UNIVERSITÀ POLITECNICA DELLE MARCHE

DIPARTIMENTO DI SCIENZE ECONOMICHE E SOCIALI

NON-MONETARY MOTIVATIONS OF AGRO-ENVIRONMENTAL POLICIES ADOPTION. A CAUSAL FOREST APPROACH.

ROBERTO ESPOSTI

QUADERNO DI RICERCA n. 459

ISSN: 2279-9575

January 2022

Collana curata da:
Massimo Tamberi

Abstract

This paper investigates the non-monetary motivations of farmers' adoption of agro-environmental policies. Unlike the monetary (income) motivations, non-monetary drivers can not be directly observed but can be identified from observational data within appropriate quasi-experimental designs. A theoretical justification of farmers' choices is firstly formulated and a consequent natural experiment setting is derived. This latter admits heterogeneous, i.e. Individual, Treatment Effects (ITE) that, in turn, can be interpreted in terms of more targeted and tailored policy expenditure. A Causal Forest (CF) approach is adopted to estimate these ITEs for both the treated and not treated units. The approach is applied to two balanced panel samples of Italian FADN farms observed over the 2008-2018 period. Results show how heterogeneous the farmers' response and the associated non-monetary motivations can be, thus pointing to space for a more efficient policy design.

KEYWORDS: Agro-Environmental Policy, Common Agricultural Policy, Behavioural Motivations, Individual Treatment Effects, Causal Forests.

JEL class.: C21, Q15, Q51

Address:

Roberto Esposti, Department of Economics and Social Sciences - Università Politecnica delle Marche, 60121, Ancona (Italy). Email: r.esposti@staff.univpm.it

Non-Monetary Motivations of Agro-Environmental Policies Adoption. A Causal Forest Approach.

Roberto Esposti*♠

*Department of Economics and Social Sciences - Università Politecnica delle Marche (Ancona, Italy); email: r.esposti@staff.univpm.it

Abstract

This paper investigates the non-monetary motivations of farmers' adoption of agro-environmental policies. Unlike the monetary (income) motivations, non-monetary drivers can not be directly observed but can be identified from observational data within appropriate quasi-experimental designs. A theoretical justification of farmers' choices is firstly formulated and a consequent natural experiment setting is derived. This latter admits heterogeneous, i.e. Individual, Treatment Effects (ITE) that, in turn, can be interpreted in terms of more targeted and tailored policy expenditure. A Causal Forest (CF) approach is adopted to estimate these ITEs for both the treated and not treated units. The approach is applied to two balanced panel samples of Italian FADN farms observed over the 2008-2018 period. Results show how heterogeneous the farmers' response and the associated non-monetary motivations can be, thus pointing to space for a more efficient policy design.

Keywords: Agro-Environmental Policy, Common Agricultural Policy, Behavioural Motivations, Individual Treatment Effects, Causal Forests.

JEL Classification: C21, Q15, Q51

1. Introduction: objective of the work

This paper has been inspired by the recent literature aiming to assess the behavioural foundation of the farmers' voluntary adoption of environmental policies. The existence and extent of non-monetary motivations, in particular, is of major policy relevance as it would either point at space for public expenditure savings, still obtaining the same environmental performance, or at the amount of additional expenditure needed to improve this performance (Esposti, 2021).

So far, the assessment of these behavioural motivations has been almost exclusively performed through experiments (Thomas et al., 2019). The main reasons is that only well designed experiments may surface those usually unobservable individual motivations that have a role in

♠ The author wishes to thank Silvia Coderoni and Alessandro Varacca for helpful discussions on several aspects of the present paper and, in particular, on data elaboration.

agents' decisions about policy adoption. This paper relies on the idea that this kind of assessment can be carried out also using observational data by adopting appropriate modelling and estimation solutions.

Though empirically challenging, this approach seems to be advantageous not only because observational data reflects real-world behaviour and not choices within "artificial" circumstances (i.e., laboratory experiments).[1] But also because, in several cases, sound lab-based or field experiments can be hardly designed and organized (Angrist and Pischke, 2010). This may be the case of environmental measures targeted to farming activities on which the present paper focuses on.

Firstly, modelling farmers' behaviour is needed in order to make the role of non-monetary motivations explicit. This theoretical framework derives the income implication of the farmers' voluntary policy choice according to a treatment-effect logic. Then, it shows how the presence and sign of the unobserved non-monetary motivations can be extracted from these treatment effects within an appropriate quasi-experimental setting. As farms are heterogenous for a whole set of observable conditioning features, as well as for the unobserved utility function and non-monetary motivations, treatments effects are themselves heterogeneous and an appropriate estimation approach is thus required.

In recent years, different methodological solutions have been put forward to identify and estimate Individual Treatment Effects (ITE). Among these, Causal Forest (CF) estimation has attracted increasing attention for its flexibility and robustness but also for its desirable econometric properties (Athey et al., 2019; Athey and Wager, 2019). This estimation approach is here adopted. The derivation of the farmers' non-monetary motivations within a theoretically founded quasi-experimental setting, and the CF estimation of the consequent ITE represent the main original contribution of the present study. The approach is applied to the sample of Italian

[1] Eventually, empirical economists usually point to a prevalence of revealed preferences on stated (or elicited) ones: "After all, economists have long favored analysis that is based on what people do rather than what they say" (Kling et al. 2012, p. 4).

Farm Accountancy Data Network (FADN) farms over the 2008-2018 period and, within this context, it concerns the Agro-Environmental Policies (AEPs) delivered through Common Agricultural Policy (CAP) before and after its 2013 reform.

The rest of the paper is structured as follows. Section 2 overviews the policy context and relevance underlying the empirical investigations. Section 3 illustrates the theoretical framework and motivates its treatment-effect interpretation. Section 4 presents the adopted CF estimation approach while section 5 describes the empirical application and the respective quasi-experimental design. Section 6 details and discusses the main results. Section 7 concludes drawing some policy implications.

2. Environmental measures within the CAP: evolution and open issues

With AEPs here we mean all those measures that are specifically and exclusively targeted to farms and that imply payments (or sanctions) conditional on the respect/achievement (or not) of some environmental standard/performance. The environmental motivation for the policy support granted to farmers has gained increasing relevance in most developed countries (Guerrero, 2021). In particular, as the empirical application here presented concerns a sample of Italian farms, the Environmental Policies (AEPs) delivered through the Common Agricultural Policy (CAP) are specifically taken into account (Barreiro-Hurle et al., 2021).

The AEPs made their first appearance within the CAP in 1988 with the introduction of the so-called set-aside incentive scheme: a payment was granted to farmers that left part of their land out of production. This voluntary approach was reinforced with the 1992 Reform (known as MacSharry reform) with the introduction of the Agri-environmental Measures (*AEM*s), i.e., further voluntary measures implying payments upon the adoption of environmentally friendly agricultural practices. At the same time, however, set-aside became a mandatory requirement in order to be eligible to receive the novel compensatory direct payments. Since then, the AEPs within the CAP have been a combination of these two lines of action: conditional measures entitling farmers to receive the ordinary direct payments (in this sense, mandatory); voluntary measures implying additional compensatory payments targeted to some environmental standard or performance.

These two lines can be intended as realizations, within the farming activity, of two well-known different philosophies in environmental policy making (Harrington and Morgenstern, 2007). On the one hand, *environmental standards* are imposed to agents (farmers) under the risk of a *monetary sanction* (the loss of the direct payments). This approach is also called *Command & Control* environmental policy. On the other hand, agents are offered a *monetary incentive* (additional payment) to achieve a better environmental performance.

The combination of conditional measures and additional payments has been maintained and reinforced with the following reform (the Agenda 2000 CAP reform). By designing the CAP architecture into two pillars, this reform also formally separated the two lines of AEPs. The first pillar had to the deliver the direct payments upon the respect of the mandatory set-aside requirements. The second pillar (or Rural Development Policy, RDP) included a long list of measures implying the voluntary participation of farmers in order to be entitled to receive an additional payment (the incentive): the *AEM*s have been included among these.

The full decoupling of the first-pillar direct payments, firstly established with the 2003 CAP Reform (or Fischler's Reform), and then completed by the 2008 (or Health Check) Reform, finalized this twofold strategy of the environmental part of the CAP. The environmental conditionality of the direct payments was mainstreamed through a set of additional and compulsory environmental standards (the so-called *Cross-Compliance*, *CC*, requirements): non-complying farmers are sanctioned with the loss of part or all of their direct payments proportionally to the degree of non-compliance.[2] At the same time, the second pillar *AEM*s have been mostly confirmed and their overall financial support expanded.

The 2013 Reform took the final step in this direction. Not only first pillar direct payments remained conditioned on a set of mandatory (*CC*) requirements. In addition, part of the direct payments themselves has been reserved to the respect of additional environmental standards, the so-called Greening payment: 30% of the total direct payments become conditional on three environmental management practices (Coderoni et al., 2021).[3] The greening measure can be considered as a sort of additional or super conditionality (Matthews, 2011). Unlike the CC requirements, however, it brings about a payment that is additional to the Basic direct payments. Therefore, complying farmers with respect to both CC and greening requirements, receive a total payment corresponding to the sum of basic and greening direct payments.

The CAP regime established by the 2013 Reform is the regime currently into force as its application has been extended to 2022. Nonetheless, European institutions already agreed the new CAP that will apply from 2023 to 2027.[4] A deeper and more technical discussion about the different AEPs within the CAP is beyond the scope of the present study (Guerrero, 2021). What is worthwhile noticing here is that, beside the often overemphasized technicalities, two invariances emerge across this thirty-years sequence of reforms.

The first invariance concerns the consolidation, after the initial reform steps, of a long-term strategy based on the two abovementioned lines of action. All changes occurred after 2003 only reinforced and refined this strategy without substantially affecting it. This seems true, in

---

[2] These requirements are the so-called Good Agricultural and Environmental Conditions (GAECs). In fact, other environmental standards apply to the farming activity (the Statutory Management Requirements, SMRs), but they concern all farmers regardless their *DP*s entitlements.

[3] On a Member-State basis, the total amount of greening payment must correspond to 30% of the total direct payments. In several EU countries (and this is the case of Italy) this condition is satisfied by automatically assigning to eligible farms 30% of total direct payments as greening payment. Therefore, at the farm-level the greening payment is approximatively 0.43 times the basic direct payments.

[4] Even though this new regime does not concern the period under investigation here, it is still worth noticing that it confirms and reinforces the previous strategy. An enhanced conditionality takes over by bundling together the *CC* requirements with the greening measures. In addition, the first pillar also includes a new tool, the so-called *eco-schemes*, i.e., voluntary measures granting the farmers an additional first-pillar payment. Second-pillar funded *AEM*s, though maintained, undergo a scope differentiation and broadening to minimize their overlap with the novel eco-schemes.

particular, for the 2013 reform. It changed the extent and modality of the environmental conditionality associated to direct payments. But from the farmers' perspective this regime change did not change the kind of choices they have to make and the consequent monetary implications. The only major change this reform might have generated could actually refer to the non-monetary motivations associated to this revised environmental conditionality (see next section). As a consequence, even though the years before and after 2015 (the first year of implementation of the 2013) can not be compared between them, these two sub-periods can be still considered as two replications of an analogous policy regime. The same methodological approach may be also replicated and the evidence emerging in these two subperiods can be confronted in order to identify the main commonalities and differences with respect to these non-monetary motivations.

The second invariance concerns the substantial equivalence of the distinct CAP AEPs from the farmers' perspective. Mandatory environmental standards and voluntary environmental incentives considerably differ as for the modalities they are designed and provided to farmers. As clarified in next section, however, from the farmer's perspective they both resolve into a monetary net incentive (the additional payment associated to the *AEM*s combined with the consequent loss of revenue and/or additional costs) or net disincentive (the loss of direct payments upon non-compliance of the *CC* and greening standards combined with the consequent gain of revenue and/or lower costs) (see next section). Moreover, they both eventually behave as voluntary AEPs since in both cases farmers are expected to evaluate this set of incentives/disincentives and then decide whether or not to adopt the respective policy regime. Farmers' response could thus be the same, and somehow indistinguishable, in the two cases unless different non-monetary motivations are activated. Beside these invariances, therefore, the open question about the farmers' response to different AEPs remains whether the analogous incentives/disincentives they deliver induce a different response due to different motivations they activate.

Eventually, both before or after 2015, farmers confront with three possible regimes and, therefore, have to choose among them. On the hand, they can ignore any environmental standard implied by the CAP, thus losing all the CAP payments but also taking advantage of the possible cost reduction and/or revenue gain. On the other hand, they may decide to take on all environmental standards (both mandatory, or conditional, and voluntary) the CAP puts forward, thus seizing direct payments and *AEM* but also possibly incurring revenue losses and/or higher costs. As a rationale compromise between these two "extreme" choices, farmers may decide to "save" the direct payments by respecting the corresponding mandatory standards while dropping the additional (voluntary) *AEM*s standards and payments.

According to these arguments, if farmers' adoption decisions were exclusively driven by monetary considerations, it would be possible, by observing the individual net incentives and disincentives, to predict the impact of a policy regime in terms of adoption. Consequently, it could be also possible to progressively improve policy design and implementation in order to maximize the environmental outcome and minimize the policy expenditure. However, not only this information on individual net incentives/disincentives might been available. More importantly, how farmers eventually behave may critically depend on those unobservable non-monetary motivations that are, in turn, the result of their idiosyncratic features.

Thomas et al., 2019 summarize the empirical (mostly experimental) evidence on the farmers' response to AEPs and on their motivations. They conclude that three behavioural factors emerge as the main drivers of farmers' decisions: the warm-glow effect or green-guilt aversion; the control aversion; the loss aversion. These drivers are not only farmer-specific but are also asymmetrically activated when farmers are confronted with mandatory or voluntary environmental standards, that is, with the respective monetary incentives and disincentives. In developing the theoretical framework of the present study, here we adapt and expand this set of non-monetary motivations.

3. The theoretical framework

3.1. The non-monetary motivations of farmers behaviour

The research question underlying the present study is the following: which is the behavioural response of farmers to these AEPs and how much is it heterogeneous? In order to answer this question, a theoretical representation of farmers' behaviour is needed. The basic idea is that farmers pursue utility maximization. Utility is the combination of a monetary component (profit or, as most agricultural production units are family farms, *net income*) (π) and a non-monetary component (*NM*) which is, in turn, the combination of four non-monetary factors (Thomas et al., 2019). So, there are five fundamental behavioural drivers:[5]

1. *Net-income* (+/-): it is due to the farmers' pursue of net income maximization (where net income also includes policy incentives and disincentives).
2. *Warm-glow effect* or *Green-guilt aversion* (+): it is due to the sense of reward (or guilt) the farmers feel whenever they act in favour (or against) the environment. Thus, they derive a positive utility from pro-environment actions regardless the net income implications (Beedell and Rehman, 2000).
3. *Control aversity* (-): it is due to the nuisance the farmers feel for the control they have to overgo on their environmental performance especially when they perceive the policy instrument as an attempt to control their individual decisions. This effect is amplified if farmers do not agree with the policy objectives or think that instrument choice was inappropriate (Vollan, 2008). Therefore, whether or not environmental measures are

[5] It could be also possible to include risk-aversion (i.e., the tendency of the farmers to minimize the response to any external change, policy included). Since production decisions must be taken ex-ante, their consequences are evidently subject to some degree of uncertainty. Consequently, under risk neutrality, farmers actually maximize $\mathbb{E}[U_{it,k}]$ while under risk-aversion also the variance of $U_{it,k}$ would matter. For simplicity, it is here assumed that farmers are risk neutral. Therefore, this further effect is excluded but it can represent a future improvement of the proposed approach.

mandatory, farmers may decide to drop their adoption even though this would in fact imply a net monetary loss.

4. *Loss aversity* (+): it is due to the fear the farmers feel of the possible future loss of support (i.e., income) due to their current unsatisfactory environmental performance. Cumulative prospect theory predicts that losses motivate behaviour more than equal gains and this is particularly true when people feel a sense of an initial endowment (Babcock, 2015). Given that most current farmers have always received CAP payments, these payments are very likely perceived as an initial endowment and, therefore, the possibility of losing them activates this effect. A tightened conditionality may induce loss averse farmers to adopt measures even with a consequent net income loss.
5. *Attrition effect* (+/-): this generic term expresses all those factors that prevent voluntary adoption of a policy, whether it is pro-environment or not. These factors make policy adoption unfeasible even though farmers would like to adhere, in principle, to the measures. All CAP measures, in particular, requires some formal application by farmers. For bureaucratic reasons or other individual-specific circumstances, farmers may be unable to apply effectively.[6]

The sign in parenthesis after any item indicates the direction of response to AEPs (+/- means more/less intense). In any case, except for net income, none of these drivers of the farmer's response can be directly observed. Even though there are arguments and experiments suggesting that these non-monetary drivers are relevant especially for specific groups of farms (Andrews et., 2013; Greiner, 2015), they remain somehow residual, that is, they can only be deduced from observed behaviour net of the net-income effect. In addition, the observable response, as well as the observable net-income is the overall effect of the combination and of

[6] Non-voluntary unadoption may be caused by circumstances independent on farmers' choices. In some cases, the application can be prevented by the fact that, at some programming level, funds have run out or the maximum number of potential beneficiaries has been reached. This may be particularly the case of *AEM*s. Moreover, farmers may encounter specific exclusion or ineligibility conditions. In any case, these circumstances are not the consequences of farmers' motivations thus are not considered in the present study. Their existence, however, suggest specific caution in the interpretation of results here obtained.

the relative strength of these four non-monetary drivers. As will be discussed later, magnitude and direction of these drivers can be identified with an appropriate empirical strategy.

3.2. The model

In order to formally investigate how all these effects combine to eventually induce the farmer's response to policy, consider a panel of *N* production units (farms) observed over *T* periods and represent the i-th unit specific utility as follows (Janssen et al., 2010; Mack et al., 2019):[7]

(1) $U_{it} = \pi_{it} + NM_{it} = (R_{it} - C_{it}) + S_{it} + NM_{it},\ \ i = 1, \dots, N;\ t = 1, \dots, T$

where: R_{it} and C_{it} indicate the i-th farm revenues and costs, respectively, at time t, and S_{it} indicates the policy support (net of possible sanctions) received by i-th farm at time t. Thus, $[(R_{it} - C_{it}) + S_{it}]$ indicates the farm-level net income (π_{it}). NM_{it} stands for the net non-monetary utility components of the i-th farm at time t coming from the combination of the four abovementioned sources.

Designate the generic k-th policy regime (treatment) at which the i-th farm is assigned at time t, and consider the three kinds of payments directly or indirectly associated to some environmental standards as expression of three different AEPs.[8] The first AEP consists in conditioning a direct payment (*DP*) on the respect of a set of mandatory environmental standards. These become prerequisites to receive *DP* and, as anticipated, are usually identified as cross-compliance (*CC*) (OECD, 2012). Farmers can still voluntarily decide to do not respect the *CC* conditions thus losing the *DP*.

The second AEP consists in an additional direct payment that is only granted if further environmental standards are met (*G*). *Strictu sensu*, this AEP is voluntary since the farmer can still decide to do not meet these additional standards still saving the *DP*. Nonetheless, also this

[7] The additive nature of (1) is evidently a simplifying assumption, this form being the easiest specification of a generic function $U_{it} = f(\pi_{it}, NM_{it})$. Under the assumptions that $\partial U_{it}/\partial \pi_{it} > 0$ and $\partial U_{it}/\partial NM_{it} > 0$, we can argue that the analysis here proposed qualitatively maintains its validity even for this generic case, but it becomes quantitively more complex.

[8] As illustrated in section 2, these three AEPs evidently summarize the current CAP toolkit but can be more generally intended as three, possibly complementary, policy strategies (Guerrero, 2021) .

AEP takes the form of a conditionality as the payment it is neither designed nor quantified to cover the additional costs and/or revenue losses associated to the standards.

The third AEP consists in a typical voluntary measure where the farmer participates to a (sometime competitive) call according to which she/he stipulates a sort of contract with the funding institution (usually a national or regional government). Under the respect of a specific set of environmental standards, the farmer receives a compensatory payment (*AEM*) whose aim is to cover the additional costs and/or revenue losses the farmer incurs to meet these standards. As this compensation is calculated as an average at some aggregation level, at the farm level it is usually weakly correlated to the actual revenue losses and/or higher costs (Thomas et al., 2019). This AEP being strictly voluntary, its environmental standards are more demanding for the farmers than those associated to *DP* and *G*.

Given the nature of these three different AEPs and the way usually farmers perceive them, the first can be interpreted as an *environmental disincentive* (or pecuniary sanction) (-*D*P), the second as an *environmental incentive* (+*G*) and the third as an *environmental compensatory payment* (+*AEM*). Due to this different nature, they also activate different, or to a different extent, non-monetary motivations of the farmer's response. In this sense, these motivations are policy dependent.

On this basis, (1) can be expanded as follows:[9]

(2) $U_{it,k} = \left[\left(R_{it} - L^R_{it,k}\right) - \left(C_{it} + L^C_{it,k}\right)\right] + DP_{it,k} + G_{it,k} + AEM_{it,k} + NM_{it,k},$

$\forall i = 1, \ldots, N;\ \forall t = 1, \ldots, T;\ \forall k = 1, \ldots, K$

[9] It is worth noticing that (2) implicitly assume that farmers' behaviour is only driven by the individual utility so it disregards all the possible social benefits and costs (positive and negative externalities) of their production decisions. Although these externalities are the real justification underlying any AEP, the assumption is that they are in no case direct drivers of farmers' choices unless they are indirectly incorporated in the $EI_{it,k}$ term or properly compensated through policy incentives (the combination of $L^R_{it,k}$, L^C_{it}, $DP_{it,k}$, $G_{it,k}$, $AEM_{it,k}$).

where $L_{it,k}^{R}$ and $L_{it,k}^{C}$ represent the i-th farm revenue losses and additional costs, respectively, at time t implied by the k-th policy regime (i.e., by the environmental standards it imposes);[10] $DP_{it,k}$, $G_{it,k}$ and $AEM_{it,k}$ indicate the three abovementioned AEP payments at time t implied by the k-th policy regime. In (2), $L_{it,k}^{R}$ and L_{it}^{C} are not observable. As $NI_{it,k} = \left[DP_{it,k} + G_{it,k} + AEM_{it,k} - \left(L_{it,k}^{R} + L_{it,k}^{C}\right)\right]$ represents the net monetary incentive/disincentive implied by the k-th policy regime for the i-th unit at time t, also this net incentive remains actually unobserved. Nonetheless, $\left(R_{it} - L_{it,k}^{R}\right)$ and $\left(C_{it} + L_{it,k}^{C}\right)$ can be observed, as well as $DP_{it,k}$, $G_{it,k}$ and $AEM_{it,k}$. More importantly, in (2) the net income $\pi_{it,k} = \left[\left(R_{it} - L_{it,k}^{R}\right) - \left(C_{it} + L_{it,k}^{C}\right)\right] + \left(DP_{it,k} + G_{it,k}\right) + AEM_{it,k}$ can be itself observed. Therefore, (2) can be written in a more compact form as:

(3) $U_{it,k} = \pi_{it,k} + NM_{it,k}, \qquad \forall i = 1, \dots, N;\ \forall t = 1, \dots, T;\ \forall k = 1, \dots, K$

As anticipated, $NM_{it,k}$ represents a treatment-dependent term. When treated with the k-th treatment, the i-th unit activates the $NM_{it,k}$ utility component. Unlike the other utility term $\pi_{it,k}$, however, $NM_{it,k}$ is unobservable. Therefore, within the present modelling approach, not only $NM_{it,k}$ is policy-dependent but it is, in fact, the consequence of the policy itself: it is the unobservable part of the policy treatment effect (TE) whereas $\pi_{it,k}$ is the observable part. The summation of these two parts constitute what we can call the *full unobserved TE*. Through $\pi_{it,k}$ and an appropriate empirical identification strategy, some information about $NM_{it,k}$ can be recovered (see below). Two assumption on this term, however, are here worth making.

Firstly, $NM_{it,k}$ can be either a positive or negative term, depending on the dominant underlying non-monetary motivations and, in principle, it could vary across time. However, these motivations are individual-specific and are expression of farmers heterogeneity (also from a

[10] It is implicitly assumed that both for $DP_{it,k}$ and for $G_{it,k}$, $L_{it,k}^{R}$ and L_{it}^{C} are independent on the respective payments as, after all, they are mostly intended to support farmers' income. As anticipated, the same may hold true also for $AEM_{it,k}$.

cultural, political and ideological perspective). Therefore, they remain arguably constant over a relatively short period of time. Consequently, it can be reasonably intended as a time-invariant term, i.e. $NM_{it,k} = NM_{i,k}, \forall t \in T$.

Secondly, $NM_{i,k}$ could also be either stochastic or deterministic or, to follow the conventional panel data terminology, either a treatment-dependent Fixed-Effect (FE) or Random-Effect (RE). In the former case, it is $NM_{i,k} = a_{i,k}, \forall i \in N, \forall k \in K$. In the latter case, it is $NM_{i,k} \sim N(a_{i,k}, \sigma^2), \forall i \in N, \forall k \in K$. As will be clarified below, the assumption implicitly made here is that this key term is deterministic thus behaving as individual-specific constant term.

3.3. The treatment-effect logic

Can we identify the farm-specific non-monetary motivations on the basis of (2)-(3)? To answer this question we can notice that, since farms are not randomly or exogenously assigned to a policy but, in fact, they voluntary choose it, we can assume that for any i-th farm at time t it is $U_{it,k}|T_k \geq U_{it,h}|T_h, \forall k,h \in K, k \neq h$: the utility of the i-th farm that has chosen the k-th policy regime at time t is higher than (or equal to) the utility it would have achieved had it chosen any other h-th regime. Therefore, for income maximizing farms the observed policy can be informative about the relative magnitude of the underlying utility components in the i-th farm at time t, i.e. $\pi_{it,k}$ and $NM_{i,k}$.

More specifically, for any given i-th unit, three alternative explanations of the k-th treatment choice can be given:

1. $U_{it,k}|T_k > U_{it,k}|T_h$ because $\pi_{it,k}|T_k > \pi_{it,h}|T_h$ ($\Delta\pi_{it} > 0$) and $NM_{i,k} > NM_{i,h}$ ($\Delta NM_i > 0$), so it necessarily is $\pi_{it,k} + NM_{i,k} > \pi_{it,h} + NM_{i,h}$.
2. $U_{it,k}|T_k > U_{it,k}|T_h$ because $\pi_{it,k}|T_k > \pi_{it,h}|T_h$ ($\Delta\pi_{it} > 0$) and $NM_{i,k} < NM_{i,h}$ ($\Delta NM_i < 0$), but it is still $\pi_{it,k} + NM_{i,k} > \pi_{it,h} + NM_{i,h}$.

3. $U_{it,k}|T_k > U_{it,k}|T_h$ because $\pi_{it,k}|T_k < \pi_{it,h}|T_h$ ($\Delta\pi_{it} < 0$) and $NM_{i,k} > NM_{i,h}$ ($\Delta NM_i > 0$), but it is still $\pi_{it,k} + NM_{i,k} > \pi_{it,h} + NM_{i,h}$.

As the i-th farm chooses the k-th instead of the h-th policy in order to achieve an utility gain, $\Delta\pi_{it}$=($\pi_{it,k} - \pi_{it,h}$) reveals the income consequence of this choice and, indirectly, reveals the sign and magnitude of the underlying non-monetary motivations, ΔNM_i relative to this $\Delta\pi_{it}$: whenever $\Delta\pi_{it} < 0$ we can conclude that $\Delta NM_i > 0$ and it is large enough to overcompensate the income loss. Assumed that both $\Delta\pi_{it}$ and T_i can be observed, this information can be interpreted in treatment-effect logic: $\Delta\pi_{it}$ is the observed TE (i.e., on the outcome variable π_{it}) of moving from h-th policy (or treatment) to k-th policy (or treatment). Since it is farm-specific, $\Delta\pi_{it}$ can be intended as an ITE. Moreover, as it will be clarified below, the potential outcome framework allows separately identifying the ITE on the treated and the expected ITE on the untreated units, thus distinguishing between the *Individual Treatment effect on the Treated* (ITT) and the *Individual Treatment effect on the Untreated* (ITU) (Wang et al., 2017).

To make this TE interpretation more clear, two simplifications are useful and are also consistent with the empirical application that will be presented later. First of all, as in the present study the adopted dataset eventually collapses to a cross-sectional sample, henceforth we omit the time index *t*. Secondly, the analysis is restricted to the simplified binary treatment case ($T_{it} = 0,1$).[11]

Given these simplifications, the *Individualised Average Treatment Effect* (IATE), $\tau(\mathbf{X}_i)$, can be identified as (Knaus et al. 2021):

(4) $\tau(\mathbf{X}_i) = \mathbb{E}[\pi_i|\mathbf{X}_i, T_i = 1] - \mathbb{E}[\pi_i|\mathbf{X}_i, T_i = 0]$

[11] Notice that, under these two assumptions, in the binary treatment case $NM_{i,k}$ can be simply written as NM_i that indicates the extra-income motivations associated to $T_{it} = 1$ (i.e., it is $NM_i = NM_{i,1}$).

where $\mathbf{X}_i$ is the (Px1) vector of P exogenous variables observed for the i-th unit and affecting both the potential outcomes and the assignment to the treatment (also called *confounding variables*).

$\tau(\mathbf{X}_i)$ can be given two opposite interpretations depending on how the binary treatment is defined and on the treatment regime of the i-th unit. In one case, the treatment ($T_i = 1$) is pro-environment, so it requires higher environmental standards with a consequent higher support. Alternatively, the treatment ($T_i = 1$) is anti-environment, so it requires lower environmental standards with a consequent loss of support.

In the first case, farms showing $IATT = \tau(\mathbf{X}_i|T_i = 1) < 0$ (i.e., $(NM_i|T_i = 1) > 0$) can be associated to a greater pro-environment non-monetary motivations, while farms showing $IATU = \tau(\mathbf{X}_i|T_i = 0) < 0$ (i.e., $(NM_i|T_i = 0) > 0$) to a greater anti-environment non-monetary motivations. According to section 3.1, the pro-environment farms are driven by the prevalence of warm-glow effect/green-guilt aversion, loss aversity and attrition, while anti-environment are driven by the prevalence of control aversity and attrition. In the second case, $IATT = \tau(\mathbf{X}_i|T_i = 1) < 0$ indicates greater anti-environment non-monetary motivations, while $IATU = (\mathbf{X}_i|T_i = 0) < 0$ greater pro-environment non-monetary motivations. The same interpretation about the prevalence of the non-monetary drivers applies.[12]

Assessing how many and which kind of farms show a negative IATE (IATT or IATU) under these two cases thus provides empirical evidence on the non-monetary motivations of farms eventually overcompensating the monetary motivations.

3.4. Policy assessment

The main interest in identifying and estimating the IATE resides in performing a policy assessment. Within the recent Optimal Policy Learning (OPL) literature (Athey et al., 2020),

[12] Section 5.2, and Table 1 in particular, illustrates these cases for the CAP case under investigation here.

estimated IATE allow computing the welfare loss (also known as "the regret") associated to the actual individual treatment assignment and, then, the difference with the respect to an ideal (possibly constrained) welfare maximizing assignment net of policy expenditure (Cerulli, 2020). An extensive OPL analysis is beyond the scope of the present study. Here, we want only to show how the IATE estimation can lead to a constructive policy assessment and sketch some initial evidence in this respect.

Assume that the policy maker cares about the net externalities (the balance between positive and negative externalities) of farming and the policy expenditure needed to induce them. A full optimization approach can be challenging as getting a reliable monetary evaluation of these externalities is difficult if not impossible. However, we can still think about a policy maker pursuing a sort of a constrained maximization that can be interpreted in two possible directions: minimize the policy expenditure for a given amount of net externalities; maximize the net externalities with a fixed policy expenditure.

Now, consider a pro-environment treatment. Two policy assessments can be performed. The first consists in computing how much policy expenditure on treated units (T_i=1) could be saved (*Total Policy Saving*, *TPS*) maintaining the same environmental performance. *TPS* can be computed by considering all those treated farmers whose monetary motivations would lead them to adopt the k-th regime even with a lower policy support. On the basis of the modelling approach illustrated above, it is:

$$\text{(5a)} \quad TPS = \sum_{i=1}^{M} PS_i \text{ with } \begin{cases} PS_i = P_i \;\; if \; P_i < \; IATT_i > 0 \\ PS_i = IATT_i \;\; if \; P_i \geq \; IATT_i > 0 \\ PS_i = 0 \;\; if \; IATT_i < 0 \; (\Delta NM_i > 0) \end{cases}$$

where PS_i is the i-th unit policy saving, P_i is the i-th unit CAP support associated to the environmental performance and delivered to the treated units (therefore, dropped by the untreated ones); *M* is the number of the traded units (i.e., farms moving from the h-th to the k-th treatment), with $M \in N$; $IATT_i$ is the estimated $\Delta\pi_i$ in treated units.

The secondo policy assessment concerns the untreated units (T_i=0) and follows an analogous logic. In such case, it possible to compute how much policy expenditure should be increased (*Total Policy Extra-Expenditure*, *TPE*) to improve the environmental net externality, namely, to convince any farm to adopt the pro-environment treatment thus providing an higher environmental performance. Even though this calculation can not be conclusive in terms of policy maker optimization, since the monetary value of this externality gain is unknown, *TPE* can still be an useful information as it can be intended as the shadow social value of this additional net externality. Following the arguments above, it is:

$$(6a) \quad TPE = \sum_{i=1}^{N-M} |IATU_i| \;\; if \;\; IATU_i < 0 \; (\Delta NM_i > 0)$$

where $IATU_i$ is the estimated $\Delta\pi_i$ in untreated units.

This policy assessment can be replicated, symmetrically, in the case of an anti-environment treatment. For the treated units (T_i=1), it will be possible to compute:

$$(6b) \quad TPE = \sum_{i=1}^{M} IATT_i = \Delta\pi_i \;\; if \;\; IATT_i > 0$$

For the untreated units (T_i=0), it will be:

$$(5b) \quad TPS = \sum_{i=1}^{N-M} PS_i \text{ with } \begin{cases} PS_i = P_i \;\; if \; IATU_i < 0, P_i < |IATU_i| < 0 \\ PS_i = |IATU_i| \; if \; IATU_i < 0, P_i \geq |IATU_i| > 0 \\ PS_i = 0 \;\; if \;\; IATU_i > 0 \; (\Delta NM_i < 0) \end{cases}$$

where P_i now is the i-th unit CAP support associated to the environmental performance and delivered to the untreated units (therefore, dropped by the treated ones).

4. The estimation approach

4.1. The identification strategy

The major empirical issue for the identification of the IATE (IATT and IATU) with observational data consists in finding appropriate counterfactuals for any i-th treated unit. The typical approach to this issue relies on the *potential outcome framework* (Imbens and Rubin,

2015) and its underlying assumption, the *Conditional Independence Assumption* (CIA, or Unconfoundedness) (Imbens, 2020):[13]

(7) $T_i \perp (\pi_i(0), \pi_i(1))|\mathbf{X}_i$

where $\pi_i(1)$ and $\pi_i(0)$ indicate the potential outcome of the i-th farm with $(T_i = 1)$ and without $(T_i = 0)$ the treatment, respectively.[14]

Under the CIA we can conclude that the observational data can be properly investigated within a quasi-experiment or natural experiment setting (Angrist and Pischke, 2010). Here, this natural experiment assumption is that the reasons why farmers choose a treatment (the unobserved non-monetary drivers included) are independent on the observed treatment effect ($\Delta\pi_i$) once we control for **X** or, more explicitly, that these drivers evidently affect the treatment choice but not the potential outcomes (Athey et al., 2020). If CIA holds true, the selection bias is excluded, that it, $\mathbb{E}[\pi_{i0}|\mathbf{X}_i, T_i = 1] - \mathbb{E}[\pi_{i0}|\mathbf{X}_i, T_i = 0] = 0$ (Angrist and Pischke, 2009, p. 54).

It remains true that within a non-experimental context we only observe the potential outcome that corresponds to the realized T_i, either $\pi_i|\mathbf{X}_i, T_i = 1$ or $\pi_i|\mathbf{X}_i, T_i = 0$. Namely, we only observe $\pi_i = T_i\pi_i(1) + (1 - T_i)\pi_i(0)$ while $\tau(\mathbf{X}_i)$ remains unobservable. The only possible way to identify $\tau(\mathbf{X}_i)$ is to compare $\pi_i|\mathbf{X}_i, T_i = 1$ with untreated units (the counterfactuals or controls) that are statistically equivalent to the i-th unit over **X**. As variables in **X** are expected to deconfound the (self)allocation of farms into the treatment, this identification strategy requires an additional assumption: together with the CIA, the balance (also known as overlap,

[13] A second assumption that is assumed valid here, and it is needed for the identification of the IATE, is the Stable Unit Treatment Value Assumption (SUTVA). It rules out any influence of an individual's treatment status on another individual's potential outcome.

[14] Condition (7) postulates the independence between the potential outcomes and the treatment conditional on a set of pre-treatment (exogenous) variables, $\mathbf{X}_i$. Therefore, conditional on observed confounders, the treatment is as good as randomly assigned" (Athey and Imbens, 2017, p. 5): given $\mathbf{X}_i$, knowledge of T_i provides no information about both $\pi_i(1)$ and $\pi_i(0)$, and viceversa. This condition is here formulated only in terms of the observed part of the TE, π_i, not in terms of the full unobserved TE. In this latter case, the CIA should be formulated as $T_i \perp \pi_i(0), \pi_i(1), NM_i(1)|\mathbf{X}_i$ since, as illustrated, the utility component *NM* only appears under the treatment. In general, we need to assume that, once we control for **X**, both parts of the TE can be properly identified. It can be argued that there can be unobservable characteristics that eventually generate the unobserved TE term, NM_i, and this would result in a selection bias. In fact, this risk of selection on unobservables does not differ between these two parts, π_i and NM_i, as these unobservable characteristics could affect π_i as well. Even though not strictly needed for the TE identification, another assumption that seems consistent with the present theoretical framework, and the additive nature of (3) in particular, is $\pi_i(0), \pi_i(1) \perp NM_i(1)$, that is, the two utility terms are perfectly separable.

or positivity, or common support) condition must be respected. It establishes that $0 < Pr(T_i = 1|\boldsymbol{X}_i) < 1$, i.e. a positive probability of both treated and untreated units within different strata of **X**. Empirically, this condition implies that there must be at least one treated unit and one control unit at each possible value of all exogenous variables in **X** (Sauppe and Jacobson, 2017).

Since we are here dealing with treatments implying a voluntary participation, assuming that statistically equivalent profit-maximizing units eventually choose different treatments and, thus, some of them can act as counterfactuals may seem contradictory. Apparently, if exactly equivalent farms on **X** select different treatments it means that something outside **X** affects the decision. Acknowledging some selection on unobservables, however, inevitably conflicts with the CIA. In the present context, however, it is still possible to conclude that both conditions remain valid under farms' heterogeneity if this heterogeneity concerns the utility function but not its determinants. The bottom line of this argument is that there can be some relevant unobservable heterogeneity across farms that interferes with the farms' utility, thus making them different and justifying their voluntary selection of different treatments. But this do not affect the eventual outcome π conditional on **X** (Imbens, 2020).

In order to estimate the IATE following this strategy, now assume that the data generating process of the outcome variable π_i follows a stochastic process defined as:

(8) $\pi_i = f(\mathbf{X}_i, T_i) + \varepsilon_i$

where f indicates an arbitrarily complex function, and ε_i represents an additive idiosyncratic disturbance term assumed i.i.d.~$N(0,\sigma^2)$. Therefore, it is $\mathbb{E}[\pi_i|T_i, \mathbf{X}_i] = f(\mathbf{X}_i, T_i)$ and, according to (4), $\tau(\mathbf{X}_i)$ can be estimated $\tau(\mathbf{X}_i) = \hat{f}(\mathbf{X}_i, 1) - \hat{f}(\mathbf{X}_i, 0)$. Consequently, an appropriate specification of function $f(\mathbf{X}_i, T_i)$ is needed. It could be explicitly specified as a parametrically function but this inevitably constraints the cross-farm technological and behavioural heterogeneity. An alternative solution consists in admitting an individual-specific function and in developing a nonparametric approach to estimate it.

4.2. Causal Forest estimation and inference

An appropriate approach to estimate (8) and, consequently, the IATE (IATT and IATU) must admit large heterogeneity across farms, thus operating without either information or assumptions about the true parametric form of the response surface. At the same time, this non-parametric estimation approach must also allow inference, that is, IATE estimates grounded on distributional properties and asymptotic theory, with a consequent standard errors estimation.

The widely growing and quickly spreading Machine-Learning (ML) toolbox offers very interesting solutions in this direction: they can deal with large heterogeneous samples, many and interacting confounding variables and possibly nonlinear relationships (Mullainathan and Spiess, 2017; Lu et al., 2018).

These techniques allow flexible (i.e., non-parametric) response surface estimation thus optimizing, within the potential outcome framework here adopted, the search of overlapping observations. Among these ML approaches, in particular, a Causal Forests (CF) estimation is here performed (Athey and Imbens, 2017; Athey and Imbens, 2019; Athey et al., 2019; Athey et al., 2020).[15] The origin of the CF estimation approach traces back to the contribution of Athey and Imbens (2016). These authors developed a method (called *causal trees*) based on the ML method of regression trees but using a different criterion for building the trees: rather than focusing on improvements in mean-squared error of the prediction of outcomes, it focuses on mean-squared error of treatment effects. The method relies on sample splitting, in which randomly selected half sample is used to determine the optimal partition of the covariates space (the tree structure), while the other half is used to estimate treatment effects within the leaves.

[15] An alternative but closely related ML approach to IATE estimation and inference is based on Bayesian Additive Regression Trees (BART). See Coderoni et al. (2021) for an application to the agro-environmental context.

The output of the method proposed by Athey and Imbens (2016) is a TE and a respective confidence interval for each subgroup. However, when the focus of the analysis is on treatment effect heterogeneity, a disadvantage of the causal tree approach is that the estimates are not personalized for each individual as all individuals assigned to a given group have the same estimate. To overcome this limitation, Wager and Athey (2015) proposed a method (i.e., CF estimation) for estimating heterogeneous treatment effects based on random causal trees. Compared to a causal tree, which identifies a partition and estimates treatment effects within each element of the partition, CF estimation leads to estimates of causal effects that change more smoothly with covariates, and in principle every individual has a distinct estimate. Random forests are known to perform very well in practice for prediction problems, but their statistical properties were less well understood until recently. Wager and Athey (2015) show that the predictions from CF are asymptotically normal and centred on the true conditional average treatment effect for each individual. They propose an estimator for the standard error, so that confidence intervals can be also obtained.

This CF approach is here adopted. First, a flexible model for treatment impacts, like (8), is fit in order to compute the IATE for both Treated (IATT) and Untreated (IATU) units.[16] On the basis of these IATE estimates, the policy assessment is then performed by computing the respective *TPS* and *TPE* (equations 5a,b and 6a,b). Finally, Average Treatment Effects (ATE) and Group Average Treatment Effects (GATE) are obtained by averaging the IATE over the full distribution of **X**, or over subgroups aggregated on some exogenous covariate, respectively. These CF estimation steps are performed with the R *grf* package (Tibshirani et al., 2018)[17] then integrated with STATA functionalities using the *MLRtime* package.

[16] One drawback of this approach is the possible presence of covariate imbalance, that is the violation of the overlap (or common support) assumption. In such case, the CF algorithm may force the model to make out-of-sample extrapolations. In the present application, the lack of overlap may represent a severe problem as the *T1* treatment group is likely to include some peculiar farms, at least for some of the covariates included in **X** (Esposti, 2017a; 2017b). The first step of the estimation approach thus excludes those units not respecting the overlapping condition.

[17] Updated versions and more details can be found at https://cran.r-project.org/web/packages/grf/index.html.

5. The empirical application

Section 3 puts forward an implicit working model with causality going from the treatment to the outcome variable as a consequence of the underlying theory (utility maximizing farmers). The theoretical background also serves to better design the quasi-experimental setting, namely, properly defining the treated and the control groups, the outcome, the treatment and the confounding variables. The rest of this section deals with all these aspects.

5.1. The observational dataset

Within the EU, the Italian agriculture is often considered an interesting case study for the wide heterogeneity of its farming conditions and traditions which inevitably affect farms' environmental impact (Coderoni and Esposti, 2018). For this reason, the dataset used in this study is the Italian 2008-2018 FADN sample. It is a representative sample of commercial farms. The whole sample is an unbalanced panel ranging from a maximum of 11,398 farms (in 2013) to a minimum of 9,580 farms (in 2015). However, as the AEP regimes are separately considered for the periods before and after 2015 (the first year of implementation of the 2013 CAP reform), in the reminder of the analysis two balanced panel samples are extracted (Table 2), the first consisting of 3502 farms (2008-2014), the second consisting of 5849 farms (2015-2018). Eventually, the total number of observations are quite similar in the two cases, 24514 and 23396, respectively.

The advantage of working with balanced panels consists in eliminating units for which the few available observations (often just one year) may introduce unreliable or noisy statistical information. In addition, also considering that the main focus here is not on the timing of AEP adoption,[18] smoothing over repeated observations is expected to provide more stable and reliable values for both the outcome variable and the exogenous confounders. Therefore, the

[18] In both balanced panels there are few farms for which the policy regime (i.e., the treatment status) switches during the period. In these cases, the treatment observed for more years is assigned.

time dimension is averaged out within the 2008-2014 and 2015-2018 periods and the two panel samples collapse to two cross-sectional samples (see Coderoni et al. 2021).

5.2. The treatment sets

As anticipated, the main reason to separately work with the two sub-periods is that in both we can find analogous policy regimes therefore allowing a comparable assessment. Within the CAP, the AEP that eventually combine are (*DP*+*CC*), *G* (only applying to years 2015-2018) and *AEM*. Therefore, three possible regimes, or treatments, can be identified: *T0* is the control group or baseline regime/treatment where farms adopts the environmental standards required to receive the *DP* (*G* included); *T1* is the anti-environment regime, where farms do not adopt any environmental standards (but those imposed to all producers) thus dropping all *DP*; *T2* is the pro-environment regime, where farms also adopt the additional requirements implied by the *AEM* payment.

Table 1 summarizes the different AEP regimes that can be observed across the 2008-2018 period, while Figure 1 displays the respective dynamics over the years. It clearly emerges how, by changing the first pillar direct payments' implementation, the 2013 CAP reform (starting in 2015) made the before-2015 and after-2015 comparison unfeasible. Nonetheless, in both sub-periods three analogous regimes emerge. A peculiar case is represented by those few farms (260 and 81 units in 2008 and 2018, respectively) that receive the *AEM* payment but do not receive first pillar *DP*. Therefore, it is difficult to compare this regime with the others in terms of environmental standards' compliance. For this reason, these farms are excluded from the present analysis.

The empirical analysis is here repeated for the three binary treatment comparisons: *T0* (contol) vs *T1*; *T0* (contol) vs *T2*; *T1* (contol) vs *T2*. Table 2 reports the aggregation of the abovementioned policy regimes into these three treatments. Comparing the treatment group *T1* with the control group *T0* corresponds to the following quasi-experimental logic: what would happen (in terms of π and, consequently, what would reveal about *NM*) to the control group farms if they were not treated with the first pillar *DP*? Comparing the treatment group *T2* with the control group corresponds to this quasi-experiment: what would happen to the control group farms if they were also treated with the second pillar payments *AEM*? The comparison between treatment groups *T1* and *T2* may finally help in confirming the results of first two "experiments" and, therefore, in assessing whether these results respect what can be referred to as the *monotonic response condition* (Esposti, 2017a,b): passing from *T1* to *T2* is expected to generate a response that is at least as large as that obtained when passing from *T0* to *T2.* Table 3 recaps the logic and the interpretation of these treatment group comparisons.

In fact, the response monotonicity could be observed on average, therefore comparing the respective ATE, but, due to TE heterogeneity, this does not necessarily respect the condition. The theoretical model here adopted actually implies that the response monotonicity applies to any unit, thus should be observed in the IATE estimates. In the present case, however, treatment group varies and, therefore, the only possible assessment of the monotonic response condition concerns the treatment group T2. It implies that for any unit of this group the treatment response (i.e., the IATT) with respect to control *T1* is of the same sign (either positive or negative) but of larger magnitude than the IATT observed with respect to control *T0*.

[Tables 1, 2 and 3 here]

[Figure 1 here]

5.3. Outcome and confounding variables

Also the proper definition of the outcome (π) and confounding (**X**) variables is driven by the theoretical framework illustrated in section 3. According to (2) and (3), π is the monetary component of the farmer's utility, that is, $\pi_{i,k} = \left[\left(R_i - L_{i,k}^R\right) - \left(C_i + L_{i,k}^C\right)\right] + DP_{i,k} + G_{i,k} + AEM_{i,k}$. π can be measured as the farm profit or, more consistently within the Italian agriculture mostly based on family farms, the farm net income. As in the agricultural case context, net income can be very volatile year-by-year, this represents an additional motivation to work with cross-sectional samples by averaging over the whole observed sub-periods.

One practical problem in using the net income as outcome variable is that it is highly size dependent. This dependency can be hardly controlled by including size in **X**, as it can not be excluded that a size bias remains. Therefore, to make π size-independent, net income is here divided by the annual units of farms' autonomous or family work. After all, per capita farm income is what the farmers really care about, so what drives their choices. Therefore, it seems the most appropriate definition of the outcome variable within the present study.

Following Coderoni et al. (2021), we can argue that, although it remains unknown and thus implicit in the present study, the firm-specific technology is the major confounder in the TE analysis: the farm technology affects, at the same time, the outcome variable and the treatment assignment or choice. It is thus assumed that π results from a firm-specific non-constant returns to scale multi-input multi-output technology. Confounding variables are selected to represent the key features of this underlying technology.

First of all, in order to express the presence of non-constant returns to scale, a size variable is included (SIZE). Size here enters as dimensional class, that is, an ordered categorical variable[19] and it is preferred to a continuous variable expressing the economic size (for instance, the Gross Production Value, GPV) as it is much more stable over time. Secondly, to take the multioutput nature of technology into account, the farm's production specialization (PRO) is included. Also in this case a (unordered) categorical variable is preferred as the share of specific products on GPV would be highly unstable.[20] Moreover, as this PRO variable does not consider forestry

[19] Farms are distributed across classes from 1 to 7 where class 1 represents to smallest size and class 7 the largest.

[20] Five farm types are considered: Livestock farms, Livestock&crops farms, Annual-crops farms, Perennial-crops farms, Other farms.

production, an additional variable (FOR) expressed as the ratio of farm's forest area on the utilised agricultural area (UAA) is included.

Thirdly, to represent the multiple production factors, a four-input technology (i.e., capital (K), labour (L), energy (E), materials (M)) is considered. Capital is expressed by the total farm's machinery power (KW), by the UAA and by the livestock units (LSU). Labour is expressed by annual working units (AWU). Energy is expressed by the total expenditure for energy (fuels included) (ENE), while materials are expressed by the total expenditure for fertilizers, pesticides and animal feed (MAT).

To complete the definition of the farm's factor endowment, two further corrections are made. First of all, input quality matters and this is particularly true for labour. To take this aspect into account, the farm holder's age is included (AGE) together with the dummy expressing her/his gender (GEN, where female=1).[21] In addition, to be consistent with the size-independent outcome variable, all the non-categorical input variables are expressed as intensities, that is, divided the farm's GPV. For instance, AWU is computed as the annual working units per unit of GPV.

A final set of confounding variables is intended to take the farm's geographical context into account. In particular, the farm's average altitude (ALT), latitude (LAT) and longitude (LON) are included among covariates. The (Px1) vector **X** is thus made of the following 10 variables (P=10): SIZE, PRO, FOR, KW, UAA, LSU, AWU, ENE, MAT, AGE, GEN, ALT, LAT, LON. All these covariates, as well as the outcome variable, enter the estimation stage as 2008-2014 and 2015-2018 averages. In the case of categorical variables for which a change is observed, the prevalent category over the period is used. Table 4 reports the descriptive statistics of all model variables in the two sub-periods.

[Table 4 here]

[21] The farm holder's education could be also useful, but this information is unavailable within the FADN.

6. Estimation results

6.1. IATE estimates across periods and treatment comparisons

Figures from 2.I to 7.III display the heterogenous individual TE estimates ordered from the lowest to the highest value. The sequence of these figures follows this logic. First, the results for the 2008-2014 period are presented; then, those concerning the 2015-2018 period. Within each period, the three comparisons are presented in this sequence: the anti-environment treatment, *T0* (control) vs *T1*; the first pro-environment treatment, *T0* (control) vs *T2*; the second pro-environment treatment, *T1* (control) vs *T2*.

In order to better appreciate the critical results, all figures report a sequence of three charts: the first (panel a) shows the estimates over the whole range of variation (so, all units); the second and the third (panels b and c) only the units with a negative and positive individual TE, respectively. Finally, for any treatment comparison the IATE are firstly reported then followed by the respective IATT and IATU. This allows visualize whether, as expected, a TE difference emerges between the treated and the non-treated units.

Before entering the details of the specific case, it is interesting to highlight some robust evidence that seems to regularly emerge across all cases. First of all, for many units the policy responses (i.e., the IATE) is quite close to 0 and, above all, not statistically significant. This evidence should not surprise as it seems consistent with the underlying theoretical background where the farmers' treatment choice may result from the combination of possibly conflicting monetary and non-monetary motivations. At the same time, a remarkable heterogeneity emerges in all circumstances with IATE spread over a wide range of values both positive and negative and with few vary large (in absolute terms) IATE. The combination of many close-to-0 values with a large dispersion can evidently generate an average TE (ATE) within the whole sample that is itself not statically significant and also of poor policy relevance.

What is worth noticing here is that in all cases many units show an unexpected result, that is IATT<0 or IATU>0. Again, for many of these cases the estimated TE is not statistically different from 0, so it is inconclusive from a policy perspective. Nonetheless, the number of units showing a statistically significant unexpected response is remarkable (see section 6.3) because, as discussed, it can only be justified by the prevalence of specific non-monetary motivations. According to (3), it thus emerges that for many units the NM_i term of the utility can counterbalance the monetary term (π_i), thus inducing the unexpected response.

Going more in the details of single comparisons and periods, Figure 2.I indicates, over period 2008-2014, a prevalence of positive IATE on negative ones, in terms of both number of units and magnitude of the response. Just few statistically significant negative responses are observed and these never exceeds 30.000 €, while among the many positive IATE we find several units with a response that is larger than 30.000 €. This would suggest that the monetary motivations prevail on the non-monetary ones in the case of the anti-environment treatment (that is, from control *T0* to *T1*). In fact, while this interpretation can be given for the treated units, this is not the case for the non-treated ones. In order to really confirm this conclusion we need to more carefully distinguish between the IATT and the IATU (Figure 2.II and 2.III, respectively).

It emerges that, within treated farms, a positive effect (IATT) is mostly observed thus suggesting that the anti-environment treatment choice is essentially driven by monetary motivations. However, several farms also show a statistically significant negative effect. For these latter cases we can conclude that non-monetary motivations (control aversity and some form of attrition) are the main determinants of the treatment choice. Among non-treated units, i.e. the control group, we observe few statistically significant values either positive or negative. These few significant negative IATU would indicate that the monetary motivations behind the choice of not entering the anti-environment treatment are relatively marginal. On the contrary, the significant positive IATU suggest that, even though they are lower in magnitude compared to the IATT, also for these farms a major non-monetary motivation occurs but moving in the opposite direction: warm-glow effect or green-guilt aversion possibly together with loss aversion seem to lead many farms to the choice of the pro-environment regime *T0*.

Similar conclusions can be drawn for the *T0* vs *T2* comparison, same period (Figure 3.I). Again, positive IATE prevail though many not statistically different from 0. Still several negative values are observed and several of them are not only statistically significant, but also show a magnitude that is higher than the highest positive responses. Even in this case, however, what seems interesting for the present study emerges from the comparison between IATT and IATU estimates (Figures 3.II and 3.III). Here no major difference is observed: in both treated and non-treated groups a large heterogeneity occurs with both positive and negative TE.

Positive values actually prevail in terms of number of farms but the negative values, when statistical significance, assume magnitude that tend to be higher than the positive ones. The only difference seems to be the presence of very high extreme positive IATT values that are not observed for IATU. As this case concerns a pro-environment treatment, the few negative IATTs unveil those farms for which a relevant pro-environment non-monetary motivations prevail. On the contrary, the statistically significant positive IATU suggest that several farms within the control group do not choose the pro-environment treatment for the presence of anti-environment non-monetary motivations.

As anticipated, the *T1* vs *T2* treatment case (Figures 4.I. 4.II and 4.III) is interesting mostly because it can provide confirmation of the results obtained in the previous two cases. Overall, the estimated IATE (Figure 4.I) confirm few statistically significant values, especially in the positive dominion. In fact, not only negative IATE are observed for almost half of the *T1+T2* sample. In addition, the statistically significant negative values prevail in both numerosity and magnitude. Separately focusing on IATT and IATU, it emerges that statistically significant negative values tend to prevail among IATT estimates, while positive cases among IATU values

(Figures 4.II and 4.III). This would suggest that in both treatment groups major on-monetary motivations emerge as determinants of the treatment choice, though the interpretation proceeds in the opposite direction. Negative IATT values indicate the presence of pro-environment motivations, while positive IATU the presence of anti-environment motivations.

More interestingly, as mentioned at the end of section 5.3, the IATT estimates here obtained can be compared with the IATT estimates of the *T0* vs *T2* case. This comparison can help assessing whether the monotonic response condition is respected. This condition implies that motivations leading to a treatment choice in the *T0* vs *T2* case should be reinforced in the *T1* vs *T2* comparison. Confronting Figures 3.II and 4.II it is confirmed that the negative IATT are more numerous and show higher magnitude when the pro-environment nature of the treatment is stronger. Evidently, when pro-environment non-monetary motivations are present and prevalent, they play a larger role when the treatment is more clearly pro-environment.

Moving from the 2008-2014 to the 2015-2018 subperiod (Figures 5-7), most of the main evidence discussed above seems to be confirmed. Nonetheless, the second period of observation reveals an overall lower statistical significance of estimated IATE. Despite the larger balanced sample, this may be attributed to the shorter time over which variable averages are computed, thus making their variability larger. As a consequence, the TE heterogeneity is even wider. Although many IATE are not statistically different from 0 on both tails of the IATE distribution, we also observe very high values (in absolute terms).

Another difference between the two periods of investigation emerges from comparing the IATT with the respective IATU. It emerges an higher/lower incidence of statistically negative/positive values among the IATU than the IATT. Following the arguments above, this would indicate a relatively greater relevance of the monetary motivations with respect to non-monetary ones in both pro- and anti-environment treatments. The comparison of the IATT for the two treatments involving the *T2* group (Figures 6.II and 7.II) confirms what observed in the previous period about the monotonic response condition. The larger is the pro-environment nature of the treatment, the more intense are the negative values as should be expected since negative IATT imply relevant pro-environment non-monetary motivations.

[Figures 2-7 here]

6.2. ATE and GATE estimates

As discussed in section 4.2, from these IATE estimates respective ATE and GATE estimates can be obtained. This is the empirical evidence on which most TE studies insist and on which policy implications are usually drawn. They are interesting results also in the present case but, under such large heterogeneity, they must be taken with caution. It is also worth reminding that heterogeneous effects ranging from negative to positive values may eventually generate small and not statistically significant average values. Therefore, on the one hand, if statistical significant, ATE and GATE estimates surface on the large IATE heterogeneity and may interpreted as the prevalent effect within the sample under observation. On the other hand, however, these values may actually apply to very few units with a consequent poor policy implication. In any case, following the argument above, here only the significant ATE and GATE estimates will be commented.

Table 5 reports the ATE, ATT and ATU estimates across the three different treatments and the two samples. In the first period (2008-2014), only for the *T0* vs *T2* comparison statistically significant ATE, ATT and ATU estimates are obtained. A positive value emerges thus indicating a prevalent monetary determinant in the farmers' treatment choice. In fact, this interpretation strictly hold true only for the treated units: the ATT is itself positive and larger, in magnitude, than the ATU. This would suggest that, even if present, possibly anti-environment non-monetary motivations do not compensate the stronger monetary incentive to participate to the treatment. Alternatively, it could be argued that pro-environment non-monetary motivations are so strong to eventually compensate the negative monetary incentive.

At the same time, however, also the ATU is positive. The interpretation in this case is the opposite: despite the significant monetary incentive, on average the non-treated units show a stronger anti-environment non-monetary motivations that lead them to remain in the control regime *T0*.

For the 2015-2018 period, the only statistically significant ATE concerns the *T1* vs *T2* case. As discussed, it could be intended as an extremization of the *T0* vs *T2* comparison with an even stronger pro-environment treatment. Results can be thus interpreted in a similar way. In the 2008-2014 period, this ATE is positive and larger in magnitude than the *T0* vs *T2* case. Respective ATT and ATU are very similar. This result can be interpreted as the relative importance of the two terms of farm's utility in generating the regime choice. The ATT signals that the monetary incentive passing from *T1* to *T2* is so strong to overcompensate any possibly anti-environment motivations. After all, in this specific case such monetary incentive consists in the combination of *DP*, *G* and *AEM* payments. At the same time, however, the estimated ATU support the idea that for the non-treated units these non-monetary motivations actually are much stronger and convince these farms to remain in the *T1* regime despite the loss of a large policy support.

A final remark on these average TE estimates concerns the ATT referring to *T2* group. Comparing the ATT in the two treatment cases *T0* vs *T2* and *T1* vs *T2* may be interpreted as an indirect assessment of the monotonic response condition. The ATT estimate is negative, of small magnitude and not statistically significant in the *T0* vs *T2* case, it remains interesting to notice that a lower ATT observed in the less extreme pro-environment treatment is consistent with the monotonic response condition only if we assume significant pro-environment motivations of these farms and, therefore, that what observed in the *T1* vs *T2* case is the combination of concordant monetary incentives and non-monetary motivations. The same conclusion should actually apply to the 2008-2014 period. However, it must be reminded, once more, that drawing these conclusions on the basis of the average TE estimates can be misleading as they can hide the major heterogeneity observed across farms in terms of response to the different policy regimes.

With the same cautions outlined in commenting the ATE estimates, also the average TE estimated over subgroups or intermediate aggregation levels of the exogenous covariates (GATE) can be informative. Table 6 reports selected GATE estimates on those covariates that reveal extreme or peculiar IATE values or that assume specific policy interest. First of all, it emerges that gender may matter as female holders tend to show larger TE in absolute terms. However, only in one case the respective GATE is statistically significant and it concerns the *T1* vs *T2* case during the 2015-2018 period. The value obtained is positive as the respective ATE but also much higher suggesting, for these farms, an higher monetary incentive associated to the pro-environment treatment and, consequently, larger non-monetary motivations eventually compensating it.

The other selected GATE reported in Table 6 concern the sub-group of young farmers and two production specializations (Livestock and Perennial Crops farms) that, especially in Italian case, are often associated to quite distinctive farm structure and external conditions. In all cases, the statistically significant GATE overlap with the ATE estimates of Table 5: the *T0* vs *T2* treatment for the 2008-2014 period, the *T1* vs *T2* treatment for years 2015-2018. In both cases, estimated values are positive, but the magnitude is significantly larger than the respective ATE, about two times and three times, respectively. This would suggest that for young holders and Livestock and Perennial Crops farms, the monetary incentive associated to the pro-environment treatment is higher than all other farms. Consequently, the anti-environment non-monetary motivations of farms remaining in the control regime have to be also stronger.

[Tables 5 and 6 here]

6.3. Policy implications

What is mostly interesting, and novel, in the approach here presented consists in the exploitation of the estimated IATE in order to assess whether and how a rationalization of the policy under investigation can be achieved. Section 3.4 presented the indicators that can be computed in this respect starting from either IATT or IATU values. Table 7 firstly reports those IATT and IATU estimates in the two balanced panel samples and across the different treatments that, according to the theoretical framework, imply relevant non-monetary motivations ($NM_i>0$).

Some remarkable difference between the two periods emerges. Starting with the *T0* vs *T1* case, in the first period (2008-2014) a limited number of IATT is negative (16%), thus revealing anti-environment non-monetary motivations and, on average, this TE amounts to about 9% of the farm net income. On the contrary, most of IATU are positive (84%) but its average incidence on net income remains the same (9%). Therefore, the presence of positive, that is pro-environment, non-monetary motivations is quite generalized among those units but its magnitude seems quite marginal with respect to the farm's net income.

The *T0* vs *T2* case presents a similar behaviour though with opposite interpretation. Only one fourth of treated units present a negative IATU, thus revealing pro-environment non-monetary motivations. For all the other *T2* farms, the monetary incentive seems to remain enough strong to choose the treatment. On the contrary, three forth of control units show a positive IATU thus indicating anti-environment non-monetary motivations leading them to remain in the control group. Also in these case, the average incidence of these individual TE is lower than 10% of the respective farm' net income.

Finally, the *T1* vs *T2* case is essentially consistent with that observed the other two treatments. For both IATT and IATU we observe about 50% of farms revealing decisive non-monetary motivations. As in the *T0* vs *T2* case, the interpretation is that about half of treated units show pro-environment motivations leading them to adopt the *T2* regime despite an income loss.

Consistently with the monotonic response condition both the number of units and the magnitude of negative IATT is larger, even though just slightly, compared to the analogous *T0* vs *T2* case. At the same time, about half of units remaining in regime *T1* makes this choice despite the income loss (due to the loss of the respective policy support) for the presence of relevant non-monetary motivations. It remains true, however, that the amount of income loss to be compensated by these motivations remains, on average, quite limited, i.e. lower than 10% of the farm's net income.

Moving to the second period of analysis, the main differences we can appreciate is that the incidence of units showing a negative IATT tends to be higher in both *T0* vs *T1* and *T0* vs *T2* cases, even though the average incidence of these negative values on the farm's net income remains below 10%. As the two treatments move in opposite directions (anti and pro-environment, respectively), this could be interpreted as an evidence of stronger non-monetary motivations to choose treatments that actually imply an income loss. When the monetary implication of the regime choice becomes too large, as in the *T1* vs *T2* case, the number of treated units accepting a negative IATT becomes very small (5%), and also the loss declines, on average, to less than 5% on net income.

Also in the case of the observed IATU some difference with respect to the first period can be observed. In the *T0* vs *T1* and *T0* vs *T2* cases, the number of untreated units showing a positive IATU is a little more than one-third, with a less than 10% average incidence on net income. Therefore, a remarkable reduction in the incidence of these units is observed compared to 2008-2014 suggesting a sort of relative downsizing of the non-monetary motivations (either anti or pro-environment) for units deciding to remain in the control regime. However, the *T1* vs *T2* case behaves differently as the incidence of these IATU in terms of both units and share on net income is much greater. This indicates that only anti-environment (arguably with a major role of attrition effect) motivations may explain why most firms in group *T1* decide to remain in this regime.

The most interesting use of the results presented in Table 7 concerns the calculation of the two policy indicators (*TPS* and *TPE*) expressing the space for rationalizing the policy intervention by either reducing the expenditure or improving the environmental performance. Table 8 reports the consequent calculation of these two indicators across periods and treatments. These calculations have been performed considering only those estimated IATT and IATU that are statistically lower and greater than 0 at the 10% confidence level, respectively. These units range between 40% and 50%, according to the treatment and the period, of the total IATT<0 and IATU>0 units.

In the first period under study, it emerges that the space of policy savings (*TPS*) is lower than the expenditure expansion (*TPE*) required to induce more pro-environment regimes. Considering both *T0* vs *T1* and *T0* vs *T2* cases, the space for savings amounts to less than 8% of total expenditure. On the contrary, the needed extra expenditure is about 30%. A strategy compensating this *TPE* with the respective *TPS* thus seems unfeasible. As could be expected from the results discussed above, this gap significantly reduces in the *T0* vs *T2* case. But the extra expenditure still remains almost double than the potential expenditure savings.

Following the already noticed differences, period 2015-2018 shows a significantly different picture. The *T0* vs *T1* case presents extra expenditure and expenditure savings of similar size, both amounting to a little more than 10% of total expenditure. In this case, funding the extra expenditure with the correspondent savings seems to be a viable solution. Also in the *T0* vs *T2* case the gap between *TPS* and *TPE* is remarkably lower than in previous period but it remains quite large, while the *T1* vs *T2* case is the only for which not only *TPS* exceeds the *TPE*, but the respective gap is the largest observed across the different comparisons. Eventually this suggests that in the more recent period of investigation, there is much more space for a policy rationalization through a better targeting and tailoring of the AEP measures on the specific characteristics of farms.

[Tables 7 and 8 here]

7. Concluding remarks

It is largely agreed that better targeting and tailoring is the key for improving policy design and implementation. This is particularly true for the EU policy making and, even more, for the CAP. The enduring sequence of CAP reforms has been explicitly oriented towards and increasing rationalization of the intervention and of the support in order to make it more efficient and effective with respect to the declared objectives. But better policy targeting and tailoring must acknowledge that potential beneficiaries, i.e. farms, are strongly heterogenous also with respect to their response to different policy regimes. This seems particularly true in the case of CAP measures that directly or indirectly targeted to some environmental standard or performance, and for which farms response may significantly depend on unobservable non-monetary motivations.

Grounding this kind of policy on the empirical evidence thus requires the identification and the estimation of the individual response to policies. TE or policy evaluation econometrics can be regarded as a mature and well established field of study, but the proper estimation and inference of ITE is a quite novel topic as only very recently ML approaches proved to be very helpful in this respect. This paper presented a CF approach to the identification and estimation of ITE of the different CAP AEP regimes across the 2008-2018 period.

Results here obtained suggest that the adopted approach seems particularly suitable to estimate ITE and to distinguish between ITT and ITU estimates. These latter are particularly informative as they can reveal the direction and magnitude of those non-monetary motivations eventually leading farmers to the adopted policy regime. In addition, this individual information can be aggregate in order to compute policy indicators expressing the potential rationalization of the policy support, both in terms of possible expenditure saving and in terms of additional needed expenditure to improve the overall environmental outcome. It also emerges that these non-monetary motivations usually do show a limited incidence with respect to farm's net income. At the same time, however, some major change apparently occurred in the relative importance of these non-monetary motivations across the 2013 CAP reform, and may leave more space to the abovementioned forms of policy rationalization.

All these conclusions must be evidently taken with caution and deserve confirmation and validation in further analogous studies in the field. In particular, even though the main objective of the present paper is to illustrate the potentials of CF estimation, it should be also clear that these quasi-experimental approaches are always dependent on assumptions about the data generating processes that can be hardly tested and require further investigation and justification on both the theoretical and empirical grounds (Sims, 2010). The ML toolkit is powerful and informative but can also be dangerous in generating unjustified and weak extrapolations, and asks for a careful robustness assessment and validation (Coderoni et al., 2021). Refinements in data availability, quasi-experimental design and ML approaches, as well as analogous applications in non-farming contexts, are therefore expected in the future research in this field.

REFERENCES

Andrews, A.C., Clawson, R.A., Gramig, B.M., Raymond, L. (2013). Why do farmers adopt conservation tillage? An experimental investigation of framing effects. *Journal of Soil and Water Conservation* 68 (6): 501-511.

Angrist, J.D., Pischke, J-P. (2009). *Mostly Harmless Econometrics. An Empiricists' Companion*. Princeton University Press, Princeton.

Angrist, J.D., Pischke, J-P. (2010). The Credibility Revolution in Empirical Economics: How Better Research Design is Taking the Con Out of Econometrics. *Journal of Economic Perspectives* 24 (2): 3-30.

Athey, S., Imbens, G. W. (2017). The State of Applied Econometrics: Causality and Policy Evaluation. *Journal of Economic Perspectives* 31 (2): 3-32.

Athey, S., Imbens, G. W. (2019). Machine learning methods that economists should know about. *Annual Review of Economics* 11: 685-725.

Athey, S., J. Tibshirani, Wager, S. (2019). Generalized random forests, *Annals of Statistics* 47 (2): 1148-1178.

Athey, S., Wager, S. (2019). Estimating Treatment Effects with Causal Forests: An Application. *Observational Studies* 5.

Athey, S. ,Wager, S., Hadad, V., Klosin, S., Muhelbach, N., Nie, X., Schaelling, M. (2020). Estimation of Heterogenous Treatment Effect. Notes prepared for "Machine Learning and Causal Inference" class (https://gsbdbi.github.io/ml_tutorial/hte_tutorial/hte_tutorial.html). Downloaded on April 1, 2021.

Babcock, B.A. (2015). Using Cumulative Prospect Theory to Explain Anomalous Crop Insurance Coverage Choice. *American Journal of Agricultural Economics* 97 (5): 1371-1384.

Barreiro-Hurle, J., Bogonos, M., Himics, M., Hristov, J., Pérez-Domiguez, I., Sahoo, A., Salputra, G., Weiss, F., Baldoni, E., Elleby, C. (2021). Modelling environmental and climate ambition in the agricultural sector with the CAPRI model. Exploring the potential effects of selected Farm to Fork and Biodiversity strategies targets in the framework of the 2030 Climate targets and the post 2020 Common Agricultural Policy. EUR 30317 EN, Publications Office of the European Union, Luxembourg.

Beedell, J. and Rehman, T. (2000). Using social-psychology models to understand farmers' conservation behavior. *Journal of Rural Studies* 16(1): 117–127.

Cerulli, G. (2020). Optimal Policy Learning: From Theory to Practice. Papers 2011.04993, arXiv.org. Downloaded on April 1, 2021.

Coderoni, S., Esposti, R. (2018). CAP payments and agricultural GHG emissions in Italy. A farm-level assessment. *Science of the Total Environment* 627: 427–437.

Coderoni, S., Esposti, R., Varacca, A. (2021). How differently do farms respond to agro-environmental policies? A Machine-Learning approach. Department of Agricultural and Food Economics, Università Cattolica del Sacro Cuore (Piacenza, Italy), unpublished manuscripts.

Esposti, R. (2017a). The empirics of decoupling: Alternative estimation approaches of the farm-level production response. *European Review of Agricultural Economics* 44 (3): 499–537.

Esposti, R. (2017b). The heterogeneous farm-level impact of the 2005 CAP-first pillar reform: A multivalued treatment effect estimation. *Agricultural Economics* 48 (3): 373–386.

Esposti, R. (2021). Policy Learning in Agro-Environmental Policies. Conceptual Issues and a Methodological Proposal. Department of Economics and Social Sciences, Università Politecnica delle Marche – Ancona (Italy), unpublished manuscripts.

Greiner, R. (2015). Motivations and attitudes influence farmers' willingness to participate in biodiversity conservation contracts. *Agricultural Systems* 137: 1–12.

Guerrero, S. (2021). Characterising agri-environmental policies: Towards measuring their progress. OECD Food, Agriculture and Fisheries Papers, No. 155, OECD Publishing, Paris.

Harrington, W., Morgenstern, R.D. (2007). Economic Incentives Versus Command and Control: What's the Best Approach for Solving Environmental Problems?. In: Visgilio G.R., Whitelaw D.M. (eds), *Acid in the Environment*. Springer, Boston, 233-240.

Imbens, G.W. (2010). Better LATE Than Nothing: Some Comments on Deaton (2009) and Heckman and Urzua (2009). *Journal of Economic Literature* 48 (2): 399-423.

Imbens, G.W. (2020). Potential Outcome and Directed Acyclic Graph Approaches to Causality: Relevance for Empirical Practice in Economics. *Journal of Economic Literature* 55 (4): 1129-1179.

Imbens, G.W., Rubin, D.B. (2015). *Causal inference in statistics, social, and biomedical sciences*. Cambridge University Press.

Kling, C.L., Phaneuf, D.J., Zhao, J. (2012). From Exxon to BP: Has Some Number Become Better than No Number? *Journal of Economic Perspectives* 26(4): 3-26.

Knaus, M.C., Lechner, M., Strittmatter, A. (2021). Machine learning estimation of heterogeneous causal effects: Empirical Monte Carlo evidence. *The Econometrics Journal* 24 (1): 134–161.

Lu, M., Sadiq, S., Feaster, D.J., Ishwaran, H. (2018) Estimating Individual Treatment Effect in Observational Data Using Random Forest Methods. *Journal of Computational and Graphical Statistics* 27 (1): 209–219.

Matthews, A. (2011). Leaked legislative proposals anticipate Commission CAP reform proposals due October 12th. 14 September, http://capreform.eu/leaked-legislative-proposals-anticipate-commission-cap-reform-proposals-due-october-12th/. Downloaded on August 1, 2021.

Mullainathan, S, Spiess, J. (*2017).* Machine Learning: An Applied Econometric Approach. *Journal of Economic Perspectives* 31 (2): 87-106.

OECD (2012). *Evaluation of Agri-environmental Policies: Selected Methodological Issues and Case Studies*. OECD Publishing, Paris.

Sauppe, J.J., Jacobson, S.H. (2017). The Role of Covariate Balance in Observational Studies. *Naval Research Logistics* 64: 323-344.

Sims, C. (2010). But Economics is not and Experimental Science. *Journal of Economic Perspectives* 24 (2): 59-68.

Sterba, S.K. (2009). Alternative Model-Based and Design-Based Frameworks for Inference from Samples to Population. From Polarization to Integration. *Multivariate Behavioural Research* 44 (6): 711-740.

Thomas, F., Midler, E., Lefebvre, M., Engel, S. (2019). Greening the common agricultural policy: a behavioural perspective and lab-in-the-field experiment in Germany. *European Review of Agricultural Economics* 46 (3): 367–392.

Tibshirani, J., Athey, S., Friedberg, R., Hadad, V., Miner, L., Wager, S., Wright, M. (2018). grf: Generalized Random Forests (Beta). URL https://github.com/grf-labs/grf. R package version 0.10.2.

Vollan, B. (2008). Socio-ecological explanations for crowding-out effects from economic field experiments in southern Africa. *Ecological Economics* 67(4): 560–573.

Wang, A., Nianogo, R.A., Arah, O.A. (2017). G-computation of average treatment effects on the treated and the untreated. *BMC Medical Research Methodology* 17(3): 1-5.

Table 1 – Policy regimes in the two balanced panel samples.

Policy regime	*Active AEPs*	*Reference period*
P0	*AEM*	Both
P1	*None*	Both
P2	*(DP+CC)*	2008-2014
P3	*(DP+CC)+AEM*	2008-2014
P4	*(DP+CC)+G*	2015-2018
P5	*(DP+CC)+G+AEM*	2015-2018

Table 2 – Total number of farms by treatment.

Treatment group	*2008-2014 balanced panel*		*2015-2018 balanced panel*	
	Policy regime	*N of farms*	*Policy regime*	*N of farms*
T0 (control)	**P2**	1811	**P4**	3676
T1	**P1**	843	**P1**	1217
T2	**P3**	848	**P5**	956
Total N of farms		3502		5849
Total obs.		24514		23396

Table 3 – Policy treatment sets across the two period and interpretation of the treatment effect ($\Delta\pi_i$) across the treatment comparisons.

<table>
<tr><td colspan="2" rowspan="2"></td><td rowspan="2">AEPs</td><td colspan="3">Interpretation of $\Delta\pi_{it}$ in treatment comparisons</td></tr>
<tr><td>T0 vs T2</td><td>T0 vs T1</td><td>T1 vs T2</td></tr>
<tr><td rowspan="3">Treatment groups</td><td>T0 (control)</td><td>Only those implied by first pillar DP, G included.
(P2 or P4)</td><td>Anti-environment (Control aversity and Attrition)</td><td>Pro-environment (Warm-glow effect/Green-guilt aversion and Loss aversity)</td><td>-</td></tr>
<tr><td>T1</td><td>None
(P1)</td><td>-</td><td>Anti-environment (Control aversity and Attrition)</td><td>Anti-environment (Control aversity and Attrition)</td></tr>
<tr><td>T2</td><td>Those implied by both first pillar DP and second pillar AEM.
(P3 or P5)</td><td>Pro-environment (Warm-glow effect/Green-guilt aversion and Loss aversity)</td><td>-</td><td>Pro-environment (Warm-glow effect/Green-guilt aversion and Loss aversity)</td></tr>
</table>

Table 4 – Descriptive statistics of outcome variables and covariates.

	2008-2014 sample		2015-2018 sample	
	Mean	St. dev.	Mean	St. dev.
Net Income per unit of family labour (Euros)	57361	44889	52023	78114
Machinery (00 KWh) per unit of GPV (Euros)	0.315	0.307	0.280	0.298
Labour units (00) per unit of GPV	0.003	0.002	0.003	0.002
Utilised agricultural area (00 ha) per unit of GPV	0.040	0.042	0.038	0,040
Livestock units (00) per unit of GPV	0.018	0.019	0.016	0.019
Chemical expenditure per unit of GPV (%)	21.6	30.2	12.7	12.8
Energy expenditure per unit of GPV (%)	17.1	28.7	7.86	6.74
Altitude (meters)	298	304	295	305
Latitude (degrees)	43.0	2.58	42.9	2.59
Longitude (degrees)	11.5	2.95	11.5	2.95
Average Age (years)	55.6	9.93	54.3	11.0
Forest area on utilised agricultural area (%)	4.9	14.8	4.45	12.8
Size (class)	4.72	1.61	4.80	1.56
Farmer gender (% males)	75		78	
Farm type:				
% Livestock	28		27	
% Livestock&crops	6		5	
% Annual crops	30		32	
% Perennial crops	27		28	
% Others	9		8	

Table 5 – ATE, ATT and ATU estimates for the two balanced panel samples.

	2008-2014								
	ATE	st. error	Obs	ATT	st. error	Obs	ATU	st. error	Obs
T0 vs *T1*	5751.32	4604.17	2654	6317.82	4529.69	843	4505.93*	4767.91	1811
T0 vs *T2*	5013.30*	2354.02	2659	5432.85*	2684.05	848	4770.59*	1900.87	1811
T1 vs *T2*	-1103.72	3815.70	1691	-1488.32	3726.29	848	-716.84	3905.64	843
	2015-2018								
	ATE	st. error	Obs	ATT	st. error	Obs	ATU	st. error	Obs
T0 vs *T1*	1412.99	2726.86	4893	1705.54	2888.84	1217	1316.15	2673.23	3676
T0 vs *T2*	-1726.61	2845.85	4633	-264.06	2944.61	956	-2051.91	2821.19	3676
T1 vs *T2*	7750.01*	2919.08	2174	7779.13*	2748.07	956	7757.11*	3053.55	1217

** Statistically significant at 5% level.*

Table 6 – GATE estimates for selected covariates in the two balanced panel samples.

	2008-2014			**2015-2018**		
	GATE	st. error	Obs	GATE	st. error	Obs
Gender = Female						
T0 vs *T1*	10980.49	9562.95	202	8911.09	10141.64	1106
T0 vs *T2*	7241.80	7697.94	213	-7322.11	8046.56	1204
T1 vs *T2*	-2895.32	14799.5	35	21184.48*	8762.43	471
Age < 40 years						
T0 vs *T1*	11483.72	13827.25	351	1666.59	7570.58	630
T0 vs *T2*	9788.02*	4699.86	380	-8635.03	7041.64	358
T1 vs *T2*	-2563.38	12583.19	125	19801.54*	8275.87	303
Type = Livestock and Livestock&crop						
T0 vs *T1*	10495.48	12241.03	1033	9105.09	10800.98	1731
T0 vs *T2*	10035.91*	4708.06	1217	-7962.91	7909.67	598
T1 vs *T2*	-1047.76	11657.45	344	23200.62*	9127.13	833
Type = Perennial crops						
T0 vs *T1*	10422.00	15051.79	80	-2096.61	6206.81	1492
T0 vs *T2*	10575.20*	4879.95	765	-7899.16	7271.99	586
T1 vs *T2*	-4359.69	12984.77	308	19553.04*	8426.36	570

** Statistically significant at 5% level.*

Table 7 – IATT and IATU estimates implying NM>0 in the two balanced panel samples.

	T0 vs *T1*				*T0* vs *T2*				*T1* vs *T2*			
	N	%	avg. TE	% on π	N	%	avg. TE	% on π	N	%	avg. TE	% on π
2008-2014												
IATT<0	138	16	-5413	8.7	211	25	-6431	8.1	405	48	-5253	9.2
IATU>0	1540	85	4340	9	1340	74	7308	7.6	448	53	3808	6.1
2015-2018												
IATT<0	446	37	-4055	9.0	496	52	-4846	9.8	50	5	-2036	4.1
IATU>0	1324	36	4437	8.8	1397	38	3783	7.6	1077	88	9003	20

Table 8 – Policy assessment indicators (TPS and TPE) in the two periods across treatment comparisons (M €)[a].

	T0 vs *T1*		*T0* vs *T2*		*T1* vs *T2*	
	Level	% total expenditure [b]	Level	% total expenditure [c]	Level	% total expenditure [d]
2008-2014						
TPS	0.47	2.14	0.83	7.74	1.76	16.1
TPE	7.86	31.7	6.48	28.3	3.12	28.7
2015-2018						
TPS	4.90	11.3	1.92	12.6	5.05	33.2
TPE	6.96	11.9	8.28	21.1	0.53	3.46

[a] *TPE and TPS are calculated by multiplying the estimated IATT or IATU by the respective family labour units.*
[b] *The % on total expenditure refers to the control group expenditure.*
[c] *The % on total expenditure refers to the control (treatment) group expenditure in the case of TPE (TPS) calculation.*
[d] *The % on total expenditure refers to the treatment group expenditure.*

Figure 1 – Distribution of farms across policy regimes within the 2008-2018 unbalanced Italian FADN sample (see Table 1 for the policy regime legend).

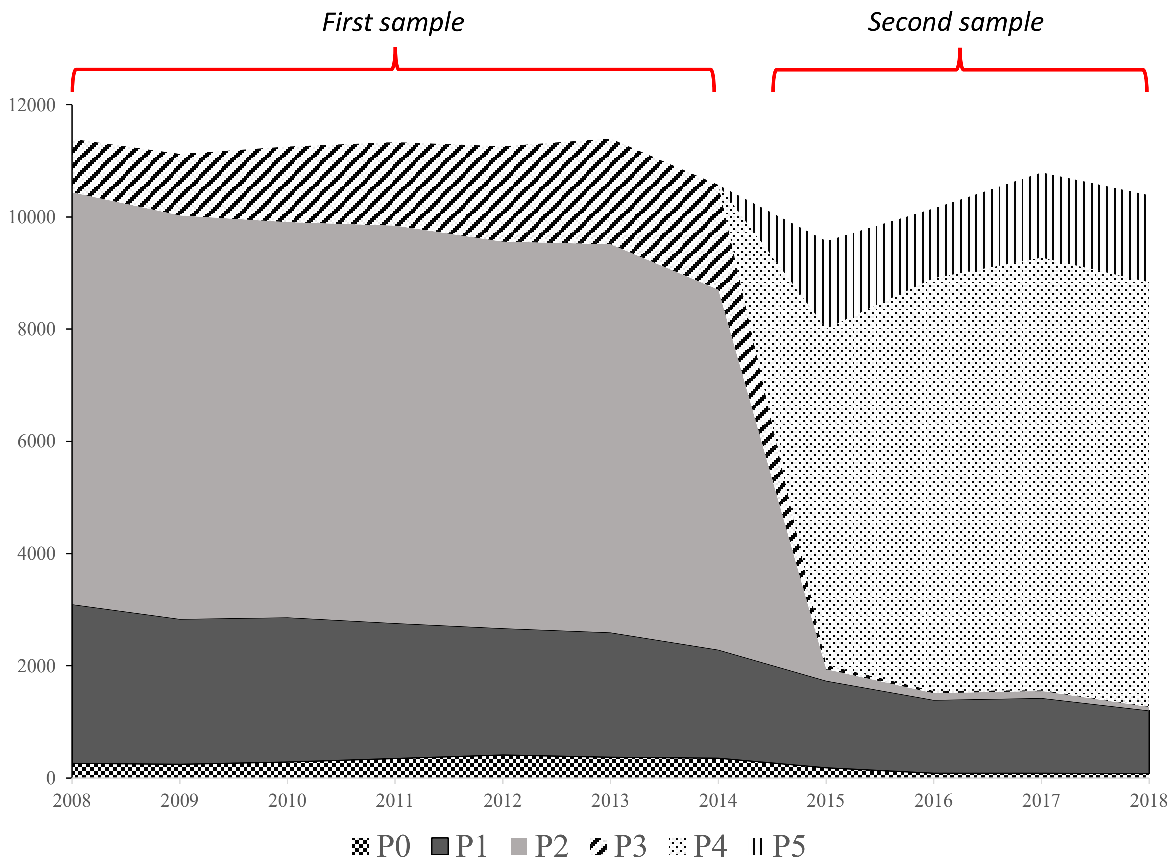


Figure 2.I – IATE estimate of T0 vs T1 comparison: a) all units; b) units with IATE < 0; c) units with IATE > 0 (2008-2014).

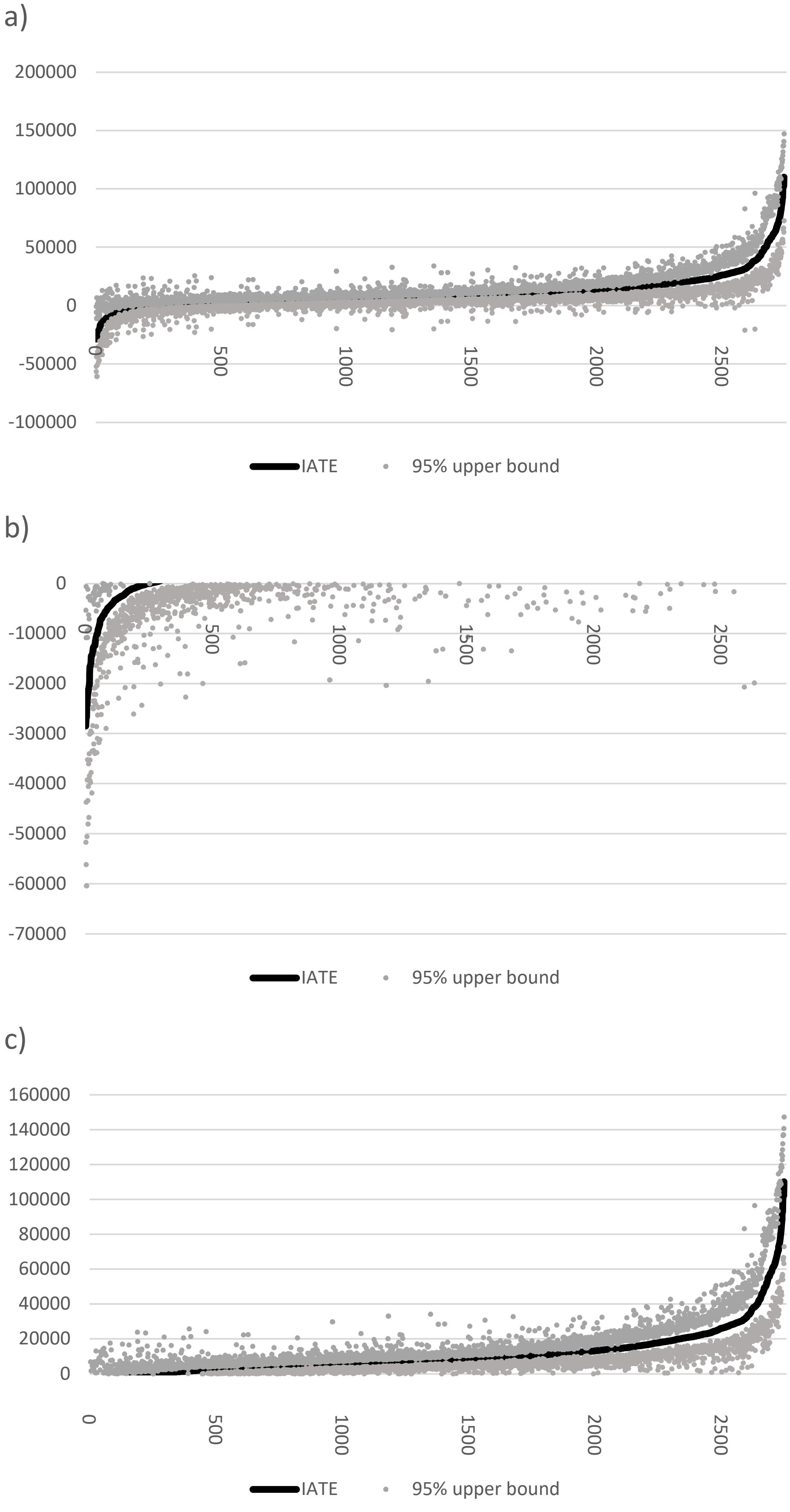


Figure 2.II – IATT estimate of T0 vs T1 comparison: a) all units; b) units with IATT < 0; c) units with IATT > 0 (2008-2014).

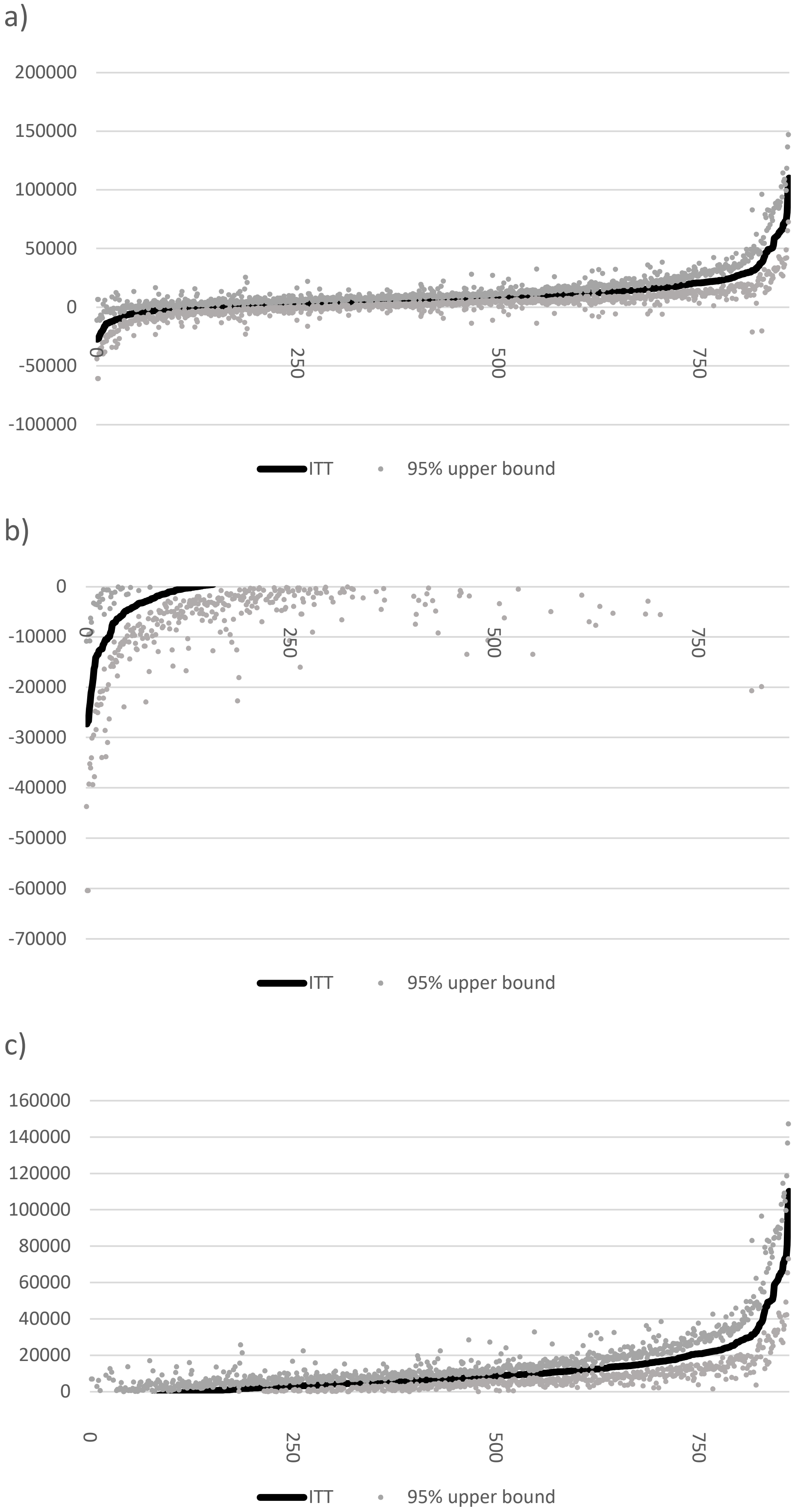


Figure 2.III – IATU estimate of T0 vs T1 comparison: a) all units; b) units with IATU < 0; c) units with IATU > 0 (2008-2014).

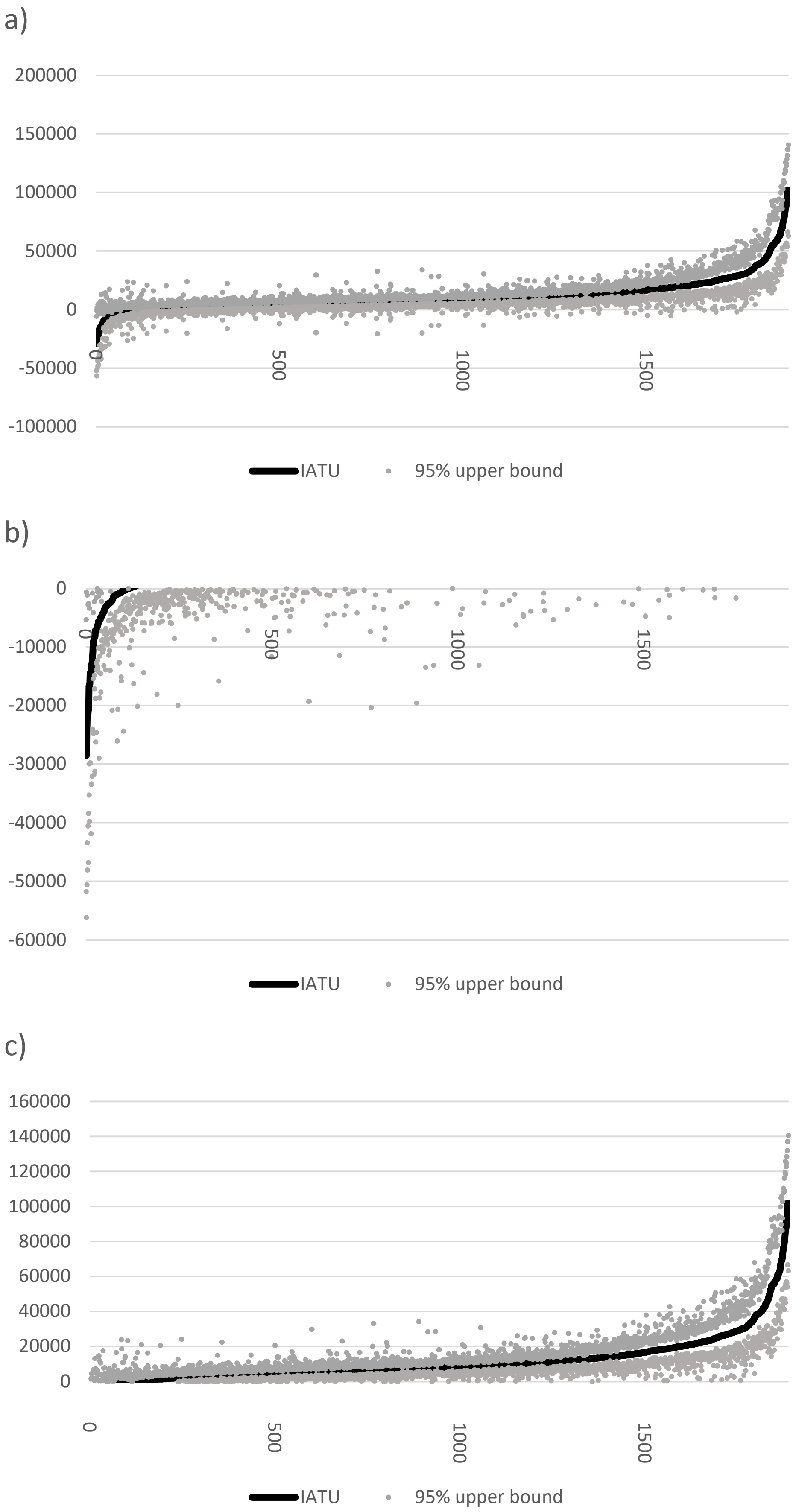
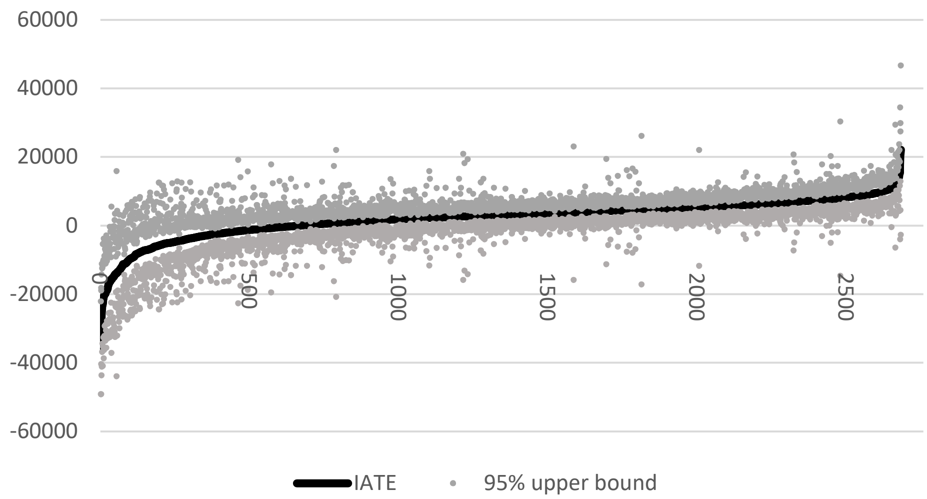
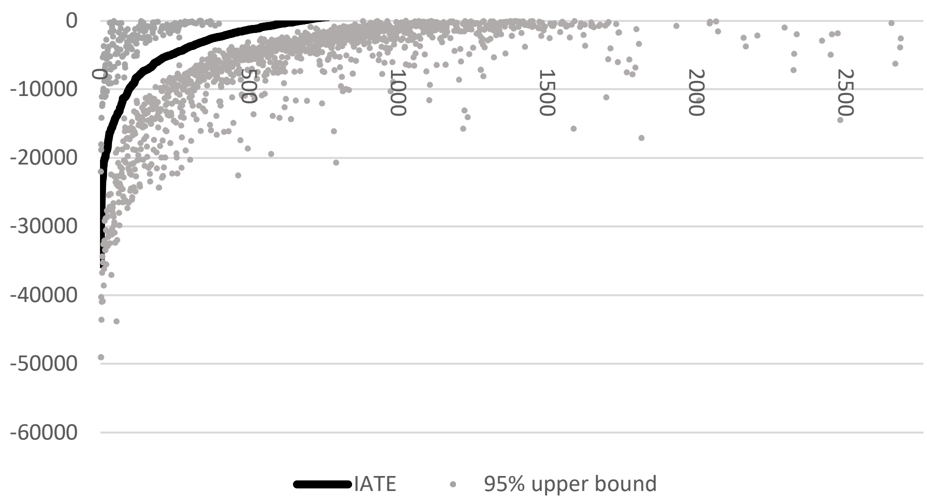


Figure 3.I – IATE estimate of T0 vs T2 comparison: a) all units; b) units with IATE < 0; c) units with IATE > 0 (2008-2014).

a)



b)



c)

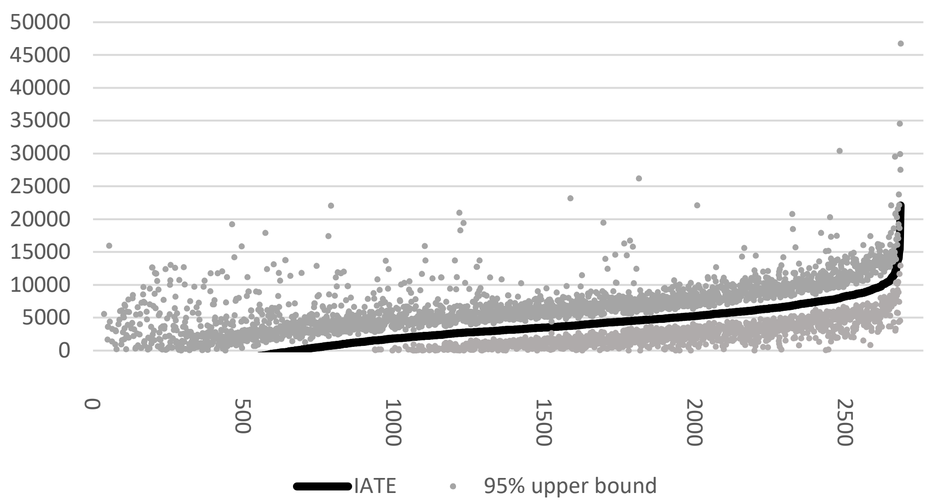
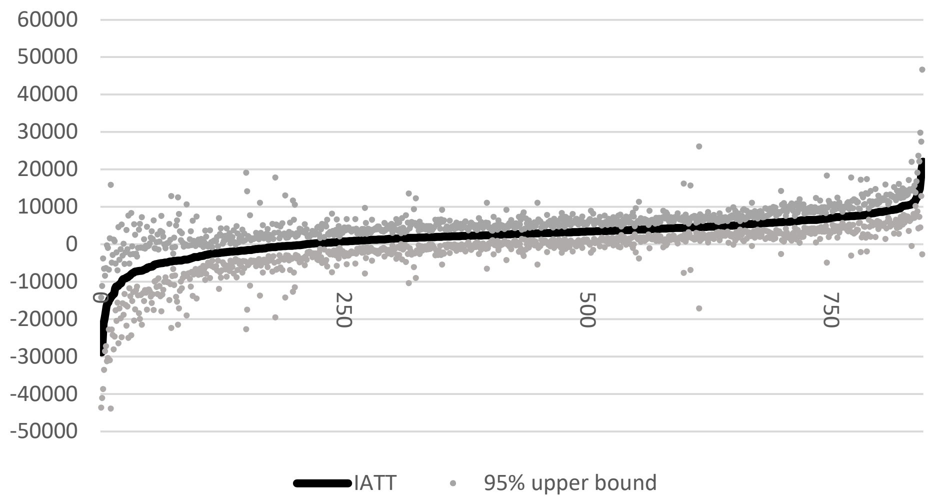
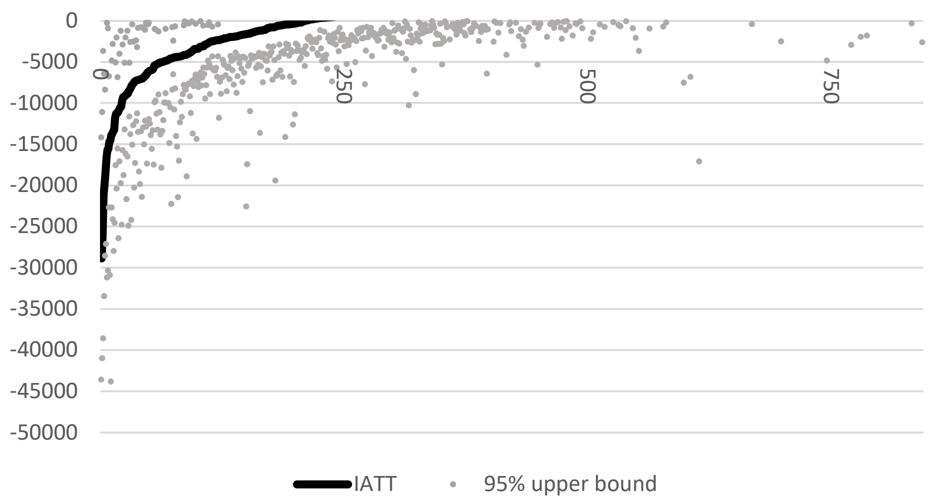


Figure 3.II – IATT estimate of T0 vs T2 comparison: a) all units; b) units with IATT < 0; c) units with IATT > 0 (2008-2014).

a)



b)



c)

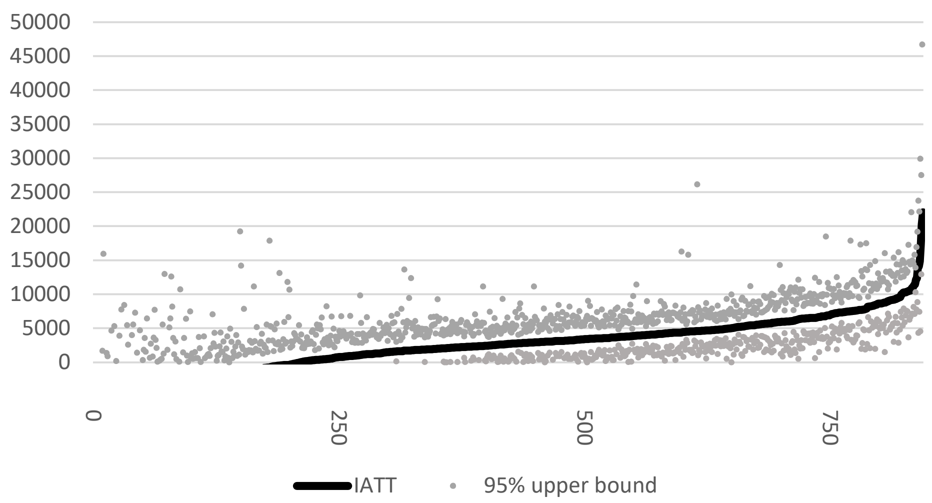
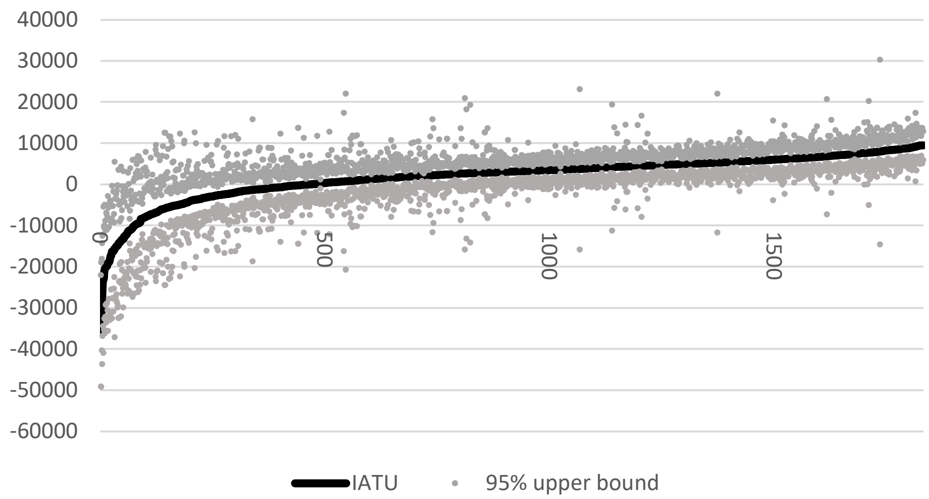
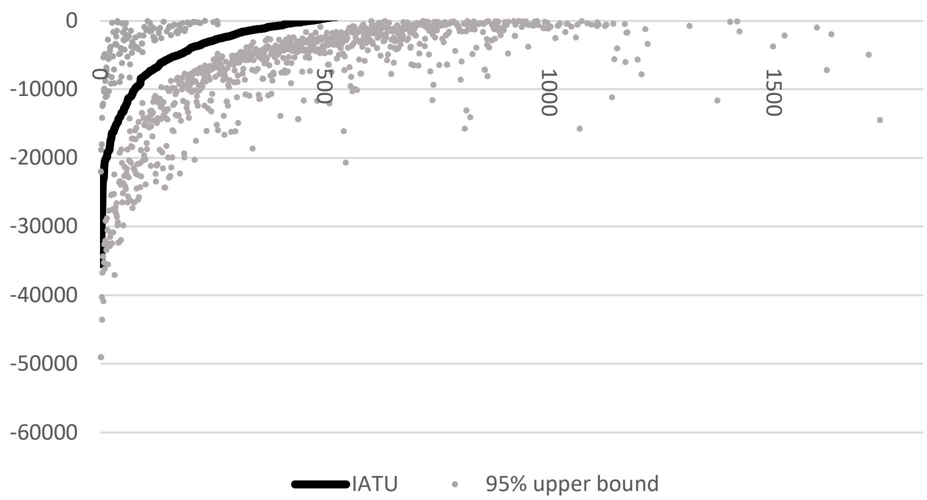


Figure 3.III – IATU estimate of T0 vs T2 comparison: a) all units; b) units with IATU < 0; c) units with IATU > 0 (2008-2014).

a)



b)



c)

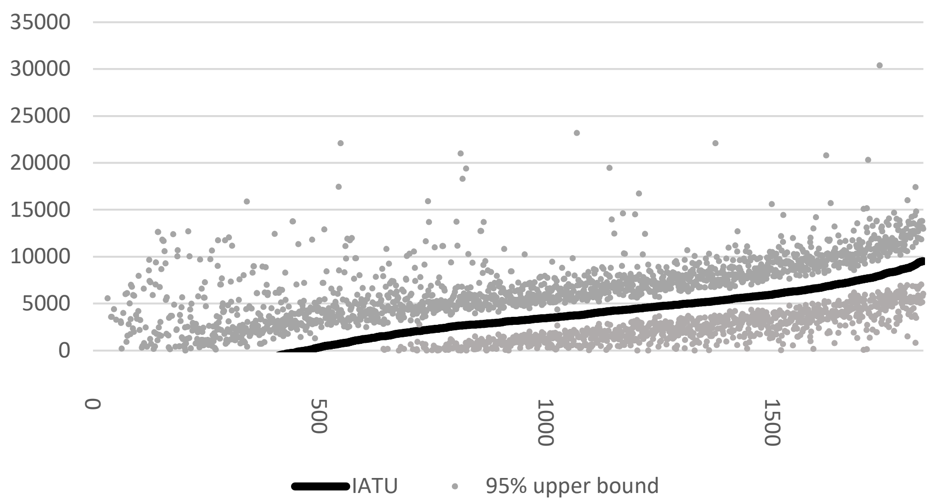


Figure 4.I – IATE estimate of T1 vs T2 comparison: a) all units; b) units with IATE < 0; c) units with IATE > 0 (2008-2014).

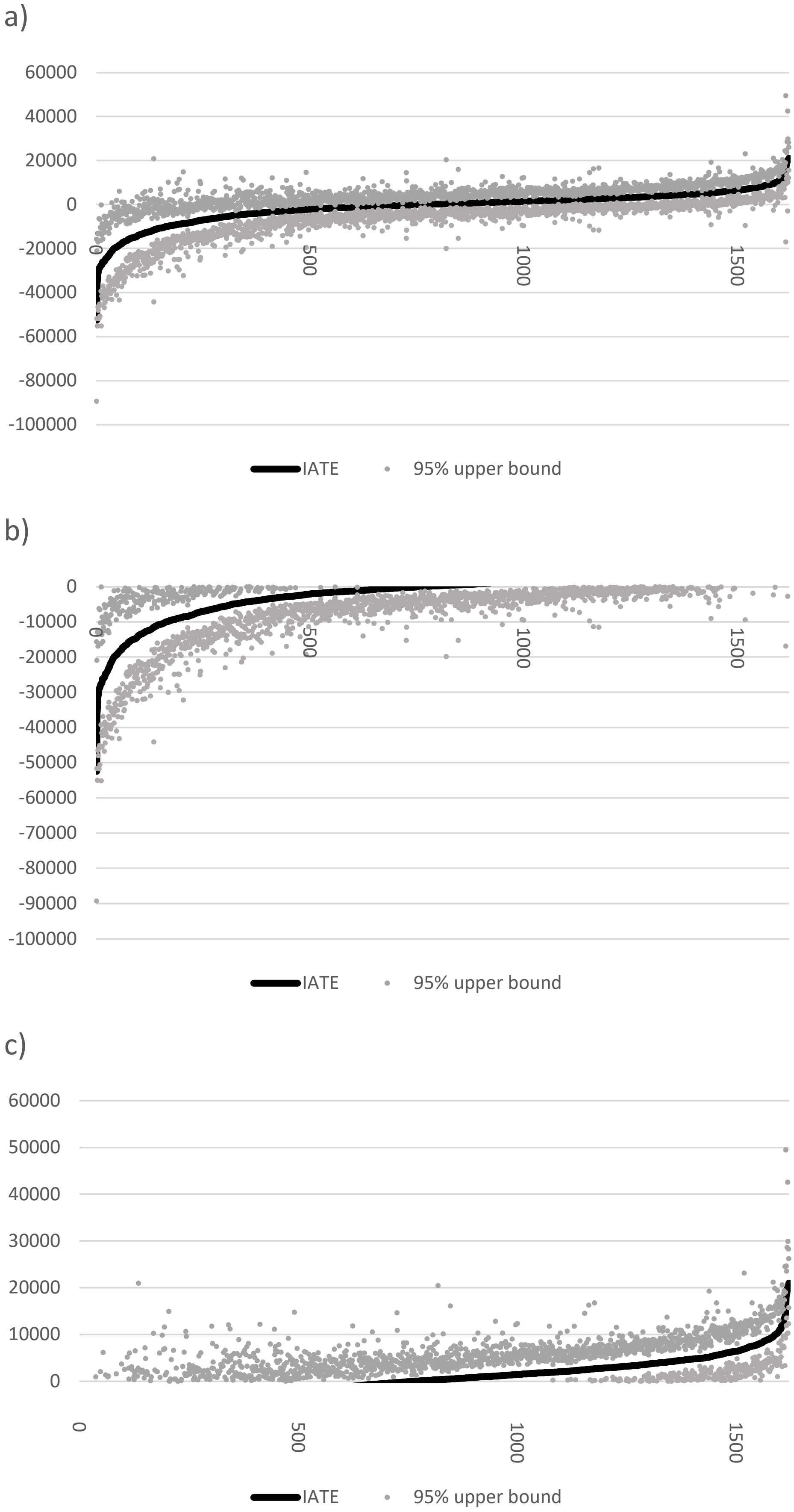


Figure 4.II – IATT estimate of T1 vs T2 comparison: a) all units; b) units with IATT < 0; c) units with IATT > 0 (2008-2014).

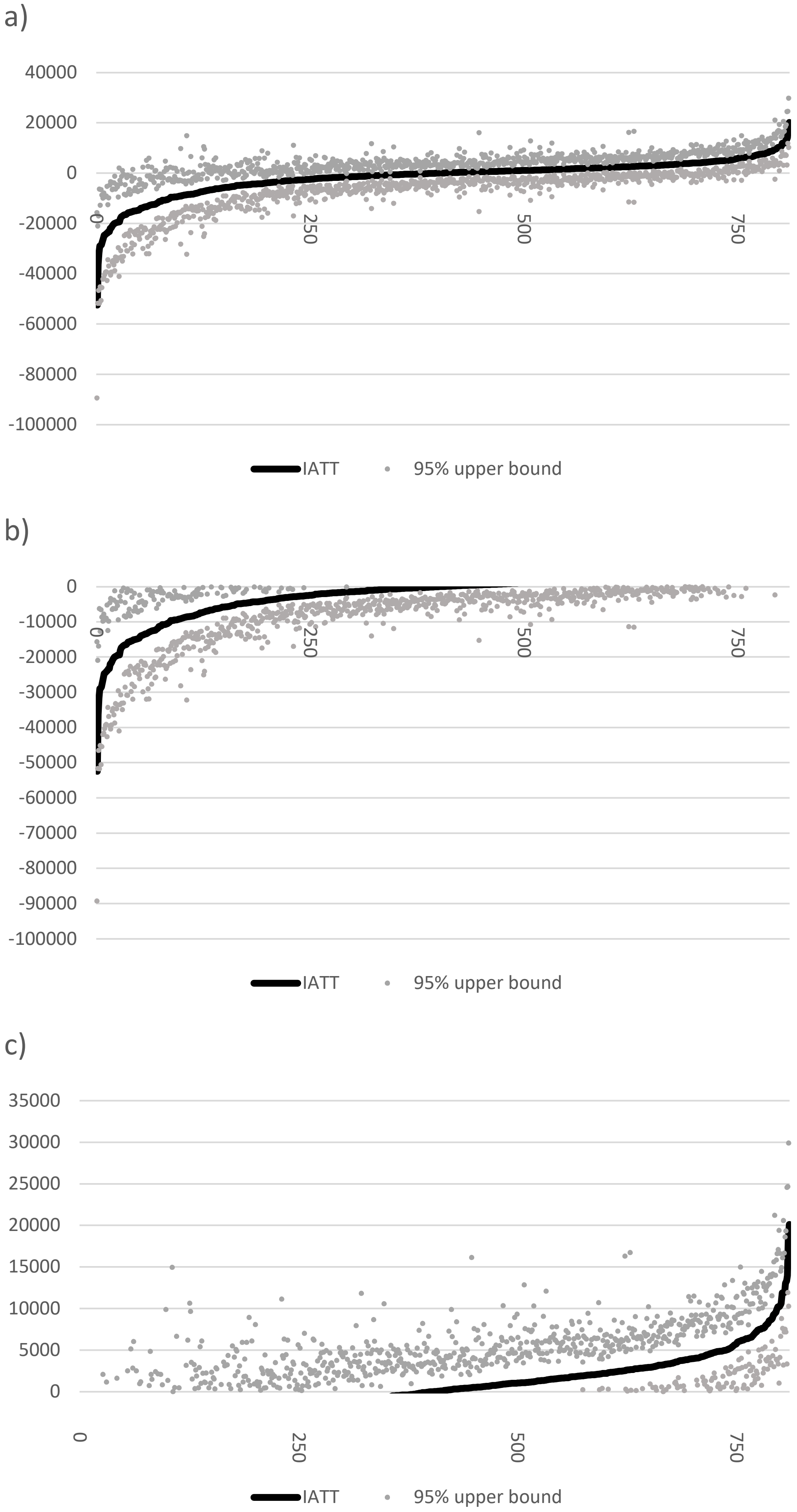
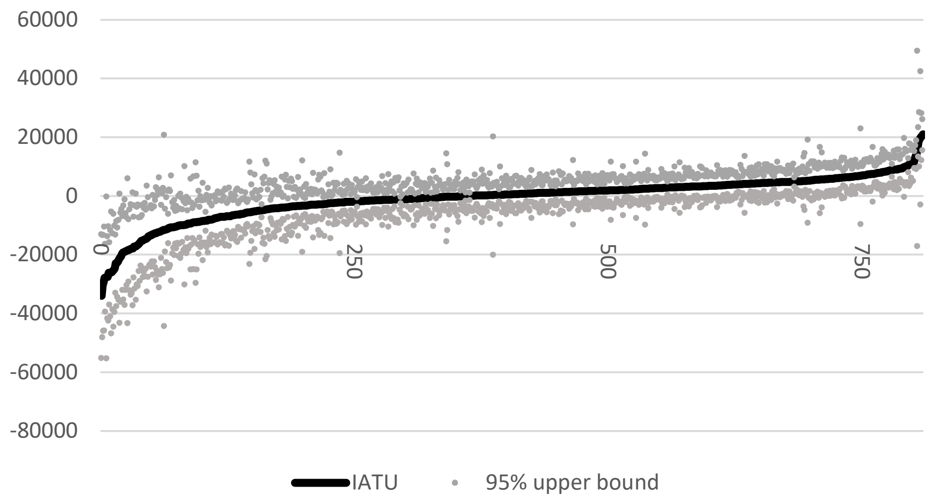
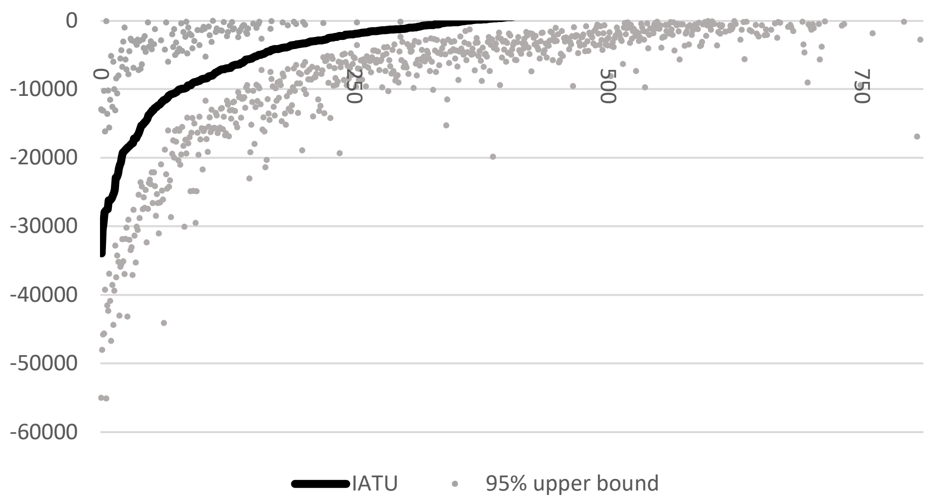


Figure 4.III – IATU estimate of T1 vs T2 comparison: a) all units; b) units with IATU < 0; c) units with IATU > 0 (2008-2014).

a)



b)



c)

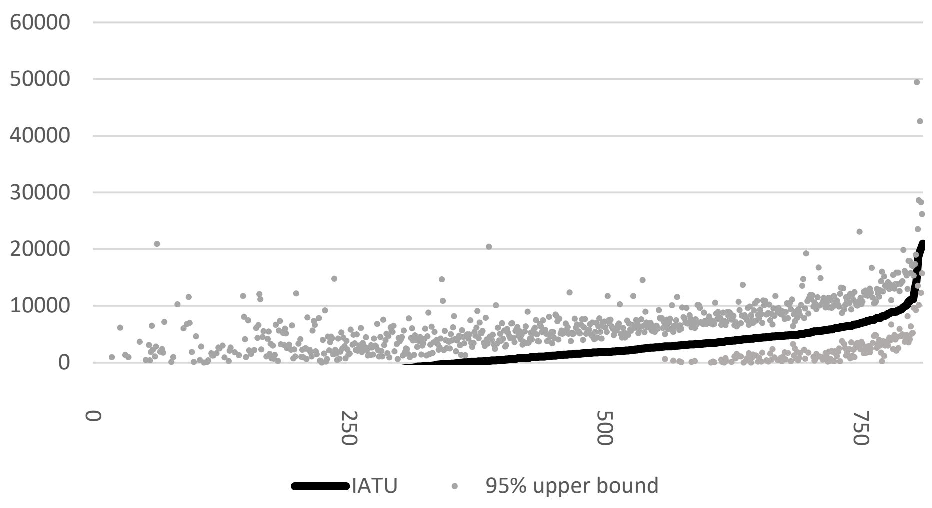
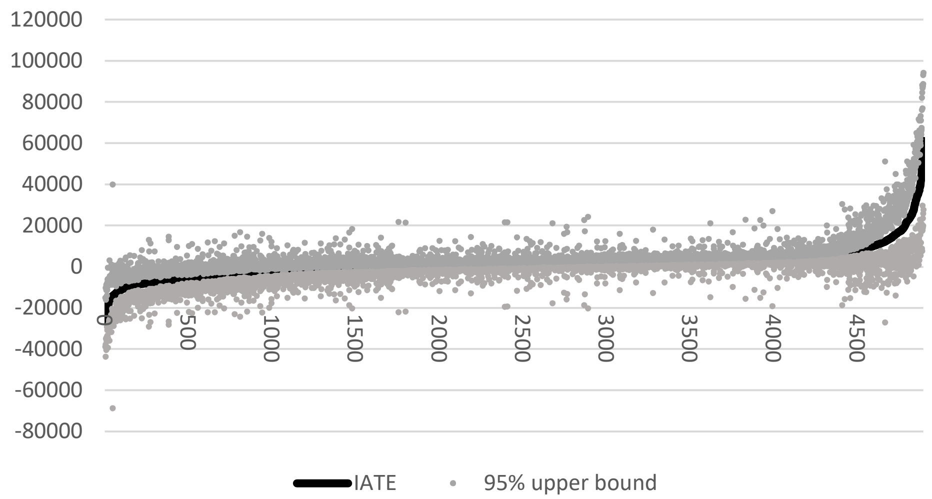
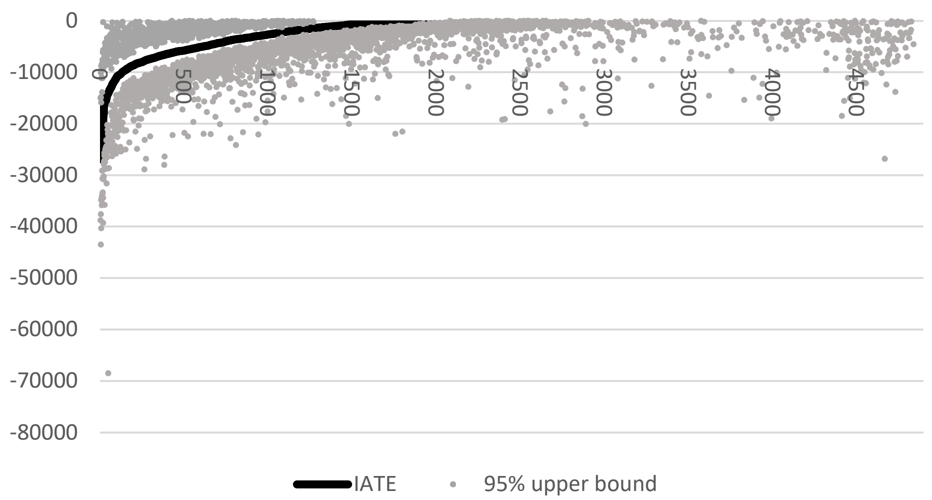


Figure 5.I – IATE estimate of T0 vs T1 comparison: a) all units; b) units with IATE < 0; c) units with IATE > 0 (2015-2018).

a)



b)



c)

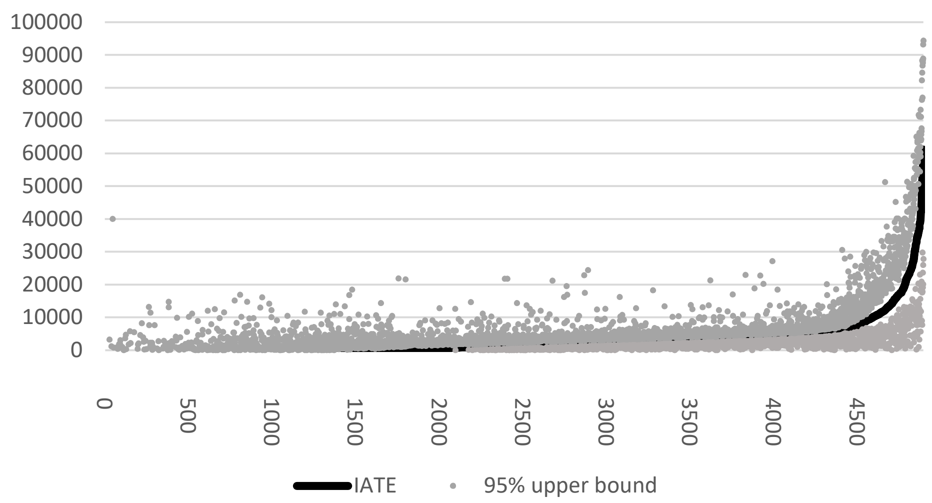


Figure 5.II – IATT estimate of T0 vs T1 comparison: a) all units; b) units with IATT < 0; c) units with IATT > 0 (2015-2018).

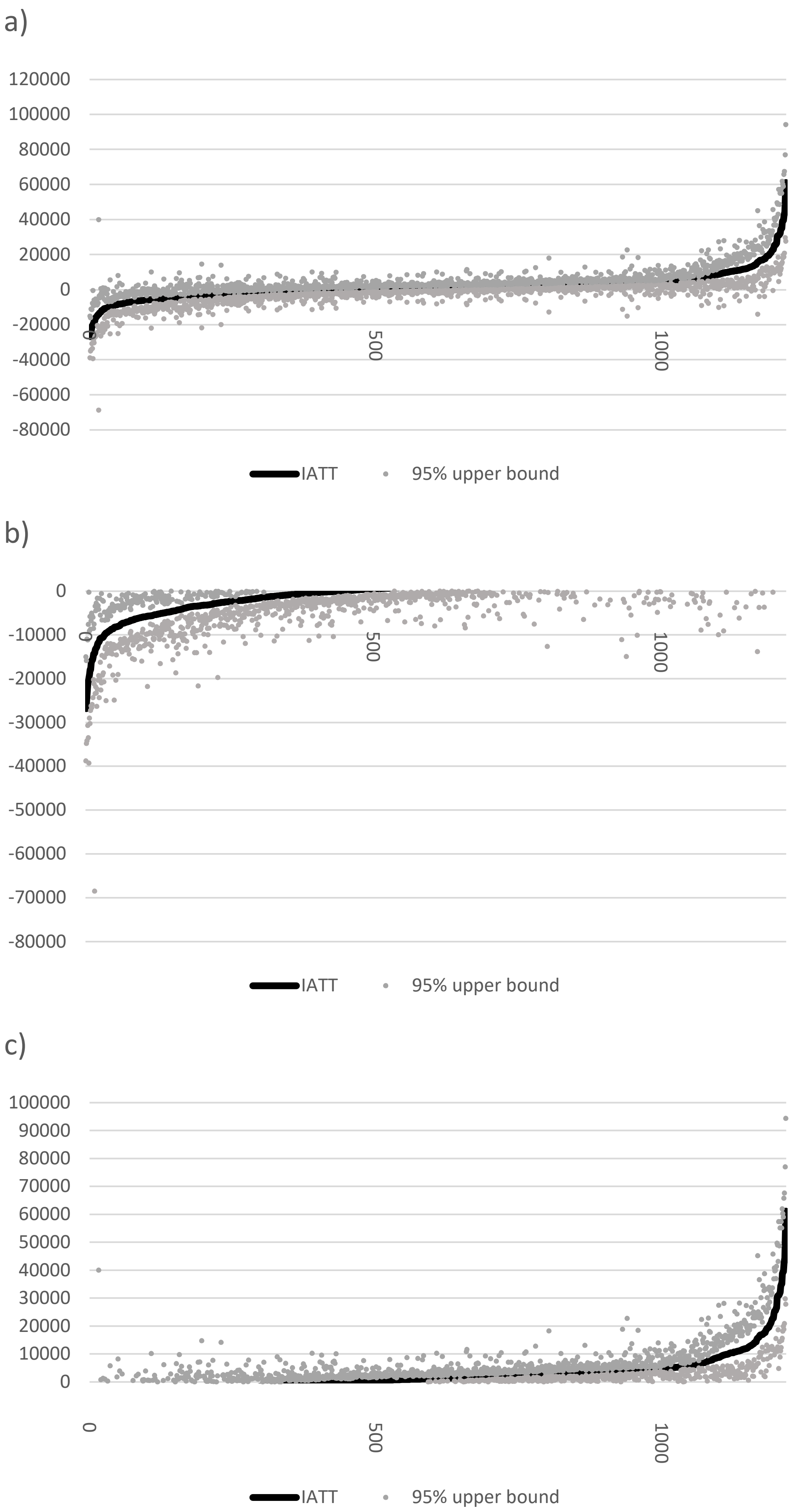


Figure 5.III – IATU estimate of T0 vs T1 comparison: a) all units; b) units with IATU < 0; c) units with IATU > 0 (2015-2018).

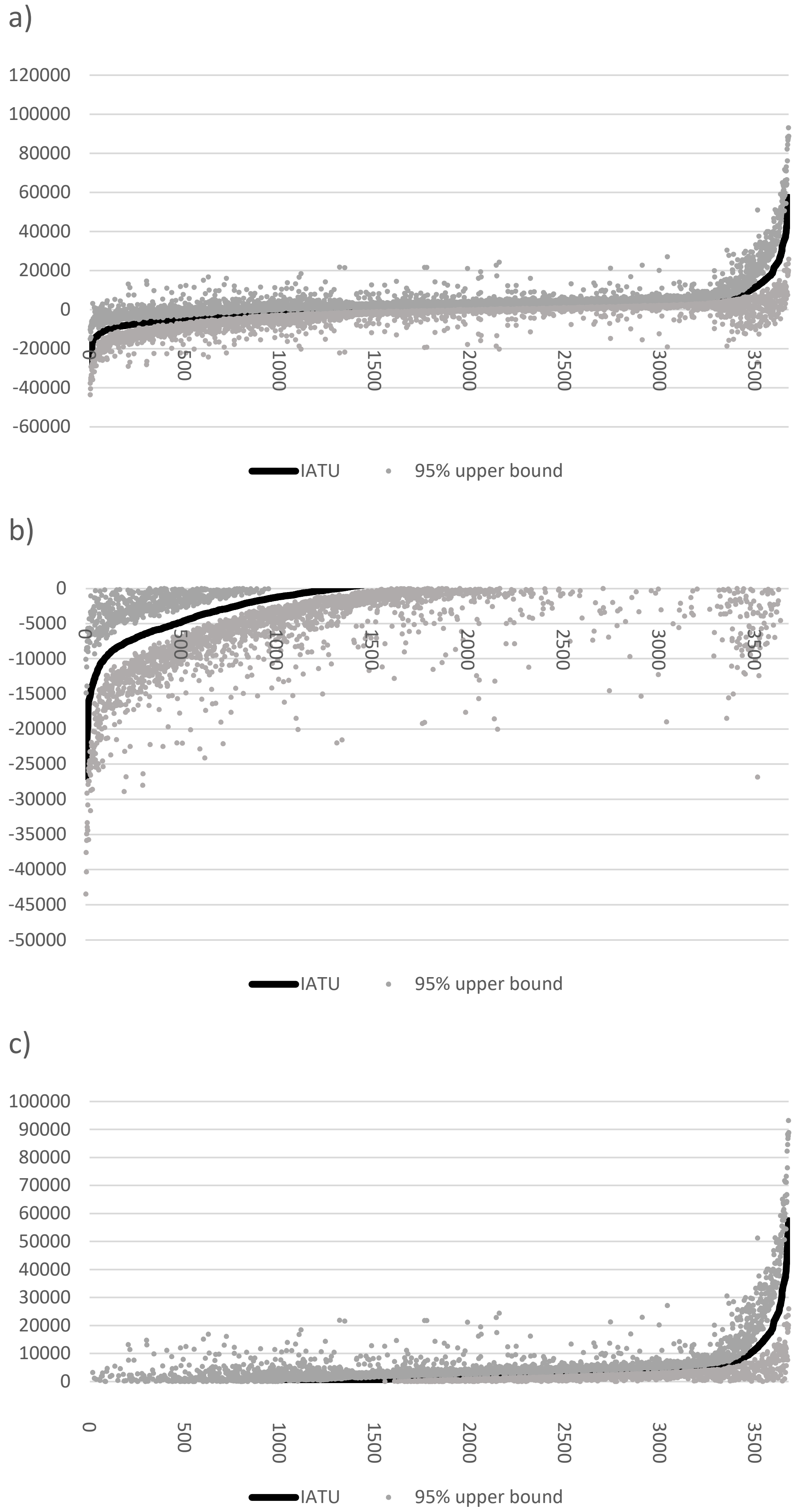


Figure 6.I – IATE estimate of T0 vs T2 comparison: a) all units; b) units with IATE < 0; c) units with IATE > 0 (2015-2018).

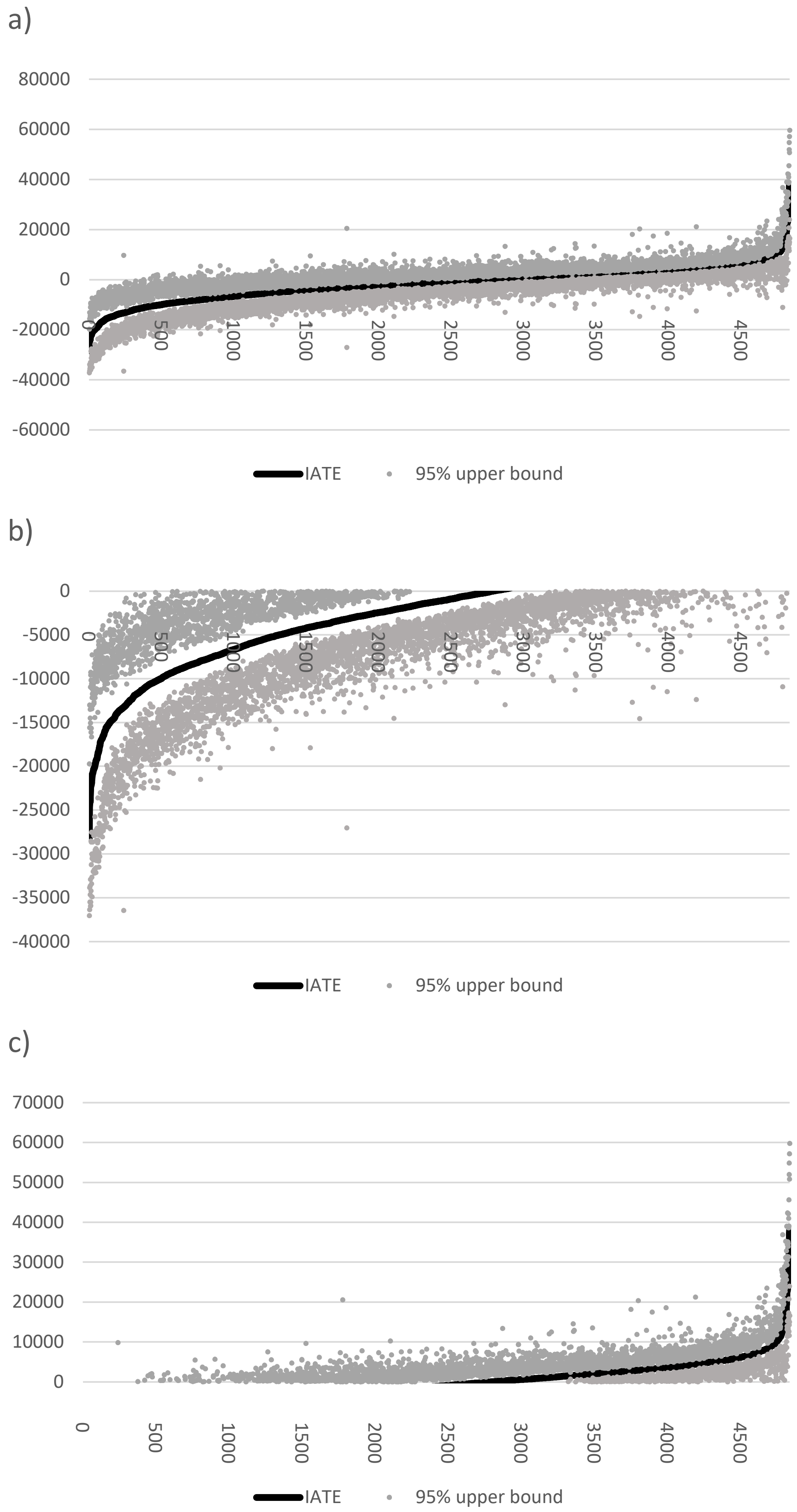


Figure 6.II – IATT estimate of T0 vs T2 comparison: a) all units; b) units with IATT < 0; c) units with IATT > 0 (2015-2018).

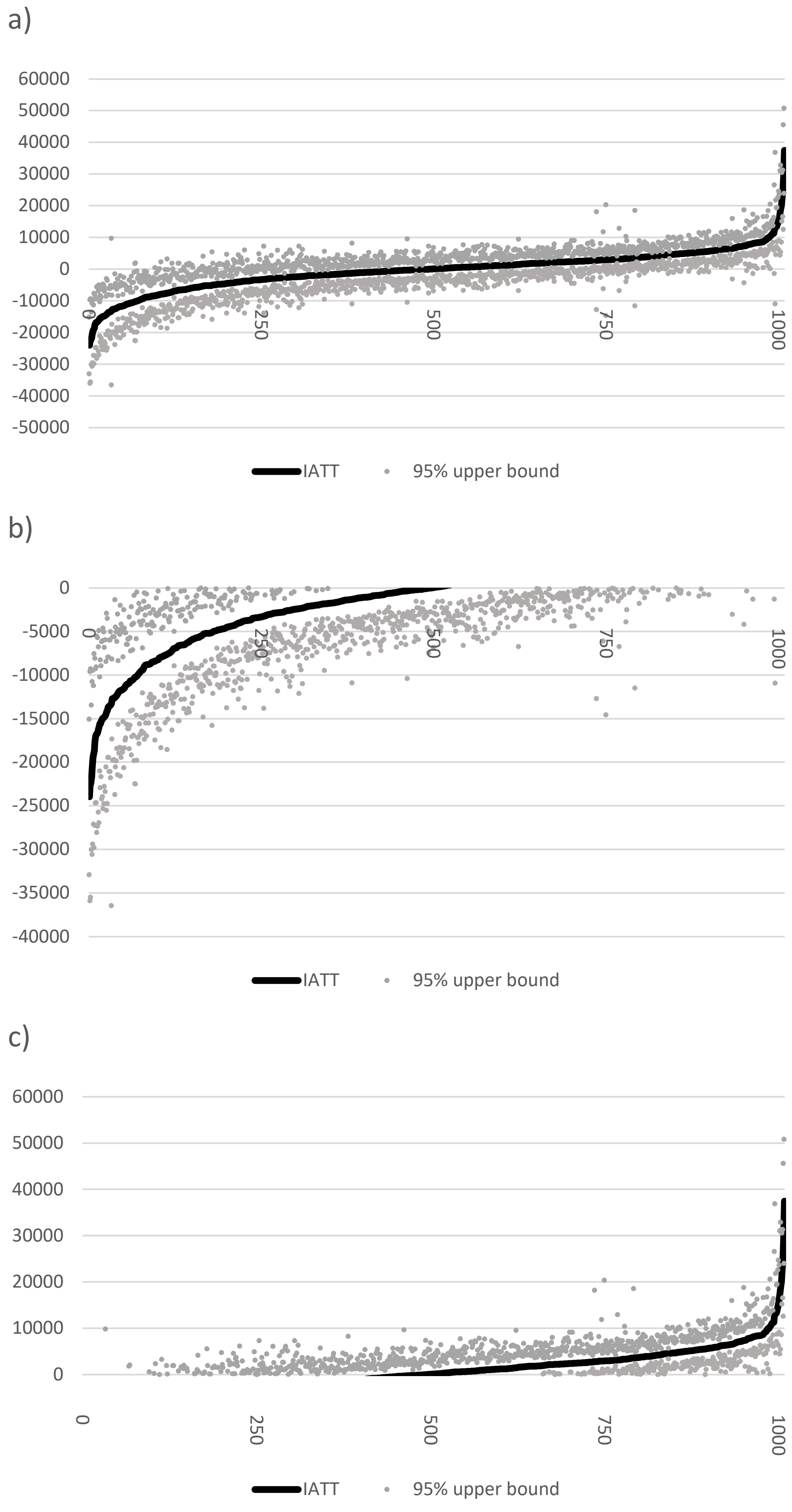


Figure 6.III – IATU estimate of T0 vs T2 comparison: a) all units; b) units with IATU < 0; c) units with IATU > 0 (2015-2018).

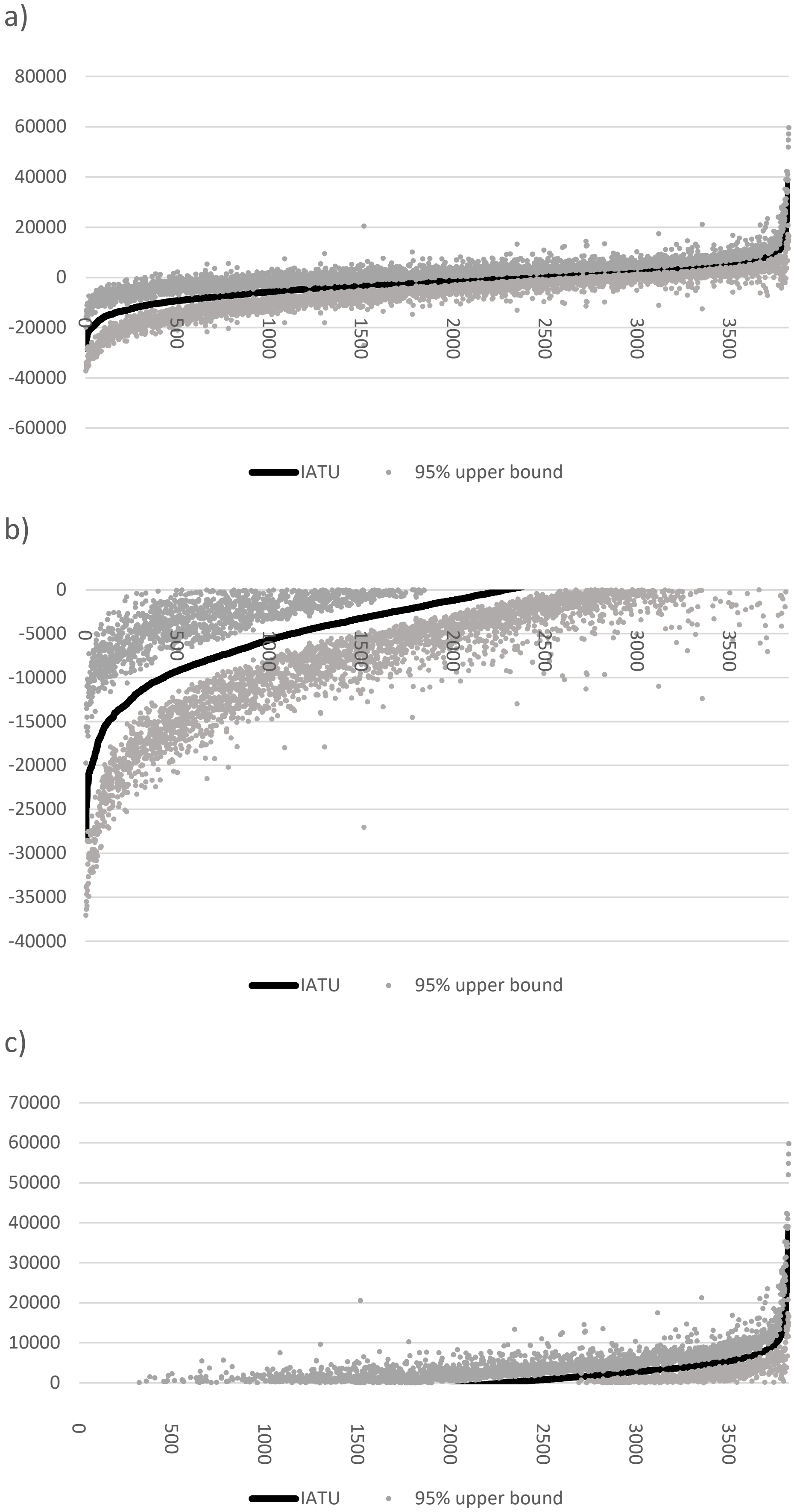
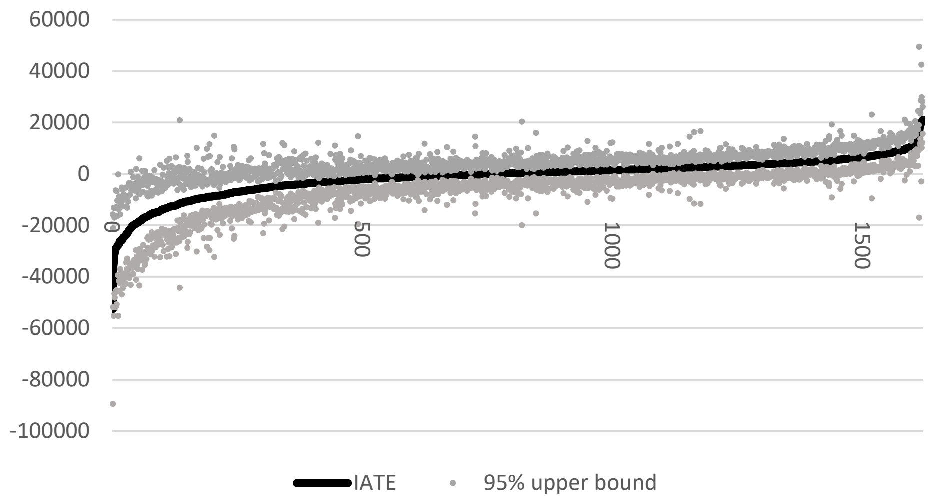
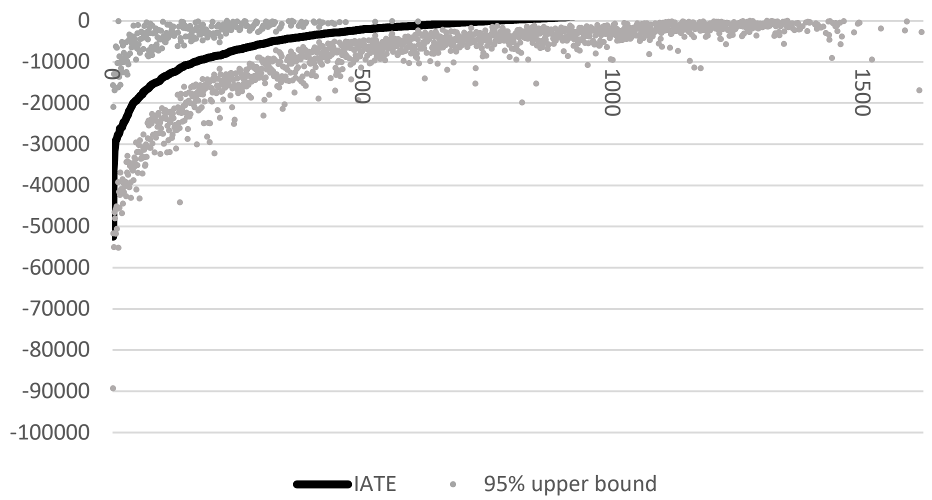


Figure 7.I – IATE estimate of T1 vs T2 comparison: a) all units; b) units with IATE < 0; c) units with IATE > 0 (2015-2018).

a)



b)



c)

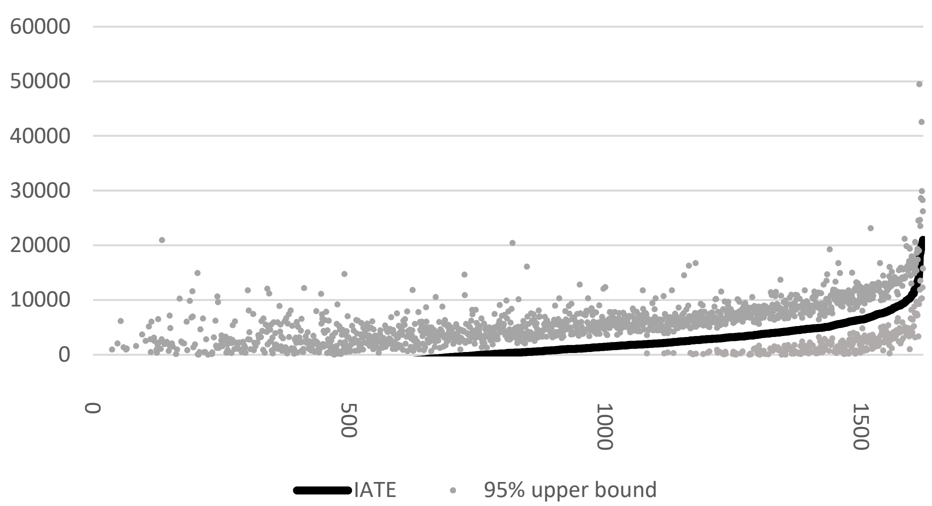


Figure 7.II – IATT estimate of T1 vs T2 comparison: a) all units; b) units with IATT < 0; c) units with IATT > 0 (2015-2018).

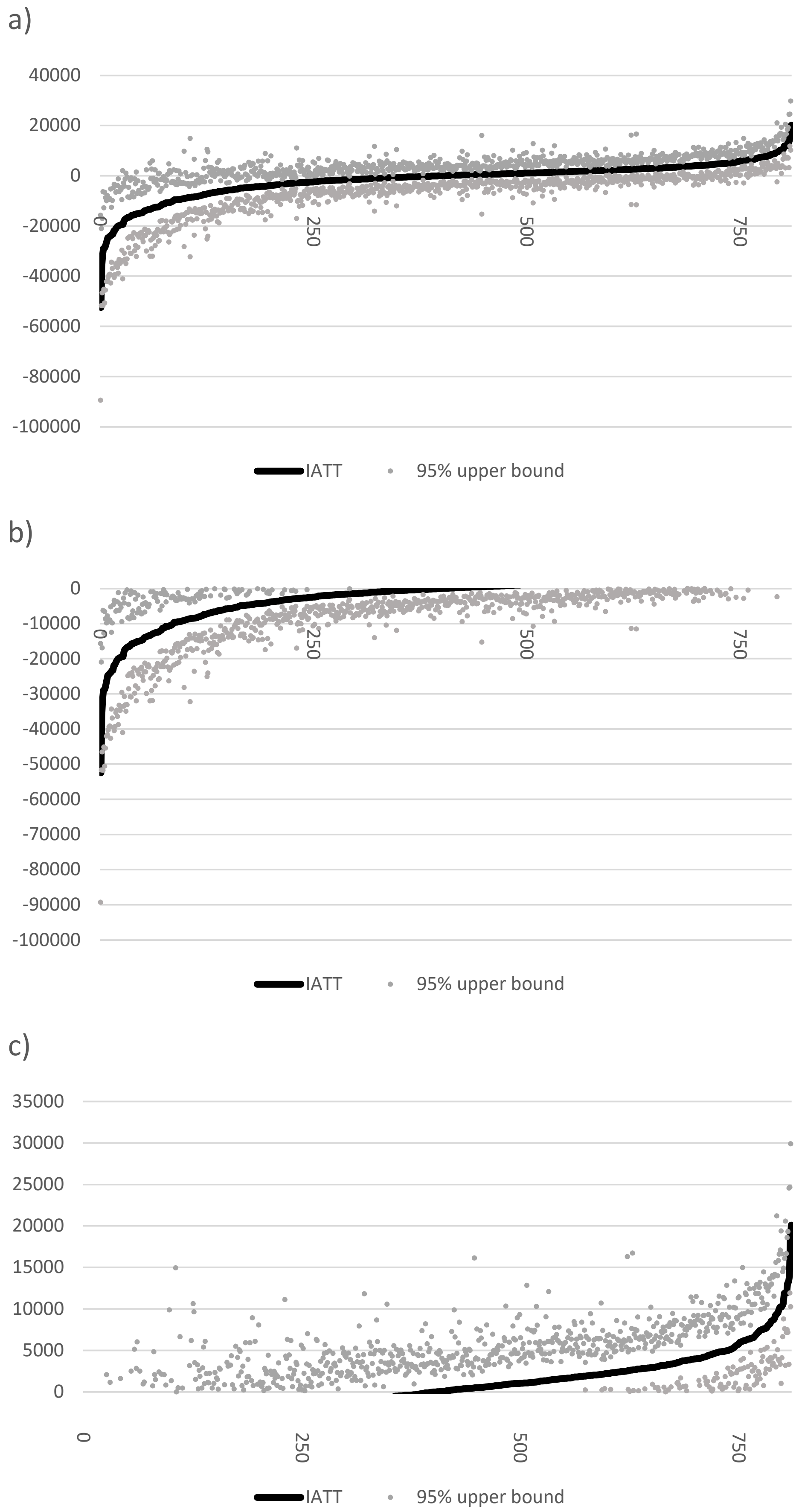


Figure 7.III – IATU estimate of T1 vs T2 comparison: a) all units; b) units with IATU < 0; c) units with IATU > 0 (2015-2018).