Who Should Pay for the Wetland? Payments for Ecosystem Services and Demand for Government Action in Rural Uganda*

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Abstract

We embed an information-provision experiment in a post-treatment survey of a multilateral-funded wetland-conservation project in rural Uganda (N = 1,636 households in 16 wetland systems across eight districts) and test how alternative framings of a prospective wetland-conservation incentive scheme shift household perceptions and preferred government action. A short script describing direct payments for ecosystem services (PES) leaves stated perceptions of conservation largely unchanged but sharply reallocates policy preferences: respondents' stated preference for government financial compensation falls by 10 percentage points against a 55% baseline ($p < 0.01$), offset by higher demand for infrastructure investment (+7 percentage points, $p < 0.01$). A long-term economic framing that makes no mention of direct payments produces a near-identical shift; a purely environmental framing produces a much smaller, marginally significant shift on the same margin and moves nothing else. Where responses differ by wetland dependence, they are concentrated among households less tied to the wetland: the reallocation attenuates for the heaviest wetland users, who respond to the PES framing with higher trust and perceived government effectiveness instead. The patterns are broadly consistent with a demand-side analogue of motivation crowding in PES, though the reduced-form evidence is also consistent with Bayesian updating about the fiscal incidence of conservation; we view the demand-side margin as a complement to the supply-side focus that has dominated the existing PES crowding literature.

JEL Codes: Q57, Q58, D83, O13, H41, Q23.

Keywords: payments for ecosystem services; wetland conservation; information provision; policy preferences; motivation crowding; adaptation finance; Uganda.

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1 Introduction

Wetlands sit at the intersection of biodiversity, climate, and rural livelihoods. They sequester disproportionate amounts of carbon for their share of land area, regulate hydrological cycles within their catchments, and supply fish, fodder, water, and seasonal cultivation grounds to the populations living adjacent to them. Globally, tropical and subtropical wetlands have been lost at several times the rate of tropical forests since the 1970s, with the most rapid losses concentrated in low- and lower-middle-income countries where agricultural-conversion pressure presses hardest against the resource. The development cost of that loss falls first on rural households whose subsistence is bound up with the wetlands they live next to, and second on the downstream populations who lose the regulating services those wetlands previously provided.

The standard set of policy responses to wetland degradation is by now well rehearsed. Command-and-control regulation—designating wetlands as protected areas, prohibiting cultivation or drainage—is the oldest and most widely deployed instrument, but its effectiveness is bounded by the enforcement capacity of subnational states and is in tension with the livelihood needs of the populations living adjacent to the resource. Alternative-livelihood programs, which seek to relieve pressure on wetlands by raising the returns to non-wetland economic activity (improved seed varieties, livestock packages, off-farm training), have a mixed record. Direct conservation transfers—payments to households or communities conditional on conservation effort—were introduced as a market-mechanism alternative to both, beginning in the late 1990s and growing rapidly through the 2000s as a standard component of internationally financed conservation portfolios in tropical settings (Ferraro and Pattanayak 2006; Jack et al. 2008).

Payments for Ecosystem Services (PES) are now the largest and best-documented class of conservation transfer. The conceptual case, that conditional payments can correct the externality between private cultivation incentives and public conservation value, is well developed, and the empirical evidence, though mixed, is generally supportive. With this evidence base, PES has become a near-default component of climate-adaptation and forest-conservation finance in low-income tropical settings.

A separate question—much less studied—is how prospective beneficiaries respond to information about a PES scheme, before any payment changes hands. The existing PES-effectiveness literature is overwhelmingly concerned with supply-side outcomes (deforestation, forest cover, household income) among populations already enrolled in or eligible for a scheme. Yet the political economy of any government-scale PES program turns substantially on what citizens come to expect of the state once they learn that an external actor may be paying for conservation. If learning about a non-state PES payer leads citizens to redirect what they ask of their government—toward enforcement and infrastructure, away from direct compensation—then the communication of PES has political-economy consequences independent of whether and how individual households eventually enroll. To our knowledge, no field-experimental evidence on this demand-side margin exists in a low-income setting. We provide it.

We embed a four-arm information-provision experiment in the endline survey of a multi-district wetland-conservation program in rural Uganda. The host program is a multi-actor wetland-restoration and climate-adaptation initiative implemented in 2017 by the Government of Uganda with support from the Green Climate Fund (GCF) and the United Nations Development Programme; it operates across 24 districts in Eastern and Southwestern Uganda. The endline survey, fielded in October 2024 under the program's Learning-Oriented Real-Time Assessment, was designed to allow a survey-experiment module after the main household questionnaire. After completing the main module, each of $N = 1{,}636$ respondents is randomly read one of four short scripts about a prospective wetland-conservation program: a *neutral* control that describes the program generically; an *environmental* framing that emphasizes biodiversity and ecosystem services; a *long-term economic* framing that emphasizes durable community gains (higher yields, sustainable tourism); and a *direct-payment* framing that describes an international organization compensating households for conservation, that is, a PES scheme. Respondents then answer nine items measuring comprehension, trust, perceived importance of conservation, perceived environmental benefits, perceived economic benefits, willingness to vacate, willingness to join alternative-livelihood programs, perceived effectiveness of government policy, and—the item around which the paper turns—a forced-choice question about which single type of government action they would most prefer.

The paper makes three points. First, framing barely moves stated perceptions. On all seven Likert items, the treatment effects are small in magnitude, and only two cross conventional significance thresholds at all: the PES framing reduces perceived importance of conservation by 0.15 points on a 5-point scale ($p < 0.01$), and the long-term economic framing reduces perceived environmental benefits by 0.12 points ($p < 0.10$). Baseline means on these items sit between 3.9 and 4.3 out of 5, so ceiling effects do real work in suppressing measured variation. Second, on the preferred-action margin, which has no ceiling because it is a forced choice over five mutually exclusive policy options, framing produces a large and sharply patterned reallocation. Both the PES and the long-term economic framings cut the share of respondents who prefer "provide financial compensation" from a 55% baseline by 10 percentage points ($p < 0.01$), with the offset going into demand for infrastructure investment (+6 to +7 pp, $p < 0.01$); the PES framing additionally reduces preference for awareness campaigns (−4 pp, $p < 0.05$). The purely environmental framing produces a much smaller, marginally significant movement on the compensation margin (−6 pp, $p < 0.10$) and nothing else. Third, the response varies with the household's relationship to the wetland, on two margins. Among households that report no direct benefits from nearby wetlands, the environmental framing sharply lowers perceived government effectiveness (−0.64, $p < 0.01$) and the PES framing raises preference for training and inputs (+0.17, $p < 0.05$); households that do benefit are unmoved on both margins (interactions +0.68, $p < 0.01$, and −0.16, $p < 0.05$). And within beneficiary households, the reallocation away from compensation is concentrated among lighter wetland users: it attenuates sharply for those with above-median wetland activity (interactions +0.29 to +0.32 on the compensation margin, $p < 0.01$).

We interpret this pattern as a demand-side analogue of motivation crowding. Existing evidence on PES crowding out, both from framed laboratory games (Cardenas et al. 2000; Andersson et al. 2018) and from programs in the field (Alix-Garcia et al. 2015; Rode et al. 2015; Ezzine-de-Blas et al. 2019), documents how introducing monetary incentives can undermine the intrinsic and social motivations underpinning voluntary conservation. In that literature, the crowding is on the supply side: on the behavior of those who would conserve. Our results suggest that a structurally similar displacement occurs on the demand side, in what citizens ask of their government. Learning that an outside party may fund conservation

appears to displace the claim on the state to compensate, with no accompanying change in stated intrinsic valuation. The mechanism is observationally near-identical to a Bayesian update about the fiscal incidence of conservation—citizens learn that the government's role will be complementary rather than substitutive—and we cannot distinguish these two readings without eliciting prior and posterior beliefs about who bears the cost. Both readings, however, point to the same implication for policy: the communication of PES reshapes citizens' policy-action demands independently of any change in how much they value the underlying ecosystem.

The paper engages three strands of literature. The first evaluates PES effectiveness in developing countries. Wunder (2007) laid out the conceptual case for performance-based conservation transfers, and Engel et al. (2008) formalized the corresponding design principles. Empirical evaluations have produced a mixed record: Pattanayak et al. (2010) synthesize early evidence and find modest, heterogeneous effects on environmental outcomes, while Samii et al. (2014) systematize the methodological challenges to attributing impact to PES schemes. Jayachandran et al. (2017) provided randomized evidence from a deforestation-reduction PES program in Uganda, demonstrating substantial avoided deforestation at modest social cost. More recent framed-field-experimental work continues in this vein: Nyanghura et al. (2024) find that conditional payments raise conservation effort among Tanzanian farmers even under unequal land ownership. With limited exception, this literature measures supply-side outcomes (forest cover, household income, conservation effort) among populations already enrolled in or eligible for a scheme. We measure what happens on the demand side, before enrollment: how the policy preferences of the population the scheme is designed to reach shift when they first learn about it.

The second strand is the behavioral and institutional critique of PES, which has questioned whether monetary incentives can durably crowd out the intrinsic, reciprocal, or social-norm-based motivations that previously sustained conservation behavior. Kosoy and Corbera (2010) argue that PES risks reframing conservation as a commodity transaction; Muradian et al. (2010) propose an alternative conceptual framework that emphasizes the relational and motivational dimensions of conservation. Rode et al. (2015) and Ezzine-de-Blas et al. (2019) review the empirical evidence on motivation crowding in conservation contexts and identify

the design conditions under which payments displace rather than reinforce pro-environmental behavior. More recent contributions have extended this critique with new field evidence: Moros et al. (2023) document persistence-of-effects issues after PES withdrawal in a Colombian field experiment, and Kolinjivadi et al. (2023) synthesize the case that PES design ignores institutional and political-economy context. Our paper adds an experimentally identified piece of evidence to this literature, in a live government-scale adaptation program in a low-income Sub-Saharan African setting, on a margin that the existing crowding literature has not addressed: the demand for government action that prospective beneficiaries express upon learning of a PES scheme, before any payment changes hands.

The third strand is the rapidly growing literature on information-provision experiments and the formation of policy preferences. Alesina et al. (2018) and Stantcheva (2021) use the design to study mobility beliefs and tax preferences; Haaland et al. (2023) survey the methodology and document the magnitudes typically recovered. Almost all of this work has been administered in OECD settings on macroeconomic or social-policy attitudes. We adapt the template to a low-income, rural climate-adaptation setting and to an environmental-policy outcome—preferred state action over a wetland-conservation menu—for which a single piece of information about a prospective external incentive produces a reallocation at the upper end of the magnitudes the literature reports.

Section 2 describes the project, the sampling frame, the experimental design, and the outcomes. Section 3 sets out the conceptual framework and the interpretive readings we will bring to the results. Section 4 describes the data and the empirical strategy and reports balance. Section 5 presents the results on perceptions, preferred actions, and heterogeneity by wetland dependence. Section 6 discusses alternative interpretations and implications, and Section 7 concludes.

2 Context and Experimental Design

2.1 Setting: Ugandan wetlands and the wetland-conservation program

Ugandan wetlands cover roughly eight percent of the country's surface area and are constitutionally held in public trust. The 1995 National Environment Act, administered by the National Environment Management Authority (NEMA), designates wetlands as environmentally sensitive areas and prohibits their conversion to private uses without authorization; the Ministry of Water and Environment holds sectoral responsibility for water resources and catchments. In practice, day-to-day wetland management is decentralized to district and sub-county environment officers and, at the community level, to wetland management committees. Against this formal architecture, expansion of cultivation into seasonal wetlands, drainage for rice and vegetable production, brick-making, and fuelwood extraction have continued to erode wetland extent, with enforcement capacity thin at the sub-national level.

The wetland-conservation program we study was designed as a multi-actor response to this degradation and to its climate-adaptation consequences for rural livelihoods. It operated in eight Ugandan districts—four in the East (Bukedea, Kumi, Namutumba, Ngora) and four in the Southwest (Bushenyi, Kisoro, Mitooma, Rubirizi)—across sixteen wetland systems between 2017 and 2025. The program had three components: restoration and catchment management on the wetlands themselves; promotion of climate-resilient livelihoods (improved varieties, livestock and aquaculture packages, energy-saving stoves) for households living adjacent to wetlands; and strengthening of climate-information and early-warning services. Implementation was coordinated with the Ministry of Water and Environment and with district-level wetland and environment offices, so restoration activity was embedded in the same governance hierarchy through which day-to-day wetland regulation is exercised. A mid-term review in 2021 recommended that a payment-for-ecosystem-services component be integrated into the program's successor phase; that recommendation is part of the motivation for the present study.

A panel of 1,644 households was surveyed between October 2022 and February 2023

and at endline between September and November 2024 under the Learning-Oriented Real-Time Assessment (LORTA) programme. Sampling was hierarchical: sixteen wetland systems (eight treated, eight comparison); six villages per system; seventeen households per village. The initial quasi-experimental restoration-effect design faced the identification difficulties familiar to environment-sector impact evaluations of this kind—no pre-intervention household data (the first survey round took place in 2022, after implementation had begun) and imperfect demographic comparability across treated and comparison systems. The endline round therefore layered a survey experiment onto the impact evaluation, both to generate independent evidence on behavioral response to PES-style incentives and to directly inform the mid-term recommendation.

Two features of this setting bear on interpretation. First, wetlands in our sampling frame are not uniformly "consumed" by adjacent households: some systems are actively used for cultivation, grazing, fishing, and water collection, while others are more peripheral to local livelihoods. We therefore treat pre-existing wetland dependence—households that report using nearby wetlands for productive or subsistence purposes—as an ecologically-motivated axis of heterogeneity, on the prior that respondents whose livelihoods are bound up with the resource may respond to PES framing differently from those for whom the resource is more distant. Second, because the formal governance architecture places wetlands in public trust with enforcement mandated but unevenly implemented, the menu of policy actions that respondents can plausibly endorse—compensation, enforcement, infrastructure, training, awareness—is a menu of actions the Ugandan state already has some standing to take. This is not a setting in which respondents are being asked to imagine policy instruments that are institutionally foreign to them.

2.2 Experimental design

At the end of the endline household questionnaire—after the modules on demographics, assets, income, expenditures, agriculture, wetland use, and food security—enumerators initiated a ten-minute survey-experiment module. Each respondent was randomly assigned (by a pre-loaded draw with equal probability) to one of four groups and read the corresponding script verbatim. The four scripts, reproduced here as administered, are:

- **Neutral** (control; $N = 386$). "Our wetland conservation program is focused on preserving the wetlands in your community. We are working to ensure that these natural areas are protected for future generations."

- **Environmental** ($N = 528$). "Wetlands are crucial for a healthy environment. They provide homes for many plants and animals, purify our water, and help control floods. By conserving wetlands, we are protecting these natural services that benefit everyone. Your participation in our conservation program helps safeguard these vital ecosystems."

- **Long-term economic** ($N = 364$). "Protecting wetlands not only preserves the environment but also supports the local economy. Healthy wetlands can improve soil fertility, increase crop yields, and provide opportunities for sustainable tourism. By participating in our conservation efforts, you are investing in the long-term economic prosperity of your community."

- **Immediate payment (PES)** ($N = 358$). "An international organization is considering introducing a Payment-for-Ecosystem-Services (PES) scheme in Uganda to reward communities for conserving wetlands. Under this scheme, community members who protect wetlands will receive financial rewards for their efforts. This program ensures that those who contribute to environmental conservation are fairly compensated, making it possible for you to see direct economic benefits from participating in wetland conservation."

The three treatment scripts differ only in the locus and timing of the return to conservation. The environmental script emphasizes ecosystem services as their own reward. The long-term-economic script emphasizes durable private returns to the community, mediated by agricultural productivity and tourism, with no external transfer. The PES script is the only one that describes an external actor transferring resources directly to households. This design allows us to estimate the marginal effect of introducing an explicit monetary external reward relative to a non-monetary community-benefit narrative (the PES versus long-term-economic contrast), and of introducing economic narrative relative to the neutral control (the long-term-economic versus neutral contrast).

2.3 Outcomes

Immediately after the script, enumerators administered nine outcome items: a comprehension check, seven Likert-scale perception items, and a single forced-choice question on preferred government action. The exact wording of all nine items is reproduced in Appendix 7. Throughout the analysis we treat the seven perception items and the five binary indicators derived from the preferred-action item as the outcomes of interest, and use comprehension as a manipulation check (discussed below).

Perception items. The seven Likert items measure trust in the conservation project, perceived importance of wetland conservation, perceived environmental effectiveness of conservation projects, perceived economic effectiveness, willingness to vacate wetland areas if requested, willingness to join an alternative-livelihood program, and perceived effectiveness of government policies in protecting wetlands. Each item uses a five-point response scale; we recode so that higher values indicate a more conservation-favorable response. These are the seven outcomes that appear in Figure 1 and Appendix Table A1.

Preferred-action item. The eighth substantive item is a forced choice: "Which type of government action do you believe would be most beneficial for wetland conservation in your community?" Respondents pick one of five mutually exclusive options—provide financial compensation, offer training and inputs, enforce environmental laws, invest in infrastructure, or conduct awareness campaigns. We construct five binary indicators, one per option, that together partition the choice set; these are the outcomes in Table 2 and Figure 2.

Comprehension. The first item asks how clear the message was on a five-point scale (recoded so that higher values are clearer). We use it in two ways. First, as a manipulation check: comprehension is uniformly high across arms (neutral-arm mean 4.43 out of 5; standard deviation 0.82). Each of the three treatment arms reports comprehension that is slightly but statistically detectably lower than the neutral arm, with differences of about -0.13 to -0.15 points on the 5-point scale (Appendix Table A8). The differences are small in magnitude—all treatment-arm means remain above 4.27—and we treat the script as having been understood broadly equivalently across arms. Second, as a (post-treatment) heterogeneity probe: Appendix Figures A6 and A7 compare subgroup treatment effects for

respondents who reported the message as "very clear" or "somewhat clear" against those who reported anything less, which we read with appropriate caution given that the conditioning variable is itself measured after treatment.

3 Conceptual Framework

The interpretation of our experimental variation draws on two related literatures. The first is the motivation-crowding tradition that has grown up around payments for ecosystem services. Starting from the general argument that explicit incentives can displace intrinsic or pro-social motivation (Frey and Jegen 2001; Bowles 2008), a substantial body of ecological-economics work has argued that PES schemes in particular risk reframing a conservation act from a moral or relational obligation into a priced transaction (Kosoy and Corbera 2010; Rode et al. 2015; Ezzine-de-Blas et al. 2019; Kolinjivadi et al. 2023; Moros et al. 2023). Empirically, the evidence is mixed: crowding effects have been documented in some settings and small or absent in others (Alix-Garcia et al. 2015; Andersson et al. 2018), and the conditions under which payments complement rather than displace pro-environmental motivation remain actively debated. A second tradition, overlapping in authorship, has emphasized that conservation values are plural and relational: people hold views about wetlands, forests, and biodiversity that are not fully captured by willingness-to-pay or by a single scalar of environmental concern, and exposure to policy framings can shift which of those values is made salient without necessarily changing any of them (Muradian et al. 2010; Chan et al. 2016; Pascual et al. 2014). Framing effects of this kind are well established in the political-economy and public-opinion literatures (Chong and Druckman 2007; Haaland et al. 2023).

This paper makes a demand-side move within that framing. Prior crowding work is almost uniformly concerned with conservation *supply*: whether households enrolled in a PES scheme continue to exert conservation effort, voluntarily contribute to a public good, or sustain pro-environmental behaviour once the payment is withdrawn. Our experiment varies information about hypothetical PES receipt and asks what respondents then *demand of government* with respect to the same resource. To our knowledge this demand-side reading

of motivation crowding has not been tested in a rural low-income setting, and it fits naturally within the relational-values tradition: if a PES script reorganizes how respondents construe the state's role in wetland conservation—say, from enforcer and provider of complementary public goods to transfer agent—that should register in the forced-choice policy-preference item even when Likert measures of environmental concern are unmoved.

We anticipate three candidate readings of any effects we observe, which we will take up again in the discussion. The first is a demand-side motivation-crowding reading: information that external compensation is available displaces the normative claim that the government should itself compensate, by reframing compensation as an externally provisioned private transfer rather than a state obligation. The second is a Bayesian-updating reading in the spirit of information-provision experiments in other domains (Haaland et al. 2023; Stantcheva 2021): respondents treat the PES script as news about the policy menu and reallocate demand toward the remaining state actions (enforcement, infrastructure) that are less substitutable with an external payment. The third is a purely cognitive-salience reading: the script primes certain policy levers and makes them more likely to be selected from a forced-choice menu, without any underlying change in preferences or beliefs. These readings are not mutually exclusive—indeed the first nests both a preference shift and a reallocation—but they imply different patterns across Likert outcomes, across alternative framings that do not mention payments, and across pre-existing wetland dependence. Section 6 returns to them once the empirical pattern is in hand.

4 Data and Empirical Strategy

4.1 Sample and balance

Of the 1,644 endline respondents, 1,636 were assigned to and completed the survey-experiment module. Table 1 reports respondent and household characteristics by treatment arm and tests for balance on each covariate against the neutral control. Most covariates are balanced to within a half-standard error, with a small number of marginally significant differences (age is about 2.3 years lower in the PES arm, household size is about 0.9 persons lower in

the environmental arm) that is consistent with chance rejection under the 42 pairwise tests displayed. Pre-treatment food-insecurity scores are 0.4 points higher in the PES arm, which we address by including the food-insecurity score in the pre-treatment control vector $\mathbf{X}_i$ defined below; results without controls are essentially identical.

Table 1: Sample description and balance across treatment arms

	(1) Mean (SD)	(2)	(3)	(4)
		Coefficient on Difference (Treatment - Control)		
	Neutral message group	Environmental benefits	Economic benefits	Immediate payment (PES)
Panel A. Respondent background				
Age of respondent	47.97	-1.11	-0.50	-2.26*
	(16.14)	(1.05)	(1.13)	(1.16)
=1 if female respondent	0.43	0.01	-0.03	-0.03
		(0.03)	(0.04)	(0.04)
=1 if respondent is household head	0.85	0.02	0.02	-0.05*
		(0.02)	(0.03)	(0.03)
Years of education of household head	6.76	-0.09	-0.01	-0.22
	(3.73)	(0.28)	(0.30)	(0.32)
Number of household members	7.23	-0.94***	-0.61**	-0.00
	(4.47)	(0.30)	(0.30)	(0.34)
=1 if own housing	0.92	0.00	0.01	-0.02
		(0.02)	(0.02)	(0.02)
Number of years having lived in current land	32.85	-0.91	0.97	-1.06
	(20.28)	(1.33)	(1.46)	(1.46)
Household dietary diversity score (HDDS)	5.73	-0.14	-0.05	0.03
	(2.87)	(0.20)	(0.21)	(0.21)
Food insecurity experience scale (FIES)	2.63	-0.20	0.24	0.40**
	(2.42)	(0.16)	(0.19)	(0.18)
Panel B. Economic welfare of household				
Monthly income (over past 12 months, USD)	225.75	13.72	1.56	0.45
	(412.75)	(29.08)	(31.92)	(31.82)
Annual expenditures (USD)	459.25	-26.73	-13.87	-3.11
	(589.54)	(38.57)	(41.36)	(43.80)
Panel C. Agriculture				
Land used for crop production (acre)	2.94	0.11	-0.01	0.01
	(3.16)	(0.23)	(0.26)	(0.25)
Total expenditure on crop inputs (USD)	57.51	-12.66*	-3.20	-10.77
	(105.75)	(6.73)	(7.81)	(7.29)
Number of crops cultivated	4.98	-1.14***	-0.93***	-0.53
	(4.37)	(0.27)	(0.29)	(0.33)
Observations	386	528	364	358

Notes. Column 1 reports the mean and standard deviation in the neutral-message (control) arm. Columns 2–4 report the coefficient on an indicator for the corresponding treatment arm from a regression of the row variable on a treatment indicator, restricted to observations in the neutral arm and the indicated treatment arm. Heteroskedasticity-robust standard errors in parentheses. Stars: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The bottom row reports the number of observations in each arm.

4.2 Specification

For each outcome Y_i we estimate the following regression specification:

$$Y_i = \beta_1 \cdot \text{Env}_i + \beta_2 \cdot \text{LongEcon}_i + \beta_3 \cdot \text{PES}_i + \mathbf{X}_i'\boldsymbol{\gamma} + \varepsilon_i, \tag{1}$$

The coefficients β_1, β_2, and β_3 are the intention-to-treat (ITT) effects of each script relative to the neutral control. $\mathbf{X}_i$ is a vector of pre-treatment controls, all measured in the endline interview in modules administered before the experimental script: household size, land owned, the food-insecurity (FIES) score, and an indicator for whether the household reports benefiting from nearby wetlands.[1] We report heteroskedasticity-robust standard errors; because treatment is assigned independently at the respondent level, there is no natural cluster larger than the respondent. We pre-specify the preferred-action indicators as the primary outcome family and apply the Romano–Wolf stepdown procedure (Romano and Wolf 2005) within that family to control the family-wise error rate; the remaining six perception items are secondary. Adjusted p-values are reported in table notes.

For heterogeneity, we augment equation (1) with an indicator for whether the household reports direct benefits from a nearby wetland, fully interacted with each treatment dummy. The indicator comes from the endline wetland-use module, which precedes the experimental script, so it is pre-treatment; 82% of households report benefiting. Because benefiting is so common, we also report an intensity-based alternative: within beneficiary households, an indicator for an above-median count of distinct wetland activities (fishing, water collection, livestock grazing, cultivation of wetland edges). In a parallel specification, reported in the appendix, we interact with an indicator for a female respondent.

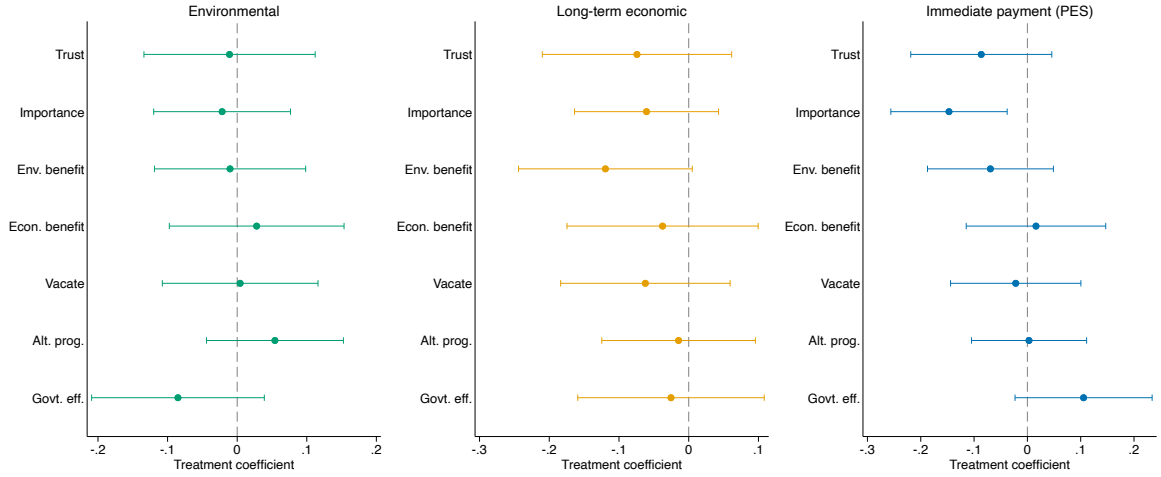
[1] There are some variables missing for a subset of households (e.g. land size is collected only in the agricultural module). We recode missing control values to zero and include missing-value binary indicators, so that every regression retains the full experimental sample of $N = 1{,}636$.

5 Results

5.1 Perceptions move very little

Figure 1 summarizes the estimated treatment effects on the seven perception items, and Appendix Table A1 reports the underlying point estimates. The picture is one of near-uniform small effects. Of the twenty-one coefficients, two cross the 10% threshold and only one clears 1%: the PES framing reduces perceived importance of conservation by 0.15 points on a five-point scale (relative to a neutral-arm mean of 4.25, $p < 0.01$) and the long-term economic framing reduces perceived environmental benefits by 0.12 points (relative to 4.19, $p < 0.10$). The PES framing's coefficient on perceived government policy effectiveness is positive but imprecise (+0.11, $p = 0.11$). All other coefficients are within ±0.09 of zero.

Figure 1: Treatment effects on seven perception items



Notes. Each panel reports the OLS coefficient from equation (1) for one treatment arm (Environmental, Long-term economic, or Immediate payment (PES)) against the neutral control, estimated separately for each outcome with the pre-treatment controls described in Section 4 (missing control values recoded to zero, with missing-value indicators included). Horizontal bars are 95% confidence intervals from heteroskedasticity-robust standard errors. All outcomes are coded on a 1–5 scale with higher values more conservation-favorable.

The near-null is itself informative. Baseline means across the perception items range from 3.89 (trust) to 4.25 (importance of conservation), so the measurement scale is compressed at the upper end: it is hard to move an already-high average in the conservation-favorable direction, and it is hard to distinguish small sincere shifts from Likert-scale rounding error. The sign pattern on the PES arm is nonetheless consistent with a particular behavioral

interpretation. The arm that mentions direct monetary compensation simultaneously (i) lowers stated intrinsic valuation of conservation and (ii) directionally raises the sense that the government is doing its job (+0.11, $p = 0.11$)—the two-part signature of motivation crowding as formalized in Frey and Jegen (2001); Bowles (2008), with the second part rather suggestive. We return to this in Section 6.

5.2 Preferred government action reallocates sharply

Table 2: Effects of message framing on preferred government action

	Financial comp.	Training & inputs	Enforcement	Infrastructure	Awareness
Environmental	-0.060*	0.017	0.012	0.021	0.010
	(0.033)	(0.025)	(0.022)	(0.015)	(0.022)
Long-term economic	-0.099***	0.018	0.031	0.061***	-0.012
	(0.036)	(0.028)	(0.025)	(0.019)	(0.024)
Immediate payment (PES)	-0.099***	0.036	0.041	0.066***	-0.044**
	(0.037)	(0.028)	(0.026)	(0.019)	(0.022)
Control mean	0.55	0.16	0.12	0.04	0.12
Control SD	0.50	0.37	0.33	0.20	0.33
Observations	1,636	1,636	1,636	1,636	1,636
R-squared	0.018	0.004	0.007	0.011	0.015

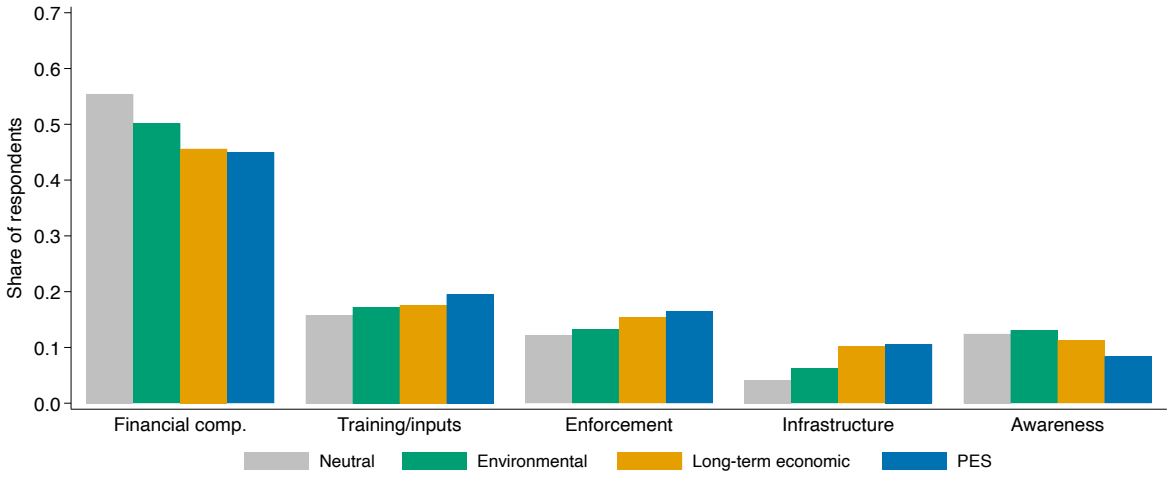
Notes. Each column is a separate OLS regression of a binary indicator for the respondent's preferred type of government action on the three treatment dummies. The outcome is 1 if the respondent names the column-header action as their single most-preferred government action and 0 otherwise; the five actions together partition the choice set. Neutral-arm means and standard deviations, computed over all 386 neutral-arm respondents, are reported in the lower panel. All regressions include the pre-treatment controls described in Section 4, with missing control values recoded to zero and missing-value indicators included, so the estimation sample is the full experimental sample. Heteroskedasticity-robust standard errors in parentheses. Stars: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Romano–Wolf stepdown-adjusted p-values for the treatment-effect coefficients are reported in Appendix Table A6.

The preferred-action results appear in Table 2 and, visually, in Figure 2. In the neutral control arm, 55% of respondents name "provide financial compensation" as their single most-preferred government action—a striking majority, consistent with an anchoring expectation that the wetland-conservation program ought to involve transfers from the state. Behind that, 16% prefer training and inputs, 12% enforcement, 12% awareness campaigns, and 4% infrastructure.

Two of the three framings substantially redistribute this composition. The long-term-economic script cuts preference for financial compensation by 10 percentage points ($p < 0.01$) and raises preference for infrastructure investment by 6 percentage points ($p < 0.01$). The

PES script cuts preference for financial compensation by an identical 10 percentage points ($p < 0.01$), raises infrastructure preference by 7 percentage points ($p < 0.01$), and reduces awareness-campaign preference by 4 percentage points ($p < 0.05$). The environmental script produces a much smaller version of the same movement: preference for financial compensation falls by 6 percentage points ($p < 0.10$), and the remaining four coefficients are within 2 percentage points of zero. Adjusted p-values under the Romano–Wolf stepdown (Appendix Table A6) preserve the significance of the two -0.10 coefficients on financial compensation and two infrastructure coefficients at the 1% level and the PES awareness coefficient at the 5% level; the environmental compensation coefficient survives at the 10% level (adjusted $p = 0.081$).

Figure 2: Composition of preferred government action by treatment arm



Notes. Each bar is the share of respondents in that arm naming the action on the x-axis as their single most-preferred government action; the five options partition the choice set, so the five bars within an arm sum to one. Vertical segments are 95% confidence intervals from Wald tests on shares. The legend at the bottom distinguishes the four arms.

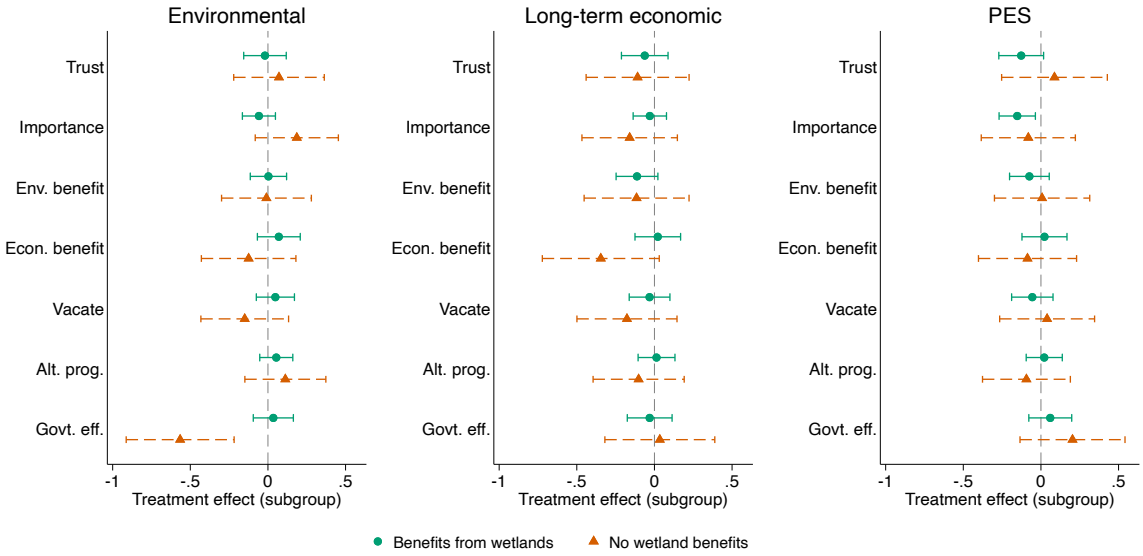
Two observations about these magnitudes are worth registering. First, the size of the reallocation is substantial: a 10-percentage-point movement off a 55%-share dominant option, after a two-minute verbal script, is an effect size consistent with the upper range reported in the information-provision literature (Haaland et al. 2023). Second, the pattern is strongly pairwise informative. The long-term-economic and PES arms produce a near-identical reallocation despite differing on the single dimension that has the sharpest behavioral interpretation—whether or not an external actor pays. The environmental arm,

which matches the neutral arm in making no reference to payments or to economic returns, comes closest to matching the neutral arm's preferred-action distribution, moving only the compensation share and only marginally. The three contrasts suggest that the effective margin is information about economic returns, private or external, rather than the mention of external monetary transfer specifically; the purely environmental message produces at most a faint echo of the same substitution.

5.3 Heterogeneity by wetland dependence

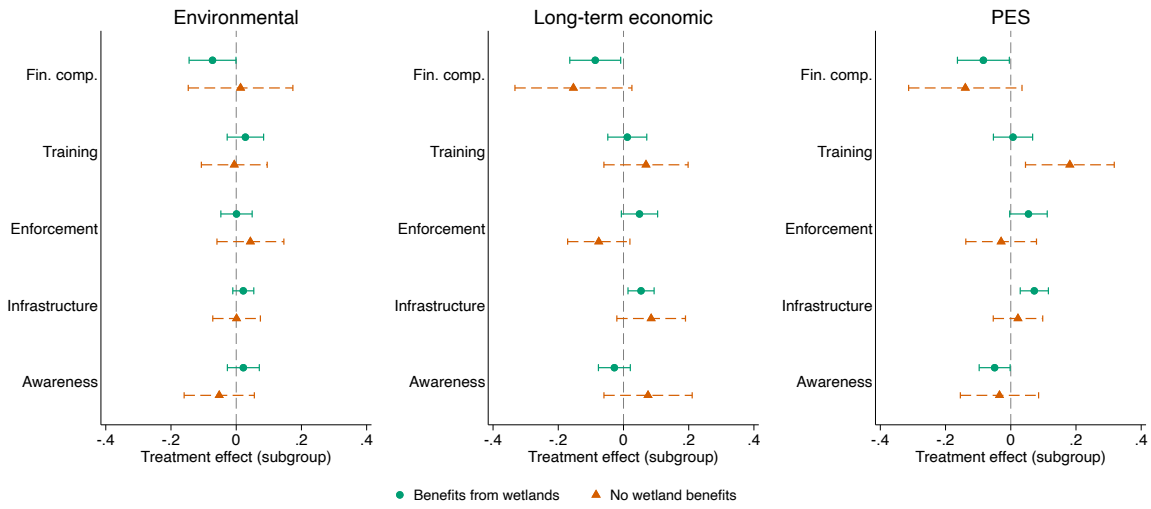
A large majority of the sample—82%, or 1,342 of 1,636 households—reports that the household directly benefits from a nearby wetland (through fishing, water collection, livestock grazing, or cultivation of wetland edges). This is also the population the program is designed to serve. We therefore examine heterogeneity on two margins: an extensive margin, comparing households that report wetland benefits against those that do not, and an intensity margin, comparing—within beneficiary households—those with an above-median count of distinct wetland activities against the rest. Figures 3 and 4 report subgroup treatment effects for the extensive margin, and Appendix Tables A2 and A3 the corresponding interaction-form regressions; Appendix Tables A4 and A5 and Appendix Figures A1 and A2 report the intensity margin.

Figure 3: Subgroup treatment effects on perception items, by wetland benefits (extensive margin)



Notes. Each row reports the estimated treatment effect (relative to the neutral control) on one perception item, separately for households that report directly benefiting from a nearby wetland and those that do not. Within each row, the upper (solid, circle) interval is the beneficiary subgroup; the lower (dashed, triangle) interval is the non-beneficiary subgroup. The three columns correspond, left to right, to the Environmental, Long-term economic, and Immediate payment (PES) arms. Horizontal bars are 95% confidence intervals from heteroskedasticity-robust standard errors. Subgroup-specific estimates are obtained by sample splits on the wetland-benefits indicator, with the pre-treatment controls described in Section 4; the corresponding interaction tests in the pooled specification are reported in Appendix Tables A2 and A3.

Figure 4: Subgroup treatment effects on preferred government action, by wetland benefits (extensive margin)



Notes. Each row reports the estimated treatment effect on the binary indicator that the respondent named the row's action as their single most-preferred government action, separately for households that report directly benefiting from a nearby wetland and those that do not. Within each row, the upper (solid, circle) interval is the beneficiary subgroup; the lower (dashed, trangle) interval is the non-beneficiary subgroup. The three columns correspond, left to right, to the Environmental, Long-term economic, and Immediate payment (PES) arms. Horizontal bars are 95% confidence intervals from heteroskedasticity-robust standard errors.

Two patterns in the extensive-margin results stand out. First, the environmental framing sharply lowers perceived government effectiveness among households that report no wetland benefits (-0.64, $p < 0.01$), while beneficiary households are unmoved; the interaction is $+0.68$ ($p < 0.01$) and is the only coefficient in the heterogeneity analysis that comfortably survives a family-wise correction across the thirty-six extensive-margin interaction tests. A message that extols what wetlands provide appears to remind precisely the households that receive none of those benefits of what the government has not delivered. Second, the PES framing raises preference for training and inputs only among non-beneficiaries ($+0.17$, $p < 0.05$; interaction -0.16, $p < 0.05$). The compensation margin itself, by contrast, reallocates for beneficiaries and non-beneficiaries alike: the treatment-by-benefit interactions on financial compensation are small and insignificant, so the headline reallocation is broad-based rather than concentrated in either group.

The intensity margin tells a complementary story. Within beneficiary households, above-median wetland users are 26 percentage points less likely than lighter users to prefer financial

compensation in the neutral arm ($p < 0.01$), preferring enforcement (+14 pp, $p < 0.01$) and awareness campaigns (+9 pp, $p < 0.05$) instead. The scripts' compensation-reducing effects are correspondingly concentrated among lighter users: the interactions on financial compensation are +0.32 (long-term economic, $p < 0.01$) and +0.29 (PES, $p < 0.01$), largely offsetting the average effect for heavy users, who have little compensation preference left to move. On the perception side, the PES script raises trust (+0.50, $p < 0.01$), perceived importance of conservation (+0.24, $p < 0.05$), willingness to vacate (+0.37, $p < 0.01$), and perceived government effectiveness (+0.34, $p < 0.05$) among heavy users relative to lighter ones. The households most enmeshed in wetland use respond to the prospect of compensation with warmth on the perception margin rather than skepticism.

Two cautions apply to this section. The intensity split is defined only within beneficiaries ($N = 1{,}342$), because the activity count is collected only for households reporting benefits. And with seventy-two interaction tests across the two definitions, individually significant interactions beyond the environmental-framing result on government effectiveness should be read as a pattern rather than as isolated findings.

Gender heterogeneity, reported in Appendix Figures A3 and A4, is sharper than the wetland-benefits margin on two items: the long-term-economic framing moves perceived government effectiveness in opposite directions for men (+0.16, $p < 0.10$) and women (−0.22, $p < 0.05$), and in the PES arm preferred enforcement rises for men (+0.11, $p < 0.01$) while falling for women (−0.06, $p < 0.10$). We do not read much into this gap: the baseline heterogeneity could reflect differential expectations about intra-household capture of downstream economic rents, or simply a different prior about the credibility of the scheme.

6 Interpretation and Discussion

The empirical pattern admits the three readings set out in Section 3; we revisit each in light of the evidence, in order of consistency with the coefficient structure.

Demand-side motivation crowding. Information that an external actor may soon pay for conservation substitutes for demanded government payment. The script is a marginal

piece of information, not a transfer; it does not change the household's budget set or observed behavior, only what it asks of the government. A long behavioral-economics literature (Frey and Jegen 2001; Bowles 2008; Rode et al. 2015; Ezzine-de-Blas et al. 2019) documents how the introduction of monetary incentives can crowd out non-market motivations that previously sustained pro-social behavior. That literature is concerned with the *supplier* of the service. Our results suggest a parallel mechanism on the *demand* side, in the political-economy sense of what the citizen asks the state to do. The reallocation away from government financial compensation and toward government roles that an external PES-provider cannot deliver—most clearly physical infrastructure, with smaller and imprecise movements toward enforcement of existing environmental law—is exactly the substitution one would predict under such a mechanism.

Bayesian updating about fiscal incidence. A structurally distinct but observationally close reading is that respondents enter the module with a prior about who will bear the cost of conservation and update that prior when they learn about a prospective external payer. The preferred-action redistribution is then a classical belief-update over the government's optimal role, given that the non-government payer will carry the direct-compensation margin. This reading does not require any appeal to non-market motivations. It does, however, predict the same reduced-form pattern. Distinguishing the two readings cleanly would require eliciting prior and posterior beliefs about the fiscal incidence of PES, which our module does not. Both readings must also accommodate the heterogeneity pattern: the compensation reallocation is broad-based across the benefits margin, and within beneficiary households it is the lighter users who move, while the heaviest users—whose demand for government compensation is low to begin with—respond to the PES script with higher trust and perceived government effectiveness. Under either reading, the PES information is least disruptive precisely where wetland use is most intensive.

Cognitive anchoring on salient options. A third reading is that framing changes the salience of different government actions in a non-belief-updating sense, consistent with the framing tradition in Stantcheva (2021) and Haaland et al. (2023). The PES script explicitly

mentions payment, which could anchor away from "financial compensation" as a preferred government action (since the message has already offered compensation as a non-government alternative)—though the anchoring is partial: financial compensation remains the modal choice in every arm, at 45% in the PES arm against 55% in the neutral arm. The environmental script does not mention payment, and produces at most a marginal movement on the compensation margin. The difficulty with the salience reading is that the long-term-economic script also does not mention payment, and yet produces the same reallocation as the PES script. Salience alone does not predict that long-term-economic and PES should match each other; substitution in the sources of expected compensation does.

The limitations of our design constrain what we can and cannot claim. First, respondents' behavior is not observed: the preferred-action item is a stated policy preference, not a contribution to a public good or an enrollment decision, and the literature on stated versus revealed preferences in PES settings is cautionary. Second, the Likert outcomes compress near the upper bound, which limits our power to detect small sincere movements on perceptions; the cleaner null above should be read as "not large" rather than as "exactly zero." Third, the experiment is embedded in a single, real-world climate-adaptation project in one country, and its external validity to other PES contexts depends on whether the "who else pays" margin is similarly informative elsewhere.

The policy implication is nonetheless concrete. International climate-finance programs that communicate prospective PES schemes to potential beneficiaries should expect the information itself to reshape citizens' policy-action demands, not merely their willingness to enroll. The direction of the reshaping is away from direct government transfers and toward complementary government investments—most clearly infrastructure. Whether this reshaping is welfare-improving depends on whether the government is positioned to deliver those complementary investments. A successful PES rollout that is not accompanied by the infrastructure the PES has raised demand for may leave a political gap that neither the implementing agency nor the GCF originally set out to fill.

7 Conclusion

A two-minute script describing a prospective wetland PES scheme, administered in a large climate-adaptation program in rural Uganda, reallocates citizens' stated preferences over government action substantially and sharply, without moving their stated intrinsic valuation of conservation. The reallocation runs away from financial compensation by the government and toward infrastructure, and is produced equally by a script that mentions direct payment and by a script that mentions only long-term private economic returns to the community. We read this as a demand-side counterpart to the motivation-crowding patterns long documented in the PES and behavioral-economics literatures: learning that conservation may be externally funded shifts what citizens ask of the state, away from direct compensation and toward complementary investment. Responses vary with the household's relationship to the wetland: the sharpest perception response to the environmental message arises among households with no stake in the wetland, and within beneficiary households the reallocation is concentrated among lighter users, attenuating for those whose livelihoods are most bound up with the resource.

Two directions for further work follow. The first is a companion module that elicits beliefs about PES fiscal incidence before and after the script, so as to distinguish the crowding and Bayesian-updating readings cleanly. The second is a revealed-preference follow-up that links script assignment to actual participation once a PES scheme is rolled out in the program's successor phase—an exercise the ongoing coordination with the Ministry of Water and Environment makes plausible.

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Appendix: Supplementary Tables and Figures

A.1 Full perception effects

Table A1: Full effects of message framing on perceptions (Likert outcomes)

	Trust	Conserve	Env. benefit	Econ. benefit	Vacate	Alt. prog.	Govt. effect.
Environmental	-0.011	-0.022	-0.010	0.028	0.004	0.054	-0.085
	(0.063)	(0.050)	(0.055)	(0.064)	(0.057)	(0.050)	(0.063)
Long-term economic	-0.074	-0.060	-0.119*	-0.037	-0.062	-0.015	-0.025
	(0.069)	(0.053)	(0.064)	(0.070)	(0.062)	(0.056)	(0.068)
Immediate payment (PES)	-0.086	-0.147***	-0.069	0.016	-0.022	0.003	0.105
	(0.067)	(0.056)	(0.060)	(0.067)	(0.062)	(0.055)	(0.066)
Control mean	3.89	4.25	4.19	4.09	4.01	4.25	3.94
Control SD	0.95	0.74	0.87	1.01	0.86	0.77	0.95
Observations	1,636	1,636	1,636	1,636	1,636	1,636	1,636
R-squared	0.031	0.039	0.054	0.054	0.044	0.029	0.086

Notes. Each column is a separate OLS regression of the column-header outcome (coded on a 1–5 scale, higher = more conservation-favorable) on the three treatment dummies, with pre-treatment controls described in Section 4 (missing control values recoded to zero, with missing-value indicators included). Heteroskedasticity-robust standard errors in parentheses. Stars: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.2 Full heterogeneity tables: two definitions of wetland dependence

Table A2: Heterogeneity by wetland benefits (extensive margin): perception items (full)

	Trust	Conserve	Env.ben.	Econ.ben.	Vacate	Alt.prog.	Govt.eff.
Environmental	0.023	0.146	-0.067	-0.181	-0.209	0.045	-0.638***
	(0.145)	(0.128)	(0.143)	(0.153)	(0.137)	(0.128)	(0.177)
Long-term economic	-0.137	-0.216	-0.147	-0.344*	-0.212	-0.153	0.031
	(0.165)	(0.158)	(0.167)	(0.193)	(0.165)	(0.149)	(0.181)
Immediate payment (PES)	0.097	-0.111	-0.034	-0.018	0.120	-0.071	0.299*
	(0.168)	(0.145)	(0.156)	(0.161)	(0.153)	(0.144)	(0.165)
Benefits from wetlands	0.267**	0.267***	0.287**	0.191	0.124	0.109	0.312**
	(0.124)	(0.102)	(0.121)	(0.128)	(0.114)	(0.107)	(0.126)
Environmental × Wet-benefit	-0.042	-0.206	0.069	0.256	0.261*	0.011	0.677***
	(0.162)	(0.140)	(0.155)	(0.168)	(0.151)	(0.139)	(0.189)
Long-econ × Wet-benefit	0.072	0.182	0.033	0.369*	0.180	0.166	-0.061
	(0.183)	(0.167)	(0.181)	(0.207)	(0.178)	(0.161)	(0.196)
PES × Wet-benefit	-0.227	-0.044	-0.043	0.042	-0.175	0.091	-0.241
	(0.184)	(0.157)	(0.169)	(0.178)	(0.167)	(0.156)	(0.180)
Observations	1,636	1,636	1,636	1,636	1,636	1,636	1,636
R-squared	0.033	0.044	0.055	0.058	0.050	0.031	0.108

Notes. Each column is a separate OLS regression of the column-header perception outcome on the three treatment dummies, an indicator for reporting direct benefits from a nearby wetland (measured at endline before the experimental script), and the three treatment-by-benefits interactions, with the remaining pre-treatment controls described in Section 4. Heteroskedasticity-robust standard errors in parentheses. Stars: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Heterogeneity by wetland benefits (extensive margin): preferred government action (full)

	Fin.comp.	Train.	Enforce.	Infra.	Aware.
Environmental	-0.001	-0.035	0.063	0.021	-0.047
	(0.079)	(0.049)	(0.053)	(0.035)	(0.051)
Long-term economic	-0.164*	0.053	-0.064	0.101*	0.074
	(0.089)	(0.065)	(0.048)	(0.053)	(0.069)
Immediate payment (PES)	-0.164*	0.169**	-0.016	0.036	-0.025
	(0.084)	(0.069)	(0.053)	(0.039)	(0.057)
Benefits from wetlands	-0.017	0.034	0.007	0.000	-0.024
	(0.067)	(0.045)	(0.042)	(0.027)	(0.046)
Environmental × Wet-benefit	-0.071	0.063	-0.062	0.001	0.070
	(0.087)	(0.057)	(0.059)	(0.039)	(0.057)
Long-econ × Wet-benefit	0.078	-0.042	0.113**	-0.047	-0.102
	(0.098)	(0.072)	(0.056)	(0.057)	(0.074)
PES × Wet-benefit	0.079	-0.163**	0.071	0.037	-0.024
	(0.094)	(0.075)	(0.061)	(0.045)	(0.063)
Observations	1,636	1,636	1,636	1,636	1,636
R-squared	0.021	0.011	0.012	0.013	0.020

Notes. Each column is a separate OLS regression of the binary indicator for the column-header preferred-action outcome on the three treatment dummies, , an indicator for reporting direct benefits from a nearby wetland (measured at endline before the experimental script), and the three treatment-by-benefits interactions, with the remaining pre-treatment controls described in Section 4. The five preferred-action options together partition the choice set. Heteroskedasticity-robust standard errors in parentheses. Stars: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Heterogeneity by wetland-use intensity within beneficiary households: perception items (full)

	Trust	Conserve	Env.ben.	Econ.ben.	Vacate	Alt.prog.	Govt.eff.
Environmental	-0.229***	-0.160**	-0.026	0.084	0.029	-0.010	0.009
	(0.089)	(0.070)	(0.083)	(0.100)	(0.082)	(0.073)	(0.091)
Long-term economic	-0.180*	-0.151**	-0.124	-0.030	-0.100	-0.038	-0.090
	(0.103)	(0.076)	(0.103)	(0.115)	(0.092)	(0.081)	(0.105)
Immediate payment (PES)	-0.382***	-0.265***	-0.144	-0.079	-0.244**	-0.109	-0.113
	(0.105)	(0.090)	(0.102)	(0.118)	(0.099)	(0.089)	(0.110)
Wetland-dependent (above-median activities)	-0.312***	-0.260***	-0.038	-0.019	-0.248**	-0.259***	-0.221**
	(0.110)	(0.083)	(0.099)	(0.115)	(0.097)	(0.085)	(0.105)
Environmental × Wet-dep	0.471***	0.221**	0.066	-0.035	0.026	0.129	0.044
	(0.139)	(0.109)	(0.119)	(0.139)	(0.125)	(0.107)	(0.131)
Long-econ × Wet-dep	0.259*	0.262**	0.027	0.104	0.154	0.122	0.135
	(0.154)	(0.110)	(0.138)	(0.150)	(0.133)	(0.119)	(0.147)
PES × Wet-dep	0.504***	0.244**	0.128	0.182	0.371***	0.273**	0.340**
	(0.150)	(0.122)	(0.134)	(0.151)	(0.138)	(0.120)	(0.145)
Observations	1,342	1,342	1,342	1,342	1,342	1,342	1,342
R-squared	0.030	0.026	0.030	0.026	0.036	0.027	0.044

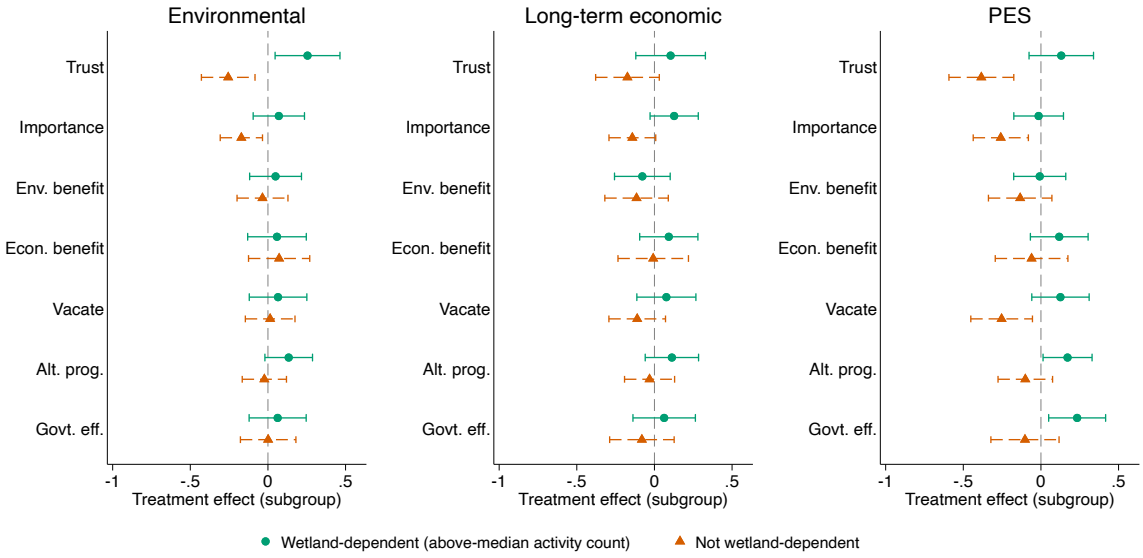
Notes. Each column is a separate OLS regression of the column-header perception outcome on the three treatment dummies, an indicator for an above-median count of distinct wetland activities, and the three treatment-by-intensity interactions, estimated within the 1,342 households that report benefiting from nearby wetlands. Both the activity count and the benefits indicator are measured at endline before the experimental script. Regressions include the pre-treatment controls described in Section 4 (missing values recoded to zero, with missing-value indicators). Heteroskedasticity-robust standard errors in parentheses. Stars: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Heterogeneity by wetland-use intensity within beneficiary households: preferred government action (full)

	Fin.comp.	Train.	Enforce.	Infra.	Aware.
Environmental	-0.139***	0.033	0.036	0.015	0.056*
	(0.048)	(0.037)	(0.026)	(0.021)	(0.029)
Long-term economic	-0.236***	0.060	0.075**	0.092***	0.009
	(0.054)	(0.043)	(0.033)	(0.032)	(0.030)
Immediate payment (PES)	-0.222***	0.105**	0.040	0.075**	0.002
	(0.058)	(0.048)	(0.033)	(0.033)	(0.032)
Wetland-dependent (above-median activities)	-0.262***	0.033	0.144***	-0.002	0.088**
	(0.056)	(0.043)	(0.038)	(0.024)	(0.040)
Environmental × Wet-dep	0.137*	-0.008	-0.071	0.016	-0.074
	(0.073)	(0.057)	(0.050)	(0.033)	(0.051)
Long-econ × Wet-dep	0.316***	-0.098	-0.062	-0.075*	-0.081
	(0.079)	(0.061)	(0.057)	(0.040)	(0.051)
PES × Wet-dep	0.289***	-0.177***	-0.001	-0.005	-0.106**
	(0.081)	(0.063)	(0.057)	(0.044)	(0.051)
Observations	1,342	1,342	1,342	1,342	1,342
R-squared	0.038	0.013	0.032	0.018	0.026

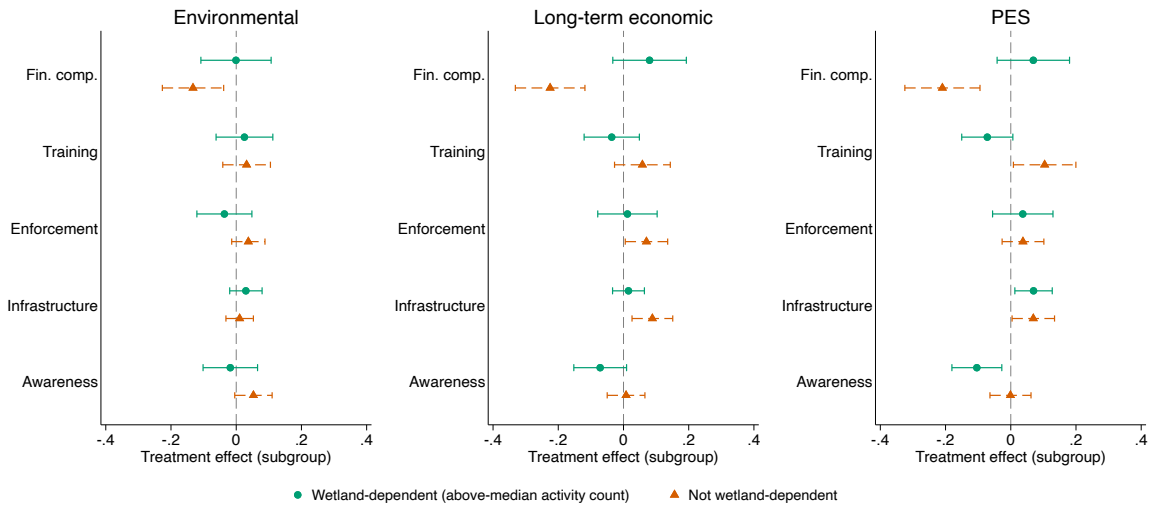
Notes. Each column is a separate OLS regression of the binary indicator for the column-header preferred-action outcome on the three treatment dummies, an indicator for an above-median count of distinct wetland activities, and the three treatment-by-intensity interactions, estimated within the 1,342 households that report benefiting from nearby wetlands. The five preferred-action options together partition the choice set. Regressions include the pre-treatment controls described in Section 4 (missing values recoded to zero, with missing-value indicators). Heteroskedasticity-robust standard errors in parentheses. Stars: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A1: Subgroup treatment effects on perception items, by wetland-use intensity (beneficiary households)



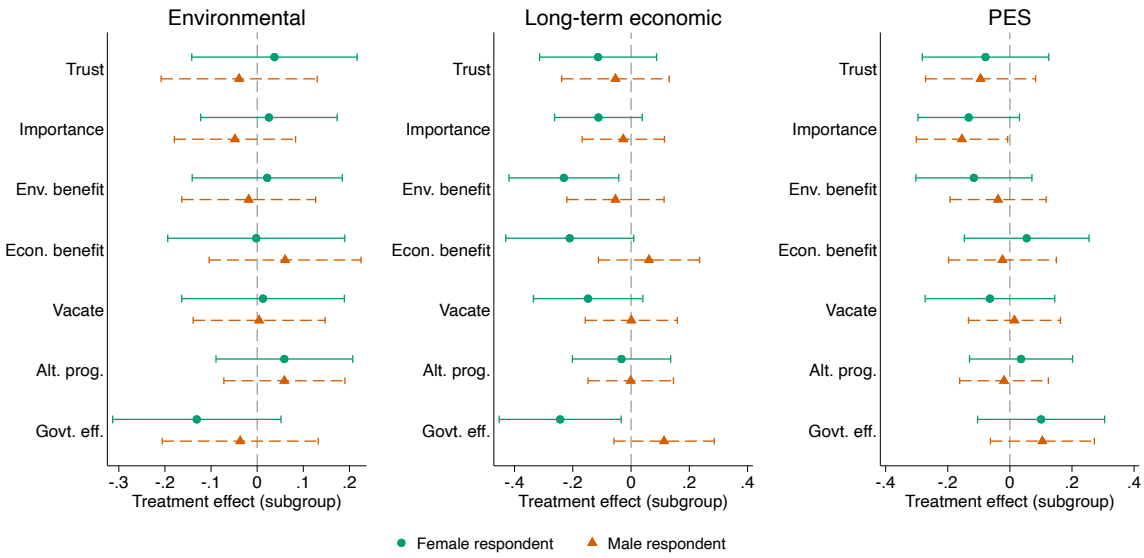
Notes. Each row reports the estimated treatment effect (relative to the neutral control) on one perception item, separately for beneficiary households with an above-median count of distinct wetland activities (solid, circle) and those at or below the median (dashed, triangle). The sample is the 1,342 households reporting wetland benefits. The three columns correspond, left to right, to the Environmental, Long-term economic, and Immediate payment (PES) arms. Horizontal bars are 95% confidence intervals from heteroskedasticity-robust standard errors. The corresponding interaction tests are reported in Table A4.

Figure A2: Subgroup treatment effects on preferred government action, by wetland-use intensity (beneficiary households)



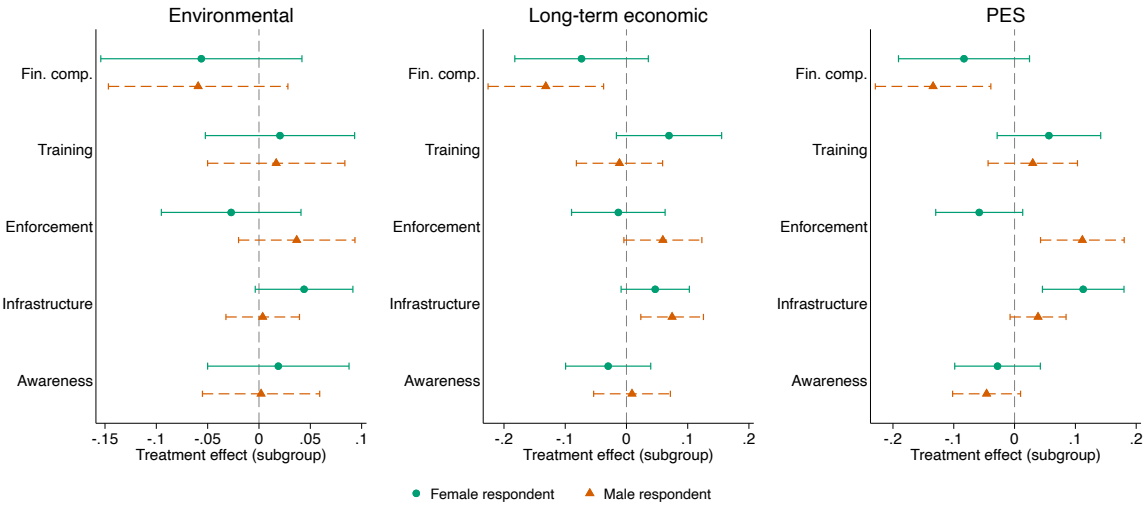
Notes. Each row reports the estimated treatment effect on the binary indicator that the respondent named the row's action as their single most-preferred government action, separately for beneficiary households with an above-median count of distinct wetland activities (solid, circle) and those at or below the median (dashed, triangle). The sample is the 1,342 households reporting wetland benefits. The three columns correspond, left to right, to the Environmental, Long-term economic, and Immediate payment (PES) arms. Horizontal bars are 95% confidence intervals from heteroskedasticity-robust standard errors. The corresponding interaction tests are reported in Table A5.

Figure A3: Subgroup treatment effects on perception items, by respondent gender



Notes. Each row reports the estimated treatment effect (relative to the neutral control) on one perception item, separately for female and male respondents. Within each row, the upper interval is the female subgroup; the lower interval is the male subgroup. The three columns correspond, left to right, to the Environmental, Long-term economic, and Immediate payment (PES) arms. Horizontal bars are 95% confidence intervals from heteroskedasticity-robust standard errors.

Figure A4: Subgroup treatment effects on preferred government action, by respondent gender



Notes. Each row reports the estimated treatment effect on the binary indicator that the respondent named the row's action as their single most-preferred government action, separately for female and male respondents. Within each row, the upper interval is the female subgroup; the lower interval is the male subgroup. The three columns correspond, left to right, to the Environmental, Long-term economic, and Immediate payment (PES) arms. Horizontal bars are 95% confidence intervals from heteroskedasticity-robust standard errors.

A.3 Multiple-hypothesis corrections

Table A6: Romano–Wolf stepdown-adjusted p-values for primary outcomes

Treatment	Outcome	Coef.	SE	Unadj. p	RW p
Environmental	Financial comp.	-0.060	0.033	0.073	0.081
	Training & inputs	0.017	0.025	0.506	0.505
	Enforcement	0.012	0.022	0.591	0.595
	Infrastructure	0.021	0.015	0.148	0.149
	Awareness	0.010	0.022	0.670	0.663
Long-term economic	Financial comp.	-0.099	0.036	0.007	0.008
	Training & inputs	0.018	0.028	0.513	0.499
	Enforcement	0.031	0.025	0.218	0.232
	Infrastructure	0.061	0.019	0.001	0.001
	Awareness	-0.012	0.024	0.615	0.599
Immediate payment (PES)	Financial comp.	-0.099	0.037	0.007	0.009
	Training & inputs	0.036	0.028	0.196	0.206
	Enforcement	0.041	0.026	0.107	0.114
	Infrastructure	0.066	0.019	0.001	0.002
	Awareness	-0.044	0.022	0.046	0.048

Notes. Family-wise error-rate-controlled p-values from the Romano and Wolf (2005) stepdown procedure, implemented with 1,000 bootstrap replications on the family of fifteen coefficients (three treatment arms × five preferred-action indicators). Unadjusted p-values reported for reference.

A.4 Program timeline and implementation

Figure A5: Timeline of program activities, baseline, and endline

Table A7: Implementation levels of Components 1 and 2 in the selected treatment and comparison sites in Eastern and Southwestern Uganda at baseline (2023)

Treatment sites			**Comparison sites**	
Wetland system	Components 1, 2 impl. level (%)	Region	Wetland system	Region
Namutumba – Mazuba mini-Mpologoma	100, 80	Eastern	Namutumba – Nabinyonyi – Kimenyulo	Eastern
Bushenyi – Nyaruzinga	95, 90	Southwestern	Bushenyi – Mbachi	Southwestern
Rubirizi – Kidubule – Ibamba – Nyakagyera – Ngoro	85, 50	Southwestern	Rubirizi – Chambura	Southwestern
Kisoro – Chotsa Bay – Mulehe Mutanda	80, 70	Southwestern	Kisoro – Mulindi – Echuya/Muchoya	Southwestern
Ngora – Agu	80, 40	Eastern	Ngora – Agwiki	Eastern
Bukedea – Lwere/Komorototo	70, 40	Eastern	Bukedea – Kapia/Okula	Eastern
Mitooma – Nyamuhizi – Kagogo	60, 80	Southwestern	Mitooma – Nchwera	Southwestern
Kumi – Oladoti/Kakores	60, 35	Eastern	Kumi – Obura	Eastern

Notes. Implementation level is reported separately for Component 1 (wetland restoration and catchment management) and Component 2 (climate-resilient livelihoods promotion). Treatment sites are ordered by Component 1 implementation level (descending). Comparison sites have no project-implementation activity by design.

A.5 Full text of outcome items

The nine outcome items administered immediately after the treatment script are reproduced below in the order they appeared in the questionnaire. The first item is a manipulation check; items two through eight are the seven five-point Likert perception items referenced in Section 2.3; item nine is the forced-choice preferred-government-action question. Likert items use the response scale *Very unclear / Somewhat unclear / Neither clear nor unclear / Somewhat clear / Very clear* (item 1) and *Strongly disagree / Disagree / Neither agree nor disagree / Agree / Strongly agree* or analogous five-point scales (items 2–8); we recode all Likert items so that higher values indicate a more conservation-favorable response.

1. **Comprehension.** "How clear was the information provided in the message for you to understand?"

2. **Trust.** "After hearing this message, how much do you trust the wetland conservation project?"

3. **Importance of conservation.** "How important do you believe it is to conserve wetlands in your community?"

4. **Perceived environmental benefits.** "How effective do you think wetland conservation projects are in the preservation of the environment?"

5. **Perceived economic benefits.** "How effective do you think wetland conservation projects are in supporting the local economy?"

6. **Willingness to vacate.** "How willing are you to vacate wetland areas if requested by the conservation program?"

7. **Willingness to participate in alternative livelihoods.** "How willing are you to participate in alternative livelihood programs provided by the conservation project?"

8. **Effectiveness of government policies.** "How effective do you think government policies will be in protecting wetlands and benefiting your community?"

9. **Preferred government action.** "Which type of government action do you believe would be most beneficial for wetland conservation in your community?" Response options: (1) Provide financial compensation; (2) Offer training and inputs; (3) Enforce environmental laws; (4) Invest in infrastructure; (5) Conduct awareness campaigns.

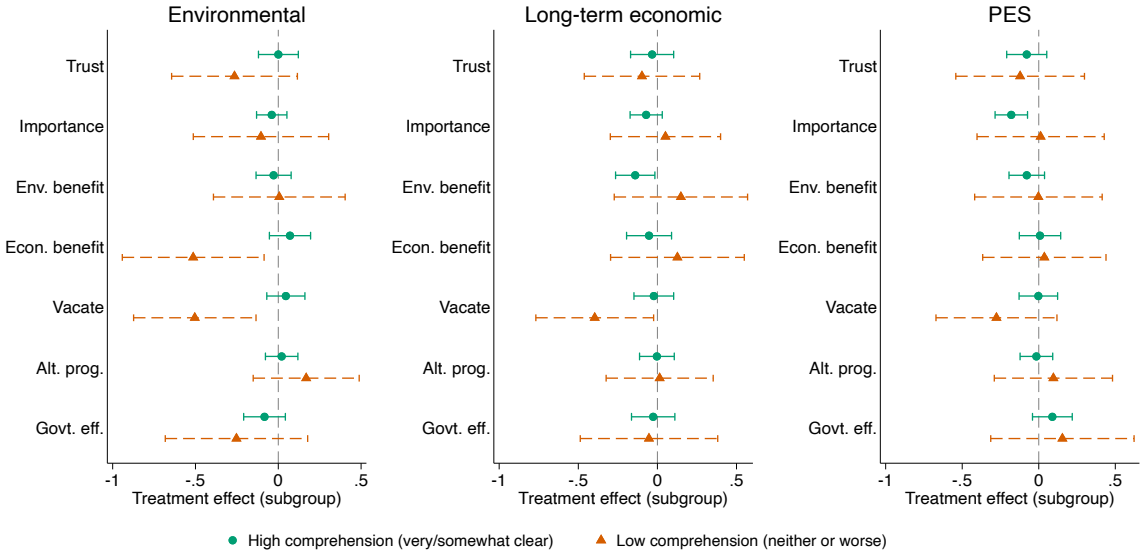
A.6 Comprehension as manipulation check

Table A8: Comprehension across treatment arms (manipulation check)

	Environmental	Long-term economic	PES
	b/se	b/se	b/se
Treatment vs. neutral	-0.132**	-0.152**	-0.153**
	(0.054)	(0.061)	(0.062)
Neutral-arm mean	4.43	4.43	4.43
Neutral-arm SD	0.82	0.82	0.82
Observations	914	750	744

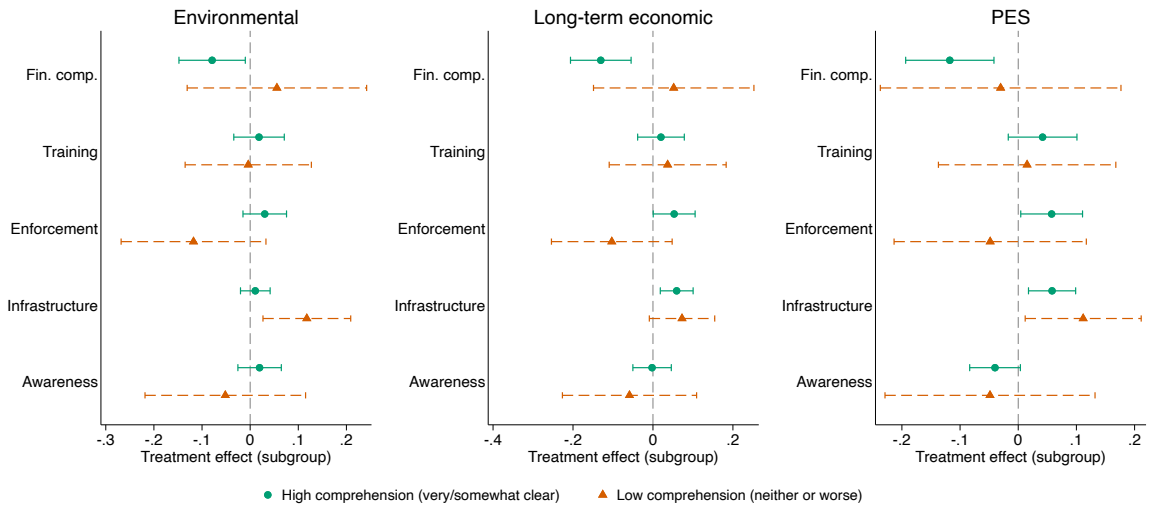
Notes. Each column reports the coefficient on the treatment-arm indicator from a regression of the comprehension item (5-point Likert, higher = clearer) on that indicator, restricted to respondents in the neutral arm and the indicated treatment arm. The neutral-arm mean of comprehension is reported in the first row. Heteroskedasticity-robust standard errors in parentheses. Stars: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A6: Subgroup treatment effects on perception items, by post-treatment comprehension level



Notes. Each row reports the estimated treatment effect (relative to the neutral control) on one perception item, separately for respondents who rated the message as "very clear" or "somewhat clear" (high comprehension) versus "neither," "somewhat unclear," or "very unclear" (low comprehension). Within each row, the upper interval is the high-comprehension subgroup; the lower interval is the low-comprehension subgroup. The three columns correspond, left to right, to the Environmental, Long-term economic, and Immediate payment (PES) arms. Horizontal bars are 95% confidence intervals from heteroskedasticity-robust standard errors. The conditioning variable is post-treatment, so this analysis is descriptive rather than causally identified within subgroup.

Figure A7: Subgroup treatment effects on preferred government action, by post-treatment comprehension level



Notes. Each row reports the estimated treatment effect on the binary indicator that the respondent named the row's action as their single most-preferred government action, separately for high- and low-comprehension subgroups (defined as in Figure A6). The conditioning variable is post-treatment, so this analysis is descriptive rather than causally identified within subgroup. Horizontal bars are 95% confidence intervals from heteroskedasticity-robust standard errors.