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Analysis

Temporary PES do not crowd out and may crowd in lab-in-the-field forest conservation in Colombia

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A B S T R A C T

Payments for ecosystem services (PES) programs exist globally and at times shift behaviors. Unlike protected areas, PES compensate land users, raising local acceptance of conservation. Yet some worry that if payments are temporary, as is often the case, conservation behaviors can be reduced by PES, 'crowded out' to be lower after PES than if no PES had existed. We conducted lab-in-the-field experiments in Colombia, where PES policies are expanding, offering either individual or collective conditional payments to 676 farmers who are potential PES participants. Those payments end, within each experimental session, for all or only for some participants. We consistently find that conservation is not lower after PES than before. Also, conservation contributions tend to fall over time without PES, in keeping with public-goods literatures. Taken together, these results imply that even after our payments end, conservation is above the baseline defined by our controls, suggesting some form of (at least short-run) crowding in.

1. Introduction

Payments for ecosystem services (PES) programs exist globally. Salzman et al. (2018), e.g., report 550 active programs with US$36–42 billion in annual transactions. From a global societal perspective, there is great interest in Reductions in Emissions from Deforestation and forest Degradation (REDD+). From a local perspective, it is positive that PES programs – unlike protected areas (PAs) – compensate those who supply global ecoservices. This increases acceptance and participation among rural dwellers.

PES do not always have impacts. Payments can be too low to shift incentives, e.g., be below the costs of shifting land uses. Programs also may fail to target threats, enrolling parcels that would feature the same land uses without PES. Further, monitoring and sanctioning are costly, financially and politically, for rural forest frontiers. This can undermine de facto conditionality of payments and thus their impacts. We add a challenge even for impactful payments: if they are temporary, introducing but then removing external financial incentives might drive post-PES conservation below where it would be without PES.

There is a past empirical basis for such concern. 'Crowding' of motivations and behavior does happen. A literature in psychology (Deci et al., 1999, Ryan and Deci, 1985, 2000) and behavioral economics (Frey, 1994; Kahneman et al., 1986; Bowles and Polania-Reyes, 2012; Gneezy and Rustichini, 2000), including with consideration of public policies (Le Grand, 2003; Titmuss, 1970; Moller et al., 2013), has shown that new extrinsic incentives could displace intrinsic motivations (*motivational crowding*) and, thereby, reduce pro-social or environmental behaviors (*behavioral crowding*). Bowles (2008) and Bowles and Polania-Reyes (2012) note a lack of separability of incentives from "moral sentiments" when both are present. Incentives and sentiments might well interact, so that their combined impact is not simply the sum of their separate effects. For instance, early experimental evidence in various domains found that in some cases the introduction of external incentives undermined ethical motives, though in only a few cases were such external incentives seen to reinforce and, thus, enhance ethical motives (see Bowles, 2008; Rode et al., 2015 and the review in Bowles and Polania-Reyes, 2012).

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We study what happens when temporary payments end – as most PES programs eventually do – by conducting lab-in-the-field experiments with conditional payments for 676 farmers in rural Colombia. PES programs often end. Payments are costly for those who pay, thus ecoservices purchasers prefer to pay only temporarily. Payers might hope 'green' practices will become the preferred land uses, so payments no longer are needed (e.g., silvopastoral practices might raise profits, as in Pagiola et al., 2016). In stark contrast to those hopes, however, some practitioners and scholars suggest that, like other external incentives, PES might lead such ecoservices suppliers' preferences to actually shift against the interests of ecoservices buyers. If sellers already are carrying out some 'green' practices, then it is possible those will fall instead of rise if the PES 'crowds out' or somehow diminishes the sellers' prior motivations for those very practices (e.g., Deci et al., 1999).

For conservation incentives, a recent review of 74 PES schemes offers some nuanced perspectives on such interactions, concluding that extrinsic incentives as in PES could also 'crowd in' such motivation. Generally, as in Engel (2016)'s summary statement that "the devil is in the details", multiple elements of PES – discursive, institutional, design, and implementation – affect impacts of PES on motivations and behaviors. PES programs could 'crowd in' pro-social or environmental behavior, after payments, if the programs: facilitate interpersonal communication and reinforces pre-existing trust (Andersson et al., 2018); provide non-monetary and collective benefits (Agrawal et al., 2015; Kaczan et al., 2017; Moros et al., 2019); bolster autonomy, social relatedness, or plural values (Grillos et al., 2019; Maca-Millán et al., 2021; Lliso et al., 2021); promote autonomous decision making (Akers and Yasué, 2019); and build assets for restoring ecosystems (Calle, 2020; Pagiola et al., 2016, 2020; Table 1 offers summaries of papers).

As to why individual payments or entire programs stop, the reasons vary (Rode, 2022). For example, a new key stakeholder shifted eligibility in "Yo protejo agua para todos" in Cundinamarca, Colombia, leading to the removal of over 130 participants. Armed conflict also affects Colombian implementation, by interrupting payments (Moros, 2019). In Ecuador, the SocioBosque PES was paused for two years, due to financial limitations (Etchart et al., 2020; Hayes et al., 2022). A Ugandan program was planned to end after two years (Jayachandran et al., 2017). In Mexico, hundreds of early participants were not renewed due to drops in the budgets for PES and to changes in PES criteria (Izquierdo-Tort, 2020).

We ask whether, when payments stop, the consequences depend upon their design and/or whether the payments stop for all or only for some PES participants. Following Alpízar, Nordén, Pfaff, and Robalino (2017), we test different rules for removal of PES. We interact different designs for payments (individual versus collective) with different rules for removal (partial versus total), focusing on details of PES design (Engel, 2016). Literatures' myriad of theories and results are consistent with some kinds of crowding, albeit in either direction, or with no crowding at all. We test against a null hypothesis of no impact.

We implemented a decision experiment in the field in rural Colombia. Colombia recently passed a PES National Regulation (Ministerio de Ambiente, 2017 Law 870) and may expand PES nationwide with at least 1,000,000 ha in new PES by 2030 (DNP, 2017). Even at such an ambitious scale, such initiatives exclude some farmers. Also, at this scale, eventually some payments end given budget restrictions. Our research informs ongoing policy design by exploring the behavioral consequences due to the introduction of new payments, followed by their later removal for some or all participants.

Our study design reflects, for example, challenges to having impact in "miPáramo!", a conservation effort in Norte de Santander in Colombia that was launched in 2018 to support forests in *páramos* of Santurbán. The *páramo* are a key ecosystem for water provision and regulation in northeast Colombia, though also a disputed area, due at least in part to the region's potential for gold mining. This conservation program is considering monetary incentives but they fear negative influences on motivations and behaviors.

Our lab-in-the-field experiments, in 2019 (Study 1) and replicated in 2021 (Study 2), have two distinct findings which are consistent across Study 1 and Study 2 – yet are more significant in the combined data given more power to test with more observations. First, for sessions with PES, across PES treatments we find consistently that post-PES contributions are at least as high as pre-PES contributions. Second, within our control groups without PES, contributions fall over time (unsurprising per public-goods literatures). Together, these results imply that conservation after PES is above our controls' baseline.

One way to describe this is as 'short-run crowding in from the difference-in-differences perspective'. Even if our controls had constant contributions over time, which could occur in some stable societies, post-PES contributions as high as pre-PES rejects 'crowding out'. Yet for our falling baseline level of public-goods contributions, we see post-PES above the control baseline. While we must caveat this as a short-run result, as our field studies clearly did not cover the longer run, even for only short-run gains from simply delaying business-as-usual (BAU) degradation – a policy goal in REDD+ programs, concerning greenhouse emissions – we are glad to highlight gains over BAU, even after PES removal.

The rest of the paper is as follows. Section 2 describes the field setting where we did our lab-in-the-field experiments. Section 3 describes our methods, with a number of institutional variations on the classic public-good games we implemented, then Section 4 presents all of our core results. Finally, Section 5 offers some discussion, including limitations and consideration of the policy implications.

2. Setting

Research was conducted in the municipalities of Pamplona, Cácota, Cucutilla, Charta, Pamplonita and Mutiscua, in the department of Norte de Santander (Fig. 1), northeast Colombia, in 2019 and 2021. All of these municipalities are part of the buffer area that delimits the *páramo*, i.e., the Andean highland wetland of Santurbán, which is an important ecosystem that features several agricultural production systems, as well as some small-scale mining, plus some prior conservation practices. It supplies water to multiple cities (Duarte-Abadía and Boelens, 2016). Generally, this region has witnessed multiple environmental conflicts due to the establishment of large-scale mining operations and agricultural practices within ecologically sensitive areas such as the *páramo* (Duarte-Abadía and Boelens, 2016).

As part of the efforts to protect this ecosystem, miPáramo! is a public-private initiative that deploys resources to stimulate additional conservation of *páramo* forests. It aims to complement sustainable productive practices with conservation strategies, as well as restoration strategies, by implementing various types of in-kind transfers with some farmers located within the buffer area of the moorland.

The program was launched with the support of the *Alianza Bio-Cuenca*, which links public and private institutions for conservation. Its process has three 'pillars': socialization; a characterization of lands; and, finally, signing of conservation agreements. The support provided is in-kind and can vary with the farmers' needs, including for instance: reservoirs to collect water for productive activities; technical assistance; native seeds; and reforestation supplies. With 1072 active participants in 9 municipalities of Norte de Santander and Santander, the program operates in the municipalities used for our studies.

Thus, our participants are active or potential participants in the miPáramo! program. We note that the selective removal of participants has not happened. Rather, to date, plus looking ahead, the program has been planning expansions into new regions. As mentioned earlier, this program also has plans to extend its efforts to include monetary incentives that support forest conservation – while fearing payments could reduce pre-existing pro-environmental motivations and thus also behaviors. Our PES results can, then, inform this and similar initiatives. Some field assistants from miPáramo! supported the recruitment of our participants, as well as the organization of our experimental sessions.

Table 1
Summary of the relevant literature concerning motivational and behavioral crowding.

Authors	Country	Type of Crowding	Method	Sample size	PES removal?	Crowding-Out?	Crowding-In?
Le Velly et al. (2017)	Mexico	Behavioral	Satellite images before and after the incentive and community surveys	10,352 polygons	Yes	Crowding out of conservation behaviors after incentive removal	No evidence of crowding-in after incentive removal
Calle (2020)	Colombia	Behavioral	Satellite images before and after the incentive	20 farms	Yes	No Crowding-out after incentive removal	When comparing silvopastoral farms relative to the surrounding landscape
Grillos et al. (2019)	Bolivia	Motivational	Randomized Control Trial	1443 PES participants 1158 non participants	Yes	No Crowding-out after incentive removal	Crowding-in of pro-environmental values
Etchart et al. (2020)	Ecuador	Behavioral	Quasi-experimental: Matching with not-enrolled combined with fixed effects panel regression analysis	63 PES participants	Yes	Enrolled properties did not maintain conservation outcomes during payment interruptions in areas of high deforestation risk	Enrolled properties conserved more during payment interruptions in areas of low deforestation risk
Kaczan et al. (2019)	Tanzania	Behavioral	Lab-in-the-field experiment: Dictator game	250 forest users	Yes	No Crowding-out after incentive removal	Potential crowding-in under mandated levels of contributions
Salk et al. (2017)	Lao PDR	Behavioral	Lab-in-the-field experiment: Common pool resources game	96 shifting cultivators	Yes	No Crowding-out after incentive removal	No evidence of crowding-in after incentive removal
Andersson et al. (2018)	Bolivia, Indonesia, Peru, Tanzania and Uganda	Behavioral	Lab-in-the-field experiment: Common pool resources game	1200 forest users	Yes	No Crowding-out after incentive removal	Users conserved more after incentive removal specially when they were able to communicate
Maca-Millán et al. (2021)	Colombia	Behavioral	Lab-in-the-field experiment: public goods game with threshold	120 Potential PES participants	Yes	No Crowding-out after incentive removal	Potential crowding-in if PES programs integrate plural motivations and values
Lliso et al. (2021)	Colombia	Behavioral	Lab-in-the-field experiment: public goods game with threshold	157 potential PES participants	Yes	No Crowding-out after incentive removal	Crowding-in in indigenous communities when the incentive highlighted relational values of the forest. Crowding-in in campesino communities when the incentive highlighted instrumental values of the forest.
Pagiola et al. (2020)	Nicaragua	Behavioral	Household survey to PES participants before and after; detailed land-use maps	52 PES participants 20 non participants	Yes	No Crowding-out after incentive removal	Crowding-in of silvopastoral practices after the incentive under an asset building PES program
Pagiola et al. (2016)	Colombia	Behavioral	Household survey to PES participants and control groups before and after	69 PES participants 29 non participants	Yes	No Crowding-out after incentive removal	Crowding-in of silvopastoral practices after the incentive under an asset building PES program
Jayachandran et al. (2017)	Uganda	Behavioral	Randomized Control Trial and household surveys	1099 farmers (564 treated, 535 control)	No	No evidence after incentive introduction	No evidence of crowding-in but slower rate of deforestation among former PES participants
Chervier et al. (2019)	Cambodia	Motivational	Quasi-experimental Matching with non-participants	325 farmers (205 participants 120 non participants)	No	Participants reported more money related reasons to protect forests and are more likely to rule breaking after a virtual stop of payments	No evidence of crowding-in after incentive removal
Agrawal et al. (2015)	India	Motivational	Quasi-experimental Before and after Matching with non-participants	2224 farmers	No	When participants received private economic benefits	When participants received communal assets or collective benefits
Narloch et al. (2012)	Perú and Bolivia	Behavioral	Lab-in-the-field experiment: public goods game with threshold	240 farmers	No	Collective payments crowd out social norms	Individual payments crowd-in social norms
Moros et al. (2019)	Colombia	Behavioral and motivational	Lab-in-the-field experiment: public goods game with threshold	257 potential PES participants	No	Crowding out of intrinsic motivations when premium price is introduced	Crowding-in of social motivations when collective payment is introduced.

(continued on next page)

Table 1 (*continued*)

Authors	Country	Type of Crowding	Method	Sample size	PES removal?	Crowding-Out?	Crowding-In?
							Crowding-in of conservation behaviors with individual and collective payments
Handberg and Angelsen (2019)	Tanzania	Behavioral	Lab-in-the-field experiment: Common Pool Resources game	480 forest users	No	No Crowding-out when an incentive (small, medium or large) is introduced	No crowding-in when an incentive (small, medium or large) is introduced
Vollan (2008)	South Africa and Namibia	Behavioral	Lab-in-the-field experiment: Common Pool Resources game	210 farmers	No	Crowding out of cooperative behavior when a restrictive penalty is introduced under a condition of low self-determination and high trust in society	No evidence of crowding-in after incentive removal
Midler et al. (2015)	Perú	Behavioral	Lab-in-the-field experiment: public goods game with threshold	176 farmers	No	Crowding out of cooperation when a collective reward is introduced	Crowding-in of cooperation when an individual reward is introduced

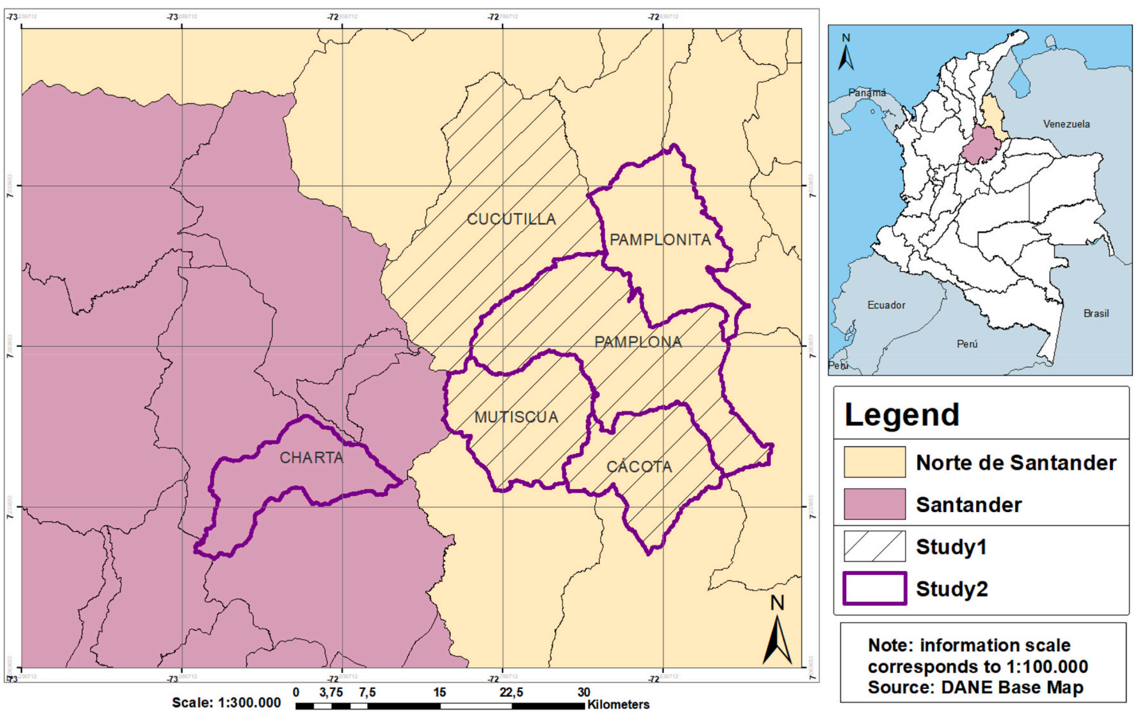

Fig. 1. Municipalities with miPáramo! in Norte de Santander and Santander- Colombia.

3. Methods

To examine the impacts due to PES introduction-and-then-removal we used a framed field experiment based upon the standard threshold-public-goods game (Moros et al., 2019). Our experiment captures the tension between conservation and agricultural expansion (Kaczan et al., 2019; Midler et al., 2015; Narloch et al., 2012; Lliso et al., 2021, Maca-Millán et al. 2020). We piloted the game in October 2019 with 40 students at Universidad de Los Andes as well as 40 rural farmers in Norte de Santander to adjust protocols and to train research assistants. We do not use pilot data in analyses.

We did two rounds of experiments, referred to here as Study 1 and Study 2. We added Study 2 to test replicability, allow a different threshold for the collective payment, and increase our sample. For Study 1, data was collected during October and November 2019. For Study 2, data was collected in October 2021, returning to some of the same municipalities for new participants (16 participants participated a second time in 2021, yet we controlled for this). For both studies, via phone calls by a local contact in each municipality we invited farmers from the miPáramo! Initiative, as well as other potential program beneficiaries, to participate in our experiment and survey. The number of sessions in each municipality depended upon the number of potential participants. We asked each participant to answer a post-experimental survey,[2] including sociodemographic questions about them as farmers, farm productivity, environmental motivations to preserve forests, and emotions during the experiment (see full survey protocol upon request). Here, we focus on the behavioral and sociodemographic data.

[2] Study 1 implemented the motivations survey for some of the experimental participants a full three months before the experimental session occurred, while Study 2 implemented the motivations survey before and after these games (able to test only for effects of participating in a game). Field work logistics prevented us from having a standardized protocol in our full sample, per any changes in motivations. For this reason, we do not report motivational results. In short, though, we do not find clear differences in motivations, in either direction, in comparing treated with non-treated participants.

3.1. Sessions

We conducted 35 experimental sessions, 15 in Study 1 and 20 in Study 2. All were in Spanish, with the support of six research assistants, and occurred in the municipality's local library or community hall. Sessions lasted about 3 hours. They had a minimum of 8 participants, with a maximum of 24.

At the beginning of each session, an informed-consent document was provided to all and read out loud to first explain the activity and then ask each farmer if he or she would like to participate. For those who opted to participate, a research assistant read the instructions out loud (the same person in all sessions in each study). One practice round was done before forming experimental groups at random. In Study 1, we asked each participant about the number of water sources on his/her lands. Those data were used to implement the removal rule for some treatments – as is explained below.

3.2. Forest-conservation game

Each participant was randomly assigned to a group of four. In each round (t), each group member (i) allocated 4 units of land to conserving the forest (f) or to crops (c). For every unit of land assigned to forest(X_f), a participant received \$200 pesos. For every unit of land assigned to crops (X_C), though, a participant received \$600 pesos. For every forest unit due to others in one's group, each participant received \$200 pesos. Each person gained from every forest unit in her group, her own and others'.

The private gain from one's own forest, though, is much lower than one's gain for crops, as is currently the situation in Norte de Santander region, as well as in other regions with high risk of deforestation.

This implies payoffs [1a] below, where $\sum kX_{fk}$ are the units of forest due to other members (k) of one's group, while *m* is non-monetary benefits from conservation, including social preferences and environmental motivations. Manipulating this expression – i.e., moving from [1a] to [1d] – emphasizes one's own net benefits from choosing forest and [1d] shows each unit allocated to forest forfeits \$400.

$$\pi_{it} = \$600x_{cit} + \$200x_{fit} + \left(\$200\sum_{k=1}^{3} kX_{fk}\right) + \mathrm{m}\ (X_{fit}) \tag{1a}$$

$$\pi_{it} = \$600(4 - x_{fit}) + \$200x_{fit} + \left(\$200\sum_{k=1}^{3} kX_{fk}\right) + \mathrm{m}\ (X_{fit}) \tag{1b}$$

$$\pi_{it} = \$2.400 - \$600x_{fit} + \$200x_{fit} + \left(\$200\sum_{k=1}^{3} kX_{fk}\right) + \mathrm{m}\ (X_{fit}) \tag{1c}$$

$$\pi_{it} = \$2.400 - \$400x_{fit} + \left(\$200\sum_{k=1}^{3} kX_{fk}\right) + \mathrm{m}\ (X_{fit}) \tag{1d}$$

The participants were randomly assigned either to a control group (CG) or to treatment groups (TG). In CG, a participant played the game for 12 rounds, deciding in each round how many units of land to put into forest, given the payoffs. For example, if a participant allocated 3 units of land to crops, and thus 1 unit to forests, while other members of her community conserved 7 total units of forest, then her earnings would be $\$600(3) + \$200(1+7) = \$\ 3{,}400$. Rewriting the earnings to focus on the disincentive to choose forest (even if forest also helps others): $\$2400 - \$400(1) + \$200(7) = \$\ 3{,}400$. In principle, the *m* term in [1a - 1d] could push one towards allocating some units of land to forest.

3.3. Treatments: Payments & removals

Treatments (TG) involve three stages, each with four rounds (Table 2): (1) a baseline, without PES; (2) introduction of PES, for all; then (3) a partial or a total removal of payments. In the second stage, we vary whether payment is based on individual forest or, instead, is a function of group conservation. Either way, in the third stage some PES are removed: either total removal (TR), i.e., all groups in a session are removed from PES; or partial removal (PR), i.e., the majority of groups in a session are removed.

Within Study 1, the partial-removal treatment had two variations. In 'random' (PRR), it was random. Else, in 'water' (PRW), the partial removal was based on the number of (self-reported) water sources in his/her land. Specifically. groups with lower average numbers of sources were removed, 'targeting' farmers who could have greater impacts upon water quality via shifts in their land use (Moros, 2019). The latter is motivated by suggestions that people understand their impacts on the public good might matter for who is prioritized in PES programs. More generally, we explored PRR versus PRW to check any effects that removal framings might have. Participants learned about removal rules before round 9. Table 2 below presents our treatments, with observations per treatment, for Study 1 and Study 2. Study 2 followed the same design as Study 1, with two differences: within the collective treatment (discussed more below); and for Study 2, the only partial removal rule that we used was the PRR rule.

The framing for the 2nd or PES stage was always that an environmental organization (EO) is interested in paying for forest conservation. The additional private payoff is a further \$450 benefit to each participant for each unit of her forest. The expressions below summarize the new individual earnings function with linear individually based payments. It is clear in [2d] that now land units in forest are beneficial:

$$\pi_{it} = \$2.400 - \$600x_{fit} + (\$200 + \$450)x_{fit} + \left(\$200\sum_{k=1}^{3} kX_{fk}\right) + \mathrm{m}\ (X_{fi}) \tag{2c}$$

$$\pi_{it} = \$2.400 + \$50x_{fit} + \left(\$200\sum_{k=1}^{3} kX_{fk}\right) + \mathrm{m}\ (X_{fi}) \tag{2d}$$

For the collective payment in Study 1, we imitated the linear individual payment using a lump sum. Thus, we offered a payment of \$1800 to each participant if her group managed to conserve 16 units of forest, as is achieved by 4 units each (\$1800 in [3d] equals 4 units in forest each at \$450, as in [2c]). Yet 16 forest is achieved only if each member of a group allocates all lands to forest, none to crops, and each member has to be confident in fellow group members to allocate all 4 units of land to forest.

For the collective payments in Study 2, again we imitate linear payments using a lump sum, yet in this case only up to the equivalent of three units of land each. Thus, we offer \$1350 to each participant if her group managed to conserve at least 12 forest units in total, as is achieved by 3 units of forest each (\$1350 in [4d] equals 3 units in forest at \$450 each). We lowered this collective threshold given the tradeoff between successful groups achieving more and more groups achieving a success. Over time, e.g., after PES, the latter may well help. Easier tasks might be less discouraging, thus more sustainable.

$$\pi_{it} = \$2.400 - \$400x_{fit} + \left(\$1.800 \text{ if } \sum X_f = 16\right) + \$200\sum_{k=1}^{3} kX_{fk}\Bigg) + \mathrm{m}\ (X_{fi}) \tag{3d}$$

$$\pi_{it} = \$2.400 - \$400x_{fit} + \left(\$1.350 \text{ if } \sum X_f \geq 12\right) + \$200\sum_{k=1}^{3} kX_{fk}\Bigg) + \mathrm{m}\ (X_{fi}) \tag{4d}$$

4. Results

Our participants are all farmers. They are either active current participants or potential participants in the miPáramo! initiative. On

Table 2
Summary of treatments, by stages and studies.

	Stage 1 (rounds 1–4)	Stage 2 (rounds 5–8)	Stage 3 (rounds 9–12)	Study 1	Study 2
NO PES	No PES	No PES	No PES	11 groups, 44 people	13 groups, 52 people
IND-TR	No PES	Individual PES for all	Total Removal for everyone	10 groups, 40 people	13 groups, 52 people
IND-PRR	No PES	Individual PES for all	Partial Removal random choice	10 groups, 40 people	22 groups, 88 people
IND-PRW	No PES	Individual PES for all	Partial Removal water sources	10 groups, 40 people	–
COL-PRW	No PES	Collective PES for all	Partial Removal water sources	15 groups, 60 people	–
COL-PRR	No PES	Collective PES for all	Partial Removal random choice	11 groups, 44 people	30 groups, 120 people
COL-TR	No PES	Collective PES for all	Total Removal for everyone	10 groups, 40 people	14 groups, 56 people
				77 groups, 308 people	*92 groups 368 people*

Study 1 collective threshold = 16.
Study 2 collective threshold = 12.

average, across both of our studies, participants were age 49, had monthly household incomes of 809,784 Colombian Pesos, and had stopped their education after completing grade 7 – while 55.2% of participants were male, 22.2% have cleared forest before, and 23.1% had received conservation payments previously. As expected, there are some differences across studies, and across the randomly assigned treatments (as seen in Annexes 3c and 3d for Study 1 and Study 2, respectively). Within our analyses, we control for those differences using our individual fixed effects.

As discussed above, to explore effects of *rationales for* partial removal (PR) – i.e., 'targeting', given that the majority of groups in the sessions are being removed – in Study 1 the partial removals were based either on random selection (PRR) or water sources (PRW). We find no robust differences across these rationales (Annex 3a). For removed individuals with collective payments, we see some difference, at 10% significance, yet this is not significant in a regression (Annex 3b). Else we see no differences. Thus, henceforth, for Study 1 we combine the data for these two rationales for our partial removals.

4.1. *Stage 1 (rounds 1–4): Pre-existing motivations support contributions to public goods*

Fig. 2a (combining our studies), Fig. 2b (for our Study 1), and Fig. 2c (for our Study 2) show the average units of land allocated to forest by round, i.e., our measure of conservation contributions. Within partial removal (PR), we separate those groups that were removed (R) from those not removed (NR). Payments, whether individual (IND) or collective (COL), were always introduced in Round 5. After that, the payments were then partially (PR) or totally (TR) removed in Round 9. Within Stage 1, the observation of any contributions to conservation – clearly non-trivial in Figs. 2 – suggests pre-existing motivation and expectations, including intrinsic motivations and social norms or pressures (recalling that individual contributions to forests in Stage 1 always lower the contributor's earnings). Since the Stage 1 contributions are significant, there is plenty of room for them to either rise or fall.

4.2. *Stage 2 (rounds 5–8): external incentives matter*

4.2.1. *Adding payments matters*

As expected, the introduction of PES payments in Stage 2 increases contributions within both studies (noting that Annexes 4a and 4b provide our tests demonstrating statistically significant differences). For Study 1, one might identify visually some downward trends in Stage 2's contributions with PES; however, that is less observable for Study 2, where the Stage 2 contributions appear to be more stable. For our controls, without any payments, within both studies we see a downward trend in contributions (see No PES controls), very much in keeping with the related literatures on public-goods experiments.

For both studies, payments continue to matter in Stage 3, for those not removed by partial removals. There may even be 're-start effects' for them (yet testing this requires a 'nobody removed' treatment in which all groups have PES rounds 5–12). Roughly, without considering any dynamics within stages, when payments exist we see they matter, independent of payment type (individual versus collective).

4.2.2. *When adding payments, changing designs doesn't matter much on average*

Comparing Collective Thresholds during Payments.

Without very large samples, randomly assigned treatment groups can differ randomly in outcomes (below we compare individual payments, while Figs. 2 show non-trivial differences in Rounds 1). In addition to this, however, for collective payments we have an explanation for the Stage 2 differences we noted, i.e., that for Study 1 contributions under PES fall in Stage 2 yet for Study 2 they do not fall.

The explanation is the different threshold. For Study 1, our collective threshold was 16, which needs complete contributions by each group member. Within a group, unless you are very confident all other members will also contribute fully – which, of course, involves them in turn being confident in you – it is very easy to conclude that contribution is no more beneficial with payments than in the baseline. Naturally, in many groups, individuals might in good faith contribute highly or even fully at the start, trying to help reach the best group outcome (i.e., the total contribution of 16, earning the payments). However, it must be expected that many groups will eventually miss that payment, and then decline.

That is not the situation for the threshold of 12, which can be achieved by just three members or even superseded by a rather enthusiastic group (who know that they can later efficiently adjust downward). There is a clear best group outcome at total contribution of 12 that should be able to be maintained. That said, even perfect such success, i.e., exactly 12 total contributions in every Stage 2 round, need not imply greater total contributions than when trying for 16 (even versus those groups that fail at 16, e.g., round contributions 15, 13, 11, 9 – always failing for 16, and falling – are equal to 12 + 12 + 12 + 12).

Figs. 2b and 2c support such thinking. In Fig. 2b, the collective groups start higher yet fall, while in Fig. 2c for the lower threshold, the solid green 'COL-PR-NR' line, e.g., starts at and stays at 3. Thus, as noted above, per tradeoffs in designs, while the lower threshold sets our sights lower for the short run, for the long run it might achieve more if that success can be maintained, avoiding collapse. Here, without truly long-

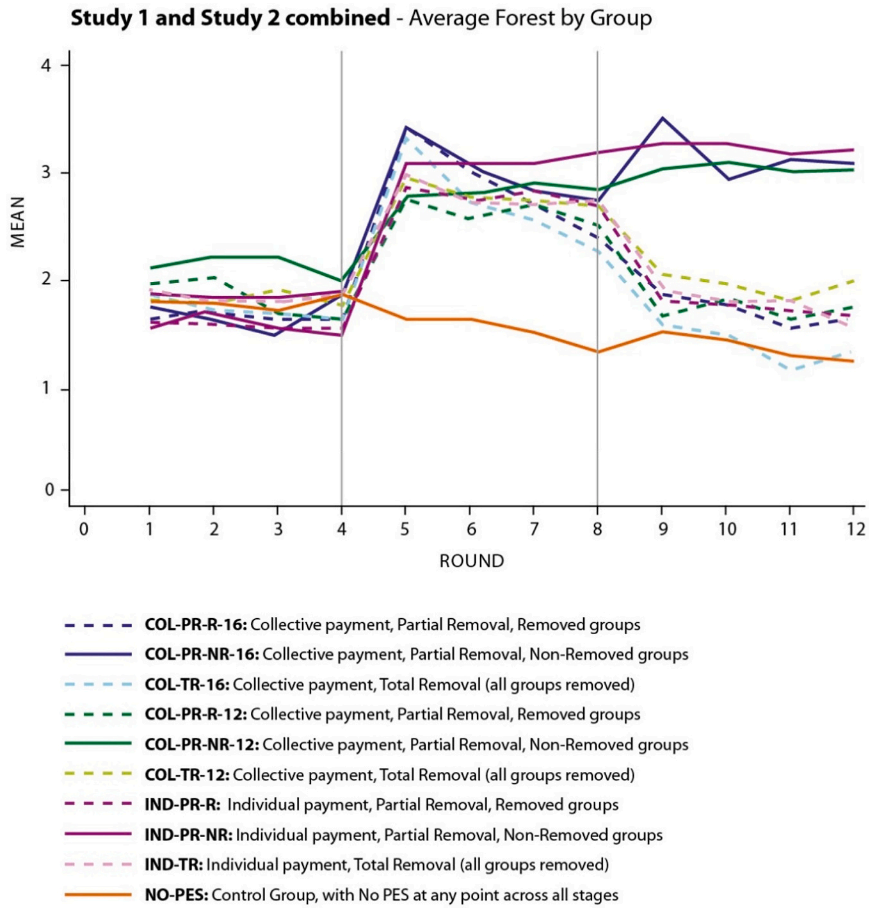

Fig. 2a. Average Forest by Group, Study 1 & Study 2 Data Combined.

run observations, starting higher but then falling balances out on average.

Collective versus *Individual during Payments.*

Unlike for our collective payment, which varied in threshold across these two studies, our individual payment is implemented the same way in each. Nonetheless, who gets assigned to individual payments is random and that can lead to different outcomes not only without a treatment (again see Rounds 1) but also with a treatment (different treatment impacts). The solid magenta lines in Figs. 2b and 2c for 'IND-PR-NR' treatment differ for Stage 2, with one above 3 units per person and one below. Thus, comparing individual to collective involves different collective treatments and different random draws for individual payments. Concerning the question of declines in contributions within Stage 2, beyond any 'restart effects' it is not clear that contributions with individual payments naturally decline, since there is no need to have confidence in others to identify individual forest units as raising earnings. Looking at Figs. 2 (in which Fig. 2 aggregates these two data sets), visually at least to first order we indeed do not see downward trends in contributions during Stage 2 for these individual payments.

From Figs. 2, then, we might not expect statistical differences during Stage 2 between the collective and individual payments designs. Indeed, tests in Annexes 5a and 5b show no significant differences between collective and individual payments in Stage 2 for both studies. As a collective threshold level of 12 does lead to 12 total, or 3 per person, that turns out to be similar to the average of our studies for individual payments, which are above and below 3 per person (and Fig. 2's magenta line is ~3). Further, as noted, the collective threshold of 16 may well start above but end up below threshold 12. At least in our context, then, these designs fared about the same (as confirmed in our annexes' tests).

To further examine statistically the pathways identified in Figs. 2, we conducted an individual-level panel regression with clusters at the group level. With 308 participants in Study 1 and 368 in Study 2, and 12 rounds for each, we have 3696 and 4416 observations in total, respectively, for these studies. Table 3 explains the lands allocated to forest, using individual fixed-effects since rounds are likely to be correlated within any subject. We use fixed effects, instead of random effects, after a Hausman test found that the latter would yield inconsistent estimates (though robustness checks had similar results).

Column (1) has results for 2019's Study 1, column (2) for 2021's Study 2, and column (3) pools studies. Column (4) pools all of the individual payments, which were unchanged in design across the studies, while disaggregating collective payments as in Study 1 the threshold was 16 while in Study 2 it was 12. Because we drop all fixed characteristics, Table 3 requires only dummies for stages and treatments. Stage dummies show the trends for No PES controls, while treatment dummies interact with stages. As we take out all fixed characteristics, that means we do not have coefficient for the treatments alone (i.e., the Stage-1 differences seen in Figs. 2), which are differenced out within our DID approach.

Critically for our 'crowd in' results, Stage 2 and 3 dummies are negative, i.e., No PES falls over time. As summarized in column (3), by Stage 3 the contributions are ~0.4 units lower per group member. That implies 'crowding in' of the same magnitude if the post-PES contributions are equal to pre-PES, while Stage 3 coefficients for PES-removed treatments are pre-PES level or a bit higher on average. Stepping back to Stage 2, all of the PES treatments increased conservation contributions in Stage 2, i.e., uniformly it was the case that, as summarized above, adding payments matters for contributions.

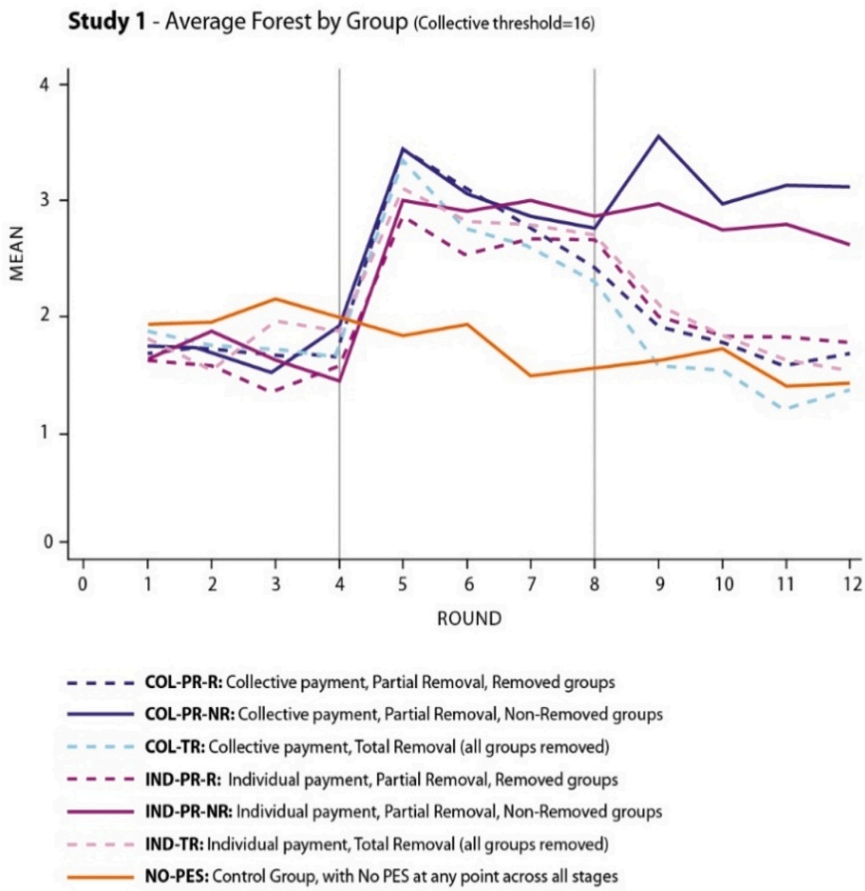

Fig. 2b. Average Forest by Group, Study 1 Data.

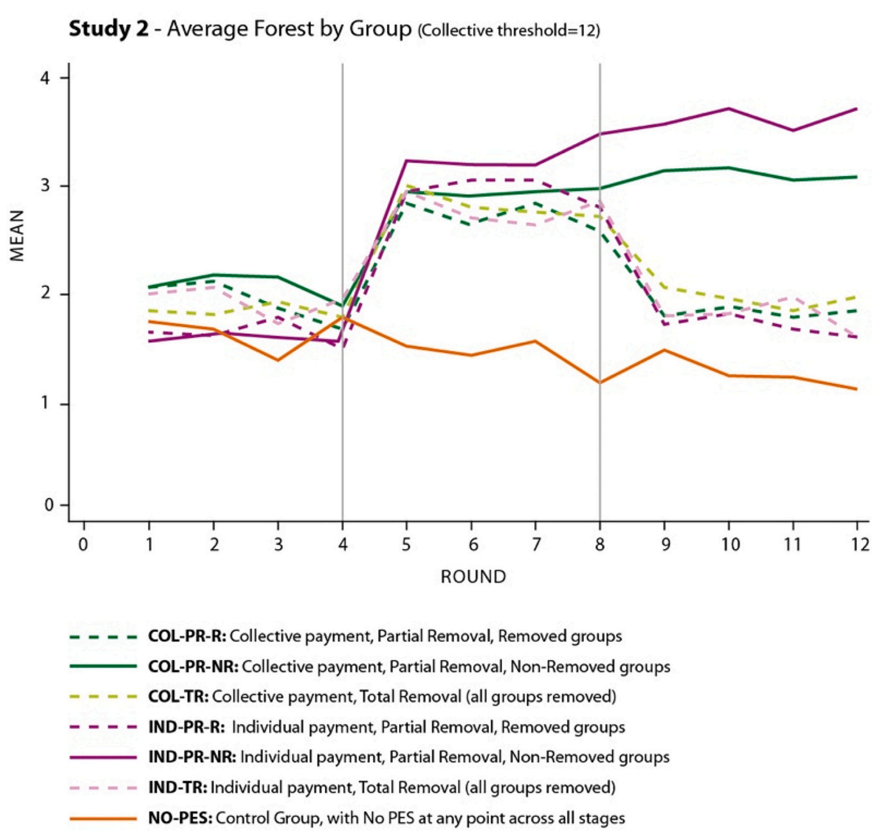

Fig. 2c. Average Forest by Group, Study 2 Data.

Table 3
Individual-level panel regression with clustering at the group level (fixed effects).

	(1)	(2)	(3)	(4)
VARIABLES	Study 1	Study 2	Combined Model	Collective Two Studies
stage2	−0.295***	−0.226**	−0.258***	−0.258***
	(0.0743)	(0.105)	(0.0666)	(0.0666)
stage3	−0.460***	−0.380**	−0.417***	−0.417***
	(0.0597)	(0.156)	(0.0889)	(0.0889)
IND-PR_R*Stage2	1.429***	1.503***	1.463***	1.463***
	(0.146)	(0.174)	(0.115)	(0.115)
IND-PR_NR*Stage2	1.598***	1.874***	1.758***	1.758***
	(0.208)	(0.288)	(0.192)	(0.192)
IND-TR*Stage2	1.339***	1.048***	1.176***	1.176***
	(0.164)	(0.170)	(0.121)	(0.121)
IND-PR_R*Stage3	0.759***	0.438**	0.595***	0.595***
	(0.126)	(0.204)	(0.126)	(0.126)
IND-PR_NR*Stage3	1.596***	2.372***	2.042***	2.042***
	(0.308)	(0.315)	(0.249)	(0.249)
IND-TR*Stage3	0.410***	0.245	0.319***	0.319***
	(0.128)	(0.178)	(0.113)	(0.113)
COL-PR_R*Stage2	1.516***	1.006***	1.246***	
	(0.200)	(0.142)	(0.126)	
COL-PR_NR*Stage2	1.601***	1.073***	1.311***	
	(0.225)	(0.217)	(0.164)	
COL-TR*Stage2	1.295***	1.181***	1.232***	
	(0.365)	(0.179)	(0.183)	
COL-PR_R*Stage3	0.501***	0.284	0.385***	
	(0.106)	(0.185)	(0.112)	
COL-PR_NR*Stage3	1.932***	1.397***	1.639***	
	(0.212)	(0.250)	(0.174)	
COL-TR*Stage3	0.129	0.500**	0.349**	
	(0.154)	(0.200)	(0.137)	
COL-PR_R-Study 1*Stage3				0.457***
				(0.125)
COL-PR_NR-Study 1*Stage3				1.889***
				(0.221)
COL-TR-Study 1*Stage3				0.0854
				(0.167)
COL-PR_R-Study 2*Stage3				0.321**
				(0.133)
COL-PR_NR-Study 2*Stage3				1.434***
				(0.214)
COL-TR-Study 2*Stage3				0.537***
				(0.154)
COL-PR_R -Study 1*Stage2				1.478***
				(0.197)
COL-PR_NR-Study 1*Stage2				1.563***
				(0.222)
COL-TR-Study 1*Stage2				1.258***
				(0.362)
COL-PR_R-Study 2*Stage2				1.037***
				(0.116)
COL-PR_NR-Study 2*Stage2				1.104***
				(0.200)
COL-TR-Study 2*Stage2				1.213***
				(0.158)
Constant	1.737***	1.845***	1.796***	1.796***
	(0.0355)	(0.0323)	(0.0249)	(0.0243)
Observations	3696	4416	8112	8112
R-squared	0.221	0.203	0.204	0.208
Number of exp_id	308	368	676	676

Robust standard errors in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

4.3. Stage 3 (rounds 9–12): PES removal leaves conservation similar to stage 1 (i.e., above controls)

4.3.1. Having faced payments doesn't matter

Fig. 2, 2b, and 2c show that if payments are removed, Stage 3 contributions roughly equal Stage 1. This can be seen in the groups' averages, i.e., broad bands of lines for treated in Figs. 2, 2b, 2c. Tables 4a and 4b then confirm numerically that for many of these treatments, and within each study, there are no statistically significant differences between Stage 3 and Stage 1. When one exists – with enough treatments we expect some random differences – the two statistically significant differences are of opposite signs. In sum, for most PES treatments and on average, post-PES is just like pre-PES.

That means 'DID' treatment impact for Stage 3 (versus Stage 1) will be driven by the control trends. This is particularly easy to see in Fig. 2c, for which the random differences across groups led the No PES control contributions in Round 1, and all of Stage 1, to be towards the bottom of the suite of treatments (PES lines in Fig. 2c). Given that, it is easy to spot the decline in contributions for control treatments, as well as the implied increase over time in the gap from the control to the PES. As the PES treatments basically return to pre-PES Stage-1 contributions levels, clearly it is the fall in the public-goods contributions over time in the controls, as in the literature, which drives such a gap.

Thus, even after they end, payments appear to 'crowd in' conservation at least in the short run (all we can comment on here), in the sense of holding off the 'business as usual' (BAU) drop in contributions. PES 'resets time', in a way, as post-PES it essentially appears as if the participants 'started life anew'. That visual impression is then confirmed in our regressions. The simplest way to see this is that the positive coefficients for the Stage-3 impacts of treatments are very close in magnitude to the negative coefficients on the Stage 3 dummies, which indicate the rate of BAU degradation within the controls. While it could be that this effect will vanish over time, we do see this form of 'short-run crowding in' and from a policy perspective – such as for REDD+ – there certainly can be value simply in a delay.

4.3.2. When removing payments, changing designs doesn't matter much (robust results)

4.3.2.1. Partial removal versus total removal. We can compare the 'Stage 3' coefficients for removed groups, between partial and total removal, to test whether their Stage3-minus-Stage1 changes differ significantly from each other (recalling that each such coefficient thereby compares that stage difference to the very same stage difference for controls). For this comparison, concerning 'relative crowding' if anything our expectation was that the removed individuals might be less happy in partial removal situations, where they are singled out to be removed. They might, as a result, be more likely to exhibit 'crowding out' or less likely to show 'crowding in'. However, for the (blended) collective treatments, there is little difference and no significance, while for individual treatments, the significant 'crowding in' is in fact significantly higher for partial removal.

From Table 3's Column 3	For Individual	For Collective
Total minus Partial	−0.276**	−0.0365
	(0.113)	(0.125)
#obs	8112	8112

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ (standard errors in parentheses).

4.3.2.2. Partial removal: considering those still paid. We may also be interested in whether groups who continue to receive PES, while others do not, respond differently to continued payment than they do to the initial arrival of conservation payments. They may feel especially grateful, as their relative position has improved between stages, even though treatment details have not. We can compare Stage 3 to Stage 2 coefficients for those groups in partial removal who still get paid. We find that, in comparison to themselves, the retained people conserve

Table 4a
Comparing Stages 1 & 3 for Study 1 (*t*-tests correcting for multiple testing using Bonferroni).

Treatment	Obs.	Mean Stage 3	Mean Stage 1	diff.	St.Err.	t-value	*p*-value
COL-PR_R	272	1.739	1.698	0.041	0.103	0.4	0.696
COL-TR	160	1.425	1.757	−0.331**	0.137	−2.45	0.016
IND-PR_R	224	1.853	1.554	0.299***	0.114	2.65	0.009
IND-TR	160	1.763	1.813	−0.05	0.139	−0.35	0.718
ALL (4)	816	1.713	1.693	0.021	0.06	0.35	0.73
NO PES	176	1.546	2.006	−0.46***	0.128	−3.6	0.001

Table 4b
Comparing Stages 1 & Stage 3 for Study 2 (t-test correcting for multiple testing using Bonferroni).

Treatment	Obs.	Mean Stage 3	Mean Stage 1	diff.	St.Err.	t-value	p-value
COL-PR_R	304	1.859	1.954	−0.096	0.104	−0.9	0.361
COL-TR	224	1.982	1.861	0.12	0.138	0.9	0.381
IND-PR_R	224	1.728	1.669	0.058	0.126	0.45	0.646
IND-TR	208	1.827	1.962	−0.135	0.13	−1.05	0.299
ALL (4)	960	1.850	1.867	−0.018	0.061	−0.3	0.773
NO PES	208	1.298	1.678	−0.38***	0.126	−3	0.003

*** $p < 0.01$, ** $p < 0.05$.

more in Stage 3, with very similar increases for individual and collective versions of this treatment (the latter more significant). At the very least for this treatment context, then, continuing payment did not diminish its impact.

From Table 3's Column 3	For Individual	For Collective
Partial, Not Removed	0.284*	0.328***
(Stage 3 minus Stage 2)	(0.148)	(0.109)
#obs	8112	8112

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ (standard errors in parentheses).

4.3.2.3. Collective versus individual payments. Returning to payments removal, we can compare 'crowding' between our individual and collective treatments (here blending collective treatments, whereas below we distinguish and compare them). We can again compare 'Stage 3' coefficients to see if their Stage3-minus-Stage1 changes differ (again these coefficients compare that stage difference to controls' stages difference). For 'relative crowding' we find little significance here, yet if anything, the collective 'crowding in' of contributions is lower. At this point, though, it may be worth recalling that in Table 3 every treatment is positive for Stage 3.

From Table 3's Column 3	Partial, Removed	Partial, Retained	Total Removal
Collective minus Indv.	−0.210*	−0.403	0.030
	(0.112)	(0.276)	(0.126)
#obs	8112	8112	8112

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ (standard errors in parentheses).

4.3.2.4. Comparing collective thresholds. 'Crowding in' also could vary, for collective-payment contexts, as a function of the collective design. As we noted above, the threshold of 12 is easier to achieve for a group, and perhaps then might be expected to be more sustained in success over time – which, in turn, in principle could set in motion new positive informal norms with levels of trust that sustain themselves. The 'crowding in' (relative to controls) was very similar for removed groups in partial removal – and not significantly different.

For the retained groups in partial removal, i.e., the groups to whom payments still are being made – meaning this result is not about 'crowding' at all – the contributions for a 16 threshold were higher. That is not too surprising, as there is still a reason for these groups to try for 16. For total removal, our priors are supported, as the 'crowding in' is significantly higher for the collective 12 threshold.

From Table 3's Column 4	Partial, Removed	Partial, Retained	Total Removal
Study 1 minus Study 2	0.136	0.455	−0.452**
	(0.132)	(0.282)	(0.189)
#obs	8112	8112	8112

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ (standard errors in parentheses).

5. Discussion

In our lab-in-the-field experiment, we found no evidence of post-PES 'crowding out' if incentives are removed for some or all recipients. In fact, our results suggested some form of short-run 'crowd in', as contributions were roughly the same after and before PES for those for whom payment ended, while controls' contributions fell.

Our robust sets of evidence included replication in two studies, which yielded extremely similar results. We also tested different PES design features: individual versus collective payment (with two versions of the collective threshold, one easier to achieve); partial versus total removal; and even a small check per the rationale on which partial removal is based. We consistently found 'crowding in' relative to the control, using the DID framework. We also found little difference in impact across these designs, although the easier collective threshold appears to be more stable than the difficult threshold (i.e., after the payments ended, better delayed the BAU fall in contributions). That said, all of this is subject to the caveat that this methodology can only comment on the short run, although for policy purposes even a temporary delay in emissions, for example, is known to be quite valuable.

These results with participants typical of rural PES programs lean against warnings about decreases in pro-environmental behaviors after an incentive is introduced then removed (Ezzine-de-Blas et al., 2019; Rode et al., 2015; Cardenas, 2000). Some report a "no pay, no care" view among some former participants if PES ends (Fisher, 2012). Yet our results fit with suggestive evidence of 'crowding in' (Akers and Yasué, 2019 review) – despite our potential for fairness-based rejections and, to start, a clear presence of pre-existing motivations that justify contributions in our pre-PES baseline.

We acknowledge concerns that lab experiments do not necessarily predict behaviors outside the lab – though experiments have predicted some behaviors better than traditional textbook microeconomics (see, e.g., a 2002 Nobel Prize). While we are not claiming external validity, lab-in-the-field experiments have the virtue that they tend to allow for more designs to be considered before trying any in reality at a greater financial and social cost. Further, while people vary greatly, our experiments are conducted with rural populations for whom considerations of actual PES introduction and removal are relevant. Finally, while often lab and daily behaviors are not compared, as far as we are aware when they have been compared (e.g., Rustagi et al., 2010; Fehr and Leibbrandt, 2011) some key gradients are consistent.

Thus, we believe our results can inform PES debates and actual PES designs. Overall, they showed participants did not conserve less forest when PES is removed, compared with our controls baseline. Comparing to controls who did not get payments, whose contributions trended downward over time, we found evidence of short-run 'crowding in' when our incentive was first offered and then removed.

One explanation for a lack of negative backlash could be simple "recognition or gratitude" (Bowles, 2008). Qualitative studies have found that if the state's presence is generally weak, PES participants may perceive payments positively, even after removal, as one form of long-awaited state recognition (Moros, 2019) – i.e., a good thing while it lasts and a fairer redistribution of the costs of conservation. When payments are removed, participants return to conservation contributions at or above their pre-PES levels, yet still a further study of participants' perceptions could add. From a policy perspective, we would hope that, even if temporary, interventions are perceived later to have improved the quality of life for their generally impoverished participants – even when sustainable land uses, per se, do not.

Perhaps realizing that someone would be willing to pay for forest had had a "frame-shifting effect" (Bowles, 2008; Ezzine-de-blas et al., 2019). Just like a 'market framing' might well trigger self-interest, the prospect of public and external willingness to pay for forest within a PES may make more salient a broad interdependence of socioeconomic and ecological systems (Lliso et al., 2020; Bernal-Escobar et al., n.d.). Our instructions highlighted the value of ecosystem services for human wellbeing. Future PES schemes might consider such communications with the potential beneficiaries.

We must also distinguish contexts, including reasons for PES removal. Some programs are finite by design, so those who sign up are aware and remain grateful when the payments end as expected. Other programs end, or have discontinuities in payments, due to implementation challenges or politics. That clearly could induce different reactions. Yet other PES adjust over time, as a matter of public policy, e.g., to a shift in political focus or in eligibility criteria. Reactions again might differ, requiring further research on the effect of different reasons for programs ending on pro-environmental behaviors and motivations. In any case, future program design and implementation should be transparent per program duration to avoid frustration, conflict, and unintended effects upon participants' behaviors.

Stepping back, many of these considerations suggest that, in design and implementation, promoters of PES may want to pay considerable attention to multiple forms of communication with the potential participants, including to shift local perceptions about who implements the PES and why. Legitimacy and trust affect local responses (Andersson et al., 2018). Local community organizations also matter and may affect individuals' motivations and behaviors. All these elements require study. While our findings are consistent with potential 'crowding in' in the short term – the devil remains in the details.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

Data will be made available on request.

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Annex 1 Payoff structure for different treatments

Baseline- No PES

Mi decisión		4 unidades de bosque y 0 en cultivos	3 unidades de bosque y 1 en cultivos	2 unidades de bosque y 2 en cultivos	1 unidad de bosque y 3 en cultivos	0 unidades de bosque y 4 en cultivos
Bosque de los demás	0	$800	$ 1.200	$ 1.600	$ 2.000	$ 2.400
	1	$ 1.000	$ 1.400	$ 1.800	$ 2.200	$ 2.600
	2	$ 1.200	$ 1.600	$ 2.000	$ 2.400	$ 2.800
	3	$ 1.400	$ 1.800	$ 2.200	$ 2.600	$ 3.000
	4	$ 1.600	$ 2.000	$ 2.400	$ 2.800	$ 3.200
	5	$ 1.800	$ 2.200	$ 2.600	$ 3.000	$ 3.400
	6	$ 2.000	$ 2.400	$ 2.800	$ 3.200	$ 3.600
	7	$ 2.200	$ 2.600	$ 3.000	$ 3.400	$ 3.800
	8	$ 2.400	$ 2.800	$ 3.200	$ 3.600	$ 4.000
	9	$ 2.600	$ 3.000	$ 3.400	$ 3.800	$ 4.200
	10	$ 2.800	$ 3.200	$ 3.600	$ 4.000	$ 4.400
	11	$ 3.000	$ 3.400	$ 3.800	$ 4.200	$ 4.600
	12	$ 3.200	$ 3.600	$ 4.000	$ 4.400	$ 4.800

Individual Payment

Mi decisión		4 unidades de bosque y 0 en cultivos	3 unidades de bosque y 1 en cultivos	2 unidades de bosque y 2 en cultivos	1 unidad de bosque y 3 en cultivos	0 unidades de bosque y 4 en cultivos
Bosque de los demás	0	$ 2.600	$ 2.550	$ 2.500	$ 2.450	$ 2.400
	1	$ 2.800	$ 2.750	$ 2.700	$ 2.650	$ 2.600
	2	$ 3.000	$ 2.950	$ 2.900	$ 2.850	$ 2.800
	3	$ 3.200	$ 3.150	$ 3.100	$ 3.050	$ 3.000
	4	$ 3.400	$ 3.350	$ 3.300	$ 3.250	$ 3.200
	5	$ 3.600	$ 3.550	$ 3.500	$ 3.450	$ 3.400
	6	$ 3.800	$ 3.750	$ 3.700	$ 3.650	$ 3.600
	7	$ 4.000	$ 3.950	$ 3.900	$ 3.850	$ 3.800
	8	$ 4.200	$ 4.150	$ 4.100	$ 4.050	$ 4.000
	9	$ 4.400	$ 4.350	$ 4.300	$ 4.250	$ 4.200
	10	$ 4.600	$ 4.550	$ 4.500	$ 4.450	$ 4.400
	11	$ 4.800	$ 4.750	$ 4.700	$ 4.650	$ 4.600
	12	$ 5.000	$ 4.950	$ 4.900	$ 4.850	$ 4.800

Collective payment ($1800) - Threshold 16

	Mi decisión	4 unidades de bosque y 0 en cultivos	3 unidades de bosque y 1 en cultivos	2 unidades de bosque y 2 en cultivos	1 unidad de bosque y 3 en cultivos	0 unidades de bosque y 4 en cultivos
Bosque de los demás	0	$800	$ 1.200	$ 1.600	$ 2.000	$ 2.400
	1	$ 1.000	$ 1.400	$ 1.800	$ 2.200	$ 2.600
	2	$ 1.200	$ 1.600	$ 2.000	$ 2.400	$ 2.800
	3	$ 1.400	$ 1.800	$ 2.200	$ 2.600	$ 3.000
	4	$ 1.600	$ 2.000	$ 2.400	$ 2.800	$ 3.200
	5	$ 1.800	$ 2.200	$ 2.600	$ 3.000	$ 3.400
	6	$ 2.000	$ 2.400	$ 2.800	$ 3.200	$ 3.600
	7	$ 2.200	$ 2.600	$ 3.000	$ 3.400	$ 3.800
	8	$ 2.400	$ 2.800	$ 3.200	$ 3.600	$ 4.000
	9	$ 2.600	$ 3.000	$ 3.400	$ 3.800	$ 4.200
	10	$ 2.800	$ 3.200	$ 3.600	$ 4.000	$ 4.400
	11	$ 3.000	$ 3.400	$ 3.800	$ 4.200	$ 4.600
	12	$ 5.000	$ 3.600	$ 4.000	$ 4.400	$ 4.800

Collective payment ($1350) - Threshold 12

Mi decisión		4 unidades de bosque y 0 en cultivos	3 unidades de bosque y 1 en cultivos	2 unidades de bosque y 2 en cultivos	1 unidad de bosque y 3 en cultivos	0 unidades de bosque y 4 en cultivos
Bosque de los demás	0	$800	$ 1.200	$ 1.600	$ 2.000	$ 2.400
	1	$ 1.000	$ 1.400	$ 1.800	$ 2.200	$ 2.600
	2	$ 1.200	$ 1.600	$ 2.000	$ 2.400	$ 2.800
	3	$ 1.400	$ 1.800	$ 2.200	$ 2.600	$ 3.000
	4	$ 1.600	$ 2.000	$ 2.400	$ 2.800	$ 3.200
	5	$ 1.800	$ 2.200	$ 2.600	$ 3.000	$ 3.400
	6	$ 2.000	$ 2.400	$ 2.800	$ 3.200	$ 3.600
	7	$ 2.200	$ 2.600	$ 3.000	$ 3.400	$ 3.800
	8	$ 3.750	$ 2.800	$ 3.200	$ 3.600	$ 4.000
	9	$ 3.950	$ 4.350	$ 3.400	$ 3.800	$ 4.200
	10	$ 4.150	$ 4.550	$ 4.950	$ 4.000	$ 4.400
	11	$ 4.350	$ 4.750	$ 5.150	$ 5.550	$ 4.600
	12	$ 4.550	$ 4.950	$ 5.350	$ 5.750	$ 6.150

Annex 2. Excerpt from protocol: removal framing

a)Water Partial Removal:

"As of this round, the environmental organization is going to suspend additional payments to some groups. This decision does not depend on your behavior in previous rounds. The environmental organization is interested in paying only the participants whose properties have the greatest number of water sources. Thus, the X groups that have, on average, MORE water sources on their land will continue to receive the payment from the environmental organization for conserving the forest. The X groups that have on average the LEAST water sources are excluded from payment in the following rounds."

b) Random Partial Removal:

As of this round, the environmental organization is going to suspend additional payments to some groups. This decision does not depend on your behavior in previous rounds. Randomly, __ groups will be chosen to CONTINUE receiving the environmental organization payment. The __ randomly selected groups continue to get paid. The __ groups that are NOT selected will NOT continue receiving the payment in the following rounds.

The way in which the __ groups that continue receiving the payment will be chosen, will be as follows: we will randomly draw __ ballots from this bag that contains _ ballots marked with the letter corresponding to each of the groups. [Assistant shows each ballot in the bag] We have the letter A, the letter B, the letter C, the letter D, and the letter E. The __ groups that are chosen CONTINUE to receive the payment from the environmental organization. The __ groups that are NOT selected, will NOT continue receiving the payment in the following rounds. Thus, ONLY the __ groups that are randomly chosen will continue to receive the payment from the environmental organization for conserving the forest.

c) Total Removal:

For collective payment: As of this round, the environmental organization suspends payments for all groups. In other words, they will NOT continue to make the additional payment of %1800/$1350 pesos to EACH participant if their community managed to have at least 16/12 forest units.

For individual payment: As of this round, the environmental organization suspends payments for all groups. In other words, you will NOT continue to receive the additional payment of an additional $450 pesos for each plot of land that you leave in the forest.

Annex 3a Differences between removal rules

	R	W	RANDOM (R)		WATER (W)		RANDOM	WATER				
Treatment	Obs.	Obs.	Mean S3	Mean S1	Mean S3	Mean S1	Diff. R S3 vs. S1	Diff. W S3 vs. S1	Diff. R vs. Diff. W	St.Err.	t-value	p-value
COL-PR_R	28	40	1.661	1.848	1.794	1.594	−0.188	0.2	−0.388	0.202	−1.9	*0.059
COL-PR_NR	16	20	3.078	1.672	3.288	1.763	1.407	1.525	−0.119	0.316	−0.4	0.71
IND-PR_R	28	28	2	1.688	1.705	1.42	0.313	0.286	0.027	0.229	0.1	0.907
IND-PR_NR	12	12	2.729	1.521	2.833	1.771	1.209	1.063	0.146	0.474	0.3	0.761

*** $p < 0.01$, ** $p < 0.05$, *$p < 0.1$.

Differences in differences comparing Stage3 [S3] and Stage 1 [S1] for partial removal treatments (correct for multiple tests using Bonferroni). “R” is for removed participants and “NR” for non removed.

Annex 3b Regression including all treatments (PR and TR) and interactions

(with dummy_wr (1 = water; 0 = else) only for removed treatments in all stages)

VARIABLES	Model 1	Model 2	Model 3
Dummy Stage2	−0.395**	−0.395**	−0.381**
	(0.162)	(0.162)	(0.181)
Dummy Stage3	−0.629***	−0.630***	−0.568***
	(0.162)	(0.162)	(0.182)
COL-PR_R	−0.215	−0.138	−0.126
	(0.309)	(0.303)	(0.323)
COL-PR_R_WR	−0.330	−0.218	−0.165
	(0.314)	(0.307)	(0.328)
COL-PR_NR	−0.368	−0.250	−0.199
	(0.286)	(0.282)	(0.302)
COL-TR	−0.294	0.072	0.021
	(0.279)	(0.282)	(0.300)
IND-PR_R	−0.446	−0.308	−0.323
	(0.308)	(0.303)	(0.325)
IND-PR_R_WR	−0.321	−0.374	−0.343
	(0.340)	(0.330)	(0.354)
IND-PR_NR	−0.434	−0.188	−0.167
	(0.326)	(0.326)	(0.349)
IND-TR	−0.254	−0.224	−0.222
	(0.278)	(0.271)	(0.290)
COL-PR_R_ST2	1.589***	1.586***	1.437***
	(0.262)	(0.262)	(0.294)
COL-PR_R_ST2_WR	1.136***	1.137***	1.056***
	(0.272)	(0.271)	(0.303)
COL-PR_NR _ ST2	2.423***	2.421***	2.208***
	(0.247)	(0.247)	(0.281)
COL-TR _ ST2	1.929***	1.927***	1.858***
	(0.237)	(0.237)	(0.268)
IND-PR_R _ST2	2.187***	2.184***	2.064***
	(0.261)	(0.261)	(0.297)
IND-PR_R _ ST2_WR	−0.442	−0.440	−0.407
	(0.286)	(0.286)	(0.320)
IND-PR_NR_ ST2	2.295***	2.299***	2.147***
	(0.278)	(0.278)	(0.314)

(continued on next page)

(*continued*)

VARIABLES	Model 1	Model 2	Model 3
IND-TR_ ST2	1.800***	1.802***	1.644***
	(0.233)	(0.233)	(0.265)
COL-PR_R _ST3	0.339	0.339	0.282
	(0.260)	(0.259)	(0.290)
COL-PR_R _ST3_WR	0.505*	0.505*	0.423
	(0.263)	(0.262)	(0.293)
COL-PR_NR _ ST3	3.030***	3.028***	2.727***
	(0.251)	(0.251)	(0.290)
COL-TR_ST3	0.154	0.154	0.203
	(0.233)	(0.233)	(0.261)
IND-PR_R_ ST3	1.053***	1.053***	0.962***
	(0.258)	(0.258)	(0.292)
IND-PR_R_ ST3_WR	−0.032	−0.032	−0.030
	(0.283)	(0.283)	(0.317)
IND-PR_NR_ ST3	2.269***	2.271***	2.015***
	(0.277)	(0.277)	(0.317)
IND-TR_ ST3	0.565**	0.565**	0.471*
	(0.232)	(0.232)	(0.261)
Age		0.011**	0.011**
		(0.005)	(0.005)
Gender		−0.085	−0.104
		(0.131)	(0.132)
Education		−0.024	−0.024
		(0.017)	(0.017)
Household Income		−0.000	−0.000
		(0.000)	(0.000)
Cut-down forest before		0.308**	0.319**
		(0.143)	(0.144)
Previous Payment		0.143	0.141
		(0.159)	(0.160)
Group forest previous round			0.053***
			(0.011)
Constant	2.017***	1.447***	1.012***
	(0.193)	(0.367)	(0.386)
Observations	3696	3696	3388
Number of exp_id	308	308	308

Standard errors in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Annex 3c Descriptive statistics Study 1

Treatment	Obs.	Age (Years)	Gender (1 = female)	Education (years)	Income (K pesos)	Have Cut? (1 = yes)	Been Paid? (1 = yes)
COL-PR_R	68	50.18**	0.309	6.368**	764.9	0.235***	0.176
COL-PR_NR	36	53.56	0.333	6.500**	683.3	0.194***	0.194
COL-TR	40	39.15***	0.525***	8.600***	603.5	0.125***	0.125**
IND-PR_R	56	52.21	0.554***	6.554***	740.2	0.339***	0.179
IND-PR_NR	24	51.50	0.500***	7.958***	2145***	0.167***	0.167
IND-TR	40	51.95	0.400	6.050	514.3	0.450	0.300**
NO PES	44	53.05	0.318	5.409	485.2	0.477	0.205

*** $p < 0.01$, ** $p < 0.05$ Indicating differences against NO PES group.
Descriptive Statistics of control variables and t-tests between CG's and TG's for Study 1 correcting for multiple testing using Bonferroni.

Annex 3d Descriptive statistics Study 2

Treatment	Obs.	Age (Years)	Gender (1 = female)	Education (years)	Income (K pesos)	Have Cut? (1 = yes)	Been Paid? (1 = yes)
COL-PR_R	76	48.62	0.461	7.803	867.8**	0.145	0.303***
COL-PR_NR	44	49.07	0.364***	7.205	697.0	0.182	0.250***
COL-TR	56	46.86	0.500	7.286	708.8	0.232***	0.196***
IND-PR_R	56	44.12	0.554	7.554	787.2	0.125	0.304**
IND-PR_NR	32	45.88	0.500	8.219	1795***	0.125	0.219***
IND-TR	52	57.67***	0.423***	7.481	795.6	0.192	0.154***
NO PES	52	47.58	0.538	7.808	625.8	0.135	0.385

*** $p < 0.01$, ** $p < 0.05$ Indicating differences against NO PES group.
Descriptive Statistics of control variables and t-tests between CG's and TG's for Study 2 correcting for multiple testing using Bonferroni.

Annex 4a Test comparing Stage 1 vs. Stage 2 Study 1

Treatment	Obs.	Mean Stage 2	Mean Stage 1	diff.	St.Err.	t-value	p-value
COL-PR_R	272	2.919	1.698	1.22***	0.109	11.25	0
COL-PR_NR	144	3.028	1.722	1.306***	0.146	8.95	0
COL-TR	160	2.756	1.756	1***	0.152	6.6	0
IND-PR_R	224	2.688	1.554	1.134***	0.105	10.85	0
IND-PR_NR	96	2.948	1.646	1.302***	0.176	7.4	0
IND-TR	160	2.857	1.813	1.044***	0.123	8.45	0
NO PES	176	1.71	2.006	−0.295**	0.132	−2.25	0.025

*** $p < 0.01$, ** $p < 0.05$.
T-tests comparing differences between Stage 1 and Stage 2 within each TG - Study 1, correcting for multiple testing using Bonferroni. "R" is for removed participants and "NR" for non removed.

Annex 4b Test comparing Stage 1 vs. Stage 2 Study 2

Treatment	Obs.	Mean Stage 2	Mean Stage 1	diff.	St.Err.	t-value	p-value
COL-PR_R	304	2.734	1.954	0.779***	0.096	8.2	0
COL-PR_NR	176	2.932	2.085	0.847***	0.121	6.95	0
COL-TR	224	2.817	1.861	0.956***	0.125	7.65	0
IND-PR_R	224	2.946	1.669	1.277***	0.11	11.65	0
IND-PR_NR	128	3.265	1.617	1.649***	0.141	11.7	0
IND-TR	208	2.784	1.962	0.822***	0.119	6.95	0
NO PES	208	1.452	1.678	−0.226	0.125	−1.8	0.071

*** $p < 0.01$, ** $p < 0.05$.
T-tests comparing differences between Stage 1 and Stage 2 within each TG - Study 2, correcting for multiple testing using Bonferroni. "R" is for removed participants and "NR" for non removed.

Annex 5a Test comparing collective vs. individual payments stage 2 and Stage 3- Study 1

	IND	COL	IND	COL				
Removal	Obs.	Obs.	Mean	Mean	Diff.	St.Err.	t-value	p-value
PR_R	224	272	2.688	2.919	−0.232	0.112	−2.05	0.039
PR_NR	96	144	2.948	3.028	−0.08	0.157	−0.5	0.611
TR	160	160	2.857	2.756	0.1	0.142	0.7	0.481

T-tests comparing differences between COL and IND for Stage 2 - Study 1, *** $p < 0.01$, ** $p < 0.05$, *$p < 0.1$, correcting for multiple testing using Bonferroni.

	IND	COL	IND	COL				
Removal	Obs.	Obs.	Mean	Mean	Diff.	St.Err.	t-value	p-value
PR_R	224	272	1.853	1.739	0.114	0.113	1	0.315
PR_NR	96	144	2.781	3.195	−0.413	0.159	−2.6	0.01
TR	160	160	1.762	1.425	0.338	0.14	2.4	0.017

T-tests comparing differences between COL and IND for Stage 3 - Study 1, *** $p < 0.01$, ** $p < 0.05$, *$p < 0.1$, correcting for multiple testing using Bonferroni.

Annex 5b Test comparing collective vs. individual payments stage 2 and Stage 3 - Study 2

	IND	COL	IND	COL				
Removal	Obs.	Obs.	Mean	Mean	Diff.	St.Err.	t-value	p-value
PR_R	224	304	2.946	2.734	0.213	0.096	2.25	0.026
PR_NR	128	176	3.265	2.932	0.334	0.121	2.75	0.006
TR	208	224	2.784	2.817	−0.034	0.118	−0.3	0.777

T-tests comparing differences between COL and IND for Stage 2 - Study 2, *** $p < 0.01$, ** $p < 0.05$, *$p < 0.1$, correcting for multiple testing using Bonferroni.

	IND	COL	IND	COL				
Removal	Obs.	Obs.	Mean	Mean	Diff.	St.Err.	t-value	p-value
PR_R	224	304	1.728	1.859	−0.131	0.12	−1.1	0.277
PR_NR	128	176	3.610	3.103	0.507	0.104	4.9	0**
TR	208	224	1.827	1.982	−0.155	0.142	−1.1	0.273

T-tests comparing differences between COL and IND for Stage 3 - Study 2, *** $p < 0.01$, ** $p < 0.05$, *$p < 0.1$, correcting for multiple testing using Bonferroni.

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