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Received: 11 October 2023 | Accepted: 9 December 2024

DOI: 10.1111/ajae.12512

ARTICLE

The effect of result-based agri-environmental payments on biodiversity: Evidence from Switzerland

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Abstract

We estimate the effects of result-based agri-environmental payments on biodiversity using a unique dataset containing information about plant vegetation. The data include information on surveyed plant species for a large number of randomly selected plots followed over a period of 20 years in Switzerland. In our estimation, we utilize a difference-in-discontinuities approach based on exogenous variation in payments triggered by (i) a policy reform in Switzerland that led to a considerable increase in payments that was uncertain prior to the implementation and (ii) an administrative threshold of reform that defines eligibility for payment depending on the botanical quality. We find that the increase in result-based payments led to an increase in the biodiversity of plots that were almost eligible for the payments before the reform but not for plots that already satisfied the eligibility criteria. Our findings have important implications for the design of result-based payments.

KEYWORDS

Agri-environmental schemes, biodiversity, outcome-based, performance-based, plant diversity, policy design, result-based, threshold

JEL CLASSIFICATION

Q18, Q15, Q57, Q58

1 | INTRODUCTION

Agricultural intensification and land-use changes severely threaten biodiversity (Foley et al., 2011; Leclère et al., 2020; Pe'er et al., 2014). To reduce the pressure from agriculture on biodiversity,

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governments worldwide have implemented agri-environmental policies. For example, in 2023, the European Union allocated over 15.4 billion Euros, and in 2021, Switzerland allocated over 0.6 billion Euros to "green" measures (European Commission et al., 2023; FSO, 2023; Text A.1). These funds equate to annual allocations per hectare of agricultural land of 94 Euros and 405 Euros in the European Union and Switzerland, respectively. Considerable shares of these funds are paid to farmers in the form of so-called agri-environmental payments.

Despite the financial efforts of governments, the effectiveness of agri-environmental payments to reduce the pressure on biodiversity has been questioned (Navarro & López-Bao, 2019; Pe'er et al., 2019, 2020, 2022). To improve the effectiveness, result-based payments were introduced[1] in several European countries (Burton & Schwarz, 2013; Mack et al., 2020; Elmiger et al., 2023). As the name suggests, the eligibility for result-based payments depends on the attainment of a certain environmental result, usually assessed via thresholds. For example, Swiss farmers are eligible for payments when at least six plant indicator species are present on their grassland (e.g., Elmiger et al., 2023). However, despite the increasing importance of agri-environmental payments as policy instruments and experts' promotion of using result-based payments, there is little empirical evidence of their effects on biodiversity (Burton & Schwarz, 2013; Elmiger et al., 2023; Herzog, 2005; Kelemen et al., 2023; Navarro & López-Bao, 2019; Pe'er et al., 2019, 2020, 2022). Further, previous studies evaluating policies based on administrative thresholds (e.g., eligibility rules) showed that such policies may introduce differential incentives at the threshold (e.g., Urquiola & Verhoogen, 2009). In this paper, we aim to close this gap by evaluating the effect of result-based agri-environmental payments on biodiversity measured in the field. Specifically, we focus on the impact of thresholds in result-based payments, which is a crucial design feature of such payments (e.g., Burton & Schwarz, 2013; Elmiger et al., 2023).

A major challenge for the empirical evaluation of result-based payments is measuring biodiversity. Measuring biodiversity through the number of, for example, plant and insect species is costly; therefore, typically, only a limited number of datapoints are observed over a narrow spatial and temporal dimension (e.g., Engist et al., 2023; Geijzendorffer et al., 2016; Herzog & Franklin, 2016; Targetti et al., 2014; Tsakiridis et al., 2022). Counts of bird species are more widely available but are difficult to assign to a given plot (e.g., Geijzendorffer et al., 2016; Herzog & Franklin, 2016). Therefore, instead of counting species, many studies that assess agri-environmental payments in Europe and the USA[2] have used proxies, such as land enrolled in agri-environmental programs, crop diversification, or pesticide and fertilizer use (Bertoni et al., 2020; Chabé-Ferret & Subervie, 2013; Claassen et al., 2018; Laukkanen & Nauges, 2014; Stetter et al., 2022; Tsakiridis et al., 2022; Wuepper & Huber, 2022). These studies find both positive and no effects, whereas positive effects were often of moderate size and effects were shown to differ across farms. However, the relationship between these proxies and actual biodiversity is not straightforward or can even be misleading. For example, the link between a proxy and biodiversity often varies considerably across space and the landscape context, thereby making the predictions based on one proxy (and even several proxies) highly uncertain (e.g., Baldi et al., 2013; Dormann et al., 2008; Socher et al., 2012). Similarly, the proxy might be noisy when it indicates only whether a farmer has implemented a given pro-environmental measure, but the quality of the implementation is unknown (e.g., hedgerows in good vs. poor conditions) or when the positive environmental impact of a measure (required for agri-environmental payments) is itself unclear[3] (Graham et al., 2018; Montgomery et al., 2020; Pe'Er et al., 2017; Pe'er et al., 2021). Moreover, proxies—such as enrolled land—do not provide a picture of the distributional changes in

[1]These result-based payments complement the more commonly used action-based payments. In action-based payments, farmers receive money for implementing predefined management actions that are considered to have beneficial environmental impacts. However, the eligibility for action-based payments is independent of the realized environmental results. Furthermore, we note that although we use the term "result-based" payments, they are also referred to as "performance-based," "result-oriented," and "outcome-oriented".

[2]Studies that investigated payments for ecosystem services more generally (mostly conducted in developing countries and forest conservation) more often considered environmental outcomes (specifically forest cover) and often found that payments reduce deforestation (e.g., Börner et al., 2017; Wunder et al., 2020).

[3]See, for example, Pe'er et al. (2017, 2021) for a discussion on excluding ineffective options.

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biodiversity or the effect due to thresholds when a payment is result based. Last, farmers might hold back from enrolling plots to payment schemes despite being eligible for them in order to retain management flexibility and avoid administrative costs and monitoring (Sander et al., 2024; Schaub et al., 2023; Schulze et al., 2024). These arguments highlight the importance of also assessing the effects on biodiversity.

An alternative approach in the literature is to use direct measures of biodiversity, such as butterflies, birds, mammals, and plant diversity (e.g., Baker et al., 2012; Kleijn & Sutherland, 2003; Marja et al., 2018; Meichtry-Stier et al., 2014; Roth et al., 2008). The studies often report positive but also nil and negative effects of agri-environmental payments on biodiversity. In their review, Kleijn and Sutherland (2003) show that studies often found no effect of payments on plant communities. Yet, studies utilizing measures of biodiversity mainly focused on the correlations between biodiversity and payment schemes. However, such correlations might misrepresent the actual effects of the policies because of the presence of endogenous selection. In particular, plots that satisfy or almost satisfy the payment scheme eligibility criteria already before the introduction of the payments are more likely to be enrolled in the scheme (e.g., Bertoni et al., 2020; Gailhard & Bojnec, 2015; Gómez-Limón et al., 2019; Hart & Latacz-Lohmann, 2005; Kleijn & Sutherland, 2003). As a result, comparing land enrolled and not enrolled by farmers may yield a spurious positive effect that is due to selection bias. Several studies that utilize measured biodiversity address this selection, but they all focus on action-based payments (not result-based payments). For example, Kleijn et al. (2001), Kleijn et al. (2006), and Knop et al. (2006) addressed endogenous selection into action-based payments by matching pairs (plots enrolled or not enrolled in agri-environmental payment schemes) based on observables, including similar-sized plots and similar environmental conditions, including soil type, groundwater level, and landscape context. The three aforementioned studies reported either no effects or positive effects—albeit often marginal or moderate—of action-based payments on biodiversity. A further study that explicitly addresses endogenous selection in the context of action-based schemes is that of Kleijn and van Zuijlen (2004), which uses an approach related to the difference-in-differences estimation. Their paper found no effect of the payment on the development of bird populations. Overall, these results indicate low effectiveness of action-based payments in many instances.

In this paper, we estimate the effect of result-based payments on actually measured biodiversity, specifically plant diversity. Our main contributions are the use of a unique dataset containing information on plant diversity and the utilization of a difference-in-discontinuities approach that allows us to identify the causal effect of result-based payments on biodiversity for a particular subgroup of grassland plots—that is, those being at the eligibility threshold of the result-based payment scheme. In detail, our dataset contains information on the presence of surveyed plant species for a large number of randomly selected plots, which are followed over a period of 20 years. These data allow us to draw a rich picture of biodiversity and its dynamics in meadows and pastures over time. We utilize exogenous variation in result-based payments triggered by two features of a Swiss agricultural policy reform implemented in 2014 to deal with the potentially endogenous selection into payment. The first feature is that the policy introduced a large increase in payments; however, the precise amount of the increase was unknown and associated with large uncertainty until the new legislation came into effect. The second feature is the administrative threshold of reform that defines eligibility for payments depending on the number of plant species. However, because farmers are not able to perfectly control the number of species around the threshold, the two features of the policy give rise to quasi-random variation in the payments at the introduced eligibility threshold. Given this setup, we can utilize a difference-in-discontinuities design (see, e.g., Grembi et al., 2016; Eggers et al., 2018) to estimate the effect of the increase in payments on biodiversity (i.e., plant species diversity) in grasslands at the eligibility threshold.

We find that the reform of result-based payments created differential incentives to invest in biodiversity. In other words, grassland plots that were just below the eligibility threshold before the policy reform benefited from the reform relative to plots that were just above the eligibility threshold. Specifically, the former increased their number of species, on average, by 0.8 indicator species relative to the latter as a result of the reform, representing a 15% increase compared to the average number of indicator species prepolicy.

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To the best of our knowledge, this is the first study that evaluates the effect of *result-based payments* on biodiversity using both (i) "in-the-field" measured biodiversity and (ii) a design for causal inference that takes endogenous selection into account. Our study using measured biodiversity is closely related to the study of Wuepper and Huber (2022), who evaluate the effect of the same reform on the enrollment of land into the result-based scheme (i.e., policy take-up). They find that an increase of 1% in the payment leads to an increase of 1% in land enrolled to result-based payment schemes. We show that a payment increase may not be beneficial in terms of biodiversity to those plots already eligible. Thus, our results support existing evidence that windfall gains may be detrimental to biodiversity (e.g., Bertoni et al., 2020; Chabé-Ferret & Subervie, 2013; Wuepper & Huber, 2022). Our results also provide support for the hypothesis that an optimal result-based policy design should explore multiple thresholds, which are connected to different payment and biodiversity levels (e.g., Burton & Schwarz, 2013; Elmiger et al., 2023).

Finally, a major additional advantage of focusing on the effect at the threshold is that it provides a reliable source of exogenous variation. Thus, we contribute to the related literature by using an innovative empirical approach to deal with endogenous selection into direct payments. To mitigate the problem of selection bias, related studies have most commonly relied on assumptions about the selection process (selection of observables; e.g., Kleijn et al., 2001, Kleijn et al., 2006; Tsakiridis et al., 2022), parallel trends (i.e., in difference-in-differences estimations), or a mixture of the two (e.g., Bertoni et al., 2020; Wuepper & Huber, 2022). Our approach complements these important contributions by highlighting the use of administrative thresholds in the context of imperfect control of environmental outcomes. Our identification difference-in-discontinuity approach relies on a combination of the assumption of the regression discontinuity approach (e.g., Wuepper & Finger, 2023) with a parallel trend assumption. Specifically, we assume that the outcome trends of plots at the threshold would have been equal in the absence of the treatment. This assumption is weaker than either of the two aforementioned assumptions. It is justified by farmers' imperfect control over the number of plant species on their plots, by the ex-ante uncertain economic incentives as well as by similarities of plots at the threshold.

The paper is structured as follows. In Section 2, we introduce the background of agri-environmental payments in Switzerland and the farmers' decisions to increase biodiversity. In Section 3, we present our data, and in Section 4, we present a microeconomic model and our empirical strategy. In Section 5, we present our results, and in Section 6, we provide a discussion of our results. Finally, in Section 7, we conclude our study.

2 | BACKGROUND

2.1 | Institutional setting

Agri-environmental policies in Switzerland and the European Union were introduced in the 1990s (Détang-Dessendre et al., 2023; Mack et al., 2020). Initially, agri-environmental payments in Switzerland were entirely action based. For grasslands, action-based payments were, and are, given to farmers after they implement certain practices, including strongly reduced or no fertilizer application and a later first cut (Agridea., 2023). In 2001, result-based payments were also included as part of a hybrid payment scheme — that is, a combination of action- and result-based payment schemes (e.g., Elmiger et al., 2023). Currently, separate result-based payments exist for various land uses, such as grasslands, croplands, and vineyards. In this paper, we focus on result-based payments for grasslands.

The eligibility for result-based payments is plot specific and is based on the number of so-called indicator species and indicator species groups growing on a given plot. Indicator species groups include several plant species that either indicate similar biodiversity properties or are difficult to differentiate. Thus, even if more species of one indicator group are present, they are counted only once. For simplicity, in the following account, we refer to indicator species groups as indicator species. As the name suggests, these species are considered important indicators of biodiversity. The precise list of species that qualify as indicator species depends on the type of grassland (meadows or pastures; e.g., Elmiger

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et al., 2023).[4] This difference reflects different ex-ante likelihoods for observing individual species. A farmer of a given plot is eligible for result-based biodiversity payments if six or more indicator species are found within a circular area with a radius of 3 m on the respective plot.[5] Moreover, for meadows and pastures, further differentiation is made by region and for meadows within regions by biodiversity potential (i.e., lower vs. higher). The criteria for the list of indicator species are summarized in Figure 1. Additionally, to those criteria for result-based payments, these payments—as part of a hybrid scheme in Switzerland—also require that the management restriction of action-based payments is met. The contract duration for result-based payments is 8 years (SFC, 2013). Enrolled plots are controlled in order to confirm if they meet the minimum threshold of six indicator species at the beginning of the contract and once more before its conclusion (FOAG, 2019).[6] Thus, farmers are required to ensure that they keep the number of indicator species above the threshold over a prolonged period (see the next section for farmers' management options).

Switzerland's agricultural policy underwent a significant reform in 2013, with major changes taking effect in 2014 (e.g., Mann & Lanz, 2013; Metz et al., 2021; OECD, 2017). One component of this reform was that the agri-environmental payments, including result-based payments, for grasslands increased substantially. On average, result-based payments increased compared to those prior to the reform by 47% and by 215% compared to the payments level in 2001 (for details, see Tables A.3 and A.4).[7] Because, as described above, the threshold of six indicator species represents a discontinuity in the result-based payment eligibility criteria, the introduction of the policy reform might have changed farmers' profit function, as it led to (i) an increased incentive to reach the threshold and (ii) a large potential direct benefit for plots and their owners that already reached the threshold pre-reform. In contrast, action-based payments are independent of any environmental thresholds or payment discontinuities. We exploit this feature in our empirical strategy.

The first draft of the policy reform was presented by the president of the Swiss confederation to the parliament and the public in an official announcement on February 1, 2012 (Meier, 2013). In March 2013, the Swiss Parliament approved the legislation of the reform, passing it on to the hearing stage (OECD, 2017). In the hearing stage, different actors—including the cantons, parties, private actors, and

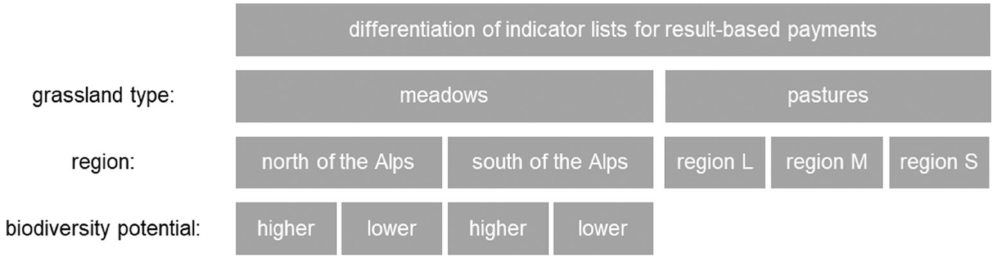

FIGURE 1 Overview of how the indicator lists for result-based payments are differentiated. Region L = Swiss Plateau and northern Alps below an elevation of 1000 m. Region M = Jura, southern Alps below an elevation of 1000 m and northern Alps above an elevation of 1000 m. Region S = southern Alps above an elevation of 1000 m, eastern central Alps and western central Alps. The biodiversity potential is determined based on a different and reduced list of indicator species (see Table A.1). The potential is defined to be high when at least three indicator species from this list are observed. See Tables A.1 and A.2 in the Appendix for an overview of the plant species included in the different lists and Figure A.1 for an overview of the different regions. The lists and threshold remained constant over time (Oppermann & Gujer, 2003).

[4]Additionally, result-based payments for grasslands exist in summering areas. However, we do not include them in the analysis due to the lack of sufficient data.

[5]Depending on the payment, additional conditions can apply (e.g., Elmiger et al., 2023).

[6]The costs for controlling are borne by farmers (FOAG, 2019).

[7]For example, for extensively managed meadows in the valley and hill zones, the payments increased from 1000 CHF to 1500 CHF per hectare and year. The result-based payments were also increased during a policy reform in 2008, but the payment level was significantly lower (Table A.4; SFC, 2007). Apart from the 2008 changes in payments, payments remained constant between 2001 and 2014.

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scientific advisors—are involved to provide input on specifying the content of the reform. Soon after the approval by the Swiss parliament, legislation was challenged by the strongest political party in Switzerland (the Swiss People's Party) and the Swiss farmers' union, which called for a referendum and a popular initiative, respectively, to overturn it (OECD, 2017). The reform was finally adopted in June 2013 by the Swiss Federal Office of Agriculture and the Swiss Federal Council (see Metz et al., 2021 for a detailed overview). However, the precise amounts of the direct payments are not part of the legislation itself. They were established through subordinate decrees after the legislation was passed. The precise content of all changes was then officially communicated by the Swiss Federal Council on October 23, 2013 (SFC, 2013). The new regulations entered into force on January 1, 2014 (Meier, 2013).

The development of the reform and timing of the communication of the precise content described above implies that during the period in which farmers could influence their pre-policy biodiversity outcomes, there was substantial uncertainty regarding the precise economic incentives of the policy reform, such as the size of the payments. We exploit this property in our empirical strategy below.[8]

2.2 | Farmers' Management decision

Farmers' management influences the productivity and biodiversity of grasslands. To achieve higher biodiversity (i.e., plant diversity), farmers have several options. These options include reducing management intensity (e.g., the number of cuts and fertilizer application) and (over)seeding grasslands with species-rich mixtures. The latter is often accompanied by reductions in management intensity. These changes in management practices create additional costs for farmers, such as opportunity costs due to lower yields when management intensity is reduced and costs for expensive seed mixtures (e.g., Huber et al., 2021; Isselstein et al., 2005; Schaub et al., 2021; Török et al., 2011; White et al., 2004). However, species occurrence after management changes is stochastic and, thus, not completely controlled by the farmer. For example, the realized number of species and the effect of management practices on them depends on uncontrollable random historic weather variability and shocks (e.g., droughts), which cause legacy effects and adversely affect the presence of species (Freitag et al., 2021; Gruner et al., 2017; Hedberg & Kotowski, 2010; Ladouceur et al., 2023; Müller & Bahn, 2022; Tilman & El Haddi, 1992; Wagner et al., 2021).

Given that plots are eligible for result-based payments, famers might still decide not to enroll these plots to the payments. This depends on the individual marginal costs and benefits of farmers. For example, farmers might not enroll an eligible plot to the scheme if they wish to remain more flexible in their future management decisions or when other costs of enrolling, such as administrative work and monitoring costs, are too high. This highlights that it is important to use measured biodiversity data to identify policy effects on biodiversity.

3 | DATA

3.1 | Survey data

We use a unique Swiss-wide dataset that contains information about surveyed plant species for the period 2001–2021 ("The Swiss Biodiversity Monitoring Data"; BDM Coordination Office, 2014).[9]

[8]In addition to analyzing the reform of result-based payments, it would also be interesting to analyze their introduction in 2001. However, the data collection of measured biodiversity data only began in 2001 (see Section 3). Thus, we have no information on biodiversity before the policy was introduced.

[9]Previous studies used the data to assess, for example, the correlation of nitrogen deposition and landscape variability with plant diversity (e.g., Hofer et al., 2011; Kammer et al., 2022; Roth et al., 2013). Moreover, Roth et al. (2008) utilized the data to assess the difference in plant diversity between plots enrolled or not enrolled in agri-environmental payment schemes. However, these studies did not consider the selection problem outlined in the introduction section. Thus, the study results, although very important, must be interpreted with caution. Moreover, the studies above did not attempt to disentangle the effects of the different components (action-based vs. result-based incentives) of the considered policies, whereas our threshold-oriented strategy distills the effect of the result-based incentives; for details, see Section 4.

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The data are part of the long-term monitoring effort by the Swiss Federal Office for the Environment to track the development of biodiversity in Switzerland.

The surveyed plots were randomly drawn from an evenly spaced grid across Switzerland.[10] The area of each circular survey plot is 10 m^2. Each plot was surveyed in 1 year[11] every 5 years (BDM Coordination Office, 2014). The data contain information about the presence of all (vascular) plant species on the plot. The recorded data include the plot-specific coordinates and the land use of the plot (e.g., meadow and pasture, cropland, and forest). In our analysis, we are interested only in grasslands in nonalpine areas, including meadows and pastures.[12] Finally, we assigned the number of indicator species to each plot based on the different lists (Figure 1; for details, see Text A.2).

Various quality assurance measures are in place to ensure consistent measurement across space and time of the survey data (BDM Coordination Office, 2014). These measures include the application of error-tolerant survey methods that provide very little room for personal interpretation, blind controls, and electronic recording of species to reduce errors in transferring manually recorded data to databases.

In our main analysis, we focus on the period from 2009 to 2018 for two reasons. First, in this time span, all plots were surveyed once before and once after the 2014 policy reform. Second, between 2009 and 2018, no other significant related policy reforms took place. In further analyses, we consider a wider range of the data in terms of time period.

3.2 | Descriptive statistics

Our final data include 403 plots (i.e., units of observations) for meadows and pastures across Switzerland (Figure 2a), which were surveyed between 2009 and 2018, once before and once after the policy reform. During the entire period covered by the data (2001–2021), 95% of those 403 plots were surveyed at least four times. Figure 2b displays a histogram of the number of indicator species per plot and measurement for the period 2009–2018. In general, we observe that the median number of indicator species remained rather constant since the first recoding in 2001 until the policy reform in 2014, at which point it shifted slightly upward (Figure A.2).

4 | MICROECONOMIC MODEL AND EMPIRICAL STRATEGY

4.1 | Microeconomic model

In this section, we present a simple microeconomic model[13] that provides insights about how farmers might react in response to result-based payments and guidance for our identification strategy. Specifically, we are interested in the farmers' optimal decision to change biodiversity on a plot given result-based payments and depending on the plots initial (i.e., prepolicy) level of biodiversity, Y_0. For simplifying the illustration and without loss of generality, we consider a setting that had initially no payment in place. In the model, we consider a farmer who is profit maximizing and assume that the cost of reaching any level of biodiversity, Y, on a plot of size one depends the initial level of

[10]The origin of the grid was selected randomly, meaning that the precise starting coordinates of the evenly spaced grid were selected at random.

[11]Some plots were surveyed once a year and others two times a year, but the survey frequency was constant for each plot.

[12]Grasslands are defined in the monitoring program based on the habitat type rather than the agricultural land use type; however, the overwhelming majority of the meadows and pastures in the sample are used for agricultural production (see, e.g., Delarze et al., 2015). Furthermore, botanists recorded indirect evidence of grassland use, such as fences; however, no direct information from farmers regarding grassland land use (e.g., use as meadow or pasture) was available (Biodiversity Monitoring Switzerland, 2020). Therefore, we use this information only for a validity check (Section 5.3).

[13]We are very grateful to an anonymous referee for suggesting a first version of this microeconomic model to us.

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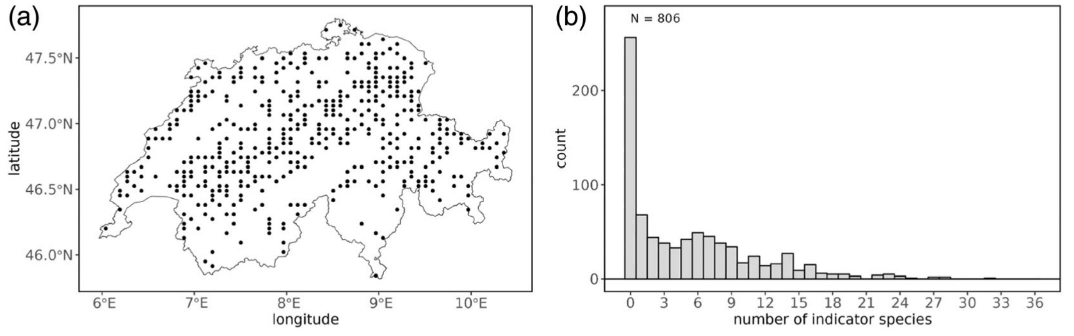

FIGURE 2 Spatial distribution of plots and a histogram of indicator species across plots for the period 2009–2018. See Figures A.3, A.4, and A.5 in the Appendix for a disaggregated overview of the data. The boundaries of Switzerland are taken from swisstopo (2023).

biodiversity, Y_0, with a positive and convex cost function $c(Y-Y_0)$ with $c(0)=0$.[14] The farmers' profits per plot are defined then as:

$$\pi = R\,\mathbb{1}\{Y \geq \overline{y}\} - c(Y-Y_0), \tag{1}$$

with R being the payment farmers are eligible to when reaching or exceeding the payment threshold, $\overline{\boldsymbol{y}}$, and $\mathbb{1}\{\cdot\}$ takes value one when its condition is satisfied and zero otherwise. Additionally, we assume that there exists a value $\widehat{y}$ with $c(\widehat{y}-Y_0)>R$ (Assumption 1), which is plausible because increasing biodiversity increases costs.

Thus, given a payment of amount R the optimal level of biodiversity Y^* on a plot given result-based payments can be expressed in three distinct cases:

$$Y^* = \begin{cases} Y_0 \; if \; Y_0 \geq \overline{y} \\ \overline{y} \; if \; Y_0 < \overline{y} \leq Y_0^* \\ Y_0 \; if \; Y_0 \leq Y_0^* < \overline{y} \end{cases}, \tag{2}$$

where Y_0^* is the endogenous, and plot-specific, reservation threshold of a given payment above which the cost of increasing biodiversity is higher than the such payment. This reservation threshold is defined by $R=c(\overline{y}-Y_0^*)$. Figure 3 provides a visual example of each of those three cases, and Text A.3 the proofs.

Case 1. $Y_0 \geq \overline{\boldsymbol{y}}$, that is the initial level of biodiversity is higher than the payment threshold. In this case, the optimal level of biodiversity is equal to the initial level: $Y^*=Y_0$. This is because changing biodiversity from the initial level incurs costs while revenues remain constant or decline.

Case 2. $Y_0 < \overline{y} \leq Y_0^*$. In this case, the payment threshold is (i) above the initial level of biodiversity, meaning that increasing biodiversity could increase revenues compared to the initial state and (ii) below or equal to the reservation threshold—the point at which the cost of increasing biodiversity on a plot would be higher than the result-based payment. Hence, the optimal response is to increase biodiversity to the level of the payment threshold, that is, $Y^*=\overline{y}$.

[14]Further, we assume that $c(Y-Y_0)$ is twice differentiable on all of its domain. Thus, $c'' \geq 0$. This assumption is made purely to simplify the exposition; however, the proof of our result does not depend on it.

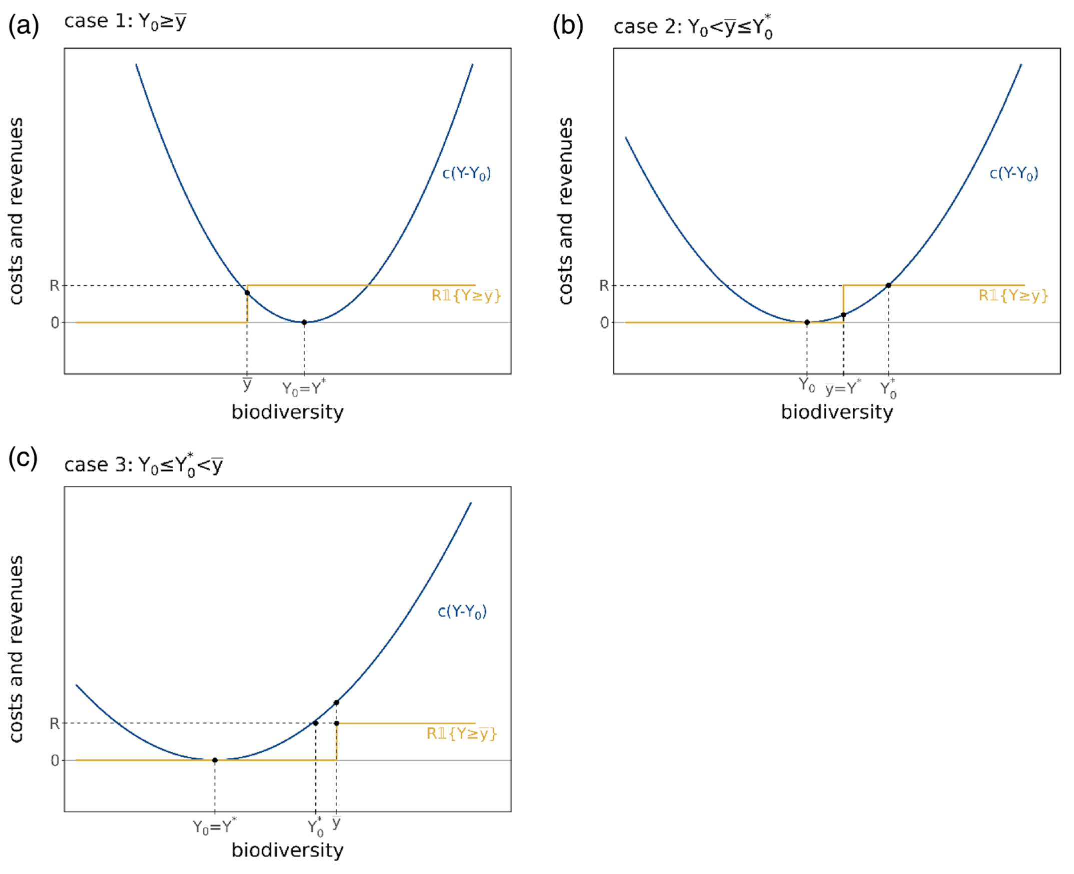

FIGURE 3 Illustration of examples of the three theoretical cases and the resulting optimal level of biodiversity given result-based payments. $\bar{y}$ is the payment threshold, Y_0 the initial (prepayment) level of biodiversity, Y^* the optimal level of biodiversity given result-based payments, Y_0^* the reservation threshold of the payment above which the cost of increasing biodiversity on a plot would be higher than the result-based payment, R, and $c(\cdot)$ the cost function.

Case 3. $Y_0 \leq Y_0^* < \bar{y}$. In this case, (i) the initial biodiversity level is below the policy threshold, and (ii) so is the reservation threshold. Thus, the optimal biodiversity level for such plots remains the initial level: $Y^* = Y_0$.

In summary, a result-based payment (and a payment increase of it) incentivizes those farmers to increase biodiversity by changing management (e.g., reduced cuts, reduced fertilizer application, and overseeding) on the plots with prepolicy levels of biodiversity are simultaneously below the actual payment threshold $\bar{y}$ and where the plot-specific reservation threshold Y_0^* is equal or above $\bar{y}$. For all other plots, the introduction of the payment has no effect.[15] The overall effect of the policy is thus equal to the sum of increases in biodiversity.

This model yields several empirical predictions. The main one is that plots with prepolicy level of biodiversity below and close to the threshold—ceteris paribus—will respond strongest to the policy. On the other hand, plots further away from the threshold are unlikely to respond to the policy.

Our empirical strategy focuses on the effect immediately at the threshold. The main reason is that we can achieve clean identification only there using a combination of regression discontinuity

[15]Note that for simplicity, we abstract from the crowding out of intrinsic motivation in the case of windfall gains, as well as from any equilibrium effects that may arise through, for example, a change in the price of farms inputs or outputs.

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and difference-in-differences approaches as described below. Nevertheless, we provide tentative evidence for plots further away from the threshold that these plots indeed were less affected by the policy.

4.2 | Identification strategy

We are interested in the effect of an increased incentive for biodiversity conservation (via agri-environmental result-based payments) on measured biodiversity—specifically, the number of indicator species on a given plot.

To fix ideas, let the random variable P_i be a binary random variable with $P_i = 1$ whenever plot i has an increased incentive to increase biodiversity due to the policy reform, and $P_i = 0$ otherwise. Note that plots with $P_i = 0$ have already reached the payment threshold prior to the reform and, thus, are already eligible for payment based on prereform levels and do not need to increase biodiversity for retaining this status. For each possible policy arm $p \in \{0, 1\}$ and each individual plot, let $Y_i(p)$ denote the potential outcome of that plot i when the plot has been exposed to treatment arm p. (Note that to keep the notation more concise, we will suppress the individual index i in our notation whenever it is not required.) Thus, the average treatment effect of the policy reform is defined as

$$\Delta = E[Y(1) - Y(0)]. \tag{3}$$

As we do not observe whether a payment is actually claimed, Δ must be interpreted as an effect of the Intention to Treat (ITT) variable P. In addition to being the only feasible parameter to estimate because of the unobservability of actual payments claimed per plot, focusing on Δ has the advantage that it reflects the overall effect of the policy given that we investigate a voluntary payment scheme. Thus, Δ includes effects of incentivizing farmers behavior and the consequential impact on biodiversity.

The naïve estimator of Δ would compare the post-treatment outcomes of plots that were not eligible at the time of the policy reform, based on prereform biodiversity, to the outcomes of plots that were eligible. However, such a comparison would capture unobserved differences in farming practices as well as unobserved differences in the type and quality of the land. These differences would lead to a spurious effect and a bias in the estimates.

To deal with the endogeneity problem, we adopt an empirical approach that relies on the inability of the farmers to precisely control the number of indicator species. Specifically, we define the random variable $Y_{i,PRE}$ as the number of prepolicy reform indicator species on plot i just prior to the introduction of the policy. Eligibility for result-based payments is determined by $\mathbb{1}\{Y_{i,PRE} \geq \bar{y}\}$, where $\bar{y} = 6$ is the threshold from which plots are eligible for result-based payments, and $\mathbb{1}\{\cdot\}$ is an indicator function that is equal to one when its condition is satisfied, and equal to zero otherwise. Thus, plots that were not eligible for payment before the reform receive a larger incentive to increase biodiversity. We formalize this incentive as a binary treatment variable P defined as $P = \mathbb{1}\{Y_{i,PRE} < \bar{y}\}$. For the rest of the analysis, this binary variable that represents the increase in incentive is the main treatment variable.

The starting point of our strategy is the insight from Section 2.2 that farmers do not have perfect control over Y_{PRE} at the threshold $\bar{y}$. The imprecise control of the number of species on a given plot implies that plots just above and below the threshold are very similar in terms of unobserved factors. In particular, as Lee and Lemieux (2010) show, this implies that, locally (i.e., at the threshold), the distribution of the (counterfactual) potential outcomes (0) are identical at the threshold. Here, we use the index $POST$ to denote outcomes in the postpolicy period. Thus, locally, P can be interpreted as being generated by a randomized experiment.

This setup gives rise to a regression discontinuity design (RDD), in which an outcome Y is impacted by a treatment P, and in which a forcing variable S determines assignment to treatment P by crossing a threshold c (see, e.g., Lee & Lemieux, 2010; Wuepper & Finger, 2023). In our case, the forcing variable is $S = Y_{PRE}$, the threshold is $\overline{y}$, and the outcome is $Y = Y_{POST}$.[16]

Although the standard RDD is typically formalized in a static setup, we exploit a dynamic context, that is, that the forcing variable is the pretreatment outcome variable. Therefore, it is important to clarify the role of the *no anticipation* assumption. Specifically, the standard RDD does not require that the forcing variable S is a pretreatment characteristic, that is, that it is determined before P, as long as the unobservables are balanced in expectation as the threshold is approached from below and above. Formally, if U denotes the unobserved characteristics, it is sufficient that continuity assumption $\mathbb{E}[U|S \rightarrow \overline{y}^-] = \mathbb{E}[U|S \rightarrow \overline{y}^+]$ holds (Hahn et al., 2001).[17] Moreover, S needs not be causally related to Y in the first place. However, as shown by Urquiola and Verhoogen (2009), anticipation of the policy and sorting may violate the continuity assumption (see also Bajari et al., 2011). Our setup does not suffer from this pitfall. Specifically, at the time of determining P, the forcing variable $S = Y_{PRE}$ has been already determined due to the reasons described in Section 2 (i.e., no anticipation of the reform by the farmers at the point in time of measuring Y_{PRE}).

As a next step, we further weaken the assumption of no perfect control at the threshold by modifying it to a difference-in-differences setup. Specifically, we assume that, on average, the outcome trends on both sides of the threshold (importantly, exactly at the threshold) would have been equal had the policy not been implemented. Formally, we assume that

$$E[Y_{POST}(0) - Y_{PRE}(0)|Y_{PRE} = 5] = E[Y_{POST}(0) - Y_{PRE}(0)|Y_{PRE} = 6]. \tag{4}$$

The left-hand side represents the counterfactual change between periods 0 (prepolicy) and 1 (postpolicy) for those plots with five prepolicy indicator species in the case that the policy was *not* implemented. The right-hand side presents the equivalent trend for the plots with six prepolicy indicator species.

Equation (4) is a local parallel trends assumption that gives rise to a difference-in-discontinuity approach. It assumes counterfactual equality of trends only at the payment threshold. To clarify its origin and meaning, it is helpful to state what assumption it does not require. First, assume that at the threshold, farmers cannot perfectly control the time trend of the number of species found in the field. This is a fairly weak assumption that takes into account the idiosyncratic stochastics of environmental outcomes. Second, we do not assume that the policy was fully unanticipated by the farmers. Our local randomization assumption is valid if farmers have no perfect control over the pretreatment number of species. Importantly, this holds even if pretreatment behavior and anticipation differ in a systematic way (Lee & Lemieux, 2010). Thus, we allow for selection on both sides of the threshold due to anticipation, as long as this selection is not perfectly controlled. However, as described in Section 2, the precise change in the payments was not known until just before the policy entered into force. The development of the reform and its timing imply that during the period in which the pretreatment outcome was determined, there was uncertainty about the precise economic incentives introduced by the policy reform. This uncertainty has likely prevented forward-looking farmers from perfectly adjusting their prepolicy behavior in anticipation of its implementation (see also Wuepper & Huber, 2022). This adds a second source of exogenous variation at the threshold in addition to the stochastic nature of environmental outcomes. Finally, note that our difference-in-discontinuities estimator uncovers the treatment effect without requiring full randomization at the threshold but instead locally requires (i.e., immediately at the threshold) the much weaker assumption of parallel trends. This assumption allows plots below and above the threshold to be

[16]The utilization of pretreatment outcomes as a running variable is not uncommon in the literature, see for example, Hyytinen et al. (2018) in the context of election outcomes and Thistlethwaite and Campbell (1960) in the context of educational outcomes.

[17]$\overline{y}^-$ and $\overline{y}^+$ refer to below and above the threshold, respectively. The predeterminism is sometimes formalized as $S(0) = S(1)$ (e.g., Lechner, 2011).

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different as long as their trends are equal. We provide convincing additional evidence for the plausibility of our assumptions in Section 5.3.

Furthermore, we define our outcome of interest as the change in the number of indicator species between two consecutive periods, $dY = Y_{POST} — Y_{PRE}$. Applying an RDD to this variable leads to a difference-in-discontinuities type of estimator (see Eggers et al., 2018; Grembi et al., 2016).

Remark 1. Our theoretical model suggests that policy incentives depend on the distance to the threshold (i.e., cost to change)—so that plots with an initial number of indicator species of, for example, 4 are differently incentivized than plots with, for example, 5 indicator species. We evaluate this conjecture along with estimating the main effect of interest by applying an RDD strategy identical to the one described above but for different thresholds.

Remark 2. Our identification setup resembles the reverse difference-in-difference setup considered by Chabé-Ferret and Voia (2021). Specifically, Chabé-Ferret and Voia (2021) consider a setup in which they look at change in the outcomes for farmers entering the treatment to the change in outcomes for farmers that are always treated. Because farmers that are always treated might also react to the change in prices, this might invalidate the empirical strategy. However, Chabé-Ferret and Voia (2021) show that in this context and under simple and intuitive conditions, the reverse difference-in-difference still produces unbiased estimates as—considering our study setup—the plots above the threshold represent the comparison group given the weighted effects of the previous and the 2014 changes in result-based payments. In our setup, there are two additional aspects that reduce the threat to the validity of the empirical approach that arises in the reverse difference-in-differences context. First, our approach relies on the administrative discontinuity rule and, in particular, on the inability of the farmers to perfectly control the number of indicator species at the threshold. Thus, as discussed above, our approach relies on two different but complementary sources of random variation. Second, we provide below evidence that past treatments were ineffective and that prepolicy trends were indeed parallel throughout the period of observation.

4.3 | Estimation strategy

In this section, we describe the econometric implementation of our empirical strategy. The section is divided into two parts. First, we introduce the estimations of the effect of the policy reform at the payment discontinuity (i.e., a threshold at six indicator species). Second, we present the estimators for the effect below and further away from the actual payment discontinuity.

4.3.1 | Effect of the policy reform at the payment discontinuity

Our main estimation of the effect of the policy reform at the payment discontinuity is based on the following nonparametric estimator:

$$\widehat{\Delta}_{5,6}^{np} = d\overline{Y}_{Y_{PRE}=5} - d\overline{Y}_{Y_{PRE}=6}, \tag{5}$$

where $d\overline{Y}_{Y_{PRE}=k}$ is defined as the average change in the number of indicator species in the period after the reform compared to before the reform for all plots that had exactly k indicator species before the reform. Thus, $\widehat{\Delta}_{5,6}^{np}$ is the postreform difference in the change in the number of indicator

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species between plots with five and six indicator species prereform, respectively. It follows from assumption (2) that $\widehat{\Delta}_{5,6}^{np}$ is a consistent nonparametric estimator of Δ.

To complement our nonparametric estimation, we follow Lee and Lemieux (2010) and estimate Δ using a linear RDD estimator as *additional analysis* of the following form:

$$dY = \beta_0 + \beta_\Delta P + \beta_r (Y_{PRE} - 6) + (\beta_l - \beta_r) P (Y_{PRE} - 6) + \beta_Z Z + \epsilon, \tag{6}$$

where β_Δ corresponds to the effect of the policy reform, and β_l and β_r are the regression line slopes to the left and right of the threshold, respectively. β_Z is a vector of coefficients for each five-yearly rota of the surveys, Z. Again, this is a difference-in-discontinuities estimator, as the left-hand side represent a difference of the two consecutive values of the dependent variable. The advantage of this estimator is that it uses a larger number of observations than the threshold-based, nonparametric estimator, $\widehat{\Delta}_{5,6}^{np}$.

The obvious disadvantage of Estimator (6) is that it gives weightage to observations far from the threshold that is equal to the weightage of the observations directly next to the threshold, thereby potentially introducing selection bias. A second disadvantage is the linear form, which is prone to misspecification errors. However, by decreasing the bandwidth in which observations are considered, the threat of selection bias and misspecification error decreases—with the potential selection bias of $\widehat{\beta}_\Delta$ and $\widehat{\Delta}_{6,5}^{np}$ being equivalent at the limit (i.e., at a bandwidth of one). This suggests conducting an analysis that assesses the sensitivity of the results toward the linear assumption and the weightage of the observations. In particular, we estimate both the nonparametric estimator and the linear estimator for different bandwidths and compare the resulting estimates and confidence intervals. As Lee and Lemieux (2010) documented, changing the bandwidths for both estimators is equivalent to estimating a nonparametric estimator (a Nadaraya–Watson estimator with a rectangular kernel in the first case and a local linear regression in the second).

Furthermore, we can also use a quadratic RDD estimator instead of the linear one with different bandwidths to reduce the weightage of observation that are further away from the threshold (e.g., Wuepper & Finger, 2023).[18]

Finally, we note that when we interpret the estimated effects of the policy, our objective is not to test a given prespecified null hypothesis, such that the effect is zero. Instead, our objective is to evaluate a report and inform the decision maker on what are the likely consequences, a major distinction made in Imbens (2021).[19] Therefore, following Imbens (2021), Cox (2020), and the statement by the American Statistical Association (Wasserstein & Lazar, 2016), we only report and discuss the confidence intervals (and not the associated p-values). Specifically, we present the 95% and 90% confidence intervals. It is important to highlight that along the confidence intervals the likelihood of the effect is not the same. For example, the upper and lower boundaries of the 95% confidence intervals are—under mild regularity conditions—roughly seven times less likely than the point estimate (Romer, 2020). This difference is significantly smaller for the 90% confidence interval, with the point estimate being four times more likely than the extremes (Romer, 2020).

4.3.2 | The effect of the policy on plots below and further away from the payment discontinuity

After estimating the effect of the policy reform at the payment discontinuity, we now focus on plots that were arguably less incentivized by the policy reform at the time of its implementation (see Section 4.1). Specifically, we re-estimate the regressions presented in the previous sections, but we

[18]Gelman and Imbens (2019) suggest not to use higher order polynomials than quadratic polynomials.
[19]Along these lines, Imbens (2021) also highlights the importance of economic as opposed to statistical significance.

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now consider a hypothetical threshold at five indicator species instead of the actual threshold at six (i.e., the actual payment discontinuity). Thus, the nonparametric estimator is modified as

$$\widehat{\Delta}^{np}_{4,5} = d\overline{Y}_{Y_{PRE}=4} - d\overline{Y}_{Y_{PRE}=5}. \tag{7}$$

This modified nonparametric estimator compares the averages of the post-treatment outcomes of the plots that had four and five indicator species before the policy. Under an equivalent local randomization assumption, $\widehat{\Delta}^{np}_{4,5}$ is an estimator of the effect of the difference in incentives created by the policy. Here, we do not consider the linear and quadratic RDD estimator, as this would always lead to the inclusion of plots above the actual payment threshold of six and, thus, would contaminate the test. The focus on this comparison is motivated by our microeconomic model, which predicts that plots closer to the threshold in prepolicy periods should be more affected than plots further away. Finally, we test the robustness of this analysis, using wider bandwidths.[20]

5 | RESULTS

5.1 | Graphical evidence

In this section, we provide graphical evidence of the impact of the policy reform on biodiversity. Figure 4 displays the distributions of the change in the number of indicator species from pre- to postreform (i.e., from period 0 to period 1) per plot. In the figure, the distributions are depicted based on the number of indicator species in the period just prior to the policy reform (i.e., period = 0). We focus on the plots that were at the threshold before the reform, that is, $Y_{PRE} = 5$ and $Y_{PRE} = 6$.

A visual inspection of the boxplots reveals the following patterns. First, the distribution of the plots with five indicator species before the policy reform has a positive variance in the change of indicator species, and the interquartile part of the distribution[21] lies entirely above 5. Thus, 75% of the plots with exactly five species in period = 0 have five or more species in period = 1. Specifically, the mean number of indicator species increases by 14% from 5.0 to 5.7. In contrast, the distribution of plots with six indicator species prereform is characterized by a roughly symmetric distribution regarding the change in indicator species from pre to postpolicy reform, which is centered at 0 (Figure 4). More specifically, the mean number of species decreases from 6.0 to 5.9 (−2%). Under the difference-in-discontinuity assumption (2), this difference in the shift in the distributions can be interpreted as a positive distributional effect of the policy reform on those not eligible and just below the threshold compared to those already eligible.

5.2 | Causal estimates

5.2.1 | Main estimate

Figure 5a shows our non-parametric estimates in orange. The estimated treatment effect $\widehat{\Delta}^{np}_{5,6}$ is equal to 0.80. This estimate suggests that the policy reform incentivized plots that were prepolicy reform just below the threshold to become more biodiverse than those prepolicy reform already eligible for

[20]Using wider bandwidths requires setting lower hypothetical thresholds to ensure that comparisons exclude plots above the actual payment threshold.

[21]This is the part of the distribution between the 25% and 75% quantiles.

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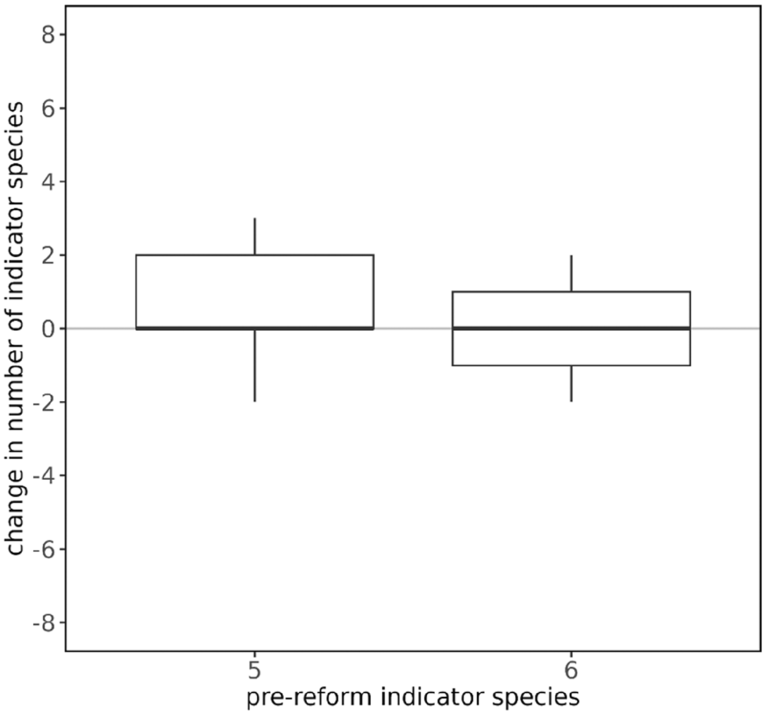

FIGURE 4 Distribution of the change in the number of indicator species from pre- to postreform depending on the prereform level of indicator species (i.e., Y_{PRE}). The figure depicts the plots at the threshold (i.e., $Y_{PRE} = 5$ and $Y_{PRE} = 6$). For a version of the same figure considering plots close to the threshold, see Figure A.6; for pre- and postreform distributions, see Figure A.7.

payments. To put the estimate into perspective, it represents a difference of 15%, considering the average prereform number of indicator species.

Next, we discuss the uncertainty associated with this estimate. The 90% and 95% confidence intervals of the estimate are [0.12 to 1.49] and [−0.02 to 1.62], respectively. First, we consider the 95% confidence interval. The lower boundary of this interval is virtually at zero, thereby implying that a lower boundary for the policy effect is no effect. In contrast, the upper boundary of this interval is above 1.5, thereby implying a large incentive for plots that are prior to the reform just not eligible as compared to those just eligible. However, as pointed out above, these two extremes should not be considered equally plausible as the point estimate, but the boundaries are roughly seven times less likely than the point estimate (Romer, 2020). For the 90% confidence interval, for which boundaries are at 0.12 and 1.49 and do not include zero, the point estimate is four times more likely than the boundaries (Romer, 2020). Overall, these results support that the policy has had an overall beneficial effect for those plots just below the threshold.

5.2.2 | Alternative estimates at the threshold

Bandwidth variation and RDD estimator comparison

We perform the two different analyses discussed in Section 4.3.1, alternating the bandwidths of the nonparametric estimation (Figure 5a) and using a linear and quadratic RDD estimator (Figure 5b).

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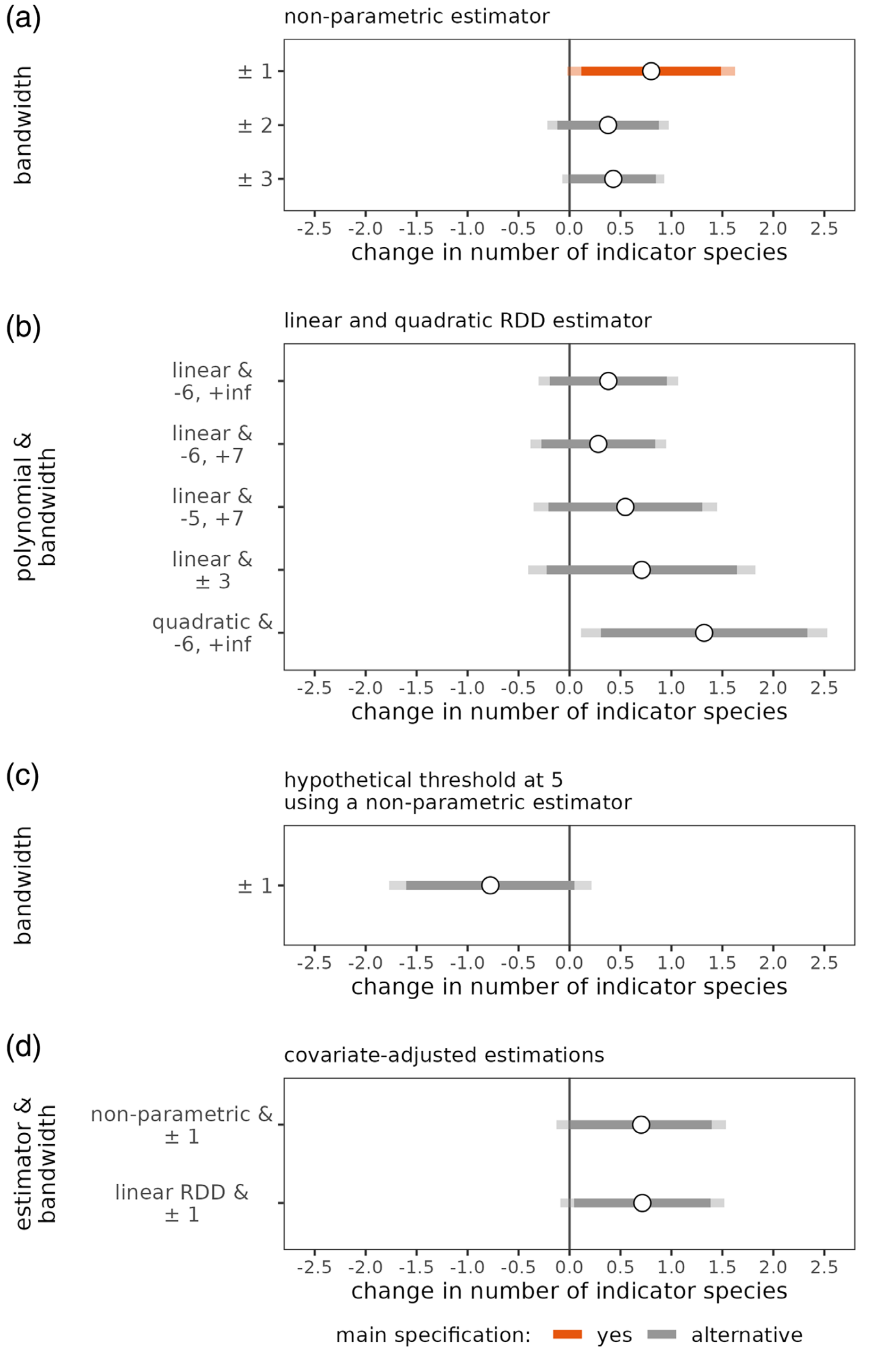

FIGURE 5 Legend on next page.

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The nonparametric estimates reveal that the empirical insights of our main estimation are robust toward changing the bandwidth. Further, although there is some heterogeneity[22] in the estimates of the linear and quadratic RDD estimator (Figure 5b), the overall pattern is consistent with the nonparametric estimates.

Data-driven bandwidth approach

As an additional check, we estimate the effect of the policy with a data-driven bandwidth approach proposed by Imbens and Wager (2019). Its magnitude is larger than the magnitude of our main result, and the confidence bound is slightly larger (Figure A.8), but both estimates agree in terms of sign and implications.

5.2.3 | The effect of the policy on plots below and further away from the payment discontinuity

The distance of the number of indicator species from the threshold are arguably related to the costs of meeting the threshold (see microeconomic model in Section 4.1). Thus, we apply our empirical strategy, but now comparing plots with prepolicy number of species equal to four and five.[23] We now find a negative nonparametric estimate equal to −0.71 (Figure 5c). The 90% and 95% confidence intervals of the point estimates are [−1.60 to 0.05] and [−1.78 to 0.22], respectively. According to this result, the higher incentives for the plots with prereform five species compared to plots with prereform four species (reflected by the greater proximity to the threshold of six) led to a larger improvement in the post-treatment period. Robustness checks with different bandwidths of the estimator reveal little sensitivity of these results (Figure A.9). This is consistent with the distance to the main threshold being a proxy for the costs necessary to attain the threshold, thus being reversely proportional to the incentives of the policy.

5.3 | Assessing the validity of the assumptions

To assess our major difference-in-discontinuity assumption (parallel trends at the threshold, see Equation 4) and validate our estimates, we follow several approaches.

FIGURE 5 Effect of the policy reform. Panel a presents the estimates (x-axis) for different bandwidths (y-axis) of the nonparametric estimator. Panel b presents the estimates (x-axis) for different bandwidths of the linear or quadratic RDD estimator (y-axis). Panel c presents the estimates (x-axis) of the nonparametric estimator at a hypothetical threshold of 5. Panel d presents the estimates (x-axis) of the nonparametric and linear RDD estimators adjusted for covariates and a bandwidth of ±1. The covariates, which were recorded before the policy reform, include altitude, slope, rootable soil depth, potential waterlogging of the soil, and permeability of the soil. All estimations are based on data from 2009 and 2018, during which all plots were surveyed once before and once after the policy reform in 2013. The change in the number of indicator species over this period is the dependent variable. Treated plots are those below the threshold, whereas control plots are those above. The threshold for all estimations is 6, except for the estimation presented in panel c, where it is 5. The sample size for each model specification is shown in Table A.5. Bandwidths are indicated with '±' for symmetric distances to the threshold, and with '−' and '+' separately for asymmetric distances. For example, our main specification considers plots with prepolicy reform values of 5 and 6, resulting in a bandwidth of ±1. Our main estimate is highlighted in orange and other estimates are indicated in gray, with the light and dark bars representing the 95% and 90% confidence intervals, respectively.

[22]For example, the sample estimate of the effect of the policy reform (i.e., $\widehat{\beta}_{\Delta}$) of the linear RDD estimator that considers all plots (−6, +inf & linear in Figure 5b) is equal to 0.38 (90% confidence interval = [−0.19 to 0.96]; 95% confidence interval = [−0.30 to 1.07]). The quadratic RDD estimator considering the same plots (−6, +inf & quadratic in Figure 5b) is equal to 1.32 (90% confidence interval = [0.31 to 2.33]; 95% confidence interval = [0.11 to 2.53]).

[23]In this estimation the $d\overline{Y}_{Y_{PRE}=4}$ is the reference point (i.e., minuend). Hence, a negative coefficient indicates $d\overline{Y}_{Y_{PRE}=4} < d\overline{Y}_{Y_{PRE}=5}$.

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5.3.1 | Pretreatment parallel trends

The main approach is motivated by difference-in-differences placebo tests of pretreatment parallel trends. Specifically, we test for the existence of a pretreatment effect by applying our main estimation strategy on plots around the threshold in points in time before 2014 (the time of the implementation of the reform). In other words, we test the null hypothesis of no effect. The estimates are shown in Figure 6. All (placebo) effect estimates are very close to 0 in magnitude and the corresponding confidence intervals are almost symmetric around 0. These estimates provide convincing empirical support for the assumption of "parallel trends at the threshold."

5.3.2 | Covariate-adjusted estimations

Next, we follow the regression discontinuity design literature and examine whether observed factors of the outcome variable other than the forcing variable are continuous at the threshold (Lee & Lemieux, 2010). In our discrete setup, the nonparametric approach amounts to a simple t-test of difference in averages for the two groups of plots directly at the threshold. We focus on the biophysical properties of plots because plots cannot be assigned directly to farms. For each plot, we collected information on altitude (taken from swisstopo, 2010), slope, and different soil properties (including rootable soil depth, potential waterlogging of the soil, and permeability of the soil; taken from FOAG, 2023) that were recorded before the policy reform. Figure 7 displays the average difference for plots that had five and six indicator species for each of these properties just before the reform. Although slope and soil properties appear to be well balanced, altitude is not.

To address this difference, we conduct a robustness check by following the matching difference-in-differences literature (e.g., Heckman et al., 1997) and adjusting for covariates in our main

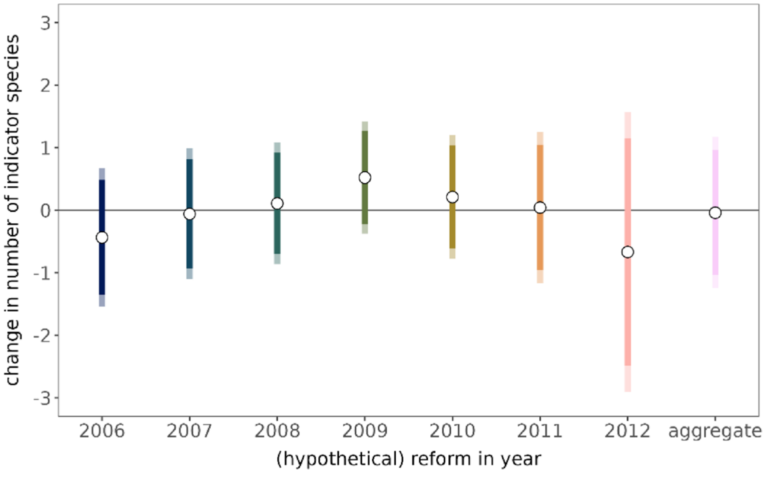

FIGURE 6 Prereform changes in the number of indicator species considering different (hypothetical) reforms. The light and dark colored areas indicate the 95% and 90% confidence band, respectively, and "aggregate" indicates the average across years. The nonparametric estimation follows Equation 5 and considers only the years before 2014. For the comparison of each (hypothetical) reforms, we consider 5 years pre and 5 years postreform, given that each plot is surveyed every 5 years, except for the hypothetical reforms in the years 2010, 2011, and 2012, in which cases the last postreform year is 2013. Note that a hypothetical reform in 2013 is not included as this year would only consider one-fifth of the data, and no plots with exactly six indicator species were observed in 2013. The threshold for all estimations is six and the bandwidth ±1. The change in the number of indicator species over this period is the dependent variable. Treated plots are those below the threshold, whereas control plots are those above. The sample size for each model specification is shown in Table A.6. The light and dark colored bars indicate the 95% and 90% confidence intervals, respectively.

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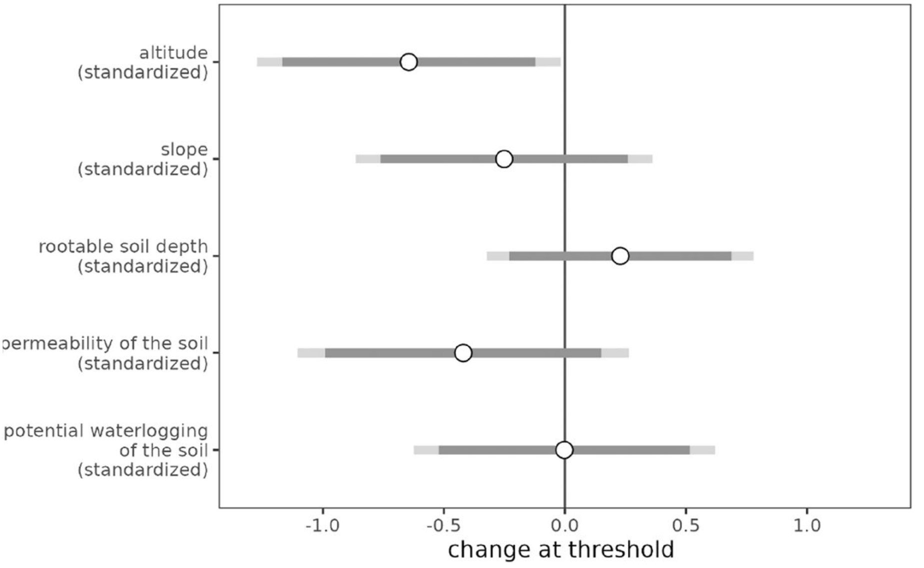

FIGURE 7 Changes in biophysical properties at the threshold. The results are based on a nonparametric estimator with a bandwidth of ±1 (it follows Equation 5). Treated plots are those below the threshold, whereas control plots are those above. The threshold for all estimations is 6. We standardized all biophysical covariates so that they have a mean of 0 and standard deviation of 1. All variables were measured prepolicy reform. The light and dark gray areas indicate the 95% and 90% confidence band, respectively.

estimation approach. This amounts to a matched difference-in-discontinuity approach (see Text A.4 for details). The results are displayed in Figure 5d and are very similar to our main unconditional estimates.

5.3.3 | Hypothetical thresholds far away from the actual threshold

The next approach to assess the validity of the difference-in-discontinuity design is to implement a different type of placebo test. Specifically, instead of varying the timing (and selecting a placebo timing prior to the actual reform), we vary the threshold. In other words, we select a hypothetical threshold in such a manner that plots around that threshold should not be affected differently by the reform because the number of species on both sides are *far* from the actual threshold. The first such group of plots are plots with 0 versus 1 and 1 versus 2 indicator species, that is, plots below and away from the *actual* eligibility threshold. Thus, the corresponding hypothetical thresholds are 1 and 2. The second group of comparisons consists of plots with species above and away from the actual threshold of six (i.e., 8 vs. 9, 9 vs. 10, and 10 vs. 11). To each of these groups, we apply our estimation approach following Equation 5. The results are displayed in Figure A.10a,b for below and away from the threshold and for above and away from the threshold. The confidence intervals of the estimates all include 0 and are almost symmetric around it. Furthermore, we conduct the same test for plots close to the threshold (i.e., 2 vs. 3, 3 vs. 4, 6 vs. 7, and 7 vs. 8), which are more likely influenced by the reform than those away (see Section 4.1). The results support that the main policy effect is below and at the actual eligibility threshold (Figure A.10c).[24]

[24]In addition, we display in Figure A.11 estimates for prereform effects for plots further away from the eligibility threshold. To this end, we assumed again various hypothetical thresholds away from the actual threshold. The figure reveals that most estimates are close to 0 and the corresponding confidence intervals include 0. Thus, the prereform parallel trends assumption cannot be rejected, which provides support for our identification assumption for plots further away from the threshold and that the identified effects further away from the threshold are valid ones (i.e., those presented in Figures 5c and A.10).

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5.3.4 | Differentiating between plot-specific land use

Thus far, we have not differentiated between plots based on their land use. To distinguish between different land uses, we use the assessment of botanists to determine whether a grassland plot is being used as a meadow or a pasture. We use this information only in the validity checks as the assessment (made without consulting farmers) can be uncertain in some cases. In total, 348 plots were clearly identified as either a meadow or a pasture. Our findings of the nonparametric and RDD estimators are consistent with the main findings when we use information about the land use of grassland plots (Figure A.12).

5.3.5 | Spatial correlation of treatment

Next, we test whether spatial correlation of treatment, P, and outcome variable, dY, are present, thus whether we need to account for it in our estimation. To this end, we map the treatment and outcome variables (Figure A.13), check for the number of connected plots (considering different radii ranging from 10 to 100 km), and estimate the Moran's I using a permutation setup (Cliff & Ord, 1973; Pebesma & Bivand, 2023).[25] We find that many plots do not have any connections (i.e., 69% and 32%) and the average number of connections is very low (i.e., 0.31 and 1.23) when considering plots at the threshold and in very close proximity (i.e., radius of 10 km and 20 km) (Table A.7). Moreover, considering plots at the threshold and within a radius above 20 km,[26] we observe a Moran's I statistic that indicates no spatial autocorrelation across different proximities (Table A.7). Based on this evidence, we conclude that spatial correlation is not an issue for our analysis. This is also consistent with our ecological expectations that being "just above" or "just below" the threshold reassembles a quasirandomized experiment.

5.3.6 | Power analysis

Finally, we conducted a power analysis, the objective of which is to assess the credibility of our results that, in some cases, depend on modest sample sizes (see Text A.5 for details). The results of this analysis indicate that a lack of power is not an issue for our main results (Figure A.15).

5.4 | Biodiversity and probability of enrolling grassland

Thus far, we focused in our analysis on intention to treat effect, which is of key policy relevance when evaluating voluntary payment schemes, as we discussed in Section 4.2. Additionally, we are interested in the relationship between the probability of claiming payments at the municipality level (proxied by the share of municipal grassland enrolled in result-based payments) and the average number of species per plot in each municipality (Text A.6).[27] To this end, we estimate an RDD type model that allows the slope to change at the threshold of six indicator species. The analysis reveals a strong positive relationship between share of enrollment and average number of species left of the threshold (Figure 8). In contrast, the slope right of the threshold is almost flat, potentially reflecting the adverse incentives of the policy above the threshold. Thus, these results support our main findings, as they show that the probability of enrolling grassland to result-based payment schemes is linked to the number of indicator species (and the payment threshold). Yet, we use the results merely

[25]The distance of all plots in our sample ranges between 3.9 and 321.2 km, with a mean distance of 104.7 km (Figure A.14).

[26]For Moran's I interpretation, we focus on distances above 20 km, given the very few connections when considering a radius of 10 and 20 km, indicating a moot spatial correlation issue and a weight matrix for computing the Moran's I mostly consisting of zeros.

[27]Plot-level data are not available.

to show the correlation between the policy (eligibility for payments, or intention to treat effect) and the actual payment, and we do not interpret them causally.

6 | INTERPRETATION AND DISCUSSION

Our two main findings can be summarized in the following manner. First, we find that plots with prereform indicator species just below the threshold benefit from the policy reform compared to those above the threshold. In other words, the reform caused farmers to enhance biodiversity on plots that were nearly eligible for payments before the reform, unlike those that had just met the eligibility criteria. In our main analysis, this effect is 0.80 indicator species representing an increase of 15% compared to the mean level of indicator species across plots, which is 5.23. Second, we show that plots further below the threshold—thus plots with arguably higher adjustment costs—were less incentivized by the reform. These results align with the prediction based on the microeconomic model presented in Section 4.1.

Let us now turn to the relevance of our analysis. Focusing on plots just below and above the threshold has important advantages. Tracing out the effect of the payments depending on the level of biodiversity before the reform is informative about differential incentives triggered by the thresholds of result-based payments. Indeed, our analysis reveals such a differential incentive. Following our microeconomic model and the argumentation by Zabel and Roe (2009),[28] only plots close to the threshold are affected by result-based payments. This means that the overall effect of the payment

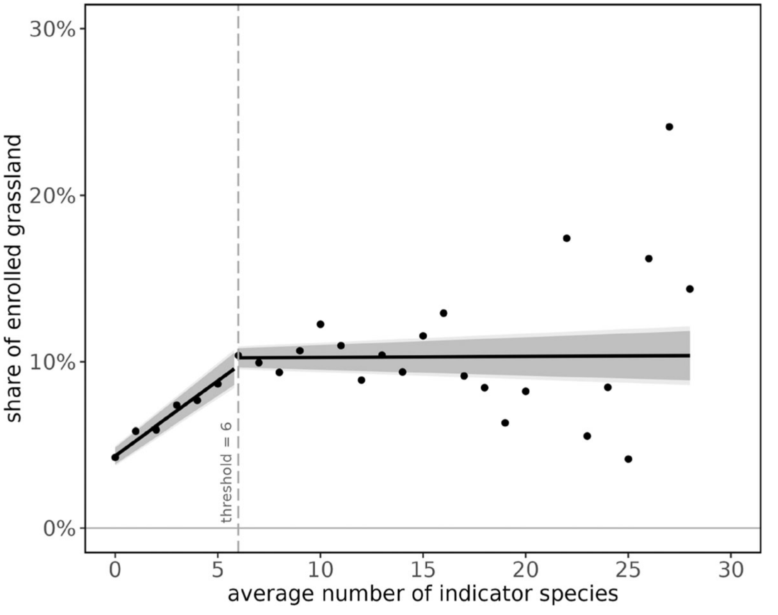

FIGURE 8 Probability of enrolling land to result-based payment schemes and average number of indicator species at the municipality level. The figure depicts a linear relationship that can change the slope at the payment threshold, which is six indicator species. The dots represent local averages, showing the mean enrolled grassland share within 1-unit bins categorized by the average number of indicator species. The share of enrolled grassland is based on the time from 2004 to 2018. Table A.8 presents the summary statistics of the municipality-level data. The light and dark areas indicate the 95% and 90% confidence intervals, respectively.

[28]Moreover, we are under the assumption that the main mechanism of the policy impact is through changing incentives to keep or increase the number of species on a plot, and not through, for example, changing the overall cost structure through equilibrium effects.

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coincides with its effect on plots that were close to the threshold in the prepolicy period. Thus, introducing result-based payments not merely with one threshold but multiple or continuous thresholds might improve the effectiveness of result-based payments. If one is willing to assume constant threshold elasticities of biodiversity, policymakers could, for example, double the impact of result-based payments by introducing a second threshold. However, constant threshold elasticities might be implausible and this requires further research. Here, result-based payments in Germany represent interesting research subjects, as these include payments with one, two, and three thresholds, depending on the state (e.g., Burton & Schwarz, 2013; Elmiger et al., 2023).

Finally, although the interpretation of our results at the threshold should be in light of the modest sample sizes, a range of sensitivity analysis and the power analysis support our findings.

7 | CONCLUDING REMARKS

In this paper, we estimated the effect of result-based agri-environmental payments on biodiversity with a focus on the differential incentive at the payment threshold. To this end, we leverage a unique nationwide dataset of plant vegetation records spanning 20 years, a quasi-natural experiment, and an innovative identification approach that utilizes a difference-in-discontinuities design. Our results highlight an important feature of agri-environmental result-based payments—that is, that they lead to disproportionate changes in biodiversity close to the threshold. Thus, using multiple or continuous thresholds instead of only one threshold could improve the effectiveness of result-based payments. However, future research is required to investigate result-based payments with more than one threshold. Improving the efficiency of result-based policy designs is particularly important given the push for more result-based agri-environmental payments (e.g., within the Common Agricultural Policy of the European Union; Pe'er et al., 2022; Elmiger et al., 2023; Kelemen et al., 2023). Other important future research directions include investigating whether the effect of result-based payments on measured biodiversity differs between land uses (e.g., grasslands vs. croplands) and the cost effectiveness of the effect of these payments. These tasks, and the present study, highlight the importance of investing in long-term and comprehensive monitoring of biodiversity, which is key in evaluating the impact of agri-environmental payments.

ACKNOWLEDGMENTS

We are grateful to Editor Christian Langpap and three anonymous reviewers for their helpful comments. We also thank Nadja El Benni (Agroscope), Robert Finger (ETH Zurich), and David Wuepper (University of Bonn) for reviewing previous versions of the paper and providing insightful comments. Moreover, we thank Victoria Cabrera Ziri Castro for answering questions about plant taxonomy and nomenclature. Furthermore, we thank the Swiss Federal Office for the Environment (FOEN) for providing the BDM data and the people that collected this data. Sergei Schaub also acknowledges the support of the Agroscope Research Program "Indicate—Measuring and Optimizing Farm Environmental Impacts."

DATA AVAILABILITY STATEMENT

The survey data can be requested from Biodiversity Monitoring Switzerland (www.biodiversitymonitoring.ch). The code to analyze the data is available in the Supporting Information.

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How to cite this article: Schaub, Sergei, Tobias Roth, and Petyo Bonev. 2025. "The Effect of Result-Based Agri-Environmental Payments on Biodiversity: Evidence from Switzerland." *American Journal of Agricultural Economics* 1–27. https://doi.org/10.1111/ajae.12512

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