		(0.02)	(0.02)	(0.02)	(0.04)	(n.a.)
Crop diversity	0205	0.02***	0.02***	0.01***	0.02*	n.a.
index		(0.01)	(0.01)	(0.01)	(0.01)	(n.a.)
Number of	0201	0.21	0.09	0.21	−0.12	n.a.
crops		(0.19)	(0.19)	(0.28)	(0.31)	(n.a.)
Number of	0205	0.33***	0.33***	0.35***	0.19	n.a.
crops		(0.09)	(0.09)	(0.09)	(0.19)	(n.a.)
Cover crops	0301	3.52***	3.60***	3.14***	3.34***	1.32
(ha)		(0.60)	(0.60)	(0.69)	(0.80)	(1.02)
Organic land	21	6.71***	4.91**	5.90**	5.58	n.a.
area (ha)		(2.53)	(2.35)	(2.65)	(4.13)	(n.a.)
Conversion to	21	13.96***	15.58***	4.05	4.81	n.a.
organic (ha)		(4.39)	(4.52)	(2.51)	(4.02)	(n.a.)

Note: Asterisks denote statistical significance at 1% (***), 5% (**), or 10% (*) level. Estimations use LLM with an Epanechnikov kernel and bandwith set to 0.05. Standard errors are in parentheses and are estimated by 500 bootstrapped replications. Details on the estimation are presented in the online 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 cannot be applied because the associated outcomes are not observed in 2003. UAA refers to usable agricultural area.

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 placebo effect on the number of crops is positive but it 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. Results are similar for AES 09. For 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.

Overall, the results of the placebo tests confirm the importance of anticipation effects and suggest a 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 a lower bound on the ATT.

5.3.3. A lower bound on treatment effects: results of triple differences estimates

We apply the triple-difference estimator, which corrects for the divergence estimated in 2003 between the participants and their matched counterparts. This estimator compares the change in practices between 2000 and 2005 to the change that would have happened had the 2000–2003 divergence continued at the same pace. Note that the triple-difference estimator leads to a lower bound on the treatment effect, since it assumes that there are no anticipation effects and that all the divergence detected in 2003 is due to time-varying selection bias.

Results of the triple-difference estimator are displayed in Table 3. 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 of 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%, compared to a reduction of 5% when estimated by DID-matching. Moreover, the average treatment effect on the number of crops is an increase of 0.8, compared to an increase of 1.05 when estimated by DID-matching. Such results indicate that the lower bound for these effects remains 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 based on DID-matching estimates; the DID-matching estimates already being 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 the DID-matching estimator. Although placebo tests clearly suggest

Table 3
Average treatment effect on the treated for AES in 2005 using DDD-matching.

Outcome	AES	DDD	DID	DID
		Sep03–Mar05 ATT[(1)]	Sep03–Mar05 ATT[(2)]	Whole sample ATT[(3)]
Main crop (% UAA)	0201	−0.04***	−0.05***	−0.03
		(0.02)	(0.02)	(0.03)
Main crop (% UAA)	0205	−0.01	−0.03***	−0.03***
		(0.01)	(0.00)	(0.01)
Crop diversity index	0201	−0.02	0.03	0.05*
		(0.03)	(0.02)	(0.03)
Crop diversity index	0205	0.00	0.03***	0.03
		(0.01)	(0.01)	(0.02)
Number of crops	0201	0.79**	1.05***	0.85**
		(0.38)	(0.37)	(0.36)
Number of crops	0205	0.07	0.67***	0.65***
		(0.12)	(0.10)	(0.23)
Cover crops (ha)	0301	4.87***	10.46***	10.66***
		(1.26)	(0.97)	(1.32)
Organic land area	21	14.07	45.01***	50.82***
		(10.11)	(6.98)	(2.79)

Note: ATT[(1)] refers to the triple-difference estimates, ATT[(2)] refers to the DID-matching estimates on the same sample (farmers who entered the AES between September 2003 and March 2005), and ATT[(3)] refers to the DID-matching estimates on the whole sample (farmers who entered the AES before March 2005). UAA refers to usable agricultural area. Estimations use LLM with an Epanechnikov kernel and bandwidth set to 0.05. Standard errors are in parentheses and are estimated by 500 bootstrapped replications.

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 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 are to 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 crossover effects that we find. First, we present a simple framework integrating ATT and windfall effects into a cost-benefit framework. We define a break-even point in the social benefit generated by the AES, above which the total net benefit in the presence of the AES is superior to the total net benefit in the absence of the AES. Second, we calculate this break-even point for each of the AESs under study. To do this, we combine our ATT estimates with data on costs extracted from the administrative files. Third, we compare the break-even point to estimates, taken from the literature, of the social benefit generated by the various agricultural practices we study. The results of these calculations are presented in Table 4.

6.0.4. A framework for cost-benefit analysis

Using Assumption 4 (no crossover effects), we can study each AES separately. 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. We thus adopt a taxpayer's view on the program and focus on consumers' surplus. We assume that the benefit from a practice is proportional to its average level. B measures the social benefit from one unit of 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 subject to 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, disregarding administrative costs, and deadweight loss due to taxation. Costs associated with the scheme are thus per hectare payments (C) multiplied by the 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 she plants or higher if the total area subject to the practice is capped and there are increasing returns from 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 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:

$$CS = \mathbb{E}[Y^1|D=1]B-\mathbb{E}[Y^p|D=1]C-\mathbb{E}[Y^0|D=1]B \tag{19}$$

Table 4
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 subject to 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 subject to 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 (21) to AESs 0201 and 0205 because payments are not tied to a given practice. We calculate the cost per additional area not planted with 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 1 ha of cover crops. N_b is the number of units of N-fertilizer whose runoff is prevented by 1 m of grass buffer strips. Sample: treated farms on the common support.

$$CS = \mathbb{E}[Y^1|D=1]B - (\mathbb{E}[Y^p - Y^1|D=1] - \mathbb{E}[Y^1 - Y^0|D=1] - \mathbb{E}[Y^0|D=1])C - \mathbb{E}[Y^0|D=1]B \tag{20}$$

$$= \underbrace{\mathbb{E}[Y^1 - Y^0|D=1]}_{ATT}(B-C) - \left(\underbrace{\mathbb{E}[Y^p - Y^1|D=1]}_{E} + \underbrace{\mathbb{E}[Y^0|D=1]}_{W}\right)C. \tag{21}$$

After rearranging, Eq. (21) shows that consumer surplus depends on the level of additionality of the program, measured by the ATT, on the level of discrepancy or declarative error E and on the windfall effect W. We say that the AES is cost-effective whenever $CS > 0$, i.e. when the social benefit B is superior to a break-even point B^* [19]

$$B^* = \frac{ATT + W + E}{ATT}C, \tag{22}$$

where B^* increases with W and E and decreases with ATT.

6.0.5. Toward a cost-benefit analysis

As a first step towards a cost-benefit analysis, we calculate the cost per hectare of the additional treatment effect (B^*) for each AES. We then compare the unit costs to estimates of the social benefit of the practices promoted by each AES. We measure C directly by dividing total payments by the total area under contract for each farmer. ATT comes from the LLM 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 subject to 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 subject to the practice (C_2), by dividing average payments by the average observed area subject to the practice.[20] Finally, we provide estimates of the social benefit B taken from the literature.

As reported in Table 4, in the case of AES 0301 (planting of cover crops), the average area under contract (21 ha) is slightly larger than that which we actually measured from survey data (17 ha), which suggests that some farmers committed to planting more cover crops than they actually did. This translates into a higher cost per observed (81 €) than per declared (68 €) planted area. Moreover, the additional treatment effect (11 ha) is equal to 60% of the planted area under cover crops, so that the windfall effect (6.58 ha) is large. Thus, almost 40% of the planted 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 planted 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. Comparing this to an estimate of the social cost of 1 kg of N-fertilizer leaching from the field provided by van Grinsven et al. [35] −0.7 € per kg—suggests a poor cost-efficiency of AES 0301.[21] Indeed, for this AES to be cost-efficient would require 1 ha of cover crop to prevent the leaching

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[19] Note that this condition does not account for farmers' surplus. We expect it to be positive though because there is free entry into the program (this is implied by our model, conditional on $\mathcal{I}$). Rigorously, this is thus a sufficient condition for cost effectiveness.

[20] For the sake of consistency, we focus on farms lying on the common support.

[21] We have only been able to find one study assessing the social costs associated with the pollution of drinking water by nitrates [35]. Epidemiological studies suggest that colon cancer may possibly be associated with nitrates in drinking water [11]. Taking the increased risk of colon cancer from a case-control study from Iowa, the authors extrapolate the results to assess the social cost for 11 EU member states by using data on incidence of cancer, nitrogen leaching, and drinking water supply. They estimate the associated increase in the incidence of colon cancer from nitrate contamination of groundwater-based drinking water at 3%, which corresponds to 0.7 € per kg of nitrate-N leaching from fertilizers.

of 131/0.7=187 kg of N-fertilizer, which seems highly unrealistic. However, if the value of avoided phosphorus leaching and of increased biological and landscape diversity is taken into account the cost-effectiveness of this measure would be improved.

In the case of AES 21, farmers converted more land to organic farming than they were paid for, so that E is negative. This is probably due to a combination of increasing returns and an informal cap on subsidized area. There is nevertheless a positive windfall effect: in the end, the cost per additional treatment effect is only slightly lower than the cost per subsidized area. Finally, subsidizing the conversion to organic farming could be highly cost effective: it costs 151 € per hectare, whereas some studies tend to show that the average social benefit from organic farming is higher. For example, Sandhu et al. [31] estimate the average benefit of organic farming relative to conventional farming to be 540 € per hectare per year.[22]

For grass buffer strips (AES 04), in order to compare our estimates with data from administrative records, we convert the *ATT* into hectares, under the assumption that a grass buffer strip is 10 m wide. Surprisingly, the average area under contract appears five times larger than the data from the survey would suggest.[23] Moreover, there is a large windfall effect and thus a very small treatment effect. This translates into a cost of almost 1800 € per additional hectare of grass buffer strips, while the mean premium for such AES is only 93 € per hectare. Lankoski and Ollikainen [20] use an estimate of 1.6 € per kg of N-fertilizer for the social cost from nitrogen runoff.[24] To reach cost-effectiveness, buffer strips thus have to prevent the runoff of 1800/ (1000*1.6)=1.1 t of N-fertilizer per hectare. Cost-effectiveness thus depends 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, 1 km of a 10-m wide buffer strip has to have a cropped watershed of 100 ha to ensure that the social benefits from this AES exceed its costs (14*100*0.8/1000=1.1). Moreover, reduced runoff of phosphorus and pesticides and increased biodiversity should also be taken into account. It thus seems that, despite high associated windfall effects, AES 04 could very well be cost-effective.

We cannot apply formula (21) to AESs 0201 and 0205 because payments are not tied to a given practice. We calculate that AES 0201 (resp. 0205) reduces area planted with the main crop by 2.30 ha (resp. 1.51 ha) on average. This translates into a cost per additional area not planted with the main crop of 990 €/ha (resp. 2900 €/ha).

7. Conclusion

AESs share with all voluntary programs the potential for large adverse selection. It is even possible that they only attract farmers that would comply with the requirements in the absence of payments, thereby generating no additional effects. Overall, we find that the French AESs that we study do not fit with this extreme scenario. All the AESs exhibit positive additional effects, even with the most stringent identification strategy. We find that the AESs which impose strong requirements, such as the AES aiming to subsidize conversion to organic farming, have large additional effects and almost nonexistent windfall effects. On the contrary, we find that the AESs with modest aims, such as the AES only requiring farmers to add one crop to the rotation, have generated very limited additional effects.

For the AESs suffering from large windfall effects, such as the one aiming to subsidize the planting of cover crops, the comparison of the cost per hectare of additional treatment effects with estimates of social benefits taken from the literature suggests that this AES may not be cost-effective. On the contrary, the AESs for which the windfall effects are small or even null may be cost-effective. The AES aiming to subsidize conversion to organic farming is a case in point. Because it was directed at conventional farmers only, the extent of windfall effects is extremely small and cost-effectiveness is high. Denying subsidies to farmers that adopt green practices out of goodwill is nevertheless ethically debatable. Formalizing this trade-off between ethics and efficiency is a nice avenue for further research, for example using insights from fair taxation [12].

Much remains to be done to improve the insertion of treatment effect estimates into a fully-fledged cost-benefit framework. Estimating farmers' surpluses from the AES would be a first step. More importantly, estimating the spatial

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[22] To our knowledge no assessment of the social value of organic farming is available for France, but at least two empirical studies may be used as approximations. Sandhu et al. [31] estimated the economic value of various ecosystem services provided on arable landscapes in New Zealand. They conducted field experiments to assess a dozen ecosystem services, such as biological control of pests, services provided by shelter-belts and hedges, nitrogen fixation or mineralization of plant nutrients. For example, the economic value of the biological control of aphids and flies was estimated on the basis of avoided cost of pesticides using their cost in New Zealand. Taking the difference between the estimated value of ecosystem services in organic fields and in conventional fields, they obtained an estimate of 540 euros per hectare per year. Lankoski and Ollikainen [20] report an alternative estimate of the social benefit associated with organic farming suggested by Aakkula [1], who used the contingent valuation method to elicit a monetary value for conversion from conventional agriculture to pro-environmental farming in Finland and found an average willingness to pay of 78.4 € per hectare. Without any evidence of the superiority of one assessment over the other, we do not exclude the idea that subsidizing conversion to organic farming can be highly cost effective.

[23] People in charge of conducting the farm surveys acknowledged that there is a large error in the measurement of the length of grass buffer strips. This is the most likely explanation of the large discrepancy we find.

[24] To the best of our knowledge, there is no study providing an estimate of the social value of reductions in nutrient runoff based on French data. Following Lankoski and Ollikainen [20], we thus use an estimate provided by Vehkasalo [36] who approximated the social benefits of reducing nitrogen runoffs from Finnish farmland by applying the avoided expenditure method. He estimated the costs associated with nitrogen reduction at municipal wastewater treatment facilities and found 1.6 € per kg of nitrogen reduced (for 10–20 % reduction). In more recent studies, Lankoski and Ollikainen [21] drew on Gren [13]'s estimates of the willingness to pay for nutrient load reduction in the Baltic Sea (4.27 € for 1 kg reduction in nitrogen). However, such estimates appear too remote from our subject and we prefer to keep to the avoided expenditure estimate, which appears to be less related to geographical features.

distribution of treatment effects would enable a finer comparison with social benefits that undoubtedly vary across space. Finally, estimating the treatment effects of the AESs directly on the environment remains an essential but very difficult undertaking. Kleijn et al. [19] 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.

Acknowledgments

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