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Selection into agroforestry adoption and crop production value among smallholder farmers in Tanzania

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Abstract Agroforestry is widely promoted in smallholder farming systems as a means of improving soil conditions, strengthening resilience to rainfall variability, and supporting rural livelihoods. However, households that adopt agroforestry may differ systematically from those that do not. We examine selection into agroforestry adoption and its relationship with crop production value per adult household member among smallholder farmers in Tabora Region, Tanzania. The practices promoted in the study area include planting the nitrogen-fixing tree Gliricidia sepium, pigeonpea–cassava intercropping, or a combination of the two. To account for potentially endogenous selection into adoption, we estimate a Poisson control-function model using household contact with extension agents as the exclusion restriction, complemented by instrumental-variable estimates and a sensitivity analysis. The control-function estimates indicate negative selection into agroforestry adoption: unobserved factors associated with a higher propensity to adopt are negatively related to crop production value in the outcome equation. Under the maintained identifying assumptions, the estimated average treatment effect on adopters is approximately USD 152 per adult household member, while the corresponding instrumental-variable estimate for households whose adoption decisions are affected by extension contact is approximately USD 412. Estimators that account only for observable differences produce estimates that are statistically indistinguishable from zero. The results suggest that agroforestry may be particularly beneficial for smallholders with relatively low counterfactual crop production value.

Keywords Agroforestry · Smallholder farmers · Technology adoption · Control function · Selection on unobservables · Instrumental variables · Tanzania · Sub-Saharan Africa

JEL Classification Q12 · Q16 · O13 · C26

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Introduction

The adoption of new farming technologies by smallholder farmers in developing countries, and the resulting welfare consequences, has long been a central topic in agricultural and development economics (e.g., Feder et al. 1985; Ramirez and Shultz 2000; Foster and Rosenzweig 2010; Ruzzante et al. 2021).

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As climate change intensifies, evidence on the welfare effects of adaptation strategies is increasingly important for supporting smallholder resilience to more frequent and severe climate shocks (Hertel and Rosch 2010; Di Falco and Veronesi 2013; Wheeler and von Braun 2013). Among the agronomic responses available to households, agroforestry has attracted particular interest because it can improve soil fertility, enhance moisture retention, and stabilize yields under variable rainfall (Jose 2009), while also providing tree-based products such as fuelwood and fodder. Additionally, where soils are often degraded and commercial fertilizers are expensive, the potential of nitrogen-fixing agroforestry systems has been documented to deliver substantial yield gains (Sileshi et al. 2008; Akinnifesi et al. 2010).

A central empirical challenge in evaluating the welfare effects of any agricultural innovation is that adoption is a household-level choice. Smallholders both produce and consume while operating in imperfect markets for goods, labor, and inputs (de Janvry et al. 1991; Dillon and Barrett 2017). Hence, adoption decisions reflect tradeoffs across multiple margins, including land use, labor allocation, and market participation. As a result, the households that adopt a new practice typically differ from those that do not in ways that are both observable and unobservable (Di Falco et al. 2011). Failing to account for non-random selection on these unobservable differences will, in general, deliver biased estimates of the population-level effect of adoption.

Although agronomic trials and meta-analyses have documented substantial yield gains from fertilizer-tree systems in sub-Saharan Africa (Sileshi et al. 2008; Akinnifesi et al. 2010), adoption rates among smallholder farmers remain low (Mercer 2004; Coe et al. 2014). To shed further light on farmers' selection into agroforestry adoption and potential productivity gains in sub-Saharan Africa, we estimate the effect of agroforestry adoption on the value of crop production among smallholder farmers in six villages of Tabora Region, Tanzania, using a household survey administered in 2019. The agroforestry systems we study include intercropping of drought-tolerant staple crops, cassava and pigeonpea, and the nitrogen-fixing leguminous tree *Gliricidia sepium*. To address potential non-random selection into agroforestry adoption, we use a Poisson control-function estimator with a binary indicator of household contact with extension agents as the exclusion restriction. We base inference on the weak-instrument-robust Anderson–Rubin test, and compare our results against the standard linear instrumental variable (2SLS) estimator and estimators that condition on observable covariates only. Since the exclusion restriction cannot be tested directly, we report local-to-zero plausibly-exogenous bounds (Conley et al. 2012) as the primary sensitivity check on potential violations of strict exogeneity.

Our central empirical finding is the strong negative selection of households into agroforestry adoption. That is, the unobservables that raise a household's propensity to adopt are negatively correlated with its productivity in the outcome equation. Conditional on this correction, agroforestry adoption is associated with a substantial increase in per-adult crop value: the average treatment effect on the treated is $152 per adult member with bias-corrected 95% bootstrap confidence interval [$57, $236]. The 2SLS local average treatment effect is approximately $412 for the subpopulation of households whose adoption decision is shifted by extension exposure. While the precise point magnitudes carry substantial uncertainty, the 95% Anderson–Rubin confidence set is bounded well above zero, with a lower bound of $23 per adult from the plausibly-exogenous interval. In contrast, when using estimators that condition on observable covariates alone, the treatment effects are not statistically significant.

We interpret the finding of negative selection into adoption as consistent with a Roy-style model in which households choose whether to adopt a technology based on heterogeneous net returns (see, e.g., Suri 2011). Although average returns from adoption are positive, households with higher productivity under existing livelihood strategies may still choose not to adopt if agroforestry entails high opportunity costs, including foregone returns from alternative land uses, such as livestock husbandry, or competing demands on labor and managerial attention. Lower-productivity adopters, by contrast, may have fewer options, making agroforestry a relatively attractive means of raising productivity by relaxing binding constraints, such as poor soil fertility, limited liquidity for purchased inputs, or exposure to production risk.

A closely related empirical literature estimates the welfare effects of agricultural technologies in

sub-Saharan Africa while explicitly correcting for endogenous adoption. Much of this work uses an endogenous switching regression model, which allows adopters and non-adopters to have different outcome equations and use the estimated selection process to construct counterfactual outcomes (Di Falco et al. 2011; Kuntashula and Mungatana 2013; Khonje et al. 2015, 2018). In the agroforestry literature, Coulibaly et al. (2017) provide one of the closest comparisons to our study, estimating the impact of fertilizer-tree adoption, including *Gliricidia sepium* and *Faidherbia albida*, on household food security in Malawi using endogenous switching regression. Asfaw et al. (2012) report a similar negative-selection pattern for pigeonpea adoption using endogenous switching regression in Ethiopia and Tanzania.

We contribute to this body of work by studying agroforestry adoption in western Tanzania, a setting where intercropping and fertilizer-tree systems have been promoted as responses to soil fertility decline and unpredictable rainfall, and by employing a Poisson quasi-maximum-likelihood control-function estimator. This specification is well suited to a non-negative, right-skewed outcome such as the value of household crop production, while also allowing us to test for endogenous selection through the generalized residual from the first-stage adoption equation. Because Poisson quasi-maximum likelihood is based on the conditional mean, it does not require the dependent variable to be Poisson distributed and can accommodate skewness and heteroskedasticity in the outcome (Wooldridge 2010). This contrasts with standard parametric endogenous switching regression models, which commonly rely on linear regime-specific outcome equations and stronger distributional assumptions on the regime-specific outcome errors.

The remainder of the paper proceeds as follows. The Study area and context section describes the agricultural systems prevalent in Tabora Region. The Methods section sets out the empirical strategy and the parameters of interest. The Data section describes the household survey, the construction of the outcome and treatment variables, and the descriptive characteristics of the estimation sample. The Results section reports the main estimates, the robustness checks, and the instrument-validity diagnostics, and the Discussion section interprets the findings in terms of the underlying agronomic and economic mechanisms. The Conclusion section offers avenues for future research.

Study area and context

The agricultural sector in Tanzania makes a substantial contribution to the country's economic activity, employing around 75 percent of the country's workforce and contributing nearly 30 percent of GDP.[1] Agricultural production is predominantly rain-fed and therefore highly vulnerable to rainfall variability and drought (Kimaro 2019). At the same time, Tanzania is undergoing a gradual structural transformation, with medium-scale and commercial farms becoming increasingly prevalent (Wineman et al. 2020).

The study area, Tabora Region in western Tanzania, has been the focus of multiple development initiatives aimed at reducing poverty and promoting more resilient farming systems, including programs supported by the United Nations Millennium Development Projects and the World Agroforestry Centre (ICRAF). Farming systems in Tabora are characterized by a mix of subsistence and market production, with households facing constraints related to market access, soil fertility, and climate risk.

Agroforestry interventions in Tabora typically combine woody drought-tolerant staple crops in intercropping systems, as well as with leguminous trees. Cassava (*Manihot esculenta*), one of Tanzania's most important staple crops, is a drought-tolerant woody perennial that produces an edible tuber widely consumed across sub-Saharan Africa. Pigeonpea (*Cajanus cajan*), a drought-tolerant legume, provides a non-animal source of protein, and contributes to soil fertility through nitrogen fixation (Kimaro et al. 2009). *Gliricidia sepium* is a nitrogen-fixing leguminous tree that can reduce erosion and improve soil organic matter. The tree is typically planted along field borders or interspersed throughout cropland. Since *Gliricidia* is integrated across cropped areas of the farm, nitrogen fixation and the incorporation of nutrient-rich biomass can improve soil fertility and organic matter across those areas (Beedy et al. 2010).

[1] See https://www.trade.gov/country-commercial-guides/tanzania-agriculture-and-agricultural-processing/ (accessed 23 January 2026).

Similarly, pigeonpea intercropping and improved fallows extend soil-fertility benefits to crops subsequently grown on the same land (Kimaro et al. 2009).

Experimental and field-based evidence from the region and elsewhere suggests that these systems can increase yields and reduce production risks. Kwesiga et al. (2003) report that in Zambia, maize yields following Gliricidia fallows reached 3.8 Mg ha^{-1} (against 2.0 Mg ha^{-1} under continuous unfertilized maize), and 2-year pigeonpea fallows in Chipata raised subsequent maize yields by an average of 1.7 Mg ha^{-1}, illustrating the whole-plot productivity effect of agroforestry practices. By integrating trees and crops, agroforestry systems may allow land-constrained households to intensify production, diversify income sources, and buffer against climate-related shocks. At the same time, the costs and benefits of these systems are likely to vary across households depending on land availability, labor constraints, and market access. This makes Tabora a suitable setting for examining heterogeneous adoption outcomes.

Before turning to the empirical strategy, we outline the mechanism, or theory of change, through which agroforestry adoption is expected to affect household crop value in Tabora. Extension contact increases the likelihood of adoption; households that adopt reallocate land, labor, and management toward planting *Gliricidia sepium* or intercropping cassava with pigeonpea. These practices operate at the plot-level and potentially raise yields for all crops grown on the affected land. The predicted impact is therefore an increase in the value of household crop production per adult member. Offsetting this benefit are opportunity costs that shape the selection into adoption, an issue we address in the empirical strategy that follows.

Methods

Treatment evaluation framework

We are interested in the causal effect of agroforestry adoption on household crop production. Let $D_i \in \{0, 1\}$ denote whether household i has adopted agroforestry, and let V_i denote the value of crop production. Following the standard potential-outcomes framework (Imbens and Wooldridge 2009), we define $V_i(1)$ and $V_i(0)$ as the household's potential outcomes under adoption and non-adoption respectively, with the realized outcome given by $V_i = D_i V_i(1) + (1 - D_i)V_i(0)$. The main effects of interest are the average treatment effect (ATE), the treatment effect on the treated (ATT), and the treatment effect on the untreated (ATU).

The central identification challenge is that adoption is unlikely to be exogenous to the outcome equation. Households self-select into agroforestry based on factors such as land quality, managerial ability, household assets, and farming experience, that also affect agricultural production (see, e.g., Di Falco et al. 2011). Not all such factors are observable in our data. Conditioning on observable covariates alone is therefore insufficient if the unobservable component of the adoption decision is systematically related to potential outcomes.

Figure 1 summarizes the identification problem and our empirical strategy in directed acyclic graph (DAG) form. The parameter of interest is the causal effect of adoption D_i on crop value V_i, represented by the arrow $D_i \rightarrow V_i$. The observable covariates X_i enter both the adoption decision and the outcome equation and are controlled for by conditioning. The unobservable component U_i also enters both equations, opening a back-door path $D_i \leftarrow U_i \rightarrow V_i$ that estimators conditioning on X_i alone cannot close.

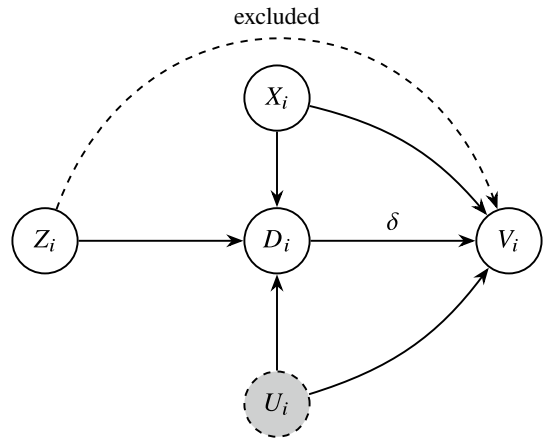


Fig. 1 Directed acyclic graph for the identification strategy. Solid arrows denote assumed causal relationships; the dashed arrow from Z_i to V_i is the path ruled out by the exclusion restriction. X_i denotes observable household and farm characteristics; U_i denotes unobservable productivity-related factors. The parameter of interest is δ, the effect of agroforestry adoption D_i on per-adult crop value V_i

Our instrument, Z_i, enters the adoption equation but is assumed not to affect V_i directly conditional on (X_i, D_i). This excluded path is shown dashed in Fig. 1 and constitutes our exclusion restriction.

Main specification: Poisson control function

The dependent variable V_i is non-negative and right-skewed, with a long upper tail. A Poisson quasi-maximum-likelihood estimator (QMLE) with log link is well-suited to such outcomes. Note that the outcome does not need to be a count variable. We therefore model the conditional mean of per-adult crop value V_i multiplicatively in household and farm-level covariates X_i, the binary adoption indicator D_i, and an unobserved outcome-equation component η_i:

$$\log E\big[V_i \,\big|\, X_i, D_i, \eta_i\big] \;=\; X_i\beta + \delta D_i + \eta_i, \tag{1}$$

where δ is the parameter of interest. By the multiplicative structure of (1), $\exp(\delta) - 1$ is the proportional change in the conditional mean of V_i associated with a switch from non-adoption to adoption, holding X_i and η_i fixed.

Adoption is potentially endogenous in (1) because η_i may be correlated with unobservable determinants of the adoption decision. We address this with a control-function approach (Wooldridge 2015), also referred to as two-stage residual inclusion (Terza et al. 2008). The adoption decision is formulated as a discrete realization of a latent index,

$$D_i^* \;=\; X_i\alpha + \pi Z_i + v_i, \qquad D_i \;=\; \mathbf{1}\big\{D_i^* > 0\big\}, \tag{2}$$

where Z_i is the excluded instrument and $v_i \sim N(0,1)$. The endogeneity of D_i in (1) arises when v_i and η_i are correlated, in which case D_i is correlated with η_i through the latent error of the adoption equation.

In the first stage, we estimate (2) by probit maximum likelihood. The estimated generalized residual is

$$\hat{u}_i \;=\; D_i \frac{\phi(\hat{a}_i)}{\Phi(\hat{a}_i)} \;-\; (1 - D_i)\frac{\phi(\hat{a}_i)}{1 - \Phi(\hat{a}_i)}, \qquad \hat{a}_i = X_i\hat{\alpha} + \hat{\pi}Z_i, \tag{3}$$

where $\phi(\cdot)$ and $\Phi(\cdot)$ denote the standard normal density and cumulative distribution function (Gourieroux et al. 1987). Under joint normality of the adoption and outcome unobservables, the conditional expectation of the outcome unobservable, η_i, is proportional to this residual. In the control function strategy, the generalized residual therefore acts as a sufficient statistic.

In the second stage, we estimate the following conditional mean by Poisson QMLE, using the estimated generalized residual $\hat{u}_i$ from (3):

$$\log E\big[V_i \,\big|\, X_i, D_i, \hat{u}_i\big] \;=\; X_i\beta + \delta D_i + \gamma\hat{u}_i. \tag{4}$$

The Poisson QMLE is consistent under correct specification of the exponential conditional mean and standard regularity conditions, without further distributional assumptions on V_i (Mullahy 1997; Wooldridge 2014). A similar procedure has been applied to agricultural-adoption settings with non-negative, right-skewed outcomes by, among others, Vaiknoras and Larochelle (2021) and Martínez et al. (2025).[2]

Within this control-function specification (Poisson-CF), the inclusion of $\hat{u}_i$ in (4) serves two purposes. First, a test of $\gamma = 0$ provides a test of the null that adoption is exogenous in the outcome equation. When $\hat{\gamma} > 0$, the evidence suggests positive selection into adoption based on unobserved productivity, whereas $\hat{\gamma} < 0$ suggests negative selection. Second, conditional on $\hat{u}_i$, the residual variation in D_i is uncorrelated with the unobserved outcome-equation component η_i, so δ can be interpreted as the effect of adoption on the log conditional mean of V_i under the maintained control-function assumptions.

Treatment effects on the level of V_i are recovered from the second-stage estimates. The individual-level predicted effect is

$$\hat{\mu}_i(1) - \hat{\mu}_i(0) \;=\; \exp\big(X_i\hat{\beta} + \hat{\gamma}\hat{u}_i\big)\big[\exp(\hat{\delta}) - 1\big], \tag{5}$$

where $\hat{\mu}_i(d) = \exp(X_i\hat{\beta} + \hat{\delta}d + \hat{\gamma}\hat{u}_i)$ denotes the predicted potential outcome under treatment status $d \in \{0, 1\}$. The ATE, ATT, and ATU are obtained by averaging (5) over the full estimation sample, the subsample of adopters, and the subsample of non-adopters, respectively, holding fixed the estimated control for latent selection $\hat{u}_i$. Note that these treatment effects

[2] A GMM-based instrumental-variable estimator of the Poisson model (Mullahy 1997) provides an alternative correction without requiring joint normality of the unobservables, but does not yield a direct test of endogeneity through a generalized residual. An example of this approach is Hirvonen and Hoddinott (2017) who apply it to study the effect of agricultural production on child diets in rural Ethiopia.

are in level units of per-adult crop value, whereas the corresponding conditional proportional effect implied by the log-link coefficient is $\exp(\hat{\delta}) - 1$.

Standard errors for the treatment-effect parameters are obtained by a non-parametric bootstrap with 1,000 replications of the full two-step procedure, which accounts for sampling variation in both stages including the generated-regressor variability in $\hat{u}_i$.

Complementary estimators. To complement the Poisson-CF estimator, we also report a linear two-stage least-squares (2SLS) estimator that uses the same covariates X_i and excluded instrument Z_i but does not impose the multiplicative conditional-mean structure of equation (4). With heterogeneous treatment effects, 2SLS identifies the local average treatment effect (LATE) for complying households, which are households whose adoption decision is shifted by extension exposure. This provides both a robustness check on the Poisson-CF and a parameter of independent policy interest, since it characterizes the effect of adoption among the marginal households whose decisions are most likely to be moved by extension-based promotion. We furthermore benchmark the Poisson-CF and 2SLS estimates against estimators that condition on observable covariates only and therefore identify the population treatment effect under the stronger assumption that selection is on observables alone.

Exclusion restriction and instrument validity

Our instrument, Z_i, is an indicator equal to one if the household reported any contact with agricultural extension agents in the twelve months prior to receiving the questionnaire, and zero otherwise. Extension services in the study area focus on disseminating technical knowledge and promoting specific agronomic practices, and we therefore expect contact with extension agents to influence adoption primarily by reducing information costs. Similar information-based variables are commonly used as instruments in related agricultural technology adoption literature (Di Falco et al. 2011; Asfaw et al. 2012; Shiferaw et al. 2014; Khonje et al. 2015; Coulibaly et al. 2017).

We assess instrument relevance and the credibility of the exclusion restriction in several ways. Our primary statistic is weak-instrument-robust inference based on the Anderson–Rubin (AR) test (Andrews et al. 2019), which is asymptotically valid under arbitrarily weak identification. The AR test and confidence set are computed on the linear 2SLS model and therefore characterize the LATE; inference on the Poisson-CF parameters is provided by the bootstrap procedure described in Sect. Main specification: Poisson control function. We also report the partial first-stage F statistic from the linear analogue of the first stage probit.[3] To assess the exclusion restriction, we conduct falsification tests using the subsample of non-adopters. Conditional on non-adoption, the instrument should not predict the outcome if it affects the outcome only through adoption (Di Falco et al. 2011). Finally, we report the local-to-zero (LTZ) bounds of Conley et al. (2012) to examine the sensitivity of the IV estimates to plausibly exogenous deviations from the strict exclusion restriction.

Data

Data were collected through a household survey administered in 2019 in six rural villages of Tabora Region, Tanzania, where the World Agroforestry Centre (ICRAF) had previously disseminated information about agroforestry practices including *Gliricidia sepium*, pigeonpea, and cassava. Tabora is located approximately 800 kilometres west of Dar es Salaam and is characterized by a unimodal rainfall pattern, with average annual precipitation of about 902 millimeters and an average annual temperature of 23.1°C. Agriculture in the region is predominantly rain-fed and smallholder-based; crops are produced both for local markets and for household subsistence.

The questionnaire collected detailed information on agroforestry practices, agricultural production, household and farm characteristics, and the household's interactions with formal sources of agricultural information. Following a pilot survey conducted between February and March 2019, the final survey was implemented from July to August 2019 in the

[3] The conventional comparison is to the Stock–Yogo critical value of 16.4, which corresponds to a maximum 5% asymptotic bias from a single instrument (Stock and Yogo 2005). Recent work argues that under heteroskedasticity-robust standard errors, substantially higher F-values are required for Wald-based inference to control test size (Lee et al. 2022; Keane and Neal 2023). This motivates our use of AR-robust inference as the primary statistic.

villages of Mbola, Isila, Msiliembe, Mbiti, Ibiri, and Migungumalo. These villages were selected because agroforestry interventions had been introduced prior to the survey, making adoption a feasible choice for the surveyed households. With the assistance of local guides we compiled a census of farming households across the six villages, from which 550 households were drawn using village-stratified random sampling. Of these, 420 households consented to participate in the survey; the principal reason for non-participation was the temporary absence of the household head, typically due to farming activities or travel, rather than refusal to participate.

Total crop value and prices

The outcome variable, V_i, is the value of household crop production per adult household member.[4] To capture both the direct and indirect effects of agroforestry, we sum crop value across the commonly farmed crops rather than measuring the yield and value of a single crop. This choice reflects two features of agroforestry practices in the study area (Sect. Study area and context). First, the soil-fertility and moisture-retention benefits of Gliricidia and legume intercropping can accrue to all crops grown on the affected land. Second, a broader outcome also captures land- and labor-allocation trade-offs. The measure we use therefore reflects the net impact of adoption on household crop production. One notable exception is tobacco, which we exclude from the outcome measure.[5] The measure also excludes livestock value since livestock is a household asset (a stock) rather than a within-season production flow. Moreover, the agroforestry practices promoted in the study area are targeted at cropping systems rather than fodder or feed provision.[6]

Table 1 Prices for crops per kilogram

Crop	Average price (USD/kg)
Millet	0.03
Squash	0.03
Watermelon	0.04
Pigeonpea	0.05
Tomato	0.06
Tree fruit	0.09
Bambara nut	0.10
Green vegetables	0.10
Cassava	0.12
Yams	0.12
Sunflower	0.13
Cowpea	0.14
Rice	0.20
Peanut	0.67
Maize	0.94

Typical food crops and their average prices based on the reported values by the households (converted to USD)

Households reported quantities harvested for each commonly farmed crop during the most recent agricultural season, together with the typical price received per kilogram. Quantities were valued at the reported price and converted from Tanzanian shillings to U.S. dollars at the 2019 exchange rate of 2,300 TSH per dollar, then summed across crops and divided by the number of adult household members. The per-adult normalization accounts for differences in household size and on-farm labor availability across households. Table 1 lists the common crops and their average price in the sample.

Estimation sample and summary statistics

The estimation sample consists of the 373 households for which D_i, V_i, Z_i, and the full vector X_i are non-missing. The treatment variable, D_i, equals one for households that have adopted any of the agroforestry

[4] In Tanzania, adulthood is legally defined as beginning at age 18, so we classify any household member aged 18 or older as an adult.

[5] Tobacco is the principal cash crop in Tabora Region. It is typically produced under contract-farming arrangements with multinational buyers, governed by external buyer relationships rather than household agronomic decisions. Adopters and non-adopters report similar per-adult tobacco values in our sample ($57 and $61 respectively).

[6] Agrosilvopastoral systems integrate trees, crops, and livestock on the same land parcel. In the case of intercropping and tree adoption in the case study, households were not widely practicing cut and carry or alternative fodder systems. The purpose of agroforestry systems, in this context, was to increase overall soil quality (reduce erosion, increase soil moisture and nutrient retention), and in the case of pigeonpea, provide a non-animal source of protein.

Table 2 Descriptive statistics by adoption status

	Non-adopters ($N = 193$)		Adopters ($N = 180$)		All ($N = 373$)	
	Mean	SD	Mean	SD	Mean	SD
Outcome						
Crop value per adult (USD)	253.9	398.0	202.2	434.8	229.0	416.4
Total crop value (USD)	958.6	1148.2	685.2	877.3	826.7	1034.1
Household and farm characteristics						
Farm size (acres)	6.77	12.89	8.49	7.98	7.60	10.82
Farming hours per week	20.05	16.96	19.87	18.69	19.96	17.79
Travel time to Tabora Town (hr)	4.45	5.04	5.96	4.55	5.17	4.86
Years of education (head)	8.00	3.01	7.93	2.99	7.97	3.00
Adult household members	5.76	3.61	5.44	3.37	5.61	3.50
Female adult share	0.558	0.190	0.502	0.178	0.531	0.186
Dependency ratio	0.90	1.09	0.85	0.73	0.87	0.93
Cement floor (0/1)	0.135	0.342	0.117	0.322	0.126	0.332
Instrument						
Extension contact (0/1)	0.352	0.479	0.556	0.498	0.450	0.498

The outcome is the dollar value of household crop production per adult household member. The treatment is an indicator for adoption of any agroforestry practice promoted in the study area. The instrument is an indicator for any household contact with extension agents. Years of education refers to the household head; the dependency ratio is the number of children per adult household member

practices promoted in the study area: planting *Gliricidia sepium* trees, intercropping pigeonpea with cassava, or both. Exposure to extension services serves as the instrumental variable. Extension contact is strongly correlated with adoption, and the validity of the exclusion restriction is assessed formally in Sect. Results. The household and farm-level covariates X_i are intended to capture the most important observable correlates of household productive capacity, market access, and demographic structure. The covariates include: farm size in acres, weekly farming hours, the round-trip travel time in hours from the household to Tabora Town (a major commodity market), years of education of the household head, the share of adult household members who are female, the household dependency ratio (number of children per adult), an indicator for the presence of a cement floor in the dwelling (a standard wealth proxy in this setting), and a full set of village fixed effects.[7]

Within the estimation sample, 193 households (51.7 percent) are non-adopters and 180 (48.3 percent) are adopters.[8] Among adopters, 44 percent have planted *Gliricidia sepium* trees, while 35 percent are intercropping pigeonpea with cassava; the rest have adopted both practices. Table 2 reports descriptive statistics for the variables used in the empirical analysis, separately by adoption status and for the full estimation sample. Adopters cultivate larger farms (8.5 acres on average, against 6.8 acres for non-adopters), live noticeably farther from Tabora Town (an average of 6.0 h of round-trip travel, against 4.5 h), and contain a smaller share of adult women (50 percent against 56 percent). They are slightly less likely to live in dwellings with cement floors, although the difference is small. Years of education of the household head and the dependency ratio are similar across the two groups on average.

Despite their larger farms, adopters report lower per-adult crop production on average than non-adopters: \$202 against \$254 per adult member, a

[7] The dependency ratio is winsorized at six children per adult (99th percentile) to reduce sensitivity to a small number of households with very few adults and many reported children; the qualitative findings are unchanged in the unwinsorized specification.

[8] This adoption rate is broadly within the range reported in comparable studies. Asfaw et al. (2012) report adoption rates of 32 percent for chickpea and 33 percent for pigeonpea; Khonje et al. (2018) report 12 percent for conservation agriculture and 32 percent for improved maize varieties; and Coulibaly et al. (2017) report that 51 percent of project participants continued managing fertilizer trees for at least five continuous years. A recent scoping review by Kpoviwanou et al. (2024) finds that reported adoption rates vary widely across African countries and study contexts.

difference of approximately twenty percent. Total crop value at the household level is also lower among adopters ($685 against $959), and average household size is similar across groups. The rate of contact with extension services is markedly higher among adopters (56 percent) than among non-adopters (35 percent), consistent with extension exposure operating as a channel through which households are introduced to the agroforestry practices promoted in the study area.

Summary statistics by agroforestry adoption type

In Appendix 1, we report descriptive statistics for the four mutually exclusive adopter categories in the estimation sample. The comparison reveals some interesting patterns. First, extension contact is more common among adopters, with exposure rising from 35 percent among non-adopters to between 51 and 60 percent across the three adopter subgroups. Second, adopters of intercropping only have a much lower observed average crop value per adult ($131) than Gliricidia-only adopters ($261) or those who adopted both ($199). Within the adopter group, however, a joint F-test cannot reject the null that mean crop value per adult is equal across the three practice subgroups ($F(2, 177) = 2.18$, $p = 0.12$). This provides descriptive support for pooling the practices into a single binary adoption indicator in the main analysis.[9] Note that pooling the practices has an implication for the interpretation of the treatment effects that follow. More specifically, the estimated ATT reflects the observed composition of Gliricidia-only, intercropping-only, and joint adoption in our sample. Beyond crop value, the appendix table reveals some interesting differences in farm and household characteristics across practice types. For example, households that adopted both practices, and to a lesser extent those adopting intercropping only, cultivate larger farms on average than non-adopters or Gliricidia-only adopters (10.6 and 9.6 acres compared with 6.8 and 6.6). Intercropping-only households additionally report fewer weekly farming hours (15.3) than the rest of the sample.

[9] Note that the subgroup sizes in our estimation sample (79 Gliricidia-only, 63 intercropping-only, and 38 joint adopters) would render a separate causal assessment of each practice too imprecise for reliable inference. We hence leave it for future work using larger samples.

Table 3 First-stage probit estimates for agroforestry adoption

	Coefficient	Std. error
Extension contact (Z)	0.572***	(0.143)
Farm size (acres)	− 0.004	(0.006)
Farming hours per week	− 0.003	(0.004)
Travel time to Tabora town (hr)	0.037***	(0.014)
Years of education (head)	− 0.003	(0.023)
Female adult share	− 1.105***	(0.390)
Dependency ratio	0.013	(0.078)
Cement floor (0/1)	− 0.037	(0.222)
Mbiti	0.041	(0.235)
Mbola	0.125	(0.200)
Msiliembe	0.166	(0.253)
Isila	0.158	(0.237)
Ibiri	− 0.300	(0.210)
Constant	0.167	(0.307)
N	373	
LR $\chi^2(13)$	39.57	
Pseudo R^2	0.077	

Probit regression of an indicator for any agroforestry adoption on the exclusion restriction Z (an indicator for any household contact with extension services in the twelve months prior to survey dissemination) and the household and farm-level covariates. Standard errors in parentheses. Migungumalo is the omitted village. Significance: $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Results

First- and second-stage estimates

Tables 3 and 4 report the first- and second-stage coefficient estimates from the Poisson control-function specification.[10] In the first stage (Table 3), household contact with extension agents is strongly associated with adoption: the probit coefficient on the exclusion restriction is +0.57 ($p < 0.001$), confirming the relevance of the instrument and corresponding to the partial first-stage F statistic of 16.1 reported below. Among the household and farm-level covariates, the share of female adults in the household enters with a large negative coefficient (−1.10, $p < 0.01$) and travel time to Tabora Town enters positively (+0.04, $p < 0.01$), indicating that more remote and male-skewed households are more likely to adopt. The rest

[10] We use Stata 19.0 to estimate the models.

Table 4 Second-stage Poisson control-function estimates

	Coefficient	Std. error
Adoption (D)	1.393**	(0.597)
Generalized residual ($\hat{u}$)	− 0.844**	(0.376)
Farm size (acres)	0.022***	(0.008)
Farming hours per week	0.030***	(0.003)
Travel time to Tabora town (hr)	− 0.093***	(0.021)
Years of education (head)	0.002	(0.018)
Female adult share	1.487***	(0.383)
Dependency ratio	0.202***	(0.051)
Cement floor (0/1)	0.325**	(0.158)
Mbiti	0.350**	(0.178)
Mbola	− 0.604**	(0.238)
Msiliembe	− 2.266***	(0.315)
Isila	0.160	(0.189)
Ibiri	0.422***	(0.157)
Constant	3.140***	(0.487)
N	373	
Log pseudo-likelihood	−32,488.3	

Second-stage Poisson quasi-maximum-likelihood regression of per-adult crop value on the household and farm-level covariates, the adoption indicator D, and the generalized residual $\hat{u}$ from the first-stage probit reported in Table 3. The link function is the log, so reported coefficients are on the log of the conditional mean. Heteroskedasticity-robust standard errors in parentheses. Significance: *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$

of the covariates are not significantly associated with adoption conditional on the other covariates.

In the second stage (Table 4), the coefficient on the adoption indicator is $\hat{\delta} = 1.39$ ($p = 0.02$), implying a multiplicative effect of $\exp(\hat{\delta}) - 1 \approx 303$ percent on the conditional mean of per-adult crop value at fixed covariates. The generalized residual enters with $\hat{\gamma} = -0.84$ ($p = 0.03$), formally rejecting the exogeneity of D and indicating that households whose unobserved characteristics raise the propensity to adopt have lower crop production absent the intervention. Among the covariates, larger farms, more on-farm labor, a higher share of female adults, and a higher dependency ratio are each associated with higher per-adult crop value, while greater distance from Tabora Town reduces it. Education of the household head is not significantly associated with crop value conditional on the other covariates.

Main treatment-effect estimates

Table 5 reports our main estimates. Column (1) reports the naive Poisson QMLE that conditions on the same observable covariates as the main specification but omits the generalized-residual correction. The coefficient on adoption (D) is 0.070 ($p = 0.553$), and the implied treatment effects in levels are small and not statistically significant: \$16 (ATE), \$14 (ATT), and \$18 (ATU).

Column (2) reports the Poisson control-function estimates. The coefficient on D is 1.393, with a bias-corrected percentile 95% bootstrap confidence interval of [0.21, 3.23] that is bounded above zero. Note that the bootstrap distribution of the Poisson-CF treatment-effect parameters is right-skewed, so we report bias-corrected percentile bootstrap intervals.[11] The implied proportional effect $\exp(\hat{\delta}) - 1 \approx 3.03$ characterizes the multiplicative increase in the conditional mean of per-adult crop value at fixed covariates and latent selection. The average treatment effect on the treated is ATT = \$152 per adult household member (BC 95% CI [\$57, \$236]).

The Poisson-CF also implies an ATE of \$471 and an ATU of \$769, but the bootstrap distributions of these estimands are wide: the corresponding BC 95% intervals are [\$51, \$3,368] for the ATE and [\$51, \$5,784] for the ATU. The ATE and ATU should therefore be read as model-based extrapolations rather than as precise causal quantities. Column (3) reports the 2SLS local average treatment effect of \$412 ($p = 0.025$), which identifies the effect of adoption among the subset of households whose adoption decision is shifted by extension exposure.

Columns (4) and (5) report two additional estimators that condition on observables only. The augmented inverse propensity weighted (AIPW) estimator gives an ATE of −\$11 ($p = 0.77$) and an ATT essentially zero; propensity score matching (PSM) with three nearest neighbours gives an ATT of \$47 ($p = 0.11$).

[11] The bootstrap resamples within village strata to preserve the village-stratified sampling design of the survey. The village fixed effects in X_i absorb between-village differences in the conditional mean.

Table 5 Treatment effect of agroforestry adoption on per-adult crop value

	(1) Naive Poisson	(2) Poisson-CF	(3) 2SLS	(4) AIPW	(5) PSM
Coefficient on *D*	0.070	1.393**	–	–	–
	(0.118)	(0.770)			
Generalized residual $\hat{u}$	–	− 0.844**	–	–	–
		(0.479)			
Treatment effects (USD per adult member)					
ATT	14.1	152.0**	–	− 2.3	47.5
	(24.9)	(48.5)		(34.1)	(29.9)
LATE	–	–	411.5**	–	–
			(184.0)		
First-stage F	–	–	16.1	–	–
Endogeneity test (*p*)	–	0.025	0.007	–	–
N	373	373	373	373	373

Column (1) reports the Poisson QMLE with log link and the same covariates as the main specification but no generalized-residual correction. Column (2) reports the second stage of equation (4). Column (3) reports the 2SLS coefficient on *D*, which identifies the local average treatment effect for compliers. AIPW (column 4) uses linear outcome and logit treatment models with the same covariates as the Poisson-CF. PSM (column 5) uses logit propensity scores and three nearest neighbours; one-nearest-neighbour matching gives an essentially identical ATT of $47 (s.e. $32, $p = 0.14$). Standard errors for the Poisson-CF (column 2) are from a non-parametric bootstrap with 1,000 replications of the full two-step procedure (first-stage probit, second-stage Poisson QMLE, and computation of treatment-effect parameters); significance stars in column (2) are based on bias-corrected percentile bootstrap confidence intervals. Standard errors in the remaining columns are heteroskedasticity-robust. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Robustness

To verify that the point estimates are not driven by a small number of high-output households in the right tail, Table 6 repeats the analysis with the outcome winsorized at the 95th percentile. The Poisson-CF coefficient on adoption falls modestly from 1.393 to 1.380, and the ATT falls from $152 to $124 per adult member (BC 95% CI [$35, $168]). The 2SLS LATE drops in parallel from $412 to $319 ($p = 0.017$), and the generalized residual continues to enter with a large, negative, and statistically significant coefficient (BC 95% CI [−1.91, −0.21]). The three observables-only estimators (naive Poisson, AIPW, PSM) all remain statistically indistinguishable from zero, with point estimates close to zero in dollar terms.

Table 6 Robustness: outcome winsorized at the 95th percentile

	(1) Naive Poisson	(2) Poisson-CF	(3) 2SLS	(4) AIPW	(5) PSM
Coefficient on *D*	− 0.018	1.380**	–	–	–
	(0.106)	(0.707)			
Generalized residual $\hat{u}$	–	− 0.889**	–	–	–
		(0.433)			
Treatment effects (USD per adult member)					
ATT	− 2.9	123.6**	–	− 20.8	18.2
	(17.4)	(31.2)		(20.5)	(19.0)
LATE	–	–	319.1**	–	–
			(134.1)		
First-stage F	–	–	16.1	–	–
N	373	373	373	373	373

The outcome is winsorized at its 95th percentile. All other definitions and conventions follow Table 5

Table 7 Instrument validity diagnostics

	Falsification regressions (non-adopters only, $N = 193$)		First stage (full sample)
	OLS in levels	Poisson-link	
Coefficient on Z	52.53	0.207	0.572***
Standard error	(59.4)	(0.161)	(0.143)
p-value	0.378	0.197	< 0.001
First-stage partial F			16.10
Stock–Yogo (5% bias)			16.4
Endogeneity test (χ^2, p)			7.33 (0.007)
Weak-instrument-robust inference (Anderson–Rubin)			
AR test (χ^2, p)			7.24 (0.007)
AR 95% confidence set (USD)			[112.8, 972.6]
Wald 95% confidence interval			[50.9, 772.2]

Falsification regressions use the subsample of non-adopters and regress the level (Poisson-link) outcome on the instrument and the same covariates as the main specification. The first-stage column reports the probit coefficient on Z in the adoption equation (s.e. in parentheses); the partial F is from the linearized first stage of the 2SLS estimator with robust standard errors. The endogeneity test is the heteroskedasticity-robust score test on D. The Anderson–Rubin test and confidence set are computed using weakiv (Finlay et al. 2013). Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Instrument validity

We examine the relevance and validity of the instrument in Table 7.[12] Inference on the treatment effect for the 2SLS LATE is based on the weak-instrument-robust Anderson–Rubin (AR) test, which is asymptotically valid under arbitrarily weak identification (Andrews et al. 2019). The AR test rejects the null of no treatment effect at $p = 0.007$, with a 95% AR confidence set of [\$113, \$973]. The set is wider than the corresponding Wald interval of [\$51, \$772], but it is bounded well away from zero and contains the 2SLS point estimate.

The partial first-stage F statistic of 16.1 provides support for the AR-based conclusion. It exceeds the conventional rule-of-thumb threshold of 10 and falls marginally below the Stock–Yogo critical value of 16.4 for a maximum 5% asymptotic bias from a single instrument. However, recent work argues that conventional Stock–Yogo thresholds are insufficient to control test size when standard errors are heteroskedasticity-robust (Lee et al. 2022; Keane and Neal 2023). We therefore adopt AR-robust inference as our primary approach.

Two further diagnostics provide support for the IV strategy. The first-stage probit coefficient on extension contact is +0.572 ($p < 0.001$), which confirms instrument relevance. Second, the heteroskedasticity-robust score test rejects the null that D is exogenous in the linear 2SLS model at $\chi^2(1) = 7.33$ ($p = 0.007$). This corroborates the test on the generalized residual in the Poisson-CF, but under a different functional form.

The exclusion restriction is examined in two complementary falsification tests on the subsample of non-adopters. Among the 193 non-adopting households, the level-scale OLS coefficient on the instrument is \$52.5 (s.e. \$59.4, $p = 0.378$, with a 95% confidence interval of approximately [−\$64, \$169]), and the Poisson-link analogue under the same functional form as the main specification is 0.207 (s.e. 0.161, $p = 0.197$). Neither test rejects the null that extension contact is uncorrelated with crop production conditional on non-adoption. The tests are, however, imprecise: with only 193 non-adopters, the 95% confidence interval on the level-scale falsification coefficient does not rule out a direct effect of extension contact on per-adult crop value of up to roughly \$169 which is comparable in magnitude to the Poisson-CF ATT of \$152, and a substantial fraction of the 2SLS LATE of \$412.

The falsification test therefore provides evidence consistent with the exclusion restriction, but since the test is under-powered, we lean more on

[12] We follow the recommendations of Henningsen et al. (2026) for IV-based identification in agricultural and applied economics.

the plausibly-exogenous bounds reported in Table 8. The local-to-zero (LTZ) procedure of Conley et al. (2012) relaxes the assumption that the instrument has no direct effect on the outcome. Instead, it allows the instrument to have a small direct effect, which is assumed to be centered at zero with 95% of the prior support lying within $\pm\omega$. We vary ω from \$0 to \$250 per adult. This range includes the upper bound of the falsification test's 95% confidence interval. Even at $\omega = \$250$, the estimated LATE remains positive and statistically significant, with a 95% confidence interval of [\$23, \$800]. The positive estimated effect of agroforestry adoption is therefore robust to non-trivial exclusion-restriction violations.

In summary, although the AR confidence sets are wide and the IV identification rests on an exclusion restriction that cannot be verified directly, the results remain robust under weak-instrument-robust inference and to non-trivial relaxations of the strict exclusion restriction. The falsification tests on the non-adopter subsample are consistent with the exclusion restriction under both functional forms. However, since the falsification tests are imprecise and cannot rule out a substantial direct effect of the instrument on the outcome, we rely on the Conley plausibly-exogenous bounds. The most conservative lower bound on the treatment effect is \$23 per adult member from the Conley interval at $\omega = \$250$, while the AR interval implies a lower bound of \$113.

Table 8 Plausibly-exogenous bounds on the LATE

Prior support of direct effect ($\pm\omega$, USD)	0	100	150	200	250
LATE point estimate	411.5	411.5	411.5	411.5	411.5
Standard error	(184.0)	(189.9)	(192.7)	(195.5)	(198.3)
p-value	0.025	0.030	0.033	0.035	0.038
95% CI lower bound	50.9	39.4	33.8	28.3	22.8
95% CI upper bound	772.2	783.7	789.3	794.8	800.2

Local-to-zero (LTZ) treatment-effect estimates from Conley et al. (2012), implemented in Stata via plausexog ltz. The procedure assumes a uniform prior on the direct effect of the instrument on the outcome with mean zero and 95% support of $\pm\omega$. The first column ($\omega = 0$) reduces to the standard 2SLS estimator. All other definitions and conventions follow Table 5

Discussion

Results and mechanisms

The negative-selection finding indicates that unobserved factors increasing the likelihood of agroforestry adoption are negatively associated with crop-value productivity per adult household member, conditional on observed covariates. The positive and statistically significant ATT estimate suggests that agroforestry is associated with substantive productivity gains for the population of adopters. When combined with the descriptive evidence that adopters cultivate larger but more remote farms and have lower per-adult crop value than non-adopters, these results are consistent with agroforestry functioning as a productivity-enhancing and constraint-relaxing strategy, particularly for households with limited alternative income options.

We interpret this pattern through a Roy-style comparative-advantage framework (see, e.g., Suri 2011). In this framework, households select into adoption based on heterogeneous net returns rather than average returns alone. Households with higher productivity under existing livelihood strategies may still choose not to adopt if agroforestry entails high opportunity costs, such as foregone returns from alternative land uses, including livestock husbandry, or competing demands on labor and managerial attention. By contrast, households facing soil degradation, long distances from commodity markets, or limited integration with cash-crop value chains may have stronger incentives to adopt because agroforestry helps stabilize crop production and consumption, diversify livelihood returns, and improve soil functioning.

The crop-value variable captures both consumed and sold crop production, so the gains we estimate among adopters reflect both potential cash income and the value of subsistence production. In Tabora, food insecurity typically occurs from October to February and on-farm activities account for the bulk of household labor. Thus, technologies that enable more stability to own-production can offer direct welfare improvements via consumption smoothing. Households facing food-security constraints have a stronger incentive to adopt practices that reduce downside risk, even at the cost of lower expected market returns. The negative-selection result is consistent with this logic: the households that self-select into adoption are those

for whom the insurance value of stabilizing on-farm production is relatively high, while households whose comparative advantage lies in market production sort out of agroforestry adoption.

Several mechanisms can plausibly account for the substantive size of the estimated treatment effects. Agroforestry systems based on *Gliricidia sepium*, pigeonpea, and cassava deliver agronomic benefits that are particularly valuable on degraded smallholder plots. Nitrogen fixation by leguminous trees increases soil fertility in the absence of commercial fertilizers, the drought tolerance of cassava and pigeonpea reduces the variance of output under the increasingly erratic rainfall patterns of the region, and the diversification of the cropping system reduces the household's exposure to the failure of any single staple. Existing meta-analyses of nitrogen-fixing agroforestry on smallholder plots in sub-Saharan Africa document yield gains of comparable magnitude in similar agroecological settings (Sileshi et al. 2008; Akinnifesi et al. 2010).

Although our results point to a negative-selection pattern, some households that would benefit from agroforestry may nonetheless be prevented from adopting. Prior reviews have examined constraints on agroforestry adoption (Pattanayak et al. 2003; Tranchina et al. 2024), identifying factors such as pests, limited land access, insufficient knowledge and skills, and a lack of capital and seeds (Kpoviwanou et al. 2024), as well as social norms and institutional barriers (Zabala et al. 2025). Our own data point to extension coverage as a driver of adoption, potentially lowering barriers such as insufficient knowledge and skills. Yet, only 45 percent of households in the estimation sample had any extension contact in 2019, and the strong first-stage relationship suggests that expanding this coverage could help increase adoption.

When comparing our findings against similar ESR-based studies of agricultural-technology adoption in sub-Saharan Africa, our estimated proportional treatment effect of 303 percent is considerably larger. For example, Asfaw et al. (2012) study improved pigeonpea adoption in Tanzania and find an ATT for log consumption expenditure per adult equivalent of 0.71, equivalent to approximately a 103-percent multiplicative increase in the conditional mean, with an analogous ATU of 0.69, approximately 99 percent. In another related study, Khonje et al. (2018) find an estimated effect of 132 percent from the joint adoption of conservation agriculture and improved maize varieties on per-capita household income. Coulibaly et al. (2017), in turn, report an ATT of approximately 35 percent on the value of food crops from adopting fertilizer trees.

However, comparisons across studies are complicated by differences in how response variables are defined. Our dependent variable is a production-side measure of crop value per adult household member, whereas other studies use broader income or consumption outcomes. Furthermore, the larger proportional effect in our analysis can be understood as reflecting strong negative selection into adoption. In our sample, households that practice agroforestry are predicted to have particularly low counterfactual crop value in the absence of adoption. Thus, the large percentage effect should be interpreted as a proportional increase from a low predicted counterfactual base, rather than as evidence that agroforestry generates unusually large gains in absolute dollar terms. The estimated ATT corresponds to an increase of $152 in crop value per adult household member, with the most conservative lower bound equal to $23.

Limitations

There are several important caveats and limitations of our analysis. First, the data are cross-sectional and capture only short- to medium-run effects of agroforestry interventions that were introduced. Panel data on the same households would allow future research to measure long-run productivity gains and to quantify dynamic dis-adoption decisions, neither of which is feasible with the available data.

Second, our treatment variable is a binary indicator of any adoption rather than a continuous measure of adoption intensity. Plot-level allocations to agroforestry are imperfectly captured in self-reported survey data, and we therefore cannot quantify how the magnitude of the treatment effect varies with the share of cultivated land devoted to agroforestry, nor identify the optimal scale of agroforestry implementation. Remotely-sensed plot data could allow future work to address this question directly.

Third, the outcome variable captures the value of agricultural production but does not include off-farm income. Although on-farm activities dominate labor allocation in the study area, agroforestry adoption may have second-order effects on the time households

allocate to non-farm work. Additional measurement of off-farm income, alongside richer information on adoption intensity, would allow future studies to test for substitution effects between agroforestry and the cash-crop-oriented production strategies pursued by non-adopting households.

Fourth, our identification rests on a moderately strong instrument and on an exclusion restriction that cannot be tested directly. Although the first-stage F statistic of 16.1 exceeds the conventional rule-of-thumb threshold and the AR-robust confidence sets are bounded well above zero, recent work suggests that conventional weak-instrument critical values may understate the F-values required for reliable Wald-based inference (Lee et al. 2022; Keane and Neal 2023). We therefore report AR-robust intervals as the primary inference statistic. The falsification tests on the non-adopter subsample and the Conley plausibly-exogenous bounds suggest that the qualitative findings are robust to non-trivial departures from these assumptions. Still, future work using stronger identification strategies would tighten these conclusions.

Finally, we recognize that Tanzania is undergoing a rapid socio-economic transformation. At the time of our survey, in 2019, many households in Tabora were still producing much of their crops for consumption or sale to local markets. Yet, medium to large-scale commercial farms are currently becoming more prominent across the country (Wineman et al. 2020). As a result, many rural Tanzanians are increasingly responding by either de-emphasizing agriculture in favor of non-farm opportunities, or are intensifying production through the use of improved seed varieties, agrochemicals, and machinery. Tanzania remains largely agrarian, yet increasingly the idea of "subsistence" farming is becoming a relic of the past. Future studies may emphasize how agroforestry is being utilized under new farming contexts to increase farming intensification, for particularly medium-sized commercial landowners.

Conclusion

Our analysis of agroforestry adoption among smallholder farmers in Tabora Region, Tanzania, yields two robust empirical findings. First, adopters are drawn disproportionately from households whose unobserved productivity lies below the population average. Second, those who have adopted experience an estimated gain of approximately $152 per adult member from adoption (BC 95% bootstrap CI [$57, $236]), with the lower bound from the most conservative sensitivity analysis at $23. In summary, agroforestry systems may serve as a productivity-recovery strategy under binding agronomic constraints, rather than displacing other livelihood activities such as off-farm work. These findings can serve as a baseline for evaluating potential benefits of agroforestry systems amid ongoing changes in Tanzania's largely agrarian economy.

Disclaimer The findings and conclusions in this paper are those of the authors and should not be construed to represent any official USDA or U.S. Government determination or policy.

Author contributions S.B.: Conceptualization, Data Curation, Formal Analysis, Investigation, Validation, Writing original draft, Writing review and editing O.P.K.: Conceptualization, Data Curation, Formal Analysis, Investigation, Methodology, Validation, Writing original draft, Writing review and editing

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Data availability The data used for this study are available through ICRAF Tanzania.

Declarations

Ethical approval Ethical approval was granted by the Institutional Review Board (IRB) at Oregon State University approval No. 8906. Additional approval was granted by the Tanzania Commission for Science and Technology (COSTECH) permit No. 2018-456-NA-2018-380

Appendix 1

See Table 9.

Table 9 Descriptive statistics by agroforestry practice type

	Non-adopter	Gliricidia only	Intercropping only	Both
Outcome				
Crop value per adult (USD)	253.9	260.8	130.8	198.8
	(398.0)	(588.3)	(166.6)	(350.1)
Total crop value (USD)	958.6	716.0	641.3	694.2
	(1148.2)	(998.7)	(774.4)	(781.2)
Household and farm characteristics				
Farm size (acres)	6.8	6.6	9.6	10.6
	(12.9)	(5.2)	(10.1)	(8.1)
Farming hours per week	20.0	23.2	15.3	20.4
	(17.0)	(18.4)	(15.8)	(22.2)
Travel time to Tabora Town (hr)	4.4	5.7	6.3	6.0
	(5.0)	(4.9)	(4.5)	(3.9)
Years of education (head)	8.0	7.5	8.4	8.2
	(3.0)	(3.2)	(2.8)	(2.8)
Adult household members	5.8	5.0	5.9	5.7
	(3.6)	(3.1)	(3.4)	(3.7)
Female adult share	0.56	0.52	0.48	0.49
	(0.19)	(0.17)	(0.17)	(0.20)
Dependency ratio	0.90	1.07	0.65	0.73
	(1.09)	(0.90)	(0.50)	(0.51)
Cement floor (0/1)	0.13	0.14	0.11	0.08
	(0.34)	(0.35)	(0.32)	(0.27)
Instrument				
Extension contact (0/1)	0.35	0.51	0.60	0.58
	(0.48)	(0.50)	(0.49)	(0.50)
Sample composition				
Observations	193	79	63	38
Share of estimation sample	51.7%	21.2%	16.9%	10.2%
Share of adopters	–	43.9%	35.0%	21.1%

Standard deviations in parentheses. Estimation sample of 373 households. Joint test of equality of crop value per adult means across the three adopter subgroups: $F(2, 177) = 2.18$, $p = 0.116$

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