UNIVERSITÀ DEGLI STUDI FIRENZE

DISEI DIPARTIMENTO DI SCIENZE PER L'ECONOMIA E L'IMPRESA

Working Papers – Economics

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Working Paper N. 17/2026

DISEI, Università degli Studi di Firenze
Via delle Pandette 9, 50127 Firenze (Italia)
www.disei.unifi.it

Beyond Policy Intentions: What Has Crop Diversification Actually Achieved? Towards a Broader Environmental and Land-Use Assessment

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Abstract

The greening of the Common Agricultural Policy has faced criticism in both ex-ante and ex-post evaluation analyses. Positioning itself within this latter strand of literature, our study provides further evidence on the impact of the crop diversification requirement under the greening on the local environmental performance and land-use practices. Exploiting the discontinuity in eligibility criteria at 10 and 30 hectares, we apply a fuzzy regression discontinuity design to a detailed georeferenced dataset for Tuscany (Italy). The data also allows to assess innovative crop-rotation indicators as an indirect measure of environmental performance. The results show that crop diversification supported the intended changes in land-use patterns, generating local environmental benefits particularly among small and medium-sized farms. However, no significant effects emerge for larger farms. Additionally, the findings indicate a positive relationship between diversification and rotation practices, suggesting potential policy implications in which diversification can be leveraged to promote crop rotation.

Keywords: *Common Agricultural Policy, Green Payments, Crop Diversification, Crop Rotation, Regression Discontinuity Design*

JEL codes: O18; Q12; Q18; Q58; R52.

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1 Introduction

The Common Agricultural Policy (hereafter, CAP) has played a crucial role over the past decades, by progressively evolving from a form of farmers' income support into a policy framework underpinning broader socioeconomic and environmental objectives (Sotte, 2023). From this perspective, the 2013 reform marks a decisive step in strengthening the environmental orientation of the CAP, by making 30% of direct payments in Pillar I conditional on compliance with a set of sustainable agricultural practices (the so-called Green Payments, or *greening*). Among these measures, the policy includes crop diversification (CD), which required farmers to cultivate a minimum number of crops, depending on the size of their arable land, with the explicit aim of countering mono-cultural production systems, typically associated with higher environmental pressures (European Commission, 2013). Through these mechanisms, the policy aims to influence farmers' land-use decisions towards more sustainable practices, fostering the local provision of environmental public and quasi-public goods—such as improvements in soil and water quality and biodiversity—while simultaneously supporting farm competitiveness and the socio-economic performance of rural contexts by aligning production strategies with sustainability-oriented development models. To this end, the policy relies on Green Payments (GPs) as a form of compensation for part of the additional costs associated with the adoption of these sustainable land-uses (Brutti et al., 2023).

Beyond policy intentions, however, the effects of the GPs regulation remain heavily debated, both in political circles (European Court of Auditors, 2017) and in academic work (Vanni, 2014). Empirically, several ex-ante impact assessment studies provide predictions of potential impacts under alternative scenarios, and a number of ex-post evaluations, utilizing counterfactual methodologies (Sauquet, 2023; Diop and Védrine, 2025; Brutti et al., 2025), quantitatively assesses the regulation's impact on changes in agricultural land-use practices and a range of environmental and economic outcome drivers. Overall, the results remain somewhat mixed.

Against this background, the study therefore provides an ex-post evaluation of the CD measure, as a specific component of the *greening* framework under the 2014–2020/22 CAP, with the aim of offering new empirical insights into the effectiveness of such a land-use policy measure in shaping local land-use practices and their environmental implications.

In particular, the paper contributes to the ongoing debate on the effectiveness of the CD measure (Sauquet, 2023; Diop and Védrine, 2025; Brutti et al., 2025) by providing, for the first time, an ex-post causal assessment based on a highly granular, georeferenced, parcel-level administrative dataset that also allows tracking changes in land allocation over time. Its extensive coverage therefore ensures representativeness and enables more robust causal insights, addressing key limitations highlighted in previous studies relying on aggregate data sources (Brutti et al., 2025).

Furthermore, the analysis employs a fuzzy Regression Discontinuity Design (RDD) to estimate the causal impact of the CD regulation. Within this framework, we enrich the empirical evidence by distinguishing between three different effects: the intention-to-treat (ITT)—that is, the effect of policy implementation regardless of actual treatment take-up; the treatment take-up—namely, the effect of policy implementation on participation; and the local average treatment effect (LATE) on compliant units—that is, the policy's actual impact on those who participate. This approach proves particularly relevant, as it enables the investigation of a non-negligible gap between potential eligibility and actual participation, with implications for policy design, at least in the Italian context in which the analysis is conducted.

Beyond this, direct evaluation of environmental effects remains constrained by data limitations—most notably the lack of micro-level environmental information (Brutti et al., 2023) and biases arising from aggregation approaches (Baldoni et al., 2023). In response, this study assesses an innovative measure of indirect environmental outcomes based on a set of crop-sequence (CS) indicators capturing changes in crop rotation (CR) practices over time (Stein & Steinmann, 2018). CR is indeed an agronomic practice widely recognised for its inherently high capacity to deliver substantial environmental benefits (Karlen et al., 1994). As such, these indicators provide a more informative and valuable proxy for the environmental implications of CD than those commonly employed in previous studies (see, Brutti et al., 2025; Diop & Védrine, 2025).

Consistent with this framework, the 2014–2022 CAP was indeed initially designed around a more ambitious CR requirement, which, however, was subsequently diluted during the negotiation process into a simplified version, namely CD. Against this backdrop, beyond assessing the environmental effects of the CD measure, it also becomes particularly relevant to further investigate the local-level relationship between CR and CD—an issue that remains largely unexplored in the existing literature, yet one with potentially substantial policy implications. Indeed, the debate surrounding CR and CD does not end with the 2014–2020/22 CAP programming period but extends to the subsequent reform cycle (2023–2027). During this latter phase, CR is initially introduced as a requirement under enhanced environmental conditionality, but is subsequently weakened following farmers' protests, with CD once again allowed at the discretion of Member States (MS) (European Commission, 2024; Marandola & Vanni, 2019; Sotte, 2023).

In addressing these issues, the paper is organised as follows. Section 2 provides a discussion on the regulatory requirement and an overview of the relevant literature. Sections 3 and 4 illustrate the data description and the methodology, respectively. Results of our analysis are presented in Section 5 and discussed in Section 6. Section 7 concludes.

2 Evaluating Green Payments: A Literature Overview

2.1 Policy design: crop diversification and implementations specificities

The 2014–2020/22 *greening* conditions, for the first time, a substantial share of direct payments (30%) within Pillar I on compliance with specific, more environmentally friendly land-use practices. Specifically, the GPs requirements are defined according to the size of farms' arable land (AL), and farmers exceeding certain thresholds—namely, from 10 hectares onwards, as discussed below—are required to adapt their production practices to the *greening* provisions; non-compliance entails a reduction in financial aid of up to 1.25 times the greening component, along with additional penalties introduced from 2017 (Brutti et al., 2026; European Commission, 2013; Gocht et al., 2017). Accordingly, farms below 10 hectares of AL are not subject to these requirements and remain eligible for the full share of direct payments, without additional obligations (Brutti et al., 2023). In doing so, the primary objective of this initiative is to enhance the environmental performance of the agricultural sector and rural areas by incentivising farmers to adopt sustainable land practices without compromising their economic competitiveness (Brutti et al., 2023).

Among these measures, the CD requirement applies to farms with more than 10 hectares of arable land (AL), which are required to cultivate at least two crops, with the main crop not exceeding the 75% of total AL. Farms with more than 30 hectares are subject to an additional requirement to cultivate a third crop, with the two main crops together not exceeding 95% of total AL. Exemption criteria include farms already applying environmentally beneficial practices, such as organic farming, as well as farms where grassland, forage area, rice cultivation, or leguminous crops cover more than 75% of the agricultural area, or where crops grown under water account for 100% of the AL (European Commission, 2013).

Despite uniform application at the EU level, MS retain discretion over payment modalities and the extent of land subject to greening obligations (Sotte, 2023). With explicit reference to the Italian case, GPs are applied only to the share of agricultural area eligible under the Basic Payment Scheme (BPS), which replaces the former Single Payment Scheme. Moreover, these payments are not uniform, as per-hectare aid varies according to the value of entitlements granted to farmers on a historical basis—stemming from the decoupling introduced in 2005 and anchored to production patterns observed between 2000 and 2002. As a result, farmers receive an individualised GP, determined by the BPS-eligible area and the historical value of the entitlements attached to that area (see, Guastella et al., 2018; Pierangeli, 2014; Pupo D'Andrea, 2014; Vanni, 2014).

2.2 Ex-ante and ex-post evaluation analysis

Against this institutional framework, a growing body of literature employs both ex-ante impact assessments and ex-post evaluation studies to assess the effectiveness of GPs. Among ex-ante analyses, Louhichi et al. (2018), using a farm-level IFM-CAP simulation model applied to FADN data at the European level, find highly limited impacts of GPs. Despite approximately 55% of farms and 86% of utilised agricultural area (UAA) being subject to greening requirements, their simulations suggest that only about 4.5% of UAA would be effectively affected by the policy, alongside projected reductions in farm income and total production. Similar conclusions are reached by Was et al. (2014), who apply the CAPRI model to the Baltic countries and identify only marginal effects of *greening*, and by Hristov et al. (2020), who simulate the environmental impacts of the Ecological Focus Area (EFA) measure—another component of GPs—and find a limited potential to enhance biodiversity and ecosystem services in two Swedish regions.

As far as geographical heterogeneity is considered, Solazzo et al. (2016) conduct an ex-ante analysis on four regions in northern Italy. Their findings, instead, indicate modest reductions in GHG emissions and changes in land-use practices, including a sharp decline in maize and other cereal crops, along with an increased spread of nitrogen-fixing crops, which are typically associated with positive environmental benefits. Similarly, Gocht et al. (2017), using FADN data combined with a farm-level CAPRI model, simulate the economic and environmental impacts of *greening* at the EU level. Their findings indicate small positive effects on sustainable land-use practices, productivity, and farm income, as well as reductions in GHG and ammonia emissions. At the same time, the authors observe slight increases in biodiversity but also in soil erosion.

A second strand of scientific contributions on GPs and CD evaluations focuses on ex-post assessment studies. Bertoni et al. (2021) use Markov chain approaches to assess the impact of *greening* on land use changes, distinguishing between already compliant farms, non-eligible farms, and new compliers in a flat area of Lombardy, Italy. The authors find evidence of a shift in farmland use among those with a lower degree of CD, suggesting adjustments in land allocation aimed at complying with the policy requirements. In contrast, Díaz-Poblete et al. (2021) find limited economic and environmental impacts of *greening* based on a difference-in-differences analysis applied to regionally aggregated data from Spain, covering a set of economic and land-use indicators. Subsequently, Sauquet (2023) observes, in a representative sample of French farms, that only farms exceeding 30 hectares show increased compliance with CD measures and cultivate a larger number of crops. Within the same national context, Diop and Védrine (2025) conduct an analysis using a Difference-in-Discontinuity approach on a sample of farms, comparing those just above and below the 10-hectare and 30-hectare thresholds in terms of economic outcomes, land-use practices, and environmental measurements. The results show that farms around the 10-hectare threshold increase CD with significant land reallocation, unlike those around the 30-hectare threshold. However, the effects are largely driven by farms already compliant with crop diversity requirements, suggesting limited additional impact from *greening*.

Further empirical evidence is then provided by Brutti et al. (2025), who conduct a counterfactual evaluation analysis of CD's effectiveness across 28 EU member states using FADN data. They find significant impacts on regulatory outcomes for farms not already compliant with regulation's requirement. The policy contribution is found higher for crops farms, and for farms with smaller AL. Along the same lines, the insights provided by Brutti et al. (2026), analysing the behaviour of Spanish farms in 2022, identify a stronger contribution of the CD measure at the 10-hectare compared with the 30-hectare threshold.

Although not strictly comparable due to differences in methodology, territorial scale, and the outcomes evaluated, the existing studies nonetheless depict a complex and at times contradictory picture, with indications that impacts tend to be limited overall.

This evidence has primarily been traced back to political and design-related factors. Indeed, the *place-blind* and *one-size-fits-all* design of the *greening* measures has been criticised since its introduction for its restricted ability to adapt to heterogeneous local socio-economic and environmental conditions, resulting in limited and potentially jeopardised impacts (Matthews, 2012). In particular, the wide diversity of agricultural systems across Europe—in terms of specialisations and production models—may lead to heterogeneous responses to these measures. This implies that a uniform threshold applied indiscriminately across diverse contexts may be appropriate in some settings but less so in others (Cortignani & Dono, 2020; Matthews, 2012).

Moreover, when focusing specifically on CD, the aforementioned replacement of CR, together with a subsequent tightening of CD participation thresholds—from 3 to 10 hectares— further reinforces the idea of a structural weakness in policy design (Pe'er et al., 2014). This configuration is, in fact, the outcome of an intense negotiation process that took place prior to its introduction, among agricultural lobbies and environmental stakeholders which contributed to shaping a compromise that may have limited the overall effectiveness of the measure (Sotte, 2023).

However, beyond policy design, a methodological dimension of the existing evidence also warrants attention. The ability to access and exploit highly detailed administrative data at the holding level appears crucial for producing consistent and reliable results (Brutti et al., 2023). In this respect, the recent contribution by Brutti et al. (2026), using administrative data from the Geospatial Aid Application (GSAA) for Spain, highlights the reliability limits of earlier ex-post evaluations relying on farm survey data. Such granular administrative information enables the identification of more robust and less biased patterns. Accordingly, in their case it allows for a qualitative improvement in empirical assessment, making it possible to detect bunching at both CD thresholds and thereby prompting the authors to apply a calibrated decision-making model to better understand farms' incentives for policy participation (Brutti et al., 2026). Prior to this contribution, only Bertoni et al. (2021) employed a detailed administrative dataset in an ex-post evaluation of the 2014–2022 CAP, albeit relying on methodologies that do not allow for causal inference (see Sauquet, 2023).

In line with this, the present study revisits the environmental and land-use effects of CD by exploiting a highly granular, parcel-level administrative dataset drawn from the Italian GSAA and covering the entire region of Tuscany (Italy). While based on the same parcel-level unit of observation as Brutti et al. (2026), the dataset is characterised by a substantially richer and more detailed set of parcel-specific variables as well as a longitudinal dimension. For the first time, these data are employed within a counterfactual impact evaluation (CIE) framework to provide causally grounded evidence on the effects of the measure. In doing so, the empirical analysis is therefore guided by the following research question:

RQ1: *What have been the environmental and land-use effects of greening CD measure?*

By jointly leveraging highly disaggregated georeferenced administrative data and a causal identification strategy, this study advances the existing literature by providing a more robust assessment of CD effects, thereby contributing new evidence to the ongoing debate on the effectiveness of this measure.

2.3 Crop Rotation Indicators for Environmental Outcome Assessment

Within this perspective, particular attention is devoted to the assessment of environmental outcomes, which remain among the most challenging dimensions to evaluate empirically. As already noted, despite the policy's explicitly "green" intention, the evaluation of the environmental impacts of the 2014-2022 CD measure remains limited by the scarce availability of micro-level environmental data that can be directly linked to individual farms, whether participating in the measure or not (Baldoni et al., 2023; Brutti et al., 2023). To overcome these limitations, much of the existing literature focuses on policy participation criteria, inferring environmental performance from compliance with the land-use requirements of the measure. Beyond this compliance-based perspective, other proxies are also suggested, including indicators of CD (e.g. the Shannon index), input-use indicators (e.g. fertiliser and pesticide use), and environmental efficiency metrics (Brutti et al., 2025; Diop & Védrine, 2025; Sauquet, 2023).

From this perspective, the study proposes a set of innovative and more informative indicators of environmental performance by measuring and assessing Crop Rotation (CR) practices. CR is indeed a land-use agronomic practice based on specific rotation cycles and is widely acknowledged for its substantial environmental benefits (Cortignani & Dono, 2020; Karlen et al., 1994). More precisely, as further detailed below, the study proposes an alternative approach in measuring CR practices that combines two complementary frameworks: the crop-sequence (CS) typification framework suggested by Stein and Steinmann (2018) and raster-based analytical techniques developed in Upcott et al. (2023). Building on this integration, the study develops an innovative set of crop-sequence (CS) indicators—defined as the ordered succession of crops grown on the same parcel across consecutive annual seasons—to measure crop-rotation (CR) practices.

Assessing CR, through CS indicators, thus provides a valuable indirect indicator of the environmental outcomes of the policy. Observed increases in crop-rotation dynamics can indeed be interpreted as evidence of positive environmental effects associated with the implementation of the CD measure. The rationale behind this assumption also lies in the partial and asymmetric complementarity between these two land-use practices. CR entails a temporally articulated land-use management strategy, requiring planned crop succession across consecutive growing seasons and typically involving higher compliance and adjustment costs. By contrast, CD operates within a single growing season, focusing on the coexistence of multiple crops in the same year, and can therefore be interpreted as a simplified and lower-cost approximation of rotation dynamics, generally associated with more limited environmental benefits. Thus, to the extent that CD supports or facilitates the adoption of CR practices, it is associated with greater environmental benefits at the local level.

In this respect, the introduction and measurement of CR practices not only represent significant value added for the assessment of the environmental effects of CD but also provide a valuable analytical lens through which to investigate the interaction between these two land-use practices.

To the best of our knowledge, existing empirical evaluations of the CD measure have largely left the interrelationship with CR under-explored. By explicitly focusing on this interaction, the present study offers a novel contribution to the literature and addresses an issue of direct policy relevance, particularly in light of the evolving design of GPs, which has repeatedly alternated between CR and CD as key environmental instruments. In doing so, this approach allows the analysis to further expand and deepen the following research question:

RQ2: *Would crop diversification have supported or hindered crop rotation practices?*

3 Data

3.1 Data description, sample size and outcome variables

Our analysis exploits the dataset provided by the Regional Agency of Tuscany for Agricultural Payments (ARTEA), which contains primary administrative data feeding into the national GeoSpatial Aid Application system (GSAA) —namely, the Graphic Crop Plans (GCPs) submitted annually by farm holdings to access various agricultural funds (e.g. CAP incentives and gas-oil subsidies). Specifically, the dataset includes georeferenced agricultural land parcels across the Tuscany region (Italy) over the period 2016–2022 (e.g. approximately 691,400 polygons in 2022). Each parcel is characterised by a high level of detail in land use, including information on the type of crop cultivated in a given year, as well as its location and size. Parcels are linked to the farm's official holding file (*fascicolo aziendale*), which contains information on the farm holder and provides detailed structural and economic information on the farms managing these parcels, such as the number of employees, farm typology, and land tenure arrangements. In addition, the dataset provides detailed information on CAP aid applications at the parcel level, enabling each land parcel to be linked to its eligibility status and the amount of Direct Payments actually received.

The dataset ensures extensive coverage, as confirmed by comparing the data on Utilized Agricultural Area (UAA) and Arable Land (AL) with those reported by the Italian National Statistical Office (ISTAT, 2022) in the latest National Agricultural Census (see Table 5 in the Appendix 7). In terms of AL, compared with the 440,829 hectares recorded in 2020 (ISTAT, 2022), the dataset contains roughly 416,000 hectares in 2022. Of these, about 246,938 hectares fall under GPs. In particular, although more than 70% of farms in 2022 operate on less than 10 hectares of AL (see Figure 3 in the Appendix 7)—reflecting the predominantly family-based agricultural model typical of the Tuscan context (IRPET, 2023)—only about 20% of AL under GPs falls within these small and medium-sized holdings, whereas approximately more than 50% of AL under GPs is managed by farms with an AL size exceeding 50 hectares.

As a preparatory step for the empirical analysis, we aggregated parcels by farm holding, and excluded exempted and invalid farms (see Appendix 7 for further details on the cleaning procedure). A final sample of 14,512 farms for the year 2022 is obtained. Although the empirical focus is on the year 2022, the same analysis can be replicated across multiple years.

Subsequently, a set of outcomes related to land-use practices and environmental drivers is constructed at the farm level. Specifically, in terms of land-use practices, the policy criteria for the CD measure are applied. The implementation of CD requirements is based on the amount of AL, according to which farms are required to cultivate a minimum number of crops. The number of AL crops cultivated by each farm is therefore measured (for further details on crop categories see Appendix 7) and, in line with the *greening* initiative, is expected to increase at the relevant policy thresholds. At the same time, the regulation also sets limits on the share of the main crop and of the two main crops, which are expected to decrease for farms involved in the policy initiative. Relative to each farm's total AL, the percentage of area dedicated to each crop is therefore measured and assessed. Preliminary statistics indicate that Tuscan farms cultivated, on average, 1.99 different crops in 2022, allocating about 81% of their arable land to the main crop and approximately 96% when considering the two main crops (see Table 7 in the Appendix). In general, while an increase in AL is expected to be associated with a higher number of cultivated crops, farm expansion is typically accompanied by a less-than-proportional increase in crop diversity (Brutti et al., 2026).

On the other hand, indirect environmental outcomes are captured by crop-rotation indicators—discussed in the following section—and by the Shannon Index (Diop & Védrine, 2025), both of which are expected to increase for farms located just above the policy threshold. The Shannon Index is computed as the number of crops cultivated by each farm, weighted by their respective acreage shares (Varacca et al., 2023), as follows:

$$S_{it} = -\sum_{c} \frac{AL_{it,c}}{AL_{it}} \log\left(\frac{AL_{it,c}}{AL_{it}}\right), \qquad \forall c \in C \tag{1}$$

where $AL_{it,c}$ indicates the arable land allocated to species c by farm i at time t, AL_{it} defines the total arable land cultivated by farm i at time t, and C indicates the cardinality of the crop set observed across all farms. This formulation captures biological diversity by reflecting both the evenness and richness of crop diversification at the farm level. It equals zero under monoculture and increases steadily with crop diversity (see, Varacca et al., 2023).

3.2 Estimating Crop Rotation through Crop Sequences

Crop rotation, widely acknowledged for its environmental benefits, provides a valuable indirect indicator of the environmental effects of the CD measure. On this basis, the analysis contributes to ex-post evaluations of the CD measure by leveraging rotation-based indicators to assess environmental performance at the farm level.

However, in measuring and monitoring CR, some important challenges emerge, particularly in relation to: (i) the wide variety and flexibility of crop combinations that farmers can implement over time, often with relatively low occurrence of each combination type; (ii) the need for a detailed, long-term dataset, typically spanning five to seven years; (iii) the fact that available crop data frequently represent only segments of full rotations, as the cycle may begin before or continue beyond the observed time window; and (iv) the difficulty of tracing rotations when parcel boundaries within a field block change from year to year (Pahmeyer et al., 2025; Stein & Steinmann, 2018; Upcott et al., 2023). Most existing approaches measure crop-rotation management using diverse models and methods (see, for example Bachinger & Zander, 2007; Castellazzi et al., 2008; Dogliotti et al., 2004; Dury et al., 2012; Levavasseur et al., 2016). Nonetheless, these approaches remain only partially suitable for capturing complete crop-rotation sequences (Stein & Steinmann, 2018) and for constructing meaningful indicators at the farm level, which is a critical dimension for the environmental evaluation of the CD rule (Brutti et al., 2023).

To address these limitations, the paper combines two frameworks: the crop-sequence typification framework proposed by Stein and Steinmann (2018) and raster-based analytical techniques as suggested by Upcott et al. (2023). Specifically, by exploiting the seven-year dataset and shifting from the concept of CR to that of CS, the analysis captures the ordered succession of crops cultivated on a given field parcel across consecutive annual growing seasons, without requiring predetermined rotation cycles, thereby allowing the reconstruction of rotation dynamics beyond partial observation windows (Stein & Steinmann, 2018). Operationally, crop sequences are derived through a typification procedure that classifies individual agricultural fields at the parcel (polygonal) level according to a set of rotation categories. The set of rotation categories is adapted to the characteristics of the dataset and therefore slightly differs from those originally proposed by Stein and Steinmann (2018). In particular, six rotation categories—namely cereals, legumes, renewable crops, aromatic crops, industrial crops, and horticultural crops—are identified and used to classify each arable-land field on an annual basis.

Once the classification is completed, we measure the *sum of transitions*, defined as the total number of crop changes in a sequence, with a maximum equal to the sequence length minus one (Stein & Steinmann, 2018). In doing so, we adopt a raster-based technique to account for possible changes in field boundaries over time (see also Pahmeyer et al., 2025, Upcott et al., 2023). Specifically, for each year a 10 × 10 m raster is created, using the six-category crop classification as bands for the entire region under analysis. Each pixel for each year indicates which of the six categories it belongs to. If a pixel does not fall into any of these categories (e.g., non-agricultural areas or permanent crops), it is assigned a a "Not Available" `NA` classification. After that, an appropriate algorithm is applied to measure the sum of transitions for each pixel (see Figure 4 in the Appendix 7). Pixels that are taken out of cultivation—that is, classified as `NA` even for a single year—are excluded according to Stein and Steinmann (2018).

To derive farm-level rotation indicators for the year of analysis, the 2022 agricultural field polygons associated with each farm are overlaid onto the resulting pixel-level *sum of transitions*. For each field polygon, the *sum of transitions* is computed as the average of the pixel-level values contained within that polygon. These field-level measures are then aggregated at the farm level by constructing three rotation indicators, defined as follows:

1. CS_{max}: the maximum value of the sum of transitions observed among different polygons within a farm;
2. CS_{avg}: the average value of the sum of transitions observed among different polygons within a farm;
3. CS_{index}: a farm-level indicator of crop-sequence intensity. This measure represents an area-weighted average of crop transitions on the plots under rotation, scaled by the share of the farm's total area that actually undergoes rotation.

   For each farm i, CS_{index} is defined as:

$$CS_{\text{index},i} = \frac{\sum_{j \in i} \left(\text{N\_transitions}_j \times \text{AL}_j\right)}{\text{AL}_i^{\text{tot}}}, \tag{2}$$

   where N_transitions_j denotes the number of crop-sequence transitions observed in plot j of farm i, AL_j the corresponding arable-land area (ha), and AL_i^{tot} the total arable-land area (ha) of farm i.

4 Methodology

4.1 Identification Strategy: RDD within a Multi-cutoff and Fuzzy Design

The implementation of CD requirements is based on specific thresholds tied to farm size metrics, which serve as criteria for compliance with administrative regulations. These thresholds can be utilized to estimate local treatment effects within a Regression Discontinuity Design (RDD) in a continuity-based framework. The RDD proves to be a valid methodology for estimating policy causal effects, as it relies on three fundamental components: a continuously distributed score variable that determines treatment assignment—here, the AL of each farm; a cutoff reference value, set in our case at the 10- and 30-hectare, where farms exceeding these levels are expected to be assigned to the treatment group; and perfect or imperfect compliance with treatment assignment and take-up, an aspect we discuss shortly. On the basis of these elements, the RDD relies on the assumption that units located immediately above and below the threshold are sufficiently comparable to approximate a randomized controlled trial; consequently, any observed discontinuity in the outcome at the cutoff can be directly attributed to the policy implementation, which thus represents the sole source of variation between units (Cattaneo et al., 2019, 2024).

Given the multi-cutoff policy design, a midpoint between the two thresholds (20 hectares, the median) is used to assign each unit uniquely to one cutoff, thus preventing correlated cutoff-specific estimates. The dataset is then partitioned into two sub-samples for separate cutoff-specific analyses (see Cattaneo et al., 2024).

In addition, preliminary data analysis reveals imperfect compliance between treatment assignment and treatment receipt at both cutoffs, consistent with a RD with a Fuzzy design (Cattaneo et al., 2024). In this context, treatment assignment is determined by total cultivated AL, with farms exceeding the 10-hectare threshold being eligible for CD requirements. By contrast, treatment receipt depends on the share of AL actually subject to CD obligations, which is linked to the area associated with activated payment entitlements under the Basic Payment Scheme (BPS). As illustrated in Table 9 (Appendix 7), a two-sided non-compliance pattern emerges: some farms located above the cutoff, although eligible, do not comply with the crop diversification (CD) requirements, while others, albeit few, below the cutoff do implement it. This discrepancy reflects the institutional legacy of how GPs have been implemented in Italy since their inception (see Section 2). In particular, effective participation does not depend solely on total AL, but on the share of AL associated with historical payment entitlements that determine access to *greening*. The value and activation of these entitlements vary across parcels, resulting in an imperfect alignment between AL farm size (eligibility) and the share of AL actually subject to greening obligations and payments (participation). This institutional configuration underlies the observed Fuzzy design, which becomes empirically identifiable thanks to the high level of detail provided by the dataset.

The presence of imperfect compliance allows for a more comprehensive assessment of the effects of the CD measure compared to a canonical Sharp RD design. In particular, beyond the effect of treatment assignment (eligibility), it is possible to further investigate how this effect changes when focusing on those farms that actually receive the treatment, i.e. farms subject to CD requirements (participation) (Cattaneo et al., 2024).

In practice, the analysis focuses on the effect of being eligible for the CD requirement on the outcomes of interest, capturing the effect of treatment *assignment*. This is known as the Intention-to-Treat (ITT) effect. The key idea is that the policy assigns farms to treatment or control based on a threshold: farms above the cutoff are eligible for CD, while those below are not. The ITT effect therefore captures the impact of this assignment rule—that is, the effect of being eligible for the policy—without distinguishing between farms that actually comply and those that do not. In addition, the analysis examines whether being eligible increases the likelihood of actually receiving the treatment—namely, being subject to CD requirements (participation). This is referred to as the *first-stage effect* and captures the discontinuity in treatment take-up at the cutoff, providing a direct measure of policy participation in CD measures at both cutoffs, conditional on the structural characteristics discussed above (Cattaneo et al., 2019, 2024).

After this step, the analysis concerns the assessment of the effect of actually *receiving* treatment—i.e. participating in the CD measure— on the outcomes of interest, obtained by estimating the *Fuzzy RD* parameter. Under the monotonicity assumption, this parameter can be interpreted as the effect of the treatment received for the subpopulation of *compliers*—that is, those units whose treatment status (participation) coincides with their treatment assignment (eligibility). This effect corresponds to the Local Average Treatment Effect (LATE) and is defined as the ratio between the difference in average outcomes (ITT) and the difference in the probability of receiving the treatment (the *first-stage effect*) between units above and below the cutoff. The estimation of the *Fuzzy RD* parameter therefore requires a non-zero *first-stage* assumption to avoid taking an undefined value. However, taking the simple ratio between the two ITT effects could, when using different bandwidths, involve a different number of observations in the estimation of the denominator and the numerator. Although this approach is theoretically valid, following Cattaneo et al. (2024), the preferred strategy is to estimate the *Fuzzy RD* parameter as a ratio, re-estimating both the ITT and the first-stage effect using the same bandwidth (Cattaneo et al., 2024).

The analysis relies on a data-driven procedure that employs local linear polynomial RD estimators with symmetric, mean-squared-error (MSE) optimal bandwidths on either side of the cutoff. Once the effects are estimated, inference is conducted using the same bandwidth applied in the estimation, while confidence intervals are adjusted through a robust bias-correction procedure (see, Cattaneo et al., 2019, 2024).

4.2 Design Validation

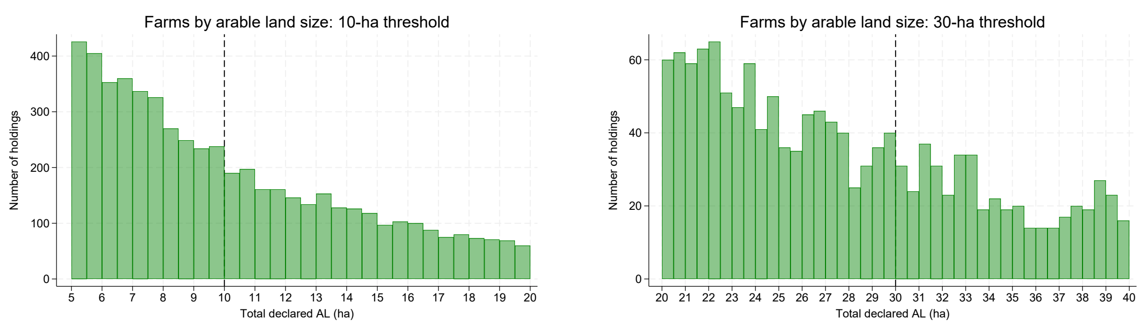
RDD is a valid empirical design if it successfully replicates the conditions of local randomization, which must therefore be tested. Specifically, knowing the rule that assigns treatment and the cutoff is not sufficient, by itself, to ensure that the assumptions required to identify the RD effect are satisfied. Indeed, units could actively manipulate their score values when they are around the cutoff in order to either avoid or participate in the treatment.

In this case, the validity of the design is ensured by administrative cross-checks, including remote controls, the use of GIS databases, and satellite monitoring carried out directly by ARTEA, which therefore oversees compliance with conditionality requirements and the accuracy of farmers' declarations (ARTEA, 2025; Rete Rurale Nazionale, 2014). Moreover, an explicit RD manipulation test based on the density of observations around the cutoff is performed to validate the design (Cattaneo et al., 2019).

In doing so, and basing the analysis on the distribution of farms by AL size, the dataset used for the RD manipulation test only is deliberately enriched with farms exempted under the regulatory criteria set out in Article 44(1), 44(3)(a), and 44(3)(b) of EU Regulation No. 1307/2013 (European Commission, 2013) (see Appendix 7, point (v)). Indeed, these farms (2,226 observations), by policy design, do not follow the smooth distribution observed for other exempted farms (e.g. organic farms). Rather, as expected, they appear mainly beyond the 10-hectare threshold—the first eligibility cutoff (see Appendix 7.0.1)—thereby constituting a sizeable and consistent share of farms located to the right of the cutoff. As a result, failing to account for these farms may generate a discontinuity in the density curve at the threshold that reflects regulatory design rather than potential manipulation by farmers, thereby biasing the results of the test. For further discussion of the rationale behind this choice, as well as the potential for farmers' bunching behaviour around the threshold, see Appendix 7.0.1.

The null hypothesis of continuity in the density of AL at both cutoffs—namely, the assumption of no "manipulation"—is then tested. The underlying idea is that, in the absence of manipulation, the number of observations just above the cutoff should be approximately equal to the number just below it. The density of observations around each cutoff is thus estimated separately using a local polynomial density estimator, which avoids data pre-binning and improves size and power relative to alternative methods (Cattaneo et al., 2019). Using this dataset (16,738 observations), the test statistic at the 10-hectare cutoff equals -1.384 ($p = 0.166$), whereas at the 30-hectare cutoff it equals -0.502 ($p = 0.615$) (see Table 10 in Appendix 7). Under the continuity-based approach, we therefore fail to reject the null hypothesis. This indicates the absence of statistical evidence of manipulation at either cutoff, thereby supporting the validity of the RDD design. Consistently, Figure 1 provides graphical evidence supporting the absence of sorting dynamics at both cutoffs. A broader distribution over the 0–50 hectare range is finally reported in Figure 9 in Appendix 7. in Figure 9 in the Appendix 7).

Figure 1: **Distribution of AL around the cut-off**



(a) 10-ha threshold

(b) 30-ha threshold

Source: Authors' elaboration based on dataset ARTEA (2022).

Although we do not find evidence of sorting that could implying avoidance (or bunching) behaviour in our sample, the existing literature documents a tendency in this direction (see Brutti et al. (2026) but also Cocco (2026), seminar presentation at the JRC-Ispra, 01/13/2026). While we do not observe such patterns in the data, their presence cannot be entirely ruled out. If avoidance behaviour were indeed occurring but remained undetected, our estimates could be upward biased. However, the absence of visible discontinuities in the density of the running variable suggests that any potential bias is likely to be more limited in magnitude compared to previous studies that document phenomena of manipulation around the policy cutoffs.

4.3 Regression Model CD e CR

After investigating whether the Regulation affected crop rotation beyond its impact on crop diversification, we turn to the question of whether crop diversification is systematically associated with crop rotation.

Conceptually, the two practices are closely related but not equivalent. A farm that adopts crop rotation inherently diversifies production over time, even if it does not cultivate multiple crops simultaneously, i.e. does not diversify. Conversely, a farm may be diversified without practising crop rotation, for example by growing several crops in different fields while maintaining the same crop allocation over time. While these conceptual distinctions are well established, empirical evidence quantifying the relationship between crop diversification and crop rotation remains limited.

To investigate this relationship, we estimate the association between changes in crop diversification observed between 2016 and 2022 and crop rotation patterns measured in 2022. Specifically, crop diversification is captured by changes in the number of crops grown and in the share of land allocated to the main crops, while crop rotation is measured through indicators based on crop sequences observed in 2022.

The analysis is conducted on the subset of farms for which information on crop sequences is available (5,962 observation). We estimate a set of nested specifications of the following general model:

$$CS_i = \beta_0 + \beta_1 \Delta n.crops_i + \beta_2 \Delta area_{1i} + \beta_3 \Delta area_{2i} + \varepsilon_i$$

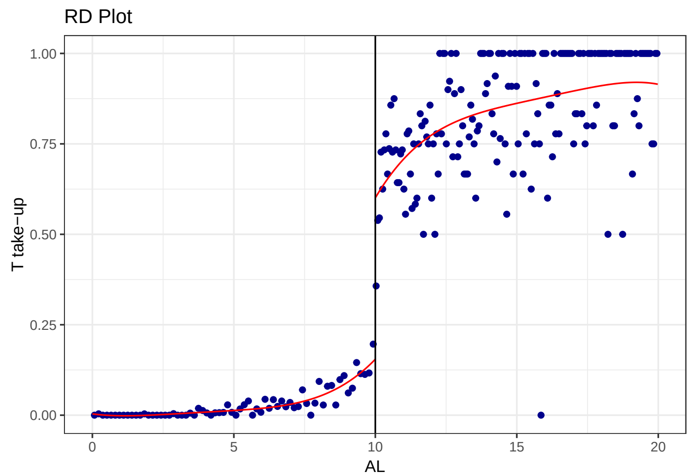
It is important to note that this analysis does not aim to identify the causal effect of the Regulation on either crop diversification or crop rotation. The agricultural practices required by the Regulation were already in force from 2015 onwards, whereas the variation in diversification measures observed between 2016 and 2022 reflects farmers' voluntary adjustments rather than regulatory requirements. Consequently, the estimated coefficients should be interpreted as capturing the association between changes in crop diversification and crop rotation patterns, rather than the impact of the Regulation itself.

5 Results

5.1 Effects of the Crop Diversification Policy

Starting with the estimation at the 10-hectare threshold (subsample of 12,777 farms), Figure 2 confirms the presence of two-sided non-compliance. The graph depicts the effect of CD assignment on treatment uptake —the *first-stage effect*—showing that some farms below the cutoff, on the ineligible side, have a positive probability of receiving the treatment, while some farms above the cutoff, on the eligible side, exhibit a probability lower than one. This pattern is thus in line with a Fuzzy RD design.

Figure 2: **RD plot of First Stage Effect at 10-ha**



Source: Authors' elaboration based on ARTEA dataset (2022); Note: Evenly-spaced bins.

The formal analysis, reported in Table 1, shows that the *first-stage* effect is 0.476, consistent with the observed discontinuity in Figure 2. This effect is statistically significant at the 1% level, with a 95% robust confidence interval ranging from 0.362 to 0.556, which does not include zero. This implies that crossing the 10-hectare total AL threshold increases the probability of taking up CD measure in 2022 by approximately 47 percentage points. This therefore provides a measure of how the introduction of the *greening* reform impacts, at the 10-ha cutoff, participation in the policy itself, namely compliance with CD requirements.

Table 1: **First-stage effect**

	cutoff=10	**cutoff=30**
First Stage	0.476***	0.171
Robust 95% CI	[0.362, 0.556]	[-0.122, 0.277]
h	3.085	8.318
Eff. Observation	2147	689
Kernel	Triangular	Triangular

Source: Author' elaboration. Note: ***, **, * denote 1%, 5%, and 10% significance levels, respectively; h: bandwidth.

At the 30-hectare threshold (subsample: 1,735 farms), by contrast, no statistically significant effect of treatment assignment on actual participation is detected (see Table 1). As a consequence, the fuzzy RD parameter for the corresponding outcomes cannot be precisely identified, since the hypothesis of indeterminacy discussed in Section 4 cannot be rejected. Moreover, the ITT analysis identifies only two weakly statistically significant effects: farms just above the 30-hectare cutoff, and thus barely eligible for the CD requirement, are more likely to reduce both the share of the main crop and that of the two main crops, consistent with the policy requirements (RD estimates respectively equal to -5.020^{*} and -3.943^{*} in Table 11 in the Appendix). Nevertheless, the evidence at this threshold remains limited and largely inconclusive.

Extending the analysis at the 10-hectare threshold, the effects of treatment assignment on the different outcomes of interest are reported in the Intention-to-Treat (ITT) table (see Table 2). Positive and statistically significant evidence emerges in relation to land-use practices. Specifically, the results show that the assignment of CD determines that farms whose score of AL is barely above the 10-ha threshold are more likely to increase their number of crops by 0.49 points and to reduce the share of their main crop and of their two main crops by 10.78 and 2.62 percentage points, respectively. Thus, this aligns with policy requirements.

In addition, the implementation of the CD measure appears to increase the likelihood that farms just above the 10-hectare threshold improve their environmental performance, as measured by the Shannon index, by approximately 0.22 points. This effect is statistically significant at the 1% level, and the corresponding 95% robust confidence interval—ranging from 0.133 to 0.315—excludes zero. However, at the same time, the analysis does not reveal any statistically significant results when CS indicators are considered (see Table 2).

Table 2: **ITT: policy assignment on CD outcomes (cutoff=10 ha)**

	RD Estimator	*Robust 95% CI*	*h*	*Eff. Observations*
Land-use practices				
Number of Crops	0.491***	[0.252, 0.785]	2.948	2041
Share of main crop	-10.778***	[-15.963, -6.266]	3.277	2294
Share of two main crops	-2.621**	[-5.051, -0.598]	3.657	2574
(Ind.) Environmental Outcomes				
Shannon Index	0.216***	[0.133, 0.315]	3.316	2328
CS_{max}	0.157	[-0.526, 0.669]	2.245	1094
CS_{avg}	-0.067	[-0.722, 0.392]	2.146	1049
CS_{index}	-0.065	[-0.741, 0.408]	2.056	994

Source: Authors' elaboration. *Notes:* ***, **, * denote 1%, 5%, and 10% significance levels; *h* = bandwidth; Kernel: triangular.

The estimation of the Fuzzy RD (FRD) parameter provides further evidence of changes in land use and environmental performance among those who are compliers (see Table 3). Specifically, farms just above the 10-hectare threshold that comply with the policy requirements exhibit a 0.47-point increase in the Shannon index and a 1.05-point increase in the number of crops, together with reductions of approximately 23 and 6 percentage points in the share of the main crop and of the two main crops, respectively. The CS coefficients remain statistically insignificant also for the subgroup of compliers.

Table 3: **FRD: policy take-up on CD outcomes (cutoff=10 ha)**

	RD Estimator	*Robust 95% CI*	*h*	*Eff. Observations*
Land-use practices				
Number of Crops	1.045***	[0.506, 1.742]	2.601	1772
Share of main crop	-23.31***	[-36.53, -12.63]	2.743	1881
Share of two main crops	-5.885**	[-11.89, -1.071]	2.749	1884
(Ind.) Environmental Outcomes				
Shannon Index	0.471***	[0.266, 0.727]	2.588	1762
CS_{max}	0.649	[-0.249, 1.401]	3.959	2036
CS_{avg}	0.201	[-0.923, 0.864]	3.357	1694
CS_{index}	0.303	[-0.747, 0.945]	3.661	1871

Source: Authors' elaboration. *Notes:* ***, **, * denote 1%, 5%, and 10% significance levels; h = bandwidth; Kernel: triangular.

5.2 The Relationship between Crop Diversification and Crop Rotation

In line with the aim of the study, this section further explores the relationship between crop diversification (CD) and crop rotation (CR) and presents the results of the empirical analysis.

The specification is estimated separately using CS_{max} and CS_{index} as dependent variables. The two indicators capture different dimensions of crop rotation responses to changes in diversification. Specifically, CS_{max} is intended to capture a local response to changes in diversification-related variables. Changes in crop diversification are likely to generate a direct mechanical effect on the specific parcels where cropping patterns are modified, and this localised adjustment should be most likely reflected in CS_{max}. By contrast, CS_{index} measures the average response at the farm level and is therefore better suited to capture broader changes in agricultural practices. While diversification may initially affect only a subset of parcels, CS_{index} reflects the extent to which these adjustments translate into a more general reorganisation of cropping patterns across the entire farm.

Results of the estimations are reported in Table 4. The results confirm the expected signs for both dependent variables. More precisely, specifications (2) and (3) show that increases in the number of crops between 2016 and 2022 are not significantly associated with higher values of the crop sequence indicators, either locally or on average. In contrast, reductions in the share of land allocated to the main crops are significantly associated with higher crop sequence values. As expected, these associations are stronger at the local level than on average, that is, for CS_{max} relative to CS_{index}.

Table 4: **Estimates of CD variations effects on Crop Sequence variables**

	CS_{max}			CS_{index}		
	(1)	(2)	(3)	(1)	(2)	(3)
$\Delta n.crops$	0.123***	0.033	-0.002	0.073***	0.007	-0.007
	(0.023)	(0.029)	(0.033)	(0.020)	(0.024)	(0.027)
$\Delta area_1$		-0.007***	-0.006***		-0.005***	-0.005***
		(0.001)	(0.001)		(0.001)	(0.001)
$\Delta area_2$			-0.007**			-0.003
			(0.003)			(0.003)
Intercept	3.305***	3.293***	3.289***	2.535***	2.526***	2.525***
	(0.028)	(0.028)	(0.028)	(0.024)	(0.024)	(0.024)
Observations	5,962	5,962	5,962	5,962	5,962	5,962
R-squared	0.004	0.008	0.009	0.002	0.005	0.005
Adj. R-squared	0.004	0.008	0.009	0.002	0.004	0.005

Robust standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.3 Robustness Checks

The robustness of the main RD findings is evaluated using three distinct methodological approaches.

First of all, we assess predetermined covariates. The main idea behind this type of robustness exercise is that, since predetermined covariates should not have been influenced by the treatment, the null hypothesis of no treatment effect should hold if the RD design is properly specified (Cattaneo et al., 2019). Given the results, the analysis is conducted only at the 10-hectare threshold. Moreover, considering the purpose of this exercise—testing the null hypothesis of no effect—the focus is placed more on inference than on point estimation. Therefore, in line with Cattaneo et al. (2019), a coverage error (CER) optimal bandwidth is used instead of the MSE-optimal one. The graphical analysis presented in Figure 10 in the Appendix 7 does not reveal any discontinuities, nor does the formal estimation reported in Table 12.

Secondly, we investigate treatment effects at placebo cutoffs. A core assumption in RD design is the continuity of the regression functions for both treatment and control groups at the threshold in the absence of the intervention. While this assumption cannot be directly tested at the actual cutoff, it is possible to examine whether the estimated regression functions for both groups are continuous at points other than the cutoff itself. Although evidence of continuity away from the cutoff is neither necessary nor sufficient to guarantee continuity at the cutoff itself, systematic discontinuities at placebo thresholds may raise concerns regarding the credibility of the RD design (Cattaneo et al., 2019). Tables 13, 14 and 15 in Appendix 7 report the analysis for artificial cutoffs of the main variables examined at the 10-hectare threshold. The first-stage estimates display statistical significance at the 9- and 11-hectare cutoffs, but with coefficients of opposite sign relative to the policy cutoff. However, this significance does not extend to the outcome variables of interest at the 11-hectare threshold. At the 9-hectare cutoff, statistical significance emerges in the ITT estimates for both the Shannon index and the share of the main crop, again with coefficients of opposite sign compared with those observed at the 10-hectare policy threshold. Nevertheless, these effects lose statistical significance once the analysis is restricted to compliers through the estimation of the fuzzy parameter (see Table 15). Overall, these results provide further support for the absence of systematic discontinuities at alternative cutoffs.

Thirdly, the same analysis at the 10-hectare threshold—excluding crop-rotation indicators, as these are defined only for 2022, as discussed above—is also repeated for 2021. The results, presented in Table 16 in Appendix 7, are similar to the main findings reported for 2022, further reinforcing the stability of the empirical evidence.

6 Discussion

The primary objective of this analysis was to assess the impacts of the CD measure within the 2014–2020/22 Green Payments framework in terms of changes in land-use practices and environmental performance at the local level. The results indicate that, in Tuscany in 2022, the CD rule contributed to the intended adjustments in land-use patterns and to improvements in environmental performance, particularly among medium-small farms located just above the 10-hectare threshold. By contrast, no statistically significant effects emerge for the 30-hectare threshold, where evidence remains largely inconclusive.

The analysis conducted at the 10-hectare threshold provides clear and statistically robust evidence that, for small and medium-sized farms, the implementation of CD requirement has operated as intended. In fact, beyond highlighting positive evidence in terms of farm participation in the measure, the results also indicate that among compliers, there are signs of increased involvement in sustainable agricultural practices. Specifically, in addition to an increase in the number of crops cultivated and a reduction in the share of arable land allocated to the main and second crop, these farms also exhibit an increase in the Shannon index, suggesting potential improvements in local environmental performance.

By contrast, at the 30-hectare threshold, only a higher probability of reducing the share of land allocated to the main crop and to the two main crops, for those who are eligible, emerges as statistically significant following the implementation of the policy. These results are not supported by other consistent evidence. Overall, there is no clear indication that the measure has effectively resulted in participation or fostered a shift toward more sustainable agricultural practices among larger farms—holdings that, given their scale, are also likely to generate more substantial impacts. These findings are consistent with the most recent contributions in the literature (Brutti et al., 2025; Diop & Védrine, 2025), particularly those based on similarly detailed administrative data (Brutti et al., 2026).

Several substantive explanations may help account for these results, including the fact that larger farms may already comply, at least partially, with diversification requirements. As documented in the literature, farm expansion is generally associated with an increase in the number of cultivated crops, although this growth tends to be accompanied by a less-than-proportional increase in crop diversity. In such circumstances, the CD requirements may fail to constitute a sufficiently binding constraint to induce additional behavioural adjustments. This may apply both to the minimum number of crops and to the limits imposed on the share of arable land allocated to the main and two main crops, which may remain relatively high and therefore insufficient to trigger substantive changes among farms that already "naturally" diversify as their size increases (Brutti et al., 2026). In this respect, it should be recalled that the thresholds were the outcome not only of technical evaluation but also of political negotiation, which may have influenced their stringency and practical effectiveness (Pe'er et al., 2014). However, the lack of statistically significant effects may not only reflect potential weaknesses in policy design, but also be partly driven by the limited number of observations available around the 30-hectare threshold, which may reduce the statistical power of the analysis.

Moreover, the identification of a fuzzy RD design, together with the estimation of a broader set of distinct effects, allows for a more structured discussion of policy design and implementation, not only in the Italian context but also in other European countries that adopt similar implementation approaches. In particular, comparing the effect of being eligible for the policy with the effect among those who actually respond to it helps to better understand how the policy works in practice. It shows that the strongest changes occur among farms that are able to adjust their behaviour in response to the incentive, at least at the 10-hectare threshold. This suggests that national implementation should aim to better align eligibility status with actual participation, as determined by the area covered by the Basic Payment Scheme (BPS), thereby strengthening the overall local effectiveness of the policy. At the same time, it underscores the crucial importance of highly granular administrative data, without which such discrepancies between formal eligibility and actual uptake would remain unobserved. The ability to identify and quantify these mechanisms is therefore not merely a methodological refinement, but a necessary condition for a more transparent and equitable evaluation of policy effectiveness.

Finally, an additional noteworthy aspect emerging from the analysis concerns the relationship between CD and CR. While the policy approach—by prioritising diversification over rotation—might have suggested a potential trade-off between the two practices, despite their underlying conceptual affinity, the empirical evidence does not support this interpretation. The regression analysis reveals an overall positive association between changes in land-use practices related to crop diversification and crop-rotation indicators, suggesting that greater crop diversification may encourage the adoption of rotational strategies. In particular, the results suggest that the ability to reduce the share of land allocated to the main crop may represent a key mechanism through which crop diversification promotes crop-rotation practices.

7 Conclusions: Policy Implications, Limitations, and Future Research

CAP is one of the most influential policies at the European level, shaping land-use practices across rural areas. Within this framework, the GPs—particularly the CD measure—are designed to encourage the adoption of more sustainable agricultural practices, while enhancing farms' environmental performance and long-term competitiveness. The heterogeneity of findings regarding the environmental and land-use effects of CD in previous ex-post analyses motivated a deeper investigation using a highly comprehensive and georeferenced administrative dataset. In this respect, this study contributes to the body of ex-post evaluation research by exploiting a detailed parcel-level dataset—beyond the commonly used FADN sources—and by adopting a causal identification strategy. By doing so, we addressed the need for greater data quality and accuracy in this type of analysis, thereby providing more robust insights, as highlighted in the literature.

The results show that the implementation of the crop diversification requirement led to positive changes in land-use practices and environmental performance, particularly for medium-small farms in Tuscany in 2022. However, similar conclusions do not hold when shifting the focus to larger farms, suggesting a missed opportunity for greener practices among these entities.

The sources of this discrepancy, as seen, may be manifold. Among them, although the paper addresses potential methodological concerns by relying on a detailed dataset with broad coverage of the local population, some limitations may still persist around the 30-hectare threshold due to the relatively small number of observations. As discussed above, this may reduce the statistical power of the analysis and partly explain the lack of statistically significant effects. At the same time, however, this pattern may also reflect a more structural limitation related to policy design. Indeed, the results raise questions about whether the design of the CD requirement itself—both in terms of 30-ha threshold and permitted concentration levels—is sufficiently stringent or appropriately calibrated to induce behavioural change among larger farms in Tuscany. In such contexts, these farms may not perceive the measure as a binding constraint, as their production models may already incorporate a certain degree of diversification. Alternatively, depending on the organisation of production and the degree of specialisation (e.g. cereal-based systems or specialised horticultural production), the feasibility and economic attractiveness of further diversification may be limited, as even allocating a relatively small share of land to additional crops may be economically disadvantageous, thereby reducing the incentive to adjust. However, the fact that these findings are consistent with existing evidence in the literature, including studies based on similarly detailed datasets, opens up broader reflections beyond the Tuscan context. In particular, it points to the need to better tailor these measures to the structural characteristics of local agricultural systems, in order to identify more appropriate and effective policy criteria capable of inducing change among larger farms, which, given their scale, are also more likely to generate substantial environmental impacts. In doing so, a possible way forward could involve combining common policy objectives at the European level with greater flexibility in implementation—also at regional, in addition to national, levels—as a promising avenue for enhancing the effectiveness of land-use policies.

Furthermore, the exploratory analysis of the relationship between crop diversification and crop-rotation practices provides evidence of a positive association between these two sustainable farming practices. From a policy perspective, these findings suggest that interventions aimed at promoting crop rotation may be more effective if they encourage changes in crop diversification, particularly through reducing the share of land allocated to the main crop. Given the practical difficulties associated with directly promoting crop-rotation practices, acting on specific dimensions of crop diversification may represent an indirect and potentially more feasible policy lever to foster more sustainable crop-rotation behaviour. Although the regression analysis is exploratory and does not account for other factors that may influence crop-rotation decisions, it nevertheless provides novel empirical evidence on the relationship between crop diversification and crop-rotation dynamics.

Moreover, with specific reference to the Italian context, it is also important to underline that the results obtained call for renewed attention to the issue of internal convergence in the value of BPS entitlements associated with arable land. In particular, greater alignment in entitlement values and the possibility of extending BPS eligibility to the entire national UAA could contribute to reinforcing and amplifying the positive effects of the policy, at least for smaller realities.

A broader policy implication emerging from this study further concerns the strategic importance of investing in high-quality, granular administrative data infrastructures. The ability to exploit detailed, georeferenced parcel-level information proved crucial for identifying heterogeneous treatment effects and disentangling behavioural responses across farms. This suggests that strengthening data collection systems—particularly by integrating environmental indicators that can be spatially linked to farm-level information—is not merely a technical improvement, but a necessary condition for more effective policy design, monitoring, and evaluation of green agricultural policies.

This study is subject to several limitations that should be acknowledged. First, the results identify a Local Average Treatment Effect (LATE), as derived from the RDD framework, which restricts the external validity of the findings beyond the immediate vicinity of the cutoff. Second, the available data do not allow us to identify or distinguish between farms that were already compliant with crop diversification requirements prior to the reform and those that adjusted their behaviour in response to it. Third, although we employ a set of indicators that are arguably more informative from an environmental perspective, environmental performance is still assessed indirectly. This limits our ability to fully capture the actual environmental impact of the introduction of sustainable agricultural practices. Moreover, although the data allow for an accurate methodological design compared with previous studies, they do not allow us to fully rule out potential avoidance or strategic compliance behaviours, which may attenuate the observed effects. Finally, the approach adopted to assess the relationship between CD and CR practices should be regarded as an initial and exploratory attempt. Future research should therefore aim to further substantiate and refine these findings through more robust empirical strategies capable of strengthening their validity.

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Supplementary Materials

Dataset coverage scope

The following table shows the dataset's coverage scope (ARTEA) by comparing figures on AL and UAA with those from the latest national census (ISTAT, 2022).

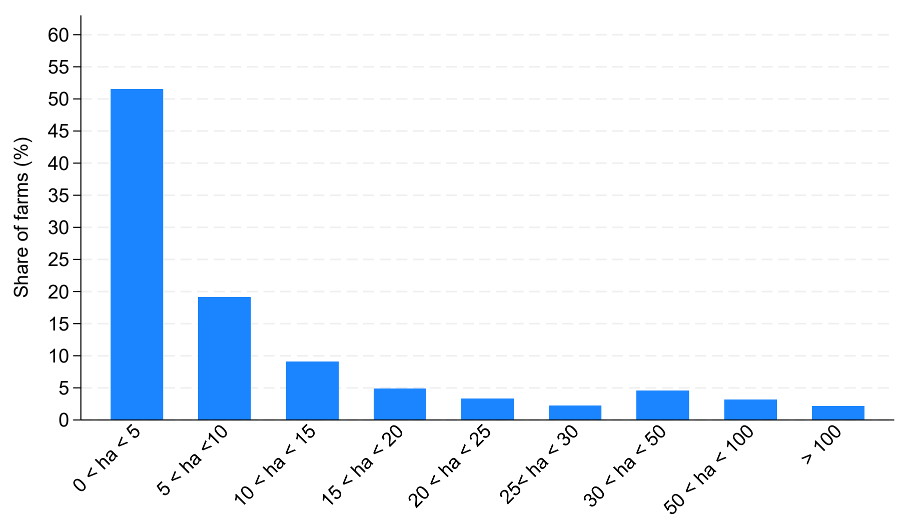
Table 5: **Dataset coverage scope**

Source	**Category**	**Surface (Ha)**
ISTAT (2022)		
	UAA Tuscany (2020)	640,110
	AL Tuscany (2020)	440,829
ARTEA DATASET		
	UAA Tuscany (2022)	611,657
	AL Tuscany (2022)	416,117

Source: Authors' elaboration based on ARTEA dataset and ISTAT (2022).
Note: UAA = Utilised Agricultural Area; AL = Arable Land.

Distribution of farms benefiting from Greening Payments by AL size classes (2022)

Figure 3: **Share of GP beneficiary farms by AL class**



Source: Authors' elaboration based on ARTEA dataset (percentages values, 2022).

Data Cleaning Procedures

The data cleaning process entails the exclusion of the following categories of farms: (i) farms with incomplete information (e.g., cases where the GCP was erroneously missing); (ii) farms included only because they applied for non-CAP subsidies; (iii) farms with some fields in Tuscany but headquartered outside the region and therefore paid by other Italian regional agencies; (iv) farms participating in other CAP measures as well as those not participating in GPs; ((v) holdings exempt from crop-diversification requirements set out in the legislation and already discussed in Section 2, namely those exempted under EU Regulation No. 1307/2013 (European Commission, 2013), Article 44(1), 44(3)(a), and 44(3)(b)—that is, farms with fallow land and/or forage, leguminous crops, rice, or permanent grassland covering more than 75% of the arable area, and/or farms whose entire arable land is under flooded cultivation; (vi) organic farms, which represent another category of farms exempt from the requirements; and (vii) farms without AL.

Crop Diversification Categories

The table presents the macro-categories used to classify arable land (AL) crops cultivated by farms.

Table 6: **Crop Diversification Categories**

Categories	
1. AROMATIC	17. LEGUMINOUS
2. BERRIES	18. MIX
3. CEREALS	19. NUTS
4. CITRUS	20. OILSEED
5. ENERGY	21. OTHER
6. EXTRA: e.g., lake	22. PERMANENT_GRASS
7. FALLOW	23. RICE
8. FIBER and TOBACCO	24. ROOT
9. FOREST	25. TEA
10. FRUIT_TREES	26. TEMPORARY_GRASS
11. GRASSES	27. TREES
12. INDUSTRIAL_OTHER	28. TROPICAL
13. LEGUMINOUS	29. VEGETABLES
14. MIX	30. ORNAMENTAL PLANTS_FLOWERS
15. GRASSES	31. VINES
16. INDUSTRIAL_OTHER	32. OLIVE TREES
	33. OTHER ARABLE

Source: Authors' elaboration based on European regulatory framework.

Table 7: **Summary Statistics**

Category	**Variable**	**Obs**	**Mean**	**SD**	**Min**	**Max**
Running Variable	Arable Land	14,512	11.94	33.84	0.00	1,070
Economic Outcomes	Standard Output	14,512	50,446	205,342	200	1.15e+07
	Standard Output/UAA	14,508	3,797	12,465	27.43	1,050,547
Land-use practices	Number of crops	14,512	1.99	1.20	1	12
	Share of main crop (%)	14,512	81.32	22.18	24.32	100
	Share of two main crops (%)	14,512	95.80	8.74	45.69	100
(Ind.) Environmental Outcomes	Shannon Index	14,512	0.38	0.44	0	1.73
	CS_{max}	7,417	3.27	2.10	0	6
	CS_{avg}	7,417	2.48	1.77	0	6
	CS_{index}	7,417	2.53	1.81	0	6

Source: Authors' elaboration based on ARTEA dataset (2022).

Descriptive Statistics

Crop Rotation Categories

Table 8: **Crop-Rotation Categories**

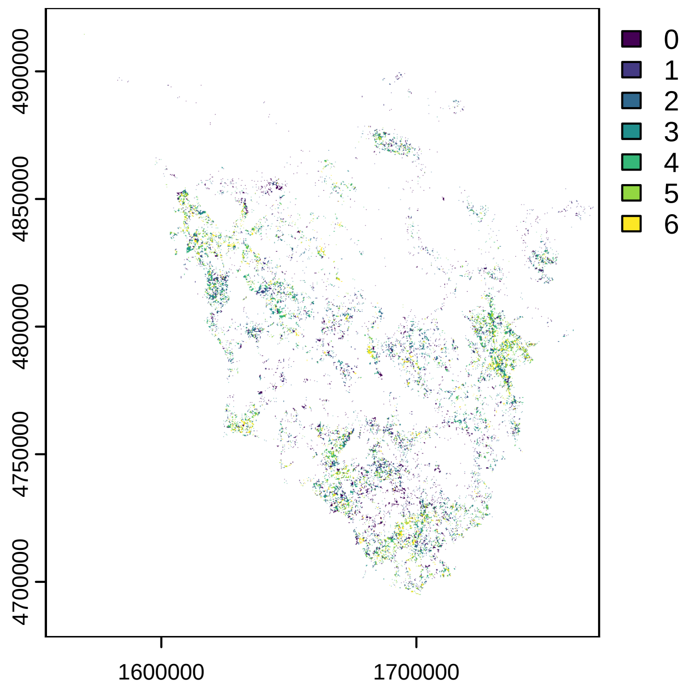
Categories	
1. Cereals	4. Aromatic/Medicinal
2. Legumes	5. Industrial
3. Renewable Crops*	6. Horticultural Crops

Source: Authors' elaboration.

**Renewable crops are short-cycle crops that require deeper soil tillage and are grown to renew and revitalise the soil. They help maintain fertility, control pests, and mitigate soil degradation, thereby supporting crop rotation. In our case, this category includes, for example, sorghum, rapeseed, potatoes, sunflower, sugar beet, fodder beet, and turnip.*

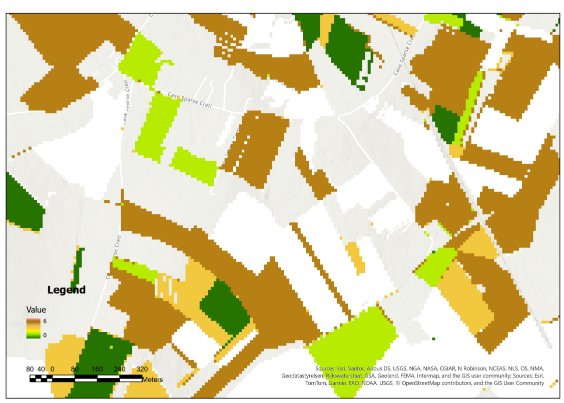
Sum of transition at the pixel level

Figure 4: **Sum of transitions in Tuscany from 2016 to 2022 at the pixel level.**



Source: Authors' elaboration based on dataset ARTEA.

Figure 5: **Sum of crop transitions from 2016 to 2022 at the pixel level (zoom on a selected area of Tuscany).**



Source: Authors' elaboration based on the ARTEA dataset.

Regression Discontinuity within a Fuzzy Design

As illustrated in Table 9, a two-sided non-compliance pattern emerges: some farms located above the cutoff (T assignment = 1), although eligible, do not comply with the crop diversification (CD) requirements, while others, albeit few, below the cutoff (T assignment = 0) do implement it. Although the reasoning appears intuitive for units above the threshold, it may be more complex for those below it (namely, farms whose share of arable land eligible for Green Payments exceeds their currently cultivated area). While specific interactions between the Basic Payment and the Green Payment within these units cannot be ruled out, it is plausible that such cases reflect minor inconsistencies in the administrative records. In the case of the 10-hectare threshold, given their extremely limited incidence (1.36% of eligible farms in the relevant subsample), these observations can reasonably be disregarded, as they do not alter the nature of the identification strategy. With reference to the 30-hectare threshold, such cases account for 13.52% of eligible farms. Although this represents a higher share compared with the lower threshold, the decision to retain these observations is motivated by the reduced overall sample size in proximity to this cutoff. Excluding them would further restrict local variability around the threshold, with potential implications for bandwidth selection and the statistical power of the analysis, which at 30 hectares is structurally more limited.

Table 9: **Fuzzy Design Representation**

	T take-up		**Total**
T assignment	**0**	**1**	
0	11,265	156	11,421
1	257	1,099	1,356
Total	11,522	1,255	12,777

(a) Fuzzy Design at the 10-ha threshold

	T take-up		**Total**
T assignment	**0**	**1**	
0	486	76	562
1	96	1,077	1,173
Total	582	1,153	1,735

(b) Fuzzy Design at the 30-ha threshold

Source: Authors' elaboration based on ARTEA dataset (2022). Note: T indicates Treatment.

Design Validation

7.0.1 Compliance or Manipulation? Strategic Responses to the CD Threshold

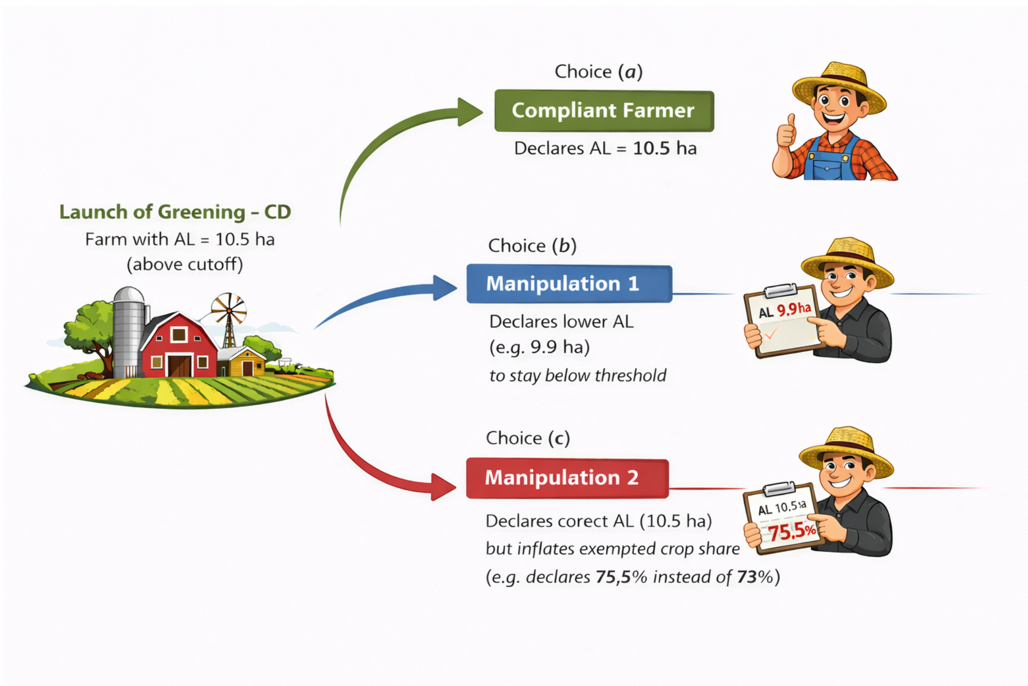
RDD is a valid empirical design if it successfully replicates the conditions of local randomization. In this regard, units should not be able to actively manipulate their score values by changing their treatment status at the cutoff; otherwise, such behaviour would introduce bias and violate the core identifying assumptions of the RDD design.

Let us see how such manipulation could potentially arise within the CD measure and assesses whether it can be ruled out in the present study.

Looking at the design of the CD initiative, two main forms of potential manipulation can be identified. The first—also the most extensively discussed in the literature (see, Brutti et al., 2026)—concerns the possibility that farmers may declare a total amount of arable land (AL) lower than the one actually farmed, thereby remaining below the policy threshold and receiving the greening payment without having to comply with the CD requirements. The second potential source of manipulation relates to the possibility of exploiting the exemptions provided by the regulation—specifically those established under Article 44 of Regulation (EU) No. 1307/2013 (European Commission, 2013). In particular, paragraphs 1 and 3 of the regulation provide an exemption for farms that devote more than 75% of their AL to fallow land and/or forage crops, leguminous crops, rice, or permanent grassland, as well as for farms whose entire agricultural area is under flooded cultivation. In this context, farms may have an incentive to declare a share of AL allocated to these uses above the 75% threshold in order to qualify for the exemption. The regulation also provides an exemption for farms already complying with environmentally friendly practices, such as organic farms. However, this kind of exemption is unlikely to represent a relevant source of manipulation. Aside from the fact that eligibility for this exemption generally requires the farm to already hold organic status, transitioning to organic farming typically requires several years and involves substantial structural organisational adjustments and certification costs; therefore, adopting such a strategy solely to avoid the CD requirements would be highly unlikely and economically irrational.

Based on this, Figure 6 presents a simplified representation of the potential choices available to eligible farms at the CD cutoff of 10 hectares, which represents the main threshold determining eligibility for treatment (i.e., all farms with $AL > 10$ hectares are potentially treated). Consider, for instance, a farm with a total AL of 10.5 hectares. The farm holder may face three possible behavioural choices—besides the option of not applying for any CAP subsidy: (a) complying with the CD requirements or legitimately qualifying for one of the exemptions provided under Article 44 (i.e., a compliant farmer) (choice (a) in Figure 6); (b) declaring a lower amount of AL than actually managed, for instance reporting 9.9 hectares in order to remain below the threshold (choice (b) in Figure 6). If farms misreported their AL strategically, it would be more plausible for them to declare a value just below the threshold (e.g., 9.9 ha) rather than a substantially lower value (e.g., 7.7 ha), since declaring much lower levels would imply misreporting a larger share of land and could increase the likelihood of detection through administrative checks; or (c) declaring the correct total amount of AL but applying for one of the exemptions under Article 44 while misreporting the eligibility requirements—for instance by declaring a share of AL devoted to the exempted crops that is higher than the one actually cultivated (choice (c) in Figure 6).

Figure 6: **Potential behavioural responses of farms around the 10-hectare CD threshold**

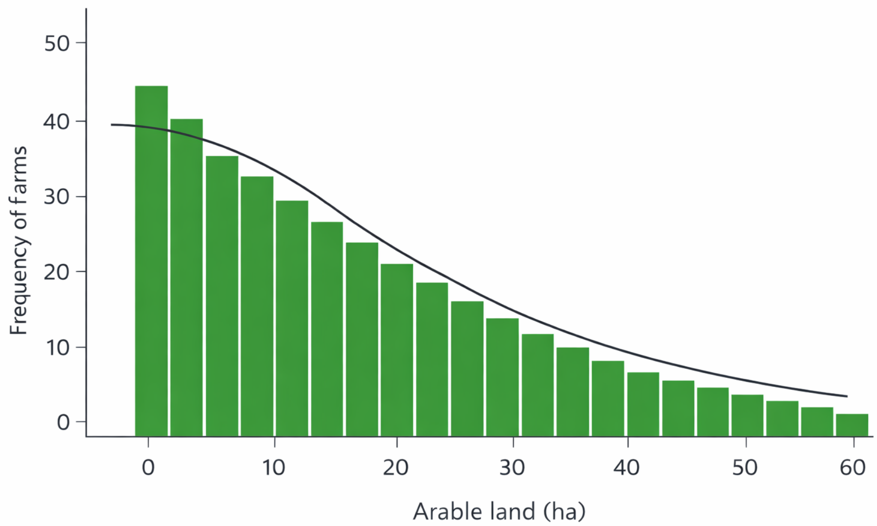


Source: Authors' elaboration. *Note:* The figure illustrates the possible behavioural responses of a farm located just above the 10-hectare arable land threshold determining eligibility for the Crop Diversification (CD) requirement. Farms may either comply with the policy requirements or strategically manipulate declarations by underreporting arable land or inflating the share of crops qualifying for exemption under Article 44 of Regulation (EU) No. 1307/2013.

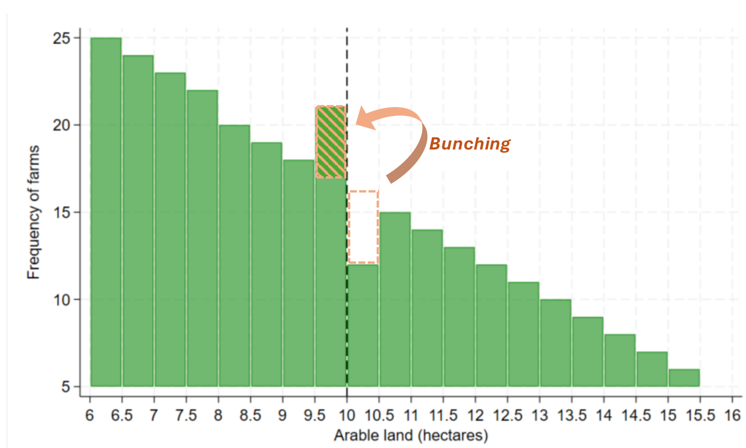
Against this background, how can we evaluate whether farms located around the cutoff predominantly comply with the policy requirements (choice (a)) rather than strategically manipulating their declarations (choice (b) or (c)), thereby supporting or challenging the validity of the RD design?

In principle, the distribution of the population of farms by AL size typically follows a smooth declining pattern, whereby the frequency of farms decreases as farm AL size increases, without exhibiting discontinuities or jumps at specific thresholds. An example of such a distribution is depicted in Figure 7 panel (a). Starting from this premise, the observation of a distribution of this type allows us to rule out manipulation of the first type. Indeed, if farms strategically underreported their total AL in order to remain below the policy cutoff (as in choice (b) of the example illustrated in Figure 6), such behaviour would generate an abnormal accumulation of farms just below the threshold—i.e., a sorting of units around the cutoff. This, in turn, would produce a visible discontinuity in the density of observations at the threshold, reflecting the relocation of units from just above to just below the

Figure 7: **Distribution of farms by arable land size and manipulation type 1**



(a) Generic distribution of farms by AL size



(b) Bunching around the threshold (Manipulation 1)

Source: Authors' elaboration.

cutoff, as schematically illustrated by the pink dashed rectangle in Figure 7, panel (b). The presence of such patterns, especially when confirmed by density tests, may suggest potential manipulation of AL by farmers (manipulation type 1) and thus threaten the validity of the RD design (Cattaneo et al., 2019).

However, some important qualifications must be made in this regard. First, it is necessary to examine the distribution curve in its entirety. Depending on the type and amount of data available, the overall distribution may appear irregular or "jagged", reflecting the limited number of observations in certain bins (Brutti et al., 2026; Cattaneo et al., 2019). This is also the case in our study, particularly when examining the 30-hectare threshold, as depicted in Figure 1. Indeed, despite the dataset providing good coverage of the entire population of Tuscan farms, the number of observations within specific bins remains relatively limited. In such cases, the presence of local fluctuations in the distribution cannot be interpreted as evidence of manipulation type-1 per se. In particular, if the entire distribution displays this jagged pattern, the mere occurrence of one of these fluctuations at the policy threshold cannot be taken as proof of strategic manipulation type-1 by farmers. In this respect, moreover, the density tests conducted at both the 10- and 30-hectare thresholds confirm, at least in our case, the absence of potential type-1 manipulation.

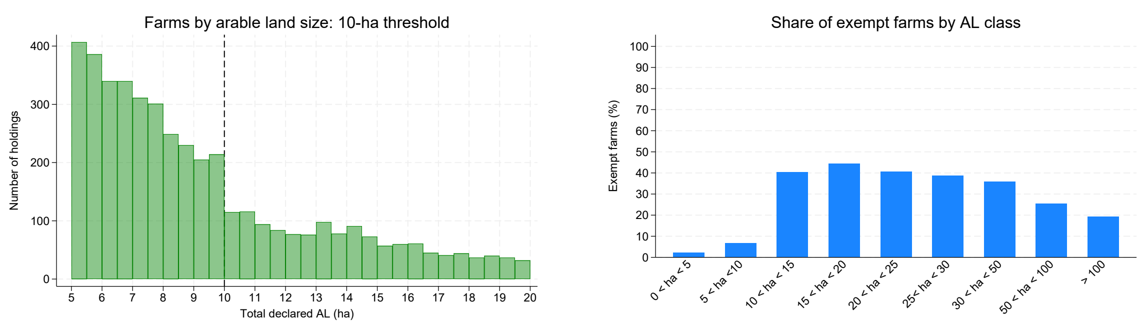
Secondly, the case illustrated in Figure 7, panel (b), highlights manipulation type-1 behaviour concentrated among farms located within the first 0.5 hectares below the threshold. More generally, manipulation type-1 may also occur within a wider range around the cutoff. If manipulation type-1 extended over a wider range, the density of observations above the threshold would become progressively depleted, while the segment immediately below the policy cutoff would become increasingly crowded as farms bunch just below the threshold. This would generate a sharp drop in the distribution at the cutoff. Above the threshold, a local depression in the density would emerge—potentially larger than the one depicted in Figure 7 panel (b)—plausibly reflecting the same mass of units that accumulated immediately below the cutoff. More generally, the wider the potential range of manipulation, the wider this depressed segment of the distribution would appear. As farm AL size increases further, however, the density would gradually return to its underlying pattern, as the scope for manipulation is expected to diminish. Indeed, it is reasonable to expect that the extent of such behaviour declines as the distance from the threshold increases. Farms located further away from the cutoff would need to underreport a larger share of their AL in order to appear non-eligible, thereby increasing both the magnitude of the misreporting and the likelihood that such discrepancies are detected.

Third, it is important to note that, without further specific investigation, this type of analysis does not allow other forms of manipulation to be ruled out, such as the behaviour we define as manipulation type 2 in Figure 6. This therefore raises an additional issue that, in our view, deserves specific consideration.

In particular, attempting to account for manipulation type 2 through the analysis of manipulation type 1 should not involve removing such farms from the observed sample. Excluding these units would mechanically generate a discontinuity in the distribution starting at the 10-hectare threshold. Given the design of the policy, exemption mechanisms determined by art. 44 (1) and (3), can only apply to farms located above this threshold. As a result, farms qualifying for these exemptions are, by construction, predominantly concentrated above 10 hectares. If these observations were removed from the dataset used to test the density of observations around the cutoff, the resulting test would artificially generate a spurious discontinuity in the distribution. This situation also differs from other types of exemptions, such as organic farms, whose presence, by construction, is independent of the policy threshold and therefore plausibly distributed along the entire AL size distribution. By contrast, farms exempted under Article 44(1) and (3) are structurally linked to the policy design and thus appear primarily above the threshold.

This is confirmed by our data. Figure 8, panel (a), shows that removing these farms generates a noticeable depression in the distribution after the 10-hectare threshold. However, it is worth noting that this pattern differs from what would be expected under type-1 manipulation. In this case, indeed, the sharp drop immediately after the threshold is followed by a relatively flat segment extending over a wider range than that typically expected under type-1 manipulation. Under this scenario, the density remains relatively stable above the cutoff rather than quickly returning to its underlying trend, reflecting the fact that the observed discontinuity is driven by the removal of exempted farms, which are likely to be distributed across different farm sizes above the 10-hectare threshold. This interpretation is further supported by the absence of any concentration of observations (sorting) just below the cutoff corresponding to the units missing above it. In other words, the typical bunching pattern illustrated in Figure 7, panel (b), does not emerge when exempted farms are removed.

Figure 8: **Exempted farms according to art 44 EU Reg. N 1307/2013**



(a) Distribution of farms by AL excluding exempted farms

(b) Percentage of exempted farms by AL classes

Source: Authors' elaboration.

Based on this, the dataset used in our analysis is therefore enriched with these exempted farms only when assessing the potential presence of type-1 manipulation. These observations are subsequently removed when conducting the counterfactual analysis, as it is essential to compare homogeneous groups above and below the cutoff.

This, in turn, calls for additional checks to detect potential patterns of type-2 manipulation, the extent of which also depends on the richness of the information contained in the dataset. One of these concerns the expected shape of the distribution of farms subject to these exemptions. In principle, such farms should not be disproportionately concentrated immediately above the 10-hectare eligibility threshold; rather, they are plausibly distributed along the range of farms located above the cutoff. This is indeed what can be observed in our dataset, as illustrated in Figure 8, panel (b), which depicts the share (percentage) of these farms across AL size classes. The figure clearly shows—consistently with the expected pattern—that the share of exempted farms rises sharply immediately after the 10-hectare threshold (while the few observations below this threshold are negligible and likely due to reporting or classification errors) and then remains relatively stable, in proportional terms, up to the AL class of 50 hectares.

Further checks may involve cross-referencing information derived from exemption requests with data on the crops actually cultivated in the fields. In our case, such checks were conducted and reveal no explicit signs of type-2 manipulation. This evidence further supports the interpretation of the density analysis related to manipulation type 1 and strengthens the validity of our RD design and RD manipulation test as seen in Section 4.

Density test

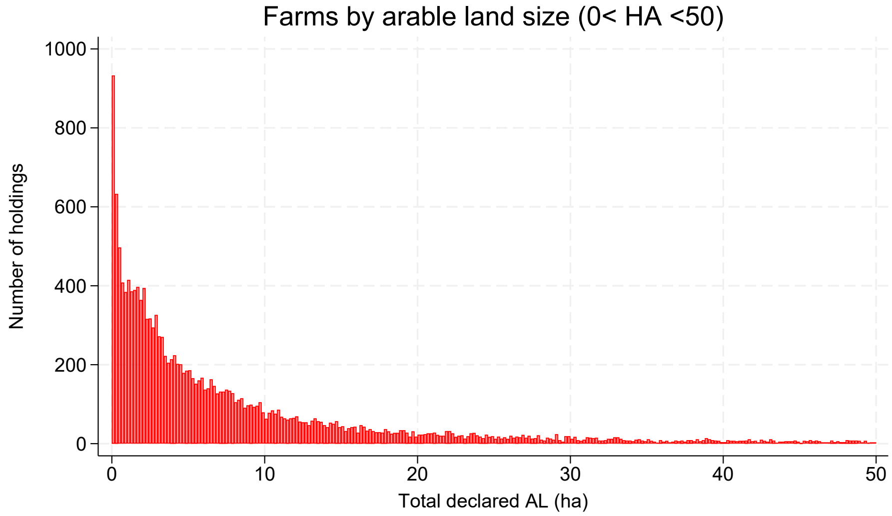
Table 10: **RD manipulation test: density continuity at the cutoff**

	cutoff = 10	**cutoff = 30**
Test statistic (T)	−1.384	−0.502
P-value	0.166	0.615
Eff. observations	2621	291
h left of c	2.933	2.285
h right of c	3.037	2.288

Source: Authors' elaboration. *Notes:* "Eff. observations" denotes the effective sample used around the cutoff; *h* are side-specific bandwidths from the local polynomial density estimator.

Farms distribution between 0 and 50 hectares

Figure 9: **Farms by AL size classes**



Source: Authors' elaboration based on ARTEA dataset.

Results at 30-Ha threshold

Table 11: **ITT: policy assignment on CD outcomes (c=30 ha)**

	RD Estimator	*Robust 95% CI*	*h*	*Eff. Observations*
Land-use practices				
Number of Crops	0.166	[-0.379, 0.517]	8.425	698
Share of main crop	-5.020*	[-13.50, 0.484]	7.869	635
Share of two main crops	-3.943*	[-9.370, 0.256]	9.034	752
(Ind.) Environmental Outcomes				
Shannon Index	0.091	[-0.032, 0.236]	8.811	731
CS_{max}	-0.020	[-0.872, 0.484]	8.213	619
CS_{avg}	0.267	[-0.499, 0.754]	9.214	704
CS_{index}	0.383	[-0.381, 0.860]	9.416	721

Source: Authors' elaboration. *Notes:* ***, **, * denote 1%, 5%, and 10% significance levels; *h* = bandwidth; c= cutoff; Kernel: triangular.

Robustness Checks

Predetermined covariates

Figure 10: **Robustness check with predetermined covariates**

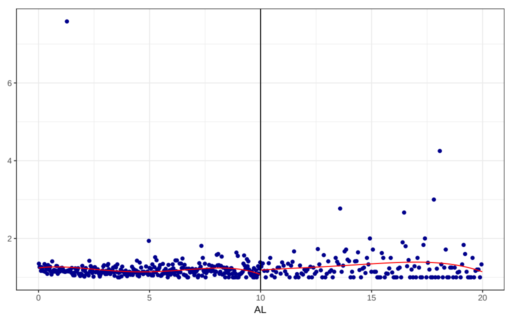


Figure 11: `No. of partners`

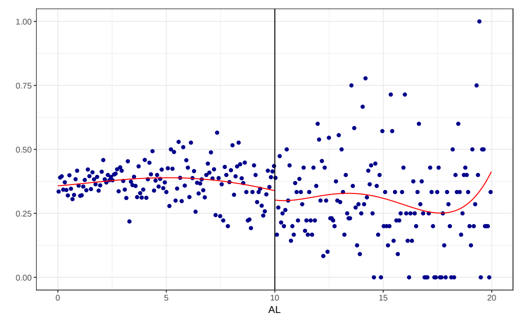


Figure 12: `Only women`

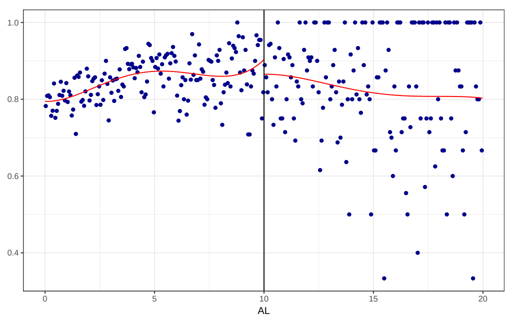


Figure 13: `Sole Proprietorship`

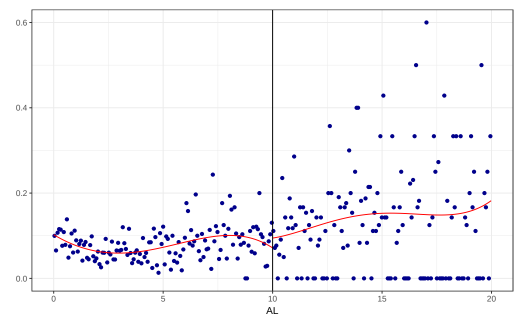


Figure 14: `Partnership`

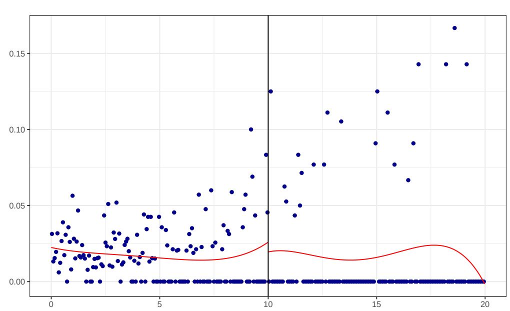


Figure 15: `Joint-stock companies`

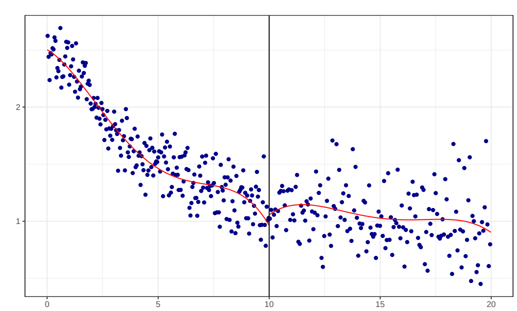


Figure 16: `No. of parcels`

Source: Authors' elaboration. Note: ES bins.

Table 12: **Formal continuity-based analysis for covariates**

Variable	**CER-Optimal bandwidth**	**RD Estimator**	**Robust Inf. P-value**	**Robust Inf. CI**	**Eff. Numb Obs**
No. of partners	1.639	0.045	0.531	[-0.116,0.225]	1067
Only women	1.524	-0.063	0.395	[-0.205, 0.080]	984
Sole proprietorship	1.893	0.015	0.739	[-0.076, 0.108]	1234
Partnership	2.178	0.002	0.888	[-0.068, 0.078]	1437
Joint-stock companies	2.099	-0.005	0.769	[-0.047, 0.034]	1373
No. of parcels	1.889	0.006	0.944	[-0.177, 0.190]	1231

Source: Authors' elaboration.

Placebo cutoffs

Table 13: **Placebo cutoffs: First Stage Effects**

Alternative cutoffs	**MES-Optimal bandwidth**	**RD Estimator**	**Robust Inf. P-value**	**Robust Inf. CI**	**N. obs left**	**N. obs right**
7	1.217	-0.008	0.705	[-0.053,0.035]	854	718
8	1.013	-0.025	0.329	[-0.096, 0.032]	620	486
9	1732	-0.102	0.003	[-0.186, -0.036]	925	583
10	3.085	0.476	0.000	[0.362, 0.556]	1563	584
11	1.410	-0.174	0.015	[-0.384, -0.041]	399	242
12	1.936	-0.004	0.700	[-0.128, 0.191]	391	318
13	1.752	-0.109	0.115	[-0.303, 0.032]	281	306

Source: Authors' elaboration.

Table 14: **ITT estimates for alternative cutoffs**

Cutoffs	**ITT: Shannon Index**		**ITT: N_crops**		**ITT: Main_crop**		**ITT: Two_main_crops**	
	RD Estimator	**Robust 95% CI**	**RD Estimator**	**Robust 95% CI**	**RD Estimator**	**Robust 95% CI**	**RD Estimator**	**Robust 95% CI**
7	-0.026	[-0.089, 0.058]	-0.015	[-0.168, 0.209]	1.482	[-2.896, 5.495]	-0.335	[-1.843, 0.987]
8	-0.007	[-0.085, 0.109]	-0.034	[-0.239, 0.266]	0.152	[-6.022, 4.297]	0.201	[-1.964, 1.844]
9	-0.084**	[-0.198, -0.002]	-0.142	[-0.421, 0.068]	4.786**	[0.430, 10.949]	1.003	[-0.754, 3.073]
10	0.216***	[0.133, 0.315]	0.491***	[0.252, 0.785]	-10.778***	[-15.963, -6.266]	-2.621**	[-5.051, - 0.598]
11	-0.023	[-0.153, 0.075]	-0.043	[-0.399, 0.330]	1.833	[-3.011, 8.698]	-0.969	[-4.058, 2.130]
12	-0.039	[-0.066, 0.180]	0.036	[-0.323, 0.368]	-1.803	[-8.986, 3.700]	-0.295	[-4.033, 3.090]
13	-0.014	[-0.154, 0.108]	0.113	[-0.330, 0.520]	1.258	[-5.164, 8.484]	-0.543	[-4.653, 2.967]

Source: Authors' elaboration. Note: ***, **, * denote 1%, 5%, and 10% significance levels, respectively.

Table 15: **FRD Estimates for the 9 ha Cutoff**

Cutoff	**FRD: Shannon Index**		**FRD: Main Crop Share**	
	RD Estimator	**Robust 95% CI**	**RD Estimator**	**Robust 95% CI**
9	1.115	[-1.259, 3.120]	-73.53	[-193.3, 73.17]

Source: Authors' elaboration.

Notes: ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

RDD Replication for 2021

Table 16: **RD results at 10-ha in 2021**

	RD Estimator	*Robust 95% CI*	*h*	*Eff. Observations*
First Stage	0.226**	[0.053, 0.342]	1.459	928
Intention-to-Treat Results				
Land-use practices				
Number of Crops	0.504***	[0.266, 0.772]	4.125	3030
Share of main crop	-10.09***	[-14.74, -4.215]	2.825	1934
Share of two main crops	-1.773	[-3.919, 0.874]	2.630	1780
(Ind.) Environmental Outcomes				
Shannon Index	0.200***	[0.096 , 0.288]	3.178	2200
Fuzzy Parameter				
Land-use practices				
Number of Crops	1.415***	[0.710, 2.444]	3.258	2271
Share of main crop	-28.39***	[-44.38, -17.12]	3.640	2607
Share of two main crops	-5.324*	[-12.44, 1.037]	3.055	2109
(Ind.) Environmental Outcomes				
Shannon Index	0.547***	[0.333, 0.855]	3.648	2612

Source: Authors' elaboration. *Notes:* ***, **, * denote 1%, 5%, and 10% significance levels; *h* = bandwidth; Kernel: triangular.