The effects of privatization on pasture productivity in southern Kazakhstan

Eduard Bukin Sarah Robinson Martin Petrick

2024-04-08

Based on rich cadastral data we examine the effects of land privatization on pasture productivity. We identify the causal effect using a design with a staggered absorbing treatment and heterogeneous treatment effects accounting for spatial spillovers. We collect a balanced panel of 16 thousand plots located in southern Kazakhstan including precise land allocation dates and remotely sensed geographic and climatic features over 24 years. Results show that land allocation has a significantly negative effect on the pasture vegetation comparable with a drought occurring once in 25 years for individual farms and ones in six years for all users on average. Controlling for the spatial spillover of privatization of neighboring land further aggravates the negative effects of titling especially in proximity to settlements. Pasture privatization under a restricted land market with imperfect institutions and high transaction costs distorts existing grazing practices and causes pastures to deteriorate.
Keywords: land privatization, leasehold, tenure, pastures quality, vegetation change, land reform, spatial spillovers, transition, Kazakhstan.
JEL Classification: O13, Q15, R14.

1 Introduction

The conventional wisdom on land tenure in agricultural economics is the idea that private property rights, when appropriately supplemented with institutions, result in improvements in land use efficiency and agricultural productivity (Deininger and Feder 2001). There is indeed much empirical evidence for these phenomena, along with associated benefits for equity (Holden and Otsuka 2014) and this makes land reform an important policy tool for agricultural intensification and global development (Deininger and Binswanger 2001). However, land reform are often partially successful, especially in the context of semi-authoritarian regimes that emerged in the transition, such as former Soviet countries (Kvartiuk and Petrick 2021; Petrick 2021). Secure property rights and liberal land markets may spur investment and create efficiency and equity benefits, but there are risks of elite capture of large land areas with inefficient and inequitable outcomes (Holden and Otsuka 2014).

The discourse around pastoral land tenure has been subject to a different set of debates, particularly in arid and semi-arid regions. The privatization of previously 'open access' pastures was seen as the solution to the problem of the 'tragedy of the commons' (Hardin 1968; Coase 1960). However, land individualization (whether private or leasehold) has also resulted in a number of negative side-effects, such as: preferential land access that left others on limited common land (Rohde et al. 2006), negative spillovers elsewhere (Masami Kaneko et al. 2009); fragmentation of grazing systems and reduction in livestock mobility (Galvin et al. 2008); reduced herbivore populations (Boone and Hobbs 2004); and land degradation (W. J. Li, Ali, and Zhang 2007). This has been termed 'the tragedy of enclosure' (Reid, Galvin, and Kruska 2008). At the same time, an understanding emerged that many of the pastures imagined by Hardin had never been 'open access' at all but were actively managed by users as common property (Ostrom 1990; Dietz, Ostrom, and Stern 2003). In other cases, some extent of open access is a crucial adaptation to high climatic variability, as it promotes the mobility and flexibility required to exploit shifting forage resources (Behnke 2018).

Many land reforms continue to promote individualization of rangeland tenure with an increasing number of studies attempting to quantify their environmental impacts. Most use remotely sensed measures of vegetation productivity and isolate the effect of tenure regime. In China, where pasture individualization has been ongoing since the 1980s, Hou, Liu, and Tian (2022) find that privatization with fencing increases grassland productivity; Lu et al. (2023) find that private rights foster better management than rental; just as improved pasture condition is associated with more formalized access rights on the American plains Buehler (2022). Although the latter study also looked at the negative spillover effects of tenure individualization on surrounding areas, most studies do not examine such effects. Studies looking at productivity trends at the landscape level found that severe degradation in Inner Mongolia coincided with the introduction of private tenure (A. Li, Wu, and Huang 2012), attributed to rising livestock numbers, fragmentation, loss of mobility and loss of ability to respond to drought and weather events (W. J. Li, Ali, and Zhang 2007; W. J. Li and Huntsinger 2011). Conversely, there is evidence that re-aggregation of pasture areas at the community level (de-privatization) has led to *improved* pasture productivity, as it re-creates both economies of scale for movement and the increases area with which livestock are free to move (Gongbuzeren et al. 2021). Informal institutions which are often an invisible part of the tenure system can also be very important to management outcomes (D. Li, Hou, and Zuo 2021).

Central Asia is an important pastoral region, with Kazakhstan alone having one of the largest areas of rangeland on earth. In 2003, Kazakhstan implemented a set of reforms towards individualization of land tenure through private property and long term lease (Robinson, Jamsranjav, and Gillin 2017). The impact of these reforms was found to be poorly effective in promoting improved productivity in the case of cropland, owing to restricted land markets preventing land redistribution after privatization (Kvartiuk and Petrick 2021). Yet effects of tenure on land quality and pasture productivity have not been conducted. Kazakhstan thus provides an interesting natural experiment to explore the impact of land reform and of associated spillover effects, on pasture productivity.

In order to address a broad **research question** regarding the effects of land privatization on pastures in south-eastern Kazakhstan, we combine detailed parcel geospatial boundaries, their privatization dynamics over three decades and their remotely sensed characteristics in three districts of Almaty province in Kazakhstan. Our findings suggest that privatization has not achieved the (presumably) intended goal of maintenance or improvement of vegetation resources, either on the privatized plots themselves or on adjacent areas which are affected by negative spillovers and competition for grazing near settlements.

This is one of the first studies that analyses the effect of land reform and private property on pasture quality, which in contrast to (H. Li and Zhu 2023; Chari et al. 2021; Hou, Liu, and Tian 2022) does not find positive effects of land privatization. Instead, we observe strong negative effects of individualized tenure regimes. These findings question the view that private property regimes promote rational resource allocation (Deininger and Jin 2005, 2006; H. Li and Zhu 2023).

In the context of Kazakhstan, despite seemingly liberal land institutions, frictions created by various restrictions to land exchange practically reduce the reform's potential, also in crop production (Kvartiuk and Petrick 2021; Petrick 2021). We contribute to the dialogue on the optimal land reform design in transitional countries by displaying first insights on pasture management and livestock production and discussing limitations and solutions to the restriction of land transfer. Finally, we present practical application to the ongoing methodological debate on estimating the average treatment effect on the treated under the staggered research design with absorbing treatment, heterogeneous treatment effect, and spatial spillovers. Building on (Xu 2023; Clarke 2017; Butts 2021b) identification, we further identify the spatial spillovers on pastures land, confirming their importance and in our case additional detrimental effect on vegetation (Masami Kaneko et al. 2009; Buehler 2022).

The paper is structured in the following way. Chapter 2 discusses land reform in Kazakhstan and its implementation, concluding with hypotheses about the outcomes of the reform on pasture management. Chapter 3 briefly describes and summaries data. Chapter 4 presents the identification of causal effect of land allocation, discusses necessary assumptions and dives into the theoretical problems of spillovers effect identification. Chapter 5 presents out main findings. Chapter 6 discusses results in the context of land reform in Kazakhstan.

2 Land reform in Kazakhstan

Land reform in Kazakhstan launched the redistribution of agricultural land from the state to long term lease or private property (Kvartiuk and Petrick 2021). In the 1990s, following collapse of livestock inventories, demand for pastures was low. As stock numbers recovered in 2000s, the new private livestock owners began once again to use more distant pastures (Kerven et al. 2016) that caused a surge in applications for land titles. These applications are made through a process of tender for which the winner is decided behind closed doors by a district land commission, (*de-jure*), often based on the applicants' ability to invest (Mukhtarova 2022). The resulting titles are of three types: short-term (1-5 years) and long-term (49 years) leaseholds; private ownership (an equivalent of the private property); and permanent land use rights (used to designate land to state-owned legal entities).

These titles are only applied to the land plots demarcated and registered in the state cadastre. Only registered individual farms (*sharýa qojalyǵy*[1]) or agricultural enterprises are legally able to rent or own pastures land. Households (non-registered small scale subsistence farms called *qosalqy sharýashylyq*[2]), which hold around 60% of all livestock in Kazakhstan, are not permitted to participate in tenders over pastureland, although they may do so as part of cooperatives, (an arrangement which is rare in the study area). Some pastureland is protected from leasing to individuals and are formally designated as "common grazing" land for residents, whether households or registered farms. These common lands are of two major types - village grazing lands, available around all settlements by law, and remote areas set aside at the discretion of local district authorities. Remaining lands are essentially open access and belong to the state reserves available for further lease or privatization. We refer to these as 'never-allocated'.

As pasture privatization proceeded from the late 1990s to 2020s, areas of never-allocated land decreased, and livestock owners without title were forced to graze on the shrinking **not-yet** or **never-allocated land** and limited **common grazing** areas, or had to make often informal access arrangements with leaseholders. This situation may have contributed to heavy use of lands close to settlements whilst many remote pastures remained little used or even abandoned (Alimaev et al. 2008; Dara et al. 2020). In between these extremes, accessible and high quality pastures were increasingly leased or privatized and, in southern Kazakhstan in particular, little accessible land remains without a designated user.

Despite the seeming transparency of the land reform, there is only a general understanding of its effects. In the case of cropland, for example, land reform failed to create incentives for efficient land redistribution, leading to an inefficient land concentration, hampered the credit market development and benefited large-scale agricultural enterprises (Kvartiuk and Petrick 2021). Farm structures, particularly in the northern grain-producing areas, remained dominated by agricultural enterprises whilst individual farms tended to take niches, specialize in other crops and in the livestock sectors (Petrick 2021). The livestock sector is at a crossroads between large commercial enterprises, which still play a relatively small role, and smaller farms facing numerous constraints,

[1] Aliased with *шаруа қожалығы* (in Kazakh cyrillic) or *krest'janskoe hozjajstvo* (in Russian).
[2] Aliased with *өзіндік қосалқы шаруашылық* (in Kazakh cyrillic) *lichnoe podsobnoe hozjajstvo* (in Russian).

including the physical, legal and institutional means to access productive pastureland (Robinson, Bozayeva, Mukhamedova, Djanibekov, and Petrick 2021).

The recent availability of the State cadastre[3] opened an opportunity for a more granular analysis of the impact of formal tenure arrangements at the parcel level, which is the key subject of our study.

2.1 Study region and observed grazing practices

The region under study includes three eastern districts (*audany*[4]) of Almaty region (*oblysy*[5]): Enbekshikazakh (*Eñbekşıqazaq*), Kegen (*Kegen*) and Raiymbek (*Raiymbek*) (described in Section A.1 in appendixes) . The area hosts a number of vertical transhumance regimes between alpine summer pastures (*zhaĭlau*) and remote winter pastures (*qystau*) on south-facing or other snow-free areas . Thus, animals, if mobile, can be kept on pastures for much of the year migrating through spring (*kökteu*) and autumn (*küzeu*) pastures . Hayland and arable land are also available for fodder production and much of the former is used as autumn pasture after the harvest . We focus our analysis only on pastures, which occupy more than 75% of the study region and correspond to land cover classes "pasture" and "pasture on slopes" .

Once parcels on pastures are defined and allocated, they are assigned a **land use** category, which include: individual farms, agricultural enterprises, forest, protected areas, common grazing, households and others[6]. Land allocation occurs under different legal arrangements which broadly consist of private ownership and lease (rent) available to individual farms and agricultural enterprises, and permanent land use applied to all common grazing land, forests and protected areas.

These differences are important for grazing livestock because whilst private ownership enables owners to freely use, lease[7] or sell their land it is only applied to 7% of allocated pastures. 52% are held under long term leaseholds, when land is still considered as state land, cannot be legally subleased and is also subject to expropriation if underused or misused (Kazakhstan 2017). Formal transfer between farmers or back to the state is also difficult and thus numerous informal land access arrangements exist including subleasing and informal grazing of underused or abandoned leaseholds (Robinson, Bozayeva, Mukhamedova, Djanibekov, Oshakbayev, et al. 2021; Robinson, Bozayeva, Mukhamedova, Djanibekov, and Petrick 2021). Overall, the combination of land use and legal arrangements impose restrictions on the re-distributional capacity of the land market and have a profound long-term effect on rational land use in Kazakhstan (Kvartiuk and Petrick 2021).

Combining land use categories and legal arrangements constitute key **tenure** categories on pasture present in our study region and extensively used in our analysis below. These are:

[3] Cadastre maps are available according to the article 44-1 of the Land Code (2023) from the following website https://aisgzk.kz/aisgzk/ru/content/maps/.

[4] Aliased with *rajon*.

[5] Aliased with *oblast*.

[6] As households cannot hold individual tenure over pastures, and those are present in the data because of the recording errors in cadastre in the negligible frequency.

[7] Such rental transactions are not observed in the cadastre data.

1. Lands **once-allocated** (allocated) to users within specific parcels registered in the cadastre:

    - **Individualized land tenure** by **individual farms** and **agricultural enterprises** with land **owned** or **rented** from the state under long-term 49 years contracts (Article 37 in Land Code (2023)). **Land ownership** represents the best Kazakh analogy of the **full private property** or **land privatization** as such land can be further formally subleased or sold. Land lease is an individualized form of tenure with restrictions on use and disposal.
    - **Common pastures**: land allocated by district administrations for common use on (i) remote pastures (Article 49-2 of the Land Code (2023)) or (ii) around villages (Chapter 11 of the Land Code).
    - **Forest land** allocated permanently to the forest department that leases areas of pasture to individuals on short term contracts (not recorded in cadastre) without auction.
    - **Protected areas**, are nature reserves or national parks, in which grazing is not permitted.

2. Lands **never-allocated** (unallocated) lacking delineated parcels registered in the cadastre**:**

    - **Never-allocated** is also aliased with **state reserve**, land eligible for allocation to users through tender or for common use[8] in future, but currently open access (Article 35 of the Land Code (2023)). Essentially this is all land that is not demarcated into specific parcels. Before land is allocated to users, opportunistic grazing is possible on them. We refer to this land as **not-yet-allocated** (before allocation) or **never-allocated** (unallocated as of the end of 2022).

Considering the above tenure categories, we suppose that land allocation is likely to have a radical effect on pastures use only under some of them. This concerns mainly individual farms and agricultural enterprises who own land, as they might be more interested in enforcing exclusion by fencing or monitoring their parcels after allocation, in order to preserve vegetation for their own flocks. For these land holders, exact allocation date (recorded in the cadastre) implies replacement of the free-grazing use patterns. For other allocated land, allocation date might be less important as pre-existing grazing patterns may be simply formalized once land is allocated.

With gradual land allocation and consequent fragmentation of the landscape, the never-allocated land is becoming less accessible to landless households and individual farmers[9]. Livestock numbers in Almaty region have nearly doubled from a low of 4.7 million livestock units in the 1990s to 8 million by 2018, with over half of these animals owned by landless households . As households tend

[8] Within never-allocated category some lands are in fact formally designated for **common use around settlements** (Chapter 11 of the Land Code (2023)). These lands have real boundaries but in most cases they not yet registered to the cadastre (e.g. in Raiymbek district).

[9] Pressure on pastures combined with policy change has led some districts to allocate increasing areas of land away from villages for common use. For example, the cadastral data which we use in this study includes these types of common remote areas designated by Enbekshikazakh district. In 2020, Kegen and Raiymebek districts have expropriated leaseholders holding under-used pasture, particularly those who leased pasture in high summer areas as an investment or for hunting or tourism purposes. Whilst some of these areas have been re-allocated to individuals by tender, others have been increasingly repurposed for common use, meaning that a number of large summer pastures are now newly available, but remain for the moment little used. These areas did not appear in the cadastre by December 2022 and are included in our analysis as never-allocated land or allocated land with the legacy tenure.

to keep their livestock near settlements, livestock density increases on these never-allocated and common grazing lands, a trend aggravated by the growing exclusion from privatized areas. This constitutes a **spillover** effect of privatization, which combined with growing livestock numbers, may affect pasture condition.

2.2 Hypotheses

Positive effects of land privatization on farms productivity and efficiency are well documented in the literature (Besley and Ghatak 2010; Deininger and Jin 2006; H. Li and Zhu 2023). Exclusive property rights promote improved land access not only through incentives to invest in conservation practices but also through efficient land use reallocation between production units (H. Li and Zhu 2023). In crop production, efficient land markets (interlinked with credit and insurance markets) is essential to fully realize the incentive-generating effects of private ownership (Binswanger, Deininger, and Feder 1995; Deininger and Feder 2001; Sadoulet, Murgai, and Janvry 2001). Ultimately, Adamopoulos et al. (2022) suggest that both common and private property are equally capable to generate allocative efficiency improvement. With respect to the land quality, theoretical literature is limited and empirical studies derive their expectation from the production economics (H. Li and Zhu 2023; Hou, Liu, and Tian 2022). Authors conclude that a friction-less land market with private property and its effortless transfer between users should and empirically does improve land and pastures quality in various settings. Therefore, combining land reform implementation, regional context and observed practices, we conclude with a set of hypotheses about land reform results.

Hypothesis 1 Land allocation improves pastures' vegetation

On average, we expect to observe a positive effect of land allocation to any type of using entity on pasture quality.

Hypothesis 2 Land allocation only has an effect under individualized tenure

Substantial land use change occurs only concerning those tenure regimes that create *individual exclusive* land use rights or physically reshape the landscape with private property that likely having stronger effect than individualization through lease similarly to comparable studies elsewhere (H. Li and Zhu 2023; Hou, Liu, and Tian 2022; Lu et al. 2023). Existing grazing practices on common grazing, forests, and protected areas tenure regimes are not affected by the act of registration in the cadastre.

Hypothesis 3 Privatization causes negative spillovers

Landscape fragmentation and frictions in land markets combined with the inability of households to obtain leaseholds may undermine the migratory grazing systems essential to good pasture management and inhibit landscape-level management. This leads to a mismatch between stocking rates and vegetation density affecting both unfenced parcels under individual tenure and remaining freely accessible lands.

Hypothesis 4 Effects of privatization increase in proximity to settlements

Village proximity increases competition for grazing land, amplifying the effects of privatization on allocated plots and negative spillovers. Common land around villages is the most easily accessible pasture area, both administratively and legally, and particularly for landless households. Using free remote pastures requires cooperation to pool and heard animals together. Thus, grazing pressure is highest in the proximity to villages on land under all tenure categories given uncommon fencing.

3 Data description

Our data starts from the land **parcels**, each with a unique cadastre number, spatial polygons, and metadata (tenure and allocation date) disseminated through the website of The Department of the Land Cadastre and Automated Information System of the State Land Cadastre in Kazakhstan (Directorate of the Land Cadastre in Kazakhstan 2023b). Land parcels were allocated between 1990 and 2022 gradually, by the end of the observation period 25% of pastures in the study region remain unallocated in the cadastre. We refer to unallocated parcels as never-allocated and define their geographic boundaries synthetically (see Section B.2 in appendix for more details).

Each parcel spans different types of **land cover** (e.g. pastures, pastures on slopes, etc.), which determines whether a plot can be used for an agricultural purposes such as crop production or livestock grazing. Land cover maps are disseminated with the cadastre data (Directorate of the Land Cadastre in Kazakhstan 2023a).

To produce **units of analysis**, we spatially intersect parcels with the land cover creating discrete **plots** (also with repeating land cover) within each parcel. The rationale behind moving from parcels to plots as units of analysis is discussed in the Chapter 4. Number of allocated plots exceeds the number of parcels by 73%, and plots are naturally smaller than parcels, however, there is no difference between climatic and geographic characteristics of the plots and parcels.

Finally, our data consists of plots on “Pasture” and “Pasture on slopes” (both referred to henceforth as **pastures**), with all tenure categories. Never-allocated land is separated into “near” and “remote” (within and beyond 5 km radius from settlement) to approximately match stratification of the common grazing land. Besides, we retain “households” and “other” tenure categories to maintain a complete population of allocated parcels. Figure 1 presents spatial distribution of our plots in the study regions aggregating ownership and rental tenures of agricultural enterprises and individual farms for a better representation.

To measure climate and geographic characteristics of the plots we use remote sensing data and state of the art methodology or aggregating raster data at the polygon level. The pasture quality is approximated by Normalized Difference Vegetation Index (NDVI), long used for the measurement of environmental change (Pettorelli et al. 2005). As a tool for measuring the impact of grazing in particular, NDVI suffers from several limitations including its inability to distinguish forage quality. However it is a working horse in pastoral range land analysis and has been used in many similar studies (Eddy et al. 2017; Behnke, Robinson, and Milner-Gulland 2016; Robinson et al. 2016; Zhumanova et al. 2018; D. Li, Hou, and Zuo 2021; Hou, Liu, and Tian 2022; Buehler 2022). It

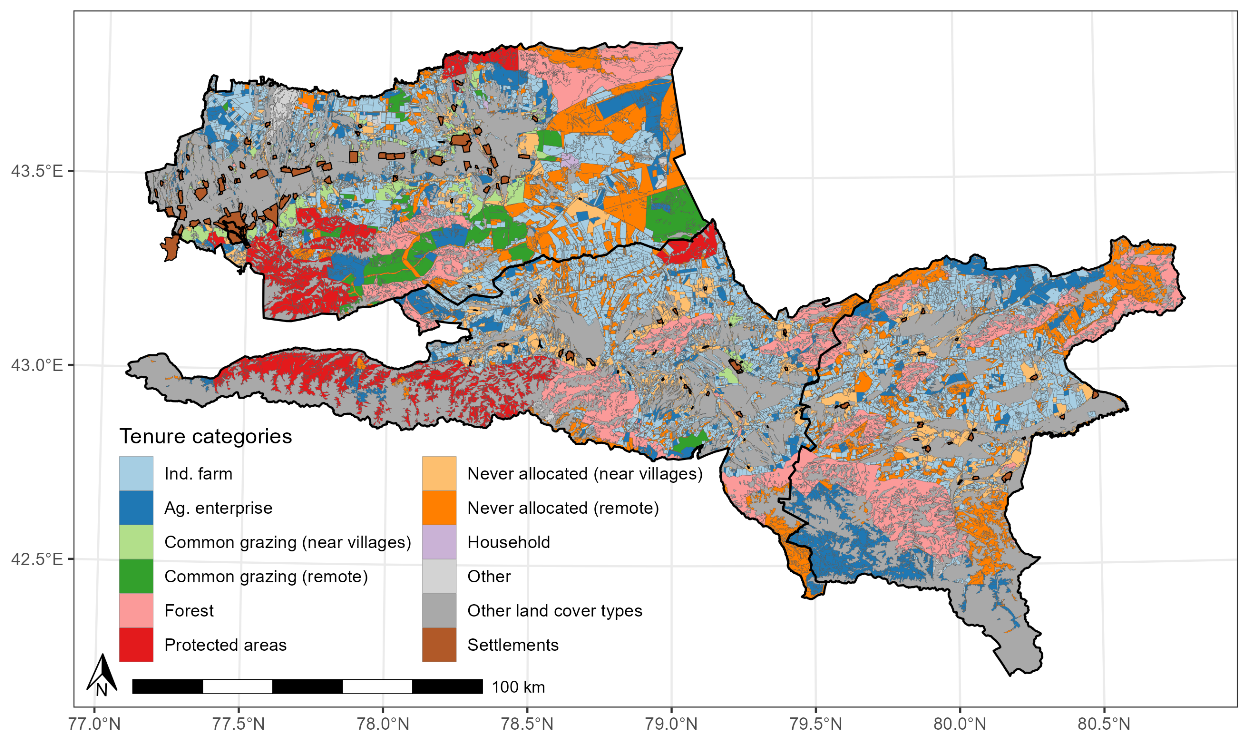

Figure 1: Plots and their land use on pastures and pastures on slopes

establishes the common baseline of comparison and has the sensitivity required to detect changes induced by climate or policy (Zhumanova et al. 2018). Although the exact aggregate of NDVI differs between geographical contexts, we rely on the annual plot-average of peak NDVI (see also Section B.2 in appendix) because of the snow cover in winter that distorts annual averages.

Table 1 presents summaries of the annual peak NDVI, geographic characteristics (plot size, elevation, slope), and climate variables for our units of analysis in total and by tenure categories. Monthly climatic data is aggregated over the period from April through August, which is considered the relevant time frame for the peak biomass.[10]. When once- and never-allocated plots are compared, they appear on average similar in their geographic and climatic features, except for the NDVI, which is significantly lower for never allocated land. When different tenure categories are compared, we find them similar in their climatic characteristics, however, their geographic features such as mean size, elevation, and distance to villages appear different. Plos distance to settlements is also homogeneous for all tenure categories as well as for never allocated land, except for common grazing land located near to villages.

[10] We also estimate time-varying monthly cumulative rainfall (millimeter), monthly average surface temperature (degree Celsius), and monthly cumulative solar short wave radiation flux (watt per square meter). Details on spatial data processing along with the auxiliary descriptive statistics are available in Section B.2 and Section D.4 in appendix.

Table 1: Plots characteristics by tenure

Tenure	N plots [parcels]	Area, 1000 ha	Size, ha	Elevation, km	Slope, degree	Distance to settlements, km	Peak NDVI	Av. N plots [plot size] in parcel
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Full sample	23 570 [13 922]	1549.0	65.7 (326.8)	1.84 (0.83)	12.4 (9.0)	12.3 (9.7)	57.9 (21.5)	1.7 [44.7]
Once allocated	15 837 [9 162]	1168.7	73.8 (366.3)	1.86 (0.84)	12.7 (9.1)	12.5 (9.5)	59.2 (21.3)	1.7 [51.5]
Never allocated	7 733 [4 760]	380.3	49.2 (224.0)	1.78 (0.80)	11.2 (8.8)	11.6 (10.1)	53.6 (21.5)	1.6 [31.7]
Ind. farm	10 931 [7 605]	499.3	45.7 (92.6)	1.61 (0.62)	8.9 (6.7)	8.2 (5.6)	53.9 (19.1)	1.4 [46.0]
Ind. farm (own)	1 819 [1 437]	65.3	35.9 (140.4)	1.06 (0.49)	7.4 (6.4)	9.4 (6.5)	49.3 (19.8)	1.3 [27.6]
Ind. farm (rent)	9 112 [6 168]	434.1	47.6 (79.6)	1.69 (0.59)	9.1 (6.8)	8.1 (5.4)	54.7 (18.9)	1.5 [50.3]
Ag. enterprise	1 955 [832]	192.6	98.5 (330.3)	2.05 (1.00)	12.8 (9.1)	17.4 (11.6)	59.6 (20.4)	2.4 [79.0]
Ag. enterprise (own)	292 [170]	17.5	59.9 (205.9)	1.85 (0.89)	10.8 (9.8)	15.3 (11.4)	69.0 (14.6)	1.7 [41.6]
Ag. enterprise (rent)	1 663 [662]	175.1	105.3 (347.2)	2.07 (1.01)	13.0 (9.1)	17.6 (11.6)	58.6 (20.7)	2.5 [88.6]
Common (near)	237 [143]	32.4	136.5 (319.0)	1.04 (0.39)	10.2 (7.3)	2.5 (1.3)	59.7 (19.4)	1.7 [151.6]
Common (remote)	189 [40]	66.2	350.1 (955.0)	1.64 (0.68)	10.8 (8.3)	16.4 (6.7)	45.3 (24.6)	4.7 [416.2]
Forest	1 076 [132]	237.6	220.9 (1087.5)	2.22 (0.85)	18.4 (8.9)	15.2 (9.3)	68.8 (21.3)	8.2 [80.2]
Protected areas	901 [20]	123.7	137.3 (549.3)	2.34 (0.83)	19.7 (8.9)	18.0 (11.5)	69.5 (18.2)	45.0 [190.2]
Household	122 [101]	3.0	24.7 (83.4)	1.13 (0.50)	9.2 (6.8)	8.4 (3.1)	43.5 (15.2)	1.2 [12.9]
Other	426 [295]	13.9	32.7 (247.6)	1.23 (0.99)	6.9 (8.4)	9.8 (7.9)	58.7 (16.9)	1.4 [15.9]

Note: Column 'Tenure' stratifies samples into the 'Full sample', subsamples of 'Once allocated' and 'Never allocated' land, and subsamples by detailed tenure categories. The first column reports a number of plots and parcels (in square brackets) under each category. In columns 3 through 7 report means and standard deviations (in parentheses) weighted by plot size. Column 8 reports the number of plots and average plot size (in square brackets) within the parcel under each category.
Source: own calculations.

4 Identification strategy

To provide empirical evidence for hypothesis about causal effect of land allocation, we need to estimate τ, which is the **average treatment effect on treated (ATT)**. We start with estimating a **benchmark** two-ways fixed effect model (TWFE) using the static (**BM static**) and event-study (**BM**) specifications Equation 1, with the individual ($\eta_{i,\cdot}$) and time ($\eta_{\cdot,t}$) fixed effects (see Section C.3 in appendixes for more details). The vegetation density is a **dependent variable** approximated by the natural logarithm of the annual maximum of the Normalized Difference Vegetation Index $Y_{i,t} = \log\left(\text{NDVI}_{i,t}^{\text{max}}\right)$. The **key variable** $D_{i,t}$ is an indicator variable[11] that is zero, when land is not-yet- or never-allocated and turns 1 and remains absorbing once parcel i is allocated at time t. As Equation 1 presents an event-study specification of the problem, it uses year-before-after-treatment (r) indicator variables $R_{i,t}$ (instead of $D_{i,t}$) and estimates corresponding coefficients γ_r. Such model is also aliased with a canonical Difference-in-Difference (DiD) setting with cohort-level treatment and synthetic controls (e.g. Card and Krueger 1994; Abadie 2021). The fact that plot allocation take place in different periods of time for each parcel makes our research design staggered, requiring to compensate for the heterogeneous treatment effect problem in detailed reviewed by (Roth et al. 2023; Baker, Larcker, and Wang 2022)[12] .

[11] Log-level relationship requires accurate interpretation $\frac{\Delta\text{NDVI}}{\Delta D} = 100(1 - e^{\tau})\%$, which implies that switch of a plot to a private property causes $\frac{\Delta\text{NDVI}}{\Delta D}$ percent change in the vegetation density.

[12] Section C.3 in appendix in details reviews the problem of heterogeneous treatment effect in staggered setting. We base our identification on (de Chaisemartin and DHaultfoeuille 2020, 2022; Athey and Imbens 2022; Sun and Abraham 2021; Callaway and Sant'Anna 2021b; Borusyak, Jaravel, and Spiess 2023; Gardner 2022; Goodman-

$$Y_{i,t} = \eta_{\cdot,t} + \eta_{i,\cdot} + \beta X_{i,t} + \sum_{r \neq -1} 1[R_{i,t} = r]\gamma_r + \epsilon_{i,t} \tag{1}$$

The non random nature of land allocation, creates highly heterogeneous treatment and control groups calling for a rigorous control for the variance with a number of time-varying remotely sensed covariates ($X_{i,t}$) that exploit possible non-linearities in the biological process of vegetation growth. These controls consist of **three groups of variables** (84 variables in total) observed monthly (m) and normalized to the standard normal distribution: monthly cumulative rainfall $\text{RF}_{M,i,t}$, mean monthly surface temperature $\text{TMP}_{M,i,t}$, and monthly cumulative short wave radiation flux $\text{RAD}_{M,i,t}$[13].

The **units of observation** are 23.5 thousand once-allocated and never-allocated land plots i with clearly defined spatial boundaries used for extracting the remotely-sensed data and controlled for with $\eta_{i,\cdot}$ individual fixed effects. The resulting data set is a balanced panel with 560 thousand observations. Multiple plots may represent a single parcel of a single tenure category if there are several units non-contiguous of pasture land cover within that parcel. The rationale behind inflating number of observations by 73% by using plots is in making use of spatial heterogeneity of land. As a parcel combines land cover types that are distinct in space and have different qualities, their use might also differ. Thus, individuals, might be willing to enforce exclusion only on some parts of their parcels, leaving other parts with unchanged land use. Having plots as units of analysis allows to better control for any endogenous plot characteristics with individual fixed effects minimizing the OVB. One may still debate that using plots introduces "bad controls" (Cinelli, Forney, and Pearl 2022). We argue (and show with the robustness checks in Section G.7) that using one of the other does not make a difference as neither fixed effect (plots or parcels) change with the treatment qualifying as "good controls" in a classic Angrist's and Pischke's (2009, 64) sense.

Never-allocated plots are synthetically constructed based on the remaining unallocated land (see Section B.2) and they do not reflect potential economically rational boundaries of the parcels that could be allocated one day in the future. However, they indicate the area where new parcels may be allocated one day and where the common-property rights around villages are in place over the span of our analysis. Model respecification by (1) using parcels as unit of observation; (2) weighting plot-level regressions by plot areas as an approximation of the population weights; (3) varying sample by excluding never allocated plots; and (4) stratifying sample by tenure categories further ensures robustness of our estimates.

Validity of the BM estimate of ATT stands on the canonical assumptions of the DiD about parallel trends and no-anticipation combined with assumptions of the staggered design: treatment effects homogeneity over time and individuals, and additional parallel trends an no anticipation assumptions in detail summarized in Section C.3 in appendix.

Bacon 2021)

[13] We introduce these three groups of controls as month-specific variables (12 variables per group, 36 regressors in total). In addition, we add within-month interaction terms between each pair of these variables (additional 36 regressors) as well as interaction between all three variables in each month (additional 12 regressors). In Section F.6 in appendix we conduct sensitivity tests of the functional simplifying and omitting control variables, which do not affect the estimates of ATT.

To estimate the ATT of land privatization, we first need to relax the treatment effects homogeneity assumption, which is a common phenomenon in recent literature (Hou, Liu, and Tian 2022; H. Li and Zhu 2023). This makes the BM estimate to be inaccurate due to the "negative weights" and "forbidden comparisons" issues voiced in (de Chaisemartin and DHaultfoeuille 2020; Goodman-Bacon 2021)[14].

Below, we utilize the Callaway and Sant'Anna (CS) (Callaway and Sant'Anna 2021b), Sun and Abraham (**SA**) (Sun and Abraham 2021), and the Gardner's imputation (**IMP**) (Gardner 2022) estimators in their default event-study specification. As the CS estimator permits choosing relevant counterfactual groups for estimating the ATT, we use both: not-yet-treated "**CS (NYT)**" and never-treated "**CS (NT)**".

The treatment effect may be heterogeneous based on some measurable characteristics relevant to each plot. For example, competition for land creates a greater burden on pastures in proximity to the livestock holding facilities: villages. Besides, the type of land use may lead to different effects of land allocation as well as different tenure types. Therefore, we stratify the sample by village proximity, land use, and tenure types in combination with the heterogeneity robust estimators to infer accurate estimates of ATT. **The parallel trends in staggered design** is another assumption that can only be justified loosely. We employ the event-study specification (Equation 1) with all estimators. This allows testing for pre-treatment trends and relaxes the parallel trends in the post-treatment period, exploring post-treatment variation in the treatment effect sizes. **The assumption of no anticipation** is fulfilled for individualized tenure; however, for land where land-use does not change as a result of allocation, no effect is expected.

4.1 Spillover effects identification

Finally, **spatial spillover effects** are probably the key challenge in our setting given detailed parcel-level data. Most of the not-yet- and some never-allocated plots (which are also not not-yet allocated common pastures) are used for grazing based on free access. Besides, exclusion is weak on allocated plots as fencing is rare. Therefore, privatization of large parcels by individuals holding disproportionately lower numbers of livestock and partial exclusion on some parcels might increase grazing intensity on the not-yet- or never-allocated as well some allocated land. This might be regarded as a combination of the parallel trend assumption violation with the spillover effect (Butts 2021b).

To identify the spillover effects, we assume that τ from Equation 1 is an unbiased estimate of the ATT if the **stable unit treatment value assumption or SUTVA** holds. Imbens and Rubin (2015) formulate SUTVA as: "the potential outcomes for any unit do not vary with the treatments assigned to other units …". In the context of the staggered design DiD, SUTVA means that there are no interference or spillover effects from other treated individuals (Xu 2023; Clarke 2017; Butts 2021b; Aronow and Samii 2017). Conversely, once SUTVA is violated, no matter which DiD estimator we use, unknown interference makes our estimates of ATT biased not only in the magnitude but also in the sign ($\tilde{\tau}$) even under random assignment of the treatment (Sävje, Aronow, and Hudgens

[14] To test the extent of this heterogeneity, in Section E.5 in appendixes, we decompose the weights of the "BM static" estimate following both de Chaisemartin and DHaultfoeuille (2020) and Goodman-Bacon (2021).

2021; Berg, Reisinger, and Streitz 2021). Despite extensive attention in the randomized experiment settings (Sävje, Aronow, and Hudgens 2021; Aronow and Samii 2017; Vazquez-Bare 2022) and natural sciences (Halloran and Hudgens 2016), spillover effects had only limited attention in the social sciences (Manski 1993), DiD literature (Di Tella and Schargrodsky 2004; Clarke 2017; Berg, Reisinger, and Streitz 2021; Xu 2023) and none (except for Butts (2021b) pre-print) in the literature on staggered design with heterogeneous treatment effects.

Following a model-based identification (Butts 2021b; Berg, Reisinger, and Streitz 2021) and design-based approach (Xu 2023) conclude that spillovers from treated units are of two kinds: **on controls** ($\tau^{\text{s. control}}$) and **on treated** ($\tau^{\text{s. treated}}$). The first occurs, when the effect of a treatment "spills" onto the "neighboring" control groups affecting the factual outcomes of no treatment. The latter occurs, when treated nearby groups indirectly affect the magnitude of treatment in the other treated groups. This makes both, counterfactuals and factual invalid. In the canonical DiD setting counterfactuals are untreated individuals. In the staggered design, different counterfactuals are used in clean and forbidden comparisons. Therefore, $\tau^{\text{s. treated}}$ as much as $\tau^{\text{s. control}}$ affects the validity of our controls and the ATT estimates.

To separate these two spillover effects from the ATT in the potential outcome framework, we first need to relax SUTVA[15].

Let us assume that potential outcome of a treatment applied to the unit i not only depends on the treatment status D_i, but also on the treatment statuses of the neighboring units $\mathbf{z_{-i}}$. $\mathbf{z} = (z_1, z_2, ..., z_n) \in \{0,1\}^n$ is the n-dimensional vector of all unit treatments, ($\mathbf{z_{-i}} = (z_1, z_2, ..., z_{i-1}, z_{i+1}, ..., z_n)$ excludes unit i). All units are spatially scattered on different distances from the treated unit i. Units i exposure to the spillovers from all other units $\mathbf{z_{-i}}$ can be differentiated based on the "exposure map" $h(\cdot)$ that takes strictly positive values. Then, potential outcome of a treatment applied to unit i can be written as $Y_{i,t}(D_i, h(\mathbf{z_{-i}}))$, which reduces to $Y_{i,t}(D_i)$ (used in Equation 4), when $h(\mathbf{z_{-i}}) = 0$.

When $h(\mathbf{z_{-i}}) \neq 0$, we observe an individual treatment effect with spillovers $\tau_i^{\text{spillovers}}$ is composed of three treatment effects at once:

$$
\begin{aligned}
\tau_i^{\text{spillovers}} &\equiv Y_{i,2}(1, h(\mathbf{z_{-i}})) - Y_{i,2}(0, h(\mathbf{z_{-i}})) \\
&\equiv Y_{i,2}(1, h(\mathbf{z_{-i}})) - Y_{i,2}(0,0) - \{Y_{i,2}(0, h(\mathbf{z_{-i}})) - Y_{i,2}(0,0)\} \\
&\equiv \underbrace{Y_{i,2}(1,0) - Y_{i,2}(0,0)}_{\tau_i^{\text{direct}}} + \underbrace{Y_{i,2}(1, h(\mathbf{z_{-i}})) - Y_{i,2}(1,0)}_{\tau_i^{\text{s. treated}}} \\
&- \underbrace{\{Y_{i,2}(0, h(\mathbf{z_{-i}})) - Y_{i,2}(0,0)\}}_{\tau_i^{\text{s. control}}}
\end{aligned} \tag{2}
$$

[15]Below, we present the potential outcomes framework that corresponds to our notations used in Section C.3 and follows (Clarke 2017; Butts 2021b; Xu 2023) in a rather simplified manner. To have a rigorous overview of spillovers identification, see also (Aronow and Samii 2017; Sävje, Aronow, and Hudgens 2021; Vazquez-Bare 2022).

For estimating the ATT that excludes spillovers (τ^{direct}), Equation 2 can be rewritten in as an average of individual treatment effects[16]. Under SUTVA $\tau^{\text{spillovers}} = \tau^{\text{direct}} = \tau$ as $\tau^{\text{s. control}}$ and $\tau^{\text{s. treatment}}$ cancel out. However, when our goal is to estimate the ATT without spillovers (τ^{direct}) under relaxed SUTVA, omitting spillover variables as in Equation 1 results with $\tilde{\tau} = \tau^{\text{spillovers}}$, or a biased estimate of ATT (Berg, Reisinger, and Streitz 2021).

From a policy-perspective, we might not only be interested in the ATT (τ^{direct}), but also in the effects of spillovers $\tau^{\text{s. treated}}$ and $\tau^{\text{s. control}}$. Recent literature, however, provides as rather limited approaches for estimating these effects. It is essentially narrowed to the traditional regression control strategies (Di Tella and Schargrodsky 2004; Clarke 2017; Butts 2021b; Berg, Reisinger, and Streitz 2021) in the canonical DiD setting with cohorts of treated and untreated units.

Initially, (Manski 1993) proposes a regression model that can account for the spillover effects between units in a spatial context. (Clarke 2017; Butts 2021b; Xu 2023) prove that an amended canonical DiD TWFE model can accurately capture spillover effects represented as a set of binary variables (whether or not each neighboring unit is treated). (Berg, Reisinger, and Streitz 2021) provides the Omitted Variable Bias (OVB) rationale that necessitates including spillover variables into the DiD TWFE model and show that spillover effects can be estimated consistently when they are encoded as continuous variable[17] (ranging between 0 and 1, share of neighboring units treated).

Spatial nature of interactions between units sometimes requires to introduce not one, but multiple ($\mathbf{z_{-i}}$) others' units treatment statuses that are spilled on the unit i. This leads to the large dimensionality problem that requires reduction. Xu (2023) overviews key methods such as clustering neighbors by socio-economic characteristics with similar spillover effect, utilizing nearest neighbors, or spatially aggregating units based on their distance to i. The author concludes that although it is important to specify the spillovers accurately, their misspecification could be to some extent compensated by the proposed estimation method. In practice, authors use spatial configuration approaches by introducing distance bins (rings) $j = (1, 2, ..., p)$ around each unit i (Clarke 2017; Butts 2021b). Binary (or continuous from 0 to 1) variables $S_{i,t,j}$ is then added to indicate if relative to the unit i at time t a treatment is applied to another unit(s) in the distance bin j. Distance bins could be decided arbitrary and should be mutually exclusive to provide an unbiased estimates of the spillover. Non-parametric cross-validation methods exist to create the bins as well (Clarke 2017).

The average effect of spillovers on the treated is the average of the bin-specific estimates weighted by the relative frequency of certain bins ($\omega_j^{\cdot}$) in the data ($\bar{\tau}^{\cdot} = \sum_{j=1}^{p} \tau_j^{\cdot}\omega_j^{\cdot} / \sum_{j=1}^{p} \omega_j^{\cdot}$) with the

[16]Average of individual treatment effects:

$$\begin{aligned}\tau^{\text{spillovers}} &\equiv E\left[\tau_i^{\text{direct}} + \tau_i^{\text{s. treated}} - \tau_i^{\text{s. control}} | D_i = 1, h(\mathbf{z_{-i}}) \neq 0\right] \\ &\equiv \tau^{\text{direct}} + \tau^{\text{s. treated}} - \tau^{\text{s. control}}\end{aligned}$$

[17]Identification of spillover effects encoded with continuous variables is often ignored. Most theoretical proofs are concerned about binary spillover variables, while (Berg, Reisinger, and Streitz 2021), uses a continuous zero to one variables without discussion potential pitfalls. We, however, assume that it is relatively safe to specify spillovers as a non-binary (continuous) variable, leaving its theoretical justification for future exploration.

delta method based standard errors. Resulting DiD TWFE model with dynamic (static[18]) treatment effect that can capture spillover effects could be written as Equation 3.

In the canonical DiD setting, Equation 3 can be estimated using conventional TWFE model. Estimands $\hat{\tau}^{\text{direct}}$, $\hat{\tau}_j^{\text{s. treatment}}$, and $\hat{\tau}_j^{\text{s. control}}$ are valid when assumption of canonical DiD are satisfied alone with additional spillover-related assumptions: the **parallel trends** in treatment and control development under spillovers and independent of them (Assumption 4, 6 in Butts (2021b) or Assumption 3 in Xu (2023)); the **'local' spillovers** assumption (Assumption 5 in Butts (2021b) and Xu (2023)); and the **parallel trends** in spillover effects (Assumption 7 in Butts (2021b)).

$$\begin{aligned} Y_{i,t} &= \eta_{\cdot,t} + \eta_{i,\cdot} \\ &+ \sum_{q<-1} 1[R_{i,t} = q]\delta_q + \overbrace{\sum_{r\geq 0} 1[R_{i,t} = r]\gamma_r}^{\tau^{\text{direct}}} \\ &+ \sum_{j=1}^{p} \tau_j^{\text{s. treated}} D_{i,t} S_{i,t,j} + \sum_{j=1}^{p} \tau_j^{\text{s. control}} (1 - D_{i,t}) S_{i,t,j} \\ &+ \beta X_{i,t} + \epsilon_{i,t} \end{aligned} \tag{3}$$

Ultimately, to account for the presence of spillovers, we explicitly estimate the model with spillovers Equation 3. As the variable that indicates spillover $S_{i,t,j}$ we use the share of land area allocated (without discriminating between tenure categories) in the radius bin j around plot i at time t. The variable $S_{i,t,j}$ is a ratio variable that ranges between 0 and 1 in each 5, 10, 15, and 30 km "rings" relative to each plot i (see Section B.2.6 in appendix). As spillover effects magnitude may vary based on the distance to villages or tenure, we combine spillover model specification with other sample stratification approaches discussed above.

5 Results

5.1 ATT of land privatization

Table 2 presents the main results of our analysis estimated on the full sample (Panel A) and one excluding synthetic never-allocated plots (Panel B). It shows the estimates of the average treatment effect on the treated (ATT) from land allocation on pastures' vegetation employing all the above-discussed estimators. The resulting estimates are robustly negative with the ATT ranging from

[18]Static TWFE mode with spillover effects:

$$\begin{aligned} Y_{i,t} &= \eta_{\cdot,t} + \eta_{i,\cdot} \\ &+ \tau^{\text{direct}} D_{i,t} + \sum_{j=1}^{p} \tau_j^{\text{s. treated}} D_{i,t} S_{i,t,j} + \sum_{j=1}^{p} \tau_j^{\text{s. control}} (1 - D_{i,t}) S_{i,t,j} \\ &+ \beta X_{i,t} + \epsilon_{i,t} \end{aligned}$$

0.2% to 1.7% decrease in peak vegetation density. Similar results are evident from the even study (Figure G1 based on Panel A, and Figure G2 based on Panel B). The main results hold robustly against functional form specification presented in Section G.7 of the appendix. These include the plot-level linear trend inclusion to compensate for the natural rate of soil degradation, weighting by the plot size and varying sample by certainty of allocation date, or aggregating over periods before-after.

The ATT is small, therefore, its economic meaning has to be interpreted in comparison with other covariates. Section G.7 in the appendix presents estimates of the main ATT along with the selected variables controlling for rainfall and temperature in May-July (the months most important for peak vegetation accumulation). As each control variable in Equation 1 is normalized to a standard normal distribution, the effect of ATT could be compared with weather anomalies such as drought or excessive heat. Our estimates show that rainfall anomalies in June that decrease more than $\pm 1\sigma$ from the historical monthly level (happens with respective probability $\approx 16\%$) reduces peak vegetation by 0.8-1.2%. Rainfall that exceeds historical levels by 1σ in July instead reduces peak vegetation by 0.5-1.1%. Therefore, the effect of land allocation is comparable with negative weather anomalies that occur one to two times in a decade, thereby it is profound and detrimental providing solid evidence for rejecting our **Hypothesis 1**.

Land allocation to agricultural enterprises has a more ambiguous effect, which change once we exclude never-allocated counterfactuals from the sample (Panel B) or vary the estimator. This signifies treatment effect heterogeneity that is observed within agricultural enterprises and the sensitivity of heteroscedasticity-robust estimators in capturing the ATT.

Table 2: Key estimates of the ATT

	BM static	BM	SA	CS (NT)	CS (NYT)	IMP static	IMP
Panel A. Full sample							
ATT	-0.0024***	-0.0030**	-0.0035***	-0.0013	-0.0020*	-0.0050***	-0.0034***
	(0.0006)	(0.0009)	(0.0010)	(0.0009)	(0.0009)	(0.0010)	(0.0007)
N obs.	565,680	565,680	565,679	565,679	565,679	534,216	534,216
N ind. FE	23,570	23,570	23,570	23,570	23,570	22,259	22,259
Within R sq. adj.	21.0	21.1	21.6				
Panel B. Excluding never-allocated land							
ATT	-0.0031***	-0.0025*	-0.0048***		-0.0013	-0.0132***	-0.0173***
	(0.0006)	(0.0013)	(0.0013)		(0.0013)	(0.0022)	(0.0030)
N obs.	380,088	380,088	380,087		380,087	334,098	334,098
N ind. FE	15,837	15,837	15,837		15,837	13,921	13,921
Within R sq. adj.	21.5	21.6	22.4				

Note: Row 'ATT' reports the average treatment effect on the treated and its heteroscedasticity robust standard errors clustered at plot level in parentheses. In models BM, SA, CS, and IMP, the ATT is computed as a weighted average of individual estimates for periods 0 to 23 after land allocation, with delta-method-based standard errors. In the CS estimator 'NT' stands for never treated and 'NYT' for not yet treated groups of counterfactuals. The never-treated (NT) group consists of the synthetic never-allocated plots, which are excluded from the sample in Panel B. Statistical significance levels are: '***' p-value < 0.001, '**' p-value < 0.01, '*' p-value < 0.05, p-value < '.' 0.1, and ' ' p-value >= 0.1.
Source: own calculations.

ATT estimates for other tenure categories, where allocation does not imply a change in the land use practices are reported in appendixes Table H1 (Section H.8). There, results are inconclusive and sensitive to the sample used and the estimator. For example, one may find the effect of common grazing tenure significant and positive with the corresponding event study (Figure H2) indicating the same effect only with 6 and 7 years lag and no effect otherwise. The dynamics of land allocation to common grazing (Table A2) suggests that only 2 parcels (out of 143) were allocated before 2017 (6 and more years before the end of the observation period), while the bulk of allocation took place in 2020-2022. This highlights how misleading the results of ATT estimates could be when treatment occurs irregularly. Concerning other tenure categories, no actual change in land use as a result of land allocation is probably the key cause behind the unstable ATT estimate.

Table 3: ATT by tenure with a sharp change in the land use

Estimator	Ind. farm (all)	Ind. farm (own)	Ind. farm (rent)	Ag. ent. (all)	Ag. ent. (own)	Ag. ent. (rent)
Panel A. Full sample						
BM static	-0.0046***	-0.0028.	-0.0049***	0.0048**	0.0119**	0.0034*
BM	-0.0044***	-0.0036.	-0.0048***	0.0080***	0.0112*	0.0071**
SA	-0.0054***	-0.0025	-0.0060***	0.0059**	0.0141**	0.0042.
IMP static	-0.0067***	-0.0026	-0.0068***	0.0029	0.0167***	0.0000
IMP	-0.0028***	-0.0003	-0.0024*	0.0005*	0.0003***	0.0002
N obs.	447,936	229,248	404,280	232,512	192,600	225,504
N ind. FE	18,664	9,552	16,845	9,688	8,025	9,396
Panel B. Excl. never-allocated						
BM static	-0.0039***	-0.0105***	-0.0034***	0.0035.	-0.0014	0.0037.
BM	-0.0043**	-0.0047	-0.0229**	0.0083**	-0.0124	0.0082.
SA	-0.0065***	-0.0241	-0.0071***	0.0074	-0.0016	0.0045
IMP static	-0.0125***	-0.0223***	-0.0068**	-0.0053*	-0.0077	-0.0064*
IMP	-0.0164***	-0.0305***	-0.0078*	-0.0063*	-0.0117.	-0.0072*
N obs.	262,344	43,656	218,688	46,920	7,008	39,912
N ind. FE	10,931	1,819	9,112	1,955	292	1,663

Note: This table reports estimates of 'ATT' derived with key estimators based on subsamples of plots by tenure and subsamples with (Panel A), and without (Panel B.) never-allocated land. Rows 'N obs.' and 'N of FE' report the number of observations and fixed effects for each sample which is the same for each estimator in the same column. Heteroscedasticity robust standard errors clustered at plot level are used to estimate significance levels but not reported

Statistical significance levels are: '***' p-value < 0.001, '**' p-value < 0.01, '*' p-value < 0.05, p-value < '.' 0.1, and ' ' p-value >= 0.1.

Source: own calculations.

Pastures under individualized land tenure undergo a sharp change in the land use practices after the land is allocated and those systematically affect pasture quality. However, the mechanisms behind change in the land use are slightly different. For individual farms, land ownership that is associated with more extensive rights to re-distribute land use between individuals creates incentives for a more rational land use than simple land rental. While renting on a long-term basis may incentivize users to do the opposite and misuse the land either overgrazing it or simply avoiding managing it rationally. This highlights the key flaw of the land reform in creating efficient institutions for private property distribution that harms pastureland.

These conclusions confirm our **Hypothesis 2** about different incentives that the land reform created for pasture use under individualized tenures. They also signify the harmful role of segregating land institutions that prevent pasture exchange between different tenures and ownership types.

5.2 Spillover effects

Table 4 presents estimates of the ATT and spillover effects derived with the heterogeneity robust estimators using the full sample (Panel A) and excluding never-allocated land (Panel B). We only shows aggregated spillover effects on treated "Spillover on allocated" and on control "Spillover on unallocated", however, in the regression model, we specify spillovers by thresholds of 5, 10, 15, and 30 km, to identify spillover bins (j from Equation 3). The results indicate that the ATT of land allocation is still negative and significant with a magnitude that is similar to the model without spillovers. However, spillover effects are themselves pronounced and negative with the comparable to the ATT magnitude. Specifically, allocation of all land around an allocated or unallocated plot reduces the peak vegetation of the plot under investigation by another 0.2-1% aggravating the total effect of land allocation. Omitting spillovers from the regression causes an upward bias of our estimates $\tau_j^{\text{s. treated}} - \tau_j^{\text{s. control}} > 0$ because spillover effects on unallocated (control) are lower than that on allocated (treated) and both are negative $\tau^{\text{s. control}} < \tau_j^{\text{s. treated}}$ based on the SA model. Section I.9 in appendixes also pretenses the event-study for the model with spillover effects. Using different estimators result slightly different results, which might relate to the strings of identifying assumptions different between the estimators. Specifically, imputation estimators are tuned better for the ATT, while the SA may be better capturing all spectrum of effects.

Table 4: ATT and spillover effects by distance to village

	BM static	BM	SA	IMP static	IMP
Panel A. Full sample					
ATT	-0.0024*	-0.0085***	-0.0081***	-0.0032.	-0.0006
Spillover on allocated	-0.0092***	-0.0098***	-0.0101***	-0.0015***	-0.0024***
Spillover on unallocated	-0.0094***	-0.0109***	-0.0113***	-0.0013***	-0.0028***
N obs.	565,680	565,680	565,679	534,216	534,216
N ind. FE	23,570	23,570	23,570	22,259	22,259
Panel B. Excl. never-allocated					
ATT	0.0003	-0.0041**	-0.0080***	0.0076.	0.0049
Spillover on allocated	-0.0093***	-0.0104***	-0.0114***	-0.0103***	-0.0094***
Spillover on unallocated	-0.0083***	-0.0119***	-0.0139***	-0.0060***	-0.0054***
N obs.	380,088	380,088	380,087	334,098	334,098
N ind. FE	15,837	15,837	15,837	13,921	13,921

Note: Row 'ATT' reports the average treatment effect on the treated. Rows 'Spillovert on allocated' and 'Spillover on unallocated' report the magnitude of corresponding spillover effects originating from allocation of the plots in the neighborhood of the unit of analysis. Heteroscedasticity robust standard errors clustered at plot level are used but not reported.

	BM static	BM	SA	IMP static	IMP

Statistical significance levels are: '***' p-value < 0.001, '**' p-value < 0.01, '*' p-value < 0.05, p-value < '.' 0.1, and ' ' p-value >= 0.1.

Source: own calculations.

Spillover effects differentiate by tenure regimes similarly to the ATT. Table 5 displays results estimated with the SA estimator for selected tenure categories with the sharp land use change due to spillovers, while Section I.9 in appendices shows it for the other tenure categories. Land allocation into ownership under individual farms is associated with a 4% reduction in pasture quality if we account for the spillover effects, which are also negative and significant. At the same time, effect of land allocation is zero, once we account for the spillovers in the ag-enterprises land tenure. Results for the other tenure categories are less conclusive.

Table 5: ATT and spillover effects by key tenure categories

	Ind. farm (all)	Ind. farm (own)	Ind. farm (rent)	Ag. ent. (all)	Ag. ent. (own)	Ag. ent. (rent)
Panel A. Full sample						
ATT	-0.0118***	-0.0399***	-0.0106***	0.0210	0.0167	0.0204
Spillover on allocated	-0.0092***	-0.0062***	-0.0111***	-0.0078***	-0.0075***	-0.0078***
Spillover on unallocated	-0.0108***	-0.0081***	-0.0124***	-0.0076***	-0.0074***	-0.0076***
N obs.	489,383	230,950	444,023	202,583	188,088	200,087
N ind. FE	20,391	9,623	18,501	8,441	7,837	8,337
Panel B. Excl. never-allocated						
ATT	-0.0163***	-0.0408*	-0.0153***	0.0343*	0.0294.	0.0455.
Spillover on allocated	-0.0112***	-0.0038	-0.0152***	-0.0190**	-0.0204*	0.0041
Spillover on unallocated	-0.0186***	-0.0126*	-0.0223***	-0.0086	-0.0126	0.0256
N obs.	303,791	45,358	258,431	16,991	14,495	2,496
N ind. FE	12,658	1,890	10,768	708	604	104

Note: The table reports results estimated using the SA estimator. Row 'ATT' reports the average treatment effect on the treated. Rows 'Spill on allocated' and 'Spill on unallocated' report the magnitude of corresponding spillover effects. Rows 'N obs.' and 'N of FE' report the number of observations and fixed effects for each sub-sample. Heteroscedasticity robust standard errors clustered at plot level are in parentheses.

Statistical significance levels are: '***' p-value < 0.001, '**' p-value < 0.01, '*' p-value < 0.05, p-value < '.' 0.1, and ' ' p-value >= 0.1.

Source: own calculations.

Finally, the spillover effects and ATT are more pronounced in proximity to villages and reduce when we move away from settlements (Table 6). For example, in the 2 and 5km proximity to villages, the ATT for all tenure categories varied between 1.1% and 1.7% reduction in the vegetation, while the spillover effects further added 1% to 1.7% in reduction of the vegetation. Moving beyond 10km from villages, only the ATT is significant in the range from 0.8% to 1.9%, whilst spillover effects are near to zero.

Different effects of land allocation and spillover by tenure categories once again confirm different land use patterns observed in the study region. From the fact that spillover effects on never-allocated land are strong and negative, we infer that landless herders and other livestock producers in the region do keep their livestock on the free land and its increasing scarcity causes livestock re-distribution to the not-yet allocated land. This is a first confirmation of **Hypothesis 3**, which holds concerning both individualized and not individualized land tenures. Besides, individuals who have access to land under ownership or rent are still using the opportunities to graze on the free un-fenced land. Therefore, we observe a strong negative effect of spillovers on allocated plots under all tenure categories. Nevertheless, spillovers on unallocated land are larger than that on allocated, suggesting that to some extent exclusion is taking place in selected tenure categories, especially in the proximity to villages. Considering individualized land tenure, such landowners may still use free land, if available, and slowly redistribute onto their land, as the neighborhood becomes increasingly allocated.

Table 6: ATT and spillover effects by distance to settlements

	2 km	2-5 km	5 km	5-10 km	10-more km
Panel A. Full sample					
ATT	-0.0171***	-0.0079*	-0.0124***	-0.0057.	0.0083*
Spillover on allocated	-0.0138***	-0.0126***	-0.0113***	-0.0173***	0.0008
Spillover on unallocated	-0.0172***	-0.0140***	-0.0135***	-0.0183***	0.0034.
N obs.	110,399	153,551	263,951	155,399	146,327
Panel B. Excl. never-allocated					
ATT	-0.0147*	-0.0117*	-0.0144***	-0.0133***	0.0191***
Spillover on allocated	-0.0097**	-0.0169***	-0.0118***	-0.0199***	0.0007
Spillover on unallocated	-0.0154**	-0.0218***	-0.0172***	-0.0268***	0.0068*
N obs.	65,831	105,695	171,527	110,231	98,327

Note: The table reports results estimated using the SA estimator. Row ’ATT’ reports the average treatment effect on the treated. Rows ’Spill on allocated’ and ’Spill on unallocated’ report the magnitude of corresponding spillover effects. Rows ’N obs.’ and ’N of FE’ report the number of observations and fixed effects for each sub-sample.
Heteroscedasticity robust standard errors clustered at plot level are in parentheses.
Statistical significance levels are: ‘***’ p-value < 0.001, ‘**’ p-value < 0.01, ‘*’ p-value < 0.05, p-value < ‘.’ 0.1, and ‘ ’ p-value >= 0.1.
Source: own calculations.

Including spillovers into the tenure-specific analysis uncovers harsh realities that a constraining land policy creates for individual land owners. Owning or (renting the land) may lead them to graze more heavily at those locations, causing a substantial reduction of the vegetation quality. This signifies the mismatch between the livestock holding and land allocation that occurred in the course of the last three decades and indicates that current institutions are failing to address this by creating land re-distributive capacities.

The ATT diminishing to zero and only mild spillover effects on unallocated land that are observed when we move away from the settlements confirms **Hypothesis 4**. This is likely to be connected to the grazing patterns that dominate the region, where using remote pastures is only possible when dedicated herders temporarily settle in those remote areas. As such migration is associated with substantial fixed costs, and livestock densities at those locations tend to be lower overall.

6 Conclusions

Our paper analyses the effect of land allocation (registration in the cadaster) on pasture quality in southern Kazakhstan. It is based on the unique natural experiment that emerged during Kazakhstan's transition from a planned economy to the market, when over three decades, state-owned land has been gradually distributed between various users under distinct tenure regimes. Land redistribution occurred through land reforms recognizing leasehold and private property rights on land, combined with the introduction of a modern cadaster. By 2022, 75% of pastures were distributed under such tenures regimes as permanent allocation to state agencies and common grazing by resident livestock owners, and individualized tenure by "individuals": agricultural enterprises and individual farms, on whom our analysis is focused in particular. Land reform enabled individuals to access land either through private ownership or (more commonly) through 49-year long-term leasehold from the state. We combined cadastral data with remotely sensed climatic and geographic characteristics, and plot vegetation density approximated with the average of the annual peak of the Normalized Difference Vegetation Index (NDVI) to ultimately understand how the land reform was implemented and what causal effect it had on pastures quality.

Our analysis shows that the act of land allocation causes on average a reduction in vegetation density equivalent to that under a mild drought that occurs once in 6 years. These results fail to confirm our hypothesis 1, which predicted an opposite outcome. We suspect that negative effects of privatization are caused by lack of land redistribution after allocation on the markets and weak enforcement of exclusion. These finding contradicts the expectations entrusted to the privatized land market (Binswanger, Deininger, and Feder 1995; Deininger and Feder 2001; Sadoulet, Murgai, and Janvry 2001; Holden and Otsuka 2014) and also observations made in rangelands in China and the USA (Buehler 2022; Hou, Liu, and Tian 2022; H. Li and Zhu 2023), where land redistribution through rental is usually possible after privatization.

Sample stratification by tenure confirms our expectations posited in hypothesis 2 about different land use practices linked to the tenures. Specifically, individualized land tenure implies a sharp change in the land use practices, immediately observed in the vegetation change. Such are findings for the individual farmers, who should be most incentivized to improve pasture management once securing tenure through ownership or even leasehold. It is surprising to observe that such producers tend to overgraze their pasture causing vegetation density to decline by up to 4% (equivalent to a drought that occurs once in 25 years). For tenures under common grazing, forests and protected areas, the act of registration only records already existing land use practices, therefore, we only observe spurious relationship between allocation and vegetation, which do not hold robustly.

The use of plot-level data permitted the estimation of spatial spillover effects, which we predicted would be negative (hypothesis 3). Although the idea of spatial spillovers is not new and their presence in pasture management has been occasionally discussed in the literature (Buehler 2022; Masami Kaneko et al. 2009), we show how detrimental their effect on allocated plots is when exclusion is weak and plot fencing does not take place after allocation. In most tenures categories, where no sharp change in land use takes place after allocation, spillovers from the allocation of the neighboring plots are the main cause of vegetation decline. Spillovers are of two kinds: from allocation of land in the neighborhood on unallocated and allocated plots. The latter spillover effect is stably smaller in magnitude than the first one. That indicates that partial exclusion does take place with land allocation, however, the rarity of fencing does not render spillover effects on allocated land to zero. Finally, as mainly landless households keep their livestock near to villages, the negative effect of land allocation and spillovers is stronger in the proximity to settlements (hypothesis 4) and reduces to zero at distances beyond which resident livestock are unlikely to move.

The response of vegetation to land allocation and additional negative spillover effects suggest that the intended results of land titling and privatization, in terms of improved pasture management, have not been realized. This may result from a combination of the poor institutional design that prevents land exchange (Kvartiuk and Petrick 2021) and ecosystem fragmentation created by individual titling (Behnke 2018). With the gradual recovery of the livestock numbers in the 2010s, livestock owners need to revive the migration grazing system that broke down in the 1990s following the restructuring of collective farms and a collapse in livestock numbers, as it allows livestock to capture the best forage at different times of the year. However, the fragmented landscape and high transaction costs of accessing pasture erect constraints to mobile grazing patterns.

We see two potential solutions to this problem. The first lies in the creation of a fully liberalized land market, allowing both sub-leasing and simplified lease transfer between producers that includes households. Such a practice of nondiscriminatory land use is being implemented in Ukraine. The second facilitates local collective action, to allocate much larger areas of common grazing land for regulated common use by households and farms with poor pasture access. The introduction of large commonly managed areas of pasture would reverse the fragmentation of pasture systems, allowing management at the landscape scale.

References

Abadie, Alberto. 2021. "Using Synthetic Controls: Feasibility, Data Requirements, and Methodological Aspects." *Journal of Economic Literature* 59 (2). American Economic Association: 391–425. doi:10.1257/jel.20191450.

Adamopoulos, Tasso, Loren Brandt, Jessica Leight, and Diego Restuccia. 2022. "Misallocation, Selection, and Productivity: A Quantitative Analysis With Panel Data From China." *Econometrica* 90 (3). The Econometric Society: 1261–82. doi:10.3982/ecta16598.

Alimaev, Ilya I, Carol Kerven, Aibyn Torekhanov, Roy Behnke, Kazbek Smailov, Vladimir Yurchenko, Zheksinbai Sisatov, and Kanat Shanbaev. 2008. "The Impact of Livestock Grazing on Soils and Vegetation Around Settlements in Southeast Kazakhstan." Book Section. In *The Socio-Economic Causes and Consequences of Desertification in Central Asia*, edited by Roy Behnke, 81–112. Springer.

Angrist, Joshua D., and Jörn-Steffen Pischke. 2009. *Mostly Harmless Econometrics*. Princeton University Press. doi:10.1515/9781400829828.

Aronow, Peter M., and Cyrus Samii. 2017. "Estimating Average Causal Effects Under General Interference, with Application to a Social Network Experiment." *The Annals of Applied Statistics* 11 (4). Institute of Mathematical Statistics. doi:10.1214/16-aoas1005.

Athey, Susan, and Guido W. Imbens. 2022. "Design-based analysis in Difference-In-Differences settings with staggered adoption." *Journal of Econometrics* 226 (1). Elsevier BV: 62–79. doi:10.1016/j.jeconom.2020.10.012.

Aybar, Cesar. 2022. *Rgee: R Bindings for Calling the 'Earth Engine' API*. https://CRAN.R-project.org/package=rgee.

Baker, Andrew C., David F. Larcker, and Charles C. Y. Wang. 2022. "How Much Should We Trust Staggered Difference-in-Differences Estimates?" *Journal of Financial Economics* 144 (2). Elsevier BV: 370–95. doi:10.1016/j.jfineco.2022.01.004.

Behnke, Roy. 2018. "Open Access and the Sovereign Commons: A Political Ecology of Pastoral Land Tenure." Journal Article. *Land Use Policy* 76: 708–18.

Behnke, Roy, Sarah Robinson, and E. J. Milner-Gulland. 2016. "Governing open access: livestock distributions and institutional control in the Karakum Desert of Turkmenistan." *Land Use Policy* 52 (March). Elsevier BV: 103–19. doi:10.1016/j.landusepol.2015.12.012.

Berg, Tobias, Markus Reisinger, and Daniel Streitz. 2021. "Spillover Effects in Empirical Corporate Finance." *Journal of Financial Economics* 142 (3). Elsevier BV: 1109–27. doi:10.1016/j.jfineco.2021.04.039.

Bergé, Laurent. 2018. "Efficient Estimation of Maximum Likelihood Models with Multiple Fixed-Effects: The R Package FENmlm." *CREA Discussion Papers*, no. 13.

Besley, Timothy, and Maitreesh Ghatak. 2010. "Property Rights and Economic Development□." In *Handbook of Development Economics*, 5:4525–95. Elsevier. doi:10.1016/b978-0-444-52944-2.00006-9.

Binswanger, Hans P., Klaus Deininger, and Gershon Feder. 1995. "Power, Distortions, Revolt and Reform in Agricultural Land Relations." In *Handbook of Development Economics*, edited by J Behrman and T. N. Srinivasan, 3:2659–2772. Elsevier Science B.V. doi:10.1016/s1573-4471(95)30019-8.

Bojinov, Iavor, Ashesh Rambachan, and Neil Shephard. 2021. "Panel Experiments and Dynamic

Causal Effects: A Finite Population Perspective." *Quantitative Economics* 12 (4). The Econometric Society: 1171–96. doi:10.3982/qe1744.

Boone, R. B., and N. T. Hobbs. 2004. "Lines Around Fragments: Effects of Fencing on Large Herbivores." Journal Article. *African Journal of Range and Forage Science* 21 (3): 147–58.

Borusyak, Kirill, Xavier Jaravel, and Jann Spiess. 2023. "Revisiting Event Study Designs: Robust and Efficient Estimation." *Review of Economic Studies*.

Buehler, Mathias. 2022. "On the Other Side of the Fence: Property Rights and Productivity in the United States." *Journal of the European Economic Association* 21 (1). Oxford University Press (OUP): 93–134. doi:10.1093/jeea/jvac029.

Butts, Kyle. 2021a. *Didimputation: Difference-in-Differences Estimator from Borusyak, Jaravel, and Spiess (2021)*. https://github.com/kylebutts/didimputation.

———. 2021b. "Difference-in-Differences Estimation with Spatial Spillovers," May. arXiv. doi:10.48550/ARXIV.2105.03737.

Butts, Kyle, and John Gardner. 2021. *Did2s: Two-Stage Difference-in-Differences Following Gardner (2021)*. https://journal.r-project.org/articles/RJ-2022-048/.

Callaway, Brantly, and Pedro H. C. Sant'Anna. 2021a. "Did: Difference in Differences." https://bcallaway11.github.io/did/.

———. 2021b. "Difference-in-Differences with multiple time periods." *Journal of Econometrics* 225 (2). Elsevier BV: 200–230. doi:10.1016/j.jeconom.2020.12.001.

Card, David, and Alan B. Krueger. 1994. "Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania." *The American Economic Review* 84 (4). American Economic Association: 772–93. http://www.jstor.org/stable/2118030.

Chari, Amalavoyal, Elaine M. Liu, Shing-Yi Wang, and Yongxiang Wang. 2021. "Property Rights, Land Misallocation, and Agricultural Efficiency in China." Edited by Uta Schoenberg. *The Review of Economic Studies* 88 (4). Oxford University Press (OUP): 1831–62. doi:10.1093/restud/rdaa072.

Cinelli, Carlos, Andrew Forney, and Judea Pearl. 2022. "A Crash Course in Good and Bad Controls." *Sociological Methods & Research*, May. SAGE Publications, 004912412210995. doi:10.1177/00491241221099552.

Clarke, Damian. 2017. "Estimating Difference-in-Differences in the Presence of Spillovers." MPRA Paper No. 81604,. https://mpra.ub.uni-muenchen.de/81604/.

Coase, R. H. 1960. "The Problem of Social Cost." *The Journal of Law and Economics* 3. University of Chicago Press. doi:10.1086/466560.

Dara, A., M. Baumann, Freitag M., N. Hölzel, P. Hostert, J. Kamp, D. Müller, Prishchepov A. V., and T. Kuemmerle. 2020. "Annual Landsat Time Series Reveal Post-Soviet Changes in Grazing Pressure." Journal Article. *Remote Sensing of Environment* 239.

de Chaisemartin, Clément, and Xavier DHaultfoeuille. 2020. "Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects." *American Economic Review* 110 (9). American Economic Association: 2964–96. doi:10.1257/aer.20181169.

———. 2022. "Two-Way Fixed Effects and Differences-in-Differences with Heterogeneous Treatment Effects: A Survey." *The Econometrics Journal*, June. Oxford University Press (OUP). doi:10.1093/ectj/utac017.

Deininger, Klaus, and Hans Binswanger. 2001. "The Evolution of the World Bank's Land Policy." In *Access to Land, Rural Poverty, and Public Action*. Oxford University Press. doi:10.1093/acprof:oso/9780199242177.003.0017.

Deininger, Klaus, and Gershon Feder. 2001. "Land institutions and land markets." In *Handbook of Agricultural Economics*, edited by B Gardner and G Rausser, 1:288–331. PART A. Elsevier Science B.V. doi:10.1016/S1574-0072(01)10009-5.

Deininger, Klaus, and Songqing Jin. 2005. "The Potential of Land Rental Markets in the Process of Economic Development: Evidence from China." *Journal of Development Economics* 78 (1). Elsevier BV: 241–70. doi:10.1016/j.jdeveco.2004.08.002.

———. 2006. "Tenure Security and Land-Related Investment: Evidence from Ethiopia" 50 (5). Elsevier BV: 1245–77. doi:10.1016/j.euroecorev.2005.02.001.

Di Tella, Rafael, and Ernesto Schargrodsky. 2004. "Do Police Reduce Crime? Estimates Using the Allocation of Police Forces After a Terrorist Attack." *American Economic Review* 94 (1). American Economic Association: 115–33. doi:10.1257/000282804322970733.

Didan, Kamel. 2021. "MODIS/Terra Vegetation Indices 16-Day L3 Global 250m SIN Grid V061." NASA EOSDIS Land Processes Distributed Active Archive Center. doi:10.5067/MODIS/MOD13Q1.061.

Dietz, Thomas, Elinor Ostrom, and Paul C. Stern. 2003. "The Struggle to Govern the Commons." *Science* 302 (5652). American Association for the Advancement of Science (AAAS): 1907–12. doi:10.1126/science.1091015.

Directorate of the Land Cadastre in Kazakhstan. 2023a. "Land cover map." Department of the Land Cadastre and Automated Information System of the State Land Cadastre in Kazakhstan. https://aisgzk.kz/aisgzk/ru/content/maps?type=ug.

———. 2023b. "Land parcels map." Department of the Land Cadastre and Automated Information System of the State Land Cadastre in Kazakhstan. https://aisgzk.kz/aisgzk/ru/content/maps.

Eddy, Ian M. S., Sarah E. Gergel, Nicholas C. Coops, Geoffrey M. Henebry, Jordan Levine, Hisham Zerriffi, and Evgenii Shibkov. 2017. "Integrating Remote Sensing and Local Ecological Knowledge to Monitor Rangeland Dynamics." *Ecological Indicators* 82 (November). Elsevier BV: 106–16. doi:10.1016/j.ecolind.2017.06.033.

Farr, Tom G., Paul A. Rosen, Edward Caro, Robert Crippen, Riley Duren, Scott Hensley, Michael Kobrick, et al. 2007. "The Shuttle Radar Topography Mission." *Reviews of Geophysics* 45 (2). American Geophysical Union (AGU). doi:10.1029/2005rg000183.

Funk, Chris, Pete Peterson, Martin Landsfeld, Diego Pedreros, James Verdin, Shraddhanand Shukla, Gregory Husak, et al. 2015. "The Climate Hazards Infrared Precipitation with Stations—a New Environmental Record for Monitoring Extremes." *Scientific Data* 2 (1). Springer Science; Business Media LLC. doi:10.1038/sdata.2015.66.

Galvin, K. A., R. S. Reid, Roy H. Behnke, and N. T. Hobbs. 2008. *Fragmentation in Semi-Arid and Arid Landscapes, Consequences for Human and Natural Systems*. Edited Book. Dordrecht: Springer.

Gardner, John. 2022. "Two-Stage Differences in Differences," July. arXiv. doi:10.48550/ARXIV.2207.05943.

Gongbuzeren, Jing Zhang, Minghao Zhuang, Jian Zhang, and Lynn Huntsinger. 2021. "Mitigating the Impacts of Fragmented Land Tenure Through Community-Based Institutional Innovations: Two Case Study Villages from Guinan County of Qinghai Province, China." Journal Article. *Ecology and Society* 26 (2).

Goodman-Bacon, Andrew. 2021. "Difference-in-Differences with Variation in Treatment Timing." *Journal of Econometrics* 225 (2). Elsevier BV: 254–77. doi:10.1016/j.jeconom.2021.03.014.

Gorelick, Noel, Matt Hancher, Mike Dixon, Simon Ilyushchenko, David Thau, and Rebecca Moore.

2017. “Google Earth Engine: Planetary-Scale Geospatial Analysis for Everyone.” *Remote Sensing of Environment*. Elsevier. doi:10.1016/j.rse.2017.06.031.

Halloran, M. Elizabeth, and Michael G. Hudgens. 2016. “Dependent Happenings: A Recent Methodological Review.” *Current Epidemiology Reports* 3 (4). Springer Science; Business Media LLC: 297–305. doi:10.1007/s40471-016-0086-4.

Hardin, G. 1968. “The Tragedy of the Commons.” Journal Article. *Science* 162: 243–1248.

Holden, Stein T., and Keijiro Otsuka. 2014. “The Roles of Land Tenure Reforms and Land Markets in the Context of Population Growth and Land Use Intensification in Africa.” Journal Article. *Food Policy*, 88–97.

Hou, Lingling, Pengfei Liu, and Xiaohui Tian. 2022. “Grassland Tenure Reform and Grassland Quality in China.” *American Journal of Agricultural Economics*, November. Wiley. doi:10.1111/ajae.12357.

Hulley, Glynn, and Simon Hook. 2021. “MODIS/Terra Land Surface Temperature/3-Band Emissivity Monthly L3 Global 0.05Deg CMG V061.” NASA EOSDIS Land Processes Distributed Active Archive Center. doi:10.5067/MODIS/MOD21C3.061.

Imbens, Guido W., and Donald B. Rubin. 2015. *Causal Inference in Statistics, Social, and Biomedical Sciences an Introduction: An Introduction*. Cambridge University Press.

Kazakhstan, Government of. 2017. “On Pastures.” Generic.

Kerven, Carol, Sarah Robinson, Roy Behnke, K. Kushenov, and E. J. Milner-Gulland. 2016. “A Pastoral Frontier: From Chaos to Capitalism and the Recolonisation of the Kazakh Rangelands.” Journal Article. *Journal of Arid Environments* 127: 106–19.

Kropko, Jonathan, and Robert Kubinec. 2020. “Interpretation and Identification of Within-Unit and Cross-Sectional Variation in Panel Data Models.” Edited by Talib Al-Ameri. *PLOS ONE* 15 (4). Public Library of Science (PLoS): e0231349. doi:10.1371/journal.pone.0231349.

Kvartiuk, Vasyl, and Martin Petrick. 2021. “Liberal Land Reform in Kazakhstan? The Effect on Land Rental and Credit Markets.” *World Development* 138 (February). Elsevier BV: 105285. doi:10.1016/j.worlddev.2020.105285.

Li, Ang, Jianguo Wu, and Jianhui Huang. 2012. “Distinguishing Between Human-Induced and Climate-Driven Vegetation Changes: A Critical Application of RESTREND in Inner Mongolia.” Journal Article. *Landscape Ecology* 27: 969–82.

Li, Dongqing, Lingling Hou, and Alec Zuo. 2021. “Informal institutions and grassland protection: Empirical evidence from pastoral regions in China.” *Ecological Economics* 188 (October). Elsevier BV: 107110. doi:10.1016/j.ecolecon.2021.107110.

Li, Haoyang, and Jiong Zhu. 2023. “Property Rights and Land Quality.” *American Journal of Agricultural Economics*, October. Wiley. doi:10.1111/ajae.12440.

Li, Wen Jun, Saleem H. Ali, and Qian Zhang. 2007. “Property Rights and Grassland Degradation: A Study of the Xilingol Pasture, Inner Mongolia, China.” *Journal of Environmental Management* 85 (2). Elsevier BV: 461–70. doi:10.1016/j.jenvman.2006.10.010.

Li, Wen Jun, and Lynn Huntsinger. 2011. “China’s Grassland Contract Policy and Its Impacts on Herder Ability to Benefit in Inner Mongolia: Tragic Feedbacks.” Journal Article. *Ecology and Society* 16 (2).

Lu, Luci, Ping Li, Margaret Kalacska, and Brian E Robinson. 2023. “Environmental Impacts of Renting Rangelands: Integrating Remote Sensing and Household Surveys at the Parcel Level.” Journal Article. *Environmental Research Letters* 18.

Manski, Charles F. 1993. “Identification of Endogenous Social Effects: The Reflection Problem." *The Review of Economic Studies* 60 (3). Oxford University Press (OUP): 531. doi:10.2307/2298123.

Marcus, Michelle, and Pedro H. C. Sant'Anna. 2021. "The Role of Parallel Trends in Event Study Settings: An Application to Environmental Economics." *Journal of the Association of Environmental and Resource Economists* 8 (2). University of Chicago Press: 235–75. doi:10.1086/711509.

Masami Kaneko, Buho Hoshino, Teruo Matsunaka, Satomi Shimada, and Chifumi Ono. 2009. "A Comparative Study of Pasture Degradation of Inner Mongolian Fenced and Unfenced Land Based on Remotely Sensed Data." *Journal Rakuno Gakuen University*. https://api.semanticscholar.org/CorpusID:131837986.

McDermott, Grant. 2023. *Etwfe: Extended Two-Way Fixed Effects*. https://CRAN.R-project.org/package=etwfe.

Mukhtarova, Akbikesh. 2022. "Land Commissions in Kazakhstan: The Problem of Civil Society Participation in Land Governance." Thesis.

Ostrom, E. 1990. *Governing the Commons: The Evolution of Institutions for Collective Action*. Book. Cambridge: Cambridge University Press.

Parliament of the Republick of Kazakhstan. 2023. "Zemel'nyj Kodeks Respubliki Kazahstan Ot 20 Ijunja 2003 Goda No 442-II (s Izmenenijami i Dopolnenijami Po Sostojaniju Na 07.03.2023 g.) [Land Code of the Republic of Kazakhstan from 20 June 2003 No. 422-II (with Changes and Ammendments as of 07 March 2023)]."

Petrick, Martin. 2021. "Post-Soviet Agricultural Restructuring: A Success Story After All?" *Comparative Economic Studies*. Springer, 1–25. doi:10.1057/s41294-021-00172-1.

Pettorelli, Nathalie, Jon Olav Vik, Atle Mysterud, Jean-Michel Gaillard, Compton J. Tucker, and Nils Chr. Stenseth. 2005. "Using the Satellite-Derived NDVI to Assess Ecological Responses to Environmental Change." *Trends in Ecology & Evolution* 20 (9). Elsevier BV: 503–10. doi:10.1016/j.tree.2005.05.011.

Reid, R. S., K. A. Galvin, and R. S. Kruska. 2008. "Global Significance of Extensive Grazing Lands and Pastoral Societies: An Introduction." Book Section. In *Fragmentation in Semi-Arid and Arid Landscapes*, edited by K. Galvin, R. Reid, Roy Behnke, and N. Hobbs, 1–24. Heidelberg, Germany: Springer.

Robinson, Sarah, Zhanyl Bozayeva, Nozilakhon Mukhamedova, Nodir Djanibekov, Dauren Oshakbayev, and Martin Petrick. 2021. "Obstacles to the Revival of Mobile Grazing Systems in Kazakhstan." Journal Article. *Proceedings of the Joint XXIV International Grassland & XI International Rangeland Congress*.

Robinson, Sarah, Zhanyl Bozayeva, Nozilakhon Mukhamedova, Nodir Djanibekov, and Martin Petrick. 2021. "Ranchers or Pastoralists? Farm Size, Specialisation and Production Strategy Amongst Cattle Farmers in South-Eastern Kazakhstan." Journal Article. *Pastoralism* 11 (31). doi:https://doi.org/10.1186/s13570-021-00217-1.

Robinson, Sarah, C. Jamsranjav, and K. Gillin. 2017. "Pastoral Property Rights in Central Asia Factors and Actors Driving the Reform Agenda." Book Section. In *Verte La Steppe?*, edited by M. Elie and C. Ferret, 220–53. Éditions de l'EHESS.

Robinson, Sarah, Carol Kerven, Roy Behnke, K. Kushenov, and E. J. Milner-Gulland. 2016. "Pastoralists as Optimal Foragers? Reoccupation and Site Selection in the Deserts of Post-Soviet Kazakhstan." *Human Ecology* 45 (1). Springer Science; Business Media LLC: 5–21. doi:10.1007/s10745-016-9870-5.

Rodell, M., P. R. Houser, U. Jambor, J. Gottschalck, K. Mitchell, C.-J. Meng, K. Arsenault, et al. 2004. "The Global Land Data Assimilation System." *Bulletin of the American Meteorological Society* 85 (3). American Meteorological Society: 381–94. doi:10.1175/bams-85-3-381.

Rohde, R. F., M. M. Moleele, M. Mphale, N. Allsopp, R. Chanda, M. T. Hoffman, L. Magole, and E. Young. 2006. "Dynamics of Grazing Policy and Practice: Environmental and Social Impacts in Three Communal Areas of Southern Africa." Journal Article. *Environmental Science and Policy* 9: 302–16.

Roth, Jonathan, and Pedro H. C. Sant'Anna. 2021. "Efficient Estimation for Staggered Rollout Designs," February. arXiv. doi:10.48550/ARXIV.2102.01291.

Roth, Jonathan, Pedro H. C. Sant'Anna, Alyssa Bilinski, and John Poe. 2023. "What's Trending in Difference-in-Differences? A Synthesis of the Recent Econometrics Literature." *Journal of Econometrics* 235 (2). Elsevier BV: 2218–44. doi:10.1016/j.jeconom.2023.03.008.

Rubin, Donald B. 1974. "Estimating Causal Effects of Treatments in Randomized and Nonrandomized Studies." *Journal of Educational Psychology* 66 (5): 688–701. doi:10.1037/h0037350.

Sadoulet, Elisabeth, Rinku Murgai, and Alian de Janvry. 2001. "Access to Land via Land Rental Markets." In *Access to Land, Rural Poverty, and Public Action*, edited by Alain de Janvry, Gustavo Gordillo, Elisabeth Sadoulet, and Jean-Philippe Platteau, 196–229. Oxford University Press. doi:10.1093/acprof:oso/9780199242177.003.0008.

Sant'Anna, Pedro H. C., and Jun Zhao. 2020. "Doubly Robust Difference-in-Differences Estimators." *Journal of Econometrics* 219 (1). Elsevier BV: 101–22. doi:10.1016/j.jeconom.2020.06.003.

Sävje, Fredrik, Peter M. Aronow, and Michael G. Hudgens. 2021. "Average Treatment Effects in the Presence of Unknown Interference." *The Annals of Statistics* 49 (2). Institute of Mathematical Statistics. doi:10.1214/20-aos1973.

Sun, Liyang, and Sarah Abraham. 2021. "Estimating Dynamic Treatment Effects in Event Studies with Heterogeneous Treatment Effects." *Journal of Econometrics* 225 (2). Elsevier BV: 175–99. doi:10.1016/j.jeconom.2020.09.006.

Vazquez-Bare, Gonzalo. 2022. "Identification and Estimation of Spillover Effects in Randomized Experiments." *Journal of Econometrics*, January. Elsevier BV, 105237. doi:10.1016/j.jeconom.2021.10.014.

Wooldridge, Jeffrey M. 2023. "Simple Approaches to Nonlinear Difference-in-Differences with Panel Data." *The Econometrics Journal* 26 (3). Oxford University Press (OUP): C31–66. doi:10.1093/ectj/utad016.

Xu, Ruonan. 2023. "Difference-in-Differences with Interference: A Finite Population Perspective," June. arXiv. doi:10.48550/ARXIV.2306.12003.

Zhumanova, Munavar, Carsten Mönnig, Christian Hergarten, Dietrich Darr, and Nicole Wrage-Mönnig. 2018. "Assessment of Vegetation Degradation in Mountainous Pastures of the Western Tien-Shan, Kyrgyzstan, Using eMODIS NDVI." Journal Article. *Ecological Indicators* 95: 527–43.

Appendixes

Available on demand.

A.1 Description of the study region

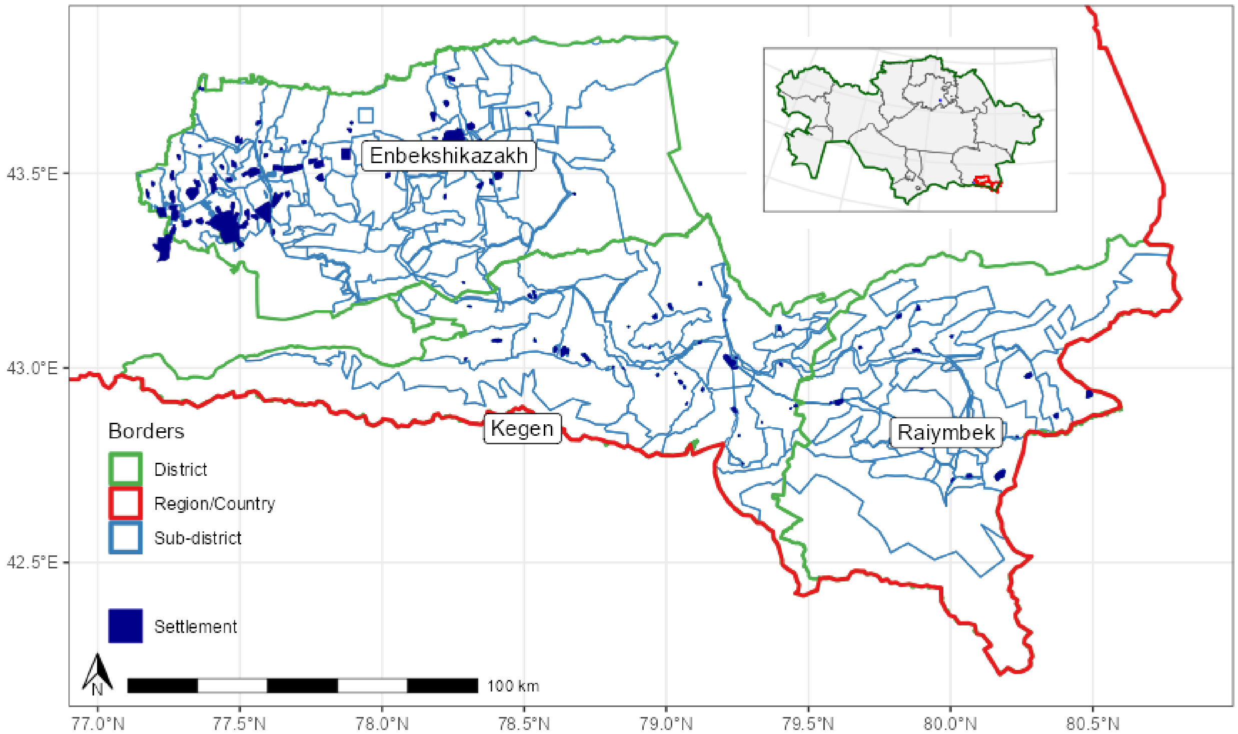

Figure A1: Administrative division of the study region

We focus on three districts of the Almaty Region Figure A1. Each district is further subdivided into sub-districts, which are used for cadastre accounting purposes. Historically sub-districts were created based on the legacy cadastre division that was used to distribute agricultural land between large-scale collective and state agricultural enterprises (Kolkhoz and Sovkhoz). Some sub-districts currently exist to represent specific roads or infrastructural objects, which we take into consideration when we create polygons of our units of analysis.

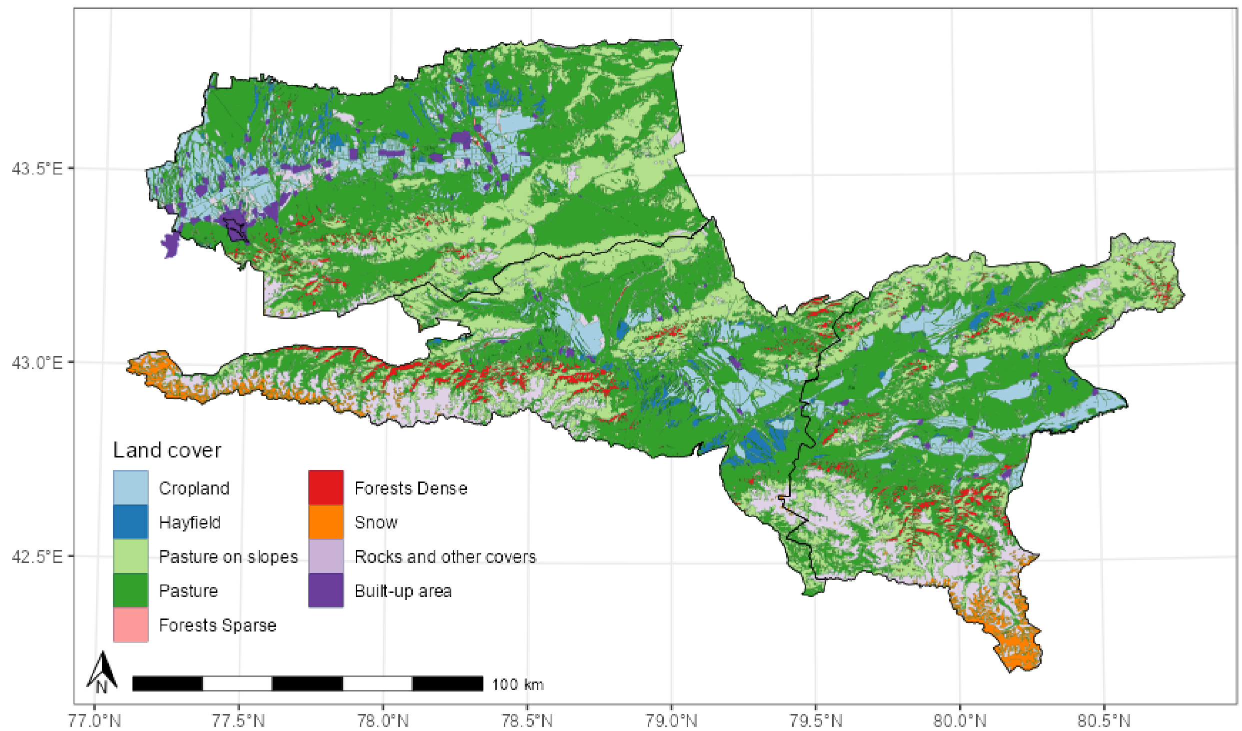

Figure A2: Land cover types in the study area

The geographic boundaries of the land cover describe classes of land that cover the surface. Initially, the cadastre map disseminates extremely detailed land cover categories, which we aggregate into a shorter list of land cover categories displayed on Figure A2. A complete mapping table could be provided on request. The land cover classes describe the possibility of using land for various agricultural purposes, such as grazing on pastures and crop production on the cropland. However, land cover categories are not restrictive according to the Land Code (2023), therefore, occasional grazing on arable land is possible, although unlikely. Pastures are represented by two broad land cover classes namely, pastures and pastures on slopes. They differ in geographical characteristics and, therefore, quality of vegetation, however, they are used for grazing without discrimination.

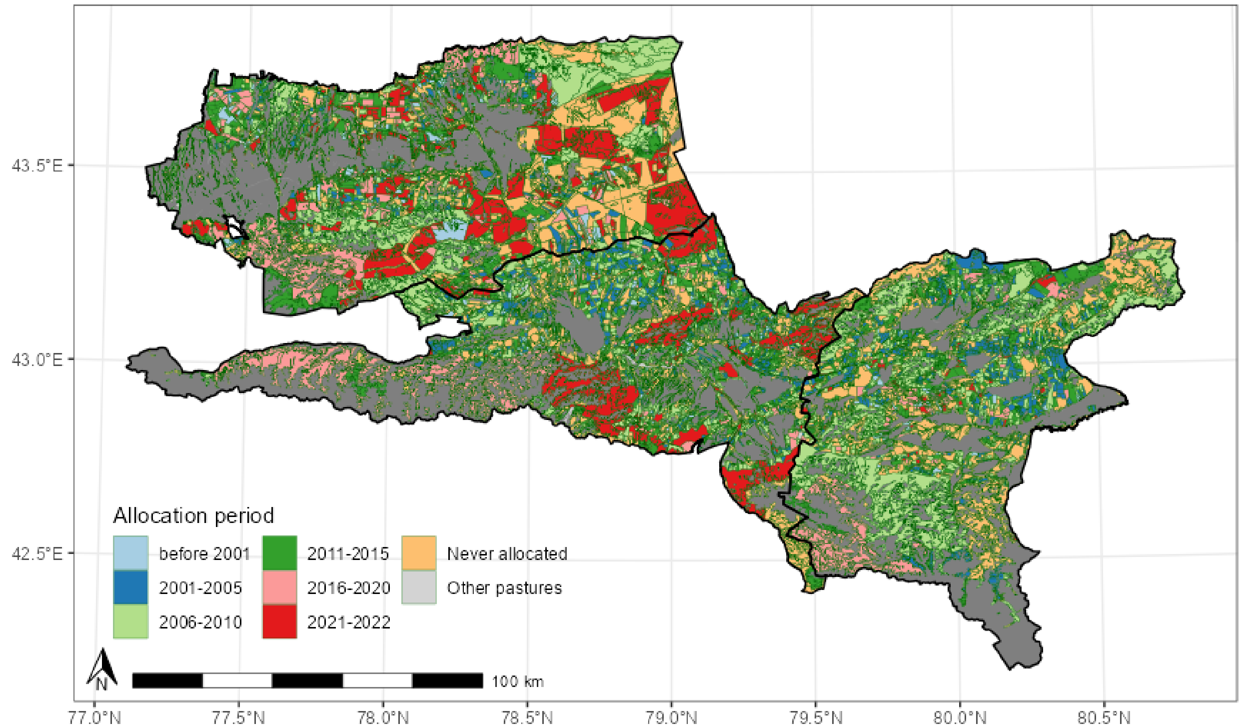

Figure A3: Spatial distribution of plots allocation by date on pastures

Figure A3 shows a map of plot allocation by periods, while tables A1 and A2 report the dynamics of land allocation by year. We observe the gradual allocation of plots over time except for some years (e.g. 2007, 2019, 2022), where an abnormally large number of plots were allocated at once. These exceptional land allocations are caused by the registration of the plots as forest lands and reserves, which are usually large and remote. Finally, most of the common grazing land was allocated in 2020-2020, while only single plots were allocated as far as 7 years before the end of observations.

Each parcel (allocated or never-allocated) spans different types of **land cover** (e.g. pastures, pastures on slopes, hay fields, arable land, forest land, etc.). Land cover determines whether a plot can or cannot be used for an agricultural purposes such as crop production or livestock grazing Figure A2. The same website disseminates detailed land cover maps (Directorate of the Land Cadastre in Kazakhstan 2023a).

Table A1: Dynamics of land allocation by plots and parcels

	By parcel on all land cover types				By plot on pastures			
Year	N	Area (HA)	cumulative % of parcels	cumulative % of area	N	Area (HA)	cumulative % of plots	cumulative % area
before 2000	892	43 862.4	6.4%	2.1%	1,174	37 272.2	4.3%	2.3%
2000	112	12 175.4	7.2%	2.7%	137	10 936.9	4.9%	3.0%
2001	329	22 016.8	9.6%	3.8%	448	19 303.1	6.5%	4.2%
2002	282	30 341.9	11.6%	5.3%	460	22 153.3	8.2%	5.6%
2003	182	14 746.5	12.9%	6.0%	240	12 324.3	9.1%	6.3%
2004	467	43 977.1	16.3%	8.1%	678	38 700.7	11.6%	8.7%
2005	590	49 808.6	20.5%	10.5%	899	43 541.4	14.9%	11.5%
2006	627	69 637.2	25.0%	13.9%	990	57 289.6	18.6%	15.0%
2007	795	277 221.1	30.7%	27.3%	1,721	210 954.3	25.0%	28.1%
2008	423	45 379.1	33.8%	29.5%	659	35 445.5	27.4%	30.3%
2009	261	20 415.8	35.6%	30.5%	373	17 714.6	28.8%	31.4%
2010	376	68 075.6	38.3%	33.8%	722	51 960.7	31.5%	34.7%
2011	350	35 346.1	40.8%	35.5%	560	24 874.5	33.5%	36.2%
2012	435	74 182.7	44.0%	39.1%	794	53 050.4	36.5%	39.5%
2013	330	29 361.7	46.3%	40.6%	475	26 172.1	38.2%	41.1%
2014	346	30 163.2	48.8%	42.0%	518	25 073.5	40.1%	42.7%
2015	393	35 591.1	51.6%	43.8%	602	29 720.1	42.4%	44.5%
2016	403	97 810.5	54.5%	48.5%	939	63 454.4	45.9%	48.5%
2017	253	71 739.1	56.4%	52.0%	505	50 971.1	47.7%	51.7%
2018	183	17 541.2	57.7%	52.8%	275	13 908.7	48.7%	52.5%
2019	214	135 784.2	59.2%	59.4%	721	70 379.2	51.4%	56.9%
2020	183	19 127.4	60.5%	60.3%	270	16 940.9	52.4%	57.9%
2021	323	90 524.6	62.8%	64.7%	662	69 961.1	54.9%	62.3%
2022	412	185 979.9	65.8%	73.8%	1,013	166 531.7	58.6%	72.6%
Never allocated	4,761	541 241.1	100.0%	100.0%	7,735	380 341.5	87.2%	96.3%

Source: own calculations.

Table A2: Dynamics of plots allocation by land use

Year	Ind. farm (own)	Ind. farm (rent)	Ag. ent. (own)	Ag. ent. (rent)	Common (near)	Common (remote)	Forest	Protected areas	Household	Other
before 2000	0.1 [7]	36.2 [1155]		1.0 [12]						
	(6)	(877)		(9)						
2000	0.0 [2]	3.2 [105]		7.7 [29]					0.1 [1]	
	(2)	(89)		(20)					(1)	
2001	0.0 [1]	19.0 [431]		0.3 [10]					0.1 [6]	
	(1)	(315)		(7)					(6)	
2002	0.1 [2]	18.9 [419]		3.2 [37]						0.0 [2]
	(2)	(266)		(12)						(2)
2003	0.1 [3]	10.7 [220]		1.5 [17]						
	(3)	(166)		(13)						
2004	1.5 [61]	30.8 [584]	0.1 [6]	6.2 [24]						0.0 [3]
	(51)	(395)	(3)	(15)						(3)
2005	1.9 [76]	36.9 [756]	0.4 [17]	4.2 [48]					0.2 [2]	
	(59)	(495)	(14)	(21)					(1)	
2006	1.9 [94]	34.1 [679]	0.1 [6]	6.7 [71]				11.4 [67]	2.1 [34]	0.9 [39]
	(77)	(466)	(5)	(35)				(1)	(24)	(19)
2007	5.1 [173]	38.7 [786]	1.5 [44]	13.8 [171]			145.8 [493]	0.1 [8]	0.0 [4]	6.0 [42]
	(132)	(521)	(18)	(67)			(25)	(1)	(4)	(27)
2008	10.4 [260]	10.3 [186]	5.3 [29]	6.4 [93]			0.1 [2]	0.0 [1]	0.1 [8]	2.9 [80]
	(205)	(135)	(13)	(42)			(2)	(1)	(5)	(20)
2009	2.2 [86]	9.7 [188]	0.3 [9]	5.3 [60]			0.0 [1]		0.1 [21]	0.0 [8]
	(70)	(134)	(9)	(22)			(1)		(21)	(4)
2010	2.2 [76]	21.2 [424]	0.2 [11]	5.9 [82]			22.4 [123]			0.1 [6]
	(64)	(261)	(11)	(29)			(6)			(5)
2011	3.1 [76]	19.0 [401]	1.0 [19]	1.2 [35]			0.3 [9]		0.1 [4]	0.2 [16]
	(63)	(248)	(7)	(16)			(3)		(2)	(11)
2012	3.9 [138]	21.5 [348]	3.2 [51]	15.1 [161]			0.5 [11]	8.7 [63]	0.1 [7]	0.1 [15]
	(114)	(246)	(11)	(40)			(4)	(2)	(4)	(14)
2013	2.1 [83]	15.2 [261]	0.5 [12]	8.0 [54]			0.2 [10]		0.0 [5]	0.2 [50]
	(67)	(173)	(11)	(25)			(2)		(4)	(48)
2014	5.0 [122]	16.4 [327]	0.4 [6]	2.9 [24]	0.1 [5]		0.0 [1]	0.1 [2]	0.1 [14]	0.1 [17]
	(81)	(209)	(6)	(17)	(1)		(1)	(1)	(14)	(16)
2015	2.7 [96]	22.0 [391]	1.2 [20]	3.1 [58]			0.2 [7]	0.0 [1]	0.0 [4]	0.5 [25]
	(80)	(236)	(15)	(31)			(3)	(1)	(4)	(23)
2016	3.8 [157]	15.7 [305]	0.2 [4]	7.2 [84]			0.4 [9]	36.0 [359]		0.1 [21]
	(135)	(188)	(4)	(44)			(9)	(2)		(21)
2017	1.4 [62]	5.7 [142]	0.9 [18]	37.5 [198]	0.1 [1]		3.8 [62]	1.5 [3]	0.0 [6]	0.1 [13]
	(56)	(104)	(10)	(30)	(1)		(33)	(1)	(6)	(12)
2018	0.9 [38]	9.9 [153]	0.6 [8]	2.2 [51]	0.0 [2]		0.2 [8]	0.0 [2]		0.1 [13]
	(35)	(101)	(5)	(21)	(2)		(8)	(2)		(9)

Year	Ind. farm (own)	Ind. farm (rent)	Ag. ent. (own)	Ag. ent. (rent)	Common (near)	Common (remote)	Forest	Protected areas	Household	Other
2019	1.4 [56]	6.8 [174]	1.2 [11]	3.1 [73]	1.1 [12]		0.0 [2]	56.6 [374]		0.2 [19]
	(44)	(105)	(9)	(23)	(9)		(2)	(5)		(17)
2020	0.9 [39]	7.6 [153]	0.2 [6]	6.0 [47]	1.9 [11]	0.3 [1]				0.1 [13]
	(30)	(103)	(4)	(23)	(9)	(1)				(13)
2021	8.8 [59]	10.5 [239]	0.1 [7]	7.3 [67]	6.6 [57]	1.0 [2]	33.7 [186]	0.0 [1]		2.0 [44]
	(28)	(158)	(7)	(42)	(33)	(2)	(21)	(1)		(31)
2022	5.8 [50]	14.3 [285]	0.2 [8]	19.4 [157]	22.6 [149]	64.9 [186]	30.0 [152]	9.2 [20]	0.1 [6]	
	(31)	(177)	(8)	(58)	(88)	(37)	(12)	(2)	(5)	
All years	65.3 [1819]	434.1 [9112]	17.5 [292]	175.1 [1663]	32.4 [237]	66.2 [189]	237.6 [1076]	123.7 [901]	3.0 [122]	13.9 [426]
	(1437)	(6168)	(170)	(662)	(143)	(40)	(132)	(20)	(101)	(295)

Note: Numbers in columns report the area of land in 1000 ha allocated each year by tenure category. The number of plots is in square brackets and the number of parcels is in parentheses. Empty cells imply no land allocated under such a category.

Source: own calculations.

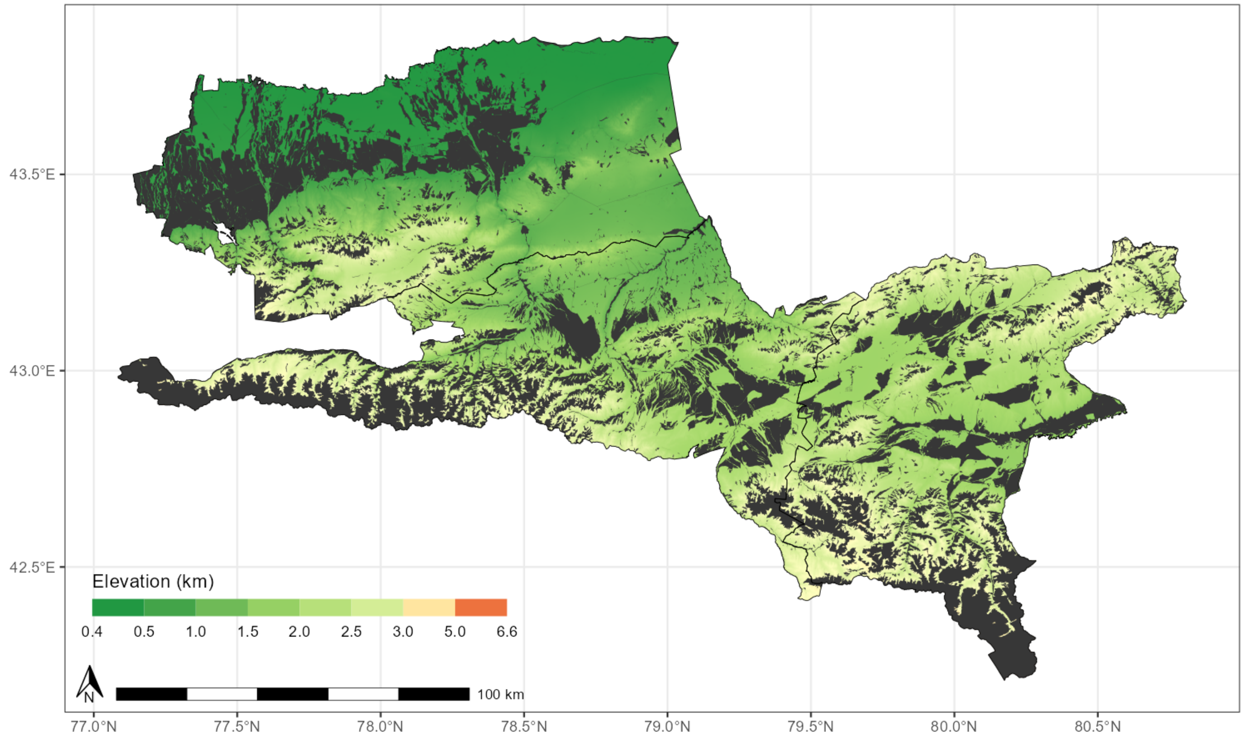

Figure A4: Elevation of pastures

B.2 Methodologies of spatial data preparation

B.2.1 Goegraphic boundaries of allocated parcels and their metadata

Kazakh State cadastre maps are available according to article 44-1 of the Land Code (2023) from the following website https://aisgzk.kz/aisgzk/ru/content/maps/. One can freely browse the map there and see what metadata accompanies the geographical boundaries of the allocated plots. Essentially, we used geospatial data as is, without manual editing. We gather that the total of 109,668 parcels were assigned in the cadastre in tour study regions as of January 2023, when our data collection was made. In the study region, we

As plots are often allocated at different times, the legal framework, when such plots are created is different. Therefore, tenure categories, legal regimes, and other metadata are often not standardized. We carefully recoded land-use categories and legal regimes relevant to each plot based on the metadata resulting in the tenure categories used in our analysis.

The land allocation date was also recorded chaotically sometimes. The majority of plots are designated with exact allocation dates. Those bared such date alone with a note in the metadata which specified the decree with which this plot was allocated and when. Other plots had the date of the first normative evaluation, which is the administrative mechanism in which the value of land is identified in the absence of the land market according to the Land Code (2023). Some plots only had a polygon creation date. As normative evaluation could take place after the plot was first assigned, usually used the plot creation data in the cadastre as an approximate indicator of the time when such a plot was allocated.

B.2.2 Land cover maps

Detailed land cover maps are also obtained from the website of the cadastre https://aisgzk.kz/aisgzk/ru/content/maps/ where they are disseminated under the name "karta ugodij". The land cover map contains a detailed list of land cover categories that nests from the general aggregates outlined in Figure A2. For the purpose of our analysis, we dissolved boundaries between landcover categories that belong to the same group from Figure A2. The land cover map is not ultimate and provides cover classes only for 97% of the study region.

B.2.3 Refining never-allocated parcels

Never-allocated parcels are created synthetically as a spatial difference between regions and all allocated parcels. Such difference as is includes any gap between the allocated parcels, which produces multiple slivers (elongated polygons), lines, and points that are condensed in a handful of multi-polygons spanning across regions. Therefore, we needed to improve the shape of the un-allocated parcels into more robustly shaped polygons. To do so, we followed our own algorithm. Table B1 summaries changes in the area of un-allocated land and number of parcels, Figure B1, Figure B2, and Figure B3 provide examples of parcels created based on the unallocated ares applying the following cleaning algorithm.

Step 1. Produce spatial differences between the area of the each sub-district and allocated parcels. In the cadastre, every exiting plot is allocated within the boundaries of each sub-district. This step created **18,109** "mega-parcels".

Step 2. Removing sliver mega-parcels by performing negative buffering of polygons for -25 meters and removing any polygon that is less than 100 sq meters in size after negative buffering. Produces **1,681** parcels in the original size and shape that constitute 99.99% of all unallocated polygons.

Step 3. After all previous steps, the unallocated polygons are still single mega-parcels that fill all gaps between allocated land. They still contain slivers that are not independent, but parts of mega-parcels. To separate these slivers from larger polygons and break large polygons into smaller uniformly shaped parts, we apply the polygon-refining algorithm 3 times with the following `buffer values` (750, 250, and 25 meters), corresponding `filtering buffer` values (100, 50, and 25 meters) and `minimal size thresholds` (1000, 500, and 100 sq meters). This cleaning step results in **8,909** parcels that are **96.2%** of the area of all unallocated land.

Polygon-refining algorithm:

1. Construct simplified polygons by buffering and de-buffering all multi-polygons by negative `buffer value` and then positive `buffer value`.
2. Construct perimeter lines out of the buffered/de-buffered polygons;
3. Use these lines to slice original polygons into multiple polygons;
4. Remove polygons that are smaller than `minimal size thresholds` after buffering by a negative `filtering buffer`.

Table B1: Number and area of parcels created on unallocated land at different cleaning steps

Step	N parcels	Area, 1000 ha
Step1	18,109	593.8
Step2	1,681	591.6
Step3	8,909	571.5

Source: own calculations.

B.2.4 Refining plots / units of analysis

Creating units of analysis consists of several steps summarised below. Table Table B2 shows the change in s number of plots, parcels, and area of land under analysis at different steps of data processing:

Step 1. Take the data with all allocated parcels as-is.

Step 2. Take all never-unallocated parcels pre-processed following the above-described methodology.

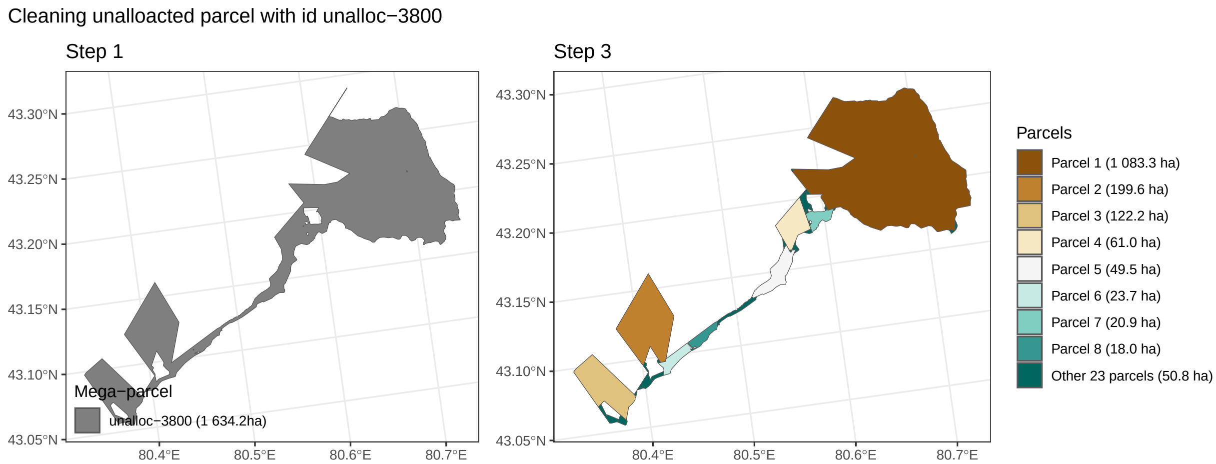

Figure B1: Example of an anallocated parcels cleaning 1

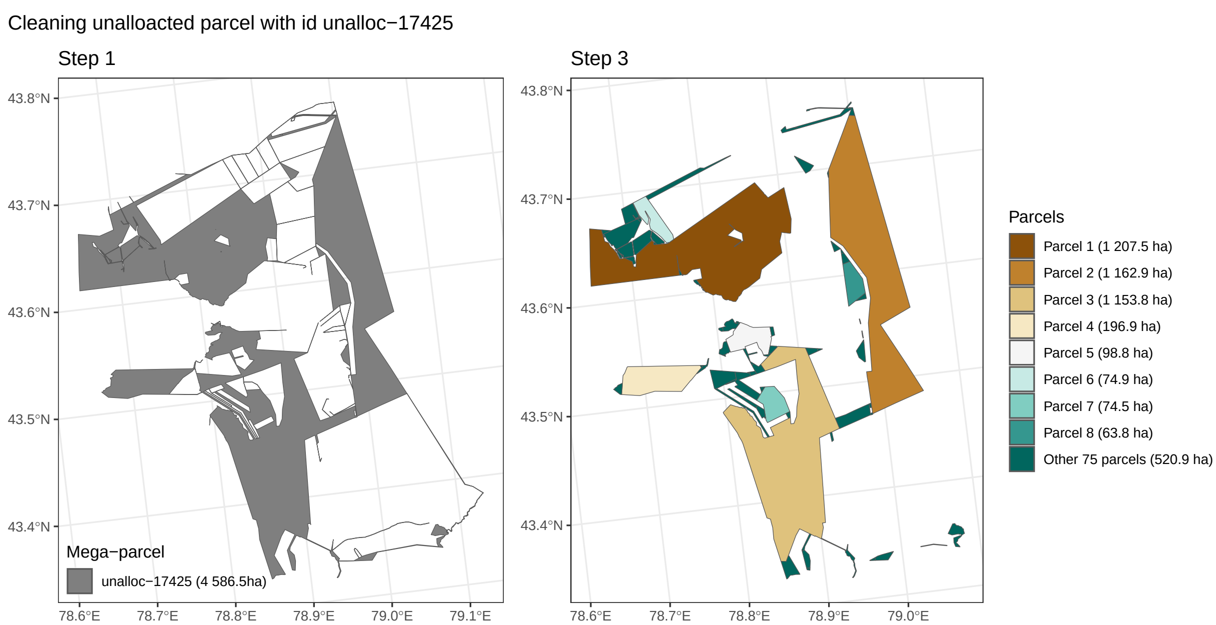

Figure B2: Example of an anallocated parcels cleaning 2

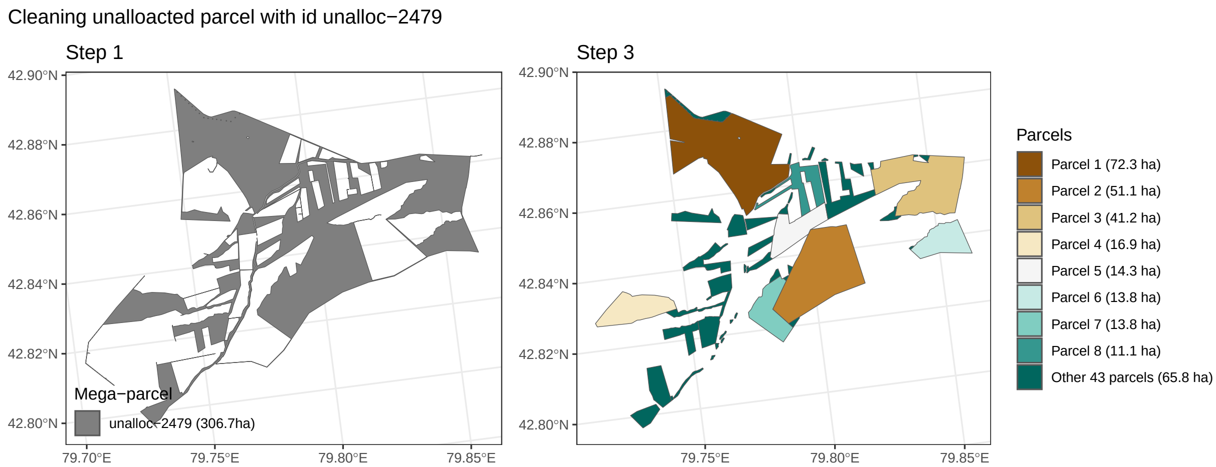

Figure B3: Example of an anallocated parcels cleaning 3

Step 3. Combine allocated and never-allocated parcels

Step 4. Spatial intersection between allocated and never-allocated parcels and land cover data creates plots. Here, the land cover map does not cover all the territory of the study region. Therefore, some parcels are filtered out as a result of the intersection.

Step 5. Keep only those plots on pastures (including pastures on slopes).

Step 6. Keep only those plots, which are greater than 100 square meters in area after buffering by negative 25 meters.

Table B2: Area and numbers of plots and parcels at different steps of preparing units of analysis

Step	N parcels	N plots	Area, 1000 ha
Step 1. All allocated parcels	109 668		1 734.27
Step 2. All never-allocated parcels	8 909		571.52
Step 3. Combined parcels data set	118 577		2 305.79
Step 4. Spatial intersection	117 341	196 209	2 253.40
Step 5. Plots on pastures	25 984	53 289	1 558.95
- incl. allocated	19 930	38 363	1 177.05
- incl. never-allocated	6 054	14 926	381.90
Step 6. Dropping small plots	13 922	23 570	1 548.98
- incl. allocated	9 162	15 837	1 168.71
- incl. never-allocated	4 760	7 733	380.26

Source: own calculations.

B.2.5 Remotely sensed data for our units of analysis

To process remotely sensed raster images and compute polygon-level statistics, we used GEE: Google Earth Engine (Gorelick et al. 2017). Instead of writing code directly in the GEE's code editor, we relied on the R Package `rgee` (Aybar 2022).

Time invariant characteristics

Plot-specific elevation, slope, and aspect are computed based on NASA SRTM Digital Elevation disseminated at 30m resolution (Farr et al. 2007) available here: https://developers.google.com/earth-engine/datasets/catalog/USGS_SRTMGL1_003. We use GEE's built-in functions for computing pixel-level slope, aspect, and subsequently exposure to north and south and then aggregated polygon-averages of the extracted measures.

Annual characteristics

As an annual measure of pastures vegetation quality is the Normalized Difference Vegetation Index (NDVI). It is measured for each pixel of remotely sensed raster images as a difference between intensity of light in the read spectrum (RED) with that in the near-infra-red spectrum (NIR): $\text{NDVI}_{IT} = \frac{\text{NIR}-\text{RED}}{\text{NIR}+\text{RED}} \in [-100, 100]$. It ranges from -100 to +100, with -100 equivalent to the dense snow cover and +100 dense green fores. We used the "MOD13Q1": MODIS Terra Vegetation Indices 16-Day L3 Global 250m SIN Grid data set (Didan 2021) accessed through and processed with GEE (Gorelick et al. 2017). The data could be access here: https://developers.google.com/earth-engine/datasets/catalog/MODIS_061_MOD13Q1.

250 meters resolution is appropriate for our analysis as the majority of our plots are much larger than one pixel. Nevertheless, to avoid missing observations for very small parcels, we re-scale raster images to 30 meters resolution to aggregate polygon-level statistics. Also the use of higher resolution imagery would have been difficult given the requirement for a consistent annual data set over a long period. Figure B4 and Figure B5 provide examples of how the remotely sensed peak NDVI measures appeared in study region and within boundaries of a single plot.

To extract a single peak annual NDVI value per polygon per year we followed a simple algorithm: (1) computed pixel-specific annual maximum of NDVI; and (2) took a mean of the pixel-specific maximums at the polygon level.

Monthly characteristics

As climate variations in different periods of the year cause different outcomes on the peak vegetation, we decided to control the climate monthly. Specifically, rainfall, surface temperature, and solar radiation data are gathered for each month in a year for each plot. The resolution of the climate data is rather coarse and varies from 1 to 27 km in the study region. Despite that coarse resolution, it is sufficient to control for some general patterns. As we have shown in the theoretical chapter, our research design does not require any controls at all and results do not change once we exclude climatic characteristics.

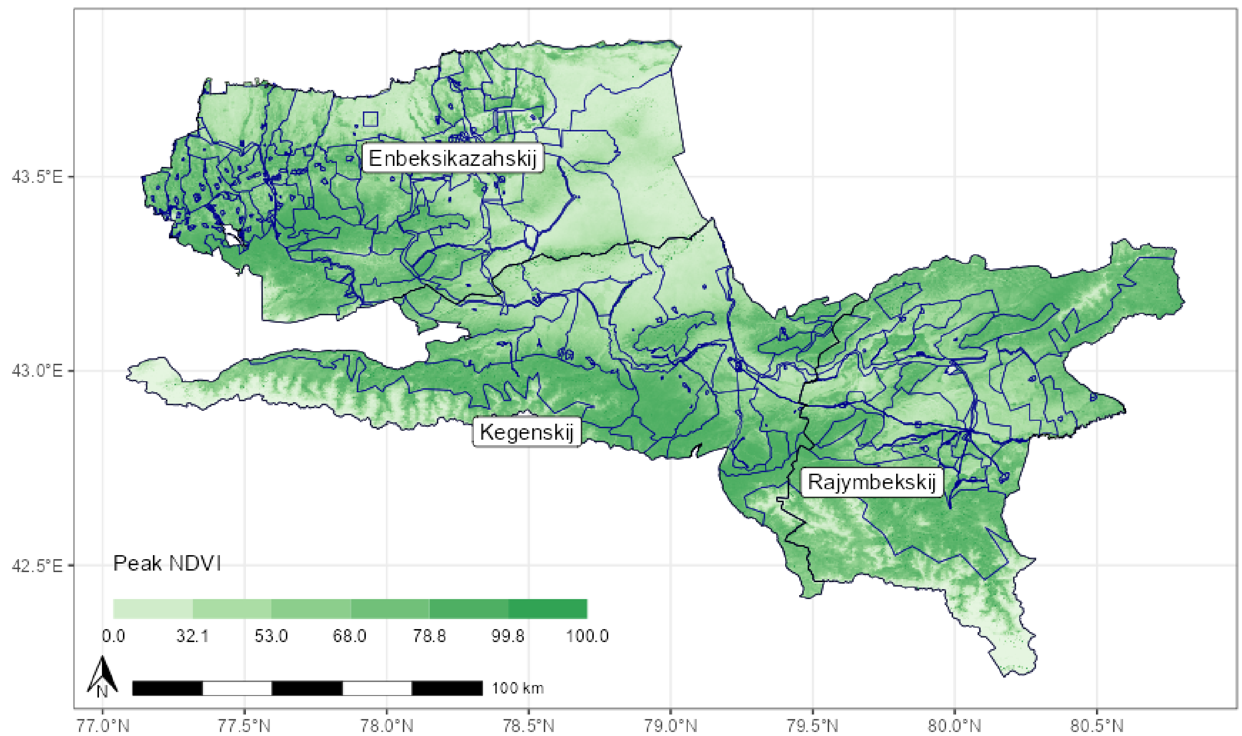

Figure B4: Peak NDVI distribution in the study region in 2020

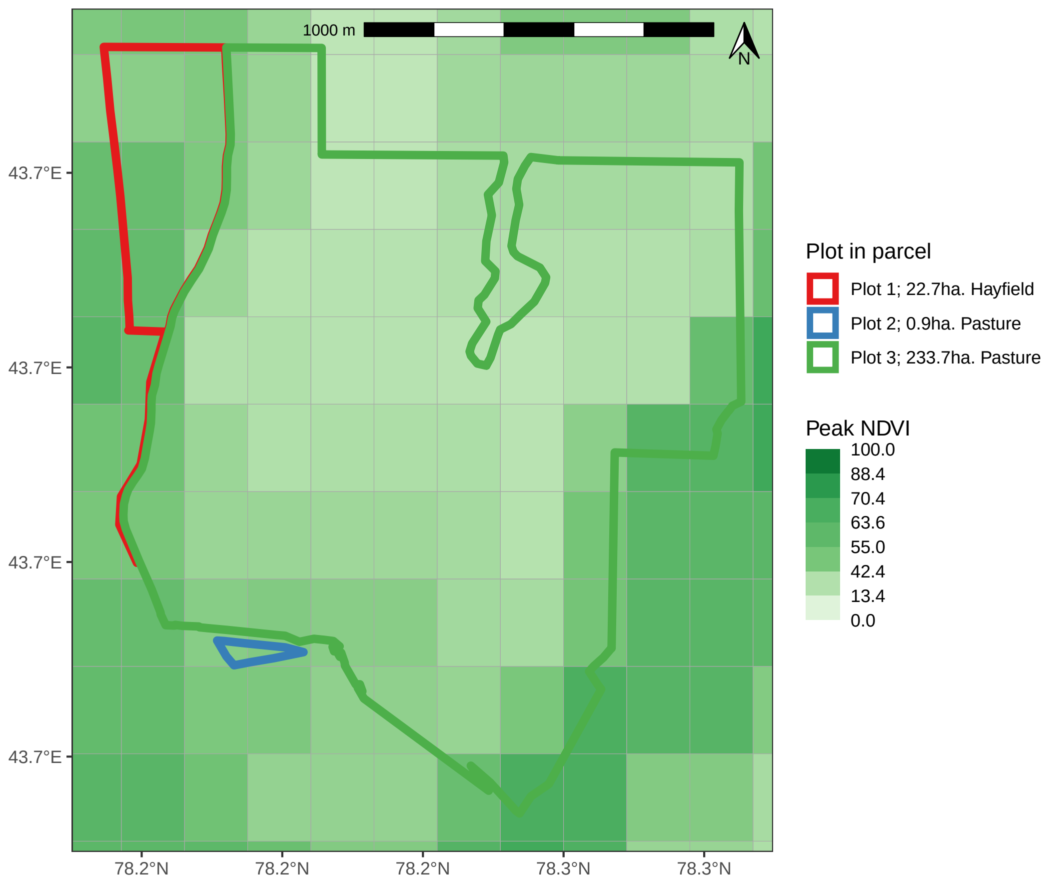

Figure B5: Peak NDVI distribution example on a selected plot in 2020

Rainfall is approximated by cumulative monthly rainfall in millimeters obtained using Climate Hazards Group InfraRed Precipitation With Station Data (Version 2.0 Final) (Funk et al. 2015) disseminated at the 10km spatial resolution. We aggregate daily data over a month for each year estimating cumulative monthly rainfall per pixel and then average multiple pixels data per polygon. Data can be accessed here: https://developers.google.com/earth-engine/datasets/catalog/UCSB-CHG_CHIRPS_DAILY.

Solar radiation is measured using short wave radiation flux in watt per sq. meter (band "SWdown_f_tavg") in GLDAS-2.1: Global Land Data Assimilation System data set (Rodell et al. 2004) disseminated at the 27km spatial resolution. Data can be accessed here: https://developers.google.com/earth-engine/datasets/catalog/NASA_GLDAS_V021_NOAH_G025_T3H. Similarly to the rainfall data, we calculate cumulative monthly radiation per pixel and then average pixel data per polygon.

Surface temperature is computed based on the Average Daytime Land Surface Temperature band ("LST_Day") from "MOD21C3.061": Terra Land Surface Temperature and 3-Band Emissivity Monthly L3 Global 0.05 Deg CMG data set (Hulley and Hook 2021) disseminated at the 1000 m spatial resolution. Data can be accessed here: https://developers.google.com/earth-engine/datasets/catalog/MODIS_061_MOD21C3. To derive monthly polygon-level surface temperature, we first compute the monthly pixel-level average and then aggregate an average of pixels over the polygon.

B.2.6 Methodology of calculating spillover variables

To measure the effect of spillover from land allocation in the neighborhood on the allocated and unallocated plots, we first constructed a distance matrix between the centroid of each plot and the nearest border of its neighbors in the 30 km radius on pastures. As livestock grazing requires the livestock to travel on a flat surface, there is a sensible distance limit to which spillovers can take place. We implement a spatial configuration with the j rings being 5, 10, 15, and 30 km and assume no spillovers beyond the 30 km point as it is unfeasible to travel such distance with cattle for one day. Using allocation date, we calculated the area of plots allocated and unallocated in each of these bins at each year of analysis. Then, a simple ratio resulted in time-varying variables indicating the share of land on pastures within different distance bins relative to each plot that is allocated in each given year. Figure B6 and Figure B7 provide examples of spillover variables calculation for respectively one allocated and never allocated plots.

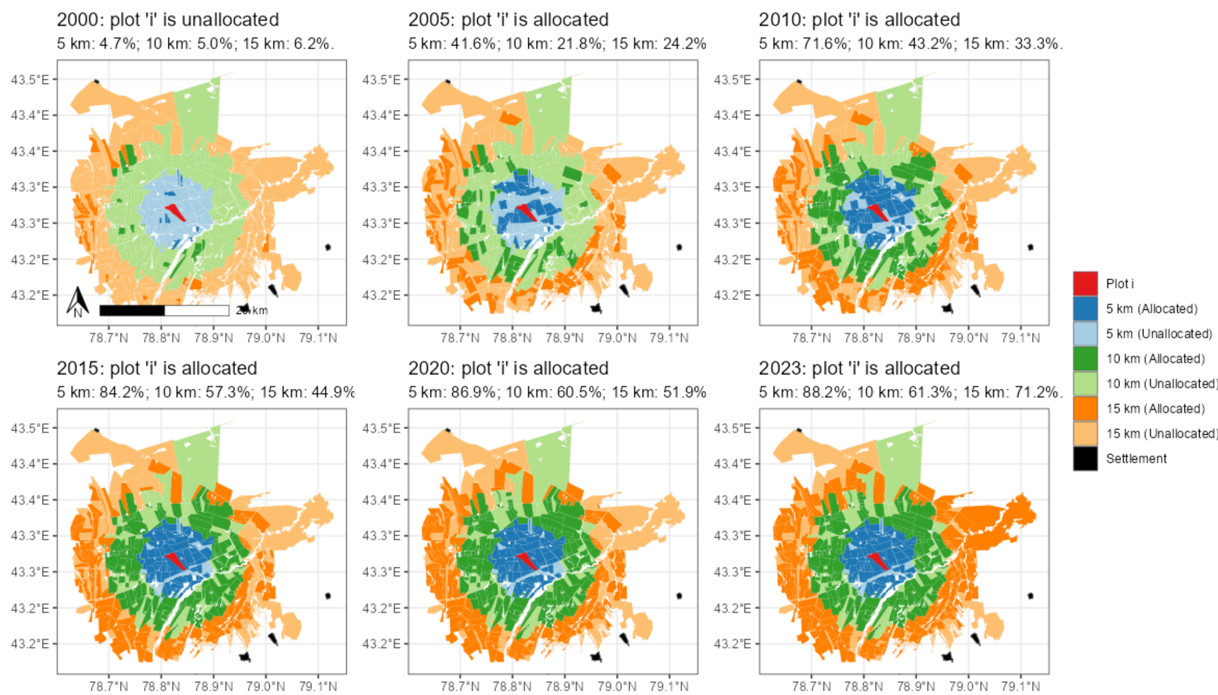

Figure B6: Example of calculating spillover effects by distance rings

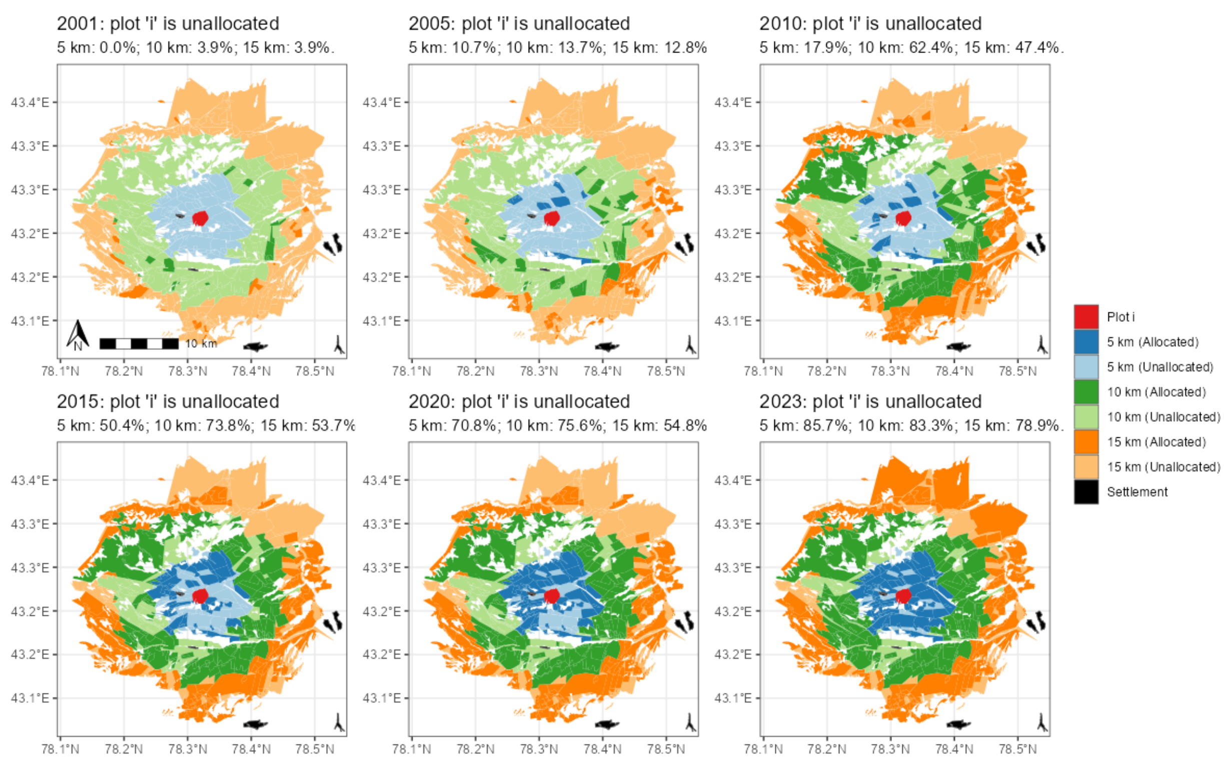

Figure B7: Example of calculating spillover effects by distance rings for never-allocated plot

C.3 Empirical approach to causal analysis

To test hypotheses about the causal effect of land allocation on pastures quality, we need to estimate the Average Treatment Effect on Treated (ATT). The outcome is annual maximum of the Normalized Difference Vegetation Index ($Y_{i,t}$) observed at the plot level i in time t. The treatment here $D_{i,t} \in \{0,1\}$ is a binary variable that indicates whether or not i is allocated in the cadastre at t. Following the potential outcome framework (Rubin 1974; Imbens and Rubin 2015), the observed vegetation quality is $Y_{i,t} = D_{i,t}Y_{i,t}(1) + (1 - D_{i,t})Y_{i,t}(0)$, where $Y_{i,t}(1)$ and $Y_{i,t}(0)$ are respectively potential outcomes of allocation and not allocation. In a two-period example ($t \in \{1,2\}$ with $i = (1,2,...,N)$ and $D_{i,t}$ reduced to D_i) a canonical Difference-in-Difference (DiD) setting with cohort-level treatment and synthetic controls (e.g. Card and Krueger 1994; Abadie 2021) identifies unit-specific treatment effect as $\tau_i = Y_{i,2}(1) - Y_{i,2}(0)$. The ATT is an average of unit-specific treatment effects Equation 4.

$$\tau \equiv E\left[Y_{i,2}(1) - Y_{i,2}(0) | D_i = 1\right] \tag{4}$$

Causal interpretation of τ stands on key assumptions of the canonical DiD: **parallel trends** in the development of treated and not treated units; **no anticipation**, when the treatment has no causal effect on the outcome before the treatment application; and **independent sampling**. In a multi-period setting $t \in (1,...,k,\infty)$, there are always treated $D_{i,0} = D_{i,1,...k} = 1$, once treated $D_{i,0} = 0$ (but $D_{i,1...k} = 1$), or never treated individuals $D_{i,0} = D_{i,1...k} = 0$ and $D_{i,\infty} = 1$. Besides, once treated, individuals remain treated until the end of the observation period. Such heterogeneous in timing and lifetime treatment is called a **staggered research design** with **absorbing** treatment.

C.3.1 Two ways fixed effect model in a staggered design

To estimate the ATT in staggered design, authors often use canonical DiD or its intuitive implementation as Two Ways Fixed Effect (TWFE) model (Equation 5) [19]. Resulting τ^{TWFE} is interpreted as a ATT of a treatment applied any time against the not-treated counterfactual: $\tau^{\text{TWFE}} = Y_{i,t}(g) - Y_{i,t}(\infty)$ for $g > t$, where g is the time of the first treatment (de Chaisemartin and DHaultfoeuille 2022; Baker, Larcker, and Wang 2022). $\hat{\tau}^{\text{TWFE}}$ is identified based on the assumptions of the canonical DiD, which are extended with the assumptions specific to the staggered design. Such are: **treatment effect homogeneity** over time and units (de Chaisemartin and DHaultfoeuille 2020; Athey and Imbens 2022); the parallel trends in **never treated** and **treated after treatment** groups, and the **staggered no anticipation** (Roth et al. 2023).

$$Y_{i,t} = \eta_{\cdot,t} + \eta_{i,\cdot} + \tau^{\text{TWFE}} D_{i,t} + \beta X_{i,t} + \epsilon_{i,t} \tag{5}$$

Lately, literature started to question the appropriateness of the TWFE models for estimating the ATT overall (Kropko and Kubinec 2020) and particularly in a staggered design (Roth et al. 2023;

[19] In Equation 5, $\eta_{\cdot,t}$ and $\eta_{i,\cdot}$ are linear and additive fixed effects of time and individuals, $\epsilon_{i,t}$ is the error terms and $X_{i,t}$ is the matrix of time-varying control variables.

Baker, Larcker, and Wang 2022). The reason for that lies in the restrictive nature of the treatment effect homogeneity assumption, which may not be always satisfied in a real application. Authors argue that in nearly all policy-related analyses, where the staggered design is used, it is impossible to assume homogeneity of the treatment effects, thus concluding that a substantial share of recent empirical literature might be in question.

C.3.2 Heterogeneous treatment effects

The TWFE it fails to estimate true ATT, when **treatment effect are heterogeneous** (over time and/or over individuals). (de Chaisemartin and DHaultfoeuille 2020; Athey and Imbens 2022; Sun and Abraham 2021; Callaway and Sant'Anna 2021b) show that theoretically, while (Borusyak, Jaravel, and Spiess 2023; Goodman-Bacon 2021; de Chaisemartin and DHaultfoeuille 2022) develop extensive empirical examples. To explain how, TWFE model fails under treatment effect heterogeneity, (de Chaisemartin and DHaultfoeuille 2020; Goodman-Bacon 2021) propose two complementary DiD decomposition approaches[20] . They show how τ^{TWFE} can be decomposed as a weighted (by ω_l) average of individual two-periods-two-groups (2x2) Difference-in-Difference estimators $\tau_l^{2\times 2}$ over l cohorts of the treated and untreated individuals. Although, both decomposition approaches are slightly different[21], they conclude that $\tau^{\text{TWFE}} = \sum_l \omega_l \tau_l^{2\times 2}$ and $\sum_l \omega_l = 1$.

Both methodologies show that some 2x2 DiD are "**clean comparisons**": occurring between **once-treated** and **never- or not-yet-treated** counterfactual; while others are "**forbidden comparisons**": where **once/later treated** are compared with **earlier / always treated** counterfactuals. "Forbidden comparisons" substantially distort the weights ω_l, leading to "negative weights" in (de Chaisemartin and DHaultfoeuille 2020) decomposition. Combination of the "forbidden comparisons" and "negative weights" makes τ^{TWFE} invalid when treatment effects are heterogeneous between individuals, over time relative to treatment (r), or both: $\tau_{\cdot,t} \neq \tau_{r,\cdot} \neq \tau_{r,t} \neq \tau$. (de Chaisemartin and DHaultfoeuille 2020; Goodman-Bacon 2021) suggest diagnostic measures such as communicating weights and prevalence of negative weights, as well as assessing the number of "forbidden" 2x2 comparisons to understand the extent of a possible bias. However, solutions to the TWFE model's flaws are not straightforward and depends on the type of heterogeneity (see (Roth et al. 2023) for a detailed discussion).

[20](Borusyak, Jaravel, and Spiess 2023) also proposes similar decomposition approach reaching same conclusions as (de Chaisemartin and DHaultfoeuille 2020; Goodman-Bacon 2021). (Athey and Imbens 2022) however follow a research design-based decomposition with the core assumption about a random assignment of the staggered treatment, coming to a similar conclusion.

[21](Goodman-Bacon 2021) as a treatment (control) group considers a combination (cohort) of all individuals with a certain starting date of treatment to the end of observations as the treatment status does not change once treated. (de Chaisemartin and DHaultfoeuille 2020) analyzes each treatment (control) period as a separate group yielding a larger number of 2x2 DiD comparisons. Besides, differences between (de Chaisemartin and DHaultfoeuille 2020) and (Goodman-Bacon 2021) lead to different weighting structures. Although both conclude that specific values of ω_l depend on the number of periods that treated groups are observed in the data in each 2x2 comparison l, (de Chaisemartin and DHaultfoeuille 2020) approach shows that some weights can take negative values, while (Goodman-Bacon 2021) usually finds positive weights that follow a convex function. With the homogeneous treatment effects assumption, negative and positive weights cancel out each other yielding true estimates of ATT.

The treatment effects **heterogeneity over time** relative to the treatment r (but homogeneity across groups l) $\tau_{l,r}^{2\times 2} = \tau_r$ for all $r = -\infty; +\infty$ violates the parallel trends assumption. To relax it, we can apply the **dynamic TWFE** model (Equation 1) aliased with an **event-study** (Roth et al. 2023; Callaway and Sant'Anna 2021b). The event-study adds year-before-after-treatment (r) indicator variables $R_{i,t}$ and estimates corresponding coefficients γ_r (Sun and Abraham 2021). In Equation 1, $R_{i,t} = t - G_i + 1$ represents the time before/after initial treatment G_i for unit i. When $t = -1$ variable $R_{i,t}$ is usually omitted to avoid perfect collinearity. The event-study results provide period-relative-to-treatment-specific estimates of ATT ($\gamma_r = \hat{\tau}_r$), while the overall estimate of the ATT ($\hat{\tau}^{\text{EVENT}}$) is an average of γ_r for $t \in \{0, T\}$ weighted by the relative frequency ω_r of each period r observation in the data ($\hat{\tau}^{\text{EVENT}} = \sum_{r=0}^{T} \gamma_r \omega_r / \sum_{r=0}^{T} \omega_r$) with the standard errors obtained using the delta method.

Results of an event-study provide useful insight on how the treatment effect changes over time and are often used to validate the parallel trends assumption. Although, the **parallel trends** is an identifying assumption for estimating the ATT, in nearly all non-experimental settings, it might hold only weakly (as any observational process is affected by time-varying confounders) and it might be sensitive to the functional form. Under treatment effect heterogeneity in the staggered absorbing treatment design, the pre-treatment dummy variable may also appear significant falsely rejecting the parallel trend assumption (Sun and Abraham 2021). Doubly-robust estimation methods can be used then to condition the parallel trends on covariates (Callaway and Sant'Anna 2021b; Sant'Anna and Zhao 2020). Ultimately, to test for any existing pre-treatment trend, it is still recommended to use the event-study type dynamic model (Equation 1) while estimating it using one of the treatment effect heteroscedasticity robust methods discussed below (Roth et al. 2023).

Γnder the treatment effect **heterogeneity across individuals,** the Equation 1 still estimates insensible ATT (Sun and Abraham 2021; Borusyak, Jaravel, and Spiess 2023). To further relax this assumption, we need to use heterogeneity-robust estimators that bypass the limitations of the TWFE models. Essentially, these estimators isolate clear comparisons between treated groups and never (not yet) treated counterfactuals and use appropriate weights that correspond to the underlying purpose of the analysis (de Chaisemartin and DHaultfoeuille 2020).

C.3.3 Treatment effect heterogeneity-robust estimators

Several treatment effect heterogeneity-robust estimators and diagnostic tools meant to correct the inference have been developed recently (see the following reviews: de Chaisemartin and DHaultfoeuille (2022), Roth et al. (2023), and Baker, Larcker, and Wang (2022)). Below, we provide a general discussion of the key methods used in our empirical analysis and in Chapter 4, we discuss identifying assumptions relevant to our analysis.

The Callaway and Sant'Anna estimator (referred to as **CS** below) (Callaway and Sant'Anna 2021b) implemented in R in the package `did` (Callaway and Sant'Anna 2021a) allows explicitly set the control groups to never-treated or not-yet-treated and produces non-negative weights for aggregating ATT. It estimates all the pairwise comparisons, where ATT is the weighted average of all 2x2 DiD and the event-study is an aggregated of certain 2x2 DiD by r. The CS assumes parallel tend in the post-treatment period, however permits to relax it, estimating the conditional parallel trends using doubly-robust or inverse probability weighting (IPW) methods.

The Sun and Abraham estimator (referred to as **SA** below) (Sun and Abraham 2021) implemented in R in packages `fixest` (Bergé 2018) follows a similar aggregating strategy as the CS estimator, however, uses never-treated or the last treated group as a counter-factual. Its results are similar to CS when the same counterfactual is used, however, under restricted parallel trends assumption it is more efficient. It is also more flexible in aggregating pre- and post-treatment periods.

The family of imputation estimators (referred to as **IMP** below) are represented by multiple alternatives (Borusyak, Jaravel, and Spiess 2023; Gardner 2022; Wooldridge 2023). They estimate regression coefficients and fixed effects of the TWFE model using the untreated sub-sample and then predict potential outcomes of no-treatment for the treated observations. The difference between predicted potential outcomes of no treatment and factual is the resulting individual-level estimates of a treatment effect, which is then aggregated into the ATT or a event-study. The IMP is valid under no anticipation and more restrictive parallel trends assumptions in all groups and all time periods, however, it also derives consistent estimates under group-specific linear trends. Given the size and complexity of our data, we use the (Gardner 2022) estimator implemented in the R package `did2s` (Butts and Gardner 2021) as the other alternatives (Borusyak, Jaravel, and Spiess 2023; Wooldridge 2023) implemented in `didimputation` and `etwfe` (Butts 2021a; McDermott 2023) respectively turned out computationally impossible, when challenged by a large sample with more than 500 thousand observations.

The CS, SA, and IMP estimators are different in what groups are used as counterfactuals, how the covariates are incorporated and how restrictive are the parallel trend assumptions. This creates trade-offs between efficiency and strength of identifying assumptions (Roth et al. 2023). The CS/SA uses the last pre-treatment period, while the IMP uses the average of pre-treatment periods. Therefore, the IMP approach may be more efficient under the strong parallel trends assumption with modest heteroscedasticity and not serial correlation (Wooldridge 2023; de Chaisemartin and DHaultfoeuille 2022). In the setting when the parallel trends assumption might not hold over long periods of time or there is a risk of a serial correlation, the CS/SA are preferred (Marcus and Sant'Anna 2021). Both approaches are rather vulnerable to the no-anticipation assumption violation, although, using the treatment announcement date instead of a treatment might be helpful (de Chaisemartin and DHaultfoeuille 2022).

There are other estimators as well, which we omit due to their lesser relevance to our observational study design. (Athey and Imbens 2022; Roth and Sant'Anna 2021; Bojinov, Rambachan, and Shephard 2021) propose estimators, where treatment timing randomization is assumed. The estimator proposed in (de Chaisemartin and DHaultfoeuille 2020) permits design with non-absorbing treatment that could turn on and off, but in the staggered design with absorbing treatment it converges to the CS estimator. (Roth et al. 2023; de Chaisemartin and DHaultfoeuille 2022) provide a more elaborate discussion of alternative estimators.

D.4 Auxiliary descriptive statistics

This section presents auxiliary descriptive statistics such as plots area and number by land cover (**tbl_lu_by_lc?**), allocation dynamics by individualized tenure and allocation date certainty (**tbl_tenure_by_certainty?**), plots characteristics by tenure and rayon Table D3 as well as derived statistics at parcel level Table D4.

Plots distance to settlements is homogeneous for all tenure categories as well as never allocated land, except for common grazing land located near to villages. Figures D4, D3, and D2 presenting the distribution of distances to villages of among different plot categories suggest that systematic land use differences may exist between common grazing land because of its distance to the villages[^18]. This calls for matching sub-samples of never-allocated land for comparing with the "common grazing" tenures. Therefore, we divide never allocated land between the one near villages (within a 5 km radius from settlement) and remote (beyond 5 km). The 5 km threshold is used based on the observation that in the cadastre, plots assigned for common grazing under sub-category "remote" are located beyond 5 km from the nearest settlement. Overall, this division creates two groups of never-allocated land that are located at a similar distance from settlements and altitudes as comparable common pastures.

There are systematic differences in NDVI between allocated and unallocated land by year Table D5.

Table D1: Plost number by land cover and tenure

Tenure	Hayfield	Pasture	Pasture on slopes
Ind. farm (ownership)	5.7[375]	51.7[1608]	13.6[211]
Ind. farm (rent)	29.4[1947]	327.9[7147]	106.2[1965]
Ag. enterprise (ownership)	1.0[46]	14.2[249]	3.3[43]
Ag. enterprise (rent)	8.7[199]	110.7[1090]	64.4[573]
Common grazing (near villages)	1.4[42]	28.7[221]	3.6[16]
Common grazing (remote)		40.9[111]	25.3[78]
Forest	0.7[36]	121.9[605]	115.7[471]
Protected areas	0.5[17]	69.8[434]	53.9[467]
Household	0.0[2]	2.3[103]	0.7[19]
Other	0.5[33]	11.7[349]	2.2[77]
Never allocated (near villages)	6.7[472]	111.8[3442]	14.8[409]
Never allocated (remote)	4.9[280]	141.5[2538]	112.2[1344]

Note: columns report plot area in 1000 ha, and plots number in square brackets.

Source: own calculations.

Table D2: Plost number by tenure and allocation certainty

Year	Ind. farm (ownership)	Ind. farm (rent)		Ag. enterprise (ownership)	Ag. enterprise (rent)	
	Approximate	Approximate	Exact	Approximate	Approximate	Exact
before 2000	0.1[7]	0.2[3]	36.0[1152]			1.0[12]
2000	0.0[2]		3.2[105]			7.7[29]
2001	0.0[1]		19.0[431]			0.3[10]
2002	0.1[2]	0.2[4]	18.7[415]			3.2[37]
2003	0.1[3]	0.1[5]	10.6[215]			1.5[17]
2004	1.5[61]	1.0[13]	29.9[571]	0.1[6]	0.0[1]	6.2[23]
2005	1.9[76]	0.6[17]	36.3[739]	0.4[17]		4.2[48]
2006	1.9[94]	2.3[47]	31.8[632]	0.1[6]	0.0[2]	6.7[69]
2007	5.1[173]	2.7[68]	36.0[718]	1.5[44]	3.1[14]	10.7[157]
2008	10.4[260]	2.1[50]	8.2[136]	5.3[29]	0.9[9]	5.5[84]
2009	2.2[86]	2.2[46]	7.5[142]	0.3[9]	0.9[19]	4.4[41]
2010	2.2[76]	1.6[42]	19.6[382]	0.2[11]	0.1[2]	5.8[80]
2011	3.1[76]	3.6[86]	15.4[315]	1.0[19]	1.0[26]	0.2[9]
2012	3.9[138]	6.7[110]	14.8[238]	3.2[51]	3.8[38]	11.3[123]
2013	2.1[83]	4.0[78]	11.2[183]	0.5[12]	5.8[20]	2.2[34]
2014	5.0[122]	4.0[82]	12.4[245]	0.4[6]	0.8[2]	2.1[22]
2015	2.7[96]	4.7[81]	17.3[310]	1.2[20]	0.5[12]	2.6[46]
2016	3.8[157]	4.9[106]	10.8[199]	0.2[4]	2.2[38]	5.0[46]
2017	1.4[62]	3.0[72]	2.7[70]	0.9[18]	0.1[2]	37.4[196]
2018	0.9[38]	1.6[54]	8.3[99]	0.6[8]		2.2[51]
2019	1.4[56]	1.9[59]	4.9[115]	1.2[11]	1.1[19]	2.0[54]
2020	0.9[39]	1.7[30]	6.0[123]	0.2[6]	0.0[4]	6.0[43]
2021	8.8[59]	3.3[64]	7.2[175]	0.1[7]	1.9[16]	5.4[51]
2022	5.8[50]	2.6[54]	11.7[231]	0.2[8]	0.8[7]	18.6[150]

Note: columns report plot area in 1000 ha, and plots number in square brackets.

Source: own calculations.

Table D3: Plots characteristics by tenure and rayon

Tenure	N plots [parcels]	Area, 1000 ha	Size, ha	Elevation, km	Slope, degree	Distance to settlements, km	Peak NDVI	Av. N plots [plot size] in parcel
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Full sample	23 570	1549.0	65.7	1.84	12.4	12.3	57.9	1.7
	[13 922]		(326.8)	(0.83)	(9.0)	(9.7)	(21.5)	[44.7]
Once allocated	15 837	1168.7	73.8	1.86	12.7	12.5	59.2	1.7
	[9 162]		(366.3)	(0.84)	(9.1)	(9.5)	(21.3)	[51.5]
Never allocated	7 733	380.3	49.2	1.78	11.2	11.6	53.6	1.6
	[4 760]		(224.0)	(0.80)	(8.8)	(10.1)	(21.5)	[31.7]
Ind. farm	10 931	499.3	45.7	1.61	8.9	8.2	53.9	1.4
	[7 605]		(92.6)	(0.62)	(6.7)	(5.6)	(19.1)	[46.0]
Ind. farm (own)	1 819	65.3	35.9	1.06	7.4	9.4	49.3	1.3
	[1 437]		(140.4)	(0.49)	(6.4)	(6.5)	(19.8)	[27.6]
Ind. farm (rent)	9 112	434.1	47.6	1.69	9.1	8.1	54.7	1.5
	[6 168]		(79.6)	(0.59)	(6.8)	(5.4)	(18.9)	[50.3]
Ag. enterprise	1 955	192.6	98.5	2.05	12.8	17.4	59.6	2.4
	[832]		(330.3)	(1.00)	(9.1)	(11.6)	(20.4)	[79.0]
Ag. enterprise (own)	292	17.5	59.9	1.85	10.8	15.3	69.0	1.7
	[170]		(205.9)	(0.89)	(9.8)	(11.4)	(14.6)	[41.6]
Ag. enterprise (rent)	1 663	175.1	105.3	2.07	13.0	17.6	58.6	2.5
	[662]		(347.2)	(1.01)	(9.1)	(11.6)	(20.7)	[88.6]
Common (near)	237	32.4	136.5	1.04	10.2	2.5	59.7	1.7
	[143]		(319.0)	(0.39)	(7.3)	(1.3)	(19.4)	[151.6]
Common (remote)	189	66.2	350.1	1.64	10.8	16.4	45.3	4.7
	[40]		(955.0)	(0.68)	(8.3)	(6.7)	(24.6)	[416.2]
Forest	1 076	237.6	220.9	2.22	18.4	15.2	68.8	8.2
	[132]		(1087.5)	(0.85)	(8.9)	(9.3)	(21.3)	[80.2]
Protected areas	901	123.7	137.3	2.34	19.7	18.0	69.5	45.0
	[20]		(549.3)	(0.83)	(8.9)	(11.5)	(18.2)	[190.2]
Household	122	3.0	24.7	1.13	9.2	8.4	43.5	1.2
	[101]		(83.4)	(0.50)	(6.8)	(3.1)	(15.2)	[12.9]
Other	426	13.9	32.7	1.23	6.9	9.8	58.7	1.4
	[295]		(247.6)	(0.99)	(8.4)	(7.9)	(16.9)	[15.9]

Note: Column 'Tenure' stratifies samples into the 'Full sample', subsamples of 'Once allocated' and 'Never allocated' land, and subsamples by detailed tenure categories. The first column reports a number of plots and parcels (in square brackets) under each category. In columns 3 through 7 report means and standard deviations (in parentheses) weighted by plot size. Column 8 reports the number of plots and average plot size (in square brackets) within the parcel under each category.

Source: own calculations.

Table D4: Parcel characteristics by tenure for all districts

Tenure	N plots / parcels	Total area, 1000 ha	Size, ha	Elevation, km	Slope, degree	Distance to settlements, km	North exposure	East exposure	Peak NDVI	Rainfall, mm	Temperature, C	Solar radiation, mW m-2	Average N plots in parcel	Average plot size in parcel, HA
Rayon: All rayons														
Full sample	13 922	2062.1	148.1	1.98	13.3	13.3	0.016	-0.002	59.9	254.2	27.6	230.9	1.693	44.7
			(1417.9)	(0.87)	(8.8)	(9.8)	(0.059)	(0.035)	(20.1)	(106.3)	(7.8)	(7.1)		
Once allocated	9 162	1520.9	166.0	1.99	13.7	13.6	0.016	-0.002	61.5	255.2	27.4	230.9	1.729	51.5
			(1634.0)	(0.87)	(8.8)	(9.5)	(0.057)	(0.033)	(19.9)	(101.2)	(7.5)	(7.4)		
Never allocated	4 760	541.1	113.7	1.97	12.2	12.6	0.018	-0.001	55.5	251.3	28.1	230.9	1.625	31.7
			(859.7)	(0.89)	(8.7)	(10.5)	(0.066)	(0.041)	(19.9)	(119.5)	(8.7)	(6.0)		
Never allocated (near villages)	2 662	225.1	84.5	1.62	8.5	2.6	0.030	-0.004	58.9	214.9	31.3	230.4	1.447	28.5
			(646.9)	(0.57)	(7.3)	(1.4)	(0.084)	(0.057)	(18.6)	(64.8)	(4.3)	(4.8)		
Never allocated (remote)	2 162	386.5	178.8	2.15	14.6	17.0	0.011	-0.001	54.8	264.2	26.4	231.2	1.796	38.8
			(1261.2)	(0.94)	(8.6)	(9.3)	(0.055)	(0.031)	(21.5)	(133.3)	(9.6)	(6.8)		
Ind. farm	7 605	567.4	74.6	1.60	8.7	8.1	0.023	-0.005	54.8	230.5	31.8	231.1	1.437	46.0
			(158.3)	(0.64)	(6.5)	(5.6)	(0.074)	(0.043)	(18.8)	(93.8)	(5.3)	(5.6)		
Ind. farm (ownership)	1 437	75.8	52.7	1.04	7.0	8.9	0.038	-0.003	50.8	166.9	34.5	230.0	1.266	27.6
			(261.5)	(0.50)	(5.8)	(6.4)	(0.087)	(0.049)	(19.4)	(57.7)	(4.4)	(6.6)		
Ind. farm (rent)	6 168	491.6	79.7	1.69	9.0	8.0	0.020	-0.005	55.5	240.3	31.4	231.2	1.477	50.3
			(121.8)	(0.61)	(6.6)	(5.5)	(0.072)	(0.042)	(18.7)	(94.5)	(5.3)	(5.4)		
Ag. enterprise	832	285.3	342.9	2.23	13.9	18.8	0.016	-0.004	60.9	267.4	24.9	229.8	2.350	79.0
			(1750.7)	(1.03)	(9.0)	(12.0)	(0.063)	(0.036)	(18.0)	(110.8)	(8.6)	(7.3)		
Ag. enterprise (ownership)	170	23.8	140.0	1.77	10.9	14.8	0.035	-0.002	67.5	286.6	27.6	232.7	1.718	41.6
			(542.4)	(0.98)	(9.7)	(11.8)	(0.084)	(0.059)	(14.4)	(148.0)	(6.8)	(9.5)		
Ag. enterprise (rent)	662	261.5	395.0	2.27	14.2	19.1	0.014	-0.004	60.3	265.7	24.6	229.5	2.512	88.6
			(1940.3)	(1.03)	(8.9)	(11.9)	(0.060)	(0.033)	(18.2)	(106.6)	(8.7)	(7.1)		
Common grazing (near villages)	143	47.2	330.1	1.03	10.0	2.9	0.017	-0.008	53.9	169.4	33.9	226.5	1.657	151.6
			(712.1)	(0.37)	(6.8)	(1.3)	(0.071)	(0.033)	(20.5)	(38.6)	(2.8)	(6.1)		
Common grazing (remote)	40	71.9	1798.5	1.66	11.3	16.4	0.027	0.009	45.7	199.4	31.4	226.8	4.725	416.2
			(3321.7)	(0.68)	(8.0)	(6.6)	(0.039)	(0.024)	(24.0)	(60.7)	(7.4)	(6.4)		
Forest	132	311.0	2355.8	2.29	19.1	14.6	0.006	0.001	70.9	282.0	24.5	234.4	8.152	80.2
			(8178.2)	(0.76)	(7.6)	(7.7)	(0.019)	(0.017)	(18.9)	(112.3)	(6.0)	(7.8)		
Protected areas	20	226.4	11321.9	2.54	20.6	20.3	0.002	-0.002	71.3	300.4	21.4	229.1	45.050	190.2
			(21893.2)	(0.70)	(6.3)	(8.1)	(0.011)	(0.005)	(14.0)	(65.9)	(5.2)	(9.1)		
Household	101	3.4	33.5	1.13	9.4	8.3	0.004	0.018	44.2	163.5	36.2	234.2	1.208	12.9
			(145.7)	(0.50)	(6.1)	(3.1)	(0.085)	(0.041)	(15.2)	(63.8)	(3.9)	(4.2)		
Other	295	21.5	72.7	1.50	9.2	11.3	0.049	-0.002	60.9	214.4	27.5	230.3	1.444	15.9
			(485.5)	(1.16)	(9.7)	(9.3)	(0.081)	(0.038)	(14.9)	(102.1)	(8.3)	(8.6)		

Note: Column 'Tenure' stratifies samples into the 'Full sample', subsamples of 'Once allocated' and 'Never allocated' land, and subsamples by detailed tenure categories. The first column reports a number of plots and parcels (in square brackets) under each category. In columns 3 through 7 report means and standard deviations (in parentheses) weighted by plot size. Column 8 reports the number of plots and average plot size (in square brackets) within the parcel under each category.

Source: own calculations.

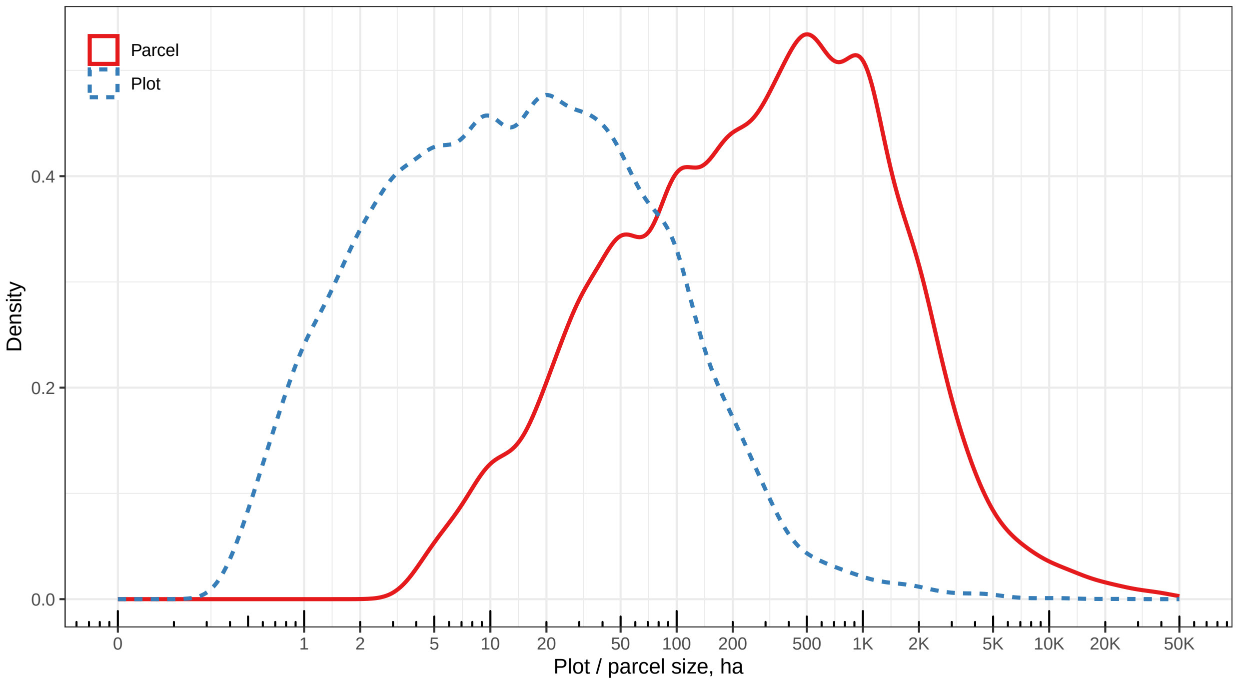

Figure D1: Distribution of plots and parcels sizes

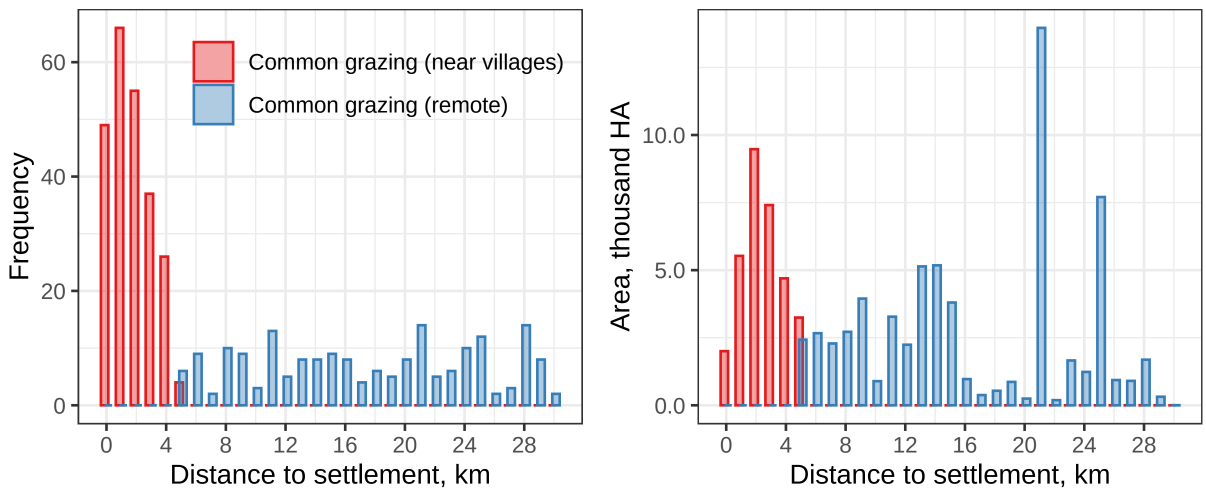

Figure D2: Distribution of the common grazing plots' distances from the settlements

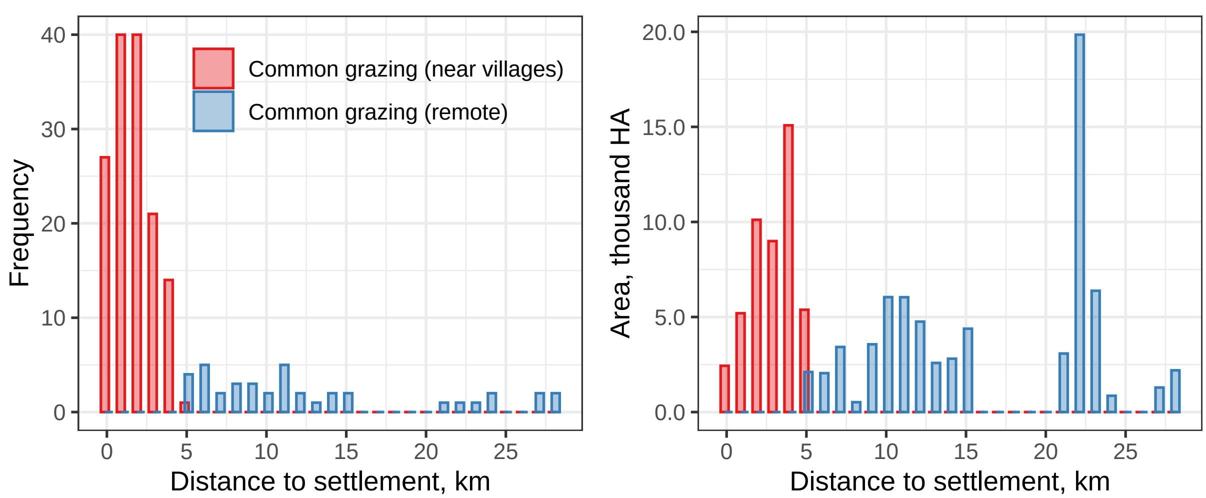

Figure D3: Distribution of the common grazing parcels' distances from the settlements

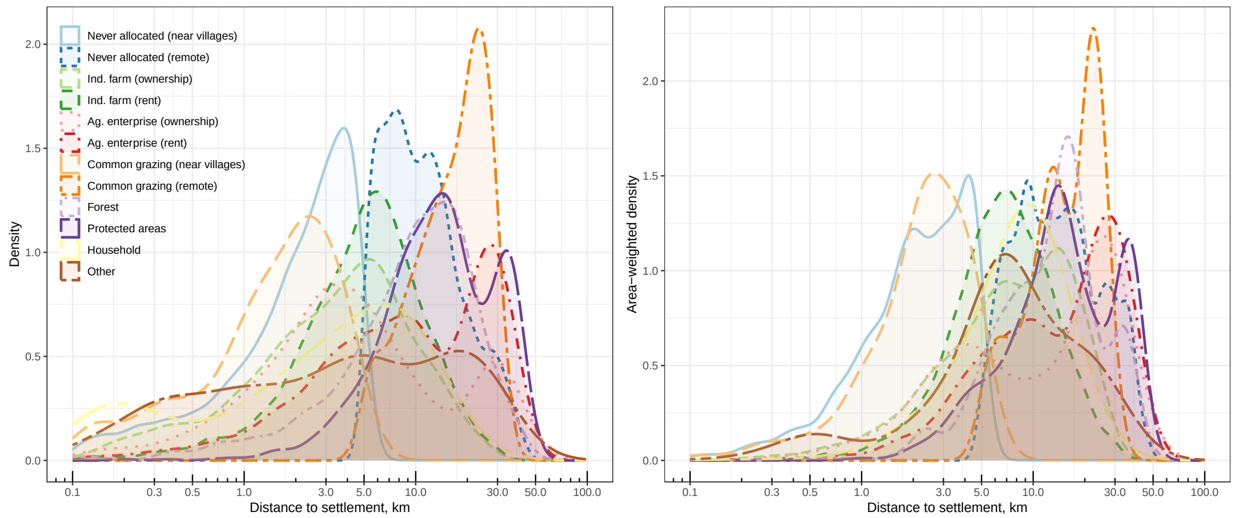

Figure D4: Distribution of the plots with other tenure regimes by distances from the settlements

Table D5: Checks and balances of the pead NDVI by allocation status

	Full sample			Excl. never allocated	
	Allocated	Not allocated	t-test	Not-yet allocated	t-test
All years	62.0 (18.8)	59.7 (19.8)	2.2*** [0.1]	6189.5 (1980.8)	34.3*** [6.5]
2000	59.2 (20.8)	57.7 (20.8)	1.5* [0.6]	5916.4 (2057.6)	5.8 [59.9]
2001	61.5 (18.0)	59.7 (19.0)	1.8*** [0.4]	6101.7 (1876.2)	52.9 [45.7]
2002	68.9 (14.9)	66.8 (16.9)	2.1*** [0.3]	6796.9 (1659.2)	97.1** [34.6]
2003	66.5 (16.8)	64.8 (18.4)	1.7*** [0.4]	6613.9 (1806.4)	38.2 [37.3]
2004	63.9 (17.2)	62.5 (18.6)	1.3*** [0.3]	6406.9 (1834.6)	-17.5 [34.8]
2005	62.2 (18.1)	61.6 (19.2)	0.7* [0.3]	6339.6 (1891.0)	-117.6*** [33.3]
2006	61.4 (19.2)	59.9 (20.1)	1.4*** [0.3]	6185.7 (1974.7)	-49.5 [33.1]
2007	64.4 (17.9)	61.4 (19.0)	3.0*** [0.3]	6308.6 (1876.9)	132.0*** [29.3]
2008	57.3 (20.3)	54.4 (20.9)	2.9*** [0.3]	5641.6 (2094.3)	87.3** [32.8]
2009	62.7 (18.7)	59.0 (20.2)	3.7*** [0.3]	6074.0 (2013.2)	197.3*** [30.9]
2010	65.0 (17.6)	61.6 (19.0)	3.4*** [0.2]	6325.7 (1908.5)	173.5*** [29.3]
2011	65.0 (17.8)	61.5 (19.4)	3.5*** [0.2]	6298.9 (1950.7)	202.6*** [30.2]
2012	60.8 (19.5)	57.1 (20.8)	3.7*** [0.3]	5872.0 (2128.0)	205.1*** [33.8]
2013	61.2 (18.8)	58.1 (20.1)	3.1*** [0.3]	6022.3 (2046.6)	94.5** [33.2]
2014	54.5 (19.3)	52.1 (20.2)	2.5*** [0.3]	5503.4 (2073.7)	-51.7 [34.7]
2015	63.1 (18.5)	59.5 (20.3)	3.6*** [0.3]	6145.2 (2089.2)	164.7*** [36.0]
2016	67.1 (15.9)	62.9 (17.9)	4.2*** [0.2]	6269.8 (1916.0)	440.9*** [35.6]
2017	65.8 (17.5)	61.2 (19.6)	4.6*** [0.2]	6144.5 (2145.9)	431.3*** [42.5]
2018	63.4 (18.4)	58.8 (20.3)	4.6*** [0.3]	5922.8 (2253.5)	419.4*** [46.5]
2019	63.0 (17.8)	57.8 (19.7)	5.1*** [0.3]	5651.4 (2251.0)	645.2*** [53.2]
2020	61.1 (18.7)	56.1 (20.4)	5.1*** [0.3]	5461.6 (2387.8)	650.6*** [60.4]
2021	57.7 (20.0)	52.4 (20.9)	5.3*** [0.3]	4978.8 (2424.3)	790.7*** [77.9]
2022	62.8 (18.8)	58.6 (19.5)	4.2*** [0.3]		

	Full sample			Excl. never allocated	
	Allocated	Not allocated	t-test	Not-yet allocated	t-test
2023	57.9 (18.9)	53.7 (19.1)	4.2*** [0.3]		

Note: this table reports means and standard deviations (in the parentheses) of peak annual NDVI for different groups of plots by their allocation status, subsample, and year. Row 'All years' computes NDVI over all years (2000-2023). The 'Allocated' column reports statistics only for those plots that were allocated in each year, thus allocated plots may appear as unallocated in the earlier years and allocated in the later years. The 'Full sample' group of columns summarizes unallocated plots that consist of plots unallocated at the time and later allocated as well as never allocated plots synthetically generated on the suitable land. 'Excl. never allocated' group of columns only compares ever allocated plots. Columns 't-test' report the difference between means in 'Allocated' and 'Not allocated' groups, statistical significance level of the difference, and standard errors in square brackets. Whelch's two-sample t-test is used with the assumption of unequal variances between two groups.

Statistical significance levels are: '***' p-value < 0.001, '**' p-value < 0.01, '*' p-value < 0.05, p-value < '.' 0.1, and ' ' p-value >= 0.1.

Source: own calculations.

Controlling for variance in NDVI with other control variables is exceptionally important as comparing simple means of NDVI by allocated and never-allocated common grazing land (Table D6) shows that there is difference between them.

Table D6: NDVI difference between common grazing and never-allocated land by their distance to settlements and year

	Near villages			Remote		
	Common grazing	Never allocated	t-test	Common grazing	Never allocated	t-test
All years	61.8 (17.3)	60.1 (16.8)	1.6*** [0.2]	49.1 (24.0)	55.5 (22.0)	-6.4*** [0.4]
2000	56.6 (17.6)	56.9 (17.8)	-0.3 [1.2]	46.7 (25.3)	53.2 (23.4)	-6.4*** [1.9]
2001	59.2 (16.5)	59.3 (15.9)	-0.2 [1.1]	47.5 (22.9)	55.3 (21.8)	-7.8*** [1.7]
2002	66.4 (15.4)	67.4 (13.8)	-0.9 [1.0]	53.3 (23.8)	62.2 (20.0)	-9.0*** [1.8]
2003	65.1 (17.0)	65.5 (15.3)	-0.4 [1.1]	50.8 (23.5)	59.6 (21.1)	-8.8*** [1.7]
2004	63.6 (17.1)	62.7 (15.7)	0.8 [1.1]	49.0 (23.2)	57.4 (21.0)	-8.4*** [1.7]
2005	64.0 (17.1)	61.1 (16.2)	3.0** [1.1]	50.0 (24.4)	56.5 (21.7)	-6.4*** [1.8]
2006	62.7 (17.3)	60.0 (17.0)	2.7* [1.2]	50.0 (24.9)	54.5 (22.6)	-4.5* [1.8]
2007	64.9 (17.3)	62.5 (16.1)	2.3* [1.2]	49.5 (23.2)	56.3 (21.3)	-6.8*** [1.7]
2008	56.2 (17.5)	53.4 (17.6)	2.9* [1.2]	45.6 (24.1)	50.9 (23.2)	-5.3** [1.8]
2009	62.6 (18.2)	60.2 (17.1)	2.4* [1.2]	47.1 (23.9)	54.2 (22.4)	-7.0*** [1.8]
2010	64.0 (16.1)	61.9 (15.6)	2.1* [1.1]	53.0 (24.6)	58.3 (21.4)	-5.2** [1.8]
2011	64.1 (16.9)	62.8 (16.1)	1.3 [1.1]	51.0 (24.7)	57.6 (21.5)	-6.6*** [1.8]
2012	58.1 (17.3)	57.2 (17.4)	0.8 [1.2]	49.2 (25.5)	54.4 (22.9)	-5.1** [1.9]
2013	62.4 (17.6)	59.2 (17.0)	3.2** [1.2]	48.7 (24.5)	54.0 (21.6)	-5.2** [1.8]
2014	55.9 (16.7)	51.4 (16.8)	4.5*** [1.1]	44.3 (23.3)	48.9 (22.0)	-4.5** [1.7]
2015	61.9 (17.5)	60.7 (16.6)	1.2 [1.2]	49.2 (24.6)	56.2 (22.3)	-7.0*** [1.8]
2016	66.0 (16.6)	65.7 (14.7)	0.2 [1.1]	52.9 (21.7)	60.2 (19.2)	-7.3*** [1.6]
2017	65.5 (16.3)	63.7 (15.6)	1.7 [1.1]	52.0 (24.7)	58.4 (21.1)	-6.4*** [1.8]
2018	63.6 (16.8)	61.4 (16.4)	2.2* [1.1]	49.4 (24.1)	55.9 (21.8)	-6.4*** [1.8]
2019	61.6 (16.7)	60.4 (15.8)	1.2 [1.1]	49.7 (23.8)	56.0 (21.3)	-6.2*** [1.8]
2020	59.9 (18.3)	58.3 (16.8)	1.7 [1.2]	46.9 (24.0)	54.5 (21.7)	-7.7*** [1.8]
2021	57.3 (17.2)	54.6 (17.2)	2.8* [1.1]	46.4 (24.7)	51.0 (23.0)	-4.6* [1.8]
2022	62.3 (16.8)	61.2 (16.4)	1.0 [1.1]	49.5 (21.9)	56.0 (21.8)	-6.5*** [1.6]
2023	58.4 (15.8)	55.4 (15.7)	3.0** [1.1]	46.4 (23.0)	52.0 (21.7)	-5.6** [1.7]

Note: this table reports means and standard deviations (in parentheses) of peak annual NDVI for common grazing plots and never allocated plots within 5 km from the settlement (near villages) and otherwise (remote). Row 'All years' computes NDVI over all years (2000-2023). Each column ignores the plot allocation date and computes statistics for all plots in the corresponding tenure categories for each year. Columns 't-test' report the difference between means in 'Common grazing' and 'Never allocated' groups, the statistical significance level of the difference, and standard errors in square brackets. Whelch's two-sample t-test is used with the assumption of unequal variances between two groups. Statistical significance levels are: '***' p-value < 0.001, '**' p-value < 0.01, '*' p-value < 0.05, p-value < '.' 0.1, and ' ' p-value >= 0.1.

Source: own calculations.

Table D7: Cumulative monthly precipitations normalized to a standard normal distribution for each month

	Normalized monthly precipitations											
	01	02	03	04	05	06	07	08	09	10	11	12
Full	0.00	0.01	-0.06	-0.02	0.02	0.01	0.01	0.01	-0.01	-0.02	-0.01	0.01
sample	(1.00)	(0.99)	(0.89)	(0.99)	(1.01)	(1.00)	(1.00)	(1.01)	(1.00)	(0.99)	(1.00)	(1.00)
2000	-0.29	-0.68	-0.49	-0.34	0.27	0.50	0.61	0.29	1.71	1.09	0.14	-0.04
	(0.57)	(0.46)	(0.45)	(0.60)	(0.88)	(0.53)	(0.61)	(0.59)	(1.38)	(1.65)	(0.71)	(0.56)
2001	-0.03	-0.67	-0.43	-0.06	0.00	-0.57	1.00	0.57	0.36	0.99	-0.78	0.25
	(0.78)	(0.35)	(0.44)	(0.69)	(0.85)	(0.60)	(0.93)	(0.80)	(0.95)	(1.58)	(0.59)	(0.87)
2002	0.45	-0.07	0.22	0.87	0.92	0.72	0.28	0.20	-0.21	0.17	-0.48	0.87
	(0.89)	(0.80)	(0.80)	(1.20)	(1.06)	(0.80)	(0.78)	(0.62)	(0.55)	(1.03)	(0.63)	(0.84)
2003	0.08	0.78	-0.01	0.69	0.84	1.61	0.92	0.00	-0.34	-0.07	1.29	0.02
	(0.75)	(0.91)	(0.74)	(1.18)	(1.00)	(1.06)	(0.62)	(0.59)	(0.53)	(0.84)	(0.93)	(0.66)
2004	-0.13	0.10	0.20	-0.06	-0.01	-0.19	2.38	-0.40	-0.38	-0.12	0.86	1.03
	(0.69)	(0.69)	(0.91)	(0.70)	(0.94)	(0.57)	(1.18)	(0.49)	(0.44)	(0.76)	(1.13)	(1.14)
2005	-0.23	-0.37	0.07	-0.36	0.35	-0.25	-0.72	2.77	-0.59	-0.22	-0.63	-0.26
	(0.55)	(0.44)	(0.83)	(0.58)	(0.99)	(0.62)	(0.50)	(1.35)	(0.43)	(0.79)	(0.51)	(0.52)
2006	0.71	0.09	-0.32	-0.36	-0.22	0.39	-0.03	-0.75	0.86	0.25	-0.33	0.02
	(0.86)	(0.90)	(0.47)	(0.61)	(0.69)	(0.73)	(0.42)	(0.42)	(0.88)	(0.99)	(0.48)	(0.55)
2007	-0.49	-0.33	0.40	-0.43	0.56	0.05	0.61	-0.11	0.40	-0.49	-0.62	-0.11
	(0.52)	(0.59)	(0.92)	(0.46)	(1.06)	(0.86)	(0.95)	(0.59)	(1.08)	(0.50)	(0.54)	(0.65)
2008	-0.20	-0.02	-0.35	-0.48	-0.36	-0.72	-0.13	-0.53	-0.21	0.30	-0.44	0.00
	(0.44)	(0.62)	(0.49)	(0.55)	(0.69)	(0.48)	(0.47)	(0.52)	(0.70)	(1.11)	(0.65)	(0.71)
2009	-0.36	-0.23	-0.22	0.72	-0.23	0.01	-0.13	0.13	1.11	-0.18	0.10	-0.05
	(0.52)	(0.76)	(0.56)	(1.06)	(0.69)	(0.60)	(0.60)	(0.56)	(1.02)	(0.76)	(0.77)	(0.58)
2010	1.36	1.98	1.00	-0.36	-0.06	1.01	0.28	0.29	-0.08	0.64	-0.78	-0.50
	(1.34)	(1.39)	(1.46)	(0.63)	(0.76)	(1.34)	(0.96)	(0.62)	(0.65)	(1.29)	(0.55)	(0.50)
2011	-0.92	1.30	-0.29	-0.41	0.52	-0.11	0.09	0.53	0.74	-0.09	0.56	-0.23
	(0.31)	(1.12)	(0.54)	(0.56)	(1.14)	(0.77)	(0.67)	(0.78)	(1.12)	(0.75)	(0.65)	(0.46)
2012	-0.70	0.43	0.16	-0.61	-0.24	-0.15	-0.53	-0.36	0.20	-0.21	0.15	0.71
	(0.42)	(0.93)	(0.80)	(0.46)	(0.68)	(0.49)	(0.46)	(0.56)	(0.89)	(0.74)	(0.68)	(0.97)
2013	1.36	-0.07	-0.27	0.29	-0.70	-0.10	-0.41	0.74	-0.59	-0.69	-0.95	0.09
	(1.41)	(0.46)	(0.50)	(1.08)	(0.54)	(0.48)	(0.53)	(0.74)	(0.45)	(0.29)	(0.48)	(0.86)
2014	0.74	0.20	-0.49	-0.25	-0.78	-0.53	-0.59	0.28	-0.11	0.28	0.18	-0.66
	(0.98)	(0.89)	(0.40)	(0.76)	(0.53)	(0.70)	(0.62)	(0.82)	(0.61)	(0.89)	(0.86)	(0.34)
2015	0.37	0.28	-0.13	-0.30	-0.07	0.48	-0.61	0.45	-0.21	-0.01	0.78	0.95
	(0.78)	(0.88)	(0.66)	(0.72)	(0.76)	(1.28)	(0.56)	(0.72)	(0.50)	(0.69)	(1.02)	(1.04)
2016	0.78	-1.25	-0.37	0.67	0.98	1.05	0.82	-0.07	-0.20	-0.10	1.23	1.57
	(1.07)	(0.16)	(0.50)	(1.34)	(1.10)	(0.81)	(0.76)	(0.80)	(0.66)	(0.70)	(1.03)	(1.27)
2017	-0.22	0.38	-0.31	1.15	-0.02	0.47	-0.56	-0.62	-0.07	-0.52	-0.06	0.22
	(0.57)	(0.71)	(0.60)	(1.53)	(0.70)	(1.09)	(0.59)	(0.50)	(0.65)	(0.40)	(0.95)	(0.59)
2018	-0.35	0.00	0.33	0.21	0.19	-0.42	-0.25	-0.77	-0.52	-0.07	-0.03	-0.32
	(0.36)	(0.65)	(0.91)	(0.96)	(0.98)	(0.77)	(0.50)	(0.44)	(0.45)	(0.73)	(0.53)	(0.51)

	Normalized monthly precipitations											
	01	02	03	04	05	06	07	08	09	10	11	12
2019	0.27	0.00	-0.67	0.11	-0.61	-0.02	-0.53	-0.64	0.40	-0.63	-0.34	0.41
	(0.83)	(0.56)	(0.33)	(0.98)	(0.65)	(0.71)	(0.64)	(0.58)	(0.79)	(0.36)	(0.84)	(0.93)
2020	-1.01	-0.19	-0.70	0.15	0.01	-0.85	-0.77	-0.15	-0.72	-0.63	-0.24	-1.38
	(0.30)	(0.70)	(0.35)	(0.89)	(0.76)	(0.55)	(0.40)	(0.60)	(0.40)	(0.34)	(0.72)	(0.18)
2021	-1.02	0.06	0.30	-0.57	-0.96	-0.99	-0.01	-0.88	-1.25	0.23	-0.79	-1.34
	(0.18)	(0.72)	(0.85)	(0.56)	(0.48)	(0.43)	(0.66)	(0.34)	(0.32)	(0.90)	(0.55)	(0.21)
2022	-0.68	-0.72	1.20	-0.50	0.82	-0.89	-1.01	-1.07	-1.10	-0.37	1.03	-1.03
	(0.54)	(0.35)	(1.50)	(0.55)	(1.27)	(0.33)	(0.43)	(0.37)	(0.28)	(0.51)	(0.98)	(0.21)
2023	0.50	-0.79	-0.39	-0.28	-0.80	-0.24	-0.52	0.42	0.59	-0.02	-0.01	0.01
	(0.77)	(0.32)	(0.55)	(0.71)	(0.60)	(0.68)	(0.47)	(0.67)	(0.80)	(0.79)	(0.69)	(0.64)

Source: own calculations.

Table D8: Monthly average of the surface temperature normalized to a standard normal distribution for each month

	Normalized mean surface temperature											
	02	03	04	05	06	07	08	09	10	11	12	01
Full	0.01	0.02	-0.02	-0.02	0.00	0.00	-0.01	-0.02	-0.01	0.01	0.01	0.01
sample	(1.00)	(1.00)	(1.03)	(1.03)	(1.02)	(1.01)	(1.02)	(1.02)	(1.01)	(1.00)	(1.00)	(1.00)
2000	0.49	-0.55	0.05	0.25	0.06	0.15	-0.15	-0.12	-1.28	-0.32	0.19	0.01
	(1.06)	(0.97)	(1.12)	(1.00)	(1.03)	(1.09)	(1.01)	(1.02)	(1.13)	(0.95)	(0.94)	(0.73)
2001	0.17	0.63	-0.42	0.26	-0.08	0.02	-0.28	-0.40	-0.39	0.97	-0.80	0.02
	(0.89)	(1.03)	(1.01)	(0.96)	(1.07)	(1.05)	(1.04)	(1.19)	(0.99)	(0.71)	(0.48)	(0.88)
2002	0.31	0.05	-0.53	-0.77	-0.55	-0.54	-0.19	-0.23	0.20	0.63	-0.74	0.16
	(0.99)	(0.95)	(1.02)	(0.89)	(0.74)	(0.86)	(0.89)	(0.89)	(0.77)	(0.93)	(0.60)	(0.99)
2003	0.05	-0.73	-0.81	-0.27	-0.20	-0.88	-0.45	-0.14	0.04	-0.91	-0.01	0.23
	(0.85)	(0.87)	(1.09)	(0.92)	(0.85)	(0.70)	(0.93)	(0.86)	(0.84)	(0.77)	(0.96)	(0.69)
2004	0.05	-0.20	-0.06	-0.23	-0.05	-0.32	-0.12	-0.18	0.18	0.47	-0.10	0.35
	(1.14)	(1.05)	(1.06)	(0.94)	(0.91)	(0.84)	(0.95)	(0.96)	(0.68)	(0.91)	(0.90)	(1.05)
2005	-0.84	0.27	0.02	-0.30	-0.04	-0.01	-0.48	0.05	0.60	0.09	-0.08	-0.15
	(0.46)	(0.78)	(0.93)	(0.86)	(0.97)	(1.07)	(0.91)	(1.00)	(0.76)	(1.01)	(0.67)	(0.72)
2006	0.60	0.47	0.23	-0.03	-0.17	-0.11	0.38	0.04	0.37	0.65	-0.02	-0.79
	(1.09)	(0.93)	(1.01)	(0.92)	(0.99)	(0.94)	(0.95)	(0.79)	(0.81)	(0.78)	(0.62)	(0.58)
2007	0.90	-0.12	0.32	-0.19	0.03	-0.24	-0.14	0.41	-0.03	0.55	-0.62	0.56
	(0.88)	(0.90)	(0.69)	(0.87)	(1.01)	(0.88)	(0.99)	(0.93)	(1.00)	(0.86)	(0.90)	(0.93)
2008	-0.38	0.65	0.07	0.47	0.47	0.28	0.27	0.11	0.02	-0.05	1.27	-1.08
	(0.80)	(0.81)	(0.93)	(1.01)	(1.07)	(0.97)	(1.02)	(0.94)	(0.93)	(0.92)	(1.06)	(0.58)
2009	0.20	0.05	-0.45	-0.21	-0.08	-0.10	-0.16	-0.74	0.22	-0.43	-0.14	0.75
	(0.89)	(0.93)	(1.00)	(1.14)	(1.04)	(0.93)	(1.02)	(1.00)	(0.78)	(0.96)	(0.77)	(0.92)
2010	-0.76	-0.50	-0.29	-0.46	-0.19	-0.33	-0.31	-0.18	0.18	0.72	0.39	-0.31
	(0.36)	(0.84)	(1.07)	(0.95)	(0.97)	(0.92)	(0.95)	(0.82)	(0.86)	(0.84)	(0.77)	(0.63)
2011	0.06	-0.49	0.07	-0.24	-0.28	-0.19	-0.05	0.11	0.15	-0.01	-0.23	-0.71
	(0.82)	(0.82)	(1.01)	(0.79)	(0.98)	(0.88)	(0.99)	(0.94)	(0.83)	(1.07)	(0.78)	(0.68)
2012	-0.84	-0.40	0.62	0.21	-0.05	0.08	0.32	0.33	-0.12	-0.18	-1.01	-0.62
	(0.38)	(0.97)	(0.83)	(0.95)	(1.03)	(1.03)	(0.95)	(0.93)	(1.05)	(0.71)	(0.48)	(0.70)
2013	-0.64	0.49	0.23	0.12	0.15	0.04	-0.07	0.41	0.99	0.57	0.67	-0.09
	(0.40)	(0.86)	(0.79)	(0.98)	(0.95)	(0.95)	(0.98)	(0.94)	(0.62)	(0.83)	(1.11)	(0.64)
2014	-1.23	-0.22	-0.05	0.60	0.34	0.41	0.55	0.24	-0.43	-0.34	-0.04	-0.15
	(0.30)	(0.70)	(0.89)	(0.96)	(0.99)	(0.94)	(1.00)	(1.00)	(0.83)	(0.95)	(0.87)	(0.63)
2015	0.36	-0.36	0.00	0.10	0.16	0.30	0.21	-0.51	0.40	-0.25	0.52	0.45
	(0.99)	(0.94)	(0.97)	(1.15)	(1.14)	(0.98)	(1.02)	(1.03)	(0.71)	(0.85)	(1.24)	(1.00)
2016	-0.03	0.85	0.27	-0.74	-0.15	-0.50	-0.54	0.10	-0.71	-0.52	0.29	0.93
	(0.94)	(0.73)	(0.88)	(0.77)	(0.86)	(0.78)	(0.97)	(0.92)	(0.82)	(0.68)	(0.93)	(1.14)
2017	0.12	-0.55	-0.37	0.08	-0.20	0.25	-0.04	0.63	0.13	0.05	0.34	0.02
	(0.83)	(1.00)	(1.10)	(0.92)	(0.97)	(1.03)	(0.93)	(0.92)	(0.69)	(0.85)	(0.85)	(0.94)
2018	-0.22	-0.06	0.01	-0.37	0.12	-0.02	0.16	-0.34	0.46	-1.18	-0.08	-0.59
	(0.74)	(0.82)	(0.89)	(1.04)	(0.98)	(0.94)	(1.01)	(0.93)	(0.82)	(0.69)	(0.75)	(0.60)

	Normalized mean surface temperature											
	02	03	04	05	06	07	08	09	10	11	12	01
2019	-0.30	0.52	-0.13	0.07	0.03	0.23	0.46	-0.34	-0.19	-0.03	0.31	0.38
	(0.67)	(0.92)	(0.83)	(1.02)	(1.04)	(1.00)	(0.97)	(0.85)	(0.96)	(0.80)	(0.79)	(0.90)
2020	0.66	0.39	0.48	0.56	0.24	0.19	0.07	-0.47	0.13	0.27	-0.30	0.13
	(0.77)	(0.88)	(0.92)	(1.00)	(1.12)	(1.00)	(0.99)	(0.94)	(0.98)	(0.89)	(0.71)	(0.73)
2021	1.00	0.04	0.30	0.59	0.39	0.61	0.22	0.73	-1.28	-0.37	0.84	0.19
	(1.05)	(0.90)	(1.20)	(1.09)	(1.09)	(0.98)	(1.10)	(0.90)	(1.07)	(0.93)	(1.22)	(0.68)
2022	0.30	-0.36	0.14	0.16	0.03	0.20	0.11	0.77	0.20	-0.21	-0.44	1.22
	(0.95)	(0.88)	(1.03)	(0.82)	(1.00)	(0.98)	(0.95)	(0.86)	(0.97)	(0.83)	(0.73)	(1.32)
2023	0.20	0.63	-0.08	-0.08	0.15	0.50	0.00	-0.74	-0.01	0.01	0.01	-0.70
	(0.85)	(0.78)	(0.96)	(1.09)	(0.89)	(0.99)	(0.89)	(0.79)	(0.82)	(0.79)	(0.75)	(0.59)

Source: own calculations.

Table D9: Monthly cumulative short wave radiation flux (watt per sq. meter) normalized to a standard normal distribution for each month

	Standardized cumulative short wave radiation flux											
	01	02	03	04	05	06	07	08	09	10	11	12
Full	0.00	-0.01	0.00	-0.01	-0.02	-0.02	-0.02	-0.01	-0.01	0.00	0.00	0.00
sample	(0.99)	(1.00)	(0.99)	(0.99)	(1.00)	(1.00)	(1.00)	(0.99)	(1.00)	(1.00)	(0.99)	(0.99)
2000	-0.49	1.48	0.59	0.39	-0.14	-0.36	-0.65	0.18	0.19	-0.92	-0.88	-0.40
	(1.16)	(0.99)	(0.80)	(0.58)	(0.66)	(0.61)	(0.55)	(0.50)	(0.69)	(0.57)	(0.94)	(1.06)
2001	0.50	0.65	1.49	0.30	0.22	0.15	-0.67	-0.02	-0.13	-0.19	1.06	-1.44
	(0.99)	(0.86)	(0.76)	(0.84)	(0.71)	(0.52)	(0.46)	(0.59)	(0.89)	(0.60)	(0.90)	(1.17)
2002	-0.44	-0.07	-0.56	-2.04	-0.28	-0.49	-0.07	0.58	1.00	0.42	0.59	-0.87
	(1.07)	(1.08)	(0.77)	(0.65)	(0.83)	(0.73)	(0.79)	(0.64)	(0.85)	(0.65)	(1.12)	(0.96)
2003	-0.54	-0.62	-0.81	-1.54	-1.05	-0.65	-0.51	1.27	1.12	0.46	-1.10	-0.19
	(0.83)	(0.73)	(0.84)	(0.70)	(0.85)	(0.81)	(0.63)	(0.53)	(0.62)	(0.65)	(1.16)	(1.41)
2004	-0.29	-0.01	-0.66	-0.23	0.55	0.10	-1.16	0.04	0.58	0.76	-1.11	-0.86
	(1.18)	(1.05)	(1.04)	(0.84)	(0.99)	(0.93)	(0.71)	(0.71)	(0.60)	(0.42)	(1.22)	(1.28)
2005	-0.83	0.15	0.23	0.98	-1.55	-0.23	0.68	-0.22	0.71	1.02	0.06	0.42
	(1.18)	(1.02)	(0.85)	(0.70)	(0.65)	(0.91)	(1.02)	(1.04)	(1.01)	(0.43)	(0.99)	(0.72)
2006	-0.73	-0.79	-0.13	0.21	-0.88	-0.39	1.09	1.74	0.60	0.20	-0.09	0.36
	(1.14)	(1.15)	(0.73)	(0.68)	(0.90)	(0.88)	(0.73)	(0.55)	(0.73)	(0.62)	(0.74)	(0.89)
2007	0.88	-0.24	-0.93	-0.05	-0.01	0.20	-1.06	1.18	1.84	1.10	0.30	-0.55
	(1.07)	(0.94)	(0.48)	(0.58)	(0.47)	(0.88)	(0.91)	(0.63)	(0.55)	(0.53)	(0.55)	(0.93)
2008	-0.06	0.26	0.50	0.11	-0.21	0.96	-0.50	-0.11	0.08	-0.08	0.65	-0.16
	(0.65)	(0.39)	(0.49)	(0.55)	(0.59)	(0.77)	(0.81)	(0.48)	(0.53)	(0.49)	(0.47)	(0.50)
2009	-0.04	-1.12	-0.37	-0.81	-0.01	0.06	0.00	-0.10	-1.16	0.59	0.14	0.16
	(0.71)	(0.76)	(0.64)	(0.68)	(0.76)	(0.92)	(0.79)	(0.73)	(0.88)	(0.30)	(0.53)	(0.48)
2010	-0.98	-1.23	-0.51	-0.25	-0.16	-1.52	-0.23	0.10	-0.45	0.19	1.46	0.75
	(0.48)	(0.55)	(0.36)	(0.78)	(0.82)	(1.15)	(0.80)	(0.64)	(0.32)	(0.31)	(0.41)	(0.28)
2011	0.63	-0.62	0.76	0.57	0.69	1.34	1.61	0.56	0.65	0.15	0.13	0.65
	(0.30)	(0.38)	(0.31)	(0.45)	(0.95)	(1.12)	(0.66)	(0.90)	(0.30)	(0.30)	(0.50)	(0.37)
2012	0.84	0.54	0.56	1.56	1.27	1.52	1.30	1.31	-0.27	0.05	-0.08	0.22
	(0.33)	(0.38)	(0.43)	(0.27)	(0.61)	(0.68)	(0.52)	(0.31)	(0.40)	(0.29)	(0.56)	(0.37)
2013	0.89	0.82	1.12	0.92	0.93	0.80	0.27	-0.29	0.32	0.67	0.32	0.01
	(0.46)	(0.51)	(0.60)	(0.38)	(0.44)	(0.52)	(0.51)	(0.38)	(0.37)	(0.25)	(0.45)	(0.74)
2014	-0.71	-0.37	0.40	0.58	1.96	0.61	1.22	0.38	-0.70	-0.32	-0.64	0.26
	(0.75)	(0.46)	(0.48)	(0.44)	(0.30)	(0.50)	(0.46)	(0.29)	(0.57)	(0.42)	(0.81)	(0.49)
2015	-0.52	-0.46	-0.08	0.45	0.10	0.55	0.49	-0.35	-0.40	-0.05	-0.68	-0.39
	(0.58)	(0.55)	(0.59)	(0.53)	(0.40)	(0.38)	(0.42)	(0.39)	(0.59)	(0.52)	(0.91)	(0.66)
2016	-0.41	1.52	0.11	-0.25	-0.47	-0.23	-0.84	-1.20	-0.76	-0.22	-0.56	-1.04
	(0.86)	(0.42)	(0.79)	(0.67)	(0.65)	(0.55)	(0.68)	(0.55)	(0.77)	(0.52)	(0.79)	(0.90)
2017	-0.27	-0.23	-0.32	-0.82	0.22	-0.39	0.36	-1.04	-0.57	-0.05	0.20	-0.08
	(0.92)	(0.55)	(0.76)	(0.72)	(0.64)	(0.39)	(0.39)	(0.45)	(0.48)	(0.30)	(0.67)	(0.78)
2018	0.32	-0.34	-1.22	-0.15	-0.18	-0.35	0.16	-0.12	-0.23	-0.21	-0.43	0.44
	(0.44)	(0.55)	(0.87)	(0.43)	(0.67)	(0.82)	(0.49)	(0.49)	(0.32)	(0.34)	(0.64)	(0.57)

	Standardized cumulative short wave radiation flux											
	01	02	03	04	05	06	07	08	09	10	11	12
2019	0.22	0.45	0.54	-0.97	0.34	-0.34	0.23	-0.22	-0.44	0.16	0.19	0.32
	(0.51)	(0.47)	(0.45)	(0.79)	(0.34)	(0.51)	(0.42)	(0.40)	(0.54)	(0.31)	(0.58)	(0.63)
2020	0.36	0.47	0.99	0.09	-0.36	-0.52	-0.76	-0.41	-0.86	0.16	-0.06	0.88
	(0.53)	(0.44)	(0.39)	(0.59)	(0.29)	(0.44)	(0.50)	(0.42)	(0.55)	(0.19)	(0.52)	(0.48)
2021	0.74	-0.62	-1.07	0.33	-0.15	-0.04	-0.86	-1.05	0.37	-0.10	0.87	0.82
	(0.45)	(0.71)	(0.56)	(0.46)	(0.52)	(0.40)	(0.51)	(0.50)	(0.43)	(0.27)	(0.51)	(0.60)
2022	0.31	0.60	-1.19	0.15	-1.20	-0.27	-0.13	-1.22	0.14	-0.13	-0.37	0.70
	(0.90)	(0.50)	(0.47)	(0.49)	(0.52)	(0.58)	(0.51)	(0.62)	(0.40)	(0.25)	(0.58)	(0.27)
2023	0.61	-0.34	0.44	0.25	-0.03	-1.00	-0.39	-1.27	-1.78	-3.76	0.00	0.00
	(0.32)	(0.41)	(0.50)	(0.49)	(0.53)	(0.46)	(0.53)	(0.45)	(0.49)	(0.10)	(0.58)	(0.60)

Source: own calculations.

E.5 Weights decomposition of the BM estimates

Table E1 presents the weights decomposition of τ^{BM} derived following the methodology of (de Chaisemartin and DHaultfoeuille 2020) based on the benchmark model (column BM) as well as BM without the synthetic unallocated plots (BM no unallocated) and BM without controls. About 20% to 30% of all weights are negative in all models. Sigma, of the cross-group heterogeneity, is small (e.g. as $|\tau^{\mathrm{BM}}| < \sqrt{3}\sigma_{fe}$), suggesting based on the Corollary 1 in (de Chaisemartin and DHaultfoeuille 2020) that even smallest plausible treatment effect heterogeneity could lead to the bias estimates of the ATT. This calls for using relevant heteroscedasticity robust estimators. Results are robust to the functional form without covariates and sample variation without synthetic unallocated plots.

Table E1: Weights decomposition of the benchmark model

	BM	BM (no unallocated)	BM (no controls)
N positive weights	175 259	153 330	172 226
N negative weights	39 661	61 590	42 694
% share of negative weights	18.5%	28.7%	19.9%
Sum of positive weights	1.111	1.316	1.111
Sum of negative weights	-0.111	-0.316	-0.111
Estimated ATT	-0.0024	-0.0031	-0.0014
Sigma of the cross-group heterogeneity	0.0022	0.0019	0.0013
Minimal value of Sigma to bias the estimate of ATT	0.00001	0.00001	0.00000

Source: own calculations.

Figure E1 presents the weights decomposition for the BM model using (Goodman-Bacon 2021) methodology. Note that we had to use BM estimation without any covariates. We use the R package `bacondecomp` for performing Goodman-Bacon (2021) decomposition. It fails to run some individual 2x2 DiD estimates with our remotely sensed covariates. The likely reason is that our covariates lack variance in some 2x2 comparisons. Climate data has a coarse spatial resolution (10 km) and plots in some 2x2 comparisons are spatially close, therefore those are heavily affected by collinearity. Nevertheless, functional form sensitivity analysis shows that our estimates are robust to omitting covariates. Therefore, we can treat Figure E1 as the second-best option.

Figure E1 shows that weighted averages of all 2x2 DiD comparisons are negative (DiD est.), even for the "forbidden comparisons". This suggests that the opposite sign bias is unlikely in our analysis. About 70% of weights are on treated vs untreated or earlier treated vs later treated suggesting that the bulk of the ATT estimate originates from the clean comparisons. This cautiously ensures the validity of the TWFE model, although calls again for the heteroscedasticity robust estimators. It is likely that with a heteroscedasticity robust estimator, the estimated ATT will not change substantially, however, it may become more efficient.

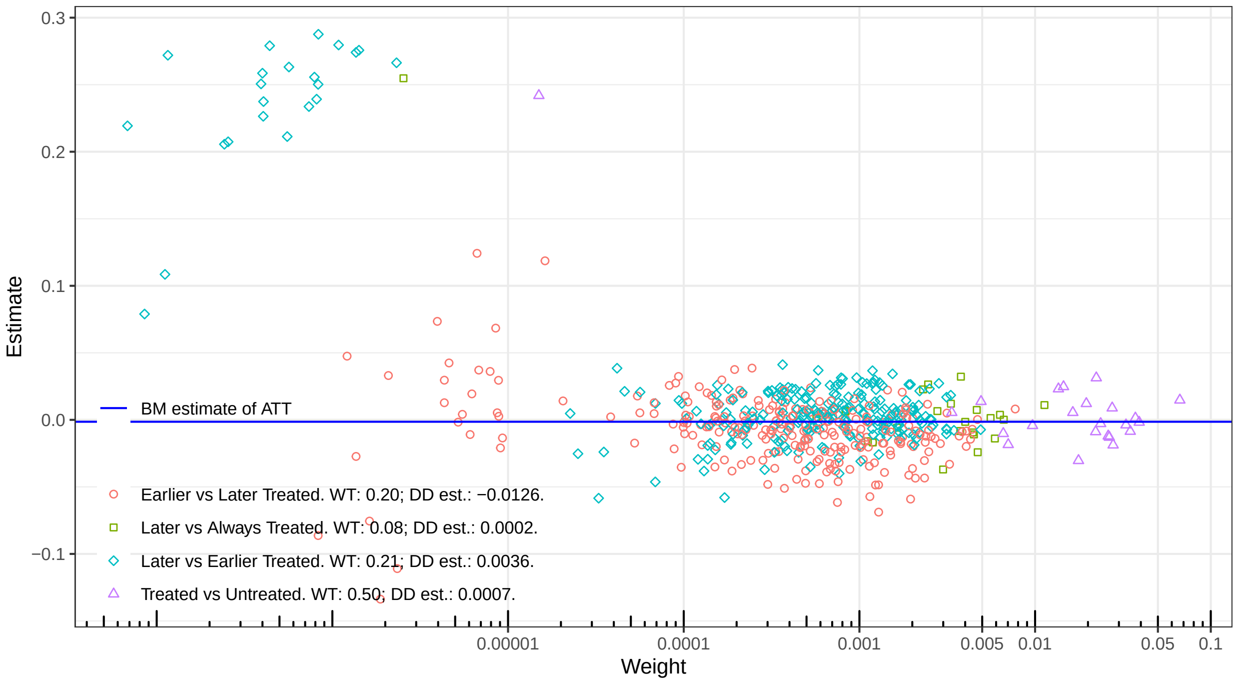

Figure E1: Weights structure of the BM estimates of the ATT using Goodman-Bacon (2021) decomposition

F.6 Functional form and control variables

Equation 6 specifies how control variables X are introduced in our regressions. To account for possible non-linearities in the biological processes of vegetation growth as a response to the fluctuations in rainfall, temperature, and solar radiation, we introduce each time-varying climatic control variable individually for each month M (36 variables with coefficients β_{1M}, β_{2M}, and β_{3M}), along with the full combination of the within-month interaction terms: pairwise (36 variables with coefficients β_{4M}, β_{5M}, and β_{6M}) and triple (12 variables with coefficients β_{7M}). Our control strategy consists of 84 control variables. In the early stages, we considered cross-month interaction terms between variables, however, such specifications quickly became prone to overfitting without regularization, which is beyond the scope of our analysis.

$$
\begin{aligned}
X = & \sum_M \beta_{1M} \mathrm{RF}_{M,i,t} + \sum_M \beta_{2M} \mathrm{TMP}_{M,i,t} + \sum_M \beta_{3M} \mathrm{RAD}_{M,i,t} + \\
& \sum_M \beta_{4M} \mathrm{RF}_{M,i,t} \times \mathrm{TMP}_{M,i,t} + \\
& \sum_M \beta_{5M} \mathrm{RF}_{M,i,t} \times \mathrm{RAD}_{M,i,t} + \\
& \sum_M \beta_{6M} \mathrm{TMP}_{M,i,t} \times \mathrm{RAD}_{M,i,t} + \\
& \sum_M \beta_{7M} \mathrm{RF}_{M,i,t} \times \mathrm{TMP}_{M,i,t} \times \mathrm{RAD}_{M,i,t}
\end{aligned} \tag{6}
$$

G.7 Auxiliary results for the main regression analysis

Table G1: Estimates sensitivity to functional form using SA estimator

	BM	SA	SA excl. interactions	SA April-August	SA excl. controls
ATT	-0.0041**	-0.0035***	-0.0024*	-0.0040***	-0.0042***
	(0.0013)	(0.0010)	(0.0010)	(0.0010)	(0.0010)
Rain, m. 5	0.0162***	0.0164***	-0.0117***	0.0166***	
	(0.0006)	(0.0007)	(0.0006)	(0.0006)	
Rain, m. 6	-0.0052***	-0.0054***	-0.0108***	-0.0066***	
	(0.0005)	(0.0005)	(0.0004)	(0.0005)	
Rain, m. 7	0.0121***	0.0129***	0.0078***	0.0134***	
	(0.0007)	(0.0007)	(0.0006)	(0.0007)	
Temperature, m. 5	-0.0594***	-0.0591***	-0.0630***	-0.0637***	
	(0.0007)	(0.0007)	(0.0008)	(0.0007)	
Temperature, m. 6	-0.0444***	-0.0445***	-0.0546***	-0.0483***	
	(0.0009)	(0.0009)	(0.0008)	(0.0009)	
Temperature, m. 7	-0.0214***	-0.0209***	-0.0302***	-0.0226***	
	(0.0009)	(0.0009)	(0.0009)	(0.0009)	
N obs.	565,680	565,679	565,679	565,679	565,679
N ind. FE	23,570	23,570	23,570	23,570	23,570
N indep. var.	130	613	565	564	529
Within R sq. adj.	21.1	21.6	16.8	18.3	1.7

Statistical significance levels are: '***' p-value < 0.001, '**' p-value < 0.01, '*' p-value < 0.05, p-value < '.' 0.1, and ' ' p-value >= 0.1.

Source: own calculations.

Table G1 tests the robustness of the estimates to the functional form. It reports results estimated with the SA estimator for models without any control variables (column "SA excl. controls"), with the same set of control, but without within-month interaction terms (column "SA excl. interactions"), and with controls for only five months between April and August. All specifications point towards the robustness of the main ATT estimands. As rainfall, surface temperature, and radiation are introduced in the normalized to the standard normal distribution form, their interpretation must be adjusted. Specifically, it is a log-level transformation where a change of the control variable by 1σ (1 standard deviation compared to the historical levels) causes approximately $100\beta\%$ change in the peak NDVI. As in all regressions, except for columns "SA excl. interactions" and "SA excl. controls", all control variables interact with each other within a month, Table G1 reports their marginal effects evaluated at the mean of the corresponding interaction terms and delta-method-based standard errors.

Table G2: Estimates sensitivity to allocation date quality and weighting by plot area

	BM static	BM	SA	IMP static	IMP
Panel A. Including never-allocated					
All allocation dates	-0.0024***	-0.0030**	-0.0035***	-0.0050***	-0.0034***
[N = 565,679; N plots =23,570]	(0.0006)	(0.0009)	(0.0010)	(0.0010)	(0.0007)
All allocation dates (area weighted)	0.0000	-0.0025	-0.0023	-0.0016	-0.0043***
[N = 565,679; N plots =23,570]	(0.0021)	(0.0030)	(0.0026)	(0.0024)	(0.0011)
Exact allocation date	-0.0040***	-0.0042***	-0.0053***	-0.0062***	-0.0021*
[N = 413,591; N plots =17,233]	(0.0008)	(0.0012)	(0.0012)	(0.0014)	(0.0009)
Exact allocation date (area weighted)	-0.0073***	-0.0091***	-0.0093***	-0.0103***	-0.0045***
[N = 413,591; N plots =17,233]	(0.0017)	(0.0023)	(0.0020)	(0.0018)	(0.0010)
Not exact allocation date	0.0014	0.0005	0.0010	0.0020.	-0.0006.
[N = 337,678; N plots =14,070]	(0.0009)	(0.0012)	(0.0012)	(0.0011)	(0.0003)
Panel B. Excluding never-allocated					
All allocation dates	-0.0031***	-0.0025*	-0.0048***	-0.0132***	-0.0173***
[N = 380,087; N plots =15,837]	(0.0006)	(0.0013)	(0.0013)	(0.0022)	(0.0030)
All allocation dates (area weighted)	-0.0011	-0.0190*	0.0016	-0.0118***	-0.0183***
[N = 380,087; N plots =15,837]	(0.0018)	(0.0089)	(0.0029)	(0.0029)	(0.0032)
Exact allocation date	-0.0026**	0.0028*	-0.0068***	-0.0056*	-0.0060*
[N = 227,999; N plots =9,500]	(0.0009)	(0.0011)	(0.0016)	(0.0022)	(0.0027)
Exact allocation date (area weighted)	-0.0030.	-0.0075**	-0.0091***	-0.0099**	-0.0121***
[N = 227,999; N plots =9,500]	(0.0017)	(0.0025)	(0.0025)	(0.0031)	(0.0035)
Not exact allocation date	-0.0046***	0.0016	-0.0165	-0.0119***	-0.0163***
[N = 152,086; N plots =6,337]	(0.0009)	(0.0104)	(0.0140)	(0.0025)	(0.0034)

Statistical significance levels are: '***' p-value < 0.001, '**' p-value < 0.01, '*' p-value < 0.05, p-value < '.' 0.1, and ' ' p-value >= 0.1.

Source: own calculations.

Table G2 presents estimates of the main regression results using a range of heterogeneity robust estimators. Row names indicate sub-samples, number of observations, number of plots (fixed effects), and (if any) weighting were used for estimating the corresponding ATT. We estimated ATT for the entire sample of all plots ("All allocation dates", same as main regression results in Table 2), and separately by plots with exact allocation date ("Exact allocation date") and approximate allocation date ("Not exact allocation date"). We further introduce weights by plot-specific areas ("area weighted") to some regression to assume that plot areas are our sampling weights that are used to represent the population of all pasture land in the study region. Finally, panels A and B distinguish between samples that include and exclude synthetic never-allocated plots.

Based on Table G2, we can conclude that our main results are robust to sample variability and weighting. The exact plot allocation date only improves the specificity of our ATT estimates making them more pronounced and negative, while, plots with the approximate allocation date only dilute our main estimation results. Sub-sample of plots with approximate allocation date either yields zero results (if we include never-allocated plots in Panel A), or shows a similar effect (Panel B).

Table G3: Main ATT estimates by parcel

	BM static	BM	SA	CS (NYT)	CS (NT)	IMP static	IMP
Panel: A. Full sample							
ATT	-0.0025**	-0.0012	-0.0028*	-0.0021*	-0.0018.	-0.0049***	-0.0022***
	(0.0008)	(0.0012)	(0.0012)	(0.0010)	(0.0010)	(0.0011)	(0.0006)
N ind. FE	13,922	13,922	13,922	13,922	13,922	12,918	12,918
N obs.	334,128	334,128	334,127	334,127	334,127	310,032	310,032
Within R sq. adj.	22.0	22.0	22.4				
Panel: B. Excl. never-allocated							
ATT	-0.0043***	-0.0025	-0.0070***	-0.0018		-0.0148***	-0.0204***
	(0.0009)	(0.0017)	(0.0015)	(0.0014)		(0.0014)	(0.0016)
N ind. FE	9,162	9,162	9,162	9,162		7,818	7,818
N obs.	219,888	219,888	219,887	219,887		187,634	187,634
Within R sq. adj.	22.8	22.9	23.4				

Note: This table uses parcels as our units of observations.

Statistical significance levels are: '***' p-value < 0.001, '**' p-value < 0.01, '*' p-value < 0.05, p-value < '.' 0.1, and ' ' p-value >= 0.1.

Source: own calculations.

One line of criticism often voiced to such analysis is the pseudoreplication of the units of observations by splitting parcels (which are the subjects of land allocation) by land cover types into plots. As discussed before the rationale between splitting parcels into plots is simple. Not all land is suitable for grazing, therefore, landowners may be willing to enforce control over their land only, where vegetation is best suited for grazing (land cover is pastures) and where vegetation is less suitable for grazing (pastures on slopes), lesser control is implemented. Having those parts of parcels as independent units of analysis (plots) allows for better control for any endogenous plot characteristics with individual fixed effects thereby estimating true ATT, and minimizing the OVB.

Although one may still debate if using plots as a unit of analysis introduces "bad controls", compared to using parcels, we would argue the opposite. Having parcels as units of observation averages the picture across land use practices within each parcel leading to "bad controls" for OBV as compared to the plot-level analysis. Table G3 reports estimates of the ATT for the parcel-level regression analysis, which have little to no difference from the main estimates of the plot-level ATT.

Table G4: Estimates sensitivity to inclusion of the plot-level linear trend

	SA	Area-weighted	Exact alloc.	Not exact alloc.
Panel: A. Full sample				
ATT	-0.0139***	0.0012	-0.0003	-0.0032***
N obs.	565,679	565,679	413,591	337,678
N ind. FE	23,570	23,570	17,233	14,070
Within R sq. adj.	23.0	35.2	22.7	23.0
Panel: B. Excl. never-allocated				
ATT	-0.0019	0.0496***	-0.0005	-0.0057
N obs.	380,087	380,087	227,999	152,086

	SA	Area-weighted	Exact alloc.	Not exact alloc.
N ind. FE	15,837	15,837	9,500	6,337
Within R sq. adj.	23.7	36.9	24.1	23.4

Note: Each regression includes plot-specifi linear trend introduced as a variable slope of the linear trend.

Statistical significance levels are: '***' p-value < 0.001, '**' p-value < 0.01, '*' p-value < 0.05, p-value < '.' 0.1, and ' ' p-value >= 0.1.

Source: own calculations.

Figure G1 and Figure G2 shows the event study results for main estimates of the ATT with y-axis transformed to percent change in vegetation due to land allocation instead of plain coefficients. Points on the plot show estimates of the ATT at different periods before (-10:-1) and after (0:20) the land allocation. Whiskers around the points indicate the 95% confidence intervals (CI) of the point estimates. Once the CI includes zero, point estimates are turning insignificant at the 5% level, which is indicated on the plot with the dashed lines of the whiskers. Similarly to the previous results, figures show that right after plot allocation (period 0) pasture qualities deteriorate, although, a negative significant effect is observed on and after the second year after allocation (period 1).

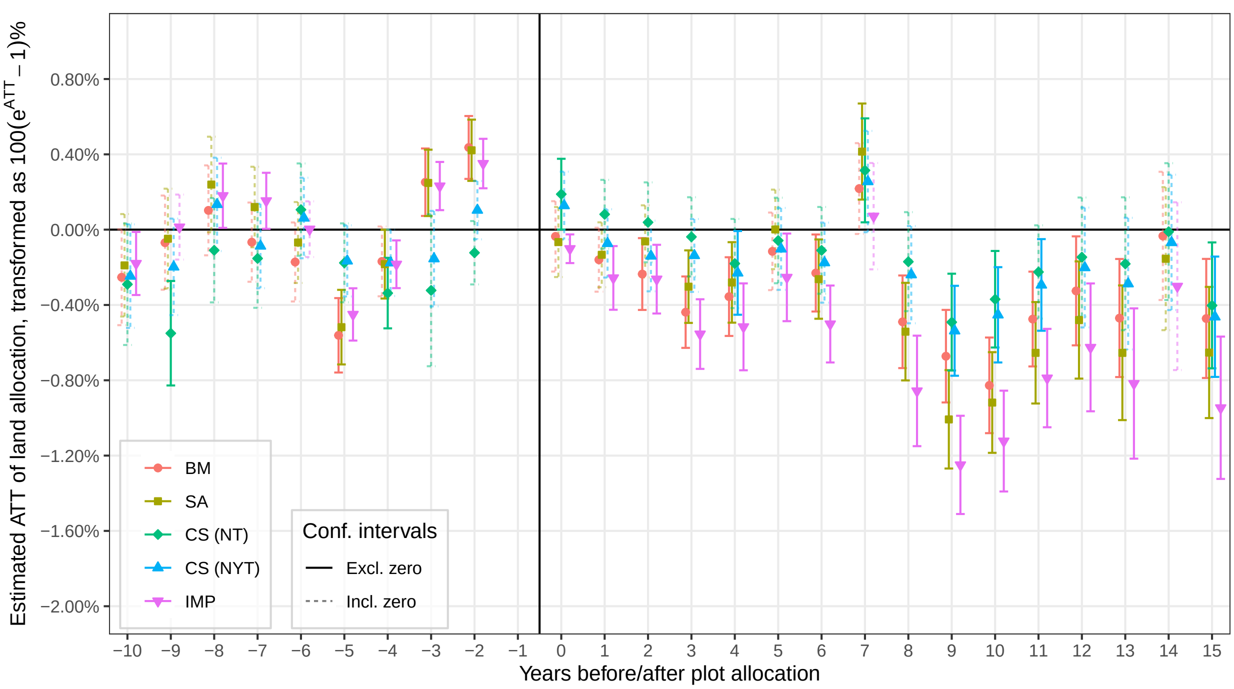

Figure G1: Event study of land allocation

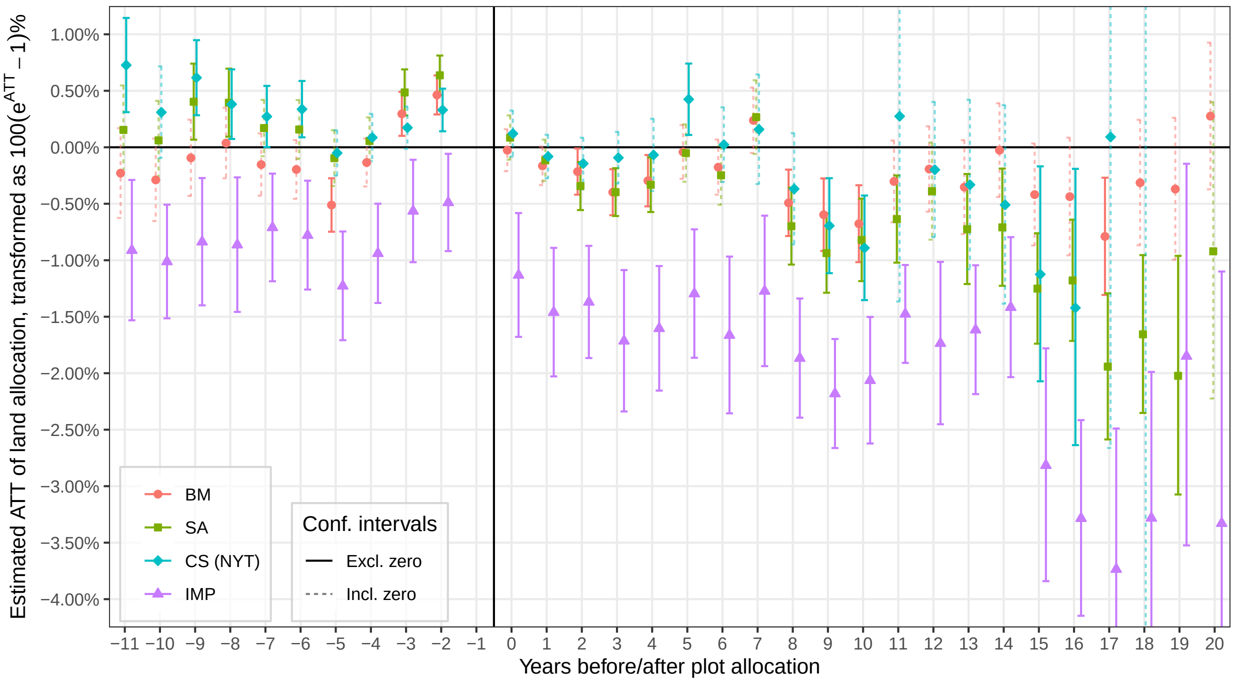

Figure G2: Event-study of land allocation on a sample that excludes never-allocated plots

Table G5: ATT sensitivity to year befor-after aggregation into bins using SA estimator

	SA (2 years)	SA (3 years)	SA (4 years)	SA (5 years)	SA (no unalloc. 2 years)	SA (no unalloc. 3 years)	SA (no unalloc. 4 years)	SA (no unalloc. 5 years)
ATT	-0.0030**	-0.0032**	-0.0030**	-0.0030**	-0.0049***	-0.0046***	-0.0043***	-0.0043***
	(0.0010)	(0.0010)	(0.0010)	(0.0010)	(0.0013)	(0.0013)	(0.0012)	(0.0011)
N obs.	552,479	552,479	552,479	552,479	366,887	366,887	366,887	366,887
N ind. FE	23,020	23,020	23,020	23,020	15,287	15,287	15,287	15,287
N time FE	24	24	24	24	24	24	24	24
N indep. var.	108	100	96	94	108	100	96	94
R sq. adj.	95.4	95.4	95.4	95.4	95.5	95.5	95.5	95.5
Within R sq. adj.	21.6	21.5	21.5	21.4	22.5	22.3	22.3	22.2

Note: Row 'ATT' reports the average treatment effect on the treated for all periods of analysis. Heteroscedasticity robust standard errors clustered at plot level are reported in parentheses.

Statistical significance levels are: '***' p-value < 0.001, '**' p-value < 0.01, '*' p-value < 0.05, p-value < '.' 0.1, and ' ' p-value >= 0.1.

Source: own calculations.

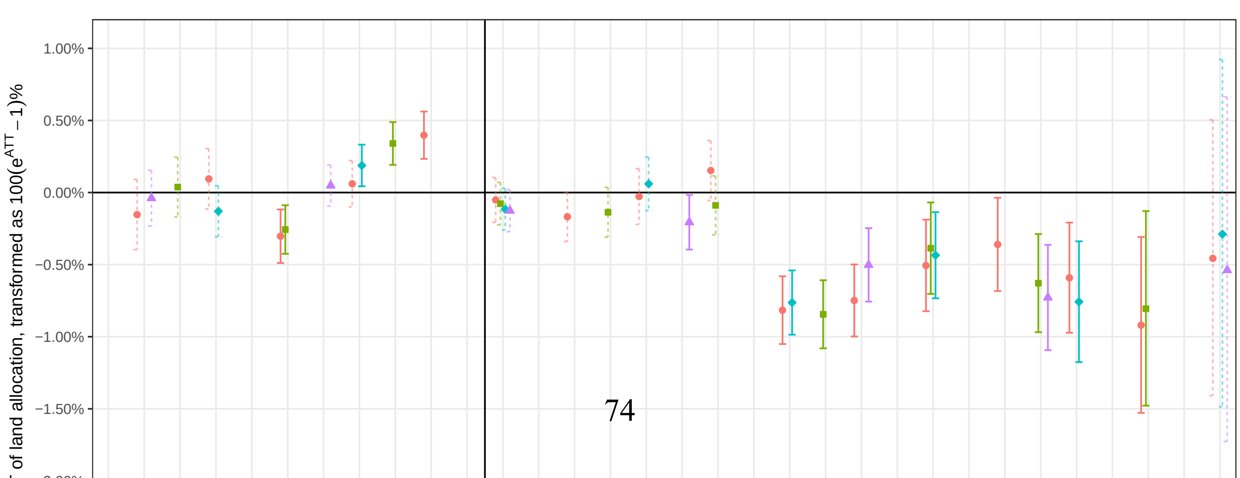

Table G6: Estimates sensitivity to sample by distance to settlement

	2km	2-5km	5km	5-10km	10km
Panel A. Including never-allocated					
BM static	-0.0056***	-0.0023.	-0.0034***	-0.0019.	0.0020.
	(0.0015)	(0.0012)	(0.0010)	(0.0011)	(0.0011)
BM	-0.0043*	-0.0019	0.0103.	-0.0038.	-0.0018
	(0.0021)	(0.0021)	(0.0055)	(0.0020)	(0.0026)
SA	-0.0051.	-0.0041.	-0.0057***	-0.0033	0.0075*
	(0.0027)	(0.0022)	(0.0017)	(0.0023)	(0.0034)
CS (NYT)	-0.0011	-0.0006	-0.0016	0.0037	0.0010
	(0.0032)	(0.0026)	(0.0019)	(0.0035)	(0.0031)
IMP static	-0.0086**	-0.0059***	-0.0079***	-0.0046**	-0.0107**
	(0.0027)	(0.0017)	(0.0015)	(0.0017)	(0.0036)
IMP	-0.0116***	-0.0072***	-0.0102***	-0.0057**	-0.0164***
	(0.0028)	(0.0020)	(0.0016)	(0.0019)	(0.0049)
N obs.	65,831	105,695	171,527	110,231	98,327
Panel B. Excluding never-allocated					
BM static	-0.0001	-0.0015	-0.0020*	-0.0012	0.0007
	(0.0016)	(0.0012)	(0.0010)	(0.0011)	(0.0012)
BM	0.0036	-0.0020	-0.0017	-0.0016	-0.0048*
	(0.0022)	(0.0017)	(0.0014)	(0.0016)	(0.0020)
SA	0.0022	-0.0022	-0.0021	-0.0015	-0.0059**
	(0.0023)	(0.0016)	(0.0013)	(0.0016)	(0.0022)
CS (NYT)	0.0000	-0.0001	-0.0010	0.0001	0.0034
	(0.0016)	(0.0014)	(0.0011)	(0.0014)	(0.0083)
IMP static	0.0002	-0.0028*	-0.0034**	-0.0017	-0.0031
	(0.0022)	(0.0014)	(0.0012)	(0.0014)	(0.0022)
IMP	-0.0003	-0.0010	-0.0012.	-0.0003	-0.0034*
	(0.0008)	(0.0008)	(0.0006)	(0.0010)	(0.0015)
N obs.	110,399	153,551	263,951	155,399	146,327

Source: own calculations.

H.8 Auxiliary results for regression analysis by tenure and landcover

Table H1: ATT by tenure with no change in the land use

Estimator	Common (all)	Common (near)	Common (remote)	Forest	Protected	Ag. other	Households
Panel A. Full sample							
BM static	0.0227***	0.0124**	0.0284**	0.0224***	-0.0064***	-0.0069	0.0231***
BM	0.0103*	0.0081	0.0056	0.0058*	-0.0071**	-0.0006	0.0154*
SA	0.0147**	0.0139**	0.0069	0.0127***	-0.0091***	-0.0015	0.0150*
IMP static	0.0225***	0.0127**	0.0274**	0.0257***	-0.0067***	-0.0058	0.0198***
IMP	0.0001***	0.0001**	0.0001**	0.0005**	-0.0003***	-0.0002	0.0001*
N obs.	195,768	98,112	97,656	211,416	207,216	195,816	188,520
N ind. FE	8,157	4,088	4,069	8,809	8,634	8,159	7,855
Panel B. Excl. never-allocated							
BM static	0.0147*	0.0084	-0.1216*	0.0009	-0.0135***	-0.0005	0.0050
BM	0.0082	0.0091	-0.0577*	-0.0044.	0.0221*	-0.0429	0.0384***
SA	-0.0258	0.0248*	0.0985*	-0.0067	0.0175**	0.0319	-0.0605***
IMP static	0.0135.	0.0081	-0.1314*	0.0103***	-0.0324***	-0.0033	0.0011
IMP	0.0135.	0.0081	-0.1314**	0.0162***	-0.0318***	-0.0017	0.0037
N obs.	10,176	5,688	4,488	25,824	21,624	10,224	2,928
N ind. FE	424	237	187	1,076	901	426	122

Note: This table reports estimates of 'ATT' derived with key estimators based on subsamples of plots by tenure and subsamples with (Panel A), and without (Panel B.) never-allocated land. Rows 'N obs.' and 'N of FE' report the number of observations and fixed effects for each sample which is the same for each estimator in the same column. Heteroscedasticity robust standard errors clustered at plot level are used to estimate significance levels but not reported

Statistical significance levels are: '***' p-value < 0.001, '**' p-value < 0.01, '*' p-value < 0.05, p-value < '.' 0.1, and ' ' p-value >= 0.1.

Source: own calculations.

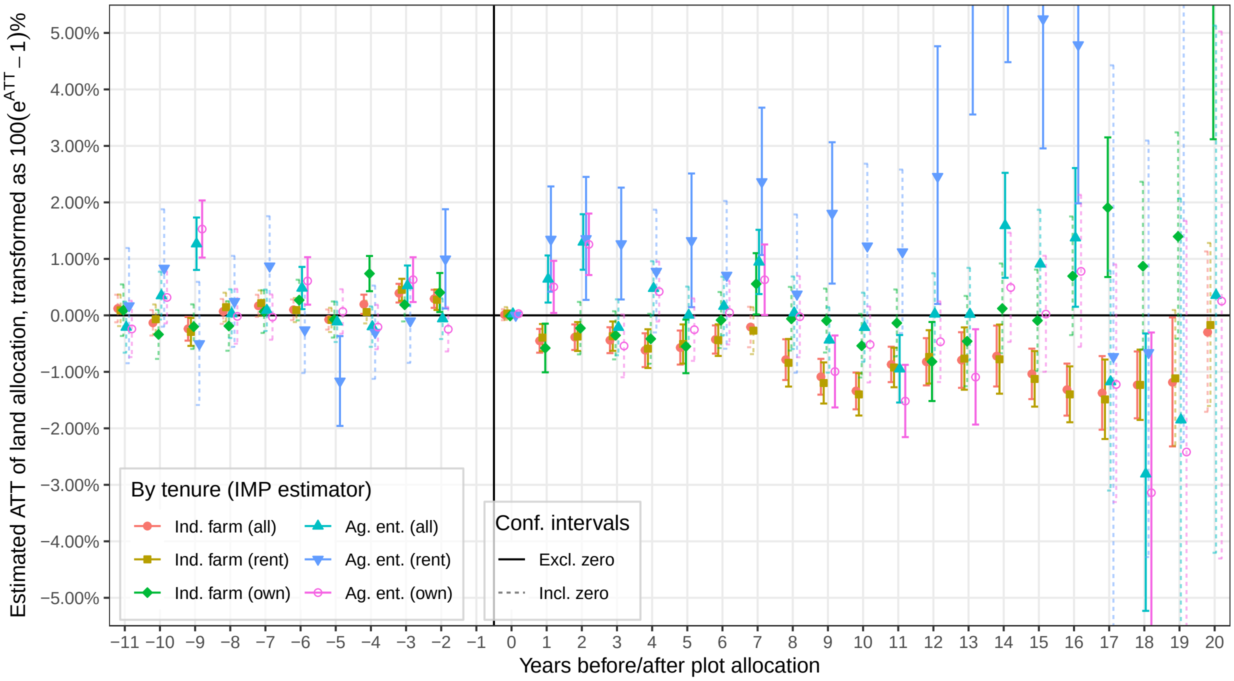

Figure H1: Event-study by key tenure categories (IMP estimator)

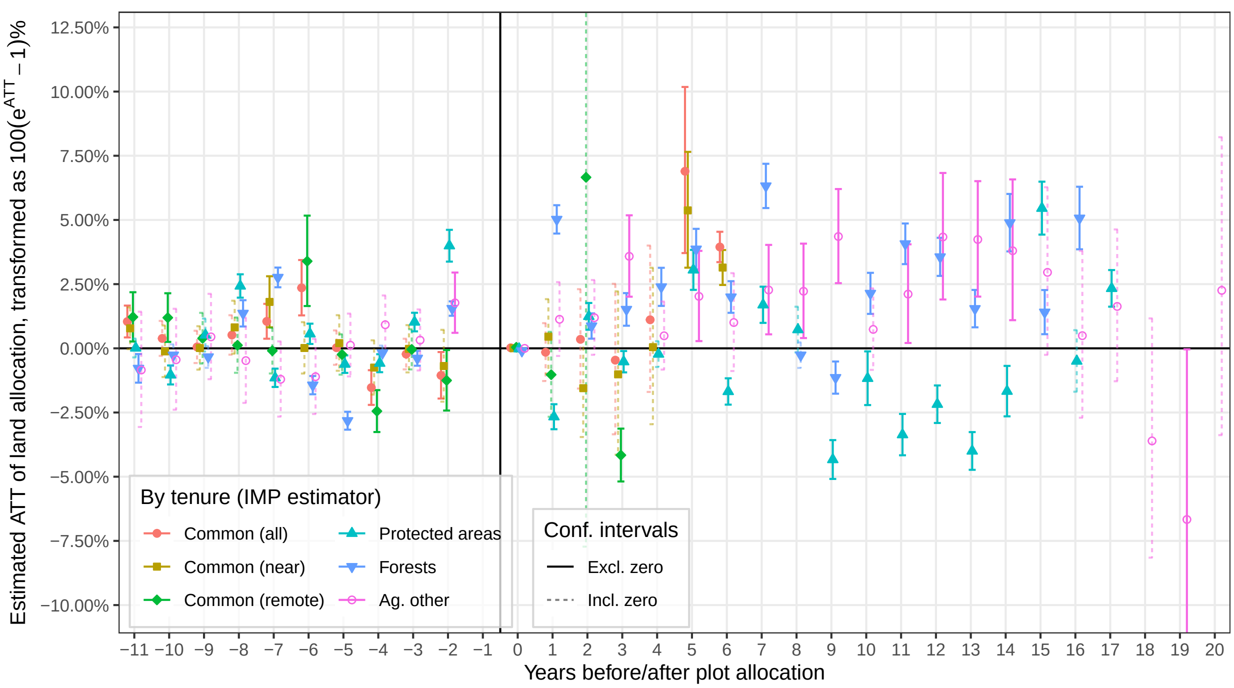

Figure H2: Event-study by minor tenure categories (IMP estimator)

Table H2 also presents an additional land cover type (Hay field) that is not used in estimating main regression results.

Table H2: ATT estimates by land cover (pasture types) using SA estimator

Estimator	Psture	Psture on slope	Hay field
Panel A. Full sample			
BM static	-0.0025***	-0.0007	0.0051***
BM	-0.0022*	-0.0047**	0.0047*
SA	-0.0028*	-0.0053**	0.0017
IMP static	-0.0048***	-0.0042**	0.0014
IMP	-0.0029**	-0.0042***	-0.0017
N obs.	429,528	136,152	82,776
Panel B. Excl. never-allocated			
BM static	-0.0038***	0.0006	0.0028*
BM	-0.0037**	0.0098***	0.0003
SA	-0.0059***	0.0021	-0.0029
IMP static	-0.0154***	-0.0062***	-0.0063*
IMP	-0.0211***	-0.0093***	-0.0148***
N obs.	286,008	94,080	64,728

Note: This table reports estimates of 'ATT' made using various estimators on the subsamples of plots by land cover and without never-allocated land. Heteroscedasticity robust standard errors clustered at plot level are reported in parentheses. Each row reports ATT estimates by subsample: BM - benchmark two ways fixed effect model; SA (Sun and Abraham, 2021), IMP imputation estimators (Gardner et. al., 2022).

Statistical significance levels are: '***' p-value < 0.001, '**' p-value < 0.01, '*' p-value < 0.05, p-value < '.' 0.1, and ' ' p-value >= 0.1.
Source: own calculations.

I.9 Auxiliary results for regression analysis with spillovers

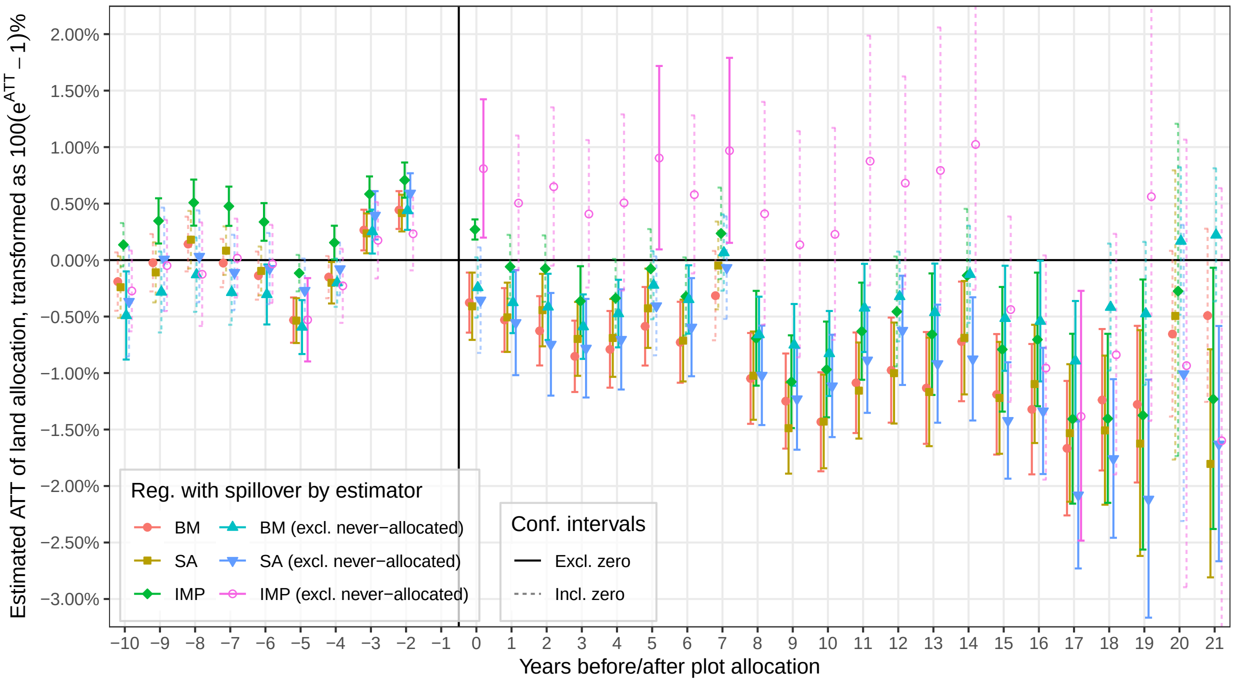

Figure I1: Event-study with spillovers

Table I1: ATT and spillover effects by minor tenure categories

	Common	Common (remote)	Common (near)	Fores	Protected	Ag. other
Panel: Panel A. Full sample						
ATT	0.1750**	0.3192***	-0.0271	0.1926***	0.0849***	-0.0169*
Spillover on allocated	-0.0077***	-0.0051**	-0.0118***	-0.0084***	-0.0076***	-0.0072***
Spillover on unallocated	-0.0075***	-0.0047**	-0.0119***	-0.0069***	-0.0071***	-0.0078***
N obs.	194,688	97,680	97,008	197,760	196,560	212,662
N ind. FE	8,112	4,070	4,042	8,240	8,190	8,861
Panel: Panel B. Excl. never-allocated						
ATT	0.0853	0.0712	0.1381***	0.0116	0.0940***	-0.0308**
Spillover on allocated	-0.0168.	-0.0314.	-0.0288*	0.0021	0.0123*	-0.0047
Spillover on unallocated	-0.0134	-0.0291	-0.0328*	0.0265***	0.0277***	-0.0103.
N obs.	9,096	4,584	4,512	12,168	10,968	27,070
N ind. FE	379	191	188	507	457	1,128

Note: The table reports results estimated using the SA estimator. Row ’ATT’ reports the average treatment effect on the treated. Rows ’Spill on allocated’ and ’Spill on unallocated’ report the magnitude of corresponding spillover effects. Rows ’N obs.’ and ’N of FE’ report the number of observations and fixed effects for each sub-sample. Heteroscedasticity robust standard errors clustered at plot level are in parentheses.

Statistical significance levels are: ‘***’ p-value < 0.001, ‘**’ p-value < 0.01, ‘*’ p-value < 0.05, p-value < ‘.’ 0.1, and ‘ ’ p-value >= 0.1.

Source: own calculations.