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THE IMPACT OF ENVIRONMENTAL POLICIES ON ADOPTERS UNDER GENERAL INTERFERENCE.

THE CASE OF EU SUPPORT TO ORGANIC FARMING.

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KEYWORDS: Staggered Treatments, Dynamic Treatment Effects, Spatial Interference, Environmental Policy, Organic Farming.

JEL Classification: C21, C22, C23, Q12, Q18.

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1. Introduction

Providing the right monetary incentives (or disincentives, like taxes) is often considered the best way to pursue environmental objectives through policy measures (Baumol and Oates, 2012). These measures would induce or convince, but not force, private agents to pursue outcomes of societal interest. It is often argued that the voluntary adoption of measures under these (dis)incentives is what eventually establishes if and how much such a policy is successful: the larger the number of adopters, the greater the consequent environmental outcome of collective interest (Esposti, 2025).

But adoption is not the only relevant aspect of policy effectiveness. Upon adoption, private agents are asked to change their behaviour and this decision making eventually generates the impact of the policy. This kind of assessment can be addressed within a Casual Inference (CI) logic, namely by regarding the impact of policies as a Treatment Effect (TE). A large body of recent literature has emerged in this field (Imbens and Wooldridge, 2009; Cerulli, 2015). It emphasizes that several identification issues have to be properly confronted in order to consistently estimate these TEs.

Among these, and besides the classical problem of unobserved confounders (or selection-on-unobservable bias), two issues have aroused interest: staggered treatments and dynamic TEs (time interference); spatial interference (or contagion). They seem particularly relevant in agri-environmental contexts, namely whenever units are spatially explicit and may interact across space. With few exceptions (Wang, 2023), existing literature has dealt with these two concerns only separately, as bringing them together raises complex identification and estimation issues and may imply computationally demanding solutions. These challenges are particularly evident when observational rather than experimental data are under study.

The present paper aims to deal with both these two sources of interference (henceforth, *general interference*) within an observational study. The proposed approach is applied to the adoption of organic farming under the EU support in the Italian agriculture during the Common Agricultural Policy (CAP) programming period 2014-2022 (Baldoni and Esposti, 2023). A balanced panel is extracted from the Farm Accountancy Data Network (FADN) dataset. It is assumed that farmers voluntarily decide to adopt organic farming depending on whether the adoption increases their profits, including the monetary support provided by the policy. But the eventual impact of organic farming on farm-level profit at a given point in time not only depends on the policy support and on the consequent choices and behaviour of the farmers. It is also affected by the two abovementioned forms of interference.

Time interference means that an initial adoption produces its effect over time. In the context of causal inference, this interference is often defined as the *carryover effect*: the effect of a treatment does not cease immediately but, when the unit remains in the treatment, it continues to influence the farm's outcome over time, thus affecting subsequent measurements (Oster, 2019). A similar meaning is that of *persistent TE,* where the emphasis is on the presence of an effect even when the treatment is over (Gertler et al., 2016). Time interference is also designated, more generically, as *dynamic TEs* (Heckman and Vytlacil, 2007; Callaway and Sant'Anna, 2021; Sun and Abraham, 2021; Liu et al., 2024). Though slightly different, we consider these terms as equivalent for the present case. Space interference (or dependence) means that both farmers' policy adoption and outcome may be affected by the decision making and performance of neighbouring units. This interference depends on the well-known economic forces, generally indicated as *localised external economies*, that have already been investigated by the recent literature in the field (Baldoni and Esposti, 2023) and often referred to as *spillover effects* or *spillovers* (Akbari, 2023).

These two forms of interference may evidently co-exist and this case of general interference is the main object of the present paper. An appropriate theoretical framework is developed to integrate general interference in the farmers' decision making process. A suitable methodological approach to identify and estimate the Average TE (ATE) under general interference is then adopted. This approach can be regarded as a generalization of the methodology proposed by Wang (2023). To our knowledge, this is the first application of this approach to policy assessment with observational data. In this respect, the main contributions of the present study consist in admitting contagion (i.e., spatial dependency in policy adoption decision) in the estimation algorithm, as well as in admitting the role of confounders in all stages of the estimation procedure.

The rest of the paper is structured as follows. Section 2 overviews the main challenges and the recent literature about general interference within CI studies. Section 3 illustrates the theoretical framework underlying the farmers' decision making about policy adoption and the consequent production choices and interprets it within a TE logic. This framework incorporates all factors of general interference. Section 4 presents the adopted sample and dataset and describes the research design. Section 5 details the adopted estimation approach, consisting of an evolution of the algorithm originally proposed by Wang (2023). Results are then reported and discussed in Section 6, while Section 7 concludes by deriving some methodological and policy implications.

2. The policy and methodological issue: an overview of the literature

Since the 1992 CAP reform, support for organic farming has been a major focus of agri-environmental policies (Esposti, 2022, 2024; Coderoni et al., 2024). However, to what extent this support has significantly contributed to the success of EU organic production over the last decades (Lampkin, 2023; Möhring et al., 2024) remains a matter of discussion. In order to actually assess this contribution, the CI logic has already been adopted in several studies (Coderoni et al., 2024 and 2025; Esposti, 2024; Martín-García et al., 2024, just to mention a few recent examples). However, an often disregarded aspect in these studies, as well as within the more general wide literature on the impact of agri-environmental policies (Esposti, 2022; Sotte and Brunori, 2025), concerns those socio-economic processes that imply adjustment over time (time interference) and diffusion over space (spatial interference). These processes may interfere with the proper identification and estimation of the TE policy and this issue is here designated as *general interference*.

Time interference occurs for two different (though often coexisting) factors: *lagged effects*, namely the impact of a treatment might not be immediate; *cumulative effects* (Imbens and Rubin, 2015), namely for some treatments the total impact depends on the duration and intensity of the treatment over time so the initial effects of a program might be different from its long-term effects. If the outcome response (the TE) takes time (i.e., more than one period) to fully occur, the proper identification and estimation of the "full" TE can be challenging, especially when after-treatment observations are few and treatment is staggered.

This is acknowledged by the EU Rural Development Policy (RDP) measure in which there is a clear distinction between support for the adoption of organic farming (Measure 11.1) and support for the maintenance of organic farming (Measure 11.2). The former support can be delivered only for the abovementioned period of conversion. The latter support can last up to five years after the end of the conversion period, though this duration can be extended by EU member states in their Rural Development Policy programming. The goal of both types of support is identical since it is meant to compensate for the additional costs and possible foregone income that organic farming encounters compared to conventional farming. The adoption subsidies tend to be higher than those for maintenance because of the costs, yield decline and risks associated with the transition phase. Nonetheless, these two distinct measures are often understood as a single policy, not only because of the same nature and justification of the support, but also because they can be intended as the natural continuation, one from the other. This is the case of the present study. At the same time, these two measures capture the twofold nature of interference in the case of organic farming adoption.

Intended as a treatment, the adoption of organic farming entails both forms of time interference. First of all, the change of farmers' behaviour and farms' performance implied by organic practices is not immediate and may take years. This progressive adjustment to new technological solutions is characteristic of agricultural production as it is typically multiannual, particularly in the case of livestock and perennial crops. This aspect is acknowledged by EU organic regulation currently in force (Regulation EU 2018/848). Within this regulation, the conversion period for farms transitioning from conventional to organic agriculture is generally two years for annual crops, or three years for perennial crops. In the case of livestock productions, the conversion time changes depending on the livestock but it is always lower than 12 months. This conversion time is required to allow the soil and farming system to adapt to the new practices without using synthetic chemicals, fertilizers, or pesticides. Secondly, time interference occurs because the TE is, to some extent, *persistent over time*. After the initial period of adjustment, the maintenance of organic farming implies, at least in part, the maintenance of the same farmer's behaviour and farm's performance.

Recent studies have stressed that spatial interference may also occur in the case of organic farming adoption. Unlike time interference, however, it originates more from the economic forces activated by the adoption rather than from the contents of the EU regulation. As extensively discussed in Baldoni and Esposti (2023), the support for organic farming can generate a twofold spatial interaction as both the participation and the response to this policy measure may be affected by the presence of neighbouring participants. Regarding participation, this influence can come from two contrasting forces. Proximity of other organic farms may induce imitation (or *contagion effect*) (Bartolini and Vergamini, 2019) while limited funding at the local level may lead to latecomers being excluded (*first-type congestion* or *crowding-out effect*). Two categories of untreated units can thus be implicitly distinguished: those that do not want to participate (voluntary exclusion); those that have been excluded for administrative reasons (involuntary exclusion).

Also in terms of the response to the policy, spatial interference may occur for two opposite reasons. The presence of other contiguous organic farms may induce a local *milieu* that facilitates organic production due to either lower costs (larger availability of some specific production factors) or higher revenues (more advanced and efficient local supply chains), namely, positive pecuniary and non-pecuniary externalities (Di Domenico and Riccetti, 2024). At the same time, however, local concentration of organic farms may saturate the local markets, increase competition, and reduce revenues. This *second-type congestion effect* expresses all those negative externalities generated by the concurrent participation in the treatment of neighbours.

Table 1 summarizes the origin and economic interpretation of all these different sources of general interference in assessing the impact of organic farming adoption.

Despite its underlying sources and forms, general interference brings about severe identification and estimation issues. The CI logic applied to policy evaluation relies on the widely accepted idea that, whether within a randomized controlled trial or an observational study, the ideal framework involves a survey panel where the same units in both the treatment and control groups are observed across multiple time periods, including some before and some after the intervention that the treatment group receives (Carattini et al., 2024).

The role of the time dimension within a panel dataset has been long and widely exploited particularly in the so-called Difference-In-Differences (DID) estimation strategy. Under the assumption of block treatments and the validity of the Parallel Trend Assumption (PTA), this estimation strategy uses time, namely observations before and after the treatment, to identify the ATE itself. Complications arise, however, when the treatment entry is staggered and when entrants are heterogeneous enough to make their staggered adoption violate the PTA. But a further complication arises for the abovementioned time interference: the TE itself shows a dynamics upon the entry into the treatment, so that any observation after the adoption is inevitably affected by the previous periods under the treatment.

Recent literature has proposed solutions for the identification and estimation of these dynamic TEs often adopting some adaptation or evolution of the Two-Way Fixed-Effect (TWFE) approach (Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; Han, 2021; Sun and Abrham, 2021; Liu et al., 2024). In all these solutions a common assumption is that of *absorbing treatments,* meaning that once a farm enters the treatment, it remains exposed to the treatment for all the following observed period. Such an assumption admits both staggered treatment and a dynamic TE but it imposes that the latter is the consequence of an adoption decision taken once and for all.[1]

Recent literature has been produced also on the complications implied by the other source of interference, that of spatial interference. In fact, studies under spatial interference are still relatively secondary within the huge CI literature of the last two decades. Spatial interference results in the

[1] The case of a sequence of adoption choices is also referred to as dynamic treatments (Heckman et al., 2016) and it is excluded here. The present study thus refers to the literature that admits staggered and dynamic TE but excludes dynamic treatments.

violation of one of its fundamental identification conditions, the so-called Stable Unit Treatment Value Assumption (SUTVA) (Imbens and Rubin, 2015). In recent years, several works have tried to deal with the possible violation of SUTVA stemming from spatial dependence and to propose appropriate estimation solutions (Kolak and Anselin 2020; Forastiere et al, 2021; Akbhari et al., 2023). Though different, these approaches agree that disregarding spatial interference may lead to a significant bias in TE estimation. On the one hand, the possible exposure of any treated unit to the treatment or the outcome of neighbouring units can lead to an over-estimation of the TE. On the other hand, it is disregarded that control units themselves can be, *de facto*, exposed to the treatment due to the presence of neighbouring treated units, leading to an under-estimation of the TE. But the nature and magnitude of the SUTVA violation depend on the underlying nature of the interaction among units. A unifying framework is thus difficult to define.

The need for a conceptual and econometric approach that unifies these different possible forms of spatial interference is at the core of a recent paper by Forastiere et al. (2021). The new identification condition they put forward has the major consequence that spatial interference turns a binary treatment into a sort of multivalued treatment, as any treated unit shows a different exposure to the treatment depending on the surrounding space. In practice, this approach makes spatial interference convert into a form of spatial heterogeneity where what makes spatial units differ is the different exposure to the treatment. A common advantage of these configurations of spatial interaction is that they allow to identify direct (or individual) and indirect (or spillover) effects (Liu et al., 2016; Aronow and Samii, 2017; Forastiere et al. 2021). A common disadvantage is that these configurations must be assumed *ex ante* and, therefore, they can always be somewhat arbitrary. While in experimental studies these configurations may be designed or controlled, in observational studies they are often unknown, case specific and peculiar.

Within this growing literature, contributions acknowledging the presence of both forms of interference and proposing consequent identification and estimation solutions are very rare. To our knowledge, at the time of writing the only study moving in this direction is Wang (2023). Wang (2023) puts forward a design-based framework to identify and estimate direct and indirect effects in panel data where temporal and spatial interference intertwine in intricate ways that are unknown to researchers. The proposed framework defines estimands that enable researchers to measure the influence of each type of interference and represents a substantial step forward compared to the conventional approaches typically adopted in panel data TE studies.

The present study aims to adopt the Wang (2023) approach to investigate the impact of environmental policies (specifically, organic farming support) under spatial interference. As discussed above, both forms of interference are likely to occur in the implementation of these policies. Design-based approaches like Wang (2023) typically rely on the physical or geographical structure of the study, namely on how units are arranged in space (Wang et al., 2024). They account for the spatial layout of the units which is assumed to be known a priori, i.e. exogenous or given. This seems to fit well with the present study on agricultural production units whose spatial dimension (i.e., location of farms) is substantially fixed, so it can be considered as an exogenous factor neither dependent nor conditioned by farmer's choices. Compared to Wang (2023), the present study aims not only to develop a suitable theoretical framework that reveals both forms of interference arising from farmers' decision-making processes. In addition, the adopted estimation approach generalizes the whole structure of interference admitted in Wang (2023) by always including the role of confounders and allowing for contagion, that is, spatial interference also occurring in the adoption decision.

3. The theoretical background

3.1. Farmers' decision making

Consider a panel of N production units (farms) observed over T time periods. For any i-th farm ($i = 1, \dots, N$) in time t ($t = 1, \dots, T$), an aggregated multi-input multi-output production technology can be represented by the feasible production set $F_{it} \subset \mathbb{R}^M$. F_{it} is farm-specific and accounts for all sources of heterogeneity in the farmer's decision to voluntarily adopt policy measures and consequent production choices (Esposti, 2024). Therefore, F_i is shaped by all the observed or unobserved specific features of the i-th farms, depending on both external and internal factors, that we generally indicate with the $(Q \times 1)$ vector $\mathbf{X}_i$. Then, define a $(M \times 1)$ vector of netputs $\boldsymbol{Y}_{it} = (Y_{it1}, \dots, Y_{itM})'$ containing farm outputs (with a positive sign) and inputs (with a negative sign). These netputs are only feasible if $\boldsymbol{Y}_{it} \in F_{it}$.

Suppose now that, at any given time t, each farm is offered the opportunity to voluntarily participate in a policy (or, alternatively, a treatment), Z_{it}. If that farm chooses to participate ($Z_{it} = 1$), then this choice is expected to induce specific production choices, $\boldsymbol{Y}_{it}$. Therefore, for any given farm, we assume that the treatment can be univocally mapped onto production choices ($Z_{it} \leftrightarrow \boldsymbol{Y}_{it}$). It is thus possible to express these choices, as well as the resulting economic and environmental performance of farms, as a function of both the treatment and the aforementioned farm characteristics, $\boldsymbol{X}_{it}$. In other words, we can write $\boldsymbol{Y}_{it} = f(Z_{it}, \boldsymbol{X}_{it})$, where $f(\cdot)$ is a vector-valued function.

The voluntary policy measure considered here promotes organic farming with the societal goal of reducing the environmental impact of conventional agricultural practices: if i-th farm adopts organic farming, and receives the corresponding support at time *t*, it is $Z_{it} = 1$.[2] However, like several other contributions on this topic (Jaime et al., 2016, Vergamini et al., 2020, Bonfiglio et al., 2022, Khan et al., 2025, to mention a few), we assume here that what really matters from the farmers' private perspective is the economic return of their adoption decision. Therefore, the TE actually concerns the outcome of interest to the farmers, namely their profit (or net farm income; see Section 4), π_{it}. The generic farm-specific profit function can be expressed as $\pi_{it} = p[f(Z_{it}, \boldsymbol{X}_{it})]$ where $p(\cdot)$ is a single-valued function. If both $\boldsymbol{Y}_{it}$ and Z_{it} are observable $\forall i \in 1, \dots, N$ and $\forall t \in 1, \dots, T$, the TE associated with the organic support policy can thus be identified as follows:

(1) $$\Delta\pi_{it} = [(\pi_{it} | Z_{it} = 1, \boldsymbol{X}_{it}) - (\pi_{it} | Z_{it} = 0, \boldsymbol{X}_{it})] = p[\Delta f(Z_{it}, \mathbf{X}_{it})]$$

where $\Delta f(\cdot) = \Delta \boldsymbol{Y}_{it} = [(\boldsymbol{Y}_{it} | Z_{it} = 1, \boldsymbol{X}_{it}) - (\boldsymbol{Y}_{it} | Z_{it} = 0, \boldsymbol{X}_{it})]$. Averaging this value across the whole sample provides the Average TE (ATE): $(\sum_{i=1}^{N} \Delta\pi_{it})/N$.

3.2. Time interference

Both time and spatial interference can be made explicit within this theoretical framework. For the former, the main complication is that units can voluntarily enter the treatment in any point in time. Not only do we admit a staggered entry, but also that the treatment can carry its effect over time after entry (namely, a dynamic TE) due to the abovementioned adjustment and cumulative effects. Suppose that the i-th farm enters the treatment at generic time (T-k_i). (T-k_i)=0, that is k_i=T, thus indicates that the i-th unit is always treated, while (T-k_i)=T, thus k_i=0, indicates that the farm is never treated. If we assume that, once entered, the i-th unit remains treated until the end period T (assumption also refereed to as *absorbing treatment*), k_i can be interpreted as the time duration of

[2] It is worth stressing that, in the FADN dataset, both the dummy 'organic' and the extent of policy support are provided. The underlying assumption here is that these two conditions always correspond: whenever a farm is organic it receives the support and vice versa. As will be clarified in Section 4, the very few cases in which this condition is not met for whatever reason are thus excluded from the analysis.

the treatment for the i-th unit.[3] Admitting a dynamic TE means that $TE_{it} = \Delta\pi_{it}$ can be identified and estimated for any point in time after the entry, that is $\forall t \in (T - k_i, \ldots, t)$. Consequently, following Wang (2023), the TE at entry time (henceforth, the *Direct TE* or *DE*) can be computed as $DE_{i(T-k_i)} = \Delta\pi_{i(T-k_i)}$ along with the *Cumulative TE* (*CTE*) up to time t as $CTE_{it} = \sum_{s=T-k_i}^{s=t} \Delta\pi_{is}$.

More importantly, this dynamic TE allows to detect the shape of the farm's response to the treatment after the entry. This shape has an interesting economic interpretation. If we assume that economic agents (farmers in the present case) behave rationally (i.e., non-myopically), the implicit consequence is that they voluntary adopt the policy measure only if they gain an economic advantage to do so. And this advantage develops over time upon the adoption of the policy measure. This idea can be formalised by arguing that rational farmers pursue dynamic optimization on $\Delta\pi$ in the form of $\left\{\max \int_{T-k_i}^{T} [\Delta\pi_{it}/(1+r)^t]\, dt\right\}$, where r is the discount rate applied at any time period (assumed constant across time and farms). The argument of the maximization is the policy adoption choice, that is, the binary variable $Z_{it} = (0,1)$. In practice, the i-th farm voluntarily adopts the treatment at time $(T - k_i)$ only if $\left\{\int_{T-k_i}^{T} [\Delta\pi_{it}/(1+r)^t]\, dt > 0\right\}$. Since the decision to enter the treatment can be made at any time *t*, farmers repeat this maximizing decision in any period until, whenever they finally enter the treatment, they remain treated up to the end.

According to this logic, the pattern of these dynamic TEs can be intended as a sort of farm-specific adjustment or growth curve upon policy adoption that may also admit a temporary negative impact of the policy on the farm's economic performance (i.e., $\Delta\pi_{it}<0\ \exists t$) as it only implies that the CTE is eventually positive (Acock, 2013).[4] In general terms, there is no theoretical argument on the shape of this growth curve or process. Depending on the farm-specific technological constraint $F_{i,t}$, the dynamic response to the treatment can take different forms. These are all the forms admitting a positive discounted cumulated response ($\int_{T-k_i}^{T} [\Delta\pi_{it}/(1+r)^t]\, dt>0$). If monotonic, this shape can be either concave, convex or linear. However, a non-monotonic shape can not be excluded, particularly due to an initial negative economic impact of the adjustment process implied by the policy adoption. Figure 1 provides a graphical representation of some of the possible different shapes of this dynamic response. Other combinations of these forms are obviously possible. The identification and estimation of the sequence of $\Delta\pi_{it}$, therefore, can be particularly informative on the economics underlying this dynamic TE to policy adoption.

Nonetheless, despite the huge progresses made in recent years by the literature in the field (Coderoni et al, 2024), identification and estimation of individual dynamic TEs with staggered entry remain very challenging due to the well-known fundamental problem of causal inference (Holland, 1986), namely the fact that the counterfactual case can not be observed. But we can still obtain identifiable estimands by averaging the individual dynamic TEs over the whole sample. Averaging can be carried out according to different logics depending on how the staggered entry is dealt with. Here it seems more informative to average across cohorts of farms. By "cohort" we intend here a group of farms with the same duration of exposure to the treatment, that is, remaining in the treatment for the same number of years. Given the assumption of absorbing treatment, this means that we can define a cohort J_t as the set farms adopting the policy at the same time *t* with $1 \le t \le T$. Thus J_1 is the cohort of always organic farms while J_T is the cohort of farms for which there

[3] The limited period under consideration and the specific case under study here makes the case of a farm becoming organic and then stopping after few years inconsistent. As discussed in Section 2, organic support requires 2 years for conversion and grants 5 year of maintenance. The support is thus awarded for a total period of 7 years Since here T=8 we can conclude that the assumption of absorbing is reasonable. Thus we also exclude (and we leave to future research) the case of repeated treatments: farms adopting the policy, then exiting and, finally, entering again.

[4] From a more theoretical perspective on this stage-based response see also Aleman et al. (2023).

is only one year of exposure as the entry time corresponds to the final period of observation. If we indicate with N_{J_t} the numerosity of cohort J_t, it is evidently $J_t<N$ $\forall t$ while the number of controls (non-organic farms) C is expressed as $C = N - \left(\sum_{t=1}^{t=T} N_{J_t}\right)$.

For any generic t it becomes possible to compute k different ATEs with $k=T-t$, that is the ATE for any year of time exposure of the cohort J_t. The consequent cohort-specific ATE for a given time exposure k can be computed as:

(2) $$ATE_{J_t}^k = \frac{\sum_i \Delta\pi_{ik}}{N_{J_t}}, \forall i \in J_t, k \in (0, \dots, T - t\,)$$

It also follows that for any cohort J_t is thus possible to compute the average direct effect (DE) by imposing k=0 in (2), that is, $DE_{J_t} = ATE_{J_t}^0$. The average CTE can be also computed as $\sum_k ATE_{J_t}^k$. It is worth noting that, for a given time exposure k, the ATE is not unique since it is cohort-specific and it can differ across cohorts as it is identified and estimated on different samples, namely the cohorts. Comparing $ATE_{J_t}^k$ across cohorts is indicative of this cross-cohort heterogeneity. Nonetheless, a further average can be computed in order to have the ATE^k over the whole sample:

(3) $$ATE^k = \frac{\sum_{J_1}^{J_{T-k}}\left(\frac{\sum_i \Delta\pi_{ik}}{N_{J_t}}\right)}{(T-k)}, \forall i \in J_t, k \in (0, \dots, T - 1\,)$$

The progression of ATE^k over k can be interpreted as the average shape of the outcome response to the policy over time and across the different cohorts (see Figure 1). Therefore, for any i-th unit we have two sets of T estimands: one concerns the cohort-specific (or minimum exposure group-specific) direct effects, the other concerns the cumulative effects over T possible treatment duration.

From (2) and (3) it also follows that the term k has two different meanings. As intended so far, it indicates the duration of exposure to the treatment. At the same time, however, it also establishes the set of cohorts over which the whole-sample ATE can be computed. These sets are here designated as *exposure sets* and are defined as $\sum_{s=k+1}^{s=T} J_{s-k}$. Therefore, exposure set for $k=(T-1)$ corresponds to J_1 and exposure set k=0 corresponds to $(J_1 + J_2 + \dots + J_T)$. Obviously, the longer the time exposure k, the thinner the panel sample because it concerns only those units that have been exposed to the treatment for at least k periods.

This twofold meaning of the term k allows a differentiated notation depending on the ATE to be identified. Whenever the direct effect is considered, we indicate the effect observed on the year of entry but averaged over the exposure set k with DE^k. Whenever the ATE of interest concerns the effect observed after k periods of exposure, it is designated as Average Time-exposure TE and is indicated as TE^k where k indicates, at the same time, the time of exposure and the consequent exposure set.

3.3. Spatial interference

Spatial interference occurs whenever two generic units within the sample, $i, j \in N$, reciprocally interfere in both treatment participation and treatment response (or outcome). In terms of farmers' decision making, this can be formalized as $\boldsymbol{Y}_{it} = f[Z_{it}(Z_{jt}), \mathbf{X}_{it}, \boldsymbol{Y}_{jt}]$. This formulation makes the dual nature of spatial interference explicit. On the one hand, the policy adoption is spatially dependent, namely neighbouring j-th unit can affect the i-th unit participation decision or, maybe, the i-th unit unintentional treatment exposure, as expressed by the term $\boldsymbol{Y}_{it} = f[Z_{it}(Z_{jt}), \cdot]$. Within the literature in this field, this spatial dependence of treatment assignment is sometimes referred to as *contamination* (Rhoads, 2011). On the other hand, the policy effect is itself spatially dependent, namely the treatment response (its performance or outcome) of the neighbouring j-th unit can influence the i-th unit treatment response. Within the literature in this field, this spatial dependence of treatment outcome is sometimes referred to as *contagion* (Imai and Jiang, 2020) and it is formally expressed by the term $\boldsymbol{Y}_{it} = f[\cdot, \boldsymbol{Y}_{jt}]$.[5] Both contamination and contagion pose relevant, but different, identification and estimation issues.

Making spatial interference explicit requires the specification of an *N*x*N* spatial weight matrix $\mathbf{W}_t$ whose diagonal elements equal zero ($w_{ii,t}$=0) while off-diagonal terms take value either of 0 ($w_{ij,t}$=0) or 1 ($w_{ij,t}$=1) depending on whether the i-th and j-th units spatially interact or not at time t. The easiest, and most common, way to define matrix $\mathbf{W}_t$ consists in establishing a radial distance *d* around any i-th unit. Any other j-th unit falling within this distance is considered a neighbouring unit, namely w_{ij}=1. In this respect, it is usually assumed that $w_{ij,t}= w_{ji,t}$, i.e., $\mathbf{W}_t$ is a symmetric matrix before row normalization.

We assume here that **W** is time-invariant, namely $\mathbf{W}_t = \mathbf{W}\ \forall t \in (0, \dots, T)$. This assumption could be questionable when **W** expresses social interaction, thus a social network, rather a spatial network. In this case interaction among units would not exclusively depend on spatial distance but also on some other criteria or metrics and could possibly vary over time. Here, however, this possibility can be ruled out since physical units are under study (farms) and interaction occurs across the geographical space. So, it seems reasonable to assume that **W** is time-invariant as units' location is fixed and permanent.

On this basis, farms' performance can be formalized as $\boldsymbol{Y}_{it} = f[\mathbf{W}_i\boldsymbol{Z}_t, \mathbf{X}_{it}, \mathbf{W}_i\boldsymbol{Y}'_{it}]$ where $\mathbf{W}_i$ it the i-th row of $\mathbf{W}$, $\boldsymbol{Z}_t$ is the *N*x1 vector of the treatment variable across the sample at time *t* and $\boldsymbol{Y}_{jt}$ is the (*M*x*N*) matrix of the netput vector across the sample at time *t*. It thus follows that the TE in (1) can be rewritten as:

(4) $$\Delta\pi_{it} = [(\pi_{it}|Z_{it} = 1, \mathbf{X}_{it}, \mathbf{W}_i\boldsymbol{Y}'_{it}) - (\pi_{it}|Z_{it} = 0,, \mathbf{X}_{it}, \mathbf{W}_i\boldsymbol{Y}'_{it})] = p[\Delta f(\mathbf{W}_i\boldsymbol{Z}_t, \mathbf{X}_{it}, \mathbf{W}_i\boldsymbol{Y}'_{it})]$$

The identification and estimation of the TE in (4) is challenging as spatial interference violates the Stable Unit Treatment Value Assumption (SUTVA), one of the fundamental identification assumptions in CI studies (Imbens and Wooldridge, 2009; Imbens and Rubin, 2015; Baldoni and Esposti, 2023). Complications are even greater if we admit the combination of time and spatial interference. This implies admitting that spatial spillovers of the i-th unit's treatment on j-th unit behaviour and performance, and vice versa, may occur over time, that is periods after the i-th unit enters the treatment. Following the formalization above, the coexistence of time and spatial interference can be expressed by the combination of different values of parameters k and d and, ideally, allows the decomposition of the total TE into different partial effects over time and space (see Section 5).

Following the same derivation of Section 4.2, it will be possible to reformulate the individual and the ATEs under a combination of both time and spatial interference (general interference), namely under any combination of k and d. In particular, ATEs identified in (2) and (3) can be indicated as $ATE^k_{Jt,d}$ and ATE^k_d, respectively.

The complex set of interdependencies across time and space implied by this theoretical framework is summarised by the Dynamic Acyclic Graph (DAG) in Figure 2. This figure builds on Wang (2023, Figure 1), but unlike Wang, it explicitly accounts for spatial interference across treatment statuses—that is, contamination. It is excluded that spatial interference across units can occur via farm characteristics, $\boldsymbol{X}_{it}$; instead, it is only allowed via farm adoption decision (*Z*) and consequent performance ($\boldsymbol{Y}$ or $\Delta\pi$).

[5] It is worth noting that, within this literature, the use of terms "contamination" and "contagion" may be actually ambiguous. Wang (2023), for instance, uses "contagion" to indicate what Rhoads (2011) intends as "contamination".

The proper identification of the estimands of interest for all different values of k and d is challenging and can only be achieved via some additional identification assumptions, some prior knowledge (or assumption) on the structure of the spatial matrix included. This identification and estimation strategy is discussed in detail in Section 5.

4. The dataset

4.1. The observational dataset and the treatment variable

In the present study we use information from the Italian FADN dataset. Italian agriculture is often considered an interesting case study for the wide heterogeneity of its farming conditions and traditions which inevitably affect farmers' decision-making and farm performance (Esposti, 2017a,b; Coderoni and Esposti, 2018; Coderoni et al., 2024). The Italian FADN sample consists of a representative collection of commercial farms observed over the CAP regime under consideration (2014-2020 then extended to 2022). Although the programming period is 2014-2022, in year 2014 the only treated units concern payments from the previous programming period. Therefore, the actual period of policy implementation of interest here is 2015-2022.[6] The consequent sample comes as an unbalanced panel ranging from 11,389 units in 2015 to 11,084 observations in 2022.

As anticipated, it is assumed that treatment is absorbing: once a unit enters it, it remains exposed to treatment over the remaining observation period. Albeit the dataset always includes treated units, those that were already treated before 2015 have been removed in order to avoid the overlap of the effect of the previous regime (2007-2013). Eventually, the 2015-2022 balanced panel consists of 3,404 farms corresponding to 27,232 observations overall. Since treatment entry is staggered, the number of adopters (i.e., receiving the organic support) rose from 47 in 2015 to 325 in 2022. Therefore, the never-treated farms are 3079 units. Following the notation adopted in Section 3, the cohort indicating always-treated farms is thus J_1=J_{2015} and includes 47 farms. The size of the remaining cohorts is the following: J_{2016}=68, J_{2017}=76, J_{2018}=14, J_{2019}=31, J_{2020}=16, J_{2021}=50, J_{2022}= J_T=23.

The "treatment" variable is here defined as a binary indicator which equals one in the case of receiving the CAP support for organic farming. Specifically, the support for organic farming falls within the RDP Measures 11.1 and 11.2 in the programming period 2014-2022 (see Stolze et al., 2016). Two aspects are worth noting about this definition of the treatment variable. First, organic farms might not all have received the specific support, both because resources are not sufficient to meet all requests for access to individual measures and because not all organic farms choose to apply for support, especially concerning the maintenance of the organic production method. In the present case, however, these cases are very few and have been excluded from the sample. Therefore, being organic and receiving the support here are two fully corresponding conditions. Secondly, such monetary support varies across farms suggesting that the treatment should be considered multi-valued rather than binary. Nonetheless, though farm-specific, this support actually depends on the farm size. Therefore, if variables are expressed in relative terms as in the present case, considering the policy support as a binary treatment remains a reasonable approximation (Coderoni et al, 2024).

4.2. Outcome variable and covariate set

The proper definition of the outcome and confounding variables (**X**) is guided by the theoretical framework presented in Section 3. The outcome variable represents the farmer's private objective

[6] At the time of writing, validated FADN data for 2023 had yet to be released and, in any case, this year would refer, at least partially, to the new CAP regime (2023-2027) that started on 1st January 2023.

in adopting the policy ($\Delta\pi$ in the theoretical model). Empirically, this outcome is measured as the change in net farm income (as reported in the FADN dataset), augmented by the policy support for organic farming. This variable, henceforth referred to as NI, is expressed per unit of autonomous or family labour, measured in Family Annual Work Units (FAWU). Therefore, the outcome variable is defined as NI/FAWU, as this is likely the metric that most directly influences farmers' decision-making.

As for the confounding variables (**X**), it is important to recall that these are intended to capture all potential external and internal sources of observable heterogeneity in farmers' production decisions (Esposti, 2024; Coderoni et al., 2023). At the same time, they are assumed to be exogenous, meaning they are expected to influence both treatment assignment and the outcome, but not to be influenced by either. In line with a common strategy in the literature (Coderoni et al., 2024, 2025), only pre-treatment or time-invariant confounders are considered.

This set of covariates is designed to characterize the operating environment, labour force, and structural characteristics of the farms. Regarding the operating environment, regional and altitude dummy variables are included. Additionally, annual cumulative rainfall, its 10-year moving average, and the annual percentage deviations from this average are considered to account for the effects of climate and weather variability. Time dummies are also included to flexibly control for temporal trends affecting all farms simultaneously. As for the labour force, the presence of employees on the farm is captured by a dummy variable. Furthermore, the age and education level of the farm head are included. Finally, the structural characteristics of the farm include: production specialization, economic size, utilized agricultural area, the share of forest area in total surface area, the share of FAWU in total labour input, the share of fixed costs in total costs, and the share of other gainful activities in total farm revenues. A quadratic polynomial of geographical coordinates is also included in the specification to proxy farm-level fixed effects (Wang, 2023).

Further details on the outcome variable and covariates used in the study are provided in Annex 1.

5. Estimation approach

The estimation of TE under general interference is based on the identification logic expressed by the theoretical model illustrated in Section 3 and depicted in Figure 2. This framework implies that the farm's outcome is influenced by the treatment history of the farm itself and of neighbouring farms. This estimation strategy adapts the estimation algorithm recently proposed by Wang (2023, p. 16). Eventually, this algorithm consists in implementing the Hajek estimator (Hajek, 1971) in a regression framework where dummy variables are used to define different treatment histories. In fact, such a regression model expresses a linear relationship between the observed outcome variable, modified to account for interference among neighbouring units, and the farm's treatment history. This modified outcome is also called *transformed outcome* and is indicated as μ_{it}.

For any i-th farm at a given time *t* and for a selected radial distance *d*, this transformed outcome is computed as

$$(5) \qquad \mu_{it}^{d} = \left(\frac{\sum_j \Delta\pi_{jt}}{\sum_j 1_j}\right)_{j\in\Omega_d}, \forall i, j \in N$$

where Ω_d is the circular area of ray *d* drawn around the i-th unit and $\sum_j 1_j$ counts the total number of units (i-th farm included) falling within the area. Therefore, this transformed outcome is the average outcome of neighbouring farms of any i-th, it includes the i-th farm itself and can be obtained by increasing the radial distance d. When $d = 0$ it follows that $\mu_{it}^{0} = \Delta\pi_{it}$. The selected radial distance d also generates the respective NxN spatial time-invariant weight matrix **W** discussed in Section 3 and now indicated as $\mathbf{W}_d$.

The challenging aspect of this empirical strategy concerns the representation of the farm's treatment history. By admitting staggered treatment and a dynamic TE, the treatment history emerges as the combination of two distinct aspects: treatment status and time exposure. On the former point, three treatment statuses can be defined under general interference. They can be expressed via tx1 dummy variables, δ, where $t \in (1,\ldots,T)$: farms that are never treated, denoted by dummy $\delta_{(0,\ldots,0)}$; farms that are always treated, denoted by dummy $\delta_{(1,\ldots,1)}$; farms treated only in the latest period, namely at time t, denoted by dummy $\delta_{(0,\ldots,1)}$. Since we assume here that the treatment is absorbing, this latter group remains exposed to the treatment for all the following period, namely interval $(t,\ldots,T)$.

Temporal interference, namely dynamic treatment effect, depends on the duration of exposure to the treatment, k, indicating how long a unit has been under treatment. For a generic period $t \in (1,..T)$, this time exposure is expressed by interval (or vector) $[t:(t+k)]$. At the entry year there is no exposure in the sense that $k=0$ and the vector $[t:t]$ simply expresses the treatment status in the entry year t. In this case, the treatment status thus takes the conventional binary form: 0 for non-treated units at time t, 1 for treated ones. When exposure occurs and becomes longer ($k>0$), it is possible to define more than two treatment statuses. For a one-year exposure after the entry, namely $k=1$ and $[t:(t+1)]$, the treatment status can be expressed by a two-element vector taking value $(0,1)$ for units that have been treated in the current year but not in the previous one, $(1,1)$ for farms that have been treated in both years, $(0,0)$ for farms that have not been treated in the last two years. For a two-year exposure after the entry, namely $k=2$ and $[t:(t+2)]$, we can have several three-element vectors, in particular $(0,0,1)$ for farms that have been treated in the current year but not in the previous ones, $(0,1,1)$ for farms that entered the treatment at year t+1, $(1,1,1)$ for farms that have been treated also in all previous two years, $(0,0,0)$ for farms that have never been treated during the last three years.

From these examples, it follows that a greater k makes the comparison $\left[\delta_{(1,\ldots,1)} - \delta_{(0,\ldots,0)}\right]$ more complete and informative about the response to the treatment. The TE obtained from a $\left[\delta_{(1,1,1)} - \delta_{(0,0,0)}\right]$ comparison is more likely to capture the carryover effect of the treatment than a $\left[\delta_{(1,1)} - \delta_{(0,0)}\right]$ comparison. Specifying different values of k thus allows the analyst to identify and estimate the TE over different treatment exposure, thus assessing the shape of the dynamic TE depicted in Figure 1. Nonetheless, the identification of such dynamic TE shape encounters a major practical problem. The longer the exposure (k) the smaller the available dataset to identify the TE. Due to the staggered treatment entry, by definition the set of treated units observed with $k=\kappa$ contains (is a superset of) the set of treated units with $k=\kappa+1$. But a smaller dataset may evidently imply less power for statistical inference, thus leading to weaker statistical robustness. This reveals the trade-off associated with the identification and estimation of the TE with different exposure k under staggered entry: a smaller k admits more observations and a higher statistical quality while a greater k allows for a more complete representation of the carryover effect or dynamic TE. Therefore, having a long-enough panel with staggered entry also allows to empirically assess the real implications of this trade-off.

As illustrated in Section 3, the combination of this time exposure term k (with the consequent treatment status expressed by vectors δ) with the spatial dimension as expressed by d (with the consequent spatial weight matrix $\mathbf{W}_d$) allows to decompose the TE into different interference terms. In particular, four estimands can be separately identified:

- *direct effect* obtained as the difference between the outcome (d=0) of farms that just entered the treatment with those that have never been treated.
- *spatial interference* obtained as the difference between the average transformed outcome (over different d>0) of farms that just entered the treatment with those that have never been treated.

- *temporal interference* obtained as the difference between the outcome (d=0) of farms that have been always treated (with exposure k>0) with farms that have never been treated.
- *both interferences* obtained as the difference between the average transformed outcome (over different d>0) of farms that have always been treated (with exposure k>0) with farms that have never been treated.

Table 2 summarizes the identification logic of the different estimands under general interference. Notably, this approach allows for the separate identification and estimation of spatial and temporal interference whenever $d > 0$ for a given k. To make these additional effects (pure spatial and time interference) explicit, Table 2 distinguishes among four different treatment effects: Direct Effect (DE^k), Time-interference Effect (TE_d^k), Spatial-interference Effect (SE_d^k), and Spatial and Time-interference Effect (STE_d^k).

The identification strategy illustrated above must satisfy the three usual restrictions on the data-generating process implied by the CI approach based on the potential outcome framework (Coderoni et al., 2025): the Conditional Independence Assumption (CIA), the Stable Unit Treatment Value Assumption (SUTVA), the overlap (or balancing) condition. In principle, and following Wang (2023), within the adopted approach the SUTVA is satisfied via the definition of the *transformed outcome*, μ_{it}^d, that takes the influence of the neighbouring units into account. The CIA condition, also known as unconfoundedness or ignorability, requires that the distribution of this potential outcome is independent of the treatment. This condition is typically met conditional on a set of pre-treatment covariates (also known as *confounders*), $\boldsymbol{X}_i$, that are expected to affect both treatment participation (or assignment). According to the theoretical framework illustrated in Section 3, these confounders correspond here to the (Qx1) internal and external structural factors affecting the farm's technology and decision making. Formally, the CIA can be formulated as $\mu_{it}^d(0), \mu_{it}^d(1) \perp T_{it}|\boldsymbol{X}_i$.

In dynamic treatment settings, however, the Conditional Independence Assumption (CIA) must also hold in a dynamic form, what Wang (2023) refers to as *sequential ignorability* (or sequential conditional independence). As illustrated in Figure 2, this assumption implies that the current treatment status of each unit depends solely on its observable history. Under sequential ignorability, the estimators presented below can effectively account for both temporal and spatial interference, although their validity still requires that unobserved confounders are not present.

Finally, the overlap condition requires that, for different configurations of $\mathbf{X}_{\mathrm{it}}$, comparable units are found in either of the two treatment conditions for different configurations of $\mathbf{X}_{\mathrm{it}}$. Since this assumption is crucial for the identification of the TE, its validity will be assessed in Section 6.4 within our farm sample.

As anticipated, in order to implement this identification approach and achieve an estimation of the ATE, namely $E(\mu_{it}^d)$, and of the different terms of Table 2, the Hajek estimator is adopted. It consists of a normalized Inverse Probability Weighting (IPW) estimator[7] and is implemented using the following weighted regression equation:

$$(6) \qquad \tilde{\mu}_{it}^d = \alpha_D \tilde{\delta}_{(0,\dots,1)} + \alpha_T \tilde{\delta}_{(1,\dots,1)} + \alpha_N \tilde{\delta}_{(0,\dots,0)} + \alpha_R \tilde{\delta}_{residual} + \widetilde{\mathbf{X}}'_{it}\boldsymbol{\beta} + \tilde{\varepsilon}_{it}$$

where: $\tilde{\mu}_{it}^d$ represents the weighted transformed outcome μ_{it}^d for the selected radial distance d; $\tilde{\delta}_{(0,\dots,1)}$, $\tilde{\delta}_{(1,\dots,1)}$, $\tilde{\delta}_{(0,\dots,0)}$ are the weighted dummies for the three abovementioned treatment statuses, while $\tilde{\delta}_{residual}$ is the dummy accounting for each intermediate treatment status, for example $(0,1,1)$ in the case of k=2; α_D, α_T, α_N, and α_R are the unknown coefficients associated

[7] Within a TE context, IPW estimation is also referred to as Inverse Probability of Treatment Weighting (IPTW) (Wang, 2023).

to the these dummies; $\tilde{\mathbf{X}}_{it}$ is the (Q x 1) vector of weighted confounders with the respective (Q x 1) vector of unknown coefficients; and $\tilde{\varepsilon}_{it}$ is the error term.

Once d is selected, equation (6) is specified and estimated for any given k (therefore, T times). These estimations are made over the respective exposure set, $\sum_{s=k+1}^{s=T} J_{s-k}$. It follows that the size of the panel sample on which the estimation is performed declines with k. In (6), the coefficient associated to the dummy of a given treatment status (namely, α_D, α_T, α_N, α_R) allows the identification of the estimands of interest. In particular, depending on the definition of the transformed outcome variable $\tilde{\mu}_{it}^{d}$, the differences $(\alpha_E - \alpha_N)$ and $(\alpha_T - \alpha_N)$ identify the effects of interest here. If d=0, thus $\tilde{\mu}_{it}^{0}$ is the observed outcome, the difference $(\alpha_E - \alpha_N)$ identifies and estimates DE^k, while the difference $(\alpha_T - \alpha_N)$ identifies and estimates TE^k. For any d>0, and consequent transformed outcome $\tilde{\mu}_{it}^{d}$, $(\alpha_E - \alpha_N)$ identifies and estimates SE_d^k, while the difference $(\alpha_T - \alpha_N)$ identifies and estimates STE_d^k. TE_d^k can also be identified with d>0 as the difference $(\alpha_T - \alpha_E)$.

It is worth noting that this identification and estimation logic motivates why Wang (2023), following a growing body of recent literature (Aronow and Samii, 2017) refers to these effects as Average Marginal Effects (AME) rather than the more conventional ATE. This terminology aligns with a design-based causal inference approach under spillovers where the standard no-interference assumption (see above) does not hold. In such settings, it is no longer meaningful to define a unit as simply "treated" or "untreated" in isolation, since control units may still be affected by treatments assigned to others. Moreover, the AME captures the change in the outcome resulting from a marginal (i.e., infinitesimal or small) change in the treatment status of a unit. As Equation (6) shows, the AME behaves like a partial derivative with respect to the treatment variable, rather than a discrete comparison between treated and untreated units. For consistency with the derivation and terminology used in Section 3, and for the sake of clarity, we continue to use the term "Treatment Effect" rather than "Marginal Effect", although the underlying approach follows Wang (2023).

Ultimately, in practical terms, this approach implies that for any treatment exposure window $[t\colon(t+k)]$, and for any selected d, a distinct specification of model (6) is estimated, yielding a corresponding set of coefficients. Therefore, estimation is repeated by varying the parameter k and radial distance d. The latter is here defined between 0 and 300 km with a step of 25 km (thus a set of 12 possible radial distances is considered),[8] while, given the period under analysis (2015-2022), the treatment exposure takes values $k = (0,1,\ldots,7)$ for period 2015-2022. Moving over different values of k and d, this estimation procedure allows to obtain consistent estimates for the different estimands (ATEs) illustrated in the theoretical section and summarized in Table 2 (direct effect, time interference, spatial interference, both interferences or total effect), and to assess the shape of the dynamic TE depicted in Figure 1.

The Hajek TE estimation is implemented by firstly performing a Least Square (LS) estimation of equation (6), therefore a Weighted LS (WLS) estimation, and then taking contrasts between the estimated dummies of treatment statuses. Appropriately weighting the units ensures that the estimated regression coefficients in (6) converge to the real underlying values, thus making this estimation consistent (Wang, 2023). The weighting of variables is performed by dividing each variable with the normalized probability of treatment history. In the case of the outcome variable, this weighting proceeds as follows:

$$(7) \qquad \tilde{\mu}_{it}^{d} = \frac{\mu_{it}^{d}}{\sqrt{\bar{p}_{i|t:(t+k)}}} \text{with} \quad \bar{p}_{i|t:(t+k)} = \frac{p_{i|t:(t+k)}}{\sum_{i}^{N} p_{i|t:(t+k)}} \text{ and } \quad p_{i|t:(t+k)} = \prod_{t}^{t+k} P(T_{it} = 1|\,\mathbf{X}_{it})$$

[8] This spatial grid is inspired by the evidence reported by Baldoni and Esposti (2021) regarding the diffusion of productivity shocks within Italian agriculture.

$P(T_{it} = 1|X_{it})$ is the treatment probability of the i-th observation at time t estimated using a Spatial Probit Model (SPM) (Martinetti and Geniaux, 2017), also known as Propensity Score (PS). A SPM is used in order to account for contamination, that is, the possible influence coming from the treatment status of neighbouring units on the it-h unit treatment assignment or participation choice at time t. As discussed in Sections 2 and 3.3, and summarised in Table 1, this influence can be particularly relevant in the case of agri-environmental measures of the CAP and, therefore, it is explicitly admitted here. This modelling solution represents a major novelty with respect to the original approach that assumes no contagion, namely the independence of the i-th unit's treatment status from neighbouring units' treatment status (Wang, 2023, p.12).[9]

Probabilities in (7) are defined differently for treated and non-treated farms. For treated farms $P(T_{it} = 1|\mathbf{X}_{it})$ simply is the probability of treatment predicted by the SPM, $P(T_{it} = 1|\mathbf{X}_{it}) = \hat{P}_{it}$. For non-treated farms, it is defined as $P(T_{it} = 1|\mathbf{X}_{it}) = 1 - \hat{P}_{it}$, that is, the probability of no treatment. Weighting for these probabilities represents the inverse probability framework that adjusts for selection bias. The rationale behind inverse probability weighting is based on the intuition that a treated unit with low probability of being treated deserves more weight because it has characteristics similar to the farms of the control group. Similarly, among non-treated units, more weight is acknowledged to control units showing similar characteristics to treated farms (Hernán and Robins, 2025; Rosenbaum and Rubin, 1983).

Once $E(\tilde{\mu}_{it}^d)$ has been estimated via regression model (6) and weights (7), respective standard errors, $\sigma(\tilde{\mu}_{it}^d)$, can also be consistently estimated using the spatial Heteroscedasticity and Autocorrelation Consistent (HAC) variance estimator with spatial weight matrix $\mathbf{W}_d$ (Conley, 1999; 2008). Estimated $\hat{\sigma}(\tilde{\mu}_{it}^d)$ allows to construct the confidence interval at any significance level around all estimands of interest[10].

6. Results

6.1. Direct effects

Among the results obtained, the first noteworthy finding concerns the "direct effect," as defined in the taxonomy discussed earlier and summarized in Table 2. This effect corresponds to the impact of adopting organic farming, estimated using conventional identification and estimation approaches that ignore both temporal and spatial interference (i.e., with d=0), such as in the case of Difference-in-Differences (DID) estimation (Esposti, 2025). However, due to staggered treatment adoption, this direct effect is expressed as DE^k, meaning it varies across cohorts k (see Section 3). To account for potential heterogeneity across sufficiently large groups of farms, results are reported for the full sample, by farm size (categorized as small, medium, and large), and by farm specialization. Due to space constraints, in the latter case, only dairy farms are distinguished from all other farms, as dairy farms appear to show the greatest variation in the effects of organic farming adoption.

Table 3 presents the estimated direct effects for the various groups and across different values of k (cohorts). It is important to recall that parameter k defines the length of the treatment history, where $k=0$ represents the treatment history only of year 2015, while $k=2$ represents the treatment history of years 2017, 2016 and 2015. Thus, results referring to $k=2$ concern all units that entered into treatment in year 2017 and that have not been treated in the two preceding years. In this sense,

[9] This possible improvement of the approach is actually suggested by Wang himself: "The assumption of no contagion can be lifted by assuming that the propensity score is a function of the outcome history and treatment assignment history of nearby units" (Wang, 2023, p.18).

[10] The cutoff value for the HAC variance estimator is set equal to d.

DE^k in Table 3 shows the cohort-specific impact of organic farming adoption exclusively at the entry year.

It emerges that, over the whole sample, the direct effect is positive for any treatment-entry cohort (namely, value of k). However, it seems quite volatile, as it ranges between 8.8 K€ (observed for k=4) and 47.1 K€ (observed for k=0). In fact, if we exclude k=0, the estimated $DE^k$is quite stable across ks (from 8.8 to 19.4 K€). This difference in the magnitude and, above all, in the statistical significance of the direct TE should not surprise since the staggered entry into the treatment (i.e.. moving across the different values of k) implies different samples of entrants into treatment in terms of characteristics and numerosity.

Similar evidence is also found for the three different size groups. The direct effect is always positive with a loss of significance with the increase of k. However, a significant positive direct effect is found mostly for k =0, though it is also obtained for some other values of k (namely, k=1, k=3, k=6 and k=7). Overall, size groups mostly differ for the magnitude of the direct effect since the impact is much larger (more than 10 times) for larger size farms. It is worth reminding that the outcome variable under consideration is NI/FAWU, therefore it is expected to be relatively size-independent in the case of family farms. Moving to large farms, however, the incidence of family work on the total farm's labour force declines and this may explain the much larger impact of organic farming adoption. It is also worth reminding that, as detailed in Section 4.2, NI includes the policy support. Therefore, an always positive direct impact seems consistent with the economic advantages farms are pursuing in adopting organic farming and which is expected to grow the larger the farm in order to justify its adoption.

In terms of heterogeneous response, the most relevant difference emerging from Table 3 concerns the farm specialization groups. Among the latter, it is worth paying attention to dairy farms that show a different behaviour with a statistically significant (but for k=0) and increasingly negative impact over cohorts. This peculiar behaviour of dairy farms is confirmed by the TE_0^k reported below and, therefore, deserves specific attention since it would suggest an apparently irrational farms' choice: they decide to adopt organic production regardless of the economic loss they suffer since the entry despite the policy support. This could only be explained by other sufficiently strong non-monetary motivations (Esposti, 2024). All the other farms behave as observed for the whole sample. Though not reported in Table 3 (results for other groups of farm specialization are available upon request), among the different specializations, effects are positive and larger particularly for farms specialised in perennials and in other livestock activities rather than dairy production.

In any case, the differences observed across farm groups, particularly in terms of size and, more notably, production specialization, and regarding both the magnitude and the sign of the direct effect, may help to explain why these effects are only weakly statistically significant in the full sample, at least for several cohorts (i.e., values of k).

6.2. Time interference

According to the theoretical framework (Section 3) and the methodological approach (Section 5) adopted, assessing the impact of organic farming adoption solely on the basis of its direct effect may be misleading. This is because various economic mechanisms can activate and interact, leading to temporal and spatial interferences. Table 4 reports the time interference (or carryover) effect after k+1 periods, that is TE_0^k. Therefore, the sequence of results with an increasing value of k provides evidence on the dynamic TE, that is, how the impact of organic farming adoption varies over time after the entry into the treatment. As already stressed, due the staggered adoption, these values are computed on the different cohorts, namely on different samples.[11] Nonetheless, the sequence of results with an increasing value of k provides evidence on the dynamic TE, that is, how the impact of organic farming adoption varies over time after the entry into the treatment.

When the whole sample is considered, it emerges that only a few k's show a statistically significant TE_0^k and they mostly concentrate in larger values of k. This helps to appreciate how a large k may stabilize the results as a longer exposure improves the statistical significance despite the associated loss in the number of observations. The nature of this time interference seems somehow unstable with alternating positive and negative values. If the whole sample is considered, in particular, it emerges that after a large impact in the entry year (the direct effect), TE_0^k is mostly not significant except for *k*=7 when a large positive dynamic effect is statistically identified. The estimated TE_0^k increases non monotonically from -15.1 K€ (*k*=2) to 13.7 K€ (*k*=6) and finally to 26.6 K€.

In fact, this non-monotonic and apparently unstable dynamic response should not come as a surprise, for two main reasons. First, as repeatedly shown in recent studies (Esposti, 2025), assessing the impact of farm choices or policy measures over a single year, or a very short time span, can be misleading or, at best, inconclusive. This study confirms such inconclusiveness. Second, heterogeneous responses across different groups of farms may result in weakly significant and unstable outcomes at the aggregate level. This heterogeneity is also confirmed here, with major differences particularly evident across farm sizes and, to some extent, farm specializations, dairy farms being a notable example.

Despite some statistical limitations, the results presented in Table 4 provide preliminary insights into the shape of the dynamic treatment effect. Figure 3 illustrates these results to help determine whether the response is increasing or decreasing, and whether it follows a monotonic pattern (as discussed in Figure 1). Except for dairy farms, Figure 3 appears to confirm a low or even negative initial effect, which then increases over the exposure years, reaching positive values towards the end of the period. This pattern seems different across different size classes, though with statistically significant and positive TE_0^k observed for *k*=5, 6 and 7. Medium-sized farms exhibit the most monotonic and steadily increasing pattern. A monotonic pattern (for *k*>1) is also observed for large farms, although starting from negative values. In contrast, small-sized farms display the most irregular and non-monotonic response. Large farms show the highest effect although only with *k*=7. Medium and small-sized farms present lower TE_0^k but have the advantage that the effect seems to manifest earlier in their treatment history. Dairy farms present the most peculiar behaviour among the farm typologies considered. Their estimated TE_0^k is negative from the very beginning (*k*=1) to the end of the period (*k*=7) with a peak of -307.4 K€ with *k*=5.

As elaborated in Section 3, the economic interpretation of the shape of this response concerns the farmers' intertemporal decision making. As the outcome variable (NI/FAWU) also includes the policy support, combining the strong direct effect reported in Table 3 with these longer-term impacts, we might argue that relatively myopic decision makers are attracted by the monetary support that is delivered from the very beginning since the adoption. But then they encounter some progressive adjustment costs that make the adoption decision much less convenient and possibly costly until this adjustment process leads to a positive gain after several years from the adoption. This interpretation, however, does not conceal the apparent irrational adoption of dairy farms, on average, unless one assumes that the positive impact, eventually making adoption rational, only emerges after such a long period of time that it falls outside the observation window considered here (namely, for *k*>7).

In any case, and regardless of the significant heterogeneity across farm typologies, these results seem to suggest that, in the absence of public support, there would be a substantial disincentive

[11] It is worth noting that *k* refers to the cohort including all units that have been treated for at least *k+1* years.

for farms to adopt organic practices in the initial years following adoption. Even if the overall discounted profit were ultimately positive, a myopic farmer might still decide to abandon adoption if sufficient policy support were not provided.

6.3. Spatial and general interference

According to arguments illustrated in Sections 3 and 5, spatial interference has to be also considered to have a comprehensive assessment of the impact of organic farming adoption on a farm's economic performance. This further effect depends on the maximum radial distance (d) at which this spatial interference may occur. Therefore, the total TE varies with both the distance d and the time of exposure k. As summarised by Table 1, however, unlike the case of time, the economics of spatial interference implies a twofold role of space in affecting the impact of organic farming adoption. The first role concerns contagion, namely whether and how adoption of neighbouring units affects the adoption decision of a given farm. In the present study, this effect is admitted by adopting an SPM to estimate the treatment probability, namely the PS, of any farm in the sample.

SPM estimation is reported in Annex 2 (Table A3).[12] Of interest, here, is the sign, magnitude and statistical significance of parameter ρ which is the coefficient associated with the spatial weight matrix of the SPM. This coefficient thus expresses the size and magnitude of spatial correlation (i.e., contagion). It emerges that contagion occurs, since parameter ρ estimate is statistically different from 0 and positive (about 0.28). This positive spatial dependence does not necessarily leave out the involuntary exclusion of some farms, namely that non-participation might not express a choice but rather depends on conjectural circumstances excluding the farm from the treatment (first-type congestion or crowding-out effect) (Table 1). This latter negative influence would imply a negative contagion effect but estimates may simply imply that, *ceteris paribus*, imitation prevails on these congestion effects. This result seems consistent with what was obtained by Baldoni and Esposti (2023) in a similar context.

The second role of space directly concerns the outcome variable and expresses whether external effects of adopting organic farming, arguably via local markets, occur and whether they are positive or negative, namely if local *milieu* prevails on second-type congestion or the other way round. Table 5 reports the estimated spatial effect at different values of d (SE_d^k) together with the total effect (STE_d^k) combining spatial and time interference as summarised in Table 1 and Figure 2. For the sake of space limitation, only results obtained on the whole panel sample are reported. Results obtained on subgroups of farms in terms of both size and production specialization are available upon request, except for the group of dairy farms whose peculiar case is reported in Annex 3.

It emerges that, for d=50 km, a positive and statistically significant spatial effect occurs for all values of ks, that is, all time-exposure cohorts. Spatial interference increases until k=3 and then starts declining. With larger values of d these results are generally confirmed but some differences also emerge. In particular, the spatial effect appears to diminish as the distance d increases, even turning slightly negative, and it loses statistical significance. This evidence suggests that while positive local externalities associated with the adoption of organic farming do emerge, their intensity weakens over time and as the radial distance defining spatial influence expands, that is, as the local nature of such spatial influence progressively fades.

[12] This estimation refers to a spatial weight matrix in which elements take the value 1 whenever farms are located within a 10 km radial distance ($\mathbf{W}_{10}$) and belong to the same region (Baldoni and Esposti, 2023). SPM estimates for greater values of d are available upon request. Longer distances do not significantly affect the results but substantially reduce the sparsity of $\mathbf{W}$, thereby increasing the computational burden. These SPM estimates for different values of d are available upon request.

For all values of *d*, the weakening of the spatial effect appears to be more pronounced for higher values of *k*, which, in fact, correspond to cohorts with longer exposure and therefore lower numerosity. It is worth noting, however, that this decline of the effect with increasing *d* and *k* should not only be attributed to weaker statistical robustness, like for *d*=50 km where statistical significance is maintained across all values of *d* and *k*. Figure 4 confirms this evidence by displaying spatial interference with varying *d* and *k* and extending the radial distance up to a maximum of 300 km.

Both Table 5 and Figure 4 also report the total effect obtained by admitting both time and spatial interference. This not only allows to assess the incidence of the spatial effect on the total impact but also provides the most comprehensive, thus the most reliable, estimation of the TE itself. Three aspects are worth mentioning in this respect. First of all, unlike direct and carryover effects (Table 3 and 4), the total effect seems more stable and statistically robust. TE estimates are positive and statistically significant for all values of *d* and *k*. Magnitude ranges between a minimum of 9.4 K€ and a maximum of 32.6 K€, with larger values concentrating at a smaller distances (*d*=50 and 100 km) and larger values of *k*. Such an increase of the net farm income per unit of family labour (NI/FAWU) seems more plausible than some extreme values observed for the direct and carryover effects.

Secondly, and besides this robustness, Table 5 and Figure 4 indicate that the incidence of spatial interference is large, particularly in the first years after entry, that is, for lower values of *k*. Figure 4 displays the spatial effects and the combination of both effects (thus, the total effect) analogously to what was proposed by Wang (2023, Figure 1). The incidence of the spatial effect is easily visualised by the gap between the two lines: the larger the gap the lower the incidence of the spatial effect. It emerges that this gap enlarges with the increase of *k* for all values of *d*. If we consider the *d*=50 km case, for which the spatial effect is always positive and statistically significant, this incidence is just below 90% for *k*=1 and *k*=2 but then declines down to about 25% for *k*=6 and *k*=7. This would confirm that the spatial interference is not invariant with *k*. Two interpretations may be given for these findings. The first interpretation can be that economic mechanisms underlying spatial effects, though relevant, seem to behave as temporary forces concentrating in the first years after policy implementation.

The second interpretation is more related to the characteristics of the estimation design and the trade-off between the estimation of temporal and spatial effects: a larger *k* allows to better capture carry-over effects but is also associated with a smaller sample size that may particularly impact the identification of spatial effects. In this respect, Annex 4 (Figure A2) provides an alternative visualization of the results shown in Figure 4, to better illustrate how the adopted methodology flexibly exploits the information in the panel dataset to separately identify spatial and temporal interference under staggered (but absorbing) treatment entry. In Figure A2, spatial, temporal, and combined effects are displayed separately, while within each visualization, estimates for different values of *k* and *d* are displayed together. This alternative visualization helps to clarify the relative prevalence of spatial interference over temporal effects, and vice versa, depending on the specific values of *k* and *d*.

Thirdly, the variation of total effects estimate over the increasing values of *k* allows to derive the shape of the dynamic response to the treatment (Figure 1). This shape is displayed in Figure 5. Unlike Figure 3, the patterns reported in Figure 5 take the spatial effects into account and are thus replicated for different possible values of *d*. The U-shape of Figure 3 now becomes a more regular and almost flat pattern with a small impact in the initial years after entry, which are then followed by a slow but constant growth that reaches its maximum after several years within the treatment. This shape seems to be observed regardless of the distance *d* considered but it tends to be slightly flatter with longer distances (*d*=150 km and *d*=200 km).

For the sake of completeness, although the spatial and total effects detailed by sub-groups of farms are not extensively reported here, it is still worthwhile to comment on the results obtained for the sub-group of dairy farms. Table A4 and Figure A1 in Annex 3 correspond to Table 5 and Figure 4, and confirm that, for these farms, the effect of adopting organic farming on economic performance becomes negative shortly after entry and then worsens steadily with longer exposure to the treatment. The prevalence of spatial interference is also confirmed and, in fact, reinforced. The peculiarity of the dairy farms thus concerns the sign of the impact of the adoption of organic farming more than the shape of the dynamic effect. As shown in Figure 5, the shape is monotonic and almost linear also for dairy farms, but it is oriented in the opposite direction. In this case, it is further confirmed that, regardless of any initial economic advantage, the adoption of organic farming entails a significant adjustment effort, resulting in a negative impact that, in the case of dairy farms, is never offset within the observation period.

6.4. Results validation

Results obtained in the present study may critically depend on the assumptions made about both the identification of the TE and the role of space. Three aspects, in particular, are under attention here to assess the robustness of our results with respect to possible violations of these assumptions.

The first aspect concerns the identifying assumption known as overlap condition for the covariates included in the analysis (see Section 5). Table A1 (Annex 1) presents summary statistics for these variables, while Table A2 (Annex 2) reports the distribution of the propensity scores (PS) obtained via Probit model estimation, both with and without spatial interference (or contagion). These estimates refer to the full dataset (i.e., the case $k = 0$). On the one hand, Table A1 shows that several covariates exhibit overlap between treated and non-treated farms, although some (such as UAA, economic size, and specialization) display statistically significant differences in mean values between the two groups.[13] In any case, the overall distribution of estimated PS indicates that treated and non-treated farms have largely overlapping probabilities of receiving the treatment (Table A2). Based on this evidence, the overlap assumption can be considered valid in the context of the current analysis.

The second assumption for which it seems worth assessing the sensitivity of the results concerns the unconfoundedness assumption (or CIA). Following the suggestion of Wang (2023), a placebo outcome test is conducted to assess the plausibility of this assumption. This test makes use of a placebo outcome variable, namely an outcome that should not be affected by the treatment or intervention being studied. The idea is that if treatment assignment is truly independent of unobserved confounders, then it should have no effect on an outcome that is not causally affected by the treatment (Imbens, 2014).[14]

Here, the outcome variable NI/FAWU is replaced with a placebo outcome, defined as the percentage deviation of yearly cumulated rainfall from its 10-year average. This variable is clearly expected to be exogenous and, most importantly, entirely independent of policy adoption as well as of any farm characteristics, let alone their treatment history. The estimation strategy described in Section 5 is replicated on this outcome variable using the same dataset, namely, the 3,404 units of the balanced panel and the same covariate set, excluding all weather and climate-related variables.

[13] A t-test is used to determine whether the difference between the means of treated and non-treated units is statistically significant. For the sake of space limitation, the results of this test are not reported here but are available upon request.

[14] Placebo tests could also be performed via a placebo treatment, namely a placebo treatment test assigns a fake or irrelevant treatment to assess whether the model incorrectly detects an effect.

Table 6 reports the full set of treatment effects, including direct, dynamic, spatial effects, and general interference, alongside the corresponding results obtained using the actual outcome variable (NI/FAWU) as reported in Tables 3, 4 and 5. Due to space constraints, only selected values of k and d are shown.[15] Specifically, k=1 (k=0 for DE) is considered, as it allows for the inclusion of all effects while preserving the largest possible sample size. Regarding distance, d=100 is chosen as the best compromise between a range that is too narrow and one that is excessively broad for capturing spatial interference. Table 6 shows that all the estimated effects on the placebo outcome are negligible and none reach statistical significance. This absence of significant associations supports the validity of the unconfoundedness assumption, suggesting that selection on unobservables is unlikely and this reinforces the reliability of the adopted identification strategy?

The final aspect for which it seems worth assessing the sensitivity of the results concerns contagion. As already illustrated in previous sections, the present study accounts for contagion, that is, spatial dependence in treatment adoption. To evaluate the extent to which accounting for contagion affects the results, the model is also estimated under the assumption of no contagion, as in Wang (2023). This consists in estimating treatment probability $P(T_{it} = 1|\boldsymbol{X}_{it})$ excluding the spatial interference term, namely using a Probit Model instead of an SPM specification.

The estimation results are expected to vary under this alternative specification, depending on the presence and nature of contagion. However, robust findings should largely persist, as contagion affects the propensity to adopt the policy rather than the policy's effect itself. Table 6 compares, for the selected values of k and d, the direct, dynamic, spatial, and total effects with and without accounting for contagion. While the estimates without contagion yield qualitatively similar results (except for the direct effects), the inclusion of contagion leads to quantitatively stronger effects, more statistically robust and indicating a larger impact of organic adoption on NI/FAWU. This can be interpreted as indirect evidence of spatial interference mechanisms, likely driven by a combination of imitation and positive local externalities.

7. Concluding remarks

This paper investigates the identification and estimation of policy effects under general interference, defined as the joint presence of spatial correlation and dynamic carryover effects. While both aspects have individually attracted much attention in recent literature, studies admitting both are relatively rare due to the major methodological challenges this combination raises. Building on Wang (2023), the present contribution aims to fill the gap by assessing the impact of organic farming adoption on farms' economic performance.

Results confirm that disregarding one of these forms of interference can produce misleading evidence, thus resulting in inappropriate policy conclusions. Though partially temporary, spatial interference occurs up to a remarkable distance of 200 km and may account for more than 90% of the total effect. The dynamic TE, or carryover effect, is itself relevant especially after several years from adoption thus pointing to a period of gradual adjustment before achieving the full economic advantage of organic farming under policy support. Heterogeneity also emerges in this respect, with large-size farms showing a much larger impact after a sufficiently long period of adoption while some specific production specializations, and dairy farms in particular, report a negative impact that is only partially reabsorbed over time of stay in the treatment.

[15] Results for all other values of k and d are available upon request.

The policy implications of the results here obtained are evident. The impact of geographically explicit measures, like environmental policies, can be either amplified or mitigated by the concurrence of both forms of interference. Not only, this should be properly considered in evaluating these policies. More importantly, this effect may bring about/lead to/enhance significant improvements in policy implementation. Spatial interaction may induce a calibration of the strength of policy intervention on the basis of the specific local context. Time interference suggests that, to be fully realized, the policy impact may take time due to the adjustment processes of farms themselves, and this should be considered in the assessment as well as in the introduction of further policy tools to accompany this adjustment. Ultimately, these results highlight the fundamental rationale for this temporary policy support. They show that, without public assistance, farmers may face significant early-stage disincentives to adopt organic farming. Even when the overall discounted profit is positive, a myopic farmer might still choose to give up adoption.

In any case, these results may surely require further confirmation and deeper understanding. Application of the proposed approach to other environmental policies or to other datasets (other EU countries or periods, for instance) could be informative particularly to assess the possible trade-off between a lower sample size but longer treatment history. In addition, and more importantly, some possible methodological improvements can be put forward. Following Wang (2023), the adopted estimation approach could be compared with alternative methods in order to assess their robustness together with further and more sophisticated placebo tests. Another possible direction for future research could be the extension of the role of covariates by either expanding the respective set or by admitting spatial interference also on these variables. A more sophisticated and accurate definition of the spatial scope within which the relative interaction occurs could also be considered. This definition might also be tailored on the specific characteristics of farms by admitting that spatial interaction, besides contiguity, is also affected by production and technological proximity.

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Table 1 – Economic motivations of the time and spatial interference in policy assessment.

Time interference

		Sign of the interference	
		Positive	*Negative*
Nature of the interference	*On treatment effect*	Accumulation of positive treatments effects.	Initially relevant adoption/adjustment costs then followed by positive treatment effects.

Spatial interference

		Sign of the interference	
		Positive	*Negative*
Nature of the interference	*On treatment participation*	Imitation/Contagion effect (under voluntary participation).	First-type congestion (or crowding-out) effect (under involuntary exclusion).
	On treatment effect	Localised external economies	Localised external diseconomies (or Second-type congestion effect)

Table 2 – Identification of the different estimands depending on the presence and nature of interference.

	$\delta_{(0,\ldots,1)}$	$\delta_{(1,\ldots,1)}$
d=0	$\delta_{(0,\ldots,1)} - \delta_{(0,\ldots,0)}$ Direct effect (DE^k)	$\delta_{(1,\ldots,1)} - \delta_{(0,\ldots,0)}$ Time interference (TE_0^k)
d >0	$\delta_{(0,\ldots,1)} - \delta_{(0,\ldots,0)}$ Spatial interference (SE_d^k)	$\delta_{(1,\ldots,1)} - \delta_{(0,\ldots,0)}$ Both interferences (STE_d^k)

Source: Author's elaboration on Wang (2023, Table 1)

Table 3 – Direct effects (DE^k) by exposure sets as identified by k (k=0,….,7) and groups of farms: K€ of net farm income per unit of family labour. Standard errors in parentheses.

	k=0	*k=1*	*k=2*	*k=3*	*k=4*	*k=5*	*k=6*	*k=7*
Observations	27,232	23,828	20,424	17,020	13,616	10,212	6,808	3,404
All units	47.1	11.7	12.2	19.4	8.8	12.2	11.9	17.8
(100%)	(15.0)***	(12.0)	(12.9)	(10.4)*	(10.8)	(10.4)	(15.7)	(21.4)
Small	3.8	12.6	10.4	9.1	3.5	-0.1	2.7	7.9
(16.6%)	(5.7)	(7.6)*	(6.5)	(7.8)	(6.2)	(5.1)	(3.3)	(2.6)***
Medium	15.9	9.0	8.8	15.9	13.2	17.1	27.2	35.7
(47.5%)	(3.7)***	(4.9)*	(7.2)	(9.1)*	(10.6)	(13.0)	(15.9)*	(21.2)*
Large	159.1	1.0	5.4	20.6	30.3	59.6	77.1	262.7
(35.9%)	(55.1)***	(30.9)	(43.0)	(51.0)	(57.1)	(67.5)	(79.3)	(61.4)***
Dairy	-61.6	-121.4	-185.7	-276.5	-321.3	-324.9	-281.5	-250.7
(11.6%)	(48.9)	(53.4)**	(56.7)***	(56.1)***	(56.3)***	(70.5)***	(68.3)***	(51.3)***

Statistical significance: ***=1%, **=5%, *=10%.

Table 4 – Dynamic treatment effects for different time exposure (TE_0^k) and groups of farms: K€ of net farm income per unit of family labour. Standard errors in parentheses.

	k=1	*k=2*	*k=3*	*k=4*	*k=5*	*k=6*	*k=7*
Observations	23,828	20,424	17,020	13,616	10,212	6,808	3,404
All units	0.5	-15.1	-5.5	-14.2	4.6	13.7	26.6
(100%)	(8.6)	(10.0)	(10.6)	(12.0)	(13.0)	(8.9)	(5.7)***
Small	0.9	0.3	3.8	-4.8	0.4	18.4	-0.7
(16.6%)	(5.2)	(6.2)	(7.8)	(5.8)	(6.9)	(7.4)**	(0.5)
Medium	5.3	3.9	12.8	11.3	22.9	28.3	39.9
(47.5%)	(4.0)	(6.7)	(9.0)	(10.0)	(11.4)**	(9.1)***	(2.0)***
Large	-32.6	-59.2	-12.7	-13.9	3.9	45.1	129.4
(35.9%)	(28.5)	(34.0)*	(53.0)	(65.7)	(53.9)	(61.1)	(33.8)***
Dairy	-36.8	-113.0	-207.5	-266.0	-307.4	-298.3	-248.6
(11.6%)	(19.4)*	(34.0)***	(46.4)***	(57.9)***	(72.5)***	(68.6)***	(45.8)***

Statistical significance: ***=1%, **=5%, *=10%.

Table 5 – Treatment effects under general interference for different time exposures and radial distances (Spatial interference=SE_d^k and Total effect=STE_d^k) (all units): K€ of net farm income per unit of family labour. Standard errors in parentheses.

	k=1	*k=2*	*k=3*	*k=4*	*k=5*	*k=6*	*k=7*
Observations	23,828	20,424	17,020	13,616	10,212	6,808	3,404
d= 50km							
Spatial interference	11.4 (2.9)***	15.9 (3.3)***	17.9 (3.3)***	11.2 (2.9)***	8.3 (3.5)**	8.7 (3.8)**	4.6 (1.7)***
Total effect	13.2 (2.5)***	18.5 (3.1)***	23.8 (3.1)***	22.4 (4.3)***	25.9 (7.4)***	27.7 (5.6)***	27.1 (8.5)***
d= 100km							
Spatial interference	11.9 (3.2)***	15.0 (4.3)***	15.7 (5.1)***	9.2 (5.4)*	7.2 (7.0)	-0.3 (8.2)	5.5 (0.9)***
Total effect	12.5 (3.0)***	15.9 (4.4)***	19.7 (5.3)***	17.5 (5.9)***	29.2 (11.1)***	30.3 (12.7)**	32.6 (6.1)***
d= 150km							
Spatial interference	11.2 (2.9)***	14.2 (4.0)***	14.1 (4.9)***	7.4 (4.8)	4.7 (6.3)	-2.3 (6.4)	-0.9 (0.3)***
Total effect	11.8 (2.8)***	16.0 (4.2)***	19.7 (5.1)***	16.9 (5.3)***	23.3 (8.1)***	21.5 (10.1)**	24.5 (6.3)***
d= 200km							
Spatial interference	9.0 (2.0)***	12.5 (2.5)***	12.6 (3.1)***	8.1 (3.1)***	5.5 (4.1)	1.1 (4.7)	-0.9 (0.4)**
Total effect	9.4 (1.8)***	14.3 (2.7)***	16.6 (3.4)***	15.0 (4.0)***	20.1 (5.8)***	19.0 (7.8)**	19.8 (3.8)***

Statistical significance: ***=1%, **=5%, *=10%.

Table 6 – Comparison of treatment effects under general interference for the actual outcome variable (NI/FAWU in K€), with and without contagion, and for the placebo outcome (% deviation in yearly cumulated rainfall). Standard errors in parentheses.

	Actual outcome with contagion	*Actual outcome without contagion*	*Placebo outcome with contagion*
DE^0	47.1 (15.0)***	-4.2 (20.5)	-0.8 (1.9)
TE_0^1	0.5 (8.6)	-5.0 (18.4)	-1.3 (2.9)
SE_{100}^1	11.9 (3.2)***	6.2 (2.6)**	2.3 (3.7)
STE_{100}^1	12.5 (3.0)***	3.3 (1.6)**	-0.1 (3.7)

Statistical significance: ***=1%, **=5%, *=10%.

Figure 1 – Possible theoretical patterns of the dynamic response (TE).

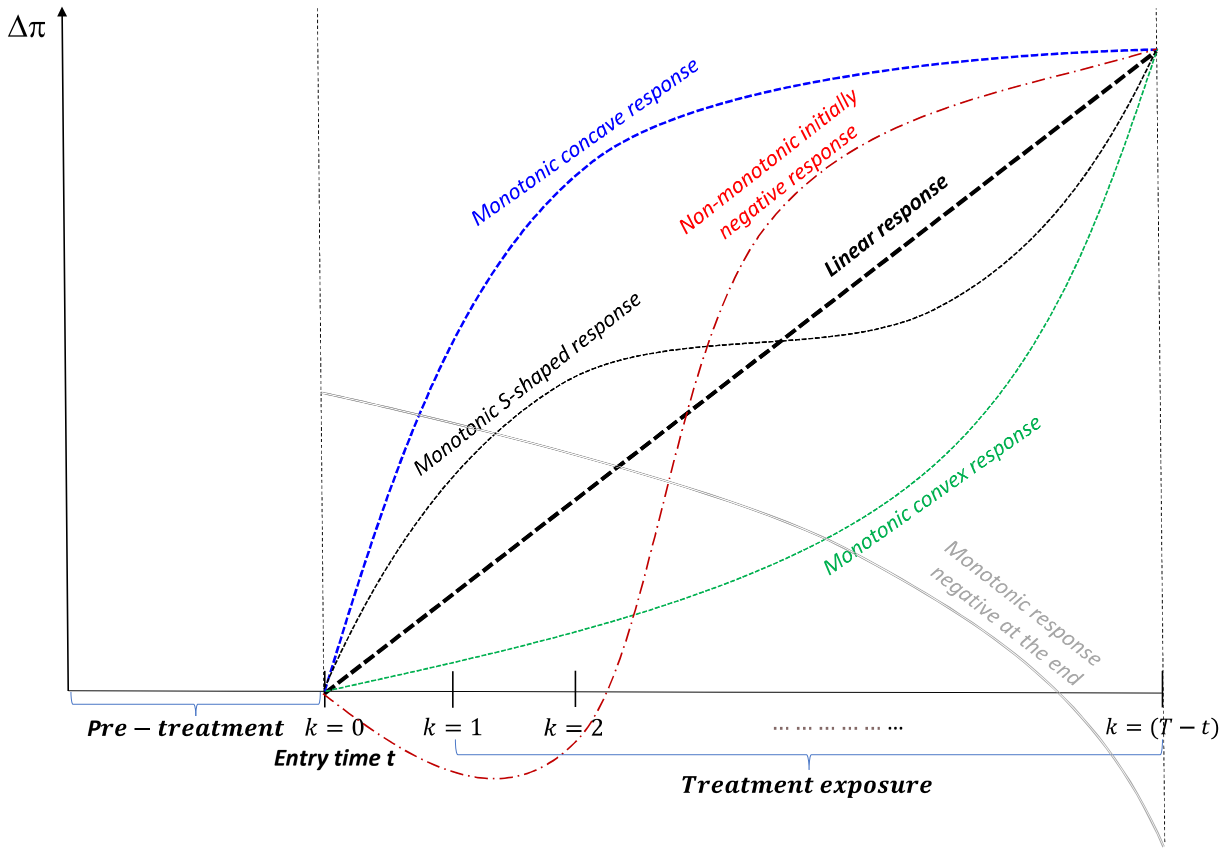


Figure 2 – Dynamic Acyclic Graph (DAG) representation of the TE under general interference according to the adopted theoretical framework (Section 3).

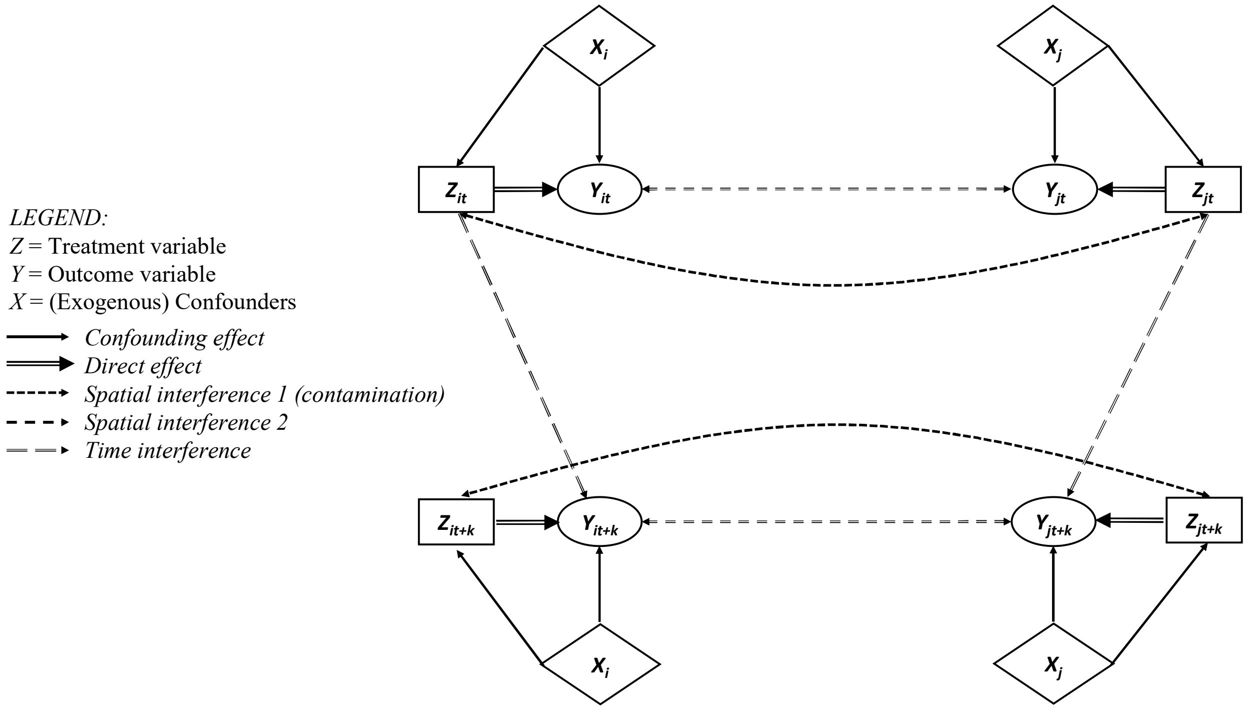
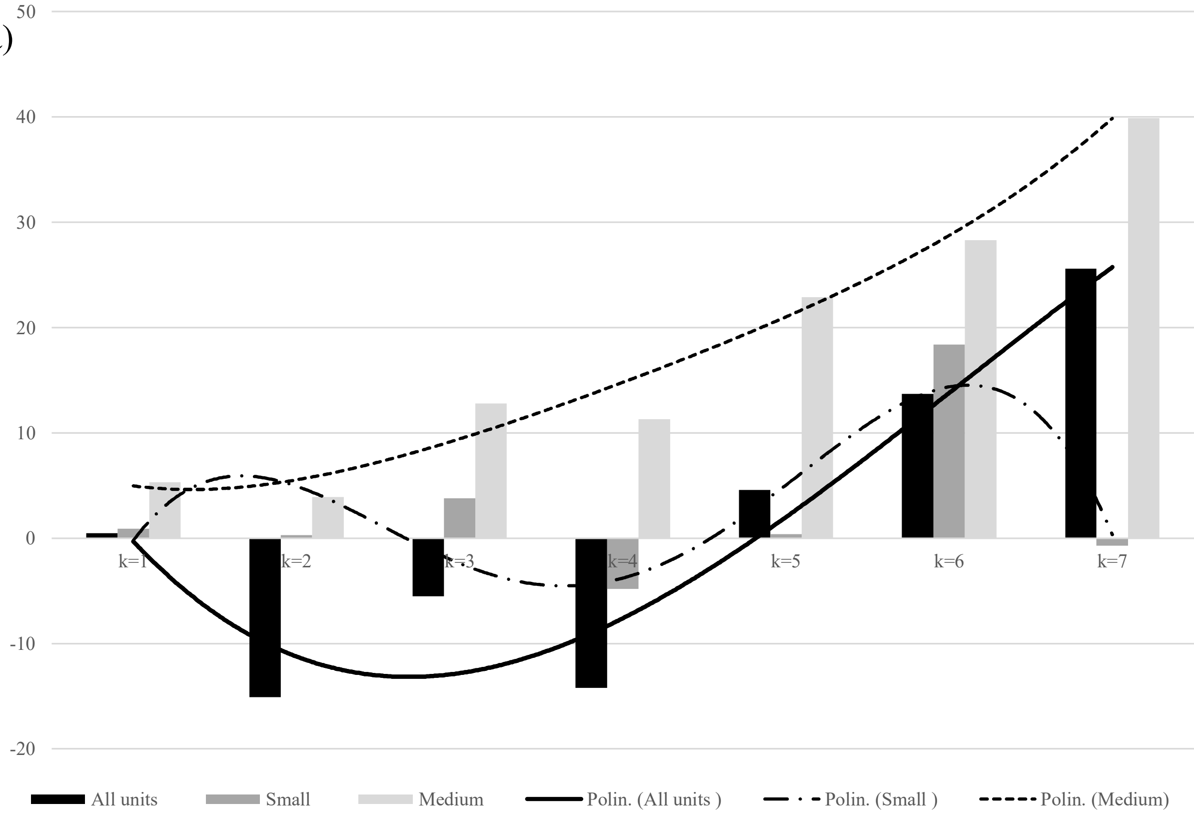


Figure 3 – Shape of the dynamic effect (no spatial effects: TE_0^k) for different groups of farms in K€ of net farm income per unit of family labour: a) all units, small and medium farms; b) large farms and dairy farms (fitted lines correspond to fourth-order polynomial approximations).

a)



b)

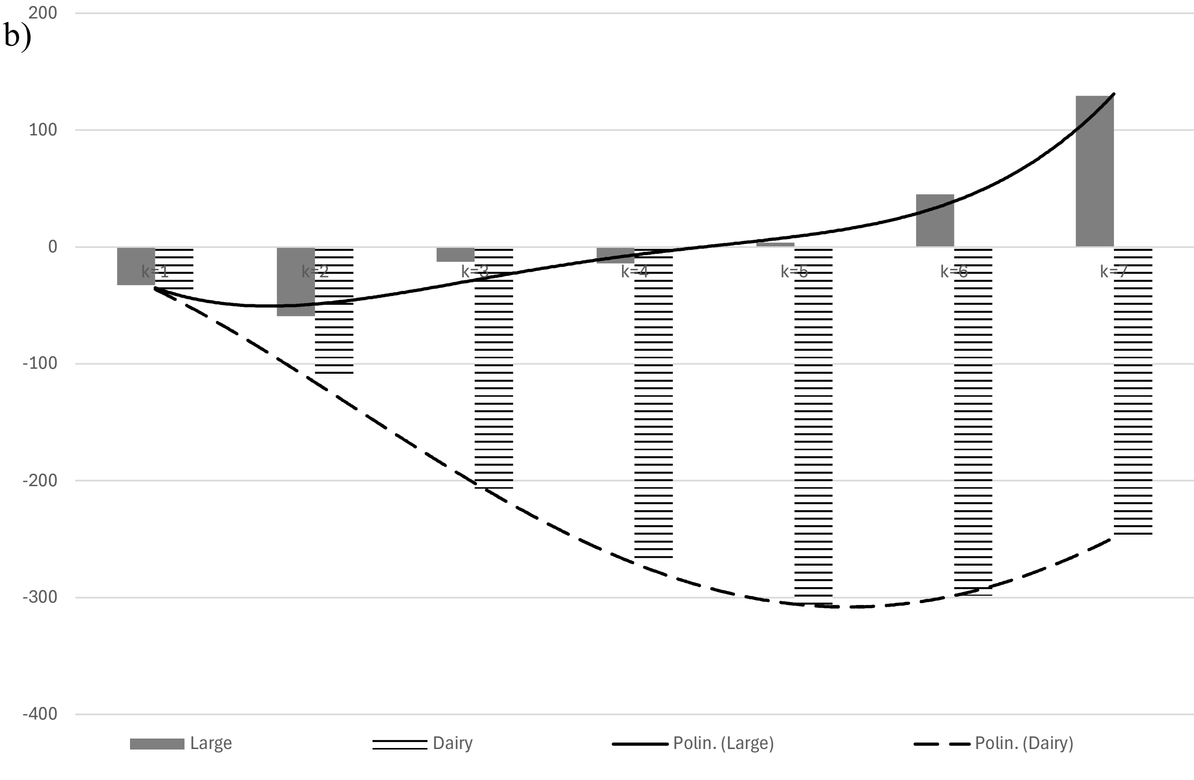


Figure 4 – Direct (DE^k), Spatial (SE_d^k), temporal (TE_d^k) and total treatment (STE_d^k) effects with varying d and k (all farms): K€ of net farm income per unit of family labour.

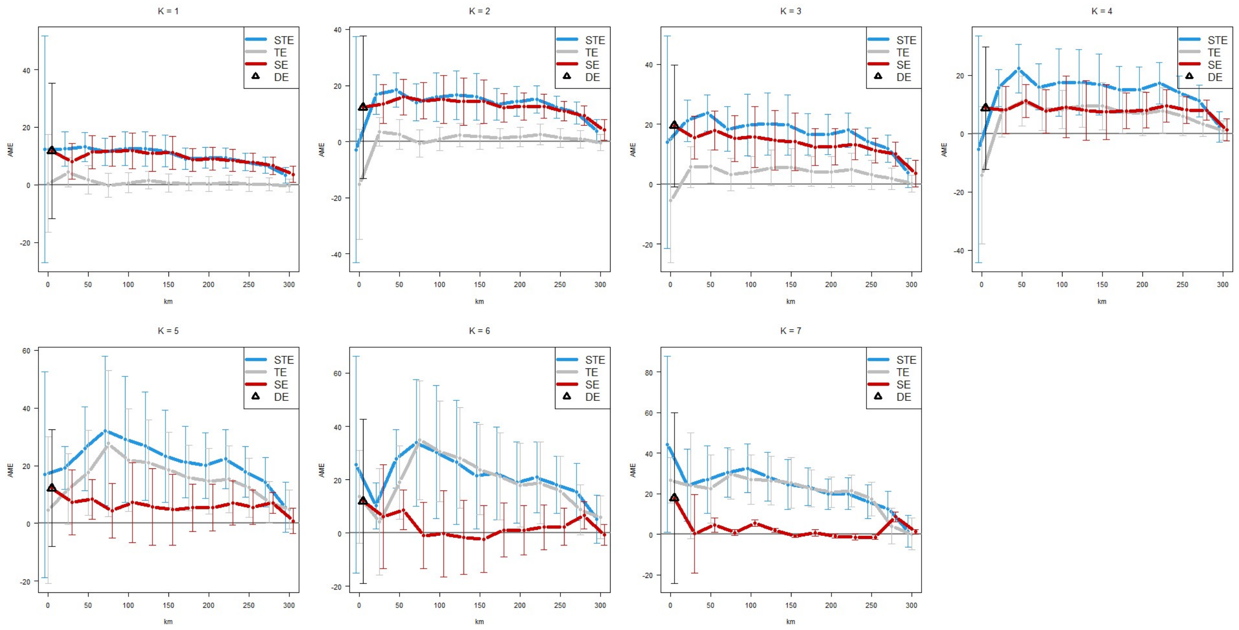
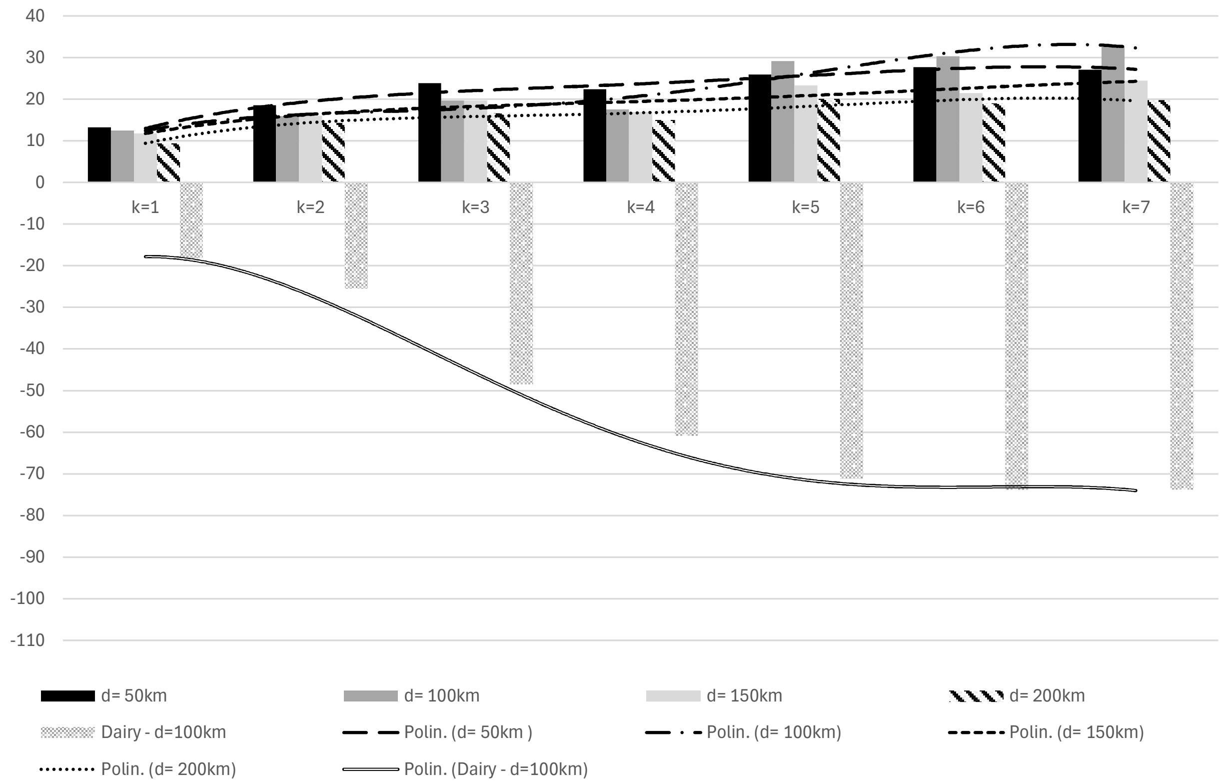


Figure 5 – Shape of the total effect (STE_d^k) for different groups of farms in K€ of net farm income per unit of family labour (fitted lines correspond to fourth-order polynomial approximations).



ANNEX 1 – Descriptive evidence

Table A1 – Mean values of all model variables by group of farms (standard deviation in parentheses).

	Units		
	All farms	Non-treated farms	Treated farms
Observations	27,232	25,559	1,673
Outcome variable (**NI/FAWU**)	42.3 (125.75)	40.01 (98.25)	77.41 (329.68)
Confounders (**X**):			
Reg221	0.02 (0.15)	0.03 (0.16)	0.01 (0.09
Reg222	0.11 (0.31)	0.11 (0.31)	0.05 (0.23)
Reg230	0.06 (0.24)	0.06 (0.24)	0.04 (0.2)
Reg241	0.03 (0.18)	0.03 (0.18)	0.05 (0.21)
Reg242	0.04 (0.2)	0.04 (0.21)	0.03 (0.16)
Reg243	0.08 (0.26)	0.08 (0.27)	0.04 (0.19)
Reg244	0.04 (0.19)	0.04 (0.19)	0.04 (0.2)
Reg250	0.05 (0.22)	0.05 (0.22)	0.01 (0.11)
Reg260	0.03 (0.17)	0.03 (0.17)	0.03 (0.17)
Reg270	0.04 (0.21)	0.04 (0.21)	0.04 (0.2)
Reg281	0.05 (0.22)	0.05 (0.22)	0.05 (0.21)
Reg282	0.04 (0.19)	0.04 (0.19)	0.02 (0.13)
Reg291	0.02 (0.13)	0.02 (0.13)	0.02 (0.13)
Reg292	0.06 (0.24)	0.06 (0.24)	0.08 (0.27)
Reg301	0.03 (0.16)	0.03 (0.16)	0.01 (0.11)
Reg302	0.08 (0.27)	0.08 (0.26)	0.12 (0.32)
Reg303	0.02 (0.14)	0.01 (0.11)	0.13 (0.34)
Reg311	0.03 (0.18)	0.03 (0.17)	0.07 (0.26)
Reg312	0.05 (0.21)	0.04 (0.21)	0.06 (0.23)
Reg320	0.05 (0.21)	0.05 (0.21)	0.05 (0.22)
Reg330	0.08 (0.27)	0.08 (0.27)	0.05 (0.22)
Year2015	0.12 (0.33)	0.13 (0.33)	0.03 (0.16)
Year2016	0.12 (0.33)	0.13 (0.33)	0.07 (0.25)
Year2017	0.12 (0.33)	0.13 (0.33)	0.11 (0.32)
Year2018	0.12 (0.33)	0.13 (0.33)	0.12 (0.33)
Year2019	0.12 (0.33)	0.12 (0.33)	0.14 (0.35)
Year2020	0.12 (0.33)	0.12 (0.33)	0.15 (0.36)
Year2021	0.12 (0.33)	0.12 (0.33)	0.18 (0.38)
Year2022	0.12 (0.33)	0.12 (0.33)	0.19 (0.4)
Sfawu	0.86 (0.25)	0.86 (0.24)	0.79 (0.28)
Sfor	0.04 (0.13)	0.04 (0.13)	0.04 (0.11)
Sfix	0.28 (0.17)	0.28 (0.17)	0.29 (0.17)
Age	55.82 (12.9)	56.05 (12.87)	52.27 (12.77)
Employees	0.59 (0.49)	0.59 (0.49)	0.55 (0.5)
Oga	4.72 (16.12)	4.75 (16.21)	4.35 (14.58)
Rain	749.18 (212.18)	749.84 (211.28)	739.05 (225.19)
Avg_Rain	839.42 (193.56)	840.39 (192.97)	824.6 (201.9)
Deviation	-0.11 (0.15)	-0.11 (0.15)	-0.11 (0.14)
Uaa	33.85 (54.47)	32.82 (50.07)	49.59 (98.68)
Education level = Professional	0.17 (0.38)	0.17 (0.38)	0.16 (0.37)
Education level = Bachelor	0.01 (0.1)	0.01 (0.09)	0.02 (0.13)

Education level = Degree	0.04 (0.21)	0.04 (0.2)	0.1 (0.31)
Education level = Elementary	0.11 (0.31)	0.11 (0.31)	0.05 (0.22)
Education level = Middle	0.39 (0.49)	0.39 (0.49)	0.3 (0.46)
Education level = Secondary	0.26 (0.44)	0.26 (0.44)	0.35 (0.48)
Education level = None	0.01 (0.11)	0.01 (0.11)	0.01 (0.1)
Education level = Postgraduate	0 (0.02)	0 (0.02)	0 (0.03)
Altitude = Innerhill	0.3 (0.46)	0.3 (0.46)	0.25 (0.44)
Altitude = Coasthill	0.14 (0.35)	0.14 (0.34)	0.17 (0.37)
Altitude = Innermountain	0.24 (0.43)	0.23 (0.42)	0.29 (0.46)
Altitude = Coastmountain	0.01 (0.1)	0.01 (0.09)	0.03 (0.17)
Altitude = Plane	0.31 (0.46)	0.32 (0.47)	0.25 (0.44)
Dairy	0.11 (0.31)	0.11 (0.32)	0.10 (0.29)
Cereals	0.11 (0.31)	0.11 (0.31)	0.09 (0.29)
Grazing Livestock	0.13 (0.34)	0.14 (0.34)	0.13 (0.33)
Fruits	0.12 (0.32)	0.11 (0.32)	0.17 (0.37)
Granivores	0.05 (0.23)	0.06 (0.23)	0.02 (0.15)
Mixed	0.07 (0.25)	0.07 (0.25)	0.07 (0.26)
Olives	0.03 (0.16)	0.02 (0.14)	0.11 (0.32)
Horticulture	0.1 (0.3)	0.11 (0.31)	0.03 (0.17)
Arable Crops	0.14 (0.35)	0.14 (0.35)	0.15 (0.36)
Wine	0.13 (0.34)	0.14 (0.34)	0.12 (0.32)
Large-Sized	0.35 (0.48)	0.35 (0.48)	0.30 (0.46)
Medium-Sized	0.46 (0.5)	0.46 (0.5)	0.51 (0.5)
Small-Sized	0.18 (0.39)	0.18 (0.39)	0.18 (0.38)
Longitude	0.14 (0.14)	0.12 (0.13)	0.3 (0.23)
Latitude	0.14 (0.14)	0.13 (0.13)	0.3 (0.23)

LEGEND: Reg221 = Piedmont; Reg222 = Aosta Valley; Reg230 = Liguria; Reg241 = Lombardy; Reg242 = Province of Bolzano-Bozen; Reg243 = Province of Trento; Reg244 = Veneto; Reg250 = Friuli Venezia Giulia; Reg260 = Emilia-Romagna; Reg270 = Tuscany; Reg281 = Umbria; Reg282 = Marche; Reg291 = Lazio; Reg292 = Abruzzo; Reg301 = Molise; Reg302 = Campania; Reg303 = Apulia; Reg311 = Basilicata; Reg312 = Calabria; Reg320 = Sicily; Reg330 = Sardinia; Sfawu = share of family labour on total farm; Sfor = share of forests on total farm land; Sfix = share of fixed costs on total farm costs; Oga = share of Other Gainful Activities on total farm Gross Production Value; Rain = annual average rainfall; Avg_Rain = 10-year moving average rainfall; Deviation = percentage deviations from average rainfall; Uaa = Utilised Agricultural Area;

ANNEX 2 – Probit estimates

Table A2 – Estimated propensity scores for treated and untreated farms, with and without spatial interference (contagion).

Variable:	Min	Mean	Max
Propensity score (No contagion)			
Treated	0.009	0.302	0.999
Non treated	0.001	0.129	0.966
Spatial propensity score (With contagion)			
Treated	0.006	0.297	0.984
Non treated	0.001	0.124	0.972

Table A3 – Estimate of the Probit Model (PM) and Spatial Probit Model (SPM)

Parameter:	PM estimates (Standard errors in parentheses)	SPM estimates (LR test in parentheses)
ρ (spatial correlation)	-	0.279 (287.4)***
Reg221	-44.185 (25.7)*	-36.755 (12.5)***
Reg222	-44.176 (25.7)*	-36.728 (12.5)***
Reg230	-44.242 (25.7)*	-36.680 (12.5)***
Reg241	-44.291 (25.7)*	-36.710 (12.5)***
Reg242	-44.548 (25.7)*	-36.918 (12.7)***
Reg243	-44.412 (25.7)*	-36.766 (12.5)***
Reg244	-43.969 (25.7)*	-36.394 (12.4)***
Reg250	-44.702 (25.7)*	-37.114 (12.6)***
Reg260	-44.690 (25.7)*	-37.043 (12.7)***
Reg270	-44.426 (25.8)*	-36.929 (12.6)***
Reg281	-44.577 (25.8)*	-36.999 (12.6)***
Reg282	-45.111 (25.8)*	-37.396 (12.7)***
Reg291	-44.476 (25.8)*	-37.001 (12.6)***
Reg292	-45.202 (25.8)*	-37.483 (12.8)***
Reg301	-45.726 (25.8)*	-37.884 (13.1)***
Reg302	-45.289 (25.8)*	-37.536 (12.8)***
Reg303	-43.733 (25.7)*	-36.447 (12.3)***
Reg311	-44.847 (25.8)*	-37.267 (12.6)***
Reg312	-45.050 (25.8)*	-37.395 (12.7)***
Reg320	-44.509 (25.7)*	-37.002 (12.6)***
Reg330	-44.907 (25.7)*	-37.373 (12.8)***
Year2016	0.088 (0.1)**	0.063 (3.0)*
Year2017	0.199 (0.1)***	0.148 (16.8)***
Year2018	0.238 (0.1)***	0.177 (25.7)***
Year2019	0.270 (0.1)***	0.202 (33.8)***
Year2020	0.287 (0.1)***	0.217 (39.9)***
Year2021	0.360 (0.1)***	0.272 (63.1)***
Year2022	0.397 (0.1)***	0.305 (75.9)***
Sfawu	-0.340 (0.1)***	-0.273 (41.2)***
Sfor	0.441 (0.1)***	0.418 (26.4)***
Sfix	0.573 (0.1)***	0.578 (70.6)***
Age	-0.011 (0.1)***	-0.011 (147.9)***
Employees	-0.042 (0.1)*	-0.046 (4.1)**
Oga	0.008 (0.1)***	0.007 (119.9)***
Rain	0.001 (0.1)**	0.001 (3.8)*
Avg_Rain	-0.001 (0.1)***	-0.001 (13.2)***

Deviation	-0.689 (0.3)**	-0.521 (3.9)**
Uaa	0.004 (0.1)***	0.004 (129.3)***
Uaa_Squared	0.000 (0.1)***	0.000 (16.9)***
Education level = Professional	-0.294 (0.1)***	-0.228 (5.8)**
Education level = Bachelor	-0.235 (0.1)**	0.175 (25.9)***
Education level = Degree	0.202 (0.1)***	-0.278 (41.4)***
Education level = Elementary	-0.304 (0.1)***	-0.247 (96.8)***
Education level = Middle	-0.257 (0.1)***	-0.325 (11.6)***
Education level = Secondary	-0.414 (0.1)***	-0.228 (5.8)**
Education level = None	1.077 (0.3)***	1.096 (15.2)***
Education level = Postgraduate	-5.196 (23.2)	-3.382 (4.3)**
Altitude = Innerhill	-5.116 (23.2)	-3.295 (4.1)**
Altitude = Coasthill	-4.952 (23.2)	-3.207 (3.7)*
Altitude = Innermountain	-4.625 (23.2)	-2.922 (2.9)*
Altitude = Coastmountain	-5.555 (23.2)	-3.672 (5.3)**
Cereals	-0.049 (0.1)	-0.075 (0.2)
Grazing Livestock	0.031 (0.1)	0.020 (0.4)
Fruits	0.400 (0.1)***	0.366 (86.3)***
Granivores	-0.589 (0.1)***	-0.597 (62.3)***
Mixed	0.251 (0.1)***	0.267 (28.3)***
Olives	0.795 (0.1)***	0.693 (161.8)***
Horticulture	-0.271 (0.1)***	-0.247 (18.5)***
Arable Crops	0.118 (0.1)***	0.139 (9.4)***
Wine	0.152 (0.1)***	0.126 (10.8)***
Medium-Sized	0.020 (0.1)	-0.006 (0.2)
Large	-0.119 (0.1)***	-0.140 (13.9)***
Longitude	0.243 (0.1)**	0.115 (2.5)
Longitude Squared	-0.006 (0.1)	-0.002 (0.5)
Latitude	2.330 (0.5)***	1.933 (21.1)***
Latitude Squared	-0.028 (0.1)***	-0.024 (22.1)***

Statistical significance: *** 1%, ** 5%, * 10%.

LEGEND: Reg221 = Piedmont; Reg222 = Aosta Valley; Reg230 = Liguria; Reg241 = Lombardy; Reg242 = Province of Bolzano-Bozen; Reg243 = Province of Trento; Reg244 = Veneto; Reg250 = Friuli Venezia Giulia; Reg260 = Emilia-Romagna; Reg270 = Tuscany; Reg281 = Umbria; Reg282 = Marche; Reg291 = Lazio; Reg292 = Abruzzo; Reg301 = Molise; Reg302 = Campania; Reg303 = Apulia; Reg311 = Basilicata; Reg312 = Calabria; Reg320 = Sicily; Reg330 = Sardinia; Sfawu = share of family labour on total farm; Sfor = share of forests on total farm land; Sfix = share of fixed costs on total farm costs; Oga = share of Other Gainful Activities on total farm Gross Production Value; Rain = annual average rainfall; Avg_Rain = 10-year moving average rainfall; Deviation = percentage deviations from average rainfall; Uaa = Utilised Agricultural Area;

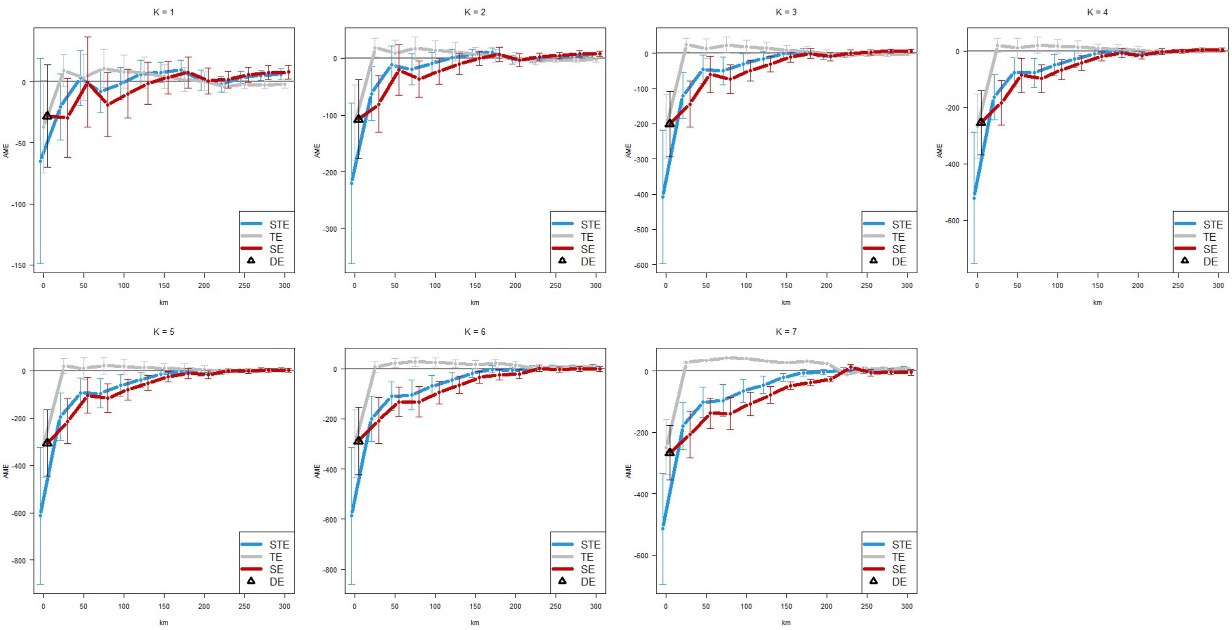
ANNEX 3 – Dairy farms

Table A4 – Treatment effects under general interference (k=0,….,7; d>0) for the subgroup of dairy farms (Spatial interference=SE_d^k and Total effect=STE_d^k): K€ of net farm income per unit of family labour. Standard errors in parentheses.

	k=1	*k=2*	*k=3*	*k=4*	*k=5*	*k=6*	*k=7*
Observations	2,765	2,370	1,975	1,580	1,185	790	395
d= 50km							
Spatial interference	-40.4 (22.8)*	-40.6 (30.3)	-80.6 (32.2)**	-95.0 (33.3)***	-95.5 (40)**	-115.5 (30.7)***	-127.4 (29.5)***
Total effect	-16.3 (16.3)	-15.4 (18.2)	-46.6 (17.2)***	-67.8 (19)***	-87.7 (24.8)***	-96.2 (26.4)***	-97.8 (26.6)***
d= 100km							
Spatial interference	-28.7 (11.5)**	-35.8 (15.2)**	-66.3 (15.4)***	-75.2 (17)***	-79.6 (21.2)***	-85.9 (23.8)***	-102.2 (21.8)***
Total effect	-18.2 (11.4)	-25.5 (10.8)**	-48.5 (9.6)***	-60.9 (12.4)***	-71.2 (17.4)***	-73.9 (19.2)***	-73.8 (19.2)***
d= 150km							
Spatial interference	-6.1 (7.2)	-6.9 (8.2)	-19.5 (7.3)***	-21.8 (8.3)***	-23.8 (10.6)**	-29.1 (13.4)**	-46.8 (7)***
Total effect	-3.7 (7.5)	-5.8 (5.8)	-16.0 (3.8)***	-21.0 (4.8)***	-25.8 (6.8)***	-27.3 (8.3)***	-24.1 (6.1)***
d= 200km							
Spatial interference	-1.8 (6.1)	-3.8 (4.8)	-8.5 (5.3)	-12.1 (6.2)*	-13.2 (8.2)	-17.2 (10.6)	-26.8 (4)***
Total effect	-0.5 (6.1)	-1.0 (3.7)	-2.9 (2.6)	-2.9 (3.5)	-2.1 (4.8)	-6.7 (5.7)	-15.2 (3.5)***

Statistical significance: *** 1%, ** 5%, * 10%.

Figure A1 - Direct (DE^k), Spatial (SE_d^k), temporal (TE_d^k) and total treatment (STE_d^k) effects with varying d and k for the subgroup of dairy farms: K€ of net farm income per unit of family labour.



ANNEX 4 – Trade-off in the identification of spatial and temporal effects

Figure A2 displays the different effects of Table 2, estimated on the full sample and presented according to the values of *k* and *d*. Unlike Figure 4, the estimates are grouped by type of effect rather than by *k*. Effects are shown using different colour gradients to reflect the values of *k*: darker shades indicate smaller *k* values, corresponding to a shorter treatment history, while lighter shades represent larger *k* values.

Focusing on the spatial effects (left panel), Figure A2 shows that estimates are positive and statistically significant when k is small. By increasing k, spatial effects tend to decline both in magnitude and statistical significance. Temporal effects (central panel) exhibit the opposite pattern: with smaller k's, estimates are mostly close to zero and non-significant; increasing k, leads to large positive and significant TE_d^k. Such behaviour could be expected as increasing the treatment history (i.e., increasing k) shortens the overall number of observations available. A smaller sample size may affect both spatial and temporal effects. However, as the length of the treatment history increases, carry-over effects are more likely to consistently emerge. Therefore, increasing k enhances the identification strategy's ability to capture temporal effects, although this comes at the expense of the number of observations.

The combined effect is represented by the right-hand side panel of Figure A2 which shows the various STE_d^k. With smaller k's, spatial effects tend to dominate over the temporal ones. Increasing k leads to a dominance of temporal effects instead. Except for *d*=0, STE_d^k tend to be positive and statistically significant ranging from 3.3 K€ (*k*=1 and *d*=300) to 33.9 K€ (*k*=6 and *d*=75).

Figure A2 – Direct (DE^k), spatial (SE_d^k), temporal (TE_d^k), and total treatment (STE_d^k) effects with varying *d* and *k* for the whole sample.