Journal of Development Economics 178 (2026) 103600

Contents lists available at ScienceDirect

Journal of Development Economics

journal homepage: www.elsevier.com/locate/devec

Regular Article

Agricultural technology adoption and deforestation: Evidence from a randomized control trial☆

Jeffrey R. Bloem[a], Clark Lundberg[b,*]

[a] International Food Policy Research Institute, United States of America
[b] Department of Economics, San Diego State University, United States of America

ARTICLE INFO

JEL classification:
O13
Q15
Q23
Q16
Q56
Q57

Keywords:
Deforestation
Agricultural technology adoption
Earth observation data
Nigeria

ABSTRACT

We study the effect of the adoption of improved agricultural inputs on deforestation using a randomized control trial in Nigeria which introduced a more efficient and environmentally-friendly nitrogen fertilizer. We combine survey data from the intervention with earth observation data to develop a generalizable method for evaluating the effects of cluster-level interventions on landscape-level outcomes. We find evidence of an intensification response to treatment exposure that reflects significant heterogeneity across land cover. On land with relatively sparse pre-intervention tree cover, treatment exposure increased deforestation while in denser forest areas the intervention reduced deforestation. We find corresponding effects showing treatment exposure increases agricultural productivity. Our results reflect an intensification response to improved agricultural technology that redirects agricultural activity away from forests and towards existing cropland.

1. Introduction

Deforestation is a leading contributor to carbon emissions globally, especially in low- and middle-income countries (World Bank, 2010), and the expansion of agricultural land is the leading contributor to deforestation in tropical areas around the world (Gibbs et al., 2010). Roughly one-third of the world's forests have been lost since the advent of agriculture, with half of this loss occurring between 1990 and 2020 (Williams, 2003; FAO, 2022). In sub-Saharan Africa, roughly 75 percent of agricultural production growth between 2000 and 2018 came from the expansion of farmland while only 25 percent came from improving agricultural yields (Jayne and Sanchez, 2021). These stylized facts present a "triple challenge" for agricultural systems around the world to (i) extend a sufficient, safe, and nutritious food supply to the roughly two billion people experiencing food insecurity, (ii) reduce poverty and improve livelihoods for the over 500 million people working within agricultural value chains, and (iii) limit environmental damage, forest and habitat loss, and greenhouse gas emissions (OECD, 2021). Environmentally-friendly agricultural technologies that improve productivity while reducing environmental externalities represent a possible pathway to address this triple challenge (Lipper et al., 2014; Phalan et al., 2016; Hill et al., 2024).

Despite the first-order benefits of environmentally-friendly agricultural technologies, however, the effects of the adoption of these technologies on deforestation is *ex ante* ambiguous (Green et al., 2005; Takasaki, 2006; Phalan et al., 2016; Villoria et al., 2014; Szerman et al., 2022). Improved agricultural technologies can increase the relative value of agricultural land and induce farmers to expand their farms into forested lands—an effect referred to as Jevons' paradox (Alcott, 2005). Conversely, "Borlaug's hypothesis" predicts that the adoption of agricultural technologies will lead to agricultural intensification, easing land conversion pressures on existing forests (Borlaug, 2007). The relationship between agricultural technology adoption and deforestation is critical in crafting policies that balance food security and local economic development against local and global environmental externalities. If agricultural technology adoption leads to deforestation, then current levels of spending on agricultural research and development might need to be adjusted to internalize harmful external costs to the environment. If agricultural technological progress maintains

☆ We are grateful for constructive comments from two anonymous referees and Ariel BenYishay, our editor at the *Journal of Development Economics*. We also thank Ian Luby for providing insights about the forest conservation environment in Nigeria. This work has also benefited from the support of CGIAR's Science Program on Policy Innovations. We are grateful to all funders who supported this research through their contributions to the CGIARTrustFund. Authors are listed in alphabetical order.

* Corresponding author.
E-mail addresses: j.r.bloem@cgiar.org (J.R. Bloem), clundberg@sdsu.edu (C. Lundberg).

https://doi.org/10.1016/j.jdeveco.2025.103600
Received 18 June 2024; Received in revised form 11 July 2025; Accepted 19 July 2025
Available online 31 July 2025

canopy cover or even reduces deforestation, however, then agricultural research and efforts to promote the adoption of improved technologies might be an efficient and effective way to address this triple challenge. Moreover, understanding the relationship between agriculture and deforestation carries important implications about the costs and benefits of conservation, which influences optimal forest conservation policy (Joppa and Pfaff, 2009; Frank and Schlenker, 2016; Luby et al., 2022).

We study the relationship between agricultural technology adoption and deforestation using data from a randomized control trial (RCT) in Nigeria that promoted the adoption of urea super granules (USG), an improved and environmentally-friendly fertilizer, among rice farmers. USG leads to first-order agricultural productivity and environmental benefits by reducing nitrogen losses which can lead to improved crop yields and reduced leaching of nitrogen into adjacent ground and surface water and can limit the excessive growth of algae (Ho et al., 2019). In the RCT, rice farmers in treated villages received information, a readily available supply of USG, and price discounts on their purchase of USG which led to the significant adoption of USG (Liverpool-Tasie et al., 2025). We consider treatment effects on deforestation using earth observation data on forest cover and agricultural productivity between 2009 and 2021 that we link to data collected as part of the RCT intervention collected in 2014 and 2015.

Linking landscape-level earth observation data to an experimental intervention randomized at the village-level presents a practical challenge (Jack and Walker, 2024). Common approaches can lead to measurement error and, especially in settings where the intervention involves an information campaign, this can generate biased effect estimates if information spills over from treatment villages into control villages. To address this challenge, we develop a novel method for evaluating the effects of a cluster-level intervention on spatially explicit, landscape-level outcomes. Using household survey data collected in conjunction with the RCT, we construct a spatially explicit measure of exposure to the experimental intervention that incorporates the possibility of spatial diffusion of information. This approach works well in our study setting and is generalizable to other settings where researchers are interested in estimating the effects of cluster-level interventions on landscape-level outcomes using earth observation data. Our approach offers a tractable methodology for conducting impact evaluations using earth observation data but, perhaps more importantly, also offers opportunities for far more refined insights into land-use responses to development interventions relative to existing approaches.

We merge earth observation data on forest cover to household survey data and estimate rates of deforestation in areas exposed to the experimental intervention. We find significant landscape-level heterogeneity that suggests an intensification response. In areas with less dense canopy cover — as is typical of land in, or adjacent to, agricultural production in the region — exposure to the experimental intervention increases tree canopy loss, whereas in areas with more dense canopy cover exposure decreases tree canopy loss. We also document positive productivity effects in areas exposed to the experimental intervention. Consistent with the findings of Liverpool-Tasie et al. (2025), and with models of learning and technological diffusion, we observe a lag in the timing of agricultural productivity effects. Together, these findings suggest that rising agricultural productivity likely mediates the relationship between agricultural technology adoption and changes in forest cover (Phalan et al., 2016). Our spatially-explicit approach uniquely allows us to identify this type of heterogeneous intensification response that might otherwise be obscured in more aggregated analyses. Indeed, *on average*, we find no difference in canopy loss in areas more exposed to the intervention relative to areas less exposed to the intervention. Our results contribute to emerging insights on the set of conditions under which gains in agricultural productivity — driven by the adoption of improved technologies — lead to increased or decreased pressure on land use in forested areas (Szerman et al., 2022).

Our work in this paper joins a classic literature studying the effect of agricultural technological progress on deforestation (see, e.g., Villoria et al., 2014, for a more extensive, albeit slightly dated, review of this literature). Early contributions use cross-sectional data (Yanggen and Reardon, 2001; Fisher and Shively, 2007; Deininger and Minten, 1999; Pelletier et al., 2020) or cross-country data (Ceddia et al., 2013; Rudel et al., 2009; Ewers et al., 2009; Bulte et al., 2007) with mixed findings. Improving on the ability to account for sub-national time-invariant confounding factors with panel data, Foster and Rosenzweig (2003) combine earth observation data and village-level surveys from India to study the effects of agricultural technological change and deforestation, finding that increased crop productivity is associated with an increase in cultivated area, a reduction in the proportion of area forested, and a decrease in forest density.[1] More recently, researchers aim to isolate plausibly exogenous variation in the introduction of improved technologies. For example, in their study of the broad effects of the Green Revolution, Gollin et al. (2021) track the diffusion of high-yielding varieties of seeds using an approach akin to a shift-share instrumental variable and find that the adoption of improved agricultural technologies is associated with a reduction in the amount of land devoted to agriculture. Other studies using quasi-experimental research designs continue to find mixed results, with Abman and Carney (2020), Abman et al. (2020) and Szerman et al. (2022) all finding evidence indicating that technology adoption and increased agricultural productivity lead to reductions in deforestation, but (Carreira et al., 2023) finding evidence of increased deforestation and cropland expansion due to the adoption of improved agricultural technologies.[2]

Our paper is most closely related to Bernard et al. (2023) who combine data from a randomized control trial in the Democratic Republic of the Congo that promoted the adoption of improved seeds with earth observation data measuring deforestation. Although the authors find no evidence of deforestation using earth observation data, these estimates are sufficiently noisy and statistically under-powered, such that although they observe meaningful reductions in deforestation on average, these estimates are statistically indistinguishable from a null effect. We build on this previous work by developing a generalizable method for evaluating cluster-level interventions on landscape-level outcomes. This method helps address practical complications of linking data with different units of observations, such as issues related to limited statistical power, uncovering granular landscape-level heterogeneity, and defining a spatially explicit measure of treatment exposure. Our use of earth observation data measuring deforestation around an RCT is also closely related to Jack and Walker (2024), who document practical guidance about integrating remotely-sensed earth observation data with RCTs.

We make three core contributions to this literature. First, while previous research conducts analysis at the cluster (i.e., village) level, we conduct our analysis at the landscape level, i.e., spatially explicit grid cells or pixels. This is useful for at least four reasons: (i) village-level analysis can be under-powered and lead to limitations relating to statistical inference, as is the case for Bernard et al. (2023), (ii) conducting analysis at the pixel level allows us to construct a spatially explicit and continuous measure of exposure to the experimental intervention based on the proximity of a pixel to one or more treatment village

[1] Other studies also use panel data and results remain mixed (Koch et al., 2019; Caviglia-Harris, 2018). Notably, Maertens et al. (2006) construct a pseudo-panel using data from Indonesia and find that the relationship depends on the type of agricultural technology. Although the adoption of irrigation technology is associated with a reduction in deforestation, the use of tractors encourages agricultural land expansion and deforestation.

[2] Our work is also related to the long-standing debate about "land-sharing" vs. "land-sparing" policy strategies which often frame efforts to promote agricultural productivity to be in tension with efforts to protect nature and the environment (Phalan et al., 2011; Kremen and Merenlender, 2018; Mertz and Mertens, 2017).

centroids, (iii) pixel-level analysis enables an assessment of granular landscape-level heterogeneity, and (iv) common approaches used to aggregate landscape-level data to match cluster-level interventions can lead to biased effect estimates if information diffuses geo-spatially and spills over between treatment and control villages. This is an important methodological contribution, especially in settings (such as ours) where the agricultural technology plausibly could lead to an intensification response indicated by granular landscape-level heterogeneous effects.

Second, similar to the work of Bernard et al. (2023), we leverage a RCT, which allows us to address possible sources of unobserved heterogeneity that might persist in existing observational and quasi-experimental studies in this literature. Moreover, the randomized design of the intervention allows us to estimate credibly causal effects insulated from omitted or unobservable confounding factors. Additionally, non-classical measurement error can persist in earth observation data and generate biased treatment effect estimates when treatment status is correlated with factors that contribute to errors in measurement (Alix-García and Millimet, 2023). Our randomized treatment helps buffer against the presence of this source of bias in our results.

Finally, we add important insights on landscape-level heterogeneity in the relationship between the adoption of improved agricultural technology and deforestation. This is important because the theoretical predictions on this relationship are highly dependent on features of the geographic and contextual environment (i.e., the functioning of local factor markets, the elasticity of demand in local output markets, etc.). We demonstrate that the relationship between agricultural technology adoption and deforestation can hinge critically on pre-intervention forest density. Notably, geographically aggregated analyses (e.g., at the village level) may fail to fully capture these granular spatial effects. We speculate that such spatial heterogeneity in treatment effects may be a contributing factor in the mixed findings in the literature, as discussed above.

The remainder of this paper is organized as follows. In Section 2 we discuss the study design and the implementation of the RCT promoting the adoption of improved fertilizer among rice farmers in Nigeria. In Section 3 we discuss our approach linking earth observation data at the landscape-level to village-level data from the RCT. We present our main results on deforestation in Section 4. In Section 5 we explore effects of treatment exposure on agricultural productivity, which represents a plausible mechanism for the relationship between agricultural technology adoption and deforestation. Finally, in Section 6 we conclude with a discussion of our results and aim to motivate future research in this literature.

2. Background and experimental intervention

The experimental intervention promoted the adoption of improved urea fertilizer among rice farmers in Kwara State, Nigeria. Urea provides nitrogen to plants — a critical nutrient for plant growth — but often leads to environmental damages when nitrogen leaches into surrounding water sources or is released into the atmosphere as nitrous oxide (Bowles et al., 2018; Ghosh and Bhat, 1998; Cameron et al., 2013). The traditional form of urea fertilizer, called prilled urea, is commonly broadcast directly on the surface of plots. This soil surface application method limits the amount of nitrogen taken up through crop roots (Dobermann, 2005) and is associated with both low rice yields (Liu et al., 2010) and potential environmental damages when unabsorbed fertilizer runs off the plot (Alam et al., 2023; Kamai et al., 2020).

The intervention introduced urea super granules (USG), an improved form of urea fertilizer that increases nitrogen delivery to plants while reducing nitrogen runoff and associated environmental pollution. Granules of USG — which are much larger than traditional prilled urea — are applied deep into the soil next to the root of the plant. The use of USG with this application method requires less total urea, and agronomic trials show evidence of increases in rice yield of between 15 to 25 percent (Lupin et al., 1983; Thomas and Prasad, 1987; Ahmed et al., 2000; Jena et al., 2003; Kabir et al., 2009; Islam et al., 2012). Prior to the intervention, production constraints limited the widespread adoption of USG. The production of USG requires a relatively expensive briquetting machine to convert prilled urea into super granules that was not widely available in Nigeria. In years prior to the implementation of the intervention, however, several private agricultural input supply firms developed a cost-effective production line for briquetting, packaging, and shipping USG to the market. For the purposes of the RCT, Liverpool-Tasie et al. (2025) partnered with one of the input supply firms producing and distributing USG in Nigeria to promote the adoption of USG among rice farmers in Kwara State.

One of the largest states in terms of land area, Kwara State in western Nigeria is one of the country's least densely populated states with ecosystems consisting mostly of either wooded savanna or forested areas. Agriculture is the primary source of income for many households with rice, maize, sorghum, and other cash crops representing the most important crops. Much of the state's northeast border follows the Niger river, which flows into Lake Jeba, and the northwest border includes a part of the Kainji National Park. Several forest reserves effectively divide Kwara state in half.[3] The experimental intervention takes place entirely in the northeast part of the state outside of the forest reserves, in an area close to the Niger river.

The experimental intervention randomly assigns villages to receive the standard marketing package used by a local private agricultural input supply company. This marketing package includes an information campaign, a demonstration plot, and a readily-available supply of USG through a local retailer. Demonstration plots specifically highlight productivity differences in plots using USG with the deep placement application method and plots using traditional practices. Field days allow for representatives of the private agricultural input supply company to present the optimal use of USG and demonstrate its benefits. Within treatment villages, a randomly-selected subset of farmers receive a voucher providing them with a 25 percent discount on their purchase of USG. The marketing package aims to address knowledge constraints associated with the urea deep placement application method.[4]

Data for this RCT was collected from a randomly selected set of 45 villages within two major rice-producing local government areas (LGAs) in Kwara State. Within those two LGAs, Liverpool-Tasie et al. (2025) created an initial list of 60 villages, each with at least 40 rice producing households to construct a sampling frame to enable the random selection of the 45 study villages. 30 villages were randomly assigned to be treatment villages. Within these treatment villages roughly half of the rice producing households received a 25 percent discount voucher to use when purchasing USG. This allows the remaining 15 villages to serve as control villages. A baseline survey was conducted in February 2014, which involved a standard multi-topic household survey instrument capturing household socio-economic and demographic characteristics, agricultural production (i.e., practices, inputs, and labor use, harvest yield), as well as economic well-being indicators (i.e., income, expenditures, and food security). After the completion of the

[3] In Nigeria, there is an important distinction between "forest reserves", which are used as a managed stock of forest area for sustainable forest-related product production and "conservation areas", which include national parks and game/wildlife sanctuaries. Additionally, Amusa (2024) notes that Nigeria's laws around forest protection are outdated and not officially backed by any legal code or act. This complicates, and limits, the enforcement of deforestation in areas categorized in ways that would provide protection in other legal environments.

[4] The optimal use of USG requires a specific application technique (i.e., placing a handful of USG 5–6 centimeters deep in the ground between four rice plants) and the adoption of a set of recommended practices including the need to use the improved variety of rice seeds, to apply NPK at the time of transplanting the rice plant, to apply USG one week later, to release irrigation water 2–3 days after USG application, and the need for frequent irrigation.

baseline survey the treatment implementation began during the pre-planting and planting seasons and endline data were collected a year later between April and May 2015.

Each of the households in the survey data farmed rice and almost all households had a male household head. Households, on average, include about three children and three adults. In addition to farming rice, households also cultivate maize and sorghum and other cash crops on between two and three agricultural plots distributed across about four hectares of land. At baseline none of the households use USG and roughly 40 percent use prilled urea. These rates do not differ across treatment status. Table A.1 in the Supplemental Appendix shows pre-intervention balance on important indicators of land use, forest cover, and forest loss.[5] Finally, households hold property rights over plots and cultivate the same plot for multiple consecutive years. Figure A.1 in the Supplemental Appendix plots the distribution of the number of years a household has cultivated a plot according to the household survey data. On average, households have cultivated a given plot for 12 years and only one plot in our data has been cultivated by a household for less than one year. Moreover, according to our household survey data, 84 percent of plots were inherited, six percent were rented, five percent were received as a gift, about one percent were purchased, less than one percent were sharecropped, the remaining less than one percent are other sources which include “free use” and “self acquired.”

Using these two waves of household surveys, collected in conjunction with the RCT, we see that the intervention led to a strong and statistically significant 28 percentage point increase in the use of USG one year after treatment, effectively establishing a strong “first stage” effect of the experimental intervention. The treatment, however, also led to significant reductions in the number of rice plots as well as total plot area with a corresponding decline in agricultural work in the year following the intervention. The treatment does not appear to meaningfully influence forest-product activity, such as firewood collection or timber harvest. We expand upon on these stylized household findings in the Supplemental Appendix, including a thorough description of the data, regression specifications, and tabular results presented in Tables A.2, A.3, A.4, and A.5. Overall, evidence from the household survey suggests that access to the improved agricultural technology may have facilitated an intensification response by treated households—adoption of the improved fertilizer along with a reduction in production at the extensive margin.

Although these results from the household survey are informative, they are also limited. While these findings do not appear to be consistent with the experimental intervention contributing to deforestation, the household survey data do not allow us to directly examine forest cover and these data only provide information from one point in time a year after the promotion of USG fertilizer began. Therefore, effects on forest cover could persist via other channels not measured in the household survey data or due to lagged effects that materialize after the endline survey. This motivates the use of earth observation data which allow us to explicitly consider the effects of the intervention of forest cover as well as dynamic effects on agricultural productivity.

3. Impact evaluation with earth observation data

In many settings, linking earth observation data with data from a RCT is not straightforward; our setting is no exception. As discussed above, the experimental intervention varies at the village level and our earth observation data is available at the pixel level. Each pixel is a 30-meter square and thus, as is the case in many experimental settings, the unit of randomization is not the same as the landscape-level earth observation unit of analysis. As discussed by Jack and Walker (2024), geo-spatial diffusion of information and/or marketing treatments represents a complicating factor, as it is difficult to cleanly define boundaries characterizing the diffusion of information, and diffusion might be mediated by village-level characteristics (i.e., transportation infrastructure, ecological factors, etc.). A common approach is to draw circles around village centroids and aggregate the earth observation data within each circle (Wilebore and Coomes, 2016; Jayachandran et al., 2017; Edwards et al., 2022). This approach can lead to measurement error by (i) coding untreated areas as treated and/or (ii) coding treated areas as untreated. If villages are sufficiently spatially disbursed, this measurement error is classical and contributes noise to estimated effects. If treatment and control villages are relatively close in geographic proximity, however, then this measurement error can also lead to biased treatment effect estimates.

In our study setting, villages are indeed relatively close in geographic proximity. Fig. 1 shows the spatial distribution of treatment and control villages where we plot buffer regions around village centroids based on the 75th percentile of reported distance between households and rice plots as measured in the baseline household survey data.[6] As distances between houses and rice plots vary across villages, the size of these circles also vary by village. This approach shows considerable overlap between village plot buffers. In many cases, we observe significant overlap between treatment and control village buffers. Furthermore, there are other settlements in the region that were not included in the RCT. Fig. 2 illustrates this feature of our study region, as well as the fact that land use is spatially continuous between settlement areas and cannot obviously be attributed to a particular settlement area. This presents a challenge in the task of aggregating our deforestation data from the pixel level to the village level. Therefore, rather than implement our analysis at the village level, we conduct our analysis estimating possible deforestation effects at the pixel level. Conducting analysis at the pixel level requires that we develop a method for defining treatment exposure for each pixel in our study area, which we describe in the following sub-section.

3.1. Defining treatment exposure

We now turn to developing a framework for evaluating the effects of a cluster-level intervention on landscape-level, spatially explicit outcomes. One of the central challenges of program evaluation in this context is assigning treatment status, or treatment exposure intensity, to particular land units. In an idealized experimental setting, we would know with certainty if a pixel is managed by a resident of a treated village. In our setting — and indeed in most experimental and quasi-experimental research settings — this is not observable (Jack and Walker, 2024). In limited circumstances, this challenge may be circumvented by an experimental design that targets particular plots of land with known boundaries. Even in that special case, however, it is impossible to know where plot expansion, or new plot development, might take place. Therefore, in general, the researcher cannot geo-locate future agricultural expansion attributable to households or villages that receive treatment.[7]

[5] Additionally, see Liverpool-Tasie et al. (2025) for more summary statistics about the households included in the survey data and a comprehensive set of balance tests that support the validity of the random assignment of this experiment.

[6] The household survey collected estimates of travel time in minutes between a respondent’s household and each rice plot. We convert these travel time estimates into geographic distance estimates based on a walking pace of 1.4 m/s. We discuss our use of these distance measures in more detail in the next sub-section.

[7] Another possible complicating factor is communal land ownership, where households might cultivate different plots in different agricultural seasons. In principle, communal plot ownership adds an additional layer of complexity especially in cases where the boundaries of a “community” overlap with treatment/control boundaries. In our setting, as discussed above, communal plot ownership does not seem to be common.

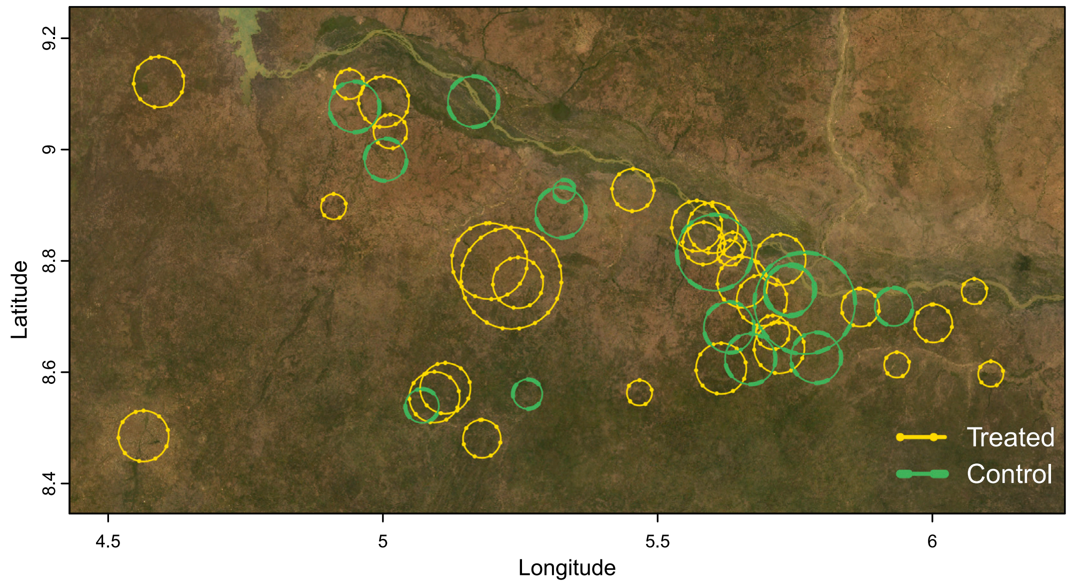

Fig. 1. Distance to rice plots by village treatment status.
Notes: This figure shows the spatial distribution of RCT villages in Kwara state, Nigeria. The circles correspond to buffers around sample village centroids with the radius determined by the 75th percentiles of distance in meters between households and rice plots, based on responses from the household survey.

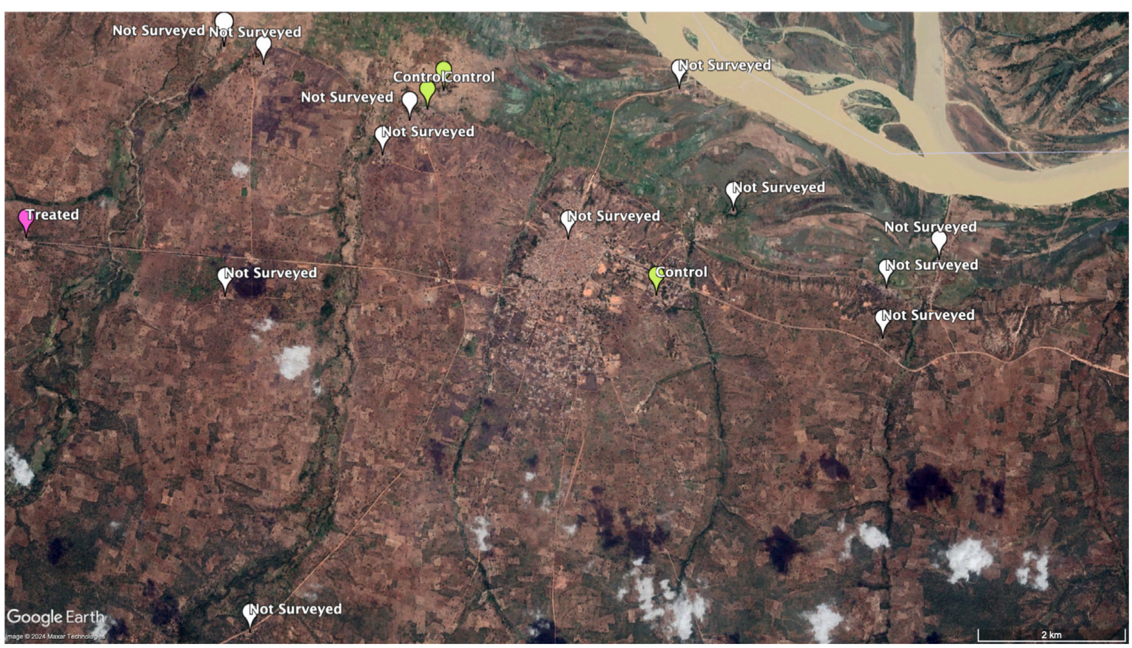

Fig. 2. Example of settlement density and land use contiguity.
Notes: This figure provides an example of settlement distribution and contiguous land use in Kwara State, Nigeria. Treated and control villages are identified using data from Liverpool-Tasie et al. (2025). Out of sample settlements identified by the authors through visual inspection. Imagery: Google, ©Maxar Technologies.

Similar to many other household survey instruments conducted in rural areas of low- and middle-income countries, our household survey data includes estimated travel times between the household and agricultural plots.[8] We use this travel time information, specifically focusing on rice plots, to construct a spatially explicit measure of treatment exposure at the pixel-level for each treated village. These plot-level travel times are recorded in walking minutes which we convert to distances using a pace of 1.4 m/s (Murtagh et al., 2021).

[8] As with any self-reported data, concern persists about the influence of measurement error. With measures of travel times recorded in both the baseline and endline surveys, we can assess the potential for measurement error by comparing responses across survey rounds. Although these measures are tightly correlated between baseline and endline (p-value = 0.001), there are differences. 15 percent of plots have the same reported difference at both baseline and endline and 85 percent have differences. We calculate the difference in responses about travel times to plots between baseline and endline and scale this difference by the travel time reported at baseline to generate a measure of relative difference. The median relative difference in our data is zero and is not correlated with treatment status. So, while some measurement error persists in this variable, as we would expect with self-reported survey data, this measurement error does not seem to be substantive or a meaningful source of bias.

For parsimony, we model distances to plot p in village v (D_{pv}) as village-level gamma distributions:

$$f(D_{pv}) = \frac{D_{pv}^{k_v-1} e^{-D_{pv}/\theta_v}}{\theta_v^{k_v} \Gamma(k_v)} \tag{1}$$

with village-specific parameters measuring shape and scale, k_v and θ_v respectively, which we fit via method of moments:

$$\hat{k}_v = \frac{\left(\frac{1}{N_v}\sum_p D_{pv}\right)^2}{\frac{1}{N_v}\sum_p \left(D_{pv} - \bar{D}_v\right)^2} \tag{2}$$

$$\hat{\theta}_v = \frac{\frac{1}{N_v}\sum_p \left(D_{pv} - \bar{D}_v\right)^2}{\frac{1}{N_v}\sum_p D_{pv}} \tag{3}$$

In our setting, these plot distances are reasonably well-described by gamma distributions which we illustrate with empirical cumulative distribution functions (CDFs) at the village-level and quantile–quantile (Q-Q) plots by treatment status in Figure A.2 in the Supplemental Appendix. We note that travel time responses are frequently discretized and censored in the survey data, as evident in the Q-Q plots.[9]

Next, we define probabilistic measures of pixel i's exposure to treatment in village v based on the fitted gamma cumulative distribution function (CDF):

$$Pr(exposure_{iv}) = 1 - F_v(d_{iv}) \tag{4}$$

where d_{iv} is the distance between pixel i and the centroid of village v, and $F_v(d_{iv})$ is the fitted gamma CDF for village v. This describes the probability that a pixel is attributable to a treated village and hence exposed to treatment.

The exercise so far generates a separate probabilistic exposure surface for every treated village. Given the considerable overlap in travel times from village centroids, however, a pixel could be covered by more than one village treatment exposure surface. This overlapping exposure captures a higher probability that the pixel is attributable to a household in a treatment village while also capturing second-order treatment exposure. For example, overlapping exposures increase the likelihood that treatment uptake might diffuse through local peer effects—in our setting, a pixel with multiple overlapping exposures but not managed by a household in a treatment village is still exposed to treatment because they might visually observe uptake of the technology on other plots.[10] To construct a measure of total exposure intensity for pixel i, we sum over village-level exposures:

$$Pr(exposure_i) = \sum_v Pr(exposure_{iv}) \tag{5}$$

where $Pr(exposure_{iv})$ is defined in Eq. (4).

Finally, we define our study region as the set of pixels that lie within 15 km of the centroid of any village in the RCT—including both treatment and control villages. Notably, this will include a number of other untreated villages that were not included in the RCT design as illustrated by Fig. 2. Fig. 3 presents the spatial distribution of baseline tree cover in our sample region (in Panel A) and our treatment exposure measure (in Panel B). Figure A.3, in the Supplemental Appendix, plots the spatial distribution of the pixel grid cells where tree canopy density fell to zero (i.e., is deforested) between 2009 and 2021.

A few remarks about how our approach to defining treatment exposure at the pixel level compares to the common approach of drawing circles around village centroids will be useful in highlighting the applicability of our approach in our study setting and the utility of our approach for future research. First, the common approach in the literature is to draw circles around village centroids and aggregate earth observation data within each circle, therefore enabling analysis at the village level (Jack and Walker, 2024). In many settings, including ours, there is uncertainty about the appropriate size of the circle around each village centroid.[11] The way researchers typically deal with this type of uncertainty in the literature is to vary the length of the radius around each village centroid. Researchers then assess how effect estimates change (or remain stable) for various radii. Second, this common approach risks aggregating over, and obscuring, critical heterogeneous effects. Opportunities to identify heterogeneous treatment effects using village circle buffers are limited. The most common solution subsets land area within a village buffer by some discrete heterogeneity dimension and then aggregates earth observation data within village subcategories to create multiple separate outcomes at the village level. In addition to constraints on the degree of heterogeneity identifiable in this approach, inference of any heterogeneous effects can be confounded by multiple hypothesis testing with these multiple, correlated outcomes. In particular, if an intensification response to the technology is possible, then it is critical that an analytical approach is designed in a way to be able to detect this type of heterogeneity. Notably, intensification could increase pressure on tree canopy loss in areas on or adjacent to cropland and reduce this pressure in more densely forested areas.

Our approach, which defines treatment exposure as a continuous function of travel times between households and agricultural plots, is effectively a spatially explicit and continuous extension of the common approach found in the literature. That is, rather than assess effects based on a small and discrete set of estimates determined by (perhaps arbitrary) radii lengths defining circles around village centroids, we explicitly define a continuous measure of treatment exposure that fully incorporates the potential for treatment diffusion across space. Given the landscape-level unit of observation, moreover, our approach is also well-equipped to detect effects consistent with an intensification response. Therefore, our approach can be viewed as a logical extrapolation of the common approach and carries with it the additional benefit of avoiding issues related to measurement error and biased estimates due to treatment diffusion.

4. Deforestation

Land cover in our study region is dominated by wooded savanna and forested areas, with agricultural plots frequently co-located with or adjacent to areas of sparse tree canopy coverage (i.e., trees located on plots or plots adjacent to uncultivated wooded savanna). For parsimony, we use deforestation and tree canopy loss interchangeably but emphasize that tree canopy loss in the region is not generally the type of clearcutting of dense tropical or sub-tropical forest seen in other parts of the world. Instead, as in many other parts of sub-Saharan Africa, tree canopy loss frequently arises from the conversion of wooded savanna into rotational agricultural plots. Fig. 4 presents satellite imagery from 2015 and 2021 illustrating tree canopy loss typical of our study region.

[9] Parametric plot distance distributions are not the only viable approach to creating plot distance surfaces. For example, the empirical distance distribution could be used instead but will only create an in-sample surface and with considerably higher computational demands.

[10] We note that in research designs that define treatment based on village or plot boundaries, this sort of local peer effect would represent a violation of the stable unit treatment value assumption (SUTVA) and bias treatment effect estimates.

[11] This is especially the case where there is an information and/or marketing treatment where diffusion of the treatment across space is natural and perhaps even a key feature of the intervention. It is also worth noting, that this challenge persists with individually targeted and non-transferable treatments but where use of the technology could diffuse via, for example, social networks or peer effects.

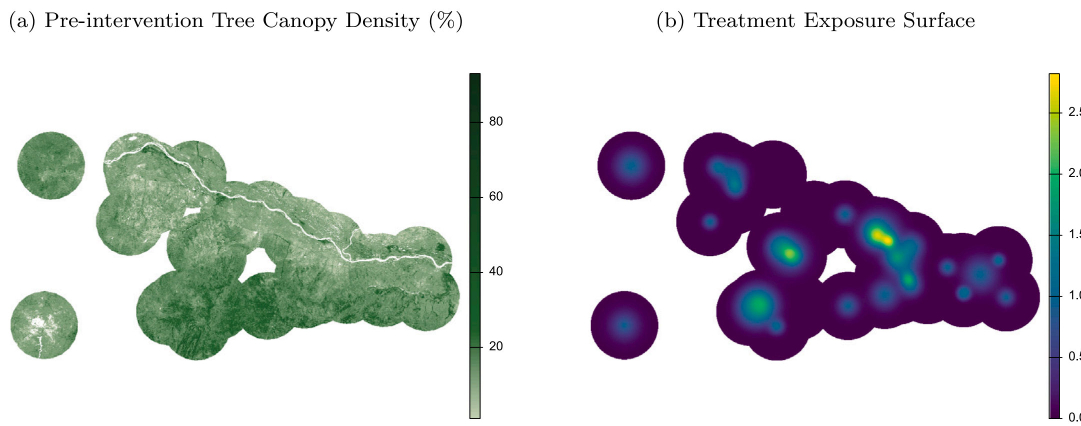

Fig. 3. Study region: Pre-intervention tree canopy cover and treatment exposure.
Notes: This figure illustrates tree canopy density in our study region for the year 2000, using the (Hansen et al., 2013) data, in Panel (a) on the left and our measure of treatment exposure intensity in Panel (b) on the right. The white line seen in the tree canopy density figure in Panel (a) is the Niger River.

Fig. 4. Tree canopy change in Nigeria: 2015–2021.
Notes: This figure presents satellite imagery of a selected area in our study region between 2015 and 2021 exhibiting tree canopy loss typical of the region. Imagery: Google, ©Maxar Technologies.

4.1. Tree canopy loss data

We use earth observation data measuring forest cover change from Hansen et al. (2013).[12] These data provide spatially-explicit estimates of tree cover and annual forest loss at a 30-meter pixel resolution. Estimates of tree cover characterize the density of canopy cover greater than five meters high across a pixel in the year 2000—ranging from zero percent to 100 percent. For any pixel with cover greater than zero percent in the year 2000, the data characterizes forest loss as the year in which tree cover falls to zero. The Hansen et al. (2013) data only provide estimates of forest *loss* at the annual level, and do not assess annual forest gain. As a result, measured forest loss in our data only reflects loss, not net changes in forest cover. Any tree growth in our sample region will not be reflected in our data, nor will the loss of any tree canopy grown since the year 2000. Consequently, loss as defined in the (Hansen et al., 2013) data is observationally irreversible—once a pixel is lost, it remains lost. From the (Hansen et al., 2013) data, we construct an annual panel of binary loss at the pixel level where, for pixels with greater than zero forest cover at baseline, loss is equal to one in the year tree cover is eliminated and zero otherwise. The structure of this panel is analogous to survival data in which units no longer appear in a dataset after a unit experiences an "event" (e.g., death). Recognizing this structure, we censor observations that follow a loss event to construct a panel suitable for survival analysis. Finally, we balance pre- and post-treatment periods by limiting our sample to the years 2009 through 2021.

One potential limitation of our study, and indeed all empirical studies that aim to estimate causal effects of socio-economic interventions on deforestation, is driven by our choice of the level of analysis. The level of analysis (i.e., pixel level, or aggregation to larger areas) can change results and scale choices that are both "too large" and "too small" can lead to bias (Avelino et al., 2016). Garcia and Heilmayr (2024) recommend that researchers should align the structure of their analysis to match the real-world units at which land use decisions are being made. Two issues complicate this decision in practice, as discussed by Avelino et al. (2016): (i) the specific land use decision making unit of analysis is often not precisely known and (ii) there are likely multiple drivers of deforestation that operate at different scales and occur simultaneously. While we ultimately cannot be sure of the precise decision making unit of analysis relevant to our study context and cannot rule-out simultaneous drivers of deforestation operating at different scales, we note the following details about the context of our study. First, our study takes place in a setting where smallholder agriculture is a primary economic activity and the leading driver of deforestation is shifting agriculture at a relatively small scale (Curtis et al., 2018). Second, we are motivated to study low-level heterogeneity in both exposure to the experimental treatment and pre-intervention canopy density levels driven by existing theory suggesting a possible intensification response to the adoption of agricultural technologies such as USG fertilizer. Both of these factors motivate us to conduct our analysis at the pixel level.

[12] We use v1.10 of the (Hansen et al., 2013) global forest change dataset accessed via Google Earth Engine.

4.2. Deforestation estimation

We follow the guidance of Garcia and Heilmayr (2024) and estimate a hazard regression that estimates the effect of exposure to the experimental intervention on the probability of canopy loss. The randomized implementation of the experimental intervention helps ensure our estimated effects are not biased due to spurious correlations embedded in our two-way fixed effects regression approach, which (Garcia and Heilmayr, 2024) show can persist in quasi-experimental studies. Therefore, we can interpret our estimates as the dose response — measured in terms of tree canopy loss — of exposure to the experimental intervention.

To estimate the effect of the experimental intervention (i.e., the introduction and promotion of USG fertilizer), we estimate a two-way fixed effects survival model via pseudo-Poisson maximum likelihood (PPML), specified as follows:

$$Pr(loss)_{pt} = \exp\left(\lambda Pr(exposure_p) \times Post_t + \kappa_p + \pi_t\right) \tag{6}$$

In this equation, p indexes pixels, t indexes years, $Pr(exposure_p)$ is our treatment exposure variable defined in Eq. (5), $Post_t$ is a dummy variable equal to one in years 2015 or later and zero otherwise, κ_p are pixel fixed effects and π_t are year fixed effects.[13] The interaction of our treatment exposure variable and a post-treatment dummy variable creates a dose–response regression specification. Our parameter of interest, λ, identifies the effect of increased exposure to improved fertilizer technology on the speed of canopy loss (i.e., the proportional hazard rate). We report Conley standard errors with 2 km bandwidth.[14]

We also consider the possibility that the landscape-level effects of fertilizer technology vary by land type. In particular, we hypothesize that improved fertilizer access is likely to lead to an intensification of agricultural activity which may relieve pressure to convert more heavily forested land into cropland. To this end, we also estimate heterogeneous treatment effects by pre-intervention tree canopy coverage by interacting our main dose variable of interest with baseline cover: $Pr(exposure_p) \times Post_t \times Baseline_p$, where $Baseline_p$ represents pre-intervention tree canopy cover in the year 2000.

We note that recent work has raised concerns related to nonclassical measurement error in satellite-derived estimates of forest loss. In Alix-García and Millimet (2023), such measurement error biases estimates of treatment effects of plot-level participation in a Mexican forest conservation program because cross-sectional differences in treatment status are correlated with factors driving measurement error (i.e., rugged terrain, persistent cloud cover, etc.). This is not a primary concern in our setting due to the randomization of treatment exposure.

Before discussing our empirical results, we make two methodological comments. First, our use of a hazard regression is motivated by insights generated by Garcia and Heilmayr (2024). It is important to note that an alternative approach to addressing the concerns raised by Garcia and Heilmayr (2024) is to aggregate binary pixels spatially. While this solves the econometric issues of interest to Garcia and Heilmayr (2024), this aggregation process can lead to measurement error and biased effect estimates as discussed above and by Jack and Walker (2024). This is especially true in a setting, such as ours, where land is not clearly attributable to villages (as shown by the meaningful overlap in land areas around villages based on travel time estimates between households and rice plots, shown in Fig. 1). Moreover, motivated by the possibility of an intensification response to the adoption of USG fertilizer, we aim to estimate granular, landscape level heterogeneous effects consistent with this type of response. Therefore, our approach of generating a continuous measure of treatment exposure, combined with data from a well-implemented RCT, is a useful contribution to the literature. That is, our approach simultaneously addresses issues about measurement error (Jack and Walker, 2024) and issues relating to estimation error (Garcia and Heilmayr, 2024). Furthermore, while our approach is particularly well-suited for our study setting, it is generalizable and could be useful in many settings where researchers want to evaluate the effects of cluster-level RCTs on landscape-level outcomes.[15]

Second, conventional pre-trend tests are not feasible in our empirical setting due to the observationally irreversible nature of our deforestation data and the continuous nature of our treatment exposure variable. These two features constrain our ability to plot pre-intervention trends in the outcome variable between mutually exclusive groups defined by treatment status. We instead present an alternative test of pre-intervention balance to support the validity of our empirical analysis and address concerns about bias stemming from factors that are correlated both with canopy loss and treatment exposure at the pixel level. Figure A.4 in the Supplemental Appendix shows evidence of balance in treatment exposure by the binary variable indicating if a tree canopy on a given pixel is lost prior to the intervention in 2014. Panel A simply plots the probability distribution of treatment exposure between plots with and without tree canopy loss in 2014. Panel B shows a quantile–quantile plot of treatment exposure by pre-intervention tree canopy loss. Both figures provide visual evidence of balance in treatment exposure by pre-intervention tree canopy loss.

4.3. Deforestation results

We now present our core results on the rate of tree canopy loss due to exposure to the experimental intervention that promoted the adoption of USG fertilizer. For computational ease, we randomly select a 10 percent sample of pixels from our study area. This 10 percent sample still includes over one million observations; nevertheless, we also show one set of our core results with the full 100 percent sample of pixels. Given the scale of our exposure variable, the estimated coefficients reported in Table 1 can be interpreted as the log of the proportional hazard rate, that is, the probability that a pixel is deforested in a given year relative to an untreated pixel.

Table 1 reports heterogeneous effects of exposure to the experimental intervention on rates of deforestation. Column (1) shows that, on average, treatment exposure had no effect on canopy loss. Specifically, the coefficient estimate shows that pixels with an additional one unit increase in treatment exposure are on average 15 percent more likely to experience deforestation in a given year, but this estimate is not statistically significant and is indistinguishable from zero.[16] This average effect estimate, however, likely conceals important heterogeneity. In column (2) we include an interaction term with a binary variable indicating if pre-intervention (i.e., in the year 2000) tree canopy cover exceeded 40 percent of the pixel. This specification shows that among pixels with less than 40 percent pre-intervention tree cover, exposure to the experimental intervention does not meaningfully influence the tree canopy loss rate. The magnitude of the estimated effect is relatively small, representing a 17 percent increase in the likelihood a pixel is deforested, and is not statistically significant. Among pixel grid cells with more than 40 percent pre-intervention tree-cover, however, we

[13] We note that PPML estimators do not suffer from the same incidental parameters problem that other non-linear models (e.g. logistic regression) do. Hence, the large number of pixel FE in our model will not undermine the statistical consistency of our treatment effect estimates.

[14] The analysis in this paper was not pre-registered as the method for this study was not sufficiently clear *ex-ante*.

[15] A limitation of this estimation approach are econometric challenges related to estimating dynamic effects in a hazard regression context. While such effects are of practical interest, it is not clear how to interpret dynamics effects with an observationally irreversible outcome variable. The recent work of Christoffersen (2021) offers a promising way forward, but applying this estimation method to the empirical context of our study requires an econometric contribution that is outside the scope of the present study.

[16] Note that $e^{0.1423} - 1 = 0.153$ percent increase.

Table 1
Forest loss (2009–2021).

	Canopy Loss Rate (relative hazard rate)			
	(1)	(2)	(3)	(4)
Exposure	0.1423	0.1534	2.797***	2.804***
	(0.2432)	(0.2443)	(0.5884)	(0.5249)
Exposure × Pre-intervention tree cover>40%		−1.783**		
		(0.8760)		
Exposure × Pre-intervention tree cover			−0.1374***	−0.1282***
			(0.0270)	(0.0194)
Sample	10%	10%	10%	100%
Observations	1,001,231	1,001,231	1,001,231	8,873,577

All results include pixel and year fixed effects.
Conley (2 km) standard-errors in parentheses. p-value: ***: 0.01, **: 0.05, *: 0.1.

find that exposure to the experimental intervention reduces the tree canopy loss rate. Specifically, among pixels with pre-intervention tree canopy cover exceeding 40 percent of the pixel, an additional one unit increase in expected treatment exposure reduces the likelihood of experiencing deforestation by, on average, 83 percent in a given year. In more practical terms, this result implies that in densely forested areas, exposure to the experimental treatment reduces the tree canopy loss rate to nearly zero. The pre-intervention tree canopy loss rate within one km around villages in our study is 0.001, and a one unit increase in treatment exposure reduces this loss rate by an order of magnitude, to 0.0001. These findings indicate that in areas with more pre-intervention tree cover, exposure to the experimental intervention meaningfully reduces rates of tree canopy loss.

We further investigate this heterogeneity in column (3) by fully interacting the treatment exposure variable with the distribution of pre-intervention tree canopy cover. Interpreting these results requires combining the two coefficients presented in column (3). When pre-intervention tree canopy cover is low, the large and positive coefficient on the non-interacted exposure variable dominates and indicates that exposure leads to higher rates of tree canopy loss. By contrast, as pre-intervention tree canopy cover increases, the negative coefficient on the interaction term dominates—reflecting lower rates of tree canopy loss in response to treatment exposure. More specifically, for a given level of exposure to the experimental treatment, pixels with one percentage point denser pre-intervention tree canopy cover are 13 percent less likely to experience deforestation in a given year. These results show that in areas with less pre-intervention forest cover, exposure to the experimental intervention increases deforestation while in areas with more pre-intervention forest cover, exposure to the experimental intervention reduces deforestation.[17] Finally, column (4) reproduces these results when we use the full 100 percent sample of pixels.

Fig. 5 illustrates these heterogeneous effects by plotting the relationship between the percentage change in tree canopy loss rate due to exposure to the experimental intervention and baseline (i.e., pre-intervention) tree canopy density. This figure shows that the inflection point is, on average, a little more than 20 percent pre-intervention tree canopy cover. Land with less than 20 percent baseline cover tends to see the canopy loss rate increase in response to treatment exposure while land with greater than 20 percent canopy cover tends to see a reduction in the canopy loss rate.

This is a novel insight of the effect of agricultural technology adoption on deforestation and provides a more nuanced description of agricultural intensification mechanisms. Our findings suggest that farmers tend to clear trees on, or adjacent to, existing plots for more efficient or intensive production while reducing tree clearing pressure on more densely canopied land that is more distant from existing plots. Plot-level investments in agricultural intensification could include some cleaning up of the plot area, involving the removal of existing trees on or overlapping with the plot boundary. For example, we can imagine a low-productivity equilibrium where removing trees on or close to a plot carries marginal costs that outweigh marginal benefits. With the adoption of agricultural technology, a higher-productivity equilibrium could emerge where the marginal benefit of cleaning up trees on or close to a plot exceeds the marginal cost. Imagery presented in Fig. 4 appears consistent with this mechanism.

5. Agricultural productivity

We now turn to investigating a plausible mechanism whereby agricultural technology adoption might influence rates of deforestation by estimating the productivity effects of the experimental intervention. Estimates from Liverpool-Tasie et al. (2025) show that in the first year that USG fertilizer was available, rice yields did not increase in treatment villages relative to control villages.[18] The authors explain that this finding is most likely driven by the observation that farmers in treatment villages did not also adopt many of the recommended practices associated with the optimal use of USG fertilizer (i.e., use of complementary fertilizers, application timing, consistent irrigation, etc.). Although this initial finding is informative for understanding the immediate yield effects of USG adoption specifically among rice farmers, our objective in this paper requires a broader assessment of the productivity effects of USG adoption. This expanded assessment is motivated by two important details. First, USG fertilizer can be used in the production of other crops, not just rice. Farmers in our study area also produce maize, sorghum, and other crops that require nitrogen and could enjoy productivity benefits from applying USG to these crops. Second, it can take time for farmers to learn about the optimal use of new technologies and this learning might not occur contemporaneously, or even within the same growing season, with adoption.

5.1. Agricultural productivity estimation

To estimate more general productivity effects of the experimental intervention we use normalized difference vegetation index (NDVI) data derived from the Landsat satellites with 30-meter resolution.[19] NDVI — calculated as the normalized difference between red and near-infrared bands from multispectral imagery — is widely used to measure

[17] We establish the robustness of these findings using randomization inference. Specifically, we repeatedly draw a one percent subsample from the full sample and re-estimate coefficients in column (3). We do this 10,000 times and construct standard errors as the standard deviation of the distribution of regression coefficients. Our estimates remain statistically significant at conventional levels.

[18] It is important to note that the yield effects reported by Liverpool-Tasie et al. (2025) rely on self-reported yield estimates as measured in the household survey data. Thus, there is concern that measurement error in these self-reported yield estimates might obscure true effects (Lobell et al., 2020). However, as we will show in this sub-section, our estimates using NDVI data align well with the results using self-reported estimates from the household survey data.

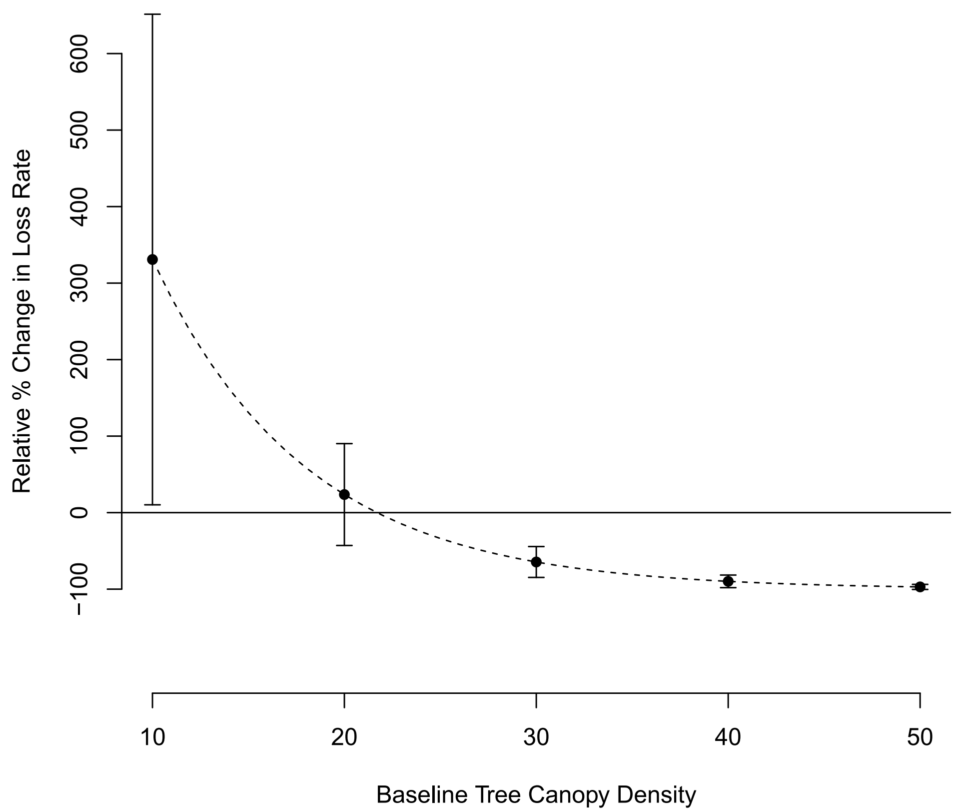

Fig. 5. Relative canopy loss rates based on pre-intervention tree cover.
Notes: This figure plots heterogeneous treatment effects by pre-intervention (year 2000) tree canopy density. The vertical axis plots relative percentage changes in the rates of tree canopy loss from a one unit increase in exposure (i.e. going from untreated to treated with 100% certainty). Parameter estimates come from column (3) in Table 1 and transformed via $100 * (e^{\hat{\lambda}} - 1)$. We include 95% confidence intervals calculated via the delta method.

vegetative health and, consequently, is frequently used to proxy agricultural yields (e.g. Huang et al., 2021; Lobell et al., 2020). Following our pixel-level analytical approach above, we use a composite pixel-level annual panel derived from median pixel-level NDVI values over each calendar year.[20] We estimate the following two-way fixed effects dose–response model via an ordinary least squares (OLS) regression:

$$NDVI_{it} = \beta Pr(exposure_i) \times Post_t + \alpha_i + \delta_t + \epsilon_{it} \quad (7)$$

In this specification, i indexes pixels, t indexes years, $Pr(exposure_i)$ is the treatment exposure variable defined in Eq. (5), $Post_t$ is a dummy variable equal to one in years 2015 or later and zero otherwise, α_i are pixel fixed effects and δ_t are year fixed effects. Given the rather narrow geographic scope of our sample region, δ_t will control for any weather shocks that may affect changes in agricultural productivity. Likewise, α_i will control for any time-invariant pixel features that may drive cross-sectional differences in productivity (e.g. soil type, slope, etc.) The β coefficient identifies the effect of increased exposure to the experimental intervention on agricultural productivity as measured by NDVI. Mirroring our tree canopy loss specifications above, we also consider heterogeneous treatment effects by pre-intervention tree canopy density to identify a potential intensification response.

5.2. Agricultural productivity results

Table 2 shows that exposure to the experimental intervention increases agricultural productivity, as measured by NDVI. Panel A reports results that use the inverse hyperbolic sine (IHS) transformation of median NDVI evaluated over the calendar year as the outcome variable, and allows us to more easily interpret the magnitude of our effect estimates. For the sake of comparison, Panel B reports results with the raw median NDVI index values as the outcomes variable, which are bounded between −1 and 1. Column (1) estimates the effect of treatment exposure using an unrestricted 10 percent sample of pixel grid cells.[21] We find that treatment exposure significantly increases

[19] Our NDVI data come from Landsat annual NDVI composite collection 2 data accessed via Google Earth Engine (LANDSAT/COMPOSITES/C02/T1_L2_ANNUAL_NDVI). Collection 2 data measure surface reflectance and include extensive processing conducted by the US Geological Survey to minimize known issues with Landsat 7 and Landsat 8 instruments. Details on USGS Collection 2 processing can be found at https://www.usgs.gov/landsat-missions/landsat-collection-2. The annual composite data include further processing by Google Earth Engine, details of which can be found at https://developers.google.com/earth-engine/landsat_c1_to_c2. Annual NDVI composite values are the median values of Landsat 7 and Landsat 8 imagery over the calendar year. We further process this data by applying a water mask based on spatially-explicit JRC water occurrence data at 30-meter pixel resolution (Pekel et al., 2016). We only consider pixels with less than 20% water occurrence.

[20] Because NDVI is a continuous measure on [−1, 1], our pixel-level NDVI panel does not have the same survival structure as our deforestation data and, therefore, does not require the same censoring. As a result, our total sample sizes for NDVI regressions are larger than for our deforestation hazard regressions, despite the same 30 m pixel-level resolution.

[21] We take a random sample of pixel grid cells to ease the computational burden of estimating this regression specification. Even with this sampling, we have over 13 million observations.

Table 2
Agricultural productivity (2009–2021).

Panel A: Inverse Hyperbolic Sine (IHS) of Median NDVI over the Calendar Year			
	(1)	(2)	(3)
Exposure	0.0176***	0.0192***	0.0138*
	(0.0044)	(0.0046)	(0.0071)
Exposure × Pre-intervention tree cover			0.0003
			(0.0005)
	Yes	Yes	Yes
Baseline Cover	unrestricted	<10%	unrestricted
Pre-intervention Mean	0.469	0.369	0.469
Observations	13,484,939	2,495,883	13,484,939
R^2	0.67931	0.62538	0.67932
Panel B: Raw Index of Median NDVI over the Calendar Year			
	(1)	(2)	(3)
Exposure	0.0124***	0.0144***	0.0096*
	(0.0031)	(0.0034)	(0.0050)
Exposure × Pre-intervention tree cover			0.0002
			(0.0003)
	Yes	Yes	Yes
Baseline Cover	unrestricted	<10%	unrestricted
Pre-intervention Mean	0.469	0.369	0.469
Observations	13,484,939	2,495,883	13,484,939
R^2	0.68147	0.62932	0.68148
Panel C: Inverse Hyperbolic Sine (IHS) of Max NDVI over the Growing Season			
	(1)	(2)	(3)
Exposure	0.0082*	0.0073*	0.0025
	(0.0045)	(0.0041)	(0.0058)
Exposure × Pre-intervention tree cover			0.0005
			(0.0003)
Baseline Cover	unrestricted	<10%	unrestricted
Pre-intervention Mean	0.610	0.503	0.610
Observations	6,299,735	1,190,514	6,299,735
R^2	0.53509	0.47827	0.53510

The growing season is July and December, inclusive.
All results use 10% sample of pixels with pixel and year fixed effects.
Conley (2 km) standard-errors in parentheses. p-value: ***: 0.01, **: 0.05, *: 0.1.

NDVI, indicating that the adoption of USG fertilizer increases agricultural productivity. Our point estimate, reported in Panel A, implies an approximately two percent increase in NDVI relative to the mean of 0.27. In column (2) we restrict the sample of pixels to only include pixels with less than 10 percent pre-intervention forest cover. We estimate this specification to more precisely investigate productivity effects driven by agricultural intensification, rather than through the expansion of farmland. We find results that are qualitatively similar to the results found when using an unrestricted sample of pixels, reported in Panel A, implying roughly a two percent increase in NDVI. This finding supports the idea that measured increases in agricultural productivity are likely driven by intensification efforts. Finally, column (3) includes an interaction term to further investigate the role of pre-intervention tree canopy cover on these agricultural productivity effects. Similar to the deforestation results, interpreting these results requires combining the two coefficients presented in column (3). When pre-intervention tree canopy cover is low, the positive and statistically significant coefficient on the non-interacted exposure variable dominates. This result indicates that exposure to the experimental intervention leads to increased agricultural productivity in areas with less pre-intervention forest cover. By contrast, as pre-intervention tree canopy cover increases, the coefficient on the interaction term becomes increasingly important. We find no evidence of a significant differential effect on more heavily canopied land.

In Panel B of Table 2 we find qualitatively similar results with raw NDVI level values as the outcome variable. Similar to the results in Panel A, we estimate qualitatively similar coefficients with the unrestricted data in column (1) and with the data restricted to the sample of pixels to only include pixels with less than 10 percent pre-intervention forest cover in column (2). Again, this finding supports the idea that measured increases in agricultural productivity are likely driven by intensification efforts. In column (3) we estimate similar coefficients when disaggregating the effects by pre-intervention tree cover.

In Panel C of Table 2, we consider an alternative statistic generated from NDVI data. To do this, we narrow our temporal window of observation from the entire calendar year to the growing season (i.e., July through December) for the primary crops cultivated in Kwara State (i.e., rice, maize, and sorghum), and generate a measure of agricultural productivity that represents the maximum value of NDVI evaluated at the pixel level. The use of this alternative measure is motivated by both conceptual reasoning and empirical results (Lewis et al., 1998) indicating that peak biomass is most directly linked with agricultural productivity. The cost of using this alternative measure is a mechanical reduction in sample size, which is — in part — due to a combination of imagery timing and cloud masking. Given there are fewer candidate images available over the growing season relative to the full calendar year, quality filtering ends up removing more pixels. Nevertheless, we find results that are qualitatively similar to the results in Panels A and B. The point estimates are, by nature, smaller because the max is mechanically larger than the median, but we continue to find that treatment exposure increases max NDVI when using an unrestricted 10 percent sample of pixel grid cells and that this effect is predominantly driven by less dense pre-intervention forest cover.

We finally estimate dynamic effects of exposure to the experimental intervention on agricultural productivity. We do this with a regression specification that estimates separate effects for each year in our panel, both before and after the implementation of the experimental intervention. This approach is akin to an event study specification, but instead

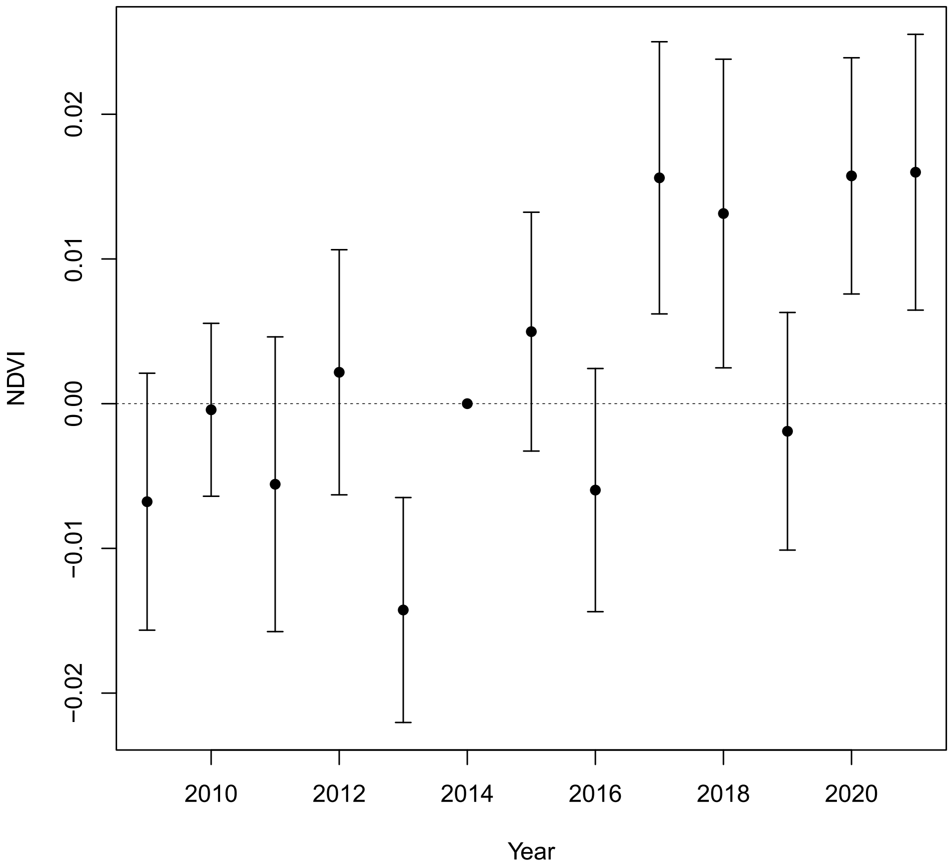

Fig. 6. Agricultural productivity effects over time (2009–2021).
Notes: Dynamic effect estimates of the experimental intervention on NDVI. 95 percent confidence intervals shown based on Conley (2 km) standard errors.

of a binary indicator of treatment status as used in traditional event study applications, we use our variable defining treatment exposure. Therefore, we interpret these results as a dynamic dose–response model of exposure to the experimental intervention. Our objective with this exercise are twofold. First, we aim to expand the time-horizon of identifiable treatment effects to differentiate transitory effects from more persistent effects. Our use of remote-sensing data uniquely allows us to do so without additional waves of survey collection. Second, our dynamic specification allows us to identify potential time lags in productivity responses that are consistent with learning and diffusion of the technology.

In Fig. 6 we plot the dynamic effect of exposure to the experimental intervention on NDVI. This figure shows that agricultural productivity effects, as measured by NDVI, took several years to materialize. While the intervention promoting the adoption of USG among rice farmers took place between 2014 and 2015, we do not observe positive productivity effects that are statistically distinguishable from zero until 2017. These results — particularly our 2015 treatment effect — are consistent with 2015 endline survey results from Liverpool-Tasie et al. (2025) who estimate null effects on rice yield in the 2015 agricultural season.[22] These results are also consistent with our finding, reported in Table A.3 in the Supplemental Appendix, showing a reduction in plot area in treatment villages as measured in the 2015 endline data. Liverpool-Tasie et al. (2025) contextualize these findings with the observation that many farmers in treatment villages did not adopt complementary practices to maximize productivity gains from USG adoption. Critically, we reach a very different conclusion on the benefits of the intervention with our ability to identify longer-horizon effects which almost certainly changes the cost-benefit calculus of policy-makers, program administrators, or other stakeholders. Our lagged productivity response is consistent with the idea that the benefits of agricultural technology adoption can take time to manifest as farmers learn how to most effectively incorporate the technology into their production processes (Foster and Rosenzweig, 1995), which can be missed in traditional RCT survey design timing. Moreover, these lagged effects could come from crops other than rice, such as maize or sorghum. In this case, the lagged effects we observe here can also be partially explained by a diffusion of USG use across crops, a process that might also take time to develop.[23]

We note a particular idiosyncrasy in these dynamic effect estimates which requires a brief comment—the negative effect of treatment exposure in 2013, two years prior to the implementation of the experimental intervention. Nigeria experienced historic floods in late 2012 that displaced millions of people and were especially dramatic along the Niger river. The significant negative pre-exposure effect in 2013 is likely driven by these floods which led to a disproportionate reduction in NDVI for pixels located close to the Niger river which runs through the northern part of our sample region (seen in Fig. 1). This randomly coincides with a region of particularly high treatment exposure. Because of the randomization of treatment exposure, however, we do not view this as a threat to causal identification in our empirical strategy.

[22] Unfortunately a direct mapping of these NDVI results to data reported in the household survey is not possible because NDVI represents a measure that is associated with agricultural productivity across all crops and the household survey focused exclusively on rice production, neglecting to measure productivity estimates on other crops such as maize and sorghum.

[23] This type of cross-crop diffusion is perhaps also best thought of as a learning process as farmers learn that the technology may benefit crops other than rice—the explicit target of the intervention.

Table 3
Alternative vegetative indices (2009–2021).

Panel A: Inverse Hyperbolic Sine (IHS) of Max EVI over the Growing Season			
	(1)	(2)	(3)
Exposure 0.0027	0.0027	−0.0338***	
	(0.0040)	(0.0031)	(0.0046)
Exposure × Pre-intervention tree cover			0.0029***
			(0.0003)
Baseline Cover	unrestricted	<10%	unrestricted
Pre-intervention Mean	0.374	0.327	0.374
Observations	6,299,735	1,190,514	6,299,735
R^2	0.40764	0.43477	0.40839
Panel B: Ratio of Max EVI to Max NDVI over the Growing Season			
	(1)	(2)	(3)
Exposure	−0.0030**	−0.0032**	−0.0357***
	(0.0013)	(0.0013)	(0.0023)
Exposure × Pre-intervention tree cover			0.0026***
			(0.0002)
Baseline Cover	unrestricted	<10%	unrestricted
Pre-intervention Mean	0.610	0.642	0.610
Observations	6,299,731	1,190,512	6,299,731
R^2	0.42232	0.38515	0.42459
Panel C: Difference in Max NDVI and Max MSAVI over the Growing Season			
	(1)	(2)	(3)
Exposure	0.0146***	0.0110**	0.0303***
	(0.0044)	(0.0046)	(0.0068)
Exposure × Pre-intervention tree cover			−0.0012***
			(0.0004)
Baseline Cover	unrestricted	<10%	unrestricted
Pre-intervention Mean	0.567	0.399	0.567
Observations	6,299,735	1,190,514	6,299,735
R^2	0.63609	0.53223	0.63616

The growing season is July and December, inclusive.
All results use 10% sample of pixels with pixel and year fixed effects.
Conley (2 km) standard-errors in parentheses. p-value: ***: 0.01, **: 0.05, *: 0.1.

Before concluding this discussion on agricultural productivity effects, it is useful to contextualize these results with our main results estimating the relationship between agricultural technology adoption and deforestation. While these agricultural productivity results might seem small in magnitude, it is important to note that the positive treatment effects we find here are net any tree canopy change effects. This is because changes in NDVI can be driven by both changes in vegetation on cropland and in forest areas. Therefore, it is worth emphasizing that our main results on deforestation indicate that tree canopy loss rates *increase* in response to exposure to the experimental treatment on more sparsely canopied land which will contribute, in part, to a *decrease* in measured NDVI ceteris paribus. This implies that in areas with sparse tree cover, our estimated effects on agricultural productivity can be viewed as an estimate of the lower bound on the true effect.

5.3. Alternative earth observation data

Our main measure of agricultural productivity is the median NDVI value evaluated at the pixel level over each calendar year. As a matter of course, we prefer the median NDVI over the calendar year as a measure of agricultural activity because it is agnostic about what crops are grown on a pixel grid cell and, therefore, the associated relevant growing season. This consideration is particularly salient in regions with bimodal rainfall patterns — like Kwara State in Nigeria — which allow for two distinct planting seasons. However, whether aggregated over the calendar year, growing season or using some other criteria, NDVI fundamentally relies on red and infrared surface reflectance (from vegetation, in particular), and thus might not precisely capture conversion from green tree canopy to green crop production. We address this concern with three alternative measures of land use based on vegetative indices.[24]

First, we consider an alternative vegetative index—the enhanced vegetative index (EVI). One general limitation of NDVI, particularly when evaluated at maximum values over some time period, is its propensity to saturate in contexts with high biomass. This saturation can limit the ability of NDVI to meaningfully display variation in agricultural productivity. EVI is an alternative index that aims to correct for atmospheric effects and canopy background "noise" by incorporating blue bands of reflection in addition to the infrared and red bands incorporated by NDVI. As a result, EVI is more robust to saturation and, as a result, more sensitive to canopy structure and total tree area. Thus, relative to NDVI, EVI is much more sensitive to canopy cover changes. *Ex ante* we expect EVI responses to treatment exposure to be concentrated on actual canopy changes rather than just net changes in "greenness". We present these results in Panel A of Table 3. While we do not find much change driven by exposure to treatment in columns (1) and (2), when we consider an interaction with pre-intervention tree cover in column (3) we observe results that qualitatively mirror our hazard regression results on canopy loss. On sparse land, EVI falls and on denser land, EVI rises. These results provide a useful validation of our main results on deforestation with an alternative data source.

Second, we consider the ratio of EVI to NDVI (again measured during the growing season) as a measure of the concentration of tree cover relative to grasslands/crops. Huete et al. (2002) illustrate the relative magnitudes of EVI and NDVI across land types, suggesting

[24] Like our NDVI data, we apply the same water occurrence mask to all alternative land use measures. We only consider pixels with less than 20% water occurrence based on Pekel et al. (2016).

the EVI-NDVI ratio is a useful metric in land use classification. The increased sensitivity of the EVI to canopy changes help to qualitatively differentiate observed changes in greenness. In particular, an increase in agricultural production (and/or productivity) relative to tree cover should be reflected in lower values of the ratio of EVI to NDVI. We present these results in Panel B of Table 3. Although we do not find much change in the ratio of EVI and NDVI in columns (1) and (2), when we include an interaction term differentiating the effect of treatment exposure by pre-intervention tree cover in column (3) we find results that indicate an increase in crops relative to trees in sparsely forested areas and a decrease in more densely forested areas. These results further support the validity of our main results that indicate an intensification response to the experimental treatment promoting the adoption of USG fertilizer.

Finally, we construct the modified soil adjusted vegetation index (MSAVI) following (Qi et al., 1994). This vegetative index addresses bias arising from "soil noise" that can spuriously increase values of NDVI when soil is directly observable (such as on agricultural plots). We consider the difference between NDVI and MSAVI as a measure of soil exposure to proxy the concentration of crops relative to tree canopy.[25] The difference will be higher in areas with more crop cultivation relative to tree cover. We show these results in Panel C of Table 3. All estimates are statistically significant at conventional levels. In column (1) we find that treatment exposure increases the difference between NDVI and MSAVI and column (2) shows that this effect is predominantly driven by land with less dense pre-intervention forest cover. In column (3), when we consider an interaction with pre-intervention tree cover, we find evidence consistent with our main results: in sparsely forested areas we observe effects that are consistent with increased agricultural activity and fewer trees. By contrast, in more densely forested areas we find evidence of reduced crop (soil) prevalence and increased tree cover.

6. Concluding remarks

We combine data from an RCT in Nigeria that promoted the adoption of an environmentally-friendly agricultural technology with earth observation data to study the relationship between agricultural technology adoption and deforestation. We find evidence that exposure to improved agricultural technology leads to an intensification response, with deforestation pressures redirected away from more heavily canopied areas towards land with sparser tree cover that characterizes agricultural plots in Nigeria. We find additional evidence of agricultural productivity effects that support this intensification effect. These findings are consistent with farmers cleaning up agricultural plots by clearing trees on plots and/or expanding plot margins into sparsely canopied adjacent land in order to farm more intensively with improved technology, leaving more dense forest area to nature.

These results are important for at least three reasons. First, our findings offer encouraging evidence for policy and technological solutions to the "triple challenge" of increasing global food security, supporting local livelihoods, and limiting environmental damage (OECD, 2021). Our results show that investment in the development of improved, and environmentally-friendly, agricultural technologies maintains canopy cover on average, and even reduces rates of canopy loss in more dense forest areas. This finding contrasts with narratives found in policy discussions that frame agricultural productivity and environmental protection as always being in tension with each other and, therefore, policies must necessarily deal with hard trade-offs. For example, a long and well-established debate contrasts "land-sharing" strategies that promote nature-friendly but low-yielding agriculture on a larger land footprint with "land-sparing" strategies that promote high-yielding agriculture on a smaller land footprint (Green et al., 2005; Balmford et al., 2005; Fischer et al., 2008; Burney et al., 2010; Phalan et al., 2011; Balmford et al., 2019). Our results align with an approach that calls for a depolarization of the debate about the relationship between agriculture and nature, and prioritizes efforts to promote high-yielding agriculture in ways that minimizes costs on the environment (Kremen, 2015; Kremen and Merenlender, 2018; Mertz and Mertens, 2017). Thus, agricultural research and corresponding promotion of improved technologies can be an effective way to limit deforestation, support food security, and improve agricultural productivity.

It is important, however, to recognize the potential sensitivity of these results to various factors leading to treatment effect heterogeneity. Indeed, our results critically hinge on existing forest density. Other factors — such as input and output market access, the local agro-ecological suitability for commodity crop cultivation, and the property rights environment — could also represent critical dimensions of heterogeneity that might influence results if this study could be implemented in, for example, a different state within Nigeria or a different country altogether. It is not difficult to imagine an environment where output demand for a given crop is low, which limits the incentive for farmers to intensify their production in response to an input supply shock. Alternatively, if output demand is sufficiently high for a given crop it is plausible that an input supply shock could generate both an intensification and extensification response by farmers. We view our results as demonstrating the importance of documenting treatment effect heterogeneity in the relationship between agricultural technology adoption and deforestation, thereby motivating future research studying this relationship in diverse settings.[26]

Second, our results are relevant for forest conservation policies. Our findings show that agricultural technology adoption leads to increases in agricultural productivity and deforestation in areas with less dense canopy cover. By contrast, areas with more dense canopy cover, which are more likely to be candidate areas for conservation efforts, are in fact relatively costly for farmers — especially smallholder farmers — to convert into farmland. Thus, the opportunity costs of conservation might be smaller than traditionally calculated (Joppa and Pfaff, 2009; Frank and Schlenker, 2016; Luby et al., 2022). Furthermore, our work suggests that improved agricultural technologies might be policy complements to forest conservation efforts and improving technology access will likely lower the costs of conservation even further.

Finally, our analytical framework provides a practical methodological path forward for future studies that combine earth observation data with data from RCTs. Our spatially-explicit approach offers opportunities to improve statistical power, identify landscape-level heterogeneous treatment effects, and identify longer-horizon treatment effects that extend beyond the typically-very-short study windows associated with RCT survey collection. The latter point is especially important in considering the possibility of persistence in fixed-term development interventions.

[25] As documented in Qi et al. (1994), NDVI includes a vegetation "signal" as well as a soil background "signal" (or "noise") that can arise from soil reflectance. The MSAVI is designed to remove the soil signal. Hence, by considering the difference in NDVI and MSAVI, we are effectively capturing the relative magnitude of this soil signal which will be higher on land with relatively more crops to tree canopy. Kadri et al. (2023) similarly use NDVI and MSAVI together to identify land use change from forest to crops but do so in a machine learning exercise using random forest models.

[26] We also note that settings where RCTs have been successfully implemented are not randomly distributed across time or space. Instead, they represent a narrowly defined setting where an implementing partner is both willing and able to successfully follow and implement experimental protocols (Deaton and Cartwright, 2018). Therefore, to the extent that a literature forms studying the relationship between agricultural technology adoption and deforestation by linking earth observation data with RCTs, we must be aware of the biases that can arise from aggregating findings exclusively from RCTs (Allcott, 2015; Ahlin, 2024).

CRediT authorship contribution statement

Jeffrey R. Bloem: Writing – review & editing, Writing – original draft, Methodology, Formal analysis, Data curation, Conceptualization. **Clark Lundberg:** Writing – review & editing, Writing – original draft, Methodology, Formal analysis, Data curation, Conceptualization.

Declaration of competing interest

The authors have no other interests or conflicts to declare.

Acknowledgments

Jeffrey Bloem received support from CGIARs Rethinking Food Markets Initiative for this project. Clark Lundberg has received funding from the USDA/NIFA for another unrelated project.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jdeveco.2025.103600.

Data availability

Data will be made available on request.

References

Abman, R., Carney, C., 2020. Agricultural productivity and deforestation: Evidence from input subsidies and ethnic favoritism in Malawi. J. Environ. Econ. Manag. 103, 102342.
Abman, R., Garg, T., Pan, Y., Singhal, S., 2020. Agriculture and deforestation. Available At SSRN 3692682.
Ahlin, C., 2024. How and when is a ‘‘risky’’ RCT more biased than a naive observational approach? Working paper.
Ahmed, M., Islam, M., Kader, M., Anwar, M., 2000. Evaluation of urea super granule as a source of nitrogen in transplant aman rice. Pak. J. Biol. Sci. 3.
Alam, M.S., Khanam, M., Rahman, M.M., 2023. Environment-friendly nitrogen management practices in wetland paddy cultivation. Front. Sustain. Food Syst. 7, 1020570.
Alcott, B., 2005. Jevons' paradox. Ecol. Econom. 54 (1), 9–21.
Alix-García, J., Millimet, D., 2023. Remotely incorrect? Accounting for nonclassical measurement error in satellite data on deforestation. J. Assoc. Environ. Resour. Econ. 10 (5), 1335–1367.
Allcott, H., 2015. Site selection bias in program evaluation. Q. J. Econ. 130 (3), 1117–1165.
Amusa, T., 2024. Nigeria's forests are fast disappearing – urgent steps are needed to protect their benefits to the economy and environment. Conversat..
Avelino, A.F.T., Baylis, K., Honey-Rosés, J., 2016. Goldilocks and the raster grid: selecting scale when evaluating conservation programs. PLoS One 11 (12), e0167945.
Balmford, B., Green, R.E., Onial, M., Phalan, B., Balmford, A., 2019. How imperfect can land sparing be before land sharing is more favourable for wild species? J. Appl. Ecol. 56 (1), 73–84.
Balmford, A., Green, R.E., Scharlemann, J.P., 2005. Sparing land for nature: exploring the potential impact of changes in agricultural yield on the area needed for crop production. Global Change Biol. 11 (10), 1594–1605.
Bernard, T., Lambert, S., Macours, K., Vinez, M., 2023. Impact of small farmers' access to improved seeds and deforestation in DR Congo. Nat. Commun. 14 (1), 1603.
Borlaug, N., 2007. Feeding a hungry world. Science 318 (5849), 359–359.
Bowles, T.M., Atallah, S.S., Campbell, E.E., Gaudin, A.C., Wieder, W.R., Grandy, A.S., 2018. Addressing agricultural nitrogen losses in a changing climate. Nat. Sustain. 1 (8), 399–408.
Bulte, E.H., Damania, R., Lopez, R., 2007. On the gains of committing to inefficiency: corruption, deforestation and low land productivity in Latin America. J. Environ. Econ. Manag. 54 (3), 277–295.
Burney, J.A., Davis, S.J., Lobell, D.B., 2010. Greenhouse gas mitigation by agricultural intensification. Proc. Natl. Acad. Sci. 107 (26), 12052–12057.
Cameron, K.C., Di, H.J., Moir, J.L., 2013. Nitrogen losses from the soil/plant system: a review. Ann. Appl. Biol. 162 (2), 145–173.
Carreira, I., Costa, F., Pessoa, J.P., 2023. The deforestation effects of trade and agricultural productivity in Brazil. J. Dev. Econ. 103217.
Caviglia-Harris, J.L., 2018. Agricultural innovation and climate change policy in the Brazilian amazon: Intensification practices and the derived demand for pasture. J. Environ. Econ. Manag. 90, 232–248.
Ceddia, M.G., Sedlacek, S., Bardsley, N., Gomez-y Paloma, S., 2013. Sustainable agricultural intensification or Jevon's Paradox? The role of public governance in tropical South America. Glob. Environ. Chang. 23 (5), 1052–1063.
Christoffersen, B., 2021. Dynamichazard: Dynamic hazard models using state space models. J. Stat. Softw. 99 (7), 1–38.
Curtis, P.G., Slay, C.M., Harris, N.L., Tyukavina, A., Hansen, M.C., 2018. Classifying drivers of global forest loss. Science 361 (6407), 1108–1111.
Deaton, A., Cartwright, N., 2018. Understanding and misunderstanding randomized controlled trials. Soc. Sci. Med. 210, 2–21.
Deininger, K.W., Minten, B., 1999. Poverty, policies, and deforestation: the case of Mexico. Econ. Dev. Cult. Chang. 47 (2), 313–344.
Dobermann, A.R., 2005. Nitrogen Use Efficiency-State of the Art. Agronomy–Faculty Publications, p. 316.
Edwards, R.B., Falcon, W.P., Hadiwidjaja, G., Higgins, M.M., Naylor, R.L., Sumarto, S., 2022. Fight fire with finance: a randomized field experiment to curtail land-clearing fire in Indonesia. TNP2K Working Paper.
Ewers, R.M., Scharlemann, J.P., Balmford, A., Green, R.E., 2009. Do increases in agricultural yield spare land for nature? Global Change Biol. 15 (7), 1716–1726.
FAO, T., 2022. The state of the world's forests 2022. Forest pathways for green recovery and building inclusive, resilient and sustainable economies. Rome, FAO.
Fischer, J., Brosi, B., Daily, G.C., Ehrlich, P.R., Goldman, R., Goldstein, J., Lindenmayer, D.B., Manning, A.D., Mooney, H.A., Pejchar, L., et al., 2008. Should agricultural policies encourage land sparing or wildlife-friendly farming? Front. Ecol. Environ. 6 (7), 380–385.
Fisher, M., Shively, G.E., 2007. Agricultural subsidies and forest pressure in Malawi's ''Miombo'' woodlands. J. Agric. Resour. Econ. 349–362.
Foster, A.D., Rosenzweig, M.R., 1995. Learning by doing and learning from others: Human capital and technical change in agriculture. J. Polit. Econ. 103 (6), 1176–1209.
Foster, A.D., Rosenzweig, M.R., 2003. Economic growth and the rise of forests. Q. J. Econ. 118 (2), 601–637.
Frank, E.G., Schlenker, W., 2016. Balancing economic and ecological goals. Science 353 (6300), 651–652.
Garcia, A., Heilmayr, R., 2024. Impact evaluation with nonrepeatable outcomes: The case of forest conservation. J. Environ. Econ. Manag. 102971.
Ghosh, B., Bhat, R., 1998. Environmental hazards of nitrogen loading in wetland rice fields. Environ. Pollut. 102 (1), 123–126.
Gibbs, H.K., Ruesch, A.S., Achard, F., Clayton, M.K., Holmgren, P., Ramankutty, N., Foley, J.A., 2010. Tropical forests were the primary sources of new agricultural land in the 1980s and 1990s. Proc. Natl. Acad. Sci. 107 (38), 16732–16737.
Gollin, D., Hansen, C.W., Wingender, A.M., 2021. Two blades of grass: The impact of the green revolution. J. Politi. Econ. 129 (8), 2344–2384.
Green, R.E., Cornell, S.J., Scharlemann, J.P., Balmford, A., 2005. Farming and the fate of wild nature. Science 307 (5709), 550–555.
Hansen, M.C., Potapov, P.V., Moore, R., Hancher, M., Turubanova, S.A., Tyukavina, A., Thau, D., Stehman, S.V., Goetz, S.J., Loveland, T.R., et al., 2013. High-resolution global maps of 21st-century forest cover change. Science 342 (6160), 850–853.
Hill, R., Nguyen, T., Doan, M.K., 2024. Climate and Equity: A Framework to Understand Welfare Impacts and Guide Policy Action. Tech. rep., Poverty and Equity Global Practice Brief, World Bank Group, Washington, D.C..
Ho, J.C., Michalak, A.M., Pahlevan, N., 2019. Widespread global increase in intense lake phytoplankton blooms since the 1980s. Nature 574 (7780), 667–670.
Huang, S., Tang, L., Hupy, J.P., Wang, Y., Shao, G., 2021. A commentary review on the use of normalized difference vegetation index (NDVI) in the era of popular remote sensing. J. For. Res. 32 (1), 1–6.
Huete, A.R., Didan, K., Miura, T., Rodriguez, E.P., Gao, X., Ferreira, L.G., 2002. Overview of the radiometric and biophysical performance of the MODIS vegetation indices. Remote Sens. Environ. 83 (1–2), 195–213.
Islam, M., Sarkar, M., Uddin, S., Parvin, S., 2012. Yield of fine rice varieties as influenced by integrated management of poultry manure urea super granules and prilled urea. J. Environ. Sci. Nat. Resour. 5 (1), 129–132.
Jack, K., Walker, K., 2024. Integrating remote sensing and randomized controlled trials. Working paper.
Jayachandran, S., De Laat, J., Lambin, E.F., Stanton, C.Y., Audy, R., Thomas, N.E., 2017. Cash for carbon: A randomized trial of payments for ecosystem services to reduce deforestation. Science 357 (6348), 267–273.
Jayne, T.S., Sanchez, P.A., 2021. Agricultural productivity must improve in sub-Saharan Africa. Science 372 (6546), 1045–1047.
Jena, D., Misra, C., Bandyopadhyay, K., 2003. Effect of prilled urea and urea super granules on dynamics of ammonia volatilisation and nitrogen use efficiency of rice. J. Indian Soc. Soil Sci. 51 (3), 257–261.
Joppa, L.N., Pfaff, A., 2009. High and far: biases in the location of protected areas. PloS One 4 (12), e8273.
Kabir, M., Sarkar, M., Chowdhury, A., 2009. Effect of urea super granules, prilled urea and poultry manure on the yield of transplant Aman rice varieties. J. Bangladesh Agric. Univ. 7 (2), 259–263.

Kadri, N., Jebari, S., Augusseau, X., Mahdhi, N., Lestrelin, G., Berndtsson, R., 2023. Analysis of four decades of land use and land cover change in semiarid Tunisia using google earth engine. Remote. Sens. 15 (13).
Kamai, N., Omoigui, L., Kamara, A., Ekeleme, F., 2020. Guide to rice production in Northern Nigeria. Tech. rep., International Institute of Tropical Agriculture.
Koch, N., zu Ermgassen, E.K., Wehkamp, J., Oliveira Filho, F.J., Schwerhoff, G., 2019. Agricultural productivity and forest conservation: Evidence from the Brazilian Amazon. Am. J. Agric. Econ. 101 (3), 919–940.
Kremen, C., 2015. Reframing the land-sparing/land-sharing debate for biodiversity conservation. Ann. New York Acad. Sci. 1355 (1), 52–76.
Kremen, C., Merenlender, A.M., 2018. Landscapes that work for biodiversity and people. Science 362 (6412), eaau6020.
Lewis, J., Rowland, J., Nadeau, A., 1998. Estimating maize production in Kenya using NDVI: Some statistical considerations. Int. J. Remote Sens. 19 (13), 2609–2617.
Lipper, L., Thornton, P., Campbell, B.M., Baedeker, T., Braimoh, A., Bwalya, M., Caron, P., Cattaneo, A., Garrity, D., Henry, K., et al., 2014. Climate-smart agriculture for food security. Nat. Clim. Chang. 4 (12), 1068–1072.
Liu, J., You, L., Amini, M., Obersteiner, M., Herrero, M., Zehnder, A.J., Yang, H., 2010. A high-resolution assessment on global nitrogen flows in cropland. Proc. Natl. Acad. Sci. 107 (17), 8035–8040.
Liverpool-Tasie, S., Dillon, A., Bloem, J., Adjognon, S., 2025. Private sector promotion of agricultural technologies: Experimental evidence from Nigeria. J. Environ. Econ. Manag. 133, 103201.
Lobell, D.B., Azzari, G., Burke, M., Gourlay, S., Jin, Z., Kilic, T., Murray, S., 2020. Eyes in the sky, boots on the ground: Assessing satellite- and ground-based approaches to crop yield measurement and analysis. Am. J. Agric. Econ. 102 (1), 202–219.
Luby, I.H., Miller, S.J., Polasky, S., 2022. When and where to protect forests. Nature 609 (7925), 89–93.
Lupin, M., Lazo, J., Le, N., Little, A., 1983. Briquetting: Alternative Porcess for Urea Supergranules, vol. T-26, International Fertilizer Development Center.
Maertens, M., Zeller, M., Birner, R., 2006. Sustainable agricultural intensification in forest frontier areas. Agricult. Econ. 34 (2), 197–206.
Mertz, O., Mertens, C.F., 2017. Land sparing and land sharing policies in developing countries–drivers and linkages to scientific debates. World Dev. 98, 523–535.
Murtagh, E.M., Mair, J.L., Aguiar, E., Tudor-Locke, C., Murphy, M.H., 2021. Outdoor walking speeds of apparently healthy adults: A systematic review and meta-analysis. Sports Med. 51, 125–141.
OECD, 2021. Making Better Policies for Food Systems. OECD, Paris, p. 280.
Pekel, J.-F., Cottam, A., Gorelick, N., Belward, A.S., 2016. High-resolution mapping of global surface water and its long-term changes. Nature 540 (7633), 418–422.
Pelletier, J., Ngoma, H., Mason, N.M., Barrett, C.B., 2020. Does smallholder maize intensification reduce deforestation? Evidence from Zambia. Glob. Environ. Chang. 63, 102127.
Phalan, B., Green, R.E., Dicks, L.V., Dotta, G., Feniuk, C., Lamb, A., Strassburg, B.B., Williams, D.R., Ermgassen, E.K.z., Balmford, A., 2016. How can higher-yield farming help to spare nature? Science 351 (6272), 450–451.
Phalan, B., Onial, M., Balmford, A., Green, R.E., 2011. Reconciling food production and biodiversity conservation: land sharing and land sparing compared. Science 333 (6047), 1289–1291.
Qi, J., Chehbouni, A., Huete, A.R., Kerr, Y.H., Sorooshian, S., 1994. A modified soil-adjusted vegetation index. Remote Sens. Environ. 48 (2), 119–126.
Rudel, T.K., Schneider, L., Uriarte, M., Turner, B.L., DeFries, R., Lawrence, D., Geoghegan, J., Hecht, S., Ickowitz, A., Lambin, E.F., et al., 2009. Agricultural intensification and changes in cultivated areas, 1970–2005. Proc. Natl. Acad. Sci. 106 (49), 20675–20680.
Szerman, D., Assunção, J.J., Lipscomb, M., Mobarak, A.M., 2022. Agricultural productivity and deforestation: Evidence from Brazil. Economic Growth Center Discussion Paper 1091, Yale University.
Takasaki, Y., 2006. A model of shifting cultivation: can soil conservation reduce deforestation? Agricult. Econ. 35 (2), 193–201.
Thomas, J., Prasad, R., 1987. Relative efficiency of prilled urea, urea super granules, sulphur coated urea and nitrification inhibitor N-serve blended urea for direct seeded rice. J. Agron. Crop Sci. 159 (5), 302–307.
Villoria, N.B., Byerlee, D., Stevenson, J., 2014. The effects of agricultural technological progress on deforestation: what do we really know? Appl. Econ. Perspect. Policy 36 (2), 211–237.
Wilebore, B., Coomes, D., 2016. Combining spatial data with survey data improves predictions of boundaries between settlements. Appl. Geogr. 77, 1–7.
Williams, M., 2003. Deforesting the Earth: from Prehistory to Global Crisis. University of Chicago Press.
World Bank, 2010. World Development Report 2010: Development and Climate Change. The World Bank.
Yanggen, D., Reardon, T., 2001. Kudzu-improved fallows in the Peruvian Amazon. In: Agricultural Technologies and Tropical Deforestation. CAB International Wallingford UK, pp. 213–230.