Can Interventions Reduce Rural Poverty While Conserving the Environment? A Systematic Review of Evidence from Randomized Controlled Trials

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The authors have nothing to declare.

Declaration of generative AI in scientific writing

During the preparation of this work the authors used ChatGPT in order to improve the readability and language of the manuscript. After using this tool, the authors reviewed and edited the content as needed; we take full responsibility for the content of the published article.

CRediT authorship contribution statement

Adriana Molina-Garzón: conceptualization, data curation, investigation, formal analysis, writing – original draft preparation, methodology, review, editing, project administration, supervision. **Lakshmi Iyer**: conceptualization, writing - original draft preparation, writing - review and editing, validation, funding acquisition. **Phani Devarakonda**: data curation, formal analysis, writing - original draft preparation. **Valena McEwen**: data curation, investigation, formal analysis, writing - original draft preparation. **Braden Roosevelt**: data curation, investigation, formal analysis. **Nicolas Tsypin**: data curation, investigation, formal analysis. **Aemro Worku**: data curation, investigation, formal analysis, writing - original draft preparation. **Poulomi Ghosh**: data curation, investigation, formal analysis, visualization. **Ellis Adams:** conceptualization, writing - original draft preparation, writing - review and editing, validation, funding acquisition. **Krister P. Andersson**: conceptualization, validation, writing - review and editing, funding acquisition; and **Daniel C. Miller**: conceptualization, writing - original draft preparation, validation, writing - review and editing, project administration, funding acquisition.

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Keywords: Poverty alleviation, human development, development policy, land use policy, forest, environmental conservation

Highlights

- Systematic review of randomized controlled trials focused on poverty alleviation or environmental conservation in LMICs published since 1990.
- Only five RCT of 112 RCTs measured both poverty and conservation, limiting knowledge of poverty-environment linkages.
- Interventions combining monetary and non-monetary components more likely to yield poverty alleviation impacts.
- Environmental metrics restricted to short-term outcomes, hampering assessment of ecological change and poverty-environment dynamics
- Substantial evidence gaps underscore the need for further RCTs, especially for dual outcomes in understudied regions over time.

Abstract

Efforts to achieve poverty reduction alongside environmental conservation are hampered by the lack of credible causal evidence. Randomized controlled trials (RCTs) are an established method for producing such evidence. Yet existing knowledge remains fragmented: most studies focus on poverty or conservation in isolation, with few examining both outcomes. To address this gap, we conducted a systematic review of RCTs of interventions in rural areas of low- and middle-income countries (LMICs) that measured poverty alleviation and/or environmental conservation outcomes. We identified 112 studies published since 1990, only five of which measured both poverty and environmental outcomes. Studies assessed a wide range of interventions, with monetary and mixed livelihood programs (e.g. cash transfers or price support) most prevalent in poverty-related studies, and training, monitoring, and compliance programs most common among conservation-related studies. Impacts were heterogeneous across the evidence base. Multi-component interventions combining monetary and non-monetary support more

consistently yielded positive and statistically significant poverty effects. By contrast, non-monetary interventions tended to yield more positive environmental impacts (e.g. on forest presence or land use change), whereas monetary or mixed designs yielded fewer significant environmental impacts. Unlike the poverty literature, several monetary interventions showed negative and significant environmental effects. The five dual-outcome RCTs revealed no generalizable "win–wins": results varied by context, mechanism, and timeframe, and may reflect trade-offs or sequencing between conservation gains and welfare improvements. By bringing poverty- and environment-focused RCTs into the same frame, this systematic review provides a comprehensive picture of the current evidence base on these critical sustainability challenges. In so doing, we identify key research gaps and potential data for further analysis while also generating insights for interventions capable of simultaneously advancing poverty and environmental goals in forest landscapes of LMICs.

1. Introduction

Efforts to combat extreme poverty in low- and middle-income countries (LMICs) have achieved substantial progress, reducing extreme poverty by nearly 65 percent since 1990 (World Bank, 2025). Yet an estimated 831 million people still live on less than $3.00 a day, the vast majority in rural areas of the LMICs (World Bank, 2025). Beyond masking stark disparities in progress across regions and population groups, these global statistics also obscure the environmental consequences of poverty alleviation efforts. Economic gains have often been accompanied by increasing ecological pressure: rapid industrialization, non-renewable resource extraction, agricultural expansion, and chemically-intensive production have accelerated deforestation, biodiversity loss, and pollution (Chakravarty & Mandal, 2020; Stern, 2017). These dynamics are particularly acute in forested landscapes, where sustained pathways out of poverty rely on healthy ecosystems. As forests degrade, households face declining soil fertility, reduced access to forest products, longer collection times, weakened safety nets, and heightened vulnerability to climate and market shocks (Barbier & Hochard, 2018; Miller et al., 2022). Achieving durable poverty reduction in such settings, therefore, requires balancing socioeconomic activity with the protection of forest ecosystems –the natural life-support systems that underpin long-term wellbeing.

The relationship between poverty alleviation and environmental change is dynamic and complex (Barbier & Hochard, 2018; Jagger et al., 2022). Scholars have theorized this relationship along two broad pathways. The first highlights how poverty alleviation measures can drive environmental degradation. In rural areas, poor households often depend directly on land and natural resources, and their strategies to increase income – through agricultural extensification, charcoal production, or forest clearing – can degrade the local environment (Suich et al., 2015). Global patterns reflect this mechanism: between 2001 and 2015, agricultural commodity expansion accounted for roughly 57% of global tree-cover loss (World Resources Institute, 2024). Anti-poverty interventions may inadvertently reinforce these dynamics. For example, subsidized seed programs in the Democratic Republic of the Congo spurred increased primary forest clearing (Bernard et al., 2023). Such cases represent win-lose scenarios in which poverty reduction efforts may boost income and other dimensions of human well-being but worsen environmental conditions. With time, however, such scenarios may become "lose-lose" if resource degradation constrains economic options and activities, possibly leading to households falling back into poverty (Barbier & Hochard, 2018; Jagger et al., 2022).

A second pathway emphasizes "win-win" possibilities wherein poverty alleviation aligns with environmental protection by promoting sustainable resource use, diversification toward non-timber forest products, or adoption of environmentally beneficial practices. Evidence suggests that sustainable management of forest goods (e.g., fuelwood, fruits, nuts, medicinal plants) can raise incomes without degrading ecosystems (Razafindratsima et al., 2021). Previous reviews of forest-related interventions like ecotourism, community forest management, agroforestry, and payments for ecosystem services (PES) have shown that these interventions are capable of alleviating poverty (Hajjar et al., 2020) and protecting the natural environment (Börner et al., 2020; Castle et al., 2021). Over time, higher incomes from these and other interventions may also increase demand for environmental quality as households shift toward cleaner energy sources, improved land management, or investments in soil conservation (Mather et al., 1999). However, as noted above and analyzed in a voluminous literature, economic growth can also lead to environmental degradation (Barbier, 2019; Díaz et al., 2019; Lin & Liscow, 2013).

These two theoretical pathways can intertwine and shift over time, challenging efforts to assess their effects empirically. Despite a long-standing and growing body of evidence (Alam, 2010; Grossman & Krueger, 1995; Malerba, 2020; Masron & Subramaniam, 2019; Stern, 2017), consensus remains elusive on the causal direction of the poverty-environment relationship and the circumstances under which “win-wins” may be possible (or at least “win-neutral” outcomes in which poverty alleviation does not come at the expense of the environment or vice versa). Untangling these dynamics is critical for identifying policy interventions potentially capable of achieving both poverty alleviation and environmental conservation goals. This lack of empirical clarity underscores the need for rigorous approaches capable of disentangling complex causal relationships.

Randomized controlled trials (RCTs) present a particularly valuable tool for understanding such causal issues. Over the past quarter century, RCTs have become the methodological standard in development economics and other fields for isolating causal impacts and probing mechanisms, offering clear counterfactuals and the ability to test competing causal pathways (Rodgers et al., 2020). As such, RCTs may offer especially powerful insights to address longstanding questions about poverty-environment dynamics. Yet few RCTs appear to have examined both poverty and environmental outcomes simultaneously. As of 2020, virtually no RCTs had assessed the environmental impacts of poverty interventions, and few conservation-oriented RCTs incorporated poverty indicators (Alpizar & Ferraro, 2020). On the poverty side, multi-country evaluations (e.g. Banerjee et al., 2015) have generated important new evidence but largely ignored environmental effects. On the environment side, RCTs have expanded slowly due to challenges in randomizing major conservation strategies, such as protected areas or different forest-management regimes (Pynegar et al., 2025). Still, the number of RCTs assessing both poverty and environmental impacts has only grown in the wake of the 2019 award of the Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel to Abhijit Banerjee, Esther Duflo and Michael Kremer for their use of RCTs in global poverty alleviation efforts (Neuwinger, 2024; Pynegar et al., 2025).

A comprehensive understanding of which interventions have been tested through RCTs, where, and with what effects on poverty and environmental outcomes remains lacking.

Consequently, we lack credible answers to the key question: *Can interventions reduce rural poverty while conserving the environment?* To address this gap, we conduct a systematic review of RCT evidence on interventions aimed at alleviating poverty and/or conserving the environment in rural LMICs.

Most existing reviews analyze either poverty or environmental outcomes in isolation; few examine both simultaneously. This situation reflects a broader pattern in the literature, where empirical analysis of joint social, economic, and environmental outcomes remains rare (Agrawal & Chhatre, 2011; Hajjar et al., 2020). Moreover, most systematic reviews focus on single instruments, such as savings promotion (Steinert et al., 2018), conditional cash transfers (Araújo, 2021), unconditional cash transfers (Crosta et al., 2024), or PES programs (Liu & Kontoleon, 2018; Snilsveit et al., 2019), rather than comparing multiple intervention types. Although comprehensive syntheses of particular poverty alleviation interventions exist (e.g. Hanna & Olken, 2026), none combine that breadth with credible causal evidence on environmental outcomes. As a result, we still lack a systematic understanding of how many RCTs evaluate poverty outcomes, how many evaluate environmental outcomes, and how many (if any) have examined both, leaving scholars and policymakers with insufficient theory and evidence on the potential for interventions to simultaneously advance poverty alleviation and environmental sustainability goals.

This article responds directly to these gaps by systematically reviewing all published RCT-based studies conducted in rural forested landscapes of LMICs that evaluate (a) poverty outcomes, (b) environmental or forest-related outcomes, or (c) both. We concentrate on RCTs to identify credible causal effects on outcomes of interest. We chose to focus exclusively on RCTs because they provide the strongest empirical basis for mapping fundamental causal relationships between interventions and multiple outcomes, offering a foundation upon which broader, intervention-specific reviews - incorporating non-experimental evidence - can later be built to shed further light on complex patterns. The specific questions guiding our analysis are:

1. What interventions have been implemented as RCTs to reduce poverty in rural areas, and what impacts have they had on poverty?

2. What interventions have been implemented as RCTs to support environmental conservation in rural areas of LMICs, and what impacts have they had on conservation?
3. Have any studies investigated both poverty alleviation and environmental conservation outcomes? If so, what impacts have they had on joint outcomes?

By synthesizing the scope and findings of the disparate studies addressing these questions, we provide the first comprehensive assessment of which intervention types are more often associated with joint gains, where trade-offs are more likely, and what evidence gaps remain. Our review also considers potential limitations and blind spots in the use of RCTs to understand joint poverty-environment outcomes and the dynamics of this relationship more generally. This comprehensive summary of the current state of knowledge helps to identify research gaps and potential data for further analysis, while also serving as a steppingstone towards the development of interventions that can successfully address poverty and conservation goals in forest landscapes of LMICs, with a view to rigorous evaluation via RCTs.

2. Methods

Our systematic review identified and analyzed studies evaluating interventions implemented as randomized controlled trials (RCTs) in rural LMIC settings that reported outcomes in poverty alleviation and/or environmental conservation domains. We retained all studies published over the past three and a half decades that measured at least one of these domains and flagged those that measured both.

2.1 Identifying the evidence base

We followed the procedures used in two relevant recent systematic reviews (Castle et al., 2021; Snilsveit et al., 2019), replicating their database coverage, search logic, and sequential screening, adapted to our research focus. Our strategy centered on intervention location (rural areas of LMICs), the evaluation strategy used (as RCTs), and outcomes assessed (poverty and/or environment). The methods we used in this systematic review, including search and screening strategy, coding rules, and synthesis decisions, are based on a pre-published protocol Molina-Garzon et al., (2025). We summarize these methods here and provide additional detail in

Appendix A, which also includes information about ways the approach we implemented differs from the protocol (Table A1).

We searched academic databases and grey-literature repositories for studies published January 1990 through August 2024 (details and full strings in Appendix A). We searched for studies not only in English but also in French, Portuguese, and Spanish, languages used in many highly forested LMIC contexts. Three reviewers independently screened titles/abstracts and then full texts, using pre-specified inclusion criteria: (1) randomized assignment at individual or cluster level; (2) rural setting in LMICs (following the World Bank country and lending group classification)[1]; (3) intervention plausibly affecting poverty and/or environmental outcomes; (4) quantitative outcome data with sufficient statistics to identify effect direction and statistical significance. Disagreements were resolved by consensus or third-reviewer adjudication.

Because many trials randomize multiple intervention arms, our unit of analysis was the intervention arm rather than the study. We classified each intervention arm into one of three categories: (i) those providing primarily monetary support, such as cash or in-kind transfers, productive assets or technologies, financial services, and price support (subsidies, vouchers); (ii) those providing primarily non-monetary support, such as governance support (e.g., land clarification, collective management systems), educational and technical assistance; or (iii) mixed interventions, containing elements of both. Outcome types were pre-defined as poverty, environment, or both. For each arm–outcome pair in included studies, we extracted information on the sample, unit/cluster, follow-up duration, outcome definition, effect direction, and statistical significance (as reported; we classify $p < 0.10$ as "statistically significant" in line with field conventions when authors used that threshold).

2.2 Outcomes Considered

Poverty outcomes follow McKinnon et al., (2016) and Cheng et al., (2019), operationalized as direct effects on income, consumption/expenditure, household or individual

[1] World Bank Country and Lending Groups. https://datahelpdesk.worldbank.org/knowledgebase/articles/906519-world-bank-country-and-lending-groups. Accessed September 1, 2025.

assets, household living conditions, agricultural production, and non-farm labor. This conceptualization embraces a multifaceted dimension of poverty rather than a narrow focus on monetary measures alone. We excluded studies focused on health outcomes as well as other intangible or less direct measures of poverty, given resource constraints: previous evidence syntheses suggest that the health literature is likely very large while relatively few studies use less tangible indicators of poverty (Lleras-Muney et al., 2025; McKinnon et al., 2016).

Environmental outcomes included direct measures of forest cover, or forest presence (e.g., tree survival or presence), vegetation, and biodiversity (Table 1). Our preregistered protocol specified a broader set of potential environmental indicators, but resource constraints necessitated that we narrow the scope (see section 2.4.2 below and Appendix A, Table A1).

Table 1. Studied outcomes and indicators

Poverty Alleviation	**Environmental Conservation**
Income Consumption and expenditure Assets Household living conditions (housing materials, living standards and livelihood) Labor (non-farm employment) Agricultural production	Forest cover change (area planted with trees, tree survival rates, deforestation, and land use changes) Vegetation Biodiversity

2.3 Search Strategy

We searched three bibliographic databases - EconLit (EBSCO), Web of Science Core Collection, and Scopus - for publications between 1990 and 2024, considering data collected after 1980. Search strings combined terms for (i) study design (“randomized”, “randomised”, “RCT”, “field experiment”), (ii) geography (“rural”, LMIC country names), and (iii) outcomes (“income”, “consumption”, “assets”, “employment”, “forest”, “deforestation”, “land cover”, “vegetation”). Table 2 presents a summary of keyword components used in these searches. Full database-specific strings are in Appendix 2 (Table A2). Where platforms lacked full Boolean operators support, we simplified accordingly. To manage volume, we screened up to the first 1,000 results (sorted by relevance) per database. We note that some of the keywords used during the search stage correspond to outcomes that were dropped at later stages in the review process as described in Appendix A (Table A1).

Table 2. Summary of search terms

Outcome	**Search terms**
Poverty	(“Randomized control* trial” OR RCT*) AND ("Poverty") AND ("Income" OR "Consumption" OR “Assets” OR “Living conditions” OR “Living standards” or “Livelihood” OR “Deprivation” OR “Non-farm employment” OR “employment” OR “Labor” OR “agricultural production” OR “capital”) AND (“Rural” OR “village”)
Environmental Conservation	(Randomized control* trial OR RCT*) AND (“Land use change” OR “Forest Cover” OR “Vegetation” OR “Biodiversity”) AND (“Rural” OR “village”)

2.4 Screening and Study Selection

2.4.1 Inclusion based on titles and abstracts

Three team members independently screened records against inclusion/exclusion criteria. “Unsure” records were escalated for secondary review, and if needed, brief full-text checks and team discussion. We validated the search by confirming the retrieval of 13 benchmark RCT studies (seven focusing on poverty outcomes and six on environmental outcomes) identified outside the workflow by the project PIs. All 13 studies appeared in our search results, validating the effectiveness of the keyword strategy.

We excluded three classes of interventions at this stage: (1) mental health programs, whose primary aims typically fall outside poverty alleviation or environmental conservation; (2) large infrastructure investments; and (3) resource-use regulation policies. These are categories rarely implemented via RCTs. For quality assurance, we double-screened a 5% random sample of included studies and computed a Jaccard similarity score (resulting in 0.889 and 0.875 across two subsets of assessment, with two coders working on one set each and a third coder working on both sets), indicating a high percentage of coder agreement (See Appendix A, Tables A3.1 and A3.2 for details).

2.4.2 Inclusion based on full-text review

Final inclusion decisions were made through full-text review. A senior researcher first re-examined studies labeled "excluded" or "unsure" to confirm first-stage decisions. During data extraction, we verified outcome definitions, corrected first-stage errors, and removed 16 duplicates. We also replaced two preprints with their published versions, and via citation chasing, added seven studies referenced by included RCTs.

At this stage, we introduced several deviations from the pre-registered protocol to ensure feasibility and internal consistency of the synthesis. We made two main adjustments. First, we restricted the corpus to peer-reviewed journal articles. This decision led us to exclude some 60 working papers and other non-peer-reviewed studies. Resource constraints and an interest in reducing potential bias introduced by non-peer-reviewed studies led us to this choice. Second, we narrowed our scope to exclude studies whose primary focus was on either a) less tangible or indirect measures of poverty (e.g., empowerment, social capital, equity), which have limited comparability across studies, or b) environmental indicators beyond forests and vegetation-related ones (e.g., on water and sanitation, or air and water quality). These and other minor changes are summarized in Appendix A, Table A1).

These scope refinements substantially reduced the initial pool to a manageable, internally comparable set of RCT studies while maintaining our focus on arm-level, causally identified estimates in rural LMIC forest landscape contexts.

2.5 Data Extraction and Coding

Eight coders used a standardized data extraction form (Appendix A, Table A4), piloted on 12 studies to harmonize rules and reduce bias. For each included study, we extracted: (i) outcomes from our predefined list (Table 2); (ii) intervention details and type of implementing agency; (iii) geographic location; and (iv) study design characteristics (sampling strategy, treatment unit, number of arms, heterogeneity analysis, and author-reported limitations). For multi-arm trials with a shared control, we extracted effects for each arm. Where feasible, we disaggregated by salient design features (e.g., conditionality, timing, delivery mode, intensity, and verification strategy).

We assessed inter-coder reliability by double-extracting a random 5% of included studies in both the poverty and environment categories (Appendix A, Table A5). For each selected study, two team members independently extracted the data; coder pairings varied across studies, and the subsample spanned both the poverty and environment sets (four coders in total participated). We compared the two extractions per study using the Jaccard similarity index, obtaining an average similarity of 0.65, which indicates substantial overlap and acceptable reliability for our purposes. Discrepancies were resolved through deliberation and eventual consensus among coders.

The final dataset of included studies and information extracted from them, as well as replication files for our analysis, are available upon request.

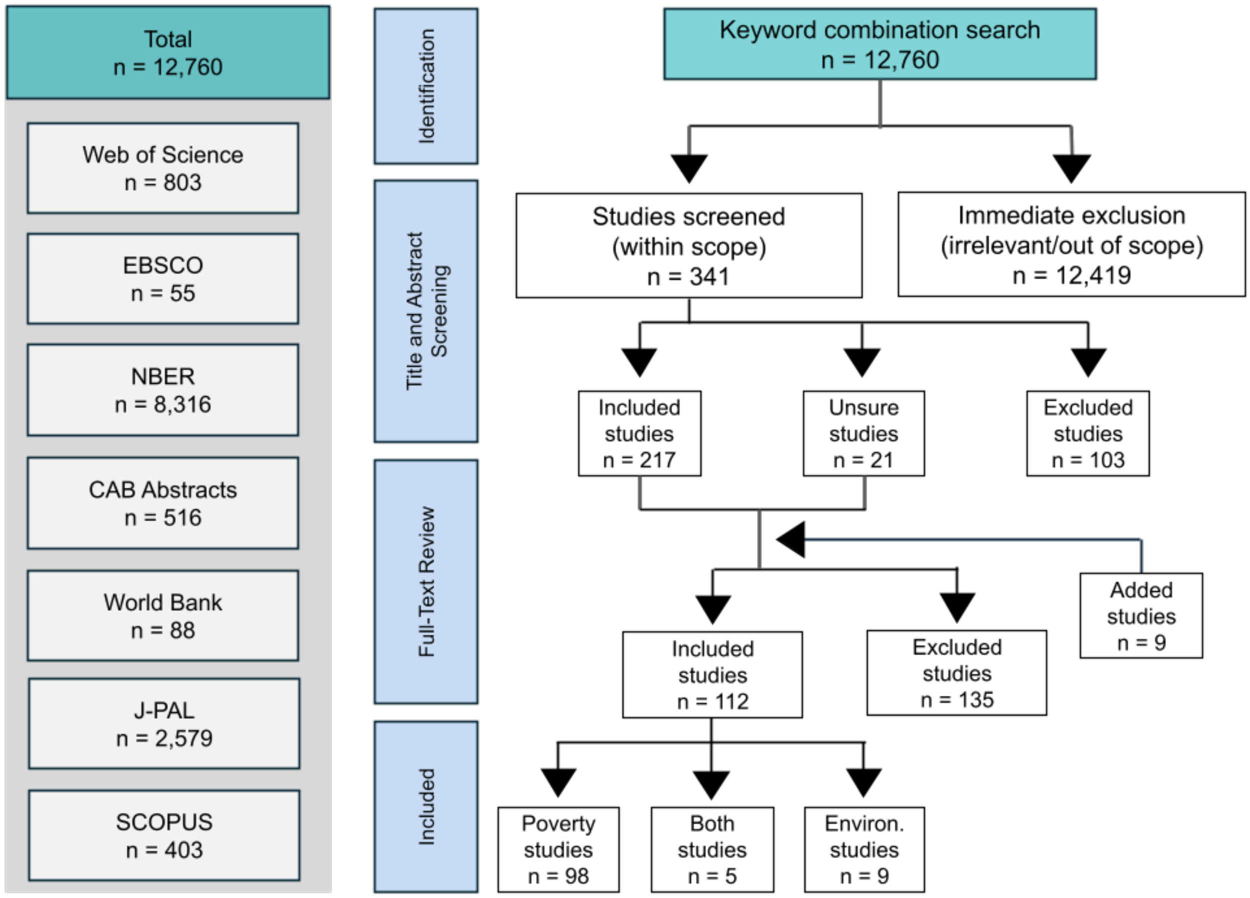
3. RESULTS

3.1 Study Descriptions

Our keyword searches returned a total of 12,771 English-language results across databases, including 5,764 studies related to poverty, 6,996 studies focused on the environment, and 11 duplicates. We also identified 1,514 studies in Portuguese, 29 in French, and 803 in Spanish, but none of these studies met our pre-specified eligibility criteria and were excluded at the title-abstract stage.

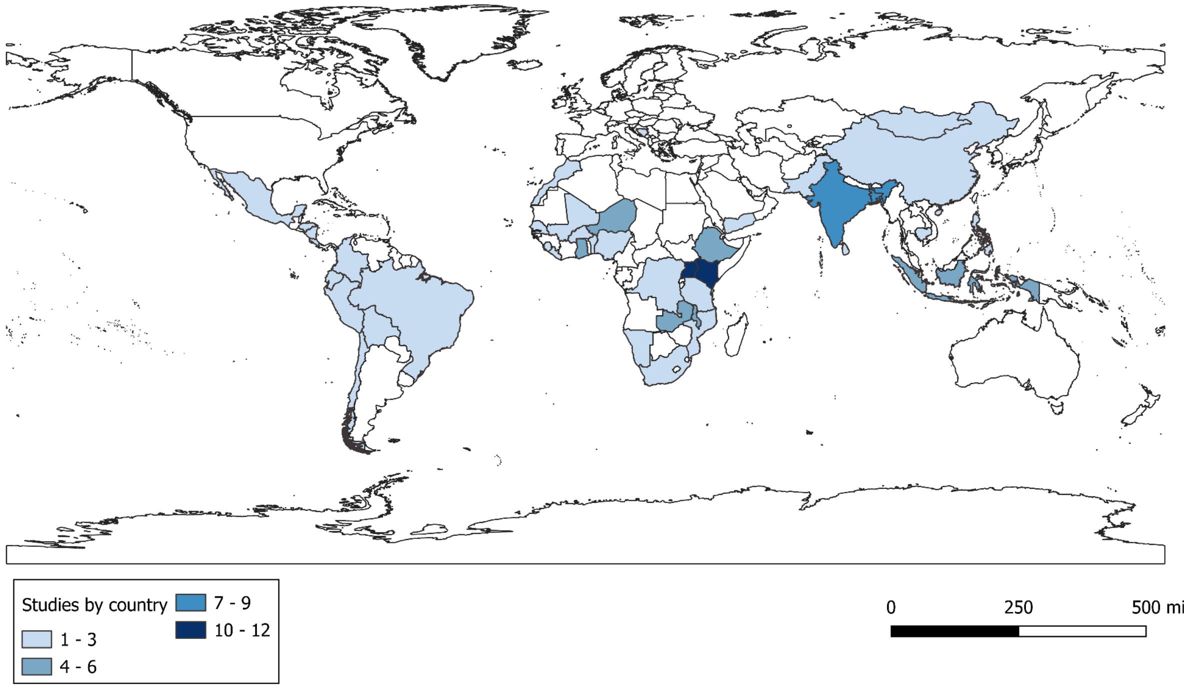
After removing duplicates, 12,760 English records remained. Of these, 341 were flagged for closer review as potentially meeting all three inclusion criteria (255 poverty-related, 84 environment-related, and two studies addressing both). A focused title-abstract assessment then excluded 103 studies (78 poverty and 25 environment) and flagged 21 as unsure. In total, 151 studies underwent full-text review by six team members. The most common reason for exclusion at this stage was a primary focus on outcomes beyond the scope of this review, even when poverty was discussed tangentially. We excluded 39 studies at full text, yielding 112 included studies: 98 with poverty-related outcomes, nine with environment-related outcomes, and five featuring both types of outcomes. The full list of included studies is available in Appendix B. Figure 1 presents a simplified flow of records through identification, screening, eligibility, and inclusion, and shows the distribution of records by source database. The number of excluded studies in the "Full-Text Review" portion of the diagram includes studies that were excluded shortly before the full-text review for various reasons (e.g., working papers, no outcomes of interest, etc.).

Figure 1. ROSES Diagram



Most of the studies targeted poor rural households, identified using various methods such as housing-material indicators, proxy means tests (PMTs), or census records. Notably, no studies explicitly targeted Indigenous Peoples. The geographic distribution of studies was concentrated in a small number of countries (See Figure 2). Kenya (13) and Uganda (10) account for the largest share, followed by Bangladesh (8) and Malawi (6). Several other countries in Sub-Saharan Africa, South Asia, and Southeast Asia are also represented, with between one and five studies each. In Latin America, study representation comes mainly from Mexico, Peru, and a few others. Overall, the sample reflects a focus on Sub-Saharan Africa, with notable contributions from South and Southeast Asia, and more limited coverage in Latin America and other regions. Many countries with large tropical forests and where extreme rural poverty is a widespread and critical issue (e.g., Congo, Madagascar, Myanmar) are not represented at all in the literature reviewed.

Figure 2. Geographical distribution of included studies



To synthesize the evidence from the 112 included studies, we classified interventions at the arm level rather than the study level. 95 of these studies feature multiple outcomes of interest. Further, 49 studies test several interventions ("treatment arms") side-by-side. For example, a study might feature both a cash transfer treatment arm and a cash + coaching treatment arm, and measure several outcomes such as income, assets, and employment. We therefore examined each outcome for each intervention. In practical terms, each treatment arm and each related outcome reported in each study has its own entry in our final dataset rather than being lumped in one line under each study. This treatment-outcome lens allows us to avoid masking differences, as study-level summaries may blur together arms that work very differently across different outcomes.

Using this classification, we extracted information for 1,373 intervention-outcome pairs. The vast majority of interventions identified focused on poverty-related outcomes: 1,280 out of 1,373 outcomes or 93% (Table 3). The most examined poverty outcome was household assets (more than 300 estimations). Agricultural production was also well represented (206 estimations), consistent with the studies being conducted in rural areas. Other well-studied outcomes included expenditure, income, and employment.

Each broad outcome category encompassed multiple indicators across the studies. Agricultural production, for example, was variously captured through livestock sales and revenues, crop sales, crop yields, or the total value of agricultural output. Asset measures also differed: some studies used the value of land owned, others relied on livestock values, asset indices, or financial assets such as savings or cash holdings. Expenditure outcomes ranged from total household spending on all goods and services to more specific measures such as food expenditures, total food consumed, or spending on durable and non-durable goods. Income was similarly heterogeneous, including total household income, non-agricultural earnings, wage income, or self-employment income. Measures of labor or employment also varied widely, capturing total time spent on income-generating activities, weekly or monthly hours worked, the presence of a non-farm enterprise, or whether an individual engaged in any paid work.

Of the 54 estimations focused on environment-related outcomes, 28 analyzed forest cover change and 26 focused on forest presence. Of the 39 estimations from the five studies that assessed both types of outcomes, the majority (21 out of 39) were environment-related outcomes, with 18 poverty-related outcomes. Studies in all three categories varied considerably in the time frame of measured outcomes, ranging from weekly outcomes to monthly or annual outcomes.

Table 3. Types of outcomes examined in our database

Outcome type	# of estimations	#of studies
<u>Poverty-related</u>	<u>1280</u>	<u>98</u>
Agricultural production	206	39
Assets	371	57
Expenditure	278	57
Household living conditions	33	5
Income	235	60
Labor or employment	157	38
<u>Environment-related</u>	<u>54</u>	<u>9</u>
Forest cover change	28	7
Forest presence	26	5

<u>Both outcome types</u>	<u>39</u>	<u>5</u>
Agricultural production	2	1
Assets	4	2
Expenditure	8	4
Income	4	3
Forest cover change	13	4
Forest presence	2	1
Vegetation cover	6	2
Total	1373	112

Note: The number of studies related to each outcome does not sum to the total number of studies under the broad outcome categories (e.g., "poverty-related") since 81 out of 112 studies included more than one outcome type.

3.2 Typology of experimental interventions

We identified a wide range of intervention types that were evaluated using an RCT (Table 4). Given this variety, we examined the impact of interventions that were primarily monetary in nature (e.g., asset or cash transfers, financial services, subsidies, or price support), those that provided non-monetary inputs (e.g., health services, training, or technology adoption), and a relatively large multi-component (mixed) category. Most estimations in poverty-focused studies (88%) derive from monetary or mixed interventions, compared to 50% of estimations in studies examining environmental outcomes and 82% in studies that have both poverty and environmental outcomes.

For the environmental outcomes, we found that training or technology adoption programs were the most widely implemented, followed closely by cash transfer programs (including PES programs) and subsidies (typically for agricultural inputs). Only 15% of estimations involved interventions with multiple components. By contrast, 40% of the estimations that focused on poverty-related outcomes involved multi-component interventions.

Most multi-component interventions were "mixed", meaning they combined either monetary and non-monetary elements or multiple components within a single category (e.g., several monetary elements or several non-monetary elements). A prominent example of a mixed intervention is the NGO BRAC's "Graduation" or "Targeting the ultra-poor" program, which provided households with a one-time productive asset (a cow, chickens, sewing machine etc.) together with technical skills training, short-term consumption support to prevent the premature sale of the asset, access to savings vehicles, and regular coaching or home visits for encouragement, goal-setting and problem-solving (Banerjee et al., 2015). Among two-component designs, the most common pairings were financial services with training/education, or technical assistance, followed by cash transfers with training/education, asset transfers with extension services/technical assistance, cash transfers with technical assistance, provision of productive technologies with technical assistance, cash with psychosocial support, cash with informational nudges, and price support (subsidies/vouchers) combined with either financial services or asset transfer.

Relatively few estimations focused only on asset transfers (61 estimations) without any other added component. Cash transfers were the second largest share of interventions studied in our database, constituting 24% of the estimations for this category of outcomes. These included both unconditional as well as conditional cash transfers. 215 estimations featured financial services, and 136 featured training or technology adoption programs (e.g., skills building, or financial/business knowledge). Only a handful of estimations examined the impact of subsidies or health services (including psychosocial services) on poverty-related outcomes. Across both single-modality and multi-component programs, there was considerable variation in delivery structure (group vs. individual), channel of provision (in-person vs digital or remote), setting where the service was provided (school-based, community-based, workplace, or health/clinic), and timing or duration of the intervention.

Finally, among the studies that considered both poverty and environment outcomes, the majority of estimations (56%) came from PES programs, followed by multi-component interventions (combining livestock and grazing management with training and infrastructure development), and seven estimations involved training or technology adoption programs.

Table 4. Types of interventions evaluated using RCTs

Intervention type	# of estimations in studies with poverty related outcomes	# of estimations in studies with environmental outcomes	# of estimations in studies with both poverty and

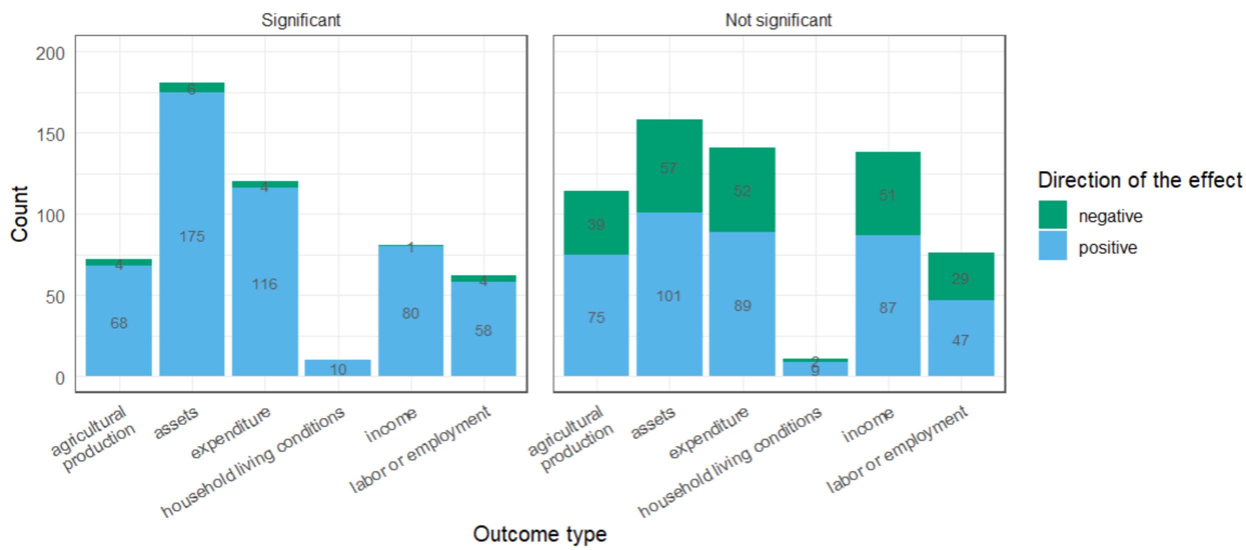
			environmental outcomes
Monetary interventions	*618*	*24*	*22*
Asset transfer	61		
Cash transfer (including PES)	311	11	22
Financial services	215		
Subsidies or price support	31	13	
Non-monetary interventions	*152*	*22*	*7*
Health services	16		
Training/education/technology	136	22	7
Multi-component interventions	*510*	*8*	*10*
Only monetary components	75	2	
Only non-monetary components		2	
Monetary + non-monetary components	435	4	10
Total	1280	54	39

3.3 Intervention impacts on poverty-related outcomes

Statistically significant effects were reported for 45% of the poverty-related intervention-outcome estimation pairs in our review.[2] The vast majority (more than 96%) of the statistically significant estimates were positive; among statistically insignificant estimates, some 64% had a positive sign. Studies measuring assets as the main outcome were more likely to document significant positive impacts, while the picture was much more mixed for studies documenting other outcomes (Figure 3). For instance, in studies measuring income, we observed 80 statistically significant positive estimates and 87 statistically insignificant positive estimates, and 51 statistically insignificant negative estimates. Similarly, for studies that measure expenditure as the main outcome, we saw 116 positive and significant estimates and 141 statistically insignificant estimates.

[2] We excluded 115 estimations of the 1,280 total included due to unclear statistical significance in the original studies, and one estimation was excluded because it had a coefficient of zero. Thus, the 45% figure represents 526 of 1164 included estimations.

Figure 3: Impacts on poverty-related outcomes across all interventions

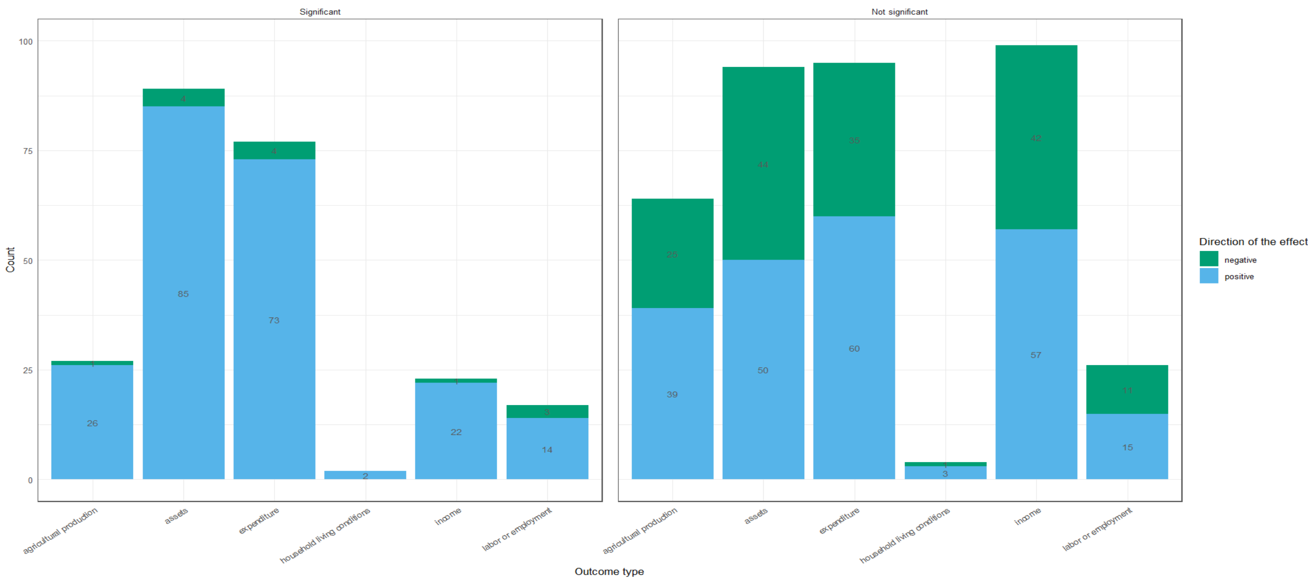


A likely contributing factor for the large number of insignificant estimates is that many trials in our sample were explicitly designed to test variations in program design rather than a single treatment. Interventions frequently randomized multiple arms, varying targeting rules, delivery modes, incentive structures, or support intensity, producing large within-study heterogeneity in expected impacts. As a result, many estimates capture tests of design-specific hypotheses rather than evaluations of an optimized intervention package. This may naturally increase the proportion of null findings even in cases where certain configurations prove effective. The above analysis also pools together the effects of all the different interventions we documented in the literature.

3.3.1 Impact of monetary interventions on poverty outcomes

Across the results of the studies in this category, we saw that monetary interventions, defined as financial services, cash transfers, in-kind asset transfers, and price support through vouchers, subsidies, or fee waivers, had significant positive impacts on household economic welfare (Figure 4). However, across all outcome categories, we see the same number or more statistically insignificant estimates as those that are statistically significant. Most statistically significant impacts concentrated on assets and expenditure. Non-significant estimates showed a more mixed picture: income, assets, expenditure, and agricultural production each showed many non-significant estimates with roughly balanced positive and negative directions, indicating substantial heterogeneity in short-run welfare responses.

Figure 4: Impacts of monetary interventions on poverty-related outcomes



Notes: Sample based on estimations featuring monetary interventions or multi-component interventions with all monetary components.

Programs expanding financial services (e.g., access to credit or savings), consistently produced gains in financial inclusion, including higher deposits and savings, more frequent account use, and greater access to formal or semi-formal services, while enterprise and household welfare outcomes were heterogeneous. Several trials reported increases in borrowing, self-employment, or business activity but limited or null changes in profits, consumption or income within the study window (Attanasio et al., 2015; Augsburg et al., 2015; Crépon et al., 2015). In Uganda, the provision of microfinance to women-only borrowing groups enabled diversification out of agriculture into service-based activities like small-scale trading (Bandiera et al., 2022).

We find that cash transfer interventions reliably raised assets and consumption in the short run, but impacts on enterprise profits, earnings, or durable income were mixed and often attenuated over time. Large transfers boosted spending and investment; macro-scale programs showed sizable short-term poverty reductions with uneven persistence (Egger et al., 2022; Haushofer & Shapiro, 2016; Wolf et al., 2024). Transfers also robustly improved household consumption and food security. An experimental program in Burkina Faso, which provided conditional environmental cash transfers during the lean season, resulted in a 12% increase in household food consumption expenditures and significant reductions in moderate (35%) and severe (60%) food insecurity (Adjognon et al., 2021). Similarly, in Niger, cash transfers delivered via mobile money led to a 9–16% improvement in diet diversity (Aker et al., 2016). An unconditional cash transfer in rural Kenya increased monthly household consumption by $36 PPP (Haushofer & Shapiro, 2016). In Zambia, the Child Grant Programme increased total household expenditure by 19% and food expenditure by 20% (Asfaw et al., 2017). Studies in Bangladesh found that both cash and food transfers increased household consumption (A. Ahmed et al., 2024).

Large-scale transfers also showed spillover effects by generating significant positive externalities for non-recipients within the local economy (Adjognon et al., 2021; Egger et al., 2022). An evaluation of Mexico's *Progresa* program found that for every 100 pesos (~10 USD) transferred to eligible households, the consumption of ineligible households in the same villages increased by approximately 11 pesos (~1.1 USD), primarily through informal insurance and credit markets (Angelucci & De Giorgi, 2009). A large-scale experiment in rural Kenya estimated a local transfer multiplier of 2.5, indicating substantial aggregate economic impacts beyond the direct recipients (Egger et al., 2022).

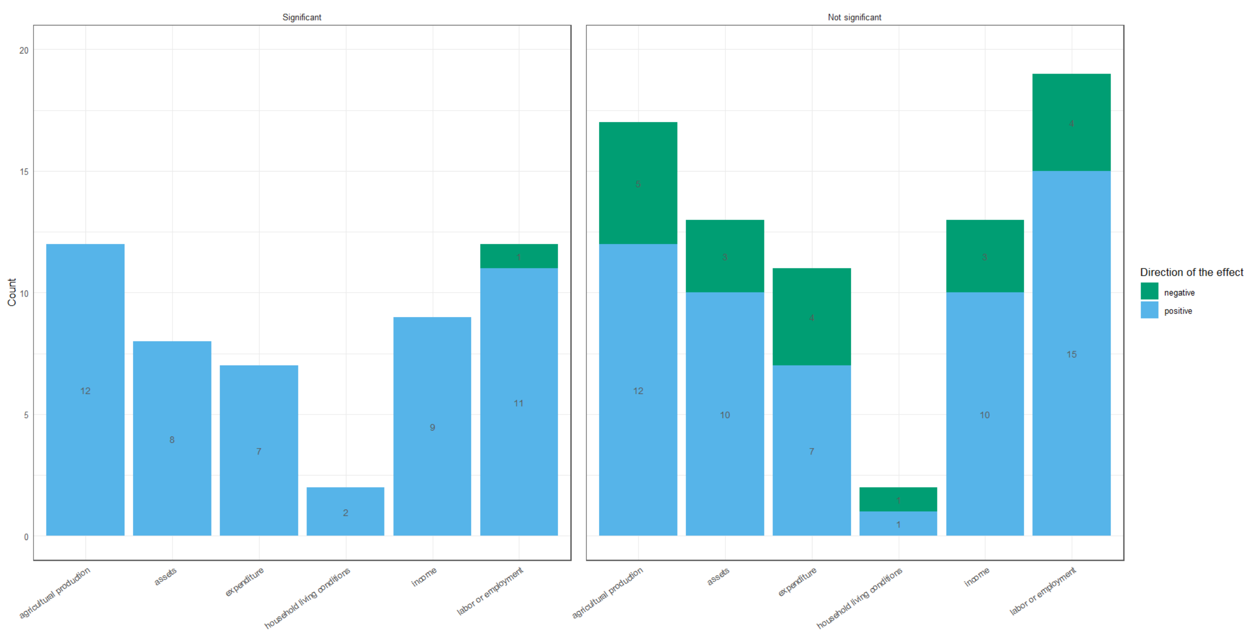
Asset or in-kind transfers (e.g., livestock or equipment) typically increased productive capacity and household asset holdings, while shifting labor or investments toward self-employment or agriculture. Similarly, interventions offering productive inputs or support for agronomic technology adoption consistently reported higher adoption and, in several cases, improved agronomic performance or input use efficiency, like plot-specific fertilizer, improved seeds (Beaman et al., 2013; Harou et al., 2022). However, short-run consumption or income effects were mixed and context-dependent, with several studies emphasizing asset accumulation and capability building rather than immediate welfare gains (Gertler et al., 2012; Green et al., 2015; Karlan et al., 2017; Premand & Stoeffler, 2022; Ward et al., 2021). Finally, trials offering price support (vouchers, subsidies, or fee waivers) increased uptake of targeted goods or services, but effects on employment, profits, or income were mixed to null in many settings (K.

Lee et al., 2020; Martínez A. & Perticará, 2017). Where vouchers were tailored to production constraints, input use and efficiency improved, yet short-term welfare changes remained muted (Harou et al., 2022; Ward et al., 2021).

3.3.2 Impact of non-monetary interventions on poverty outcomes

Estimations involving non-monetary interventions yielded positive and statistically significant effects on all poverty-related outcomes in our database (Figure 5, left panel). However, like monetary interventions, more than half of the estimations involving non-monetary interventions resulted in statistically insignificant effects (Figure 5, right panel). It is suggestive that almost three-quarters of the insignificant effects were also positive in sign, and negative significant effects were rarely seen.

Figure 5: Impact of non-monetary interventions on poverty-related outcomes



The majority of the non-monetary interventions provided training or education to improve capability (skills, knowledge, financial literacy, or curriculum-linked competencies). These interventions showed mixed short-term effects on earnings, profits, or household consumption expenditure (A. Ahmed et al., 2024; Horn et al., 2023; Koomson et al., 2021; J. N. Lee et al., 2021; Rosas et al., 2022). For example, a financial education program in Uganda found that while it increased financial knowledge in the short run, this effect disappeared after five years (Horn et al., 2023). Psychosocial or cognitive therapy interventions typically improved wellbeing, self-efficacy, and reduced stress, and, when paired with job search or business coaching, raised job-search intensity, though income and employment effects remained mixed or context-dependent (Abel et al., 2019; Angelucci & Bennett, 2024; Bandiera et al., 2017; Cecchi et al., 2022; Jensen, 2012; McKenzie et al., 2022). A study in the Philippines found that an aspirations-focused training for entrepreneurs led them to set higher savings goals but did not increase savings and resulted in lower business investment and borrowing, potentially due to frustration from not meeting overly ambitious goals (McKenzie et al., 2022).

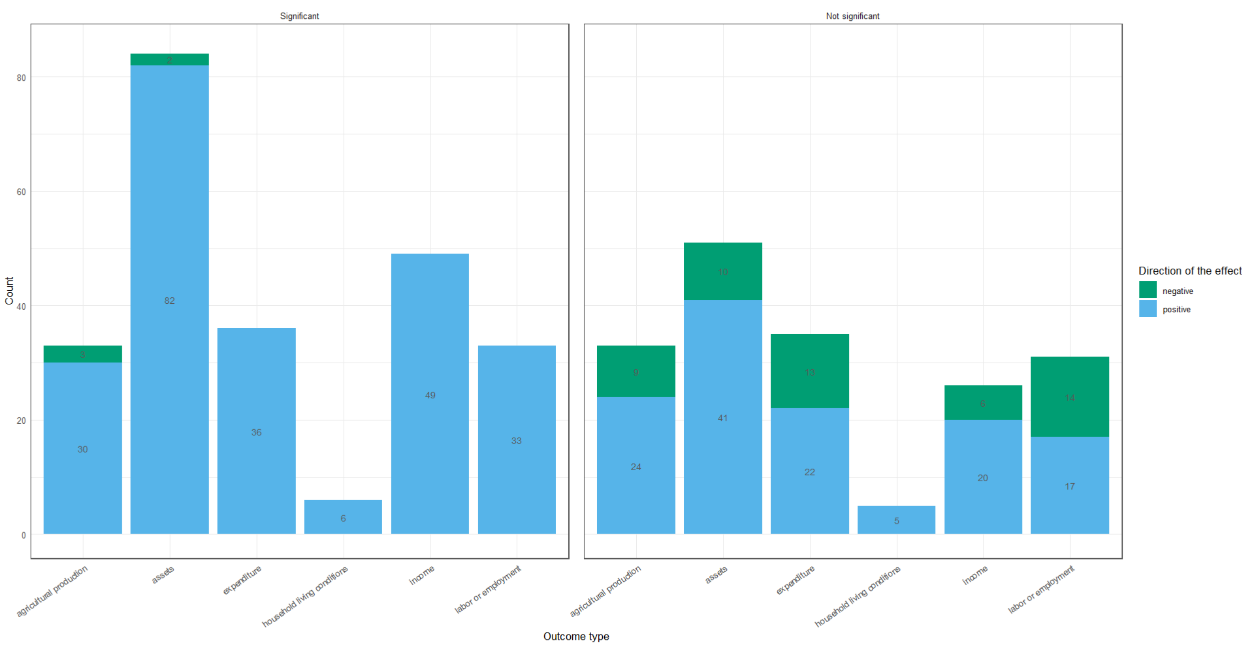
Technical assistance or extension services often generated process adherence and practice adoption, with heterogeneous impacts on profits and earnings. For example, the ANGeL project saw increases of 55–57 percentage points in women's likelihood of adopting an improved practice and found that agricultural training increased homestead production of fruits, vegetables, and eggs, but the absolute changes were modest due to low baseline production (A. Ahmed et al., 2024). Similarly, an aquaculture training in Ghana increased fish productivity by 0.45 kg/m^2 and profits by GHC 1.80/m^2 on average (Ragasa et al., 2022). Low-cost informational prompts moved targeted behaviors (attendance, saving regularly, application completion), but rarely shifted income or profits in the short run without complementary inputs. An information campaign about market reforms in Senegal led farmers to change fertilizer use and improve onion sorting (Bernard et al., 2017). Job matching or placement support increased applications, interview rates, and short-term placement, with mixed earning effects (Abel et al., 2019; Alfonsi et al., 2020).

Trials providing medical services showed improved service utilization and proximate health outcomes depending on the location of the delivery, with limited evidence on welfare effects in the short run, but some significant economic impacts in the long run. A deworming program in Kenya targeted primary school children through mass, school-based delivery, irrespective of individual infection status. The authors identified significant long-term economic benefits twenty years after the intervention. Men who received treatment worked 14% more hours, had 13% higher hourly earnings, and were 9% more likely to live in urban areas, while women experienced a 25% increase in secondary school attendance (Baird et al., 2016). Another study provided free cataract surgery to elderly, visually impaired individuals in rural China, showing substantial increases in personal income (a 54% relative increase) one year later, largely through enabling more unpaid productive activities like domestic work and farming (Tan et al., 2023). A third study provided community-based pharmacotherapy for depressed adults in India. The authors found no significant increase in earnings or consumption and even a temporary reduction in work hours for those receiving pharmacotherapy alone (Angelucci & Bennett, 2024).

3.3.3 Impact of mixed interventions on poverty outcomes

Mixed interventions displayed a clear pattern of effects on poverty-related outcomes: 86% of reported estimations were positive and statistically significant. Positive and significant effects outnumber insignificant effects across all outcome categories, and negative significant findings were rare (Figure 6). Even among statistically insignificant effects, positively signed impacts strongly outnumbered the negatively signed ones.

Figure 6: Impacts of mixed interventions on poverty-related outcomes



A recurring finding was that combining monetary transfers (cash or in-kind) with non-monetary components, such as training, often yielded larger and more sustained impacts than transfers alone. Impacts were strongest when bundles incorporated ongoing coaching or case management layered on training plus financial or asset support, which translated resources into sustained behavior change (e.g., practice adherence, continued saving/ingesting) (A. Ahmed et al., 2024; Alfonsi et al., 2020; Ambler et al., 2020; Edmonds & Theoharides, 2020; Harou et al., 2022; Leight et al., 2021). This result suggests that simply relaxing financial or liquidity constraints is often insufficient to produce lasting change and that addressing informational, skill-based, or psychosocial barriers simultaneously is critical.

Other pairings showed similar patterns. When financial access was paired with skills or business training, studies typically recorded higher account ownership/savings and improved financial or business knowledge, with mixed short-run effects on business performance and earnings. More intensive coaching strengthened adoption of recommended practices and program engagement (Ambler et al., 2020; Angelucci et al., 2023; Bossuroy et al., 2022; Leight et al., 2021; Rahman et al., 2021; J. S.-H. Wang et al., 2021; Weiser et al., 2015). Unrestricted or lightly conditioned cash combined with multi-session training generally raised asset accumulation, program take-up, and planned investments, while earnings/profit gains remained context-dependent; follow-up mentoring reinforced these results (Aker & Jack, 2025; Ambler et al., 2020; Ban et al., 2020; Banerjee et al., 2015, 2022; Bossuroy et al., 2022; Edmonds & Theoharides, 2020). For example, an agricultural training intervention in Niger found that providing farmers with training on an agricultural technique was the primary driver of adoption, increasing it by over 90 percentage points. The addition of conditional or unconditional cash transfers had no significant additional effect on adoption rates over the medium term, suggesting that information and technical knowledge were the binding constraints, not liquidity (Aker & Jack, 2025). Overall, conditionality varied across programs. In some cases, despite the lack of conditionality, programs were "framed" or "labeled" for a specific purpose, such as farm investment or education (Ambler et al., 2020). This mental accounting technique was intended to nudge beneficiaries toward using the funds for the intended purpose without imposing strict conditionality.

Other combinations of cash transfers with either extension services or psychosocial components promoted behavioral changes and adherence but showed unclear welfare gains.

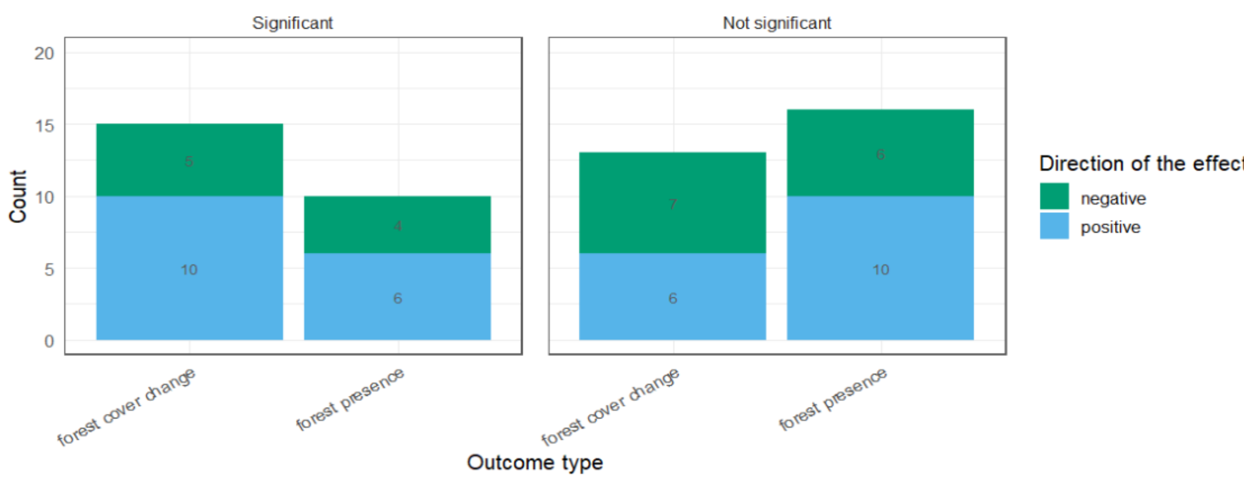
Liquidity relief coupled with individualized technical support tended to lift asset or input investments and behavioral adherence, with variable near-term welfare gains (Banerjee et al., 2022; Blattman & Annan, 2016; Bossuroy et al., 2022; Harou et al., 2022). Pairing cash with light informational nudges increased compliance and follow-through, but did not, on its own, guarantee income gains (Bossuroy et al., 2022). Extension services paired with the promotion of technology adoption or provision of productive inputs showed consistent adoption and productivity gains, while income effects were generally context-specific (Bossuroy et al., 2022; Harou et al., 2022; Leight et al., 2021). Finally, price support (subsidies or vouchers) paired with financial access or asset provision increased uptake of targeted inputs or services, but welfare shifts were limited in the short run (Aggarwal et al., 2018; Couture et al., 2021).

Our search yielded several multifaceted "Big Push" or "Graduation Programs," which are sequenced interventions that bundle multiple complementary components to address the many constraints that may trap households in extreme poverty (Banerjee et al., 2015; Bird et al., 2022; Harou et al., 2022). These programs typically combine a one-time productive asset grant with technical skills training, short-term consumption support (cash or food) to prevent the premature sale of the asset, savings promotion, regular coaching or home visits for encouragement, goal-setting, and problem-solving. Other variants also incorporate health, social, or rights-based training tailored to local needs. The evidence from our identified studies showed that multifaceted programs were able to generate significant and lasting improvements in economic well-being for extremely poor households. Consistent with the pattern of strong effectiveness of mixed programs, Banerjee et al. (2022) found that an "asset-only" intervention showed no evidence of any positive welfare effects after two or three years, compared to large asset (livestock) accumulation among households that received the multi-component graduation program. In general, across diverse settings, including fragile or conflict-affected contexts, these graduation programs have been shown to be effective and cost-efficient (Angelucci et al., 2023; Bandiera et al., 2017; Banerjee et al., 2015; Bird et al., 2022).

3.4 Impact of experimental interventions on environmental outcomes

Examining estimations with environmental outcomes, we found a similar number of statistically significant and insignificant estimates (25 and 29; Figure 7). Interestingly, unlike poverty outcomes, where positive gains dominated, the environmental estimates included a substantial share of negative effects: more than one-third of significant estimates indicated environmental losses, while insignificant effects were distributed more evenly across positive and negative directions. As with poverty outcomes, an important consideration is that many environmental trials tested multiple intervention variants rather than single treatment arms. Consequently, many null or negative estimates may reflect exploration of alternative program configurations rather than evidence against intervention effectiveness per se. This result mirrors the heterogeneity observed across poverty outcomes, underscoring the need to interpret environmental impacts considering the experimental design structures being tested.

Figure 7: Impact of all interventions on forest conservation outcomes

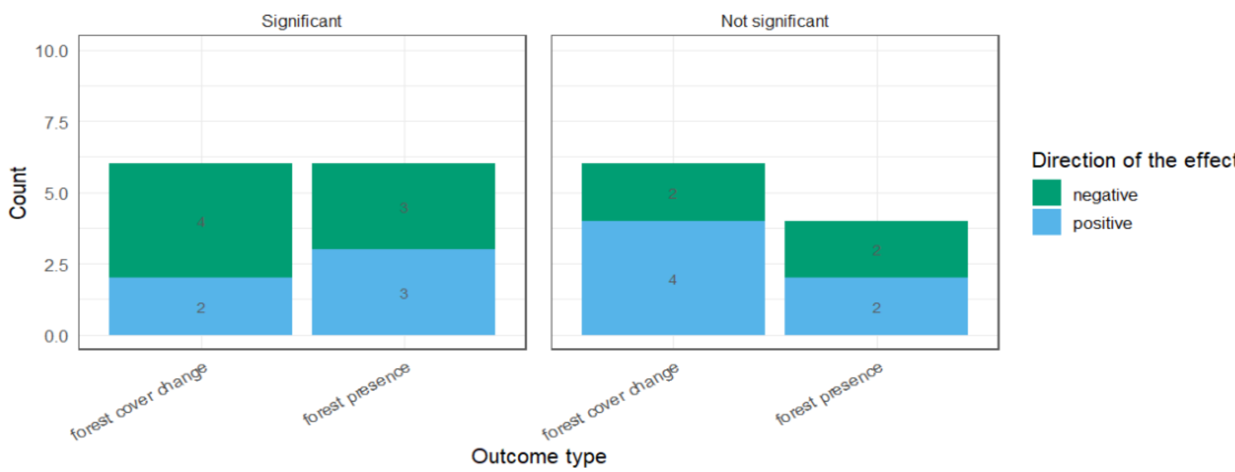


Included studies employed a variety of methods in an RCT design, with many combining remote sensing with detailed ground-based surveys (e.g., direct observation, reports from monitoring teams, household and plot-level surveys) to measure changes in forest cover. Forest cover change had most of the evidence from the studies reporting estimations on environmental outcomes and skewed toward significant effects (15 significant vs. 13 non-significant). Results on forest presence were more mixed, with more non-significant (16) than significant (10) outcome estimates.

3.4.1 Impact of monetary interventions on environmental outcomes

Monetary interventions did not yield consistently positive results for environmental outcomes. Our review yielded only 5 positive and statistically significant estimates, compared to 10 statistically insignificant and 7 negative and significant estimates (Figure 8). The presence of many negative and significant estimates differs from our findings on the impact of monetary interventions on poverty outcomes, where negative and significant findings were extremely rare.

Figure 8: Impact of monetary interventions on forest conservation outcomes



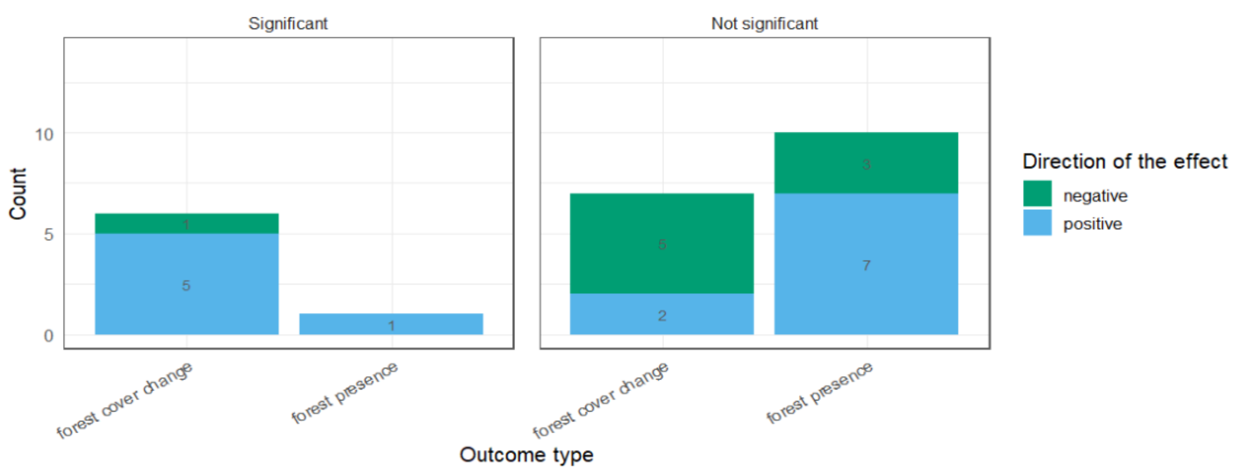
Across five of the conservation-focused randomized evaluations (Bernard et al., 2023; Garbach et al., 2012; Jack, 2013; Slough et al., 2021; Wilebore et al., 2019), monetary instruments were more closely connected with four recurrent designs that yielded distinct conservation patterns: Performance-based payments (PES or contracts) that tied payments to conservation or survival outcomes (e.g., biodiversity-carbon indices or tree-survival targets) generally produced positive conservation gains, especially when combined with better targeting. In an afforestation trial, auction targeting outperformed lottery assignment by channeling contracts to landholders with higher survival, thereby mitigating within-farm leakage and improving net environmental performance (Jack, 2013). In a silvopastoral PES, payments indexed to environmental services supported adoption and delivered “positive conservation outcomes,” while local institutions and informal channels mediated take-up (Garbach et al., 2012). On the contrary, broad unconditional liquidity injections risked increasing deforestation by relaxing constraints on agricultural expansion; namely, absent conservation conditions, cash could have unintentionally financed land-clearing investments (Wilebore et al., 2019).

Providing satellite-based deforestation alerts, training, and incentives for community monitors reduced internal slippage clearing and strengthened community-level management, with impacts strongest where baseline deforestation threats were higher (Slough et al., 2021). Finally, interventions providing subsidies and value-chain upgrades (e.g., modern seed adoption or access to trucking) altered production choices and observed deforestation effects were modest or mixed, with "limited expansion overall", suggesting conservation gains dependent on complementary safeguards and the spatial distribution of adoption (Bernard et al., 2023).

3.4.2 Impact of non-monetary and mixed interventions on environmental outcomes

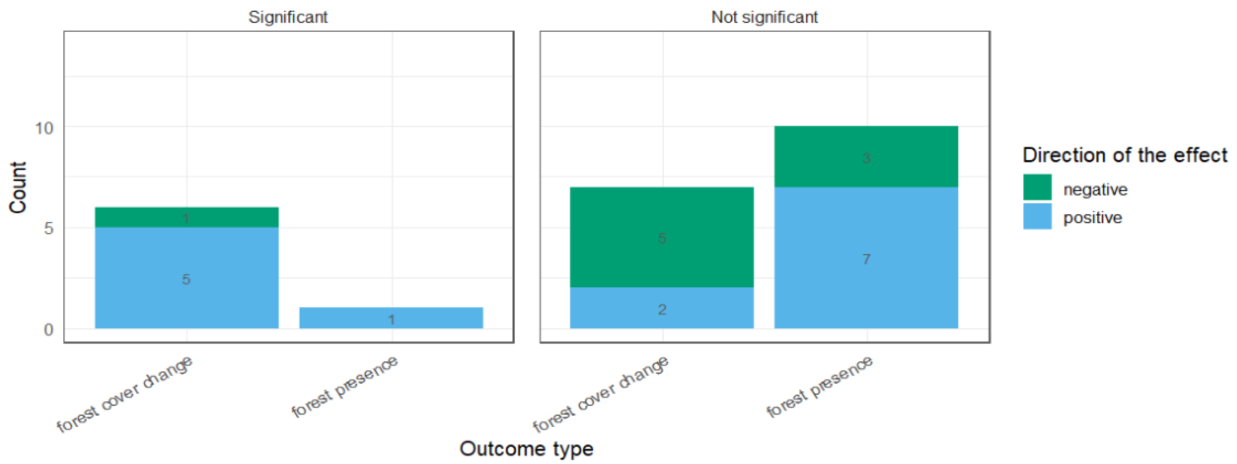
In contrast to the underwhelming impact of monetary interventions, we found many more positive conservation effects arising from non-monetary interventions. Our review identified 6 positive and statistically significant estimates, compared to 17 insignificant ones and only one negative and significant estimate (Figure 9). The count of non-significant estimates showed an even split across positive and negative impacts. While the effects appeared heterogeneous across studies and outcomes, the positive-leaning findings suggest that capability and practice-oriented designs (e.g., training or extension services) can yield positive conservation impacts in the short term, especially on forest cover change.

Figure 9: Impact of non-monetary interventions on forest conservation outcomes



Mixed interventions, combining monetary and non-monetary attributes, constituted a small number of estimates, with significant results concentrated in positive effects on forest-cover change, and a mixed pattern for forest presence (Figure 10).

Figure 10: Impact of mixed interventions on forest conservation outcomes



Taken together, the figures show that within our small (n=9) set of studies reporting impacts on environmental outcomes, most of the statistically significant conservation estimates appeared in non-monetary or mixed interventions and are concentrated on forest-cover change, while few monetary interventions lead to positive and significant estimates. We note that implementation intensity, targeting, and duration of exposure may relate to these patterns, but the evidence here does not allow us to infer likelihoods or make causal claims about which design "works" better. In short, the environmental evidence base we summarize is limited and heterogeneous; our results should be read as descriptive counts of estimates, not as estimates of effectiveness or probability of achieving desired outcomes.

The five conservation-focused studies we classified as non-monetary interventions, most consistently reported changes in behavior or adoption practices (e.g., uptake of conservation or restoration practices) and engagement or compliance, with more limited and heterogeneous statements about forest-cover or deforestation outcomes. Designs that combined some degree of technical assistance with clearer compliance architecture (explicit conditionality and/or monitoring) tended to be linked to more positive results, while education/awareness or inputs/equipment alone yielded ambiguous effects. Interventions that provided hands-on advisory services or capacity building were repeatedly associated with improved adoption and engagement. One program that paired technical assistance with an explicit conditional framework reported increased adoption of specific conservation practices, though effectiveness varied by practice type (Garbach et al., 2012). Another study that embedded training alongside community-led monitoring documented increased local awareness and engagement and slight conservation gains in monitored areas (Eisenbarth et al., 2021).

By contrast, studies centered on information campaigns and seedling or input provision showed mixed or unclear effects on planting/adoption when these elements were not paired with technical assistance or compliance mechanisms. Romero et al. (2019) tested an information campaign with seedling provision, reporting ambiguous impacts on the decision to plan. Similarly, Rudolf et al. (2020), compared information-only versus information-plus-seedlings, finding heterogeneous responses across farmers. In both cases, consistent improvements in conservation results were less clear. Only two studies spoke directly to forest outcomes using remote sensing or similar metrics. Community-led monitoring was associated with slight conservation gains in monitored areas (Eisenbarth et al., 2021), as well as land governance initiatives through land formalization programs focused on agricultural land (Wren-Lewis et al., 2020).

Finally, three studies (8 estimations) were classified as mixed interventions. One study found that the adoption of sustainable practices or behavioral changes was most strongly associated with designs that bundled technical assistance with explicit rules and monitoring (Garbach et al., 2012). The other two studies shared a common focus on promoting tree planting through information/education campaigns combined with the provision of seedlings or other agricultural inputs, all delivered at the household level (Romero et al., 2019; Rudolf et al., 2020). However, their results diverged in important ways. Romero et al. (2019) reported mixed or unclear effects on adoption decisions, while Rudolf et al. (2020) documented heterogeneous responses across farmers, with no clear directional average effect when comparing information-only interventions to information-plus-seedling packages. Together, these findings suggest that while combining information with material inputs can influence adoption decisions, the magnitude and direction of effects vary substantially across contexts and farmer characteristics.

3.5 Intervention impacts in studies measuring both poverty and environmental outcomes

Five randomized evaluations met our inclusion criteria and reported both poverty alleviation and environmental outcomes, contributing 39 estimations for our review (Bragança et al., 2022; Coppock et al., 2022; Jack & Cardona Santos, 2017; Jayachandran et al., 2017; Saavedra, 2025). All five studies paired household, producer, or enterprise data with environmental measurements (typically high-resolution remote sensing of land cover or rangeland conditions), to test whether incentive- or capability-enhancing policies can protect ecosystems while supporting livelihoods. Designs fell into mixed monetary (Uganda PES, Malawi afforestation PES, and Namibia's CBRLM programs), and purely non-monetary packages (Brazil's extension services and Colombia's ecotourism). None were purely monetary. They spanned multiple causal pathways (altering returns to conservation), production (shifting technologies or specialization), and social dynamics (collective action and governance), and varied widely in units of randomization (households, villages/municipalities, or rangeland areas), intensity and composition of support (from information only to training plus hands-on technical assistance), beneficiary-selection mechanism (e.g., lotteries vs. auctions), and evaluation horizons. A simplified synthesis of setting, design, pathways, and the poverty-conservation balance appears in Table 5; fuller details of the interventions and the direction of their effects are provided in Appendix C.

Across the studies, economic effects were heterogeneous and often slow to materialize. Training + intensive TA in Brazil delivered sizable revenue and profit gains among cattle producers (Bragança et al., 2022). Ecotourism in Colombia increased employment, but showed no short-run profit or household income gains, consistent with adjustment lags (Saavedra, 2025). PES designs in Uganda and Malawi were poverty-neutral on standard income/expenditure aggregates; Malawi showed labor-reallocation consistent with liquidity management under afforestation contracts (Jack & Cardona Santos, 2017; Jayachandran et al., 2017). CBRLM in Namibia yielded no economic gains three years post-program (Coppock et al., 2022).

In terms of pure conservation effects, Uganda PES halved tree-cover loss without leakage (Jayachandran et al., 2017). Malawi afforestation PES improved environmental performance when targeted via auctions, which screened on opportunity costs and reduced within-farm leakage compared to lotteries (Jack & Cardona Santos, 2017). Ecotourism reduced deforestation around promoted sites, well below standard social-cost benchmarks (Saavedra, 2025). Brazil's extension services scaled adoption of sustainable pasture practices, generating sizable climate benefits from avoided deforestation. (Bragança et al., 2022). The CBRLM program in Namibia showed no improvements and declined in several rangeland-condition indicators, likely due to increased grazing pressure, including spill-ins attracted by improved water points (Coppock et al., 2022).

Taken together, these RCTs revealed heterogeneous balances between poverty and conservation. Payments for ecosystem services (PES) for avoided deforestation in Uganda (Jayachandran et al., 2017) primarily activated the allocation pathway, raising the private value of standing forest, and halving tree-cover loss without detectable short-run changes in household welfare, yielding an environmental win with neutral poverty effects. In Malawi's afforestation PES, Jack & Cardona Santos (2017) randomized not only participation but also the targeting mechanism: auction pricing screened on opportunity costs and reduced within-farm leakage, delivering stronger environmental performance than lottery assignment without harming welfare. An environmental win with neutral poverty effects. Shifting the production margin, eco-tourism promotion in Colombia (Saavedra, 2025) provided training, product design, marketing, and marketplace access at the municipal level; it increased tourist arrivals and employment and reduced deforestation around promoted sites, yet profits and household incomes did not rise within the evaluation window, an environmental win with null short-run poverty effects, consistent with longer adjustment horizons. On the capability side, pasture restoration among cattle producers in Brazil's Cerrado (Bragança et al., 2022) randomized training alone versus training plus intensive technical assistance (TA). The training plus TA arm produced meaningful adoption and revenue gains alongside positive environmental indicators, a clear win–win, whereas training alone generated weaker and inconsistent effects. Finally, community-based rangeland and livestock management (CBRLM) in Namibia (Coppock et al., 2022) strengthened collective action and governance behaviors (social dynamics) but did not produce measurable improvements in rangeland condition or household economic outcomes during the study period, process gains without detectable outcome gains.

In summary, three patterns recurred. First, allocation-heavy incentives (PES) delivered environmental wins with neutral poverty effects in the short run (Uganda; Malawi with auctions). Second, deep, targeted capability upgrades (training + TA) produced win-wins (Brazil), while re-specialization strategies (ecotourism) yielded localized environmental gains with lagged income responses (Colombia). Third, coordination-only approaches (CBRLM) could strengthen governance without relaxing biophysical or market constraints, producing process gains without outcome-gains, with some negative ecological indicators (Namibia).

Two design lessons emerged across these cases. First, implementation intensity and targeting mattered. Programs that paired resources or training with tailored, ongoing technical assistance (Bragança et al., 2022) or that screened effectively on opportunity costs (Jack & Cardona Santos, 2017) were more likely to move from partial gains to joint gains. Second, time horizons mattered. Several interventions required relatively longer periods for economic benefits to follow environmental protection or practice adoption (e.g., Saavedra, 2025), whereas incentive-focused designs can generate early environmental wins with neutral household effects (Jayachandran et al., 2017). Across the five studies, evaluation windows were generally short to medium run (around 1–3 years), limiting detection of slower-moving welfare responses and complicating comparisons when environmental benefits (e.g., carbon) were not consistently monetized against program costs (only a minority reported such accounting).

In short, win–wins tended to arise when capability upgrades were deep and targeted; environmental wins with neutral welfare when allocation incentives dominated; localized environmental gains without immediate income changes when production re-specialization took time; and governance improvements without near-term ecological or economic returns when coordination alone did not relax biophysical or market constraints.

Table 5. Description of Interventions for Studies Measuring Both Outcome Categories

Study	**Intervention**	**Poverty**	**Environment**	**Balance of**

	Description	outcome types, direction and significance	outcome types, direction and significance	impacts (Environment-poverty)
(Bragança et al., 2022)	Pasture restoration: training vs. training+TA (Brazil, Cerrado)	Expenditure: 1 positive and significant, 1 negative and insignificant.	Vegetation cover: 1 positive significant, 1 positive insignificant.	Win–win for training+TA. Neutral-lose for training.
Coppock et al. 2022	Community-based rangeland/livestock management (Namibia)	Assets: 1 negative and insignificant, 1 zero and insignificant Expenditure: 1 negative and insignificant, 1 zero and insignificant	Vegetation cover: 1 positive insignificant, 2 negative insignificant, 2 negative and significant.	Lose-neutral.
Jack & Cardona Santos 2017	Household-level afforestation PES with auction vs. lottery targeting (Malawi)	Agricultural production: 2 positive and insignificant; Assets: 2 positive and insignificant; Expenditure: 1 positive and insignificant, 1 negative and insignificant; Income: 2 negative and insignificant.	Forest cover change: 2 positive and significant; 2 negative and insignificant; 2 negative and significant. Forest presence: 1 positive and significant, 1 positive and insignificant.	Neutral-Conditional Win. Environmental win with auction as targeting mechanism; poverty neutral
Jayachandran et al. 2017	Village-level PES for avoided deforestation (Uganda)	Expenditure: 1 positive and significant, 1 positive and insignificant.	Forest cover change: 3 positive and significant	Win-neutral
Saavedra	Municipal eco-	Income: 1	Forest cover change:	Win-neutral[3]

[3] The classification in the "Balance of impacts" column reflects only the poverty and environmental outcomes included in this review. In a few cases, original studies also report effects on additional outcomes (e.g., business revenues, municipal income, sectoral growth) that fall outside the scope of our

2025	tourism promotion (training, product design, marketing, marketplace access) (Colombia)	negative and insignificant.	2 positive and significant.	

1

4. Discussion

Alpízar & Ferraro (2020) underscore a fundamental evidence gap at the heart of global sustainability debates: despite decades of investment in anti-poverty and conservation programs, we still lack rigorous causal knowledge about how interventions in one domain affect outcomes in the other. They argue that the near absence of randomized controlled trials that measure both poverty and environmental outcomes has left scholars reliant on observational designs and speculative theorizing about the pathways through which livelihood improvements might degrade or enhance natural resources, or through which conservation efforts might secure or undermine household welfare. The authors highlight that RCTs are uniquely positioned to resolve these uncertainties by explicitly testing causal mechanisms, adjudicating competing assumptions, and clarifying when progress on one goal reinforces or hinders progress on the other. Their critique is pointed: while development RCTs have proliferated, environmental outcomes are rarely measured; environmental programs, in turn, rarely employ randomized designs or track poverty effects. Even in environment-focused evaluations, such as those assessing PES, REDD+, or certification schemes, poverty outcomes are not consistently measured or are only captured in the short term (Ferraro & Agrawal, 2021; Jayachandran et al., 2017; Miller et al., 2017; Vorlaufer et al., 2023). This has left the field with weak causal foundations for policy choices in contexts where both objectives matter.

The five dual-outcome RCTs we identified in this systematic review respond directly to this gap. Although few in number, they demonstrate that it is not only feasible but empirically illuminating to evaluate poverty and environmental effects in tandem. They also reveal that the simplistic expectation of universal “win-win” solutions might be misplaced: the relationship

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outcome categories. As a result, the balance presented here may differ slightly from how authors characterize results in their own analyses, particularly where gains occur at levels other than the household or forest plot.

between poverty alleviation and conservation is heterogeneous, pathway-dependent, and sensitive to design choices such as the type of intervention and time horizons. These studies illustrate precisely the type of causal insight that Alpízar & Ferraro (2020) argue has been missing, and provide an interpretive frame for understanding the broader patterns that emerge in our review.

These dual-outcome studies also illustrate how rigorous evaluations can overcome the deeper structural barriers identified by Alpízar & Ferraro (2020) - namely the "crisis discipline" belief that environmental interventions are inherently beneficial and therefore not in need of experimental testing; the widespread reliance on simple empirical designs presumed sufficient for assessing conservation programs; and the disincentive created by funding environments that reward perceived success rather than rigorous learning. Each study proceeded from genuine uncertainty about both welfare and environmental effects, thereby rejecting the assumption of unqualified benefit. All five studies were explicitly designed as RCTs supported by high-level funders or research partnerships that prioritized learning and accountability, which reduced the political and institutional risks typically associated with discovering null or negative results. Several incorporated measurement strategies capable of detecting negative spillovers, leakage, or livelihood costs, revealing that environmental interventions can produce complex and sometimes adverse distributional or ecological consequences. In practice, these studies showed that experimental designs can illuminate mechanisms, unintended consequences, and trade-offs that remain obscured under the before-and-after or with-without approaches still dominant in sustainability science. Together, they illustrate that rigorous evidence not only strengthens claims of program success where appropriate but also yields policy-relevant learning even when results are mixed.

The overwhelming majority of studies in the RCT landscape we reviewed, evaluated only single outcome categories, most commonly poverty. As a result, our knowledge of interventions capable of improving welfare and environmental outcomes simultaneously remains limited. Still, meaningful patterns emerged. Statistically significant poverty gains clustered in assets and income, with smaller but notable improvements in consumption and expenditure. These gains were concentrated in monetary and mixed designs, particularly interventions that increased liquidity or transferred productive assets and paired them with complements such as training, technical assistance, or coaching. Non-monetary programs (information, education, short trainings) rarely shifted income or consumption. A coherent pattern emerged: relaxing financial constraints could yield short-run improvements in consumption and basic asset accumulation, whereas sustained income or profit gains may require capability upgrades and sufficient time for investments to mature.

This logic aligns with evidence beyond RCTs. Conditional cash transfers (CCTs) have raised consumption and human capital and reduced child mortality in Mexico, Brazil, and beyond (Araújo, 2021; McKinnon et al., 2016). Lump-sum transfers, similarly, enable investment in income-generating assets and improve food security (Sulaiman et al., 2016). Savings-promotion programs yield small but positive effects on expenditures, income, and resilience (Steinert et al., 2018). However, financial inputs alone rarely deliver durable income growth without complementary capability-building. Microfinance, for instance, produces more durable gains when paired with training or technical support (Singh & Chudasama, 2020). Even for large cash transfers, initial gains can fade: evidence from Kenya suggests short-run improvements may dissipate relatively quickly, and similar attenuation appears in business practice outcomes after a couple of years (Sulaiman et al., 2016). The evaluation of the RISEMP program in Costa Rica similarly found that payments were insufficient to maintain long-term land use choices without technical assistance and other adaptive management strategies (Rasch et al., 2021). By contrast, integrated designs - cash or assets plus training, technical assistance, or coaching - more consistently support changes in practices, savings, and enterprise structure (A. Ahmed et al., 2024; Miller et al., 2021; Sulaiman et al., 2016). The “graduation” model exemplifies this logic.

On the environmental side, non-monetary designs such as training/extension and monitoring/compliance architectures most often produced detectable conservation gains, particularly in forest presence and, less frequently, in forest-cover change. Technical assistance combined with financial incentives, such as PES, produced mixed results depending on context and design (Snilsveit et al., 2019). Targeted incentive instruments (e.g., PES with auctions that screened opportunity costs), coupled with clear rules, verification, and sufficient duration, tended to perform well. By contrast, stand-alone liquidity infusions sometimes increased land-use pressure by easing investment in expansion, whereas training plus hands-on technical assistance shifted practices quickly and generated early, focal environmental improvements. These patterns are consistent with evidence from recent reviews, showing that reductions in deforestation are more likely when programs incorporate spatial targeting, strong local governance, and credible conditionality (Kolinjivadi et al., 2023; Snilsveit et al., 2019).

Together, these results indicate that interventions can potentially alleviate rural poverty while conserving the environment, but such outcomes depend on careful institutional design that acknowledges trade-offs, such as targeting high-threat areas versus prioritizing the poorest, and the balance between short- and long-term effects. Among the few RCTs measuring both domains, three patterns stand out. First, allocation-heavy incentives such as PES yielded clear environmental gains with neutral short-run poverty effects, with auction targeting improving performance by curbing leakage. Second, deep, targeted capability upgrades (training plus intensive technical assistance) yielded win–wins by improving adoption, revenue, and environmental indicators. Third, production re-specialization (e.g., ecotourism) produced localized conservation benefits with delayed income responses. Governance-first programs that did not relax biophysical or market constraints strengthened collective action but did not shift welfare or environmental outcomes in the short term. Across these cases, implementation intensity, precise targeting, and longer time horizons were pivotal, and environmental protection or practice adoption often preceded measurable welfare improvements. These patterns align with broader reviews showing that simultaneous gains are not guaranteed and hinge on institutional design that mitigates elite capture, acknowledges pre-existing inequalities, and balances livelihood and sustainability objectives (Ma et al., 2017; Miller et al., 2021).

In addition to these broad categories, several cross-cutting design features surfaced across the studies in our review. Although not systematically examined, they were frequently described as shaping participation, behavior change, or implementation effectiveness, and thus offer useful hypotheses for future research. Conditionality (whether strict, soft, or absent) often influenced behavioral responses, with conditional transfers consistently shifting targeted behaviors and unconditional transfers raising consumption and assets but yielding mixed longer-term effects.

Payment schedules (lump-sum versus recurring) and delivery modalities (digital versus in-person, individual versus group-based) were also repeatedly noted as affecting participation, take-up, and learning. Among non-monetary and mixed interventions, multi-session formats, recurring contact, and coaching were commonly associated with deeper capability gains than one-off training. Group-based formats fostered peer support, while community-based conservation efforts were linked to stronger collective engagement. Several studies also emphasized that the implementing actor's capacity and organizational norms shaped adoption and program performance. Together, these observations suggest that how support is delivered, and under what institutional conditions, may be as important as the type of intervention itself, and merit greater attention in future evaluation and theory-building.

Our findings also highlight broader issues that shape how RCT evidence should be interpreted. The distribution of RCTs is highly uneven, concentrated in Kenya, Uganda, Malawi, and Bangladesh, while major forest-rich countries (including the DRC, Madagascar, Myanmar, and Papua New Guinea) remain unrepresented. External validity is therefore limited: forest-related poverty is shaped by local ecological and institutional systems, and evidence from a handful of countries cannot be assumed to generalize.

In addition, no RCT in our sample measured biodiversity directly. Environmental outcomes were proxied by forest presence, vegetation cover, or land-use change. On the poverty side, outcomes focused heavily on direct poverty metrics like income, consumption, and assets. Overall, RCTs measuring only poverty-related outcomes primarily used monetary or mixed designs, while conservation RCTs used non-monetary, compliance-oriented interventions. Most evaluations identified in our review lasted 12-36 months, yet many conservation and livelihood impacts unfold over longer time scales. Interventions such as ecotourism development, capability upgrading, or sustainable land-use transitions require time to mature. Short windows of assessment risk underestimating long-term benefits or missing delayed trade-offs.

4.1 Emerging Insights for Intervention Design

While the evidence base remains small and heterogeneous, our synthesis suggests several potential design features that appeared repeatedly in studies showing joint gains or minimized trade-offs. These patterns should be viewed as hypotheses for future research rather than definitive prescriptions, particularly given ongoing work - including Sims & Alix-Garcia (Forthcoming) - that aims to more systematically analyze design-effect relationships.

First, aligning an intervention with the binding constraint may matter. When liquidity constraints dominate, transfers tend to shift consumption and assets in the short run (Araújo, 2021; Steinert et al., 2018; Sulaiman et al., 2016). When limited skills or management capacity are the bottleneck, interventions pairing training with sustained technical assistance often show stronger changes in practices (A. U. Ahmed et al., 2024; Liu & Kontoleon, 2018; Miller et al., 2021; Singh & Chudasama, 2020; Sulaiman et al., 2016). For land-use pressures driven by high opportunity costs, targeted PES with clear conditionality and monitoring may deliver conservation benefits (Liu & Kontoleon, 2018; Ma et al., 2017; Snilsveit et al., 2019; Steinert et al., 2018). Yet this targeting raises equity questions: directing payments to where threats are highest may not always align with poverty-alleviation goals (Ma et al., 2017). These tensions highlight the importance of explicitly matching intervention goals with the relevant target population.

Second, combining resources with capability enhancements emerged as a promising route in studies that succeeded on both fronts. Mixed bundles - cash or assets paired with training or coaching - more consistently translate inputs into behavioral or enterprise changes than stand-alone components (Sulaiman et al., 2016). This integrated approach may be especially relevant for interventions seeking durable, two-sided gains, though this remains an area requiring further testing.

Third, precise targeting and credible verification mechanisms appeared to support sustained environmental change. Tools such as auctions can reduce leakage and increase cost-effectiveness; monitoring and audits helped reinforce compliance (Liu & Kontoleon, 2018; Snilsveit et al., 2019). Poor targeting risks low additionality, while combining socioeconomic and environmental criteria may help improve alignment between conservation benefits and equity considerations. Conditionality likewise shifted targeted behaviors in several studies (Snilsveit et al., 2019), consistent with broader evidence showing that monitoring and sanctions enhance the effectiveness of conservation rules such as protected areas and logging bans (Börner et al., 2020).

Fourth, delivery modalities may shape engagement and adoption. Group-based, in-person formats often deepened learning and participation, and repeated touchpoints (such as coaching or home visits) were frequently mentioned as being associated with more persistent changes (Singh & Chudasama, 2020; Steinert et al., 2018; Sulaiman et al., 2016). Evidence from PES programs similarly suggested that peer learning, workshops, and field visits can strengthen take-up and norm formation (Snilsveit et al., 2019). These suggestive patterns invite further research into how delivery choices influence both adoption and welfare.

Finally, time horizons matter. Many environmental and income effects unfold beyond the typical 12–36-month evaluation window. Identifying when short-run responses predict longer-run gains or hide emerging trade-offs remains an important research frontier. Similarly, questions of cost-effectiveness, design optimization, and long-term dynamics should be central in future work.

Taken together, these insights provide an initial map of where joint gains may arise and where trade-offs are more likely: liquidity plus complements tends to support short-run poverty effects; capability-building and compliance tend to support conservation; and combinations of intensive capability upgrades or well-targeted incentives show the most promise for achieving both. However, these patterns should be treated as emerging hypotheses, not settled conclusions. Advancing this agenda will require coordinated efforts, longer-term evaluations, and systematic meta-analytic work to test which design features most reliably support dual poverty–conservation objectives.

4.2 Bias assessment

We conducted a structured risk-of-bias assessment for every randomized evaluation in the review, adapting the domains and coding scheme from Hombrados and Waddington, (2012). Each study was independently coded on a five-point scale - yes/probably yes/unclear/probably no/no - for the following domains: (i) protection against selection bias (including correct randomization and balance checks); (ii) deviations from intended interventions, including spillovers, cross-overs, and contamination; (iii) performance bias (behavioral responses to being observed or treated); (iv) outcome-measurement bias (use of objective indicators, recall periods, and survey design); (v) analysis-reporting bias (pre-registration, disclosure of outcomes, and model transparency); and (vi) unit-of-analysis issues (e.g., clustering for cluster designs). We also recorded whether trials were pre-registered and whether participants/observers were blinded (rare in field RCTs). Discrepancies between coders were reconciled by discussion.

Overall, selection bias was low: randomization and balance checks meant ~85-90% of studies were judged free or probably free of risk. Deviations from intended interventions were adequately mitigated in ~80% of studies, although several reported partial contamination (e.g., materials or information reaching control areas). Performance bias was judged low in most cases (~85-90%), but concerns arose when interventions entailed intensive training, repeated contact, or strong social desirability incentives. Outcome-measurement bias was low ~80-85% of studies, with higher risk where outcomes relied on self-reports, long recall windows, or subjective constructs. By contrast, analysis-reporting transparency was uneven: fewer than half of studies were probably free of selective reporting, pre-registration was uncommon, and protocols were rarely cited. As expected for field trials, blinding of participants was largely not feasible. Detailed study-level bias ratings, justifications, and examples appear in Appendix A, Figure A1.

4.3 Study limitations

Three structural limitations constrain what can be inferred from the existing evidence base in this review. First, dual-outcome RCTs that measure both poverty and environmental outcomes remain scarce. Even within this small set, biodiversity indicators are absent, and outcome definitions vary widely across studies, making synthesis and comparison difficult. Second, evaluation horizons are typically short, often 12-36 months, restricting the ability to detect slower-moving ecological changes or long-term welfare effects, and limiting rigorous assessments of environmental cost-effectiveness. Third, the literature is geographically concentrated and frequently relies on self-reported measures or administrative records with incomplete documentation, while the use of pre-analysis plans and standardized reporting remains inconsistent.

More fundamentally, our review focuses intentionally and exclusively on RCTs, which necessarily narrows the scope of interventions covered. Many policies and programs relevant to poverty reduction or environmental conservation - such as macroeconomic reforms, inflation control, large-scale infrastructure investments, or area-based conservation strategies like protected areas and watershed governance - are not amenable to random assignment. These interventions have been studied extensively through other empirical designs, and there are many systematic reviews outside the RCT literature that speak to their effectiveness. However, because RCTs offer the strongest causal identification under well-implemented conditions, mapping what has been credibly tested through randomized designs is essential for understanding where robust causal evidence exists and major gaps remain.

At the same time, RCTs have well-recognized limitations. External validity may be narrow when results reflect specific populations, contexts, or implementation conditions. Internal validity can be weakened by non-compliance, spillovers, or deviations from protocol. Measurement error is common when relying on self-reports, statistical power is often limited for volatile outcomes such as profits or revenues, and attrition or multiple hypothesis testing can introduce bias. These challenges do not diminish the value of RCTs, but they clarify why a systematic review is needed - to identify what has been rigorously tested, highlight where causal evidence remains thin, and inform future research and policy design across the broader landscape of poverty-environment interventions.

Future work should (i) embed dual-domain outcomes ex-ante; (ii) test targeting and implementation-intensity variants experimentally (e.g., auctions, TA frequency); (iii) extend long-run follow-up; and (iv) adopt common reporting standards (pre-registration, shared codebooks, harmonized outcomes) to support comparability and meta-analytic synthesis.

5. Conclusion

This review provides a comprehensive synthesis of randomized controlled trials evaluating poverty alleviation and environmental conservation interventions in rural LMIC settings. Across 112 RCTs, we found a literature that was both broad in thematic coverage and highly heterogeneous in design, implementation, and outcome measurement. Most of these studies focused exclusively on poverty outcomes, while rigorous environmental evaluations, and especially dual-outcome assessments, remained exceedingly rare. As a result, the core problem identified by Alpízar & Ferraro (2020) persists: despite increasing interest in broad, sustainability-focused development, we still lack a substantial causal evidence base linking livelihood interventions to environmental change, or conservation programs to household welfare.

Where environmental outcomes were measured, they relied heavily on proxies such as forest presence or land-use change and often tracked only short-run responses within 12–36 months. On the poverty side, most trials measured immediate welfare indicators rather than structural transformations. Combined with the uneven geographic distribution of studies, these features limit the generalizability of existing findings and constrain our understanding of how poverty and conservation dynamics play out across diverse socioecological settings.

The cumulative lesson from this review is therefore not that particular designs consistently produce synergistic outcomes, but rather that the current experimental evidence base remains too narrow (topically, temporally, and geographically) to draw strong inferences about when and how interventions can advance both objectives simultaneously. Expanding the frontier of multiple-outcome RCTs, improving environmental measurement, embedding longer-term follow-up, and developing common reporting standards are essential next steps. Addressing these gaps is critical not only for resolving the empirical questions raised in this review but also for enabling actors concerned with the design and implementation of future programs and interventions to navigate trade-offs and complementarities with greater confidence.

Appendix A. Methods Detail
Appendix B. Reference Information for the 112 Included Studies
Appendix C. Additional Detail on Studies Measuring Both Poverty and Environmental Outcomes

Data availability

Data will be made available on request.

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Appendix A. Methods Detail

Deviations from the Published Protocol

Resource limitations and other practical considerations encountered while carrying out this systematic review led us to adjust some aspects of our original plan (Molina-Garzon et al., 2025). Table A1 summarizes these changes, including the rationale for each.

Table A1. Summary of deviations from the protocol

Protocol item (planned)	Deviation (actual systematic review)	Rationale
Outcomes: Extract all primary outcomes including multidimensional deprivation, wellbeing, individual empowerment, social capital, traditional knowledge, and equity	Listed outcomes here dropped at data extraction phase and not included in the analysis	Resource constraints given the large volume of potentially relevant studies, with many in preliminary test sample found not to be relevant
Scope: Include all published and unpublished studies, including working papers	Excluded working papers (from NBER and CAB abstracts), which resulted in a total exclusion of 8832 studies (8316 from NBER and 516 from CAB abstracts) at title/abstract screening stage.	Resource limitations in processing very large number of unpublished papers and interest to reduce the risk of bias
Data extraction form: Include details on intervention locations and ecological/biome characteristics	Dropped fields related to specific locations and ecological zones/biomes	Identifying and extracting such information proved time consuming, with a level of detail not required for our final analysis

Databases: include CAB abstracts and NBER	Excluded CAB abstracts and NBER	Resource limitations, as described above
Searching process: doing hand searches of other systematic reviews to extract studies not found in the database search process	Hand searches were not conducted	Resource constraints coupled with ensuring a clear and comprehensive main search process
Guiding questions: 1. What interventions have been implemented to reduce poverty in rural areas, and what impacts have they had on poverty and environmental conservation? 2. What interventions have been implemented to support environmental conservation in rural areas of LMICs and what impacts have they had on conservation and poverty alleviation? 3. What are the characteristics of the most effective interventions targeting poverty alleviation and conservation outcomes separately and together?	1. What interventions have been implemented as RCTs to reduce poverty in rural areas, and what impacts have they had on poverty? 2. What interventions have been implemented as RCTs to support environmental conservation in rural areas of LMICs, and what impacts have they had on conservation? 3. Have any studies investigated both poverty alleviation and environmental conservation outcomes? If so, what impacts have they had on joint outcomes?	Guiding questions were refined to make our focus clearer and to align better with available evidence from RCTs, specifically.

Search Strategy Detail

In each of the identified databases, we used consistent combinations of search terms to ensure both comprehensiveness and replicability. We began with a BOOLEAN search that included all relevant search terms, categorized as follows:

1. Type of Study: ("randomized control trial" OR "RCT" OR "RCTs")
2. Topic: ("poverty") OR ("environment"),
3. Outcomes of Interest: For poverty-related searches - ("income" OR "consumption" OR "asset" OR "living conditions" OR "living standards" OR "livelihood" OR "deprivation" OR "wellbeing" OR "non-farm employment" OR "employment" OR "labor" OR "empowerment" OR "capital" OR "equity" OR "traditional knowledge" OR "agricultural production"). For environment-related searches - (“Land use change” OR “Forest Cover” OR “Vegetation” OR “Biodiversity”)
4. Geographic Location: ("village" OR "rural")

After conducting the initial search, we filtered the results by publication year. If the initial search yielded no results, we then systematically explored various combinations of the search terms, starting with one term from each category and then substituting with or adding other terms within the same category. Topic terms remained constant, depending on whether we were focusing on poverty-related or environment-related searches. This process was repeated across all languages of interest, and the searches were conducted between June 2024 and December 2024.

Keyword Searches

Table A2. Variations in Keyword Searches Across Databases

Database	**Search Variations / Boolean Combinations (Sample)**

Web of Science	ALL=("randomized control trial" OR "rct" OR "rcts") AND ALL=("poverty") AND ALL=("income" OR "consumption" OR "asset" OR "living conditions" OR "living standards" OR "livelihood" OR "deprivation" OR "wellbeing" OR "non-farm employment" OR "employment" OR "labor" OR "empower" OR "capital" OR "equity" OR "traditional knowledge" OR "agricultural production") AND ALL=("rural" OR "village") ALL=(“Randomized control trial”) AND ALL=(“poverty”) AND ALL=(“household living conditions”) AND ALL=(“rural”) ALL=(“RCT”) AND ALL=(“poverty”) AND ALL=(“income”) AND ALL=(“village”)
EBSCO: Econ Lit	ALL=("randomized control trial" OR "rct" OR "rcts") AND ALL=("poverty") AND ALL=("income" OR "consumption" OR "asset" OR "living conditions" OR "living standards" OR "livelihood" OR "deprivation" OR "wellbeing" OR "non-farm employment" OR "employment" OR "labor" OR "empower" OR "capital" OR "equity" OR "traditional knowledge" OR "agricultural production") AND ALL=("rural" OR "village") ALL=("randomized control trial" OR "rct" OR "rcts") AND ALL=(“Land use change” OR “Forest Cover” OR "Air Quality" OR “Water quality” OR “Soil quality” OR "Fires" OR “Carbon emissions” OR "Biodiversity" OR “Ecosystem service*” OR “Vegetation”) AND ALL=("rural" OR "village")
SCOPUS	(“Randomized control* trial” OR “RCT*s” OR “RCT”) AND (“Poverty”) AND (“Consumption”) AND (“Rural”)

Intercoder Reliability at the First (Identification) Stage

Our 5% sample included 17 studies that were independently classified as included or excluded by three coders. One coder worked on a subset of 9 studies, and a second coder also worked on a second subset of 9 studies. The third coder evaluated both sets of studies, allowing us to assess the consistency of classifications between Coder 1 and Coder 3 for one subset of studies, and between Coder 2 and Coder 3 for the second subset.

Table A3.1 presents the cross-tabulation of the classifications made by Coders 1 and 3 for Subset 1 studies, while Table A3.2 displays the results for Coders 2 and 3 for Subset 2 studies. Each cell in these matrices represents the number of studies classified as either included or excluded by the coders.

Table A3.1. Coder classifications for Subset 1 studies

		Coder 3		
		Include	Exclude	Sum
	Include	(a) 8	(b) 0	8
	Exclude	(c)1	0	1
Coder 1	Sum	9	0	

Table A3.2. Coder classifications for Subset 2 studies

		Coder 3		
		Include	Exclude	Sum
	Include	(a) 7	(b) 0	7
	Exclude	(c)1	1	2
Coder 2	Sum	8	1	

We used the Jaccard Similarity Index (JSI) to assess the reliability between coders to determine how effectively they identified and applied the inclusion criteria in our sample of studies. A high Jaccard Similarity Index indicates that our inclusion criteria were clear enough for the coders to accurately include or exclude studies, and vice versa. The JSI was calculated as follows:

JSI = $\frac{a}{a + b + c}$

where, a corresponds to the number of studies that were classified as included by both coders,
b corresponds to the number of studies that were classified as included by only Coder 1 (for subset 1) or 2 (for subset 2), and
c corresponds to the number of studies that were classified as included by only Coder 3

The JSI for Subset 1 studies was 0.8889 and for Subset 2 studies it was 0.875.

Data Extraction Form

The data extraction sheet included 40 columns of information to be extracted from each study, as available. These columns, or variables, are listed in Table S4, with definitions and guidance for coding provided for each.

Table A4. Data extraction form

S.No	Variable / Column Name	Definition / Guidance for Coding
1	StudyID	Unique ID assigned to each of the studies
2	Study citation	Chicago style
3	Name of Team Member - Coder	
4	Date of Assessment	
5	Publication Type	Categories could include: Working paper, Academic article, Book chapter
6	Study Author(s)	
7	Year of Publication	
8	Title of Study	
9	Journal or Book Title	

10	Language	
11	Country of first author affiliation	
12	Country where study was conducted	
13	Original/follow-up	A follow-up RCT would be described as a continuation of a previous RCT testing a new adjustment in the intervention or some variation of it.
14	Category	Focus of study on Poverty, Environment, or Both
15	Data Year	Mention of the years in which baseline and endline surveys were collected. When 2 or more endlines are present, include all years until last endline.
16	Length of study (years)	Difference between endline and baseline survey years. Convert months to years when possible. Eg. 6 months = 0.5 years
17	Sampling Strategy Process	The approach used to select participants from the target population for inclusion in the study.
18	Sampling intensity (Sample/population %)	
19	Sample size	
20	# Control units	Number of participants in control group. If there are different treatment arms with different controls, separate the information in the rows corresponding to the treatment arm of reference. If the control group remains

		the same for all treatment arms this information will be the same in all rows
21	# Treated units	Number of participants in treatment. If there are different treatment arms, separate the information in the rows corresponding to the treatment arm of reference
22	Lowest level of randomization	
23	Randomization type	Method used to assign participants to different treatment or control arms
24	Number of RCT Arms (include control)	The total number of groups in the study, each receiving a different intervention or treatment. Include the control arm in the count
25	Eligibility criteria	A set of predefined rules used to determine who can or cannot participate in the study
26	Implementing agency type	The kind of organization responsible for carrying out the study alongside the authors, such as a government body, non-governmental organization (NGO), academic institution, or private company.
27	Intervention_type	The specific kind of treatment, program, or action being tested. If each treatment unit is receiving a package of multiple interventions, say

		" mixed". For multiple treatment arms the information here will correspond to the specific type of intervention category corresponding to the information entered in the row. Intervention types could be (mixed, cash transfer, asset transfer, training/education, technology adoption).
28	Subgroup analysis?	Whether the authors test heterogeneity in their results, mention the subgroups tested if any
29	Treatment Unit	Level at which the treatment was assigned (e.g., individual, household etc)
30	Population studied	Description of the study's target population
31	Intervention detail	Description of the intervention
32	Unit of analysis	Level at which outcomes were measured
33	How was the outcome measured?	Method used to collect outcome information
34	Outcome type	Outcome family across poverty or environmental conservation
35	Specific outcome	Name of the outcome as it appears in the results table
36	Indicator variables used	Metrics or variables used to define the outcome
37	Time frame of outcome measurement	Recall period or time reference for outcomes.

38	Direction of outcome	Whether the intervention changed the outcome negatively, positively or not at all?
39	Substantive findings	summary of results
40	Policy recommendation as stated by authors	Summary of policy recommendations stated in the study (if any)

Intercoder reliability at the data extraction stage

We evaluated inter-coder reliability for data extraction using a sample of nine studies. Unlike the inclusion stage, where coders assessed whether an entire study should be included or excluded on a binary scale, in the extraction stage, coders independently identified the relevant estimations from each study and extracted them. We then assessed the reliability of both coders by comparing the number of estimations extracted, which allowed for a more continuous evaluation rather than a binary one.

Table A5. Cross-tabulation of the number of estimations extracted by Coders 1 and 2.

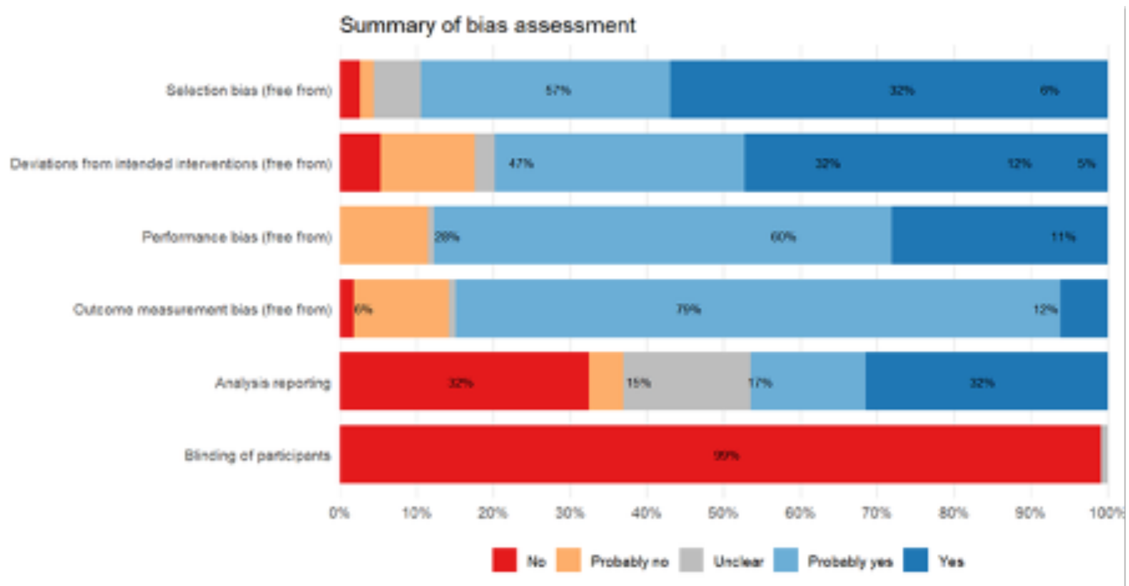
		Coder 2		
		Extracted	Not Extracted	
	Extracted	(a) 114	(b) 53	167
	Not Extracted	(c)9	0	9
Coder 1		123	53	

The Jaccard Similarity Index (JSI) was calculated as described above. The result at this stage was 0.6477

Bias Assessment Detail

The review examined a comprehensive range of potential biases across the included studies. Specifically, we assessed whether trials were pre-registered, whether the correct unit of analysis was used, and whether studies were free from major risks of selection bias, deviations from intended interventions, performance bias, and outcome measurement bias. We also examined the transparency of analysis reporting and whether observers or participants were blinded. Figure A1 presents the results for each criterion across all randomized studies.

Figure A1. Summary of bias assessment



Randomization generally ensured that the risk of selection bias was relatively small, with a large majority of studies (89%) judged free or probably free from this risk. In most cases, authors explicitly tested for and attempted to rule out bias. For example (Bernard et al., 2017) acknowledged that their sample was not representative of the overall population, since it disproportionately included treated farmers. To address this, they added intermediate fixed effects and compared their findings with earlier market studies, concluding that farmer characteristics did not drive the imbalance. While the sampling frame was imperfect, these checks suggest that selection bias was probably not a major threat to validity. The remaining 11% of studies were marked as unclear due to missing or insufficient information on attrition (S. Ahmed et al., 2020) provide a strong example: they lost nearly 60% of their sample due to village-level attrition and reported relatively large (7.5–12%) differential attrition across arms.

Although the differences were not statistically significant, the authors themselves concluded that treatment assignment at the village level could no longer be considered cleanly experimental.

Regarding risks of spillovers, crossovers, and contamination, 80% of studies were adequately safeguarded. For example, Abel et al. (2019) restricted workshops to minimize cross-group exposure, making spillovers unlikely. Similarly, Wang et al. (2018) ensured that all students within a school received the same treatment, reducing the risk of spillovers. Only six studies (5%) had outcomes at clear risk of deviations, while 15% were either probably at risk or unclear due to missing information. For example, (Ksoll et al., 2016; Romero et al., 2019; Rudolf et al., 2020) reported that some farmers in control groups nevertheless received elements of the intervention (such as manuals, seedlings, etc.) with treatment take-up spilling into control villages, indicating partial contamination.

Another potential bias during data collection is performance bias, where most studies (88%) were judged to be free/probably free from performance bias, stating that participation or monitoring was unlikely to influence behavior. For example Ahmed et al. (2024) implemented an intensive training program, and participants' overwhelming positive feedback raises the possibility that responses reflected social desirability rather than the actual effects of the intervention. However, a minority of studies (12%) raised concerns, typically when the intervention involved intense training, repeated contact, or heightened awareness of observation, which may have influenced participant responses independent of the intervention itself. Similar concerns were noted in Rudolf et al. (2020) and Slough et al. (2021), where self-reported intentions regarding tree planting, forest cover, and governance outcomes were likewise sensitive to social desirability.

Most studies (85%) were free or probably free from outcome measurement bias. In many cases, outcomes were measured using standardized surveys or objective indicators, limiting the scope for outcome bias. However, a minority of studies (15%) raised concerns, typically when outcomes relied on self-reports, long recall periods, or subjective indicators. For example, Wang et al. (2018) measured savings through self-reported surveys with children, which the authors acknowledged could be inaccurate. However, most studies acknowledged that self-reported data and the subjective nature of such outcomes could introduce bias.

Across the RCTs, transparency and reporting were often limited. Just under half of the studies (46%) were considered probably free from this risk, as they reported results with reasonable transparency. In contrast, 37% of studies showed clear signs of selective reporting, making it uncertain whether all relevant analyses were disclosed, while 17% of studies were unclear due to insufficient information. For example, Wong et al. (2020) did not reference any pre-analysis plan, trial registry, or pre-registered outcomes, while Ahmed et al. (2020) noted pre-registration but provided no trial protocol. None of the studies blinded participants to their treatment status, which is expected in field-based RCTs where participants are directly involved in receiving training, payments, or inputs. Pre-registration was also uneven across studies.

Appendix B. Reference Information for the 112 Included Studies

Author(s)	Year of Publication	Title of Paper	Journal or Book Title	Category
Abel, M., Burger, R., Carranza, E., Piraino, P	2019	Bridging the Intention-Behavior Gap? The Effect of Plan-Making Prompts on Job Search and Employment	American Economic Journal: Applied Economics	Poverty
Adjognon, G.S., van Soest, D., & Guthoff, J.	2021	Reducing Hunger with Payments for Environmental Services (PES): Experimental Evidence from Burkina Faso	American Journal of Agricultural Economics	Poverty
Aggarwal, S., Francis, E., & Robinson, J	2018	Grain today, gain tomorrow: Evidence from a storage experiment with savings clubs in Kenya	Journal of Development Economics	Poverty
Akhter Ahmed, Fiona Coleman, Julie Ghostlaw, John Hoddinott, Purnima Menon, Aklima Parvin, Audrey Pereira, Agnes Quisumbing, Shalini Roy, and Masuma Younus	2024	Increasing production diversity and diet quality: Evidence from Bangladesh	American Journal of Agricultural Economics	Poverty
Akhter U. Ahmed, John Hoddinott, Shalini Roy, Esha Sraboni	2024	Transfers, Nutrition Programming, and Economic Well-Being: Experimental Evidence from Bangladesh	World Development	Poverty
Ahmed, S., McIntosh, C., & Sarris, A.	2020	The Impact of Commercial Rainfall Index Insurance:	American Journal of Agricultural Economics	Poverty

		Experimental Evidence from Ethiopia		
Aker, J.C., Boumnijel, R., McClelland, A., & Tierney, N.	2016	Payment Mechanisms and Antipoverty Programs: Evidence from a Mobile Money Cash Transfer Experiment in Niger	Economic Development and Cultural Change	Poverty
Aker, J. C., & Jack, B. K.	2023	Harvesting the rain: The adoption of environmental technologies in the Sahel	Review of Economics and Statistics	Poverty
Alfonsi, L., Bandiera, O., Bassi, V., Burgess, R., Rasul, I., Sulaiman, M., & Vitali, A	2020	Tackling Youth Unemployment: Evidence from a Labor Market Experiment in Uganda	Econometrica	Poverty
Ambler, K., de Braw, A., & Godlonton, S.	2020	Cash Transfers and Management Advice for Agriculture: Evidence from Senegal	The World Bank Economic Review	Poverty
Angelucci, M., & Bennett, D.	2024	The Economic Impact of Depression Treatment in India: Evidence from Community-Based Provision of Pharmacotherapy	American Economic Review	Poverty
Angelucci, M., Heath, R., & Noble, E.	2023	Multifaceted Programs Targeting Women In Fragile Settings: Evidence from the Democratic Republic of Congo	Journal of Development Economics	Poverty
Arouna, A., Michler, J. & Lokossou, J.	2021	Contract Farming And Rural Transformation: Evidence from a Field Experiment in Benin	Journal of development economics	Poverty

Asfaw, S., Carraro, A., Davis, B., Handa, S., & Seidenfeld, D.	2017	Cash transfer programmes, weather shocks and household welfare: evidence from a randomised experiment in Zambia	Journal of Development Effectiveness	Poverty
Ashraf, N., Gine, X., Karlan, D.	2009	Finding Missing Markets (and a Disturbing Epilogue): Evidence from an Export Crop Adoption and Marketing Intervention in Kenya	American Journal of Agricultural Economics	Poverty
Attanasio, O., Guarin, A., Medina, C., & Meghir, C	2017	Vocational Training for Disadvantaged Youth in Colombia: A Long-Term Follow-Up	American Economic Journal: Applied Economics	Poverty
Attanasio, O., Augsburg, B., Haas, R.D., Fitzsimons, E., and Harmgart, H.	2015	The Impacts of Microfinance: Evidence from Joint-Liability Lending in Mongolia	American Economic Journal: Applied Economics	Poverty
Ausburg, B., de Haas, R., Harmgart, H., & Meghir, C	2015	The Impacts of Microcredit: Evidence from Bosnia and Herzegovina	American Economic Journal: Applied Economics	Poverty
Austrian, K., Soler-Hampejsek, E., Behrman, J. R., Digitale, J., Hachonda, N. J., Bweupe, M., and Hewett, P. C.	2020	The impact of the Adolescent Girls Empowerment Program (AGEP) on short and long term social, economic, education and fertility outcomes: a cluster randomized controlled trial in Zambia	BMC public health	Poverty
Bola Amoke Awotide, Taiwo Timothy Awoyemi, Kabir	2013	Impact of Seed Voucher System on Income Inequality and Rice Income per Hectare	Quarterly Journal of International Agriculture	Poverty

Kayode Salman, and Aliou Diagne		among Rural Households in Nigeria: A Randomized Control Trial (RCT) Approach		
Baird, S., Hicks, J.H., Kremer, M., & Miguel, E.	2016	Worms at Work: Long-run Impacts of a Child Health Investment	The quarterly journal of economics	Poverty
Ban, R., Gilligan, M., Rieger, M.	2020	Self-help groups, savings and social capital: Evidence from a field experiment in Cambodia	Journal of Economic Behavior & Organization	Poverty
Oriana Bandiera, Robin Burgess, Narayan Das, Selim Gulesci, Imran Rasul, and Munshi Sulaiman	2017	Labor Markets and Poverty in Village Economies	The Quarterly Journal of Economics	Poverty
Bandiera, O., Buehren, N., Burgess, R., Goldstein, M., Gulesci, S., Rasul, I., & Sulaiman, M.	2017	Women's Empowerment in Action: Evidence from a Randomized Control Trial in Africa	American Economic Journal: Applied Economics	Poverty
Bandiera, O., Burgess, R., Deserranno, E., Morel, R., Rasul., I., Sulaiman, M., Thiemel, J.	2022	Microfinance and diversification	Economica	Poverty
Abhijit Banerjee, Esther Duflo, Nathanael Goldberg, Dean Karlan, Robert Osei, William Parienté, Jeremy Shapiro, Bram	2015	A multifaceted program causes lasting progress for the very poor: Evidence from six countries	Development Economics	Poverty

Thuysbaert, and Christopher Udry				
Banerjee, A., Karlan, D., Osei, R., Trachtman, H., Udry, C.	2022	Unpacking a multi-faceted program to build sustainable income for the very poor	Journal of Development Economics	Poverty
Banerjee, A., Hanna, R., Olken, B. A., Satriawan, E. and Sumarto, S.	2023	Electronic Food Vouchers: Evidence from an At-Scale Experiment in Indonesia	American Economic Review	Poverty
Banerjee, A., Duflo, E. & Sharma, G	2021	Long-Term Effects of the Targeting the Ultra Poor Program	American Economic Review: Insights	Poverty
Baseler, T.	2023	Hidden Income and the Perceived Returns to Migration	American Economic Journal: Applied Economics	Poverty
Batista, C. and Vicente, P. C.	2020	Improving access to savings through mobile money: Experimental evidence from African smallholder farmers.	World Development	Poverty
Battaglia, M., Gulesci, S., & Madestam, A.	2023	Repayment Flexibility and Risk Taking: Experimental Evidence from Credit Contracts	Review of Economic Studies	Poverty
Beaman, L., Karlan, D., Thuysbaert, B., & Udry, C.	2013	Profitability of Fertilizer: Experimental Evidence from Female Rice Farmers in Mali	American Economic Review	Poverty
Beaman, L., Karlan, D., Thuysbaert, B., & Udry, C.	2023	Selection into Credit Markets: Evidence from Agriculture in Mali	Econometrica	Poverty

Bernard, T., de Janvry, A., Mbaye, S. &, Sadoulet, E.	2017	Expected Product Market Reforms and technology Adoption by Senegalese Onion Producers	American Journal of Agricultural Economics	Poverty
Bird, S.S., Carter, M.R., Lybbert, T.J., Mathenge, M.W.K., Njeru, T.N., Tjernström, E.	2022	Filling a Niche? The Maize Productivity Impact of an Adaptive Breeding by a Localized Company in Kenya	Journal of Development Economics	Poverty
Blattman, C. & Annan, J.	2016	Can Employment Reduce Lawlessness and Rebellion? A Field Experiment with High-Risk Men in a Fragile State	American Political Science Review	Poverty
Blattman, C., Fiala, N., & Martinez, S.	2014	Generating Skilled Self-Employment in Developing Countries: Experimental Evidence from Uganda	The Quarterly Journal of Economics	Poverty
Brune, L. & Kerwin, J.T	2019	Income timing and liquidity constraints: Evidence from a randomized field experiment	Journal of Development Economics	Poverty
Brune, L., Giné, X., Goldberg, J. & Yang, D	2016	Facilitating Savings for Agriculture: Field Experimental Evidence from Malawi	Economic Development and Cultural Change	Poverty
Brune, L., Chyn, E., & Kerwin, J.	2021	Pay Me Later: Savings Constraints and the Demand for Deferred Payment	American Economic Review	Poverty
Brune, L., Kerwin, J.T., & Li, Q	2022	How Important is temptation Spending? Maybe Less tha We Thought	The World Bank Economic Review	Poverty

Bryan, G., Choi, J. J., and Karlan, D.	2021	Randomizing religion: The impact of protestant evangelism on economic outcomes	The Quarterly Journal of Economics	Poverty
Bryan, G., Chjowdhury, S., Mobarak, A.M., Morten, M. & Smits, J	2023	Encouragement and distortionary effects of conditional cash transfers	Journal of Public Economics	Poverty
Cahyadi, N., Hanna, R., Olken, B.A., Prima, R.A., Satriawan, E., & Syamsulhakin, E.	2020	Cumulative Impacts of Conditional Cash Transfer Programs: Experimental Evidence from Indonesia	American Economic Journal: Economic Policy	Poverty
Cecchi, F., Garcia, A., Lensink, R., and Wydick, B.	2022	Aspirational hope, dairy farming practices, and milk production: Evidence from a randomized controlled trial in bolivia.	World Development	Poverty
Couture, V., Faber, B., Gu, Y., Liu, L.	2021	Connecting the Countryside via E-Commerce: Evidence from China	American Economic Review: Insights	Poverty
Bruno Crépon, Florencia Devoto, Esther Duflo, William Pariente	2015	Estimating the Impact of Microcredit on Those Who Take It Up: Evidence from a Randomized Experiment in Morocco	American Economic Journal: Applied Economics	Poverty
de Mel, S., McIntosh, C., Sheth, K., and Woodruff, C.	2022	Can mobile-linked bank accounts bolster savings? evidence from a randomized controlled trial in sri lanka	Review of Economics and Statistics	Poverty
Dean, J.T. & Jayachandran, S.	2019	Changing Family Attitudes to Promote Female Employment	AEA Papers and Proceedings	Poverty

Duflo, E., Kremer, M., & Robinson, J.	2008	How High Are Rates of Return to Fertilizer? Evidence from Field Experiments in Kenya	American economic review	Poverty
Edmonds, E. & Schady, N.	2012	Poverty Alleviation and Child Labor	American Economic Journal: Economic Policy	Poverty
Edmonds, E. & Theoharides, C.	2020	The Short Term Impact of a Productive Asset Transfer in Families with Child Labor: Experimental Evidence from the Philippines	Journal of Development Economics	Poverty
Egger, D., Haushofer, J., Miguel, E., Niehaus, E., & Walker, M.	2022	General equilibrium effects of cash transfers: experimental evidence from Kenya	Econometrica	Poverty
Field, E., Pande, R., Rigol, N., Schaner, S., & Moore, C.T.	2021	On Her Own Account: How Strengthening Women,Äôs Financial Control Impacts Labor Supply and Gender Norms	American Economic Review	Poverty
Günther Fink, B. Kelsey Jack, Felix Masiye	2020	Seasonal Liquidity, Rural Labor Markets and Agricultural Production	American Economic Review	Poverty
Gertler, P.J., Martinez, S.W. & Rubio-Codina, M.	2012	Investing Cash Transfers to Raise Long-Term Living Standards	American Economic Journal: Applied Economics	Poverty
Green E.P.; Blattman C.; Jamison J.; Annan J.	2015	Women's entrepreneurship and intimate partner violence: A cluster randomized trial of microenterprise assistance and partner participation in post-conflict Uganda	Social Science & Medicine	Poverty

Hamory, J., Miguel, E., Walker, M., Kremer, M., Baird, S.	2021	Twenty-year economic impacts of deworming	Proceedings of the National Academy of Sciences	Poverty
Harou, A.P., Madajewicz, M., Michelson, H., Palm, C.A., Amuri, N., Magomba, C., Semoka, J.M., Tschirhart, K., Weil, R.	2022	The joint effects of information and financing constraints on technology adoption: Evidence from a field experiment in rural Tanzania	Journal of Development Economics	Poverty
Johannes Haushofer, Jeremy Shapiro	2016	The Short-term Impact of Unconditional Cash Transfers to the Poor: Experimental Evidence from Kenya	The Quarterly Journal of Economics	Poverty
Hjelm, L., Handa, S., de Hoop, J., Palermo, T.	2017	Poverty and perceived stress: Evidence from two unconditional cash transfer programs in Zambia	Social science & medicine	Poverty
Horn, S., Jamison, J., Karlan, D., & Zinman, J.	2023	Five-Year Impacts of Group-Based Financial Education and Savings Promotion for Ugandan Youth	Review of Economics and Statistics	Poverty
Jensen, R.	2012	Do Labor Market Opportunities Affect Young Women’s Work and Family Decisions? Experimental Evidence from India	The Quarterly Journal of Economics	Poverty
Karlan, D., Osei, R., Osei-Akoto, I., Udry, C.	2014	Agricultural Decisions After Relaxing Credit and Risk Constraints	The Quarterly Journal of Economics	Poverty

Karlan, D., Savonitto, B., Thuysbart, B., & Udry, C.	2017	Impact of savings groups on the lives of the poor	Proceedings of the National Academy of Sciences	Poverty
Lee, Kenneth, Miguel, and Wolfram	2020	Experimental Evidence on the Economics of Rural Electrification	Journal of Political Economy	Poverty
Koomson, I., Villano, R.A., & Hadley, D.	2021	Accelerating the impact of financial literacy training programmes on household consumption by empowering women	Applied Economics	Poverty
Koomson, I., Villano, R.A., & Hadley, D.	2022	The role of financial literacy in households' asset accumulation process: evidence from Ghana	Review of Economics of the Household	Poverty
Ksoll, C., Lilleor, H.B., Lonborg, J.H., Rasmussen, O.D.	2016	Impact of Village Savings and Loan Associations: Evidence from a cluster randomized tria	Journal of Development Economics	Poverty
Lee, J.N., Morduch, J., Ravindran, S., Shonchoy, A., and Zaman, H.	2021	Poverty and Migration in the Digital Age: Experimental Evidence on Mobile Banking in Bangladesh	American Economic Journal: Applied Economics	Poverty
Leight, J., Awonon, J., Pedehombga, A., Ganaba, R., Martinez, E., Heckert, J., Gelli, A.	2022	The impact of an integrated value chain intervention on household poultry production in Burkina Faso: evidence from a randomized controlled trial	Journal of Development Effectiveness	Poverty
Macours, K., Premand, P., & Vakis, R.	2022	Transfers, Diversification and Household Risk Strategies: Can Productive Safety Nets Help Households Manage Climatic Variability?	The Economic Journal	Poverty

Martinez, C.A. & Perticara, M.	2017	Childcare effects on maternal employment: Evidence from Chile	Journal of Development Economics	Poverty
Matsuda, A., Aoyagi, K., Mochizuki, T., & Uematsu, M.	2024	A booster to conditional cash transfer programme: experimental evidence on financial inclusion and livelihood enhancement project	Journal of Development Effectiveness	Poverty
McKenzie, D., Mohpal, A., & Yang, D.	2022	Aspirations and Financial Decisions: Experiemental Evidence from the Phillipines	Journal of Development Economics	Poverty
Muralidharan, K., Niehaus, P., & Sukhtankar, S.	2023	General Equilibrium Effects of (Improving) Public Employment Programs: Experimental Evidence from India	Econometrica	Poverty
Nakano, Y., & Magezi, E. F.	2020	The impact of microcredit on agricultural technology adoption and productivity: Evidence from randomized control trial in Tanzania	World Development	Poverty
Premand, P., & Stoeffler, Q.	2022	Cash transfers, climatic shocks and resilience in the Sahel	Journal of Environmental Economics and Management	Poverty
Ragasa, C., Amewu, S., Agyakwah, S. K., Mensah, E.T.D., Asmah, R.	2022	Impact of aquaculture training on farmers' income: Cluster randomized controlled trial evidence in Ghana	Agricultural Economics	Poverty
Ragasa, C., Amewu, S., Agyakwah, S. K.,	2022	Impact of aquaculture training on farmers' income: Cluster	Agricultural Economics	Poverty

Mensah, E.T.D., Asmah, R.		randomized controlled trial evidence in Ghana		
Rahman, A., Bhattacharjee, A., and Das, N.	2021	A good mix against ultra-poverty? evidence from a randomized controlled trial (RCT) in Bangladesh.	Review of Development Economics	Poverty
Rosas, N., Acevedo, M. C., and Zaldivar, S.	2022	Starting points matter: Cash plus training effects on youth entrepreneurship, skills, and resilience during an epidemic.	World Development	Poverty
Schaner, S.	2018	The persistent power of behavioral change: Long-run impacts of temporary savings subsidies for the poor.	American Economic Journal: Applied Economics	Poverty
Tan, XH., Han, XT., Zheng, YF., Jin, L., Qiu, XZ., Zhu, Y., Chen, C., Zhang, JQ., Dickey, H., Wang, DC., Huang, SS., Liu, B., Liang, XL., Zeng, YF., Lin, HT., He, MG., Luo, LX., Huang, WY., Congdon, N., Liu, YZ., AF Tan, Xuhua., Han, Xiaotong., Zheng, Yingfeng., Jin, Ling., Qiu, Xiaozhang., Zhu, Yi., Chen, Chuan., Zhang, Jiaqing., Dickey,	2023	Impact of Cataract Surgery on Income in Rural Southern China: The SUCCESS Randomized Controlled Trial	The Asia-Pacific Journal of Ophthalmology	Poverty

Heather., Wang, Decai., Huang, Shengsong., Liu, Bin., Liang, Xiaoling., Zeng, Yangfa., Lin, Haotian., He, Mingguang., Luo, Lixia., Huang, Wenyong., Congdon, Nathan., Liu, Yizhi				
Tarozzi, A., Desai, J., & Johnson, K.	2015	The Impacts of Microcredit: Evidence from Ethiopia	American Economic Journal: Applied Economics	Poverty
Bossuroy, T., Goldstein, M., Karimou, B., Karlan, D., Kazianga, H., Parient√©, W., ... & Wright, K. A.	2022	Tackling Psychosocial and Capital Constraints to Alleviate Poverty	Nature	Poverty
Wang, J.S-H., Malaeb, B., Ssewamala, F.M., Neilands, T.B., Brooks-Gunn, J.	2021	A Multifaceted Intervention with Savings Incentives to Reduce Multidimensional Child Poverty: Evidence from the Bridges Study (2012, 2018) in Rural Uganda	Social Indicators Research	Poverty
Ward, Patrick S.; Mapemba, Lawrence; and Bell, Andrew R.	2021	Smart subsidies for sustainable soils: Evidence from a randomized controlled trial in southern Malawi	Journal of Environmental Economics and Management	Poverty
Warrier Mukherjee. S., Bergquist. L.F.,	2024	Unlocking the Benefits of Credit Through Saving	Journal of Development Economics	Poverty

Burke, M, & Miguel, E.				
Weiser S.D.; Bukusi E.A.; Steinfeld R.L.; Frongillo E.A.; Weke E.; Dworkin S.L.; Pusateri K.; Shiboski S.; Scow K.; Butler L.M.; Cohen C.R.	2015	Shamba Maisha: randomized controlled trial of an agricultural and finance intervention to improve HIV health outcomes	AIDS	Poverty
Wolf, S., Kembou, S., Ogan, A., & Jasinska, K	2024	Cash transfers improve economic conditions and reduce maternal stress in rural Côte d'Ivoire	Journal of Child and Family Studies	Poverty
Wong, H.L., Wei, X., Kahsay, H.B., Gebreegziabher, Z., Gardebroek, C., Osgood, D.E., Diro, R.	2020	Effects of input vouchers and rainfall insurance on agricultural production and household welfare: Experimental evidence from northern Ethiopia	World Development	Poverty
Yuliani, E., & Nasrudin, R. A.	2024	The effects of the Indonesian conditional cash transfer program on transition out of agriculture.	World Development	Poverty
Julia Shu-Huah Wang, Fred M. Ssewamala, Torsten B. Neilands, Laura Gauer Bermudez, Irwin Garfinkel, Jane Waldfogel, Jeannie Brooks-Gunn, Jing You	2018	Effects of Financial Incentives on Saving Outcomes and Material Well-Being: Evidence From a Randomized Controlled Trial in Uganda	Journal of Policy Analysis and Management	Poverty

Ahmed, A., Hoddinott, J., & Roy, S	2025	Food Transfers, Cash Transfers, Behavior Change Communication and Child Nutrition: Evidence from Bangladesh	The World Bank Economic Review	Poverty
Shalini Roy, Melissa Hidrobo, John Hoddinott, Akhter Ahmed	2019	Transfers, Behavior Change Communication, and Intimate Partner Violence: Postprogram Evidence from Rural Bangladesh	The Review of Economics and Statistics	Poverty
Angelucci, Manuela, and Giacomo De Giorgi	2009	Indirect Effects of an Aid Program: How Do Cash Transfers Affect Ineligibles' Consumption?	American Economic Review	Poverty
Tanguy Bernard, Sylvie Lambert, Karen Macours, Margaux Vinez	2023	Impact of small farmers' access to improved seeds and deforestation in DR Congo	Nature Communications	Environment
Sabrina Eisenbarth, Louis Graham, Anouk S. Rigterink	2021	Can community monitoring save the commons? Evidence on forest use and displacement	Proceedings of the National Academy of Sciences	Environment
Garbach, Kelly, Mark Lubell, and Fabrice A. J. DeClerck.	2012	Payment for Ecosystem Services: The Roles of Positive Incentives and Information Sharing in Stimulating Adoption of Silvopastoral Conservation Practices.	Agriculture, Ecosystems & Environment	Environment
Jack, K.	2013	Private Information and the Allocation of Land USe Subsidies in Malawi	American Economic Journal: Applied Economics	Environment

M. Romero, M. Wollni, K. Rudolf, R. Asnawi, B. Irawan	2019	Promoting biodiversity enrichment in smallholder oil palm monocultures - Experimental evidence from Indonesia	World Development	Environment
Rudolf, Katrin, Miriam Romero, Rosyani Asnawi, Bambang Irawan, and Meike Wollni	2020	Effects of Information and Seedling Provision on Tree Planting and Survival in Smallholder Oil Palm Plantations	Journal of Environmental Economics and Management	Environment
Slough, T., Kopas, J., & Urpelainen, J.	2021	Satellite-based deforestation alerts with training and incentives for patrolling facilitate community monitoring in the Peruvian Amazon	Proceedings of the National Academy of Sciences of the United States of America	Environment
Wilebore, Voors, Bulte, Coomes, & Kontoleon	2019	Unconditional transfers and tropical forest conservation: Evidence from a randomized control trial in Sierra Leone.	American Journal of Agricultural Economics	Environment
Wren-Lewis L, Becerra-Valbuena L, Houngbedji K.	2020	Formalizing land rights can reduce forest loss: Experimental evidence from Benin	Science Advances	Environment
Braganca, A., Newton, P., Cohn, A., & Searchinger, T.D.	2022	Extension services can promote pasture restoration: Evidence from Brazil's low carbon agriculture plan	Proceedings of the National Academy of Sciences	Both
Coppock, D. L., Crowley, L., Durham, S. L., Groves, D.,	2022	Community-based rangeland management in Namibia improves resource governance	Communications Earth & Environment	Both

Jamison, J. C., Karlan, D., Norton, B. E., & Ramsey, R. D.		but not environmental and economic outcomes		
B. Kelsey Jack, Elsa Cardona Santos	2017	The Leakage and Livelihood Impacts of PES Contracts: A Targeting Experiment in Malawi	Land Use Policy, Volume 63, Pages 645–658	Both
Jayachandran, Seema, Joost de Laat, Eric F. Lambin, Charlotte Y. Stanton, Robin Audy, and Nancy E. Thomas	2017	Cash for carbon: A randomized trial of payments for ecosystem services to reduce deforestation	Science	Both
Saavedra, Santiago	2022	Economic Development and Environmental Conservation: Evidence From Eco-Tourism	Economic Development and Cultural Change	Both

Appendix C. Additional Detail on Studies Measuring Both Poverty and Environmental Outcomes

We identified five studies that assessed both poverty and environment related outcomes. These studies are briefly described below. Across these studies, a total of 39 outcome-intervention arm estimations were reported. Figures C1-C3 summarize the reported effects across these interventions, categorized as monetary, non-monetary or both. Of the 39 reported arm estimations, 11 were excluded from these figures due to unclear statistical significance in the original studies, and a further 2 estimations were excluded because the magnitude of the reported effects were zero.

1. Jayachandran et al., (2017) evaluated village-level payments for environmental services (PES) for avoided deforestation in Uganda. Villages were offered annual payments conditional on conserving privately held forests, and the analyzed outcomes were measured using high-resolution satellite data and household surveys. The theoretical expectation was that conditional transfers would increase the private value of standing forest, lowering the incentives to clear. The program halved tree-cover loss relative to controls, which we classified as an environmental win (with a statistically significant effect). However, the study identified no strong detectable changes in household economic outcomes, although there was some weak evidence of an increase in non-food expenditures. We consider this to be a poverty alleviation win/neutral effect (no statistically significant effects).
2. Jack & Cardona Santos (2017) tested a contract at the household level in Malawi, including double random assignment for contract allocation and mechanism (auction vs. lottery). The intervention plausibly engaged both allocation (payments) and production (tree planting, competing with labor/land for crops) pathways. In the pooled comparison, environmental gains were modest and sometimes offset by within-farm leakage (lottery arm), while welfare effects were near zero. By contrast, auction targeting attenuated leakage and improved environmental performance without detectable welfare losses, which we categorize as an environmental win (statistically significant), and a poverty win/neutral (although statistically not significant).

3. Saavedra (2025) randomized a program for eco-tourism promotion in Colombia at the municipal level, including support through training, product design, marketing, and marketplace access. The program operated through changed production practices by shifting labor toward services, as well as a changed allocation of resources (raising returns for nature conservation), and some ancillary social elements like the promotion of business networks. As a result, the intervention substantially increased tourist arrivals and employment, and reduced deforestation around promoted sites, which we consider an environmental win. However, it did not raise profits or household incomes over the study horizon, which we categorized as a poverty loss (although statistically not significant). The author described this as being consistent with time-to-profitability and adjustment costs.
4. Bragança et al., (2022) tested a pasture-restoration program for cattle producers in the Cerrado, Brazil, randomizing training only versus training and technical assistance (TA) at the producer level. The main mechanism expected to produce results was the adjustment in production dynamics, including the adoption of improved pasture and herd practices, with potential environmental gains from intensification (avoiding expansion and enhancing soil carbon). The authors identified significant intervention effects. Training and TA delivered larger and economically meaningful gains in adoption and revenues, alongside positive environmental benefits (which we categorize as a win-win result). Training alone produced weaker, inconsistent effects (environmental win and poverty loss, although not statistically significant in either case).
5. Coppock et al., (2022) randomized community-based rangeland and livestock management (CBRLM) across rangeland intervention areas in Namibia. Their theory of change rested primarily on social dynamics (collective governance, rotational grazing, coordinated herding) with a secondary production channel (husbandry/marketing). According to the authors, the program generated large, persistent improvements in collective action and governance behaviors, yet did not translate into measurable improvements in rangeland condition or household economic outcomes over the study period. We classified this result as poverty win/neutral and environment loss (although statistically insignificant in both cases).

Figure C1. Impacts of monetary interventions on poverty and environment outcomes

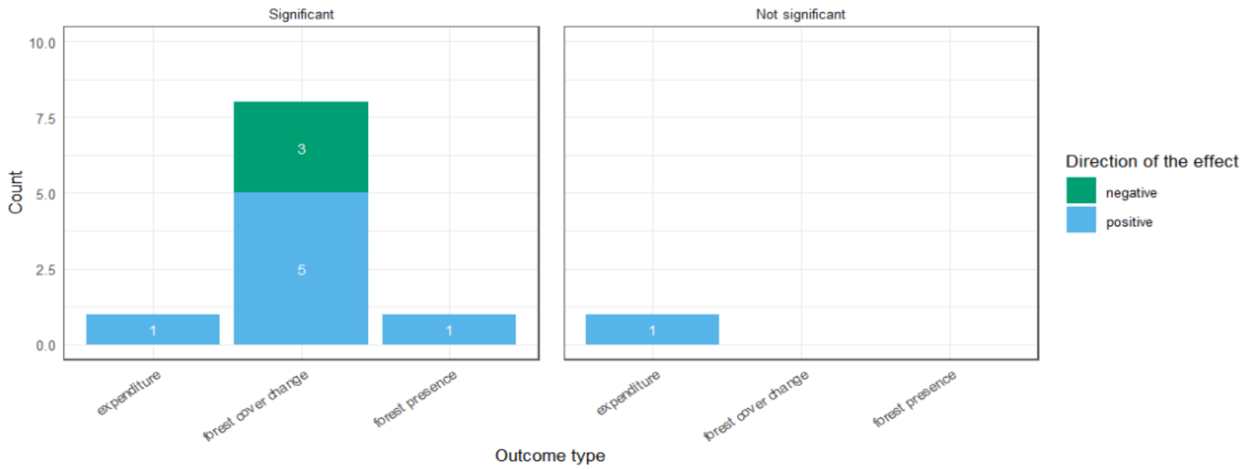


Figure C2. Impacts of non-monetary interventions on poverty and environment outcomes

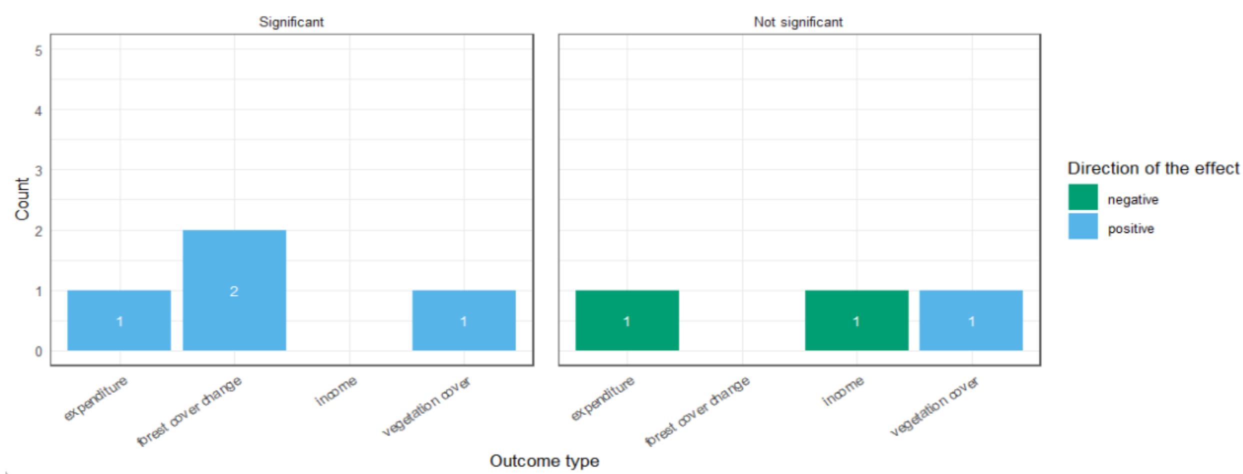


Figure C3. Impacts of mixed interventions on poverty and environment outcomes