Non-monetary factors as moderators of motivation crowding in incentive-based environmental management: An experimental approach

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A la memoria de Cequi.

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Summary

Environmental degradation is threatening the provision of ecosystem services (IPBES, 2019). In an attempt to reverse this degradation trend, the use of economic incentives has increasingly gained prominence in environmental policy. In particular, payments for ecosystem services (PES), a type of economic incentive in which ecosystem services providers voluntarily receive a payment in exchange for the provision of ecosystem services, has increasingly gained relevance worldwide (Salzman et al., 2018). PES aim to enhance environmental conservation by altering the economic costs or benefits associated with targeted pro-environmental actions. However, a large amount of empirical evidence shows that PES, like other economic incentives, could potentially crowd out (or crowd in) intrinsic motivations for environmental conservation, which may sometimes translate into unexpected undesirable overall effects (for reviews, see Bowles and Polania-Reyes, 2012; Rode et al., 2015). Up to date, the conditions under which PES, and economic incentives in general, induce such motivational crowding effects are still not fully understood.

In analyzing these effects, economic theory has often taken for granted non-monetary factors of economic incentives because they do not involve changes in prices or incomes. However, experimental evidence show that such factors could be relevant to preventing crowding-out effects or even creating crowding-in effects (e.g., see Ariely et al., 2009; Heyman and Ariely, 2004). In the context of PES, a large proportion of the experimental literature has focused on studying the effect of design features related to the monetary elements of these programs (e.g., Kerr et al., 2012; Kolinjivadi et al., 2019; Midler et al., 2015; Moros et al., 2019; Narloch et al., 2012), while fewer experimental studies have focused on non-monetary factors such as the degree of participation in the design or implementation process (e.g., Jack, 2009; Moros et al., 2019; Vollan, 2008). The present thesis contributes to this line of literature by examining whether PES could be more or less effective when specific non-monetary factors are adjusted in PES design or implementation.

This thesis comprises an introductory chapter (Chapter 1) and a concluding chapter (Chapter 5) that serve as an overview of the thesis, together with three research articles (Chapters 2, 3 and 4). In each of the research articles belonging to this thesis, a specific non-monetary factor in the design or implementation context of PES is analyzed. Prior versions of the research articles in chapters 2-4 have been published as working papers on SSRN. Their content is briefly summarized below.

Chapter 2 is entitled “Beyond a Market Discourse: Is Framing a Solution to Avoid Motivational Crowding-Out in Payments for Ecosystem Services?” and is co-authored by S. Engel and E. Midler. The goal of this article is to examine the role of the framing of a PES policy in preventing motivational crowding effects. In particular, it focusses on the short- and long-term effects of two dimensions of framing on: 1) using different terms to denote the payment, and 2) emphasizing different types of ecosystem services obtained from nature to motivate its conservation. This article analyses this topic with a lab-in-the-field experiment conducted with 157 farmers from a Colombian municipality. The findings of this article suggest that the framing of a policy could be a rather inexpensive tool to mitigate motivational crowding effects. In particular, a framing that acknowledges forest conservation as an achievement and a framing that emphasizes the cultural ecosystem services obtained from forest results in better conservation outcomes relative to a control framing.

Chapter 3 is entitled **“**Who is Benefiting Downstream? Experimental Evidence on the Relevance of Upstream-Downstream Geographic Distance for Water Provision.” and is co-authored by S. Engel, E.

Midler and T. Vorlaufer. The goal of this article is to study the relevance of the social distance between ecosystem service providers and beneficiaries for the short- and long-term motivational crowding effects of a PES policy. This article analyses this topic with a lab-in-the-field experiment in which the salience of social identity is defined by the real geographical distance between ecosystem service providers and beneficiaries, rather than being artificially induced. In particular, the experiment involves 60 rural farmers from a Colombian municipality, whose water provision decisions affected passive downstream beneficiaries in either the same municipality or the capital city of Bogotá. The findings suggest that although sharing a closer place identity with downstream beneficiaries is relevant to determine baseline water provision, it does not affect average motivational crowding effects in the short and long term. Nonetheless, predictions from the econometric analysis of the heterogeneous treatment effects suggest that emphasizing the benefits provided to people with whom upstream providers feel more socially identified could mitigate long-term crowding-out effects on providers with weak levels of place identification, without compromising the short-term effectiveness on farmers with a strong place identity and already high provisioning levels. Therefore, in the absence of a well-defined group of downstream beneficiaries financing a PES program, it could be more effective to emphasize local benefits, rather than the general benefits to society.

Chapter 4 is entitled **"**Behavioral Spillovers from Mixing Conservation Policies in Neighboring Areas: An Experimental Analysis on Fairness Perceptions toward Unequal Policies." and is co-authored by S. Engel and E. Midler. The goal of this article is to analyze the impact of fairness concerns on the effectiveness of a policy that is implemented in an unequal institutional context. In particular, it compares the effectiveness of implementing a specific economic incentive when a monetary reward is being implemented in a neighboring area, to the effectiveness of implementing the same economic incentive over the entire area. This article analyses this topic with a lab-in-the-field experiment conducted with 276 farmers from a Colombian municipality. In particular, the treatment recreated three institutional contexts: 1) a high priority area where PES are implemented next to a low priority area that is excluded from PES, 2) a protected area with land-use restrictions surrounded by a buffer area where PES are implemented, and 3) a protected area where PES are implemented on top of land-use restrictions, surrounded by a buffer area with only PES. Surprisingly, the findings show that fairness concerns do not increase with the level of inequality between neighbors. Although PES exclusion and simultaneously implementing PES inside and outside a protected area tend to increase fairness concerns and reduce forest conservation relative to the control groups, implementing PES only in the buffer area of a protected area decreases fairness concerns and increases forest conservation by those within the protected area, even after the policies are removed. Overall, this article stresses the relevance of considering the institutional context and context-specific fairness perceptions as a relevant factor when defining the targeting criteria for the implementation of environmental policies.

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Chapter 1 Introduction

1.1. Motivation and overview of the thesis

Nature is a source of well-being for humans. From direct products such as food and fuel, to non-material benefits such as recreation and spiritual experiences, nature provides many and varied benefits to humans, so-called *ecosystem services*. Ecosystem services can include provisioning, regulating, cultural, or supporting services (MEA, 2005). Nevertheless, despite the importance of these benefits, individuals typically have insufficient incentive to maintain them because many ecosystem services are public goods and/or their degradation is due to the existence of environmental externalities (both spatial and temporal). Solving these so-called social dilemmas has proven to be a difficult task, resulting in unprecedented rates of environmental degradation. The expansion of agriculture, forestry, urbanization, and material harvesting over the last five decades has led to an approximately 47% reduction of global indicators of ecosystem extent and condition against their estimated natural baseline levels (IPBES, 2019). This high rate of decline in ecosystems is threatening ecosystems ability to continuously provide ecosystem services (Ibid).

In an attempt to reverse this degradation trend, the use of economic incentives has increasingly gained prominence in environmental policy. In this context, economic incentives are external instruments that aim to enhance environmental conservation by altering the economic costs or benefits associated with targeted pro-environmental actions such as, e.g., reforestation, watershed protection or reduced carbon emissions. Under a budget constraint, the challenge involves finding a cost-effective incentive level that induces socially-efficient pro-environmental actions. However, since economic incentives not only alter the net benefits for the targeted action but could also change individuals' motivations for taking such action, the implementation of economic incentives can lead to unexpected results (for reviews, see Bowles and Polania-Reyes, 2012; Rode et al., 2015). Therefore, the conditions under which a specific economic incentive does or does not work are still not fully understood. The present thesis contributes to this line of literature by examining whether economic incentives could be more or less effective when adjusting non-monetary factors in their design or implementation context.

In particular, this thesis has a strong focus on payments for ecosystem services (PES). PES are a type of economic incentive in which providers of ecosystem services voluntarily apply for receiving a payment (monetary or in-kind) that is conditional on providing these services or implementing specific practices that ensure the provision of these services (Engel, 2016). Although PES are increasingly being implemented worldwide (Salzman et al., 2018), many of which are in the Global South (Schomers and Matzdorf, 2013), their effectiveness has been so far highly context-dependent. While studies have suggested that this policy might be serving its purpose in some cases (Bremer et al., 2014; Grillos, 2017; Arriagada et al., 2018; Grillos et al., 2019), others have found evidence of counter-productive effects (van Hecken and Bastiaensen, 2010; Fisher, 2012; Rico García-Amado et al., 2013).

Among the ecosystem services that have been most severely degraded are those associated with watersheds (e.g., fresh water purification and provision; MEA, 2005; IPBES, 2019). Due to the existing asymmetry in water appropriation, watershed management involves dealing with upstream land use externalities on downstream water availability. To improve watershed management, direct payments to upstream water providers are increasingly being implemented as a complementary tool to conventional

regulatory approaches (Porras et al., 2008). Therefore, I designed a lab-in-the-field experiment to recreate the typical social dilemmas that emerge in watershed management and used it to study the behavior of Colombian farmers from areas of high hydrological richness. Specifically, I assessed farmers' decisions regarding upstream land use given the externalities that upstream land-use changes can have on downstream water availability and quality. Within each experiment, farmers were exposed to an external incentive whose monetary level was fixed across treatments but differed in certain non-monetary factors related to the design or the implementation context. By comparing changes that the implementation of incentives produced in farmers' decisions between treatments, I could disentangle the effect of each non-monetary factor on short- and long-term incentive effectiveness. Short term here refers to the period when the monetary incentive was introduced, while long term refers to after the incentive was removed.

In the next three chapters of this thesis, I evaluate the relevance of three non-monetary factors for the design and implementation of PES. In Chapter 2, I examine the role of the discourse used to communicate the implementation of a monetary reward—an aspect typically known as framing. In Chapter 3, I compare the effect of implementing a reward in two different contexts, both of which vary in the social distance that exists between ecosystem service providers and beneficiaries. In Chapter 4, I focus on the relevance of the institutional context in which an economic incentive is being implemented. In particular, I compare the effectiveness of implementing a specific economic incentive in an unequal institutional context in which a monetary reward is being implemented in a neighboring area, with that of implementing the same economic incentive over the entire area. Finally, in Chapter 5, I present the conclusions of this thesis, starting with a general discussion of the main insights in each chapter, followed by a look at the limitations of my approach.

This first introductory chapter presents an overview of the thesis and is divided into five sections. In Section 1.2, I present the conceptual foundation of this thesis based on a literature review of what motivates human behavior. In Section 1.3, I describe the contributions of this thesis based on the gaps identified in the literature on PES. In Section 1.4, I introduce the research questions addressed in each chapter. In Section 1.5, I describe the methodology I used to disentangle the effect of each individual non-monetary factor on incentive effectiveness.

1.2. Conceptual and theoretical foundation

This thesis draws on self-determination theory to explain human motivation behind pro-environmental behavior (PEB), which here refers to the behavior of consciously reducing the negative impact of one's actions on the biophysical environment (e.g. reforestation, watershed protection, reduced carbon emissions). Self-determination theory considers that the human motivation to perform an action lies in a continuum between being purely intrinsically motivated and being purely extrinsically motivated (Ryan and Deci, 2000). A purely extrinsically motivated individual performs an action solely to attain some separable outcome that is external to the enjoyment of the activity itself. On the contrary, an intrinsically motivated individual is also motivated by how they feel when performing the action based on the satisfaction inherent in the activity itself. Figure 1 presents the framework used in this thesis to conceptualize individuals' motivation behind PEB. Given that humans are part of societies and nature, this framework highlights the importance of an individual's decision context by embedding the individual and its motivations within a socio-ecological system (light gray shaded box in Figure 1). This means that human motivations are shaped by an individual's perceptions of their surrounding social and biophysical contexts,

which are subsequently modified by their choices and behavior (light gray arrow in Figure 1). For this reason, individual motivations are dynamic and susceptible to change.

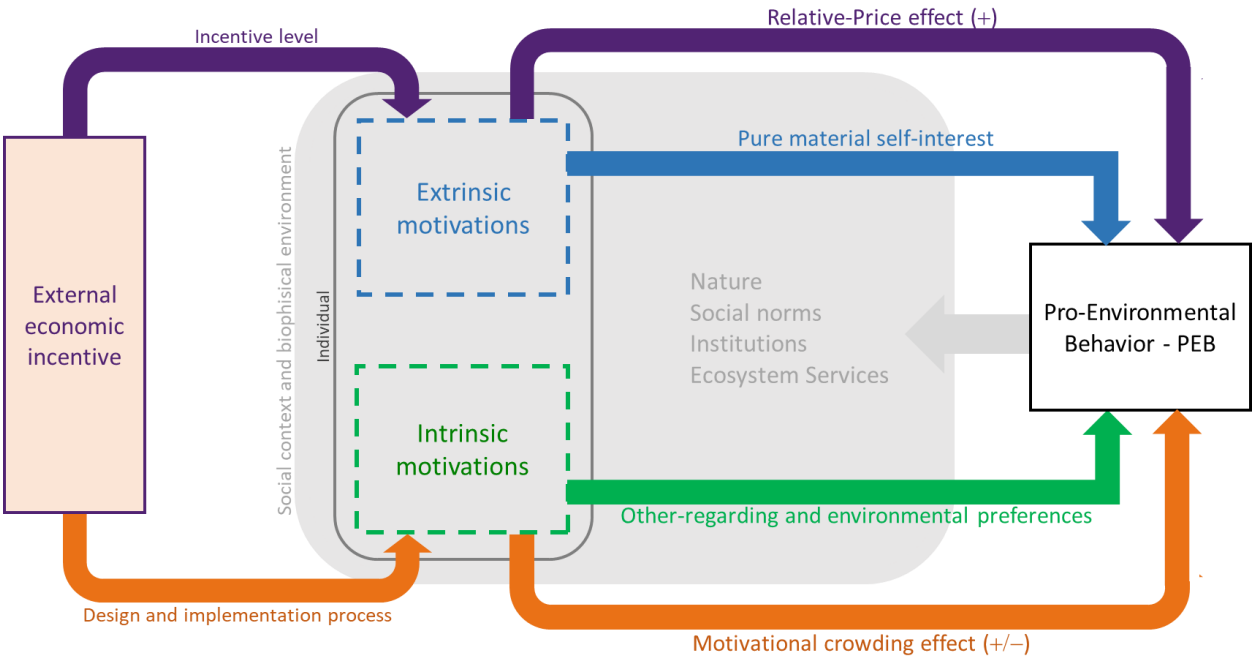

Figure 1. Conceptual framework of the effect of external economic incentives on PEB. The relative-price effect of the external economic incentive (purple arrows) directly affects the individual's extrinsic motivations for pro-environmental behavior (PEB). This effect is always positive (i.e., increases PEB) and proportional to the level of the incentive. The motivational crowding effect of the external economic incentive (orange arrows) indirectly affects the individual's intrinsic motivations for PEB. This effect could be positive or negative depending on the incentive design and implementation process.

In this framework, I link motivations to social preferences. Therefore, a purely extrinsically motivated individual equates to a self-interested, utility-maximizing, rational individual, driven solely by the material or instrumental benefits obtained from their PEB (blue dotted box and blue arrow in Figure 1). If the individual also considers social and environmental costs and benefits within their utility maximization process (i.e. other-regarding and environmental preferences), the individual's PEB will then also be intrinsically motivated (green dotted box and green arrow in Figure 1). Since PEB often involves internalizing externalities or providing public goods, individuals—in the absence of external intervention—have few extrinsic motivations for PEB. Furthermore, empirical evidence has shown that other-regarding and environmental preferences are often insufficient to reach socially optimal levels of PEB (Gneezy et al., 2011). To find a solution to the aforementioned dilemma, a social planner has the option of increasing extrinsic motivations for PEB through the introduction of an external economic incentive (light orange box in Figure 1).

According to standard economic assumptions, the effectiveness of the external economic incentive in promoting PEB would be determined by the size of the direct effect that the incentive has on extrinsic motivations. This so-called relative-price effect reduces the opportunity cost of PEB by changing the monetary costs and benefits of targeted actions. The greater the incentive level, the greater the size of the relative-price effect and thus the expected increase in PEB (purple arrows at the top of Figure 1).

However, motivation crowding theory[1] challenges this assumption by proposing that economic incentives could also have an effect on intrinsic motivations (Frey, 1992, 1993, 1997). More specifically, depending on certain aspects of the economic incentive design and implementation process, the implementation of external economic incentives could erode or reinforce intrinsic motivations for PEB (orange arrows at the bottom of Figure 1).

Self-determination theory suggests that the level of intrinsic motivation depends on how satisfied a set of psychological needs are when making a choice or performing an action. In particular, the theory proposes three psychological needs: 1) "autonomy" or the need to control our own decisions; 2) "competence" or the need to have our abilities recognized; and 3) "relatedness" or the need to feel belongingness and connectedness with others. Therefore, external economic incentives could crowd-out or erode intrinsic motivations if: 1) an individual no-longer feels responsible for their own actions but attributes the responsibility to the provider of external incentive instead, 2) the external incentive is perceived as denying individuals' ability to impact society, or 3) the social contract of trust and reciprocity breaks down or cannot be verified due to the introduction of the external incentive.

For instance, negative incentives like sanctions and taxes are often seen as rather controlling as they seek to ban detrimental practices by making them more costly or less profitable (Pannell, 2008; Vollan, 2008). They could also become an excuse for selfishness by changing the mode of behavior from "social" to "strategic" (Falkinger et al., 2000; Gneezy and Rustichini, 2000), destroying intrinsic trust (Fehr and Rockenbach, 2003; Fehr and List, 2004) or signaling the price for undesirable behavior, which, once paid, could relieve people of the moral responsibility to avoid such conduct (Frey, 1992; Gneezy et al., 2011). On the contrary, positive economic incentives like rewards or subsidies are often seen as more acknowledging, as they seek to encourage or recognize the performance of sustainable practices (Pannell, 2008; Vollan, 2008). Nonetheless, monetary rewards could also shift an individual's mindset from a "social reciprocal mindset" to a "market competitive mindset" in which the performance of sustainable behaviors would be conditional on getting paid (Gneezy and Rustichini, 2000; Heyman and Ariely, 2004; Mellström and Johannesson, 2008). Similarly, perceptions of fairness could also affect incentive acceptance (Pascual et al., 2014; Friedman et al., 2018). Although negative incentives might be more easily associated with equity concerns because they could limit livelihood options for poorer farmers dependent on natural resources, positive incentives could also involve equity concerns depending on, for example, how rewards are distributed.

Thus, although the relative-price effect always increases extrinsic motivations for PEB, the motivational crowding effect could increase or decrease intrinsic motivations for PEB. If the relative-price effect and the motivational crowding effect go in opposite directions, whether the external economic incentive increases or reduces PEB will depend on which effect dominates. To better illustrate this, Figure 2 presents a hypothetical baseline scenario in which the sum of intrinsic and extrinsic motivations (level B in the figure) are not enough to achieve the socially optimal level of PEB (denoted as C). The social planner expects to solve this social dilemma by introducing an external economic incentive that increases extrinsic motivations for PEB from level A to level A', which together with intrinsic motivations is then expected to result in the socially optimal level C (see the Expected scenario 1 of Figure 2). However, as mentioned before, the observed amount of PEB would also depend on the size and direction of the motivational

[1] Motivation crowding theory was integrated into economic theory based on social psychologists' cognitive evaluation theory (Deci and Ryan, 1985), a sub-theory of self-determination theory.

crowding effect. If the external economic incentive erodes or crowds out intrinsic motivations more than it increases extrinsic motivations, the observed levels of PEB in the short-term do not increase—but rather decrease—relative to the baseline scenario (see the Scenario 1 of Figure 2, where the resulting level is D<B). This is one of the reasons empirical evidence shows that the use of low levels of incentive is more likely to lead to less prosocial behavior (Gneezy and Rustichini, 2000; Cardenas, 2004; Handberg and Angelsen, 2019).

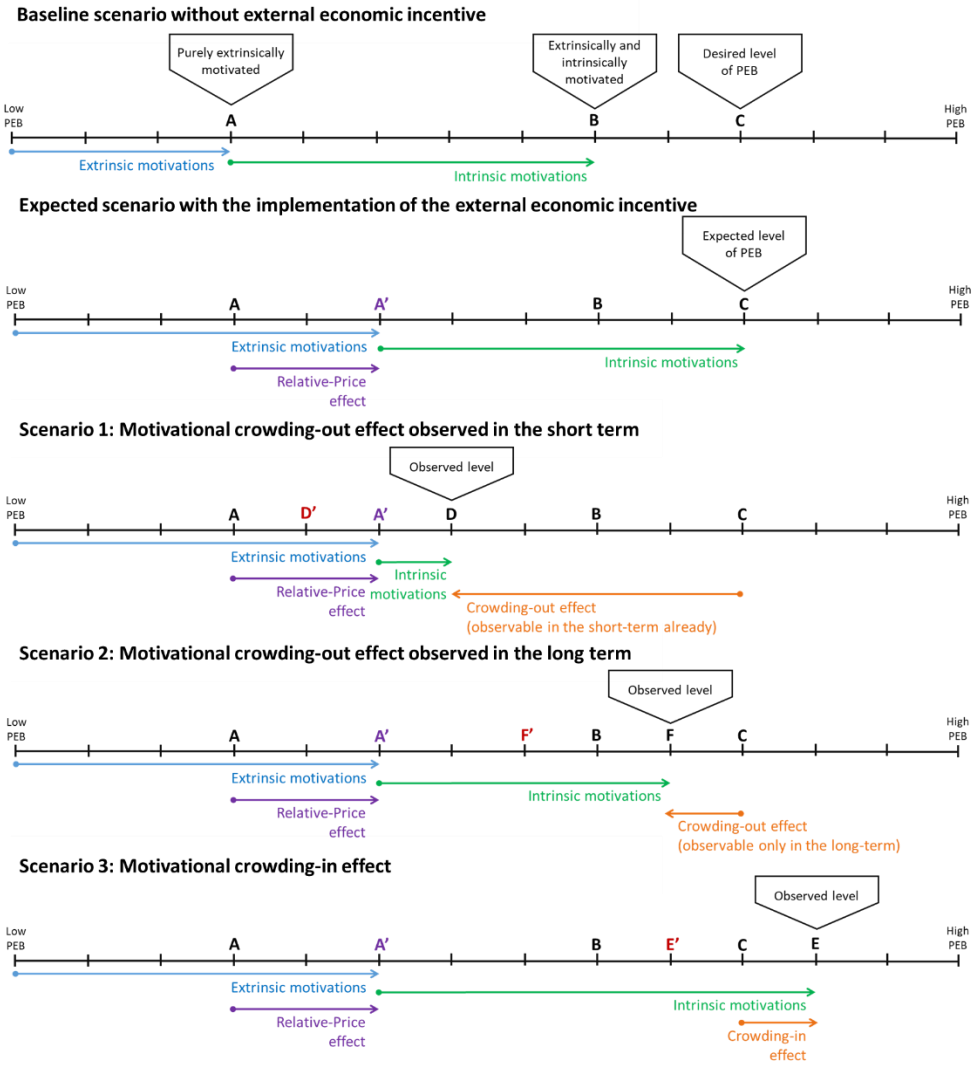

Figure 2. Types of motivational crowding effects by external economic incentives. The introduction of an external economic incentive is expected to increase PEB levels from the suboptimal baseline level B by increasing extrinsic motivations (blue arrows) from level A to level A' (purple arrows). The crowding effect of the intervention (orange arrows) on intrinsic motivations (green arrows) together with the resulting levels of PEB are presented in three possible scenarios. In Scenario1, a crowding-out effect is already observable during incentive implementation (i.e., in the short term) because observed PEB level is D<B. Conversely, in Scenario 2, a motivational crowding-out effect is only observable after the incentive is removed (i.e., in the long term) because PEB initially increases to level F>B but then goes back to level F'<B after the incentive is removed. Finally, a motivational crowding-in effect is represented in Scenario 3, in which PEB still increases to level E'>B after the incentive is removed.

Nevertheless, external incentives could crowd out intrinsic motivations while still having an effect in the desired direction in the short term. As shown in the scenario 2 of Figure 2, if the relative-price effect is larger than the crowding-out effect, the levels of PEB would actually increase relative to the baseline scenario, resulting in PEB level F>B. Still, once the external incentive is removed the relative-price effect vanishes and individuals engage in fewer PEBs than they did initially, resulting in PEB level F'<B. This long-term effect would be much more accentuated in Scenario 1, in which PEB would further decrease from level D to level D'<D<B once the external incentive is removed. Finally, if the external economic incentive actually enhances or crowds in intrinsic motivations, the sum of the relative price-effect and the crowding-in effect would lead to PEB levels that are beyond the expected social optimum (see the scenario 3 of Figure 2, where the resulting level is E>C). When external economic incentives enhance intrinsic motivations, they can also lead to more desirable behavior in the long term. Thus, PEB levels in this scenario would increase to level E'>B even after the incentive is removed.

Given this large number of possible outcomes and the great variability of psychological mechanisms through which economic incentives could crowd out intrinsic motivations, the challenge has been to identify the conditions that could prevent economic incentives to backfire. Experimental evidence has found, for instance, that when sanctions are public instead of private (d'Adda, 2011; Xiao and Houser, 2011; Lopez et al., 2012), long-lasting (Irlenbusch and Sliwka, 2005; Gachter et al., 2008) or endogenously defined (Tyran and Feld, 2006), they are less likely to crowd out intrinsic motivations. Similarly, rewards are less likely to crowd out intrinsic motivations when they are private instead of public (Ariely et al., 2009) or in-kind instead of cash (Heyman and Ariely, 2004). In that sense, experimental evidence shows that non-monetary factors of economic incentives—which economic theory often assumes to be irrelevant as they do not change prices and incomes—could be highly relevant to preventing crowding-out effects.

In what follows, I review the empirical evidence on motivational crowding effects in the specific context of PES and present my contributions to this literature.

1.3. Contributions to the literature on motivational crowding effects in PES

In the context of PES, the potential importance of non-monetary factors in motivating environmental conservation and PES participation has been highlighted in a few non-experimental case studies (e.g., Kosoy et al., 2007, 2008; Rico García-Amado et al., 2011; van Hecken and Bastiaensen, 2010). Nevertheless, a large proportion of the experimental literature has focused on studying the effect of design features related to the monetary elements of these programs. For instance, studies have compared different types of payments, such as low payment levels to high payment levels (e.g., Travers et al., 2011; Handberg and Angelsen, 2019), individual payments to collective payments (e.g., Kerr et al., 2012; Kolinjivadi et al., 2019; Midler et al., 2015; Moros et al., 2019; Narloch et al., 2012), flat payment rates to redistributive rates (e.g., based on opportunity cost or wealth levels; Vorlaufer et al., 2017) or action-oriented payments to results-oriented payments (e.g., Dörschner and Musshoff, 2015). Another major part of the experiments have focused on comparing the effects of a reward to those of other economic incentives such as fines, taxes or insurances (e.g., Vollan, 2008; Reichhuber et al., 2009; Travers et al., 2011; Salk et al., 2017; Kaczan et al., 2019; Moros et al., 2019). Fewer experimental studies have focused on non-monetary factors such as including public non-monetary rewards to increase social recognition (Maca-Millán et al., 2021), using different framings to justify the implementation of the PES policy (Lliso et al., 2021), partially ending the payments (e.g., due to budgetary constraints; Moros et al., 2020), or recognizing participants in the design or implementation process through, for example, the endogenous

definition of the payment level (Jack, 2009) or the distributive rule (e.g. Vollan, 2008; Travers et al., 2011; Moros et al., 2019). In this thesis, I contribute to the literature by studying the motivational crowding effects of other non-monetary factors that have been (largely) ignored by the experimental literature.

In addition to economic experiments, quasi-experimental analyses have been used for studying motivational crowding effects of PES. In some cases, PES participation appeared to have raised environmental awareness (e.g., Arriagada et al., 2018; Grillos et al., 2019), which suggests the crowding in of intrinsic motivations. In other cases, PES participation was associated with a crowding-out effect since it was found to reduce the acknowledgment of environmental values associated with environmental conservation (e.g., ecosystem services) while increasing the acknowledgment of monetary values (e.g. Rico García-Amado et al., 2013; Agrawal et al., 2015). However, quasi-experimental analyses typically measure changes in participants' statements about their environmental awareness and motivations for environmental conservation without being able to confirm whether or not these changes affect PEB. Furthermore, quasi-experimental analyses do not provide information on the effects after the incentive program ends because PES contracts typically remain in force at the time of analysis. Economic experiments, in contrast, allow the direct measurement of changes in decision-making, both while the policy is taking place and while the policy is removed. With some exceptions (e.g., Salk et al., 2017; Andersson et al., 2018; Kaczan et al., 2019; Kerr et al., 2019; Lliso et al., 2021; Maca-Millán et al., 2021), most of the economic experiments to date have only focused on the period in which the PES policy is taking place. Indeed, they compared a baseline period without external intervention to a treatment period in which the external policy is implemented, but did not include a final period in which the external intervention is removed. In this thesis, I also contribute to the literature by always including a third period without external intervention to estimate the effect of each studied non-monetary factor after the policy ends.

Finally, previous studies typically measure motivational crowding effects by comparing behavioral changes in a treatment period with external intervention relative to a baseline period without external intervention between different types of external interventions. Since PES often increases PEB during PES implementation (Ezzine-de-Blas et al., 2019), an external intervention that further increases behavioral levels compared to other interventions is then interpreted as an intervention that crowds in intrinsic motivations, while an intervention that does not increase (or only slightly increases) behavioral levels relative to others is interpreted as an intervention that crowds out intrinsic motivations. However, as I will show next, this relative comparison could confound the motivational crowding effect with the relative-price effect if the relative-price effect of each external intervention differs between treatments.

Let us assume a hypothetical scenario in which we want to compare the motivational crowding effect of *incentive A* to that of *incentive B* (see Figure 3 below). For simplicity, the baseline level of extrinsic and intrinsic motivation is assumed to be constant across interventions, resulting in a baseline level of PEB (i.e., level Z). Once implemented, both incentives increase PEB relative to this baseline level. However, *incentive B* does this more effectively since it increases PEB up to a higher level (level B) than the level achieved with *incentive A* (level A). Based on this result, we could conclude that it would be more advisable to use *incentive B* since *incentive A* crowds out intrinsic motivations relative to *incentive B*. Nonetheless, in the scenario in Figure 3 a), *incentive B* had a higher incentive level (and therefore a larger relative-price effect) relative to *incentive A*, and thus it would be more advisable to use *incentive A* because *incentive B* crowded out intrinsic motivations, while *incentive A* crowded them in. Incentive A could therefore have a more positive impact in the long term.

Despite the importance of considering the relative price-effect, previous experimental studies only tend to emphasize on cost-effectiveness comparability between incentives, i.e., they implemented incentives that are equivalent in terms of total conservation cost for the social planner (e.g., Narloch et al., 2012; Midler et al., 2015; Moros et al., 2019). However, this is no guarantee that incentives would also be comparable at the incentive level. For example, a collective payment in which individuals are equally paid based on group contributions to a collective fund will only be comparable in terms of total cost to an individual payment in which individuals are paid based on their own contributions to the collective fund, if the payment level per unit of contribution in the collective payment is lower than that of the individual payment. Furthermore, when using games that involve strategic considerations (e.g., public good games, common-pool resource games or trust games), the relative-price effect could also vary depending on each participant's beliefs about the behavior of other participants. In this thesis, I also contribute to the literature by keeping the level of incentive fixed between treatments on the one hand and using a dictator game that avoids the effect of strategic considerations on behavior on the other. As shown in Figure 3 b), having control over the relative-price effect allows to better disentangle the relative crowding effect between external interventions. It is important to clarify that by including a third period in which the external intervention ends it is also possible to disentangle the motivational crowding effect because the relative price effect disappears once the external intervention ends by definition. Nevertheless, by also keeping the relative-price effect fixed between treatments, it is possible to assess whether the short-term effect persists (or worsens) after the external intervention ends.

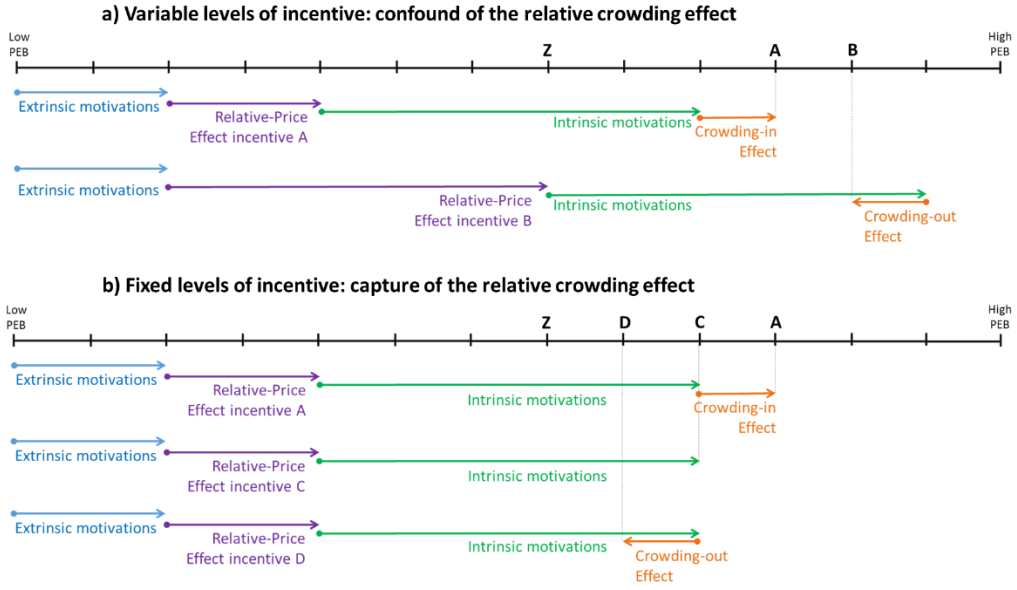

Figure 3.Relevance of controlling for incentive level when comparing the motivational crowding effects of different external economic incentives. At the top, a comparison between two incentives that vary in their incentive level (i.e. in their relative price effect). Incentive B is associated with a crowding-out effect while incentive A is associated with a crowding-in effect. However, because the relative price effect of both incentives differs, the relative crowding effect between incentives is confounded, and the observed levels of PEB wrongly suggest that incentive B could be more effective than incentive A. At the bottom, three incentives with the same level of incentive are compared. In this case, the relative comparison of PEB changes between incentives allows capturing the real crowding effect of each incentive.

In the next sections, I introduce the research questions addressed in each chapter of this thesis, followed by a description of the methodology I used to answer each of these research questions.

1.4. Research questions

The central interest of this thesis lies in studying how external economic incentives could enhance or erode intrinsic motivations for PEB. More specifically, I use lab-in-the-field experiments to investigate the potential of various non-monetary factors of external economic incentives to mitigate or enhance crowding-out effects. This investigation aims to contribute to the improvement of the design of incentive-based environmental policies, with a particular interest in the case of PES.

This aim is translated into three research questions (RQ), which are specified as follows. RQ1 focuses on evaluating the effect of framing manipulations. RQ2 involves studying the effect of varying the social distance that exists between ecosystem service providers and beneficiaries. Finally, RQ3 relates to the relevance of the institutional context in which an economic incentive is implemented.

In Chapter 2, I explore the first non-monetary factor: framing. Framing refers to the discourse used to communicate a policy and is not a neutral characteristic given that the choice of words could convey different interpretations toward targeted actions. For example, the act of paying for the provision of ecosystem services could suggest a focus on economic reasoning by viewing ecosystem services as commodities. As such, a reduction in the marketization in PES discourse could prevent the shift of an individual's focus from social to economic reasoning (Bowles, 2008). Furthermore, a conservation policy based solely on the provision of targeted marketable ecosystem services (e.g., water) could motivate the conservation of only what is shown to be useful (Klain et al., 2017). In that sense, raising environmental awareness about additional values of ecosystem services (e.g., spiritual values, climate regulation) could help mitigate or prevent motivational crowding-out effects (Arriagada et al., 2018; Grillos et al., 2019). Based on these considerations, I formulate the following research questions:

RQ1.1: Does changing the terms used to denote the payment affect whether and to what degree PES have a motivational crowding effect in the short or long term?

RQ1.2: Does emphasizing other values beyond targeted marketable ecosystem services to justify environmental conservation affects whether and to what degree PES have a motivational crowding effect in the short or long term?

In Chapter 3, I explore the second non-monetary factor: the identity of ecosystem services beneficiaries. One of the greatest challenges in watershed management relates to the heterogeneity of actors involved in this process. On top of this heterogeneity, the fact that watersheds often cover large areas crossing political and geographical boundaries reduces interaction between stakeholders and makes it difficult to create a sense of belonging. Still, a large body of literature shows that humans are found to consistently act more pro-socially towards those with whom they share a closer social identity relative to those who are more socially distant (Everett et al., 2015). Therefore, who benefits downstream could be very relevant for intrinsic motivations and policy effectiveness. To address this concern, I formulate the following research question:

RQ2: Does the size and direction of the short- or long-term motivational crowding effects of a PES policy depend on the geographical identity of ecosystem services beneficiaries? Are these effects proportional to the geographical distance between ecosystem services providers and beneficiaries?

Finally, in Chapter 4, I explore the last non-monetary factor: the institutional context in which an external economic incentive is implemented.

One of the mechanisms to increase the effectiveness of an external intervention is to target the intervention to specific areas or landowners. In the case of PES, the so-called additionality criterion recommends that PES programs target areas facing significant threats of environmental degradation (Wunder, 2005, 2007; Engel et al., 2008). However, such targeting strategies could result in equity concerns when they reinforce pre-existing inequalities or deny previous conservation efforts, affecting the effectiveness of the external intervention (Pascual et al., 2014; Alpizar et al., 2015, 2017). For instance, the results of Alpízar et al., (2017a, 2017b) suggest that when PES are targeted to those with the lowest record of environmental conservation, those with the highest record of environmental conservation could reduce their subsequent conservation behavior. In contrast, random selection or the rewarding of prosocial behavior does not result in the same behavioral spillover effect, suggesting that the rationale for exclusion is relevant due to fairness considerations. I hypothesize that the emergence of an unequal institutional context could also raise such fairness concerns because people generally care about their relative outcomes compared to those of others (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000). Therefore, in this chapter, I analyze whether facing a pecuniary incentive (i.e., relative-price effect) in a disadvantageous institutional context leads to a stronger crowding effect relative to facing the same pecuniary incentive in an egalitarian context. In particular, I focused on three policy scenarios that involve unequal policy conditions between two neighboring areas: 1) a high priority area where PES are implemented next to a low priority area that is excluded from PES, 2) a protected area with land-use restrictions surrounded by a buffer area where PES are implemented, and 3) a protected area where PES are implemented on top of land-use restrictions, surrounded by a buffer area with only PES. Thus, I formulate the following research question:

RQ3: Are short- or long-term motivational crowding effects in one area affected by whether the same or different policy conditions prevail in a neighboring area? Does the level of inequality between the policy conditions on each area affect the degree to which the above effect occurs?

1.5. Methodology

1.5.1. Experimental design and analytical approach

The main method used in this thesis to answer the research questions formulated in the previous section were economic experiments. In contrast to other methods like surveys, participants in economic experiments receive a financial incentive in relation to their own decisions. This induces participants to make decisions more carefully, thus providing more accurate and meaningful results (e.g., see Harrison and List, 2004). Furthermore, to increase the generalizability of my findings, I used a lab-in-the-field experiment that mimicked a real-world situation, which was presented to subjects for whom the decision-making process in the recreated situation was familiar and relevant to their daily lives. In particular, I designed a lab-in-the-field experiment that was framed within the context of a watershed, which I implemented to farmers living in upstream hydrologically rich areas.

Thus, I used a modified dictator game that simulated the negative externality produced by changes in upstream land use on downstream water availability and quality. More specifically, participating farmers were presented with a hypothetical farm with 8 hectares of upstream forest. Participating farmers had the opportunity to transform their forest to cattle farming, which was associated with more revenue per

hectare. However, for every hectare of cattle farming introduced, the water provided downstream by this hectare of forest was depleted. Hence, when deciding how much forest to conserve, farmers simultaneously decided how much water they provided downstream. Figure 4 summarizes the situation that was presented to farmers during the experiment.

Most of the previous studies looking at PES motivational crowding effects applied public goods or common-pool resource games. In section 1.3, I argued that these kinds of games could lead to confounding the motivational crowding effect with the relative-price effect. In addition to that, I suggest here that these kind of games may also not be the best way to represent the context behind PES, all the more in the case of watershed management. With few exemptions (e.g., Blanco et al., 2018), ecosystem services providers in public goods or common-pool resource games simultaneously act as the only beneficiaries. This oversimplifies the extent of the externality, since ecosystem services normally provide benefits for many members of society, while only a small subset of individuals contribute to their provision. In this sense, I consider that by using the previously described modified dictator game I better reflected the relationship between water providers and beneficiaries.

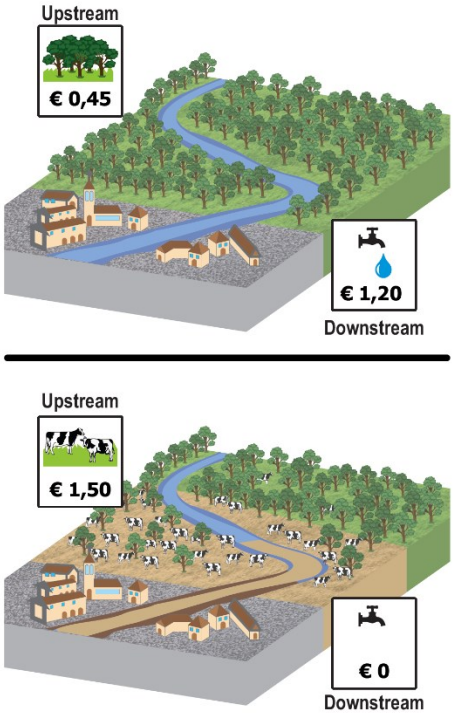

Figure 4. Graphical representation of the social dilemma described in the modified dictator game.

Economic experiments also allow researchers to establish a causal relationship between a treatment and the measured behavioral outcome because decision-making of participants is measured in a controlled environment. Having control over the environment makes it possible to manipulate the conditions presented to participants and to estimate the impact of this manipulation on the behavioral outcome. In general, economic experiments include two types of experimental designs: between-subjects design or within-subjects design. In a between-subjects design, the behavior of a group of subjects exposed to a control condition without manipulation is compared to the behavior of a group of subjects exposed to a

manipulated treatment condition. On the contrary, in a within-subjects design, a group of subjects is exposed to both, the control condition and the treatment condition, and the impact is measured by the changes in each subject's behavior.

In this thesis I used a mixed design that combined a within-subjects design with a between-subjects design. Thus, participants played three rounds $(R = 1,2,3)$ of the previously described modified dictator game, one of which was randomly chosen to be payoff relevant. In the first round, participants played a baseline setting without external intervention. Then, in the second round, the external economic incentive was introduced. Lastly, in the third round, the external economic incentive was removed and participants played again the initial baseline setting. This within-subjects aspect in the design allowed me to get an estimate of the short- and long-term effect of the external intervention on forest conservation at the individual level. However, since I did not always include an additional control group without external intervention, I could not assess the extent to which the effect of the incentive was associated with crowding in or crowding out. Nonetheless, my interest was in comparing the crowding effect associated with different non-monetary manipulations in the design or manipulation context of the external intervention, rather than measuring the overall crowding associated with the external manipulation. To achieve this, participants played all three rounds in a unique treatment group or control group to which they were randomly assigned. In each treatment group, a target non-monetary factor related to the design or implementation context of the external economic incentive was manipulated, while in each control group it was not. Thus, this between-subjects feature in the design allowed me to estimate the short- and long-term effect of the external economic incentive in the treatment group relative to those in the control group. In this sense, a larger increase—or a smaller decrease—in forest conservation levels in the treatment compared to the control group would indicate a *relative crowding-in effect*, while a smaller increase—or a greater decrease—in forest conservation levels in the treatment compared to the control group would indicate a *relative crowding-out effect*. This methodology for analysis is also known as a differences-in-differences (DD) approach.

A DD approach has the advantage that it eliminates the effect of any pre-existing differences in the measured behavioral outcome (in this case, the amount of hectares of forest conserved) between the treatment group and the control group. If for any reason, the treatment is correlated with the initial level of behavioral outcome (i.e., the levels of forest conservation are significantly higher or lower in the treatment group relative to the control group), the DD estimator would still be unbiased. However, the DD model does require that the temporal trend of the behavioral outcome would be the same in both the treatment group and the control group. This is known as the parallel trend assumption.

In summary, an estimate of the short- and long-term motivational crowding effects was calculated through the relative comparison of behavioral changes between rounds. Namely, in the short-term analysis I looked at the changes in forest conservation between the second round and the first round, while in the long-term analysis I looked at the changes between the third round and the first round. Then, in order to estimate the relative short- and long-term motivational crowding effect, I compared these changes between the treatment group and the control group.

1.5.2. Study area

This research was conducted with farmers in the municipality of Junín, located in the department of Cundinamarca in the center of Colombia. The municipality of Junín is part of the Guavio region, a region known to have a valuable hydrological richness which include the establishment of a hydroelectric plant

in the Guavio reservoir. The municipality of Junín stands out within the Guavio region because it has a significant percentage of its territory within primary forests and páramos, a mountainous wetland ecosystem critical for the provision of water in many Andean urban areas. Thus, Junín has a large variety of water bodies, distributed in lakes, lots of springs, rivers and streams that pour their waters into the Guavio reservoir. This makes this municipality a strategic area at the national level for its considerable importance for the country's energy supply.

Junín has a total surface of about 34,000 ha, 99% of which correspond to rural areas, and a total population of about 9,000 habitants, 68% of whom live in rural areas. The rural area is divided into 24 *veredas* (4 of which include areas with páramo ecosystem) and 3 inspections. I specifically worked with farmers from 6 of the 24 *veredas:* San Francisco (Chapters 2 and 3), San Antonio, Santa Bárbara and San Rafael (Chapter 4). The agricultural sector is the most important sector in the municipality, including crops of coffee, corn, yucca, potato, beans, sugarcane and blackberry. Nevertheless, livestock farming for meat and/or milk production is a widespread economic activity throughout the municipality, and the main source of income for many rural households.

Junín is also a strategic municipality at the regional and national level because the páramos in its territory are part of the eastern area of the complex of Páramos of Chingaza, a complex critical for the provision of water in Bogotá, the biggest and most populated city of Colombia. In order to secure water provision in Bogotá, many environmental programs, including PES, have been implemented in this páramo complex over the last two decades. However, most of these programs have focused on the western area of the complex, because until 2009, when a military base was located inside the páramo complex, access to the eastern area was limited by the civilian conflict in Colombia. This context offered the opportunity to analyze the effect of external interventions in areas not only of hydrological relevance but also little "contaminated" by previous experiences with external economic incentives. Moreover, given the current similarities between the economies of the eastern and western areas of the complex of Páramos de Chingaza, there was great potential for extrapolation of the results to other areas.

1.5.3. Data collection strategy

In order to reduce selection bias of participants and to have a representative sample of the study area, I, along with a field team, walked all the *veredas* listed in the previous section. We visited a total of 558 farmers, each from a different household, representing approximately 90-95% of the households living in each *vereda*. During this first visit we conducted a pre-experimental survey. Farmers were informed that at a later time they will be contacted again to participate in an additional activity (i.e., the economic experiment). In both visits, we obtained each farmer's written consent, ensuring their participation was voluntary.

1.5.3.1. Pre-experimental survey

In the pre-experimental survey we collected information on each participating farmer's socio-demographic characteristics, level of trust towards several institutions, and degree of collective action. We also asked about their environmental awareness, perceptions of environmental vulnerability, degree of pro-environmental actions, and motivations for conservation. Finally, we also elicited each farmer's degree of risk aversion and inequality aversion through three incentivized tasks.

In particular, to elicit risk aversion, we asked farmers to choose one of nine different gambles, using a similar procedure as in (Eckel and Grossman, 2008). To elicit disadvantageous inequality aversion, we used a strategy elicitation method to identify farmers' minimum-acceptable offer in an ultimatum game. Finally, to elicit advantageous inequality aversion, we again used the strategy method to identify the minimum egalitarian allocation (between each farmer and a passive recipient) that each farmer was willing to accept to waive their right to keep an initial endowment. Both elicitation tasks were based on Blanco et al. (2011). For the last two tasks, each farmer was matched with another anonymous farmer who was being interviewed in another area.

1.5.4. Economic experiments

As previously mentioned, each participating farmer took the role of a dictator in a modified dictator game. Considering that the decisions of each dictator farmer do not require any interaction with other farmers, the sessions in which the effect of framing or the effect of social distance was analyzed were conducted individually. In the case of the analysis of the effect of an unequal institutional context (Chapter 4), although there was similarly no interaction among dictator farmers, each treatment required the presence of other farmers to evoke an unequal institutional context through the implementation of different rules among farmers participating in the same session.

When the experiment was conducted individually (Chapters 2 and 3), the sample of farmers previously surveyed was divided into 10 groups based on farmers' gender and the sector of the *vereda* in which they lived. Then, in order to improve balance between treatments, a fixed proportion of farmers in each group was randomly assigned to each one of the treatments. When the experiment was conducted in groups (Chapter 4), each previously surveyed farmer was randomly assigned to one of the treatments. Later on, sessions were conducted on each *vereda* with a group of farmers who had been previously assigned to the same treatment.

The general instructions of the game, the consent form and the baseline decision form are presented in Appendix A.

The next three chapters present in detail each of the three research articles of this thesis. Finally, in the last chapter, the conclusions are summarized.

Chapter 2 Beyond a market discourse: Is framing a solution to avoid motivational crowding out in payments for ecosystem services?

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Summary

The implementation of payments for ecosystem services (PES) raises concerns about the effects of external economic incentives on intrinsic motivations. Their instrumental orientation could send an implicit signal that people have the right to degrade unless they get paid not to do so. Nevertheless, it has been suggested that features in the design and implementation of PES programs could help mediate motivational crowding if they can align PES goals with an individual's intrinsic motivations. In this paper, we investigate whether the discourse used to communicate the policy (i.e., the framing of the policy) could be a mediating feature. We used a lab-in-the-field experiment with farmers in Colombia to study the effects of two dimensions of framing on PES environmental effectiveness: 1) using different terms to denote the payment; 2) emphasizing different types of ecosystem services obtained from nature to motivate its conservation. Acknowledging forest conservation as an achievement resulted in better conservation outcomes relative to a control framing. Similarly, emphasizing the cultural ecosystem services obtained from forests could crowd in motivations for forest conservation relative to emphasizing only the targeted water service; however, the results are not conclusive. Our findings stress the importance of the framing used to communicate the PES policy, which could be used as a rather inexpensive tool to increase policy effectiveness and reduce motivational crowding-out.

2.1. Introduction

The increasing demand for ecosystem services (i.e., the benefits people obtain from ecosystems) due to accelerated economic and population growth threatens the ability of ecosystems to continuously provide these services. Although many people benefit from ecosystem services, individuals typically have insufficient incentives to maintain them because of their public good nature and the presence of external effects. In recent decades, payments for ecosystem services (PES) have been proposed as a solution to address ecosystem degradation through direct economic transfers to individuals or groups in exchange for ensuring the provision or maintenance of specific ecosystem services through environmental conservation or the implementation of sustainable practices. By providing external motivation for environmental conservation, PES are argued to motivate ecosystem services provision.

However, concerns have been raised regarding the effect that PES could have on intrinsic motivations, i.e., the motivations of those who inherently value environmental conservation (so-called motivational crowding; Rode et al., 2015; Ezzine-de-Blas et al., 2019). This is because the beneficiary-pays approach inherent in PES could send an implicit signal that people have the right to degrade the environment unless they get paid not to do so (Swallow et al., 2009; Kosoy and Corbera, 2010; Muradian et al., 2010). Since PES budgets are limited, long-term environmental conservation could be threatened if intrinsic

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motivations to conserve are replaced by monetary interests. When and how PES can affect intrinsic motivations remain unclear because empirical evidence on motivational crowding is mixed. Some PES case studies have found evidence intrinsic motivations being undermined (i.e., crowding out; van Hecken and Bastiaensen, 2010; Fisher, 2012; Rico García-Amado et al., 2013), while others have suggested that intrinsic motivations are enhanced (i.e., crowding-in; Arriagada et al., 2018; Bremer et al., 2014; Grillos, 2017; Grillos et al., 2019).

It has been proposed that features in the design and implementation of PES programs could mediate between an individual's intrinsic and extrinsic motivations, depending on the psychological response that they trigger (Ezzine-de-Blas et al., 2019). If the design and process of a policy align with the psychological moderators of the internal satisfaction generated in an individual when conserving the environment (i.e., need-satisfaction moderators), external incentives could be internalized (Ibid). Previous efforts have focused on studying the effect of design features related to the monetary elements of these programs, such as the type and amount of economic incentive or the distribution of payments (Kerr et al., 2012; Narloch et al., 2012; Midler et al., 2015; Salk et al., 2017; Andersson et al., 2018; Handberg and Angelsen, 2019; Moros et al., 2019; Kaczan et al., 2019; Kolinjivadi et al., 2019; Loft et al., 2020). In contrast, little attention has been paid to a relatively inexpensive tool that could help mediate motivational crowding: the discourse used to communicate the policy (i.e., the framing of the policy). In this study, we investigated whether the framing of a PES policy is a relevant feature in motivational crowding.

Framing is not a neutral characteristic of policy instruments since it can shift an individual's focus toward other reasoning processes (e.g., from social to economic reasoning; Bowles, 1998; Bowles and Polania-Reyes, 2012; Rode et al., 2015). In that sense, the marketization in PES discourse could evoke a market logic that leads to a crowding-out effect. Notably, concerns of this type have been raised in the literature. Currently, the discussion has moved to the terms used to define payments and how they could be linked to different perceptions of generalized entitlements. Broader labels have been proposed and used to refer to PES, such as "rewards" (van Noordwijk et al., 2004), "compensations" (Rosa et al., 2004), and "co-investments" (van Noordwijk and Leimona, 2010) for ecosystem services. However, all of these distinctions have largely been used to conceptualize different types of PES programs and few have attempted to measure the alleged effects of using different terms.

Furthermore, the framing of a PES policy goes beyond the terminology used to define the payments. The way that motivations and objectives behind PES implementation are exposed could also influence social norms by defining what is valuable, what is appropriate behavior, and how people relate to nature. A PES program with an instrumental orientation whose main focus is on the provision of targeted marketable ecosystem services (e.g., fresh water) could motivate the conservation of only what is shown to be useful or monetarily valuable (Klain et al., 2017), which can affect the maintenance of other ecosystem services that are not targeted by a program (Chan et al., 2012). This is particularly important for cultural services (e.g., spiritual values, cultural identities) because their inherently non-material and intangible dimensions make them difficult or controversial to value in monetary terms (Kumar et al., 2010; Kenter et al., 2014; Kenter, 2016). Therefore, the explicit inclusion of a broader set of ecosystem services within the motivations and objectives communicated as underlying PES implementation could serve an important role in motivating conservation. If such framing helps reinforce existing values and norms or create new ones, it could prevent the undermining of intrinsic motivations to conserve.

In this study, we explored whether the framing of a PES policy could mediate motivational crowding. Our main research question was: Does the way a PES policy is framed affect whether and to what degree motivation crowding occurs? In particular, we were interested in the effects of two dimensions of framing on PES environmental effectiveness: 1) using different terms to denote the payment (*payment framing*); 2) emphasizing different types of ecosystem services obtained from nature to motivate its conservation (*ES framing*). While prior literature on these issues has largely been conceptual or based on case studies, we contributed by conducting an experimental analysis that allowed us to establish causality based on real incentivized decisions. We implemented a lab-in-the-field experiment with farmers from the rural area of Junín—a Colombian municipality of enormous water wealth, where watershed protection clashes with its agricultural and cattle farming traditions. Our experimental design allowed us to study two different stages of motivational crowding: during policy and post-policy. Through the policy stage, we measured how the framing affected conservation decisions while the PES policy was in place. Through the post-policy stage, we measured whether the former effect persisted after the incentive was removed.

In what follows, we expand on the concept of framing and elaborate on how it is likely to influence cooperative behavior and motivational crowding in PES based on a brief literature review of existing empirical evidence. Section 3 describes the study area, the design of the game, and our research hypotheses. Section 4 presents the results. Finally, the discussion and conclusion are presented in the final section.

2.2. The influence of framing in shaping attitudes and behaviors

The term "framing" is applied to define two distinct processes: communication or thoughts (Druckman, 2001). Communication focuses on what a speaker communicates, while thoughts focus on what a spectator thinks, believes, or interprets. A speaker may affect the attitudes and behaviors of an audience by shaping or influencing their audience's thought frame by using different communication frames (Chong and Druckman, 2007). This process is typically known as a framing effect. In this paper, we focus on changes in the communication frame of a PES policy and study how these changes could influence participants' motivations for pro-environmental behavior. We will use the terms "frame" or "framing" interchangeably to refer to a frame in communication.

Dufwenberg et al. (2006) distinguishes between two types of framing frequently study in economic experiments: valence framing and label framing. Valence framing changes the reference point of the game by presenting the same critical information in either a positive (e.g., public good) or negative (e.g., public bad) frame. On the contrary, label framing (also known as context framing) does not change the logic of the game but only changes the wording of the instructions. Typically, under label framing, changes in the instructions are small (e.g., the title of the game, player labels) and focus on associating the game with different institutions or norms (e.g., "giving game" vs "keeping game"; Dreber et al., 2013) or on stressing specific aspects of the game (e.g., adding the sentence "Note that your recipient relies on you" in dictator games; Brañas-Garza, 2007). Our study was focused on label framing since we changed the framing of a PES policy between logically equivalent games.

2.2.1. Framing and cooperative behavior

Previous experimental research in economics and social psychology has shown that evoking a market discourse typically decreases cooperation levels (Elliott et al., 1998; Liberman et al., 2004; Dreber et al., 2013). However, the effectiveness of a particular framing depends on pre-existing individual profiles (e.g.,

social preferences, perceptions, norms, beliefs), which sometimes cause individual framing effects to cancel out on average (Park, 2000; Dufwenberg et al., 2006, 2011; Bouma and Ansink, 2013). Similarly, the use of environmental framings increases pro-environmental behavior (Gerlagh and van der Heijden, 2015). Individuals with higher levels of trust in environmental authorities, collective action, abilities to influence environmental decisions (Bouma and Ansink, 2013), as well as awareness of environmental conservation (Dufwenberg et al., 2006, 2011) and pro-environmental predispositions or values (Chong and Druckman, 2007) are more receptive to pro-environmental frames. Additionally, a frame can also mediate between social and monetary relationships. Shifting an individual's attention from social relationships to economic reasoning can lead to the crowding out of intrinsic motivations to cooperate (Rode et al., 2015). For example, mentioning the price of a gift rather than simply offering a gift shifts people from being altruistic to reciprocating according to the price of the gift (Heyman and Ariely, 2004).

2.2.2. Framing and PES

A few relevant studies relied on quasi-experimental evidence. Such studies often focused on values, beliefs, and norms as the outcome variables studied, which are commonly understood to be important precursors to motivations for pro-environmental behaviors (Stern, 1999; Dietz et al., 2005). Grillos et al.'s (2019) evaluation of a Bolivian PES program showed that when compared to control villages where contracts were never offered, highlighting pre-existing social norms when offering the program increased the prioritization of environmental values over instrumental values in target villages—even among households that chose not to enroll in the program. Similarly, supplementary activities that aim to increase environmental awareness and are often part of the design of PES programs (e.g., training and workshops on forest management) amplify perceptions about the relevance of other ecosystem services in addition to provisioning services (e.g., cultural and regulating services; Arriagada et al., 2018). Notably, this contributes to the emergence of more environmentally-friendly social norms (Bremer et al., 2014; Grillos, 2017). However, it is difficult to link reported changes in self-reported awareness and values with actual changes in behavior.

To the best of our knowledge, only three experimental studies have analyzed framing effects in the context of PES. Clot et al.'s (2017) survey experiment compared Madagascan students' perceptions toward variants of PES programs that only differed in the term used to define the payments. They found that the word "compensation" (rather than "payment") elicited higher favorability ratings. Nonetheless, unlike our study, the external validity was limited given that the survey was presented to students rather than farmers or the public. Similarly, the experimental survey was not incentivized, reducing the accuracy of results. More recently, Maca-Millán et al. (2021) implemented a threshold public good game in rural Colombia to assess the effect of a mood induction activity on PES effectiveness. The mood induction activity was conducted by an environmental psychologist and was expected to increase participants' environmental awareness. The mood induction activity did not increase PES effectiveness during PES intervention, but it increased the probability of full cooperation by nearly 13% after PES ended, relative to a control group with PES alone. More closely related to our study is the study of Lliso et al. (2021) on the effect of using different framings to justify the implementation of the PES policy on PES effectiveness. The authors implemented a somewhat similar modified dictator game in three different communities in Colombia. In their framing manipulation, participants were given a 5-min presentation about either the instrumental values or the relational values of the forest, followed by a group discussion. Their results indicated that a framing that is more adjusted to pre-existing human-nature relational models could be more effective in the long-term. In this study, we also contributed to this literature, by examining whether

a similar framing effect could be obtained through a shorter and more concise message, which could have a greater reach by being easier to incorporate into policy design.

2.3. Research design

Our research was conducted with small-scale cattle farmers from the *vereda*[2] San Francisco within the municipality of Junín in the Guavio province of Colombia. This province is known to have valuable hydrological richness since it supplies 12% of the country's electricity and 80% of the water used in the capital city of Bogotá (van der Hammen et al., 2015). During the summer season, water use conflicts between upstream and downstream users and between agricultural activities and human consumption have been reported in the province (Instituto Alexander von Humboldt, 2016).

We used a modified dictator game to simulate the negative externality produced by changes in upstream land use on downstream water availability and quality. In the game, each farmer was presented with a hypothetical farm with 8 hectares of upstream forest that they could transform into cattle farming at the expense of reducing water availability for an anonymous downstream beneficiary. Downstream beneficiaries were represented by real households from Bogotá. We explicitly framed it this way to resemble a realistic situation and to increase the salience of the experiment and thus the relevance of findings (Harrison and List, 2004).

Between December 2017 and January 2018, we walked the *vereda* along with local leaders who guided us and supported our goal of having a representative sample. We visited a total of 164 farmers, each from a different household. During each visit, we conducted a survey to collect information on sociodemographic characteristics, environmental awareness, and attitudes. We also asked farmers to list the pro-environmental actions they have performed in the past year and select from a list the reasons why they performed those actions. Available options were not mutually exclusive and included cost-effectiveness reasons (e.g., reduce expenses), moral reasons (e.g., ensure the well-being of future generations), and pro-environmental reasons (e.g., reduce climate change effects). Finally, we elicited farmers' fairness preferences in terms of an incentivized inequality aversion measure[3].

Farmers were informed that they would be visited again at a later time to complete another survey (i.e., the economic experiment). We stratified our sample in terms of gender, age, and *vereda* sector and then randomly assigned each stratified group to one of our treatments. Between February and March 2018, we revisited each farmer to deploy our experiment. In both visits, we obtained each farmer's written consent before deploying the survey or experiment to ensure that their participation was voluntary (see

[2] A *vereda* is a territorial administrative subdivision of the rural area of municipalities in Colombia.

[3] To elicit inequality aversion, we replicated Blanco et al.'s (2011) incentivized elicitation tasks for disadvantageous inequality (α) and advantageous inequality (β). Specifically, to elicit disadvantageous inequality, we used an ultimatum game in which the minimum acceptable offer is taken as an estimate of α_i. To elicit advantageous inequality, we used a modified dictator game in which β_i is estimated based on the egalitarian allocation that makes the dictator indifferent between keeping all their endowment and the egalitarian allocation. During this part of the survey, each farmer was randomly matched with another anonymous farmer who was being interviewed in another location. First, the farmers informed their decisions in all possible scenarios (i.e., we used a strategy-elicitation method). The interviewers then mediated between each pair of farmers by reporting the decisions and corresponding outcomes/payoffs via radio without revealing the identity of the farmers. Overall, 66% of the farmers in our sample were averse to disadvantageous inequality to some degree ($\alpha>0$), while 52% were completely averse to advantageous inequality ($\beta=1$).

Appendix A). Approximately 4% of the farmers (n=7) refused to continue with the experiment during our second visit. Thus, we obtained a total of 157 completed experimental sessions.

2.2.1. Experimental design

In the experiment, each farmer was presented with a hypothetical farm with 8 hectares of upstream forest. Farmers had to decide how to allocate this land between forest conservation and the introduction of cattle farming. Conserving the forest provided fresh water for households living downstream. However, each hectare with cattle farming was associated with more revenue from the commercialization of milk and meat. However, this implied losing the positive externality downstream.

For each participating farmer, one household from the neighborhood *Rincón de Bolonia* in Bogotá was randomly selected as a downstream water recipient. The water that was provided downstream by the participating farmer was translated into an actual credit to the water bill of this downstream beneficiary household. Participating farmers were aware of this procedure and had access to a signed letter confirming the agreement between a representative from Osnabrück University and a leader of the recipient neighborhood.

The experiment consisted of three rounds, one of which was randomly chosen to be payoff-relevant. In the first round, farmers played the baseline setting and privately decided their preferred combination of forest and cattle farming by marking their decision on a paper form that visually illustrated all possible scenarios and payoffs for both the upstream farmer and downstream household (see Appendix A). In the second round, the PES policy was introduced as a mechanism to increase water provision downstream. Farmers again had to decide how to allocate a new set of 8 hectares of farmland between forest and cattle farming. However, in this round, each farmer received a payment from the environmental authority for each hectare of forest they decided to conserve. Since in reality many PES programs do not fully cover opportunity costs (Kosoy et al., 2007; Bennett, 2008; Namirembe et al., 2014; Bétrisey et al., 2018; Chervier et al., 2019), the payment level was set below it. Finally, in the third round, the game reverted to the initial setting without the PES policy. Figure 5 summarizes our experimental design.

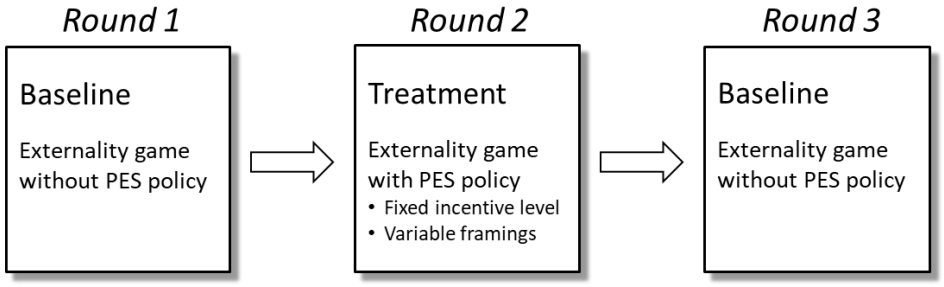

Figure 5. Overview of the experimental design.

In summary, each farmer (i) was endowed with 8 hectares of upstream forest (L) associated with a marginal return (f) for each farmer and a marginal water benefit (w) for a randomly assigned downstream beneficiary household (j). Farmers had to decide how to allocate their land between forest conservation (x) and cattle farming $(L - x)$. Each hectare with cattle farming produced zero water benefits downstream but yielded constant benefits (c) that were higher than the marginal return of conserving the forest $(c > f)$. In the second round, the opportunity cost of conserving the forest was

partially offset by a PES policy that offered a payment per hectare of forest conserved (P). Thus, the allocation decision of each participating farmer resulted in the following payoff functions: one for each farmer (π_i) and a second one for each randomly assigned downstream beneficiary household (π_j):

$$\pi_i = (f + P)x_i + c(L - x_i)$$

$$\pi_j = wx_i$$

where $c > f + P$, $P = 0$ for the first and third rounds and $P > 0$ for the second round. Moreover, $f + w > c$, meaning that forest conservation is socially optimal.

The ratio of marginal benefits between forest conservation and cattle farming (f/c) was set based on the percentage of self-consumption participation in the economy of family farming production in Colombia as a benchmark. According to the Ministry of Agriculture and Rural Development of Colombia self-consumption represents 32.6% of the agricultural production of a family, on average (Resolution 464 of 2017). Therefore, we fixed the marginal return from forest conservation at 30% of the marginal return from cattle farming $(f = 0.3\ c)$.

In this game, we referred to money directly instead of using points to ease the understanding of the game. The parameters were established so that the average payoff covered the daily minimum wage in the area, which was 8€ (see Table 1). Thus, a hectare of farmland in cattle farming implied 1,50€ in farm value (c) but zero water benefits downstream, while a hectare of conserved forest implied 0,45€ in farm value (f) but 1,20€ in benefits downstream (w), for a total social benefit of 1,65€ $(f + w)$.

Table 1. Overview of parameters and payoffs.

Round	c	f	P	w	Individual best strategy $(x_i = 0)$			Social optimum $(x_i = L = 8)$		
					π_i	π_j	$\pi_i + \pi_j$	π_i	π_j	$\pi_i + \pi_j$
1, 3	1,50 €	0,45 €	0 €	1,20 €	12,00 €	0 €	12,00 €	3,60 €	9,60 €	13,20 €
2	1,50 €	0,45 €	0,45 €	1,20 €	12,00 €	0 €	12,00 €	7,20 €	9,60 €	16,80 €

Note: c=marginal return of cattle farming, f=marginal return of forest conservation, P=payment per hectare, w=marginal return of downstream water, i=upstream participant, j=downstream participant, x=units of cattle farming, π=payoffs.

We implemented five different treatments that were identical in the structure and level of the incentives described above. The first treatment without any framing manipulation was run as a control. In this control treatment, the instructions referred to a payment for forest conservation, which was said to provide downstream fresh water. In the remaining four treatments, one of two dimensions within the framing of the PES policy was manipulated: *payment framing* or *ES framing*. On the one hand, *payment framing* changed the discourse used to communicate the payments. Specifically, the *reward* treatment, referred to the "payment" as a "prize for pro-environmental behavior through forest conservation", while the *compensation* treatment referred to a "compensation for loss and effort incurred during forest conservation". On the other hand, *ES framing* stressed some of the additional ecosystem services that people obtain from forests beyond downstream water provision, thus highlighting the importance of conserving forests and implementing the PES program. Specifically, the *cultural-ES* treatment focused on

cultural services, while the *regulating-ES* treatment focused on further regulating services (see Appendix A for the exact wording). Table 2 provides a summary of our treatments.

Table 2. Overview of treatments.

Name of treatment	**Description of framing used to communicate the PES policy**		Number of participants
	ES framing: which ecosystem services do communities obtain from forests?	**Payment framing**: how is the payment communicated?	
Control	Regulating downstream fresh water provision	*Payment* for forest conservation	30
Reward	Regulating downstream fresh water provision	*Prize* for pro-environmental behavior through forest conservation	30
Compensation	Regulating downstream fresh water provision	*Compensation* for loss and effort incurred from forest conservation	31
Cultural-ES	Regulating downstream fresh water provision *+ Cultural services* (welfare of future generations, cultural heritage, spiritual values)	*Payment* for forest conservation	34
Regulation-ES	Regulating downstream fresh water provision *+ Further regulating services* (climate and water regulation, air quality maintenance)	*Payment* for forest conservation	32

2.2.2. Analytical approach and research hypotheses

Based on Frey and Stutzer (2006), we assumed that the PES policy would affect each farmer's decision to conserve the forest through two mechanisms: the relative price effect on the marginal cost of conserving the forest and the motivational crowding effect on the marginal utility of conserving the forest. The relative price effect is always positive because the payment of a monetary incentive reduces the cost of conserving the forest, whereas the motivational crowding effect could go either way—either bolstering or undermining the marginal utility of conserving the forest. In this study, we measured which frames result in better conservation outcomes relative to a control framing. To achieve this, we used a difference-in-differences approach, for which we compared our treatments with the control group. More specifically, between each treatment and the control, we compared forest conservation levels between rounds 2 and 1 and between rounds 3 and 1. Since our framing manipulations did not vary incentive level, the relative-price effect of the incentive was constant across treatments. Therefore, in our comparisons, we could isolate the effect of each framing manipulation from the direct effect of the incentive. However, since we did not include an additional control group without PES, we could not assess the extent to which the direct effect of the incentive was associated with crowding in or crowding out. Thus, our focus was to measure the crowding effect associated with each framing manipulation relative to a control framing, rather than measuring the overall crowding associated with the PES policy in each treatment. In this sense, a larger

increase—or a smaller decrease—in forest conservation levels in the treatment compared to the control group would indicate a relative crowding-in effect, while a smaller increase—or a greater decrease—in forest conservation levels in the treatment compared to the control group would indicate a relative crowding-out effect.

To guide our analysis, we formulated the following research hypotheses:

Payment framing

We contend that the choice of words could convey a specific objective and meaning, while also building specific expectations for farmers. By stating that the environmental authority is going to pay farmers for environmental conservation, the payment monetizes forest conservation and suggests a focus on economic reasoning. In contrast, by saying that the environmental authority compensates for a loss or rewards positive behavior, the payment is acknowledging the losses (compensation) or achievements (reward) behind forest conservation rather than setting a price on forest conservation. Therefore, we expected the following:

- H1 (*payment framing, policy stage*): A PES policy that frames the payment as either a "compensation for the loss and effort incurred during forest conservation" or as a "prize for through forest conservation" will crowd in average forest conservation levels at the policy stage relative to a PES policy that frames it as a "payment for conserving the forest."

Additionally, payment framing can also affect motivations once payments are removed. The fact that the phrase "compensation for the loss and effort incurred during forest conservation" recognizes and emphasizes that the costs associated with forest conservation could—in the absence of actions to reduce associated costs—lead farmers to demand compensation in the future. On the contrary, we expected that the phrase "prize for pro-environmental behavior through forest conservation" would increase farmers' pride and satisfaction about conserving forests without increasing their demand for future rewards. Therefore, we developed the following hypotheses:

- H2a (*reward framing, post-policy stage*): A PES policy that frames the payment as a "prize for pro-environmental behavior through forest conservation" will crowd in average forest conservation levels in the post-policy stage relative to a PES policy that frames it as a "payment for conserving the forest."

- H2b (*compensation framing, post-policy stage*): A PES policy that frames the payment as a "compensation for the loss and effort incurred during forest conservation" will not crowd in average forest conservation levels in the post-policy stage relative to a PES policy that frames it as a "payment for conserving the forest."

ES framing

Empirical evidence suggests that increasing environmental awareness about the different values of ecosystem services could mitigate or prevent motivational crowding-out effects (Arriagada et al., 2018; Grillos et al., 2019; Lliso et al., 2021; Maca-Millán et al., 2021). Therefore, we expect:

- H3 (*ES framing, policy stage*): A PES policy that—beyond the target regulating service—motivates forest conservation by emphasizing cultural or further regulating ecosystem services obtained

from forests will crowd in average forest conservation levels at the policy stage relative to a PES policy that only emphasizes downstream fresh water provision.

- H4 (*ES framing, post-policy stage*): A PES policy that—beyond the target regulating service—motivates forest conservation by emphasizing cultural or further regulating ecosystem services obtained from forests will crowd-in average forest conservation levels in the post-policy stage relative to a PES policy that only emphasizes downstream fresh water provision.

2.3. Results

2.3.1. Descriptive statistics and randomization check

We first present descriptive statistics from our sample. Cattle farming was the main source of income for approximately 89% of the farmers who participated in the experiments (n=139). Although approximately 13% (n=20) had heard of PES programs, none of them reported participating in one. Farmers' most frequently mentioned reasons to engage in pro-environmental actions were pro-environmental (64%, n=100), followed by cost-effectiveness (50%, n=79) and morality (39%, n=61). All descriptive statistics for participating farmers are reported in Appendix B.

Before measuring the effect of our framing manipulations on farmers' behavior, we first checked whether the treatment groups were comparable to the control group (see Appendix C). We used logistic regressions to test for the balance of three sets of observed covariates: environmental awareness and attitudes, sociodemographic characteristics, and fairness preferences regarding inequality aversion. We found no statistically significant differences between the control and each treatment on environmental awareness and attitudes. However, we found statistically significant differences between the control and the *compensation* treatment in terms of sociodemographic characteristics. Farmers in the *compensation* treatment had smaller farms with more cattle and fewer crops. Since these variables are related to the general setting of the game and could be associated with baseline forest conservation differences, we controlled for these variables in the regression analysis of this treatment. In terms of inequality aversion, farmers from all treatment groups were significantly less averse to disadvantageous inequality in comparison to the control group. Additionally, farmers in the *regulation-ES* treatment were also significantly less averse to advantageous inequality than those in the control group. Therefore, we controlled for inequality aversion in the regression analysis of all treatments.

Finally, we used Fisher's exact tests to evaluate whether each treatment was comparable to the control in terms of reported motivations for pro-environmental actions (see Appendix C for details). We found that the treatment and control groups were comparable in terms of the proportion of farmers that reported moral motives. However, we found that a smaller proportion of farmers in the *reward* treatment reported pro-environmental motives when compared to the control. Since pro-environmental motives could be related to differences in baseline forest conservation levels, we controlled for environmental motives in the regression analysis of this treatment.

2.3.2. Data analysis

Approximately 90% of participating farmers (n=141) kept at least one unit of forest in the first round (baseline), which suggests that most of them had social preferences to conserve forests. The average giving in dictator games is approximately 28% of endowment (for meta-analysis, see Engel, 2011). Notably,

the baseline average giving in our experiment was approximately 20% higher. On average, 3.8 units of land were allocated to the forest during the baseline (s.d. = 2.2) and the median allocation to forests was 4 units.

The upper panel of Figure 6 summarizes the average units of land allocated to forest per round and treatment. With the introduction of the incentive in round 2, average forest conservation levels significantly increased in comparison to the baseline for all treatments except for the *compensation* treatment (Wilcoxon matched-pairs signed-rank, control p-value=0.025, *cultural-ES* p-value=0.001, *regulating-ES* p-value=0.023, *reward* p-value=0.005, *compensation* p-value=0.459). This increment was nearly 1 hectare for the *reward* treatment (0.933 ha), which was followed by the *cultural-ES* (0.853 ha) and *regulating-ES* (0.781 ha) treatments. In the control group, the increment was nearly 0.5 hectares (0.467 ha).

Once the incentive was removed (round 3), average forest conservation levels in the control group decreased by 0.267 hectares in comparison to the baseline. This decrease was not statistically significant (Wilcoxon matched-pair signed-rank, p-value=0.233). In the treatment groups, the removal of the incentive resulted in an increase of average forest conservation levels for the *cultural-ES* and *reward* treatments and a decrease in average forest conservation levels for the *regulating-ES* and *compensation* treatments. However, this change was only statistically significant for the *reward* treatment (Wilcoxon matched-pairs signed-rank, *cultural-ES* p-value=0.293, *regulating-ES* p-value=0.376, *reward* p-value=0.077, *compensation* p-value=0.290).

To test our hypotheses about the relative crowding effects associated with the framing manipulations, while both the PES policy was being implemented (i.e., policy stage, H1, and H3) and after it was removed (i.e., post-policy stage, H2a, H2b, and H4), we first compared the sizes of the average changes in rounds 2 and 3 relative to round 1. The bottom panel of Figure 6 compares, between each treatment and the control, the changes in forest area between rounds. The policy stage estimates show the change in forest area (ΔForest area) between round 2 and round 1, while the post-policy stage estimates show the change in forest area between round 3 and round 1. The difference between treatment and control (i.e., the relative crowding effect) is shown as yellow bars in Figure 6.

*Figure 6. Relative motivational crowding effects during the policy and post-policy stages associated with one of two dimensions of framing manipulation: ES framing (left) or payment framing (right). Top graphs summarize the average units of land allocated to forest per round and treatment (green bars for each treatment group, blue bars for the control group). Bottom graphs represent the changes in forest area within treatments (green bars) between round 2 and round 1 (policy stage) and between round 3 and round 1 (post-policy stage) and compare them to the control group (blue bars). Yellow bars measure the difference between these changes when comparing treatment to control groups, which can be interpreted as the relative motivational crowding effect associated with the framing manipulation. For example, the change in forest area during the policy stage was 0.467 ha in the control group and 0.933 ha in the reward treatment. Thus, the difference was 0.933 – 0.467 = 0.466 ha. Error bars represent the 95% confidence interval. Asterisks imply statistically significant differences based on a Mann-Whitney-U test (*** p < 0.01, ** p < 0.05, * p < 0.1).*

Given the panel structure of our data, we completed this statistical analysis using an econometric one. This allowed us to control for the imbalance detected in some of the covariates (see Section 4.1). We used a difference-in-differences (DD) approach to compare the units of land allocated to forest between each treatment and the control. Since the share of land allocated to forest was censored at 0 and 8 hectares, we used a random effects Tobit model for each treatment. Each model included two dummy variables to represent the policy and post-policy stages and another dummy variable to represent the treatment group. For instance, for the analysis of the cultural ES framing effect, the variable treatment was equal to 1 for farmers in the cultural ES treatment group and 0 for farmers in the control group. We interacted the

treatment variable with each stage dummy to determine the additional effect of each framing manipulation on forest conservation at the policy and post-policy stages. The four resulting regressions are presented in Table 3.

Table 3. Random effects Tobit estimates of DD models per framing manipulation.

Dependent variable: Forest area	Payment framing				ES framing			
	Reward		Compensation		Cultural-ES		Regulating-ES	
Treatment	-0.532	(0.740)	1.530**	(0.711)	0.679	(0.668)	0.110	(0.675)
Policy	0.598**	(0.277)	0.601*	(0.309)	0.588**	(0.254)	0.601**	(0.302)
Policy x Treatment	0.473	(0.388)	-0.627	(0.427)	0.364	(0.346)	0.446	(0.424)
Post-policy	-0.289	(0.278)	-0.288	(0.310)	-0.289	(0.256)	-0.290	(0.304)
Post-policy x Treatment	0.686*	(0.390)	-0.185	(0.426)	0.474	(0.347)	0.304	(0.426)
Constant	3.524***	(0.819)	4.123***	(0.793)	3.704***	(0.608)	1.859*	(1.123)
Control variables	YES		YES		YES		YES	
Observations	180		183		189		180	
Number of id	60		61		63		60	
Prob > chi2	0.000		0.005		0.000		0.000	

Note: Control variables included fairness preferences regarding inequality aversion in the analysis of all treatments, environmental motives in the analysis of the reward treatment, and some sociodemographic information in the analysis of the compensation treatment (see Appendix D for the full version showing all coefficients). Standard errors in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1.

We first examined the effect of the payment framing (H1, H2a, and H2b). Our results show that depending on the terms used to denote the payments, the PES policy could be more or less effective. Figure 6 indicates that in the policy stage, a reward framing increased average forest conservation levels compared to the control by nearly half a hectare (0.466) while a compensation framing reduced forest conservation levels by nearly half a hectare (-0.499). However, these average changes were not statistically significant (Mann-Whitney U test, p-value reward=0.195, p-value compensation=0.174). Once the PES policy was removed, the two observed trends persisted. On the one hand, a reward framing significantly increased average forest conservation levels compared to the control by more than half a hectare (0.600) in the post-policy stage (Mann-Whitney U test, p-value=0.019), suggesting a relative crowding in of forest conservation. On the other hand, a compensation framing reduced the average forest area by 0.185 hectares when compared to the control, suggesting a tendency to relatively crowd out forest conservation. However, this decrease was not statistically significant (Mann-Whitney U test, p-value=0.437).

These results were confirmed by the regression analysis. Table 3 shows that the coefficient for the interaction between treatment and policy was positive for the *reward* treatment and negative for the *compensation* treatment; however, both of them were not statistically significant. Similarly, the coefficient for the interaction between treatment and post-policy was positive and statistically significant

for the *reward* treatment, while the coefficient was negative and non-statistically significant in the *compensation* treatment. Therefore, our results reject H1 and support H2a and H2b.

We continue with the analysis of the effect of ES framing (H3 and H4). The PES policy tends to be more effective when stressing the ecosystem services obtained from forests in addition to downstream water provision. Figure 6 shows that average forest conservation levels increased by 0.386 hectares for the *cultural-ES* treatment and 0.314 hectares for the *regulating-ES* treatment when compared to the control (see Figure 6). However, these increments were not statistically significant (Mann-Whitney U test, p-value *cultural ES*=0.140, p-value *regulating ES*=0.385). Again, the trend toward increasing average levels of forest conservation observed in the policy stage persisted after the PES policy was removed. The effect was greater if the framing emphasized cultural ecosystem services. In the *cultural-ES* treatment, we observed a relative crowding-in effect that significantly increased the level of forest conservation by nearly half a hectare (0.443) with respect to the control (Mann-Whitney U test, p-value=0.058). However, the regression analysis did not confirm this result (see Table 3). In the *regulating-ES* treatment, the relative crowding-in effect resulted in a non-statistically significant increment of 0.205 hectares of forest when compared to the control (Mann-Whitney U test, p-value=0.455). Table 3 shows that although the coefficients for the interaction between treatment and policy—as well as those between treatment and post-policy—were positive for both ES framing treatments, they were not statistically significant. Thus, our results reject H3 and only partially support H4 for the *cultural-ES* treatment.

2.4. Discussion and conclusion

This paper used a lab-in-the-field experiment to study the relationship between motivational crowding effects and the framing of a PES policy in the context of forest water services. We compared decisions regarding forest conservation between treatments with equivalent payment levels but differing in terms of the framing used to describe the PES policy. In particular, we studied the following: 1) the effect of using different terms to denote the payments (*payment framing*); 2) the effect of stressing some of the ecosystem services that people obtain from forests in addition to the water services targeted (*ES framing*). The field experiment involved 157 farmers from a municipality in Colombia that is highly relevant for the provision of water in the country. In summary, our findings show that the effectiveness of the incentive under all framing manipulations did not significantly differ from that of the control group. Once the incentive was removed, a payment being framed as a prize that acknowledges forest conservation as an achievement (*reward framing*) was associated with the crowding in of forest conservation relative to the control framing. In contrast, and as expected, framing PES as compensation did not show a significant post-policy difference from framing it as payment. Moreover, we found weak evidence that an emphasis on the cultural ecosystem services obtained from forests (*cultural-ES framing*) could also induce a crowding-in effect when compared to emphasizing only the targeted water services.

The observed relative crowding-in effect of using a reward framing could be explained by an increment in farmers' feelings of competence due to the recognition of their ability to conserve forests (Ezzine-de-Blas et al., 2019). Payments that are viewed as a recognition of effort and know-how have indeed been found to be key motivators for conservation in previous empirical studies (Bose et al., 2019; van Hecken and Bastiaensen, 2010; Kolinjivadi et al., 2019; Maca-Millán et al., 2021). Simultaneously, the suggestive evidence of a relative crowding-in effect from a framing that emphasizes the cultural services obtained from nature could be the product of greater awareness regarding the benefits provided by nature. Previous studies have shown that PES programs that reinforce pre-existing human-nature relationships

have the potential to motivate pro-environmental behaviors (Grillos et al., 2019; Lliso et al., 2021). In this sense, environmental relatedness (e.g., awareness of the links between a landscape and one's cultural values) has been suggested as a moderator of the crowding in of intrinsic motivations (Ezzine-de-Blas et al., 2019). As an exploratory analysis, we used the information collected during the pre-experimental survey about farmers' reasons to engage in pro-environmental actions to test if the ES framing manipulations performed better in farmers whose reasons for engaging in pro-environmental actions were more aligned to the framing manipulation (see Appendix E). In line with the study of Lliso et al. (2021), we found suggestive evidence of the cultural framing having a stronger effect on farmers who reported moral reasons for taking pro-environmental actions, without any detrimental effect in the behavior of those motivated by other reasons not aligned with the cultural framing. This could have relevant implications for policy design, since it would facilitate the implementation of a specific framing without requiring the characterization of the ways in which each participant relates to nature. However, further evidence is needed to understand whether and how environmental relatedness could moderate the effect of framing.

Although we did not find statistically significant differences in the effectiveness of the incentive between other framing manipulations and the control, it is important to note that—unlike in all other treatments—the PES policy under the compensation framing was ineffective overall: once introduced, it did not increase the average levels of forest conservation relative to the baseline round. Therefore, our results contradict Clot et al.'s (2017) suggestion of the word "compensation" being more favorably received than the word "payment," which is in line with the authors' fears concerning the external validity of using a non-incentivized measure with students. A possible explanation for this result lies in the fact that the incentive did not fully cover the opportunity cost of conservation in our experiment. The existence of fair and just relationships between individuals and society has also been suggested to moderate motivational crowding (Ezzine-de-Blas et al., 2019). Moreover, the promise of "compensation" could be favorably received because it is perceived as fair since it recognizes the costs associated with conserving the environment. However, offering too low levels of compensation could also be perceived as an offense that could lead to the reversal of an initially favorable perception (Handberg and Angelsen, 2019). Payment distributions that do not recognize the heterogeneity of opportunity costs and wealth have also been found to predict motivational crowding out (Chervier et al., 2019; Rico García-Amado et al., 2011). Despite farmers stating that the incentive seemed fair to them because it reduced their losses during post-experimental discussions with the community, a more rigorous analysis is required to infer any conclusions. Therefore, investigating the interaction between the level of the incentive and the policy framing could be another promising extension of the present work.

A clear limitation of our study is the fact that our sample size was small. Previously measured framing effects in the context of PES were small when they only included subtle framing manipulations (e.g., Clot et al., 2017). Moreover, the fact that our experiment represented such a familiar decision setting to participating farmers could have make them less susceptible to small framing manipulations. Compared to an abstract context, an environmental context has previously led to more anchoring of decisions based on off-game experiences and beliefs (Gerlagh and van der Heijden, 2015). In our sample, 35% of participating farmers (n = 55) did not change their decisions once the incentive was offered, while 26% (n = 41) did not change their decisions throughout the game. The influence of the general setting on farmers' decisions was later confirmed in post-experimental discussions with the community. In general, farmers considered it important to have a balance between forest conservation and cattle farming. While they

were in favor of the implementation of incentives, they were emphatic in saying that “in no way were they going to stop raising livestock completely since it is part of their culture and well-being.” Despite the existing limitations, our results still confirmed a mild association between some of the framing manipulations, which highlights the importance of considering the framing to be used when communicating a PES policy. Therefore, we consider our results to be generally supportive of the potential of framing as a feature in the design and process of a PES program that could mediate the motivational crowding effect associated with the use of external economic incentives. Despite this, we call for caution in assuming that a single framing would fit all contexts since these and other framing manipulations could have different impacts on different people. We further encourage pilot studies to be performed *in situ* to identify the most beneficial framing to be used.

In conclusion, this paper stresses the importance of considering the framing used to communicate a PES policy. The use of a specific frame could either reinforce or harm policy objectives by interacting with local social norms and values. In light of time and budget constraints, framing could be a relatively inexpensive tool for increasing the effectiveness of a policy and mitigating the potential motivational crowding effects associated with the use of monetary incentives.

Chapter 3 Who is benefiting downstream? Experimental evidence on the relevance of upstream-downstream geographic distance for water provision

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Summary

One of the greatest challenges of water management in watersheds arises from the asymmetry in water appropriation: people upstream always have first access to water supplies and their decisions affect downstream users. Payments for watershed services (PWS) aim to incentivize water provision upstream by directly paying upstream land users for the water services they provide for downstream water users. Nevertheless, since people often exhibit parochial behavior, the question of who benefits from the ecosystem services provided could be relevant to PWS effectiveness. We implemented a modified dictator game in the field to study the effect of varying the geographical identity of downstream beneficiaries on 1) baseline water provision in the absence of PWS, 2) PWS effectiveness, and 3) provision decisions after the payments end (i.e., "motivational crowding"). Our experiments involved 60 rural farmers from the Colombian municipality of Junín, whose water provision decisions affected passive downstream beneficiaries in either the same municipality or the capital city of Bogotá. Our findings suggest that while sharing a closer place identity with downstream beneficiaries is relevant to determining baseline water provision, it does not affect PWS effectiveness or the emergence of motivational crowding effects.

3.1. Introduction

Ecosystem services that have been degraded globally over the past 60 years include those associated with watersheds, especially fresh water purification and provision (MEA, 2005; IPBES, 2019). One of the greatest challenges of water management in watersheds arises from the asymmetry in water appropriation due to their vertical nature (Cardenas et al., 2011). People upstream always have first access to water supplies; therefore, the quantity and quality of water downstream depend on upstream decisions regarding land use and water extraction. Notably, watersheds often cover large areas crossing political and geographical boundaries. This reduces social interactions between stakeholders, making it difficult for them to build empathy, trust, and reciprocity. However, these interactions are crucial factors for conflict resolution and the facilitation of collective action in water systems (Fujiie et al., 2005; Swallow et al., 2006). Furthermore, collective action in watersheds often involves several groups of actors with different interests since the context of a watershed is characterized by a variety of stakeholders occupying

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different regions in the watershed (e.g., farmers, urban water suppliers, and mining and energy companies).

Payments for watershed services (PWS) potentially help to overcome these problems by motivating upstream landholders to provide more ecosystem services through external incentives. PWS are a particular case of payments for ecosystem services (PES). PES are voluntary transactions in which the beneficiaries of certain ecosystem services pay providers directly on the condition that they ensure the provision of those services or conduct activities thought to induce such services (Wunder, 2005). However, in PWS, the beneficiaries (water users) typically have little incentive to individually pay for improved watershed service provision due to the public good nature of these services (Porras et al., 2008). Therefore, PWS are often intermediated by governments, NGOs, or international agencies and financed by governmental bodies or external donors (Bösch et al., 2019).

From a standard economic perspective, PWS provide incentives for upstream water providers to increase positive externalities or minimize negative externalities for downstream water users. Without these additional payments, narrowly self-interested landowners would simply not care about water provision downstream. Yet, a large body of research has shown that human behavior is not solely driven by material self-interest, but that humans often exhibit other-regarding preferences. In particular, people often exhibit parochial behavior (i.e., they consistently act more prosocial toward those with whom they share a closer social identity; for a review, see Everett et al., 2015). If collective action is indeed led by parochial altruism before PWS are implemented, who benefits downstream could influence the effectiveness of a PWS program.

Furthermore, in a context of competition for resources, parochial behavior could lead people to act antisocially toward individuals with antagonistic social identities. Since inequalities in access to safe drinking water persist between rural and urban areas (UNDP, 2019), rising urban populations intensify rural-urban water conflicts because water is reallocated from rural to urban areas to meet the growing demand for fresh water in cities (Garrick et al., 2019). In this context, the take-up and effectiveness of PWS programs could potentially be affected if upstream providers perceive people from larger cities as the main water beneficiaries. Yet, little evidence exists regarding the relationship between upstream provision and the identity of downstream beneficiaries.

In this paper, we contribute to this gap by implementing an economic experiment in form of a modified dictator game that recreates the asymmetric appropriation of water in a watershed. We analyzed the effect of the identity of downstream beneficiaries on upstream water provision. Since the relationship between upstream providers and downstream beneficiaries is pre-defined by place-specific geographies (Bösch et al., 2019), we focused on place identity—a particular case in which one's identity is based on belonging to a geographically defined group (for a review, see Lewicka, 2011). In our experiment, upstream providers were rural farmers from a municipality in Colombia, while downstream beneficiaries were either neighbors living in the same municipality or residents of the capital city of Bogotá. Our experimental design included three rounds, with PWS being offered only in the second round. This setup allowed us to evaluate the effects of changing the geographical distance between upstream providers and downstream beneficiaries on PWS effectiveness and prosocial behavior after the payments ended. This is a relevant issue since one of the main concerns about PES programs is their potential to replace intrinsic motivations for environmental conservation with extrinsic motivations (so-called "motivational crowding"; for reviews, see Rode et al., 2015; Ezzine-de-Blas et al., 2019).

Our paper is related to the few experimental studies on social identity that included aspects of externalities. For example, Schwartz-Shea and Simmons (1990, 1991) and (Delaney and Jacobson, 2014) conducted lab experiments to study how in-group behavior was affected by varying the identity of outsiders that were passively harmed by in-group cooperation. Outsider beneficiary groups in Schwartz-Shea and Simmons (1990, 1991) were two real foundations with different purposes (a scholarship fund and a children's medical center). The results suggest that the size of the negative externality depends on the participants' perceptions of the needfulness of outsiders compared to that of individuals in the in-group. In Delaney and Jacobson (2014), the negative externality was reduced when outsiders were present in the experiment compared to when they were in a different room. Closest to our study is the lab-in-the-field experiment of Jack (2009), which implemented a trust game (also known as an investment game) in which the roles of first and second mover were assigned based on participants' location in a Kenyan watershed. First movers were members of upstream communities, while second movers were members of downstream communities. Unlike in our study, Jack (2009) did not vary downstream identity since the exact location of second movers was not mentioned. Still, social identity was an important component of individual transfers (investments). Transfers by upstream players were higher if they perceived upstream communities to have better living conditions than downstream communities. This finding supports the relevance of needfulness perceptions. Similarly, returns from downstream players were higher if they agreed that downstream individuals should compensate upstream communities. Since compensations or returns for upstream offers were not guaranteed but defined by downstream players, trust and experienced trustworthiness were likely one of the main determinants for upstream behavior. However, in PWS, incentives are only conditional on the provision of specific ecosystem services. Therefore, in our study, we avoided strategic considerations by implementing PWS on top a modified dictator game instead of a trust game.

The remainder of this paper is organized as follows. In Section 2, we describe the study area. The next two sections present the experimental design and introduce our research hypotheses. Section 5 presents the results and a discussion is provided in the final section.

3.2. Study area

We conducted our experiments in the *vereda*[4] San Francisco in the municipality of Junín, a Colombian municipality known for its valuable hydrological systems. Notably, cattle farming is the main source of income for many households in the *vereda*. It is characterized by a family production system of traditional smallholders with extensive cattle farming and low profitability. Water-related conflicts within the municipality are often caused by unattended cattle upstream that pollute water sources and the use of rudimentary water bowl systems that result in water waste (Instituto Alexander von Humboldt, 2016). In the absence of a local aqueduct, downstream households in the *vereda* are particularly vulnerable to the negative effects of these practices.

This area also plays a crucial role in the water supply of Bogotá. The Chingaza supply system, one of the most important water supply systems for the city of Bogotá, takes water from this and other neighboring municipalities. The Chingaza supply system consists of two reservoirs that store water collected from three different rivers, as well as a water treatment plant (WTP) where water is treated before being distributed to Bogotá (see the magenta-colored line in Figure 7). The Chingaza supply system already

[4] A *vereda* is a territorial administrative subdivision of the rural area of municipalities in Colombia.

ensures nearly 80% of Bogotá's water demand (EAAB-ESP, n.d.). However, due to the growing urban expansion of Bogotá, the aqueduct company plans to expand its capacity. Through the Chingaza II expansion project, Bogotá's aqueduct company proposes the capture of water from seven other tributaries and the construction of an additional reservoir. In particular, a planned expansion to the North seeks to capture water from the basins of four additional rivers, one of which (the Chorreras River) is the main source of water supply in the *vereda* San Francisco (see purple-colored lines in Figure 7).

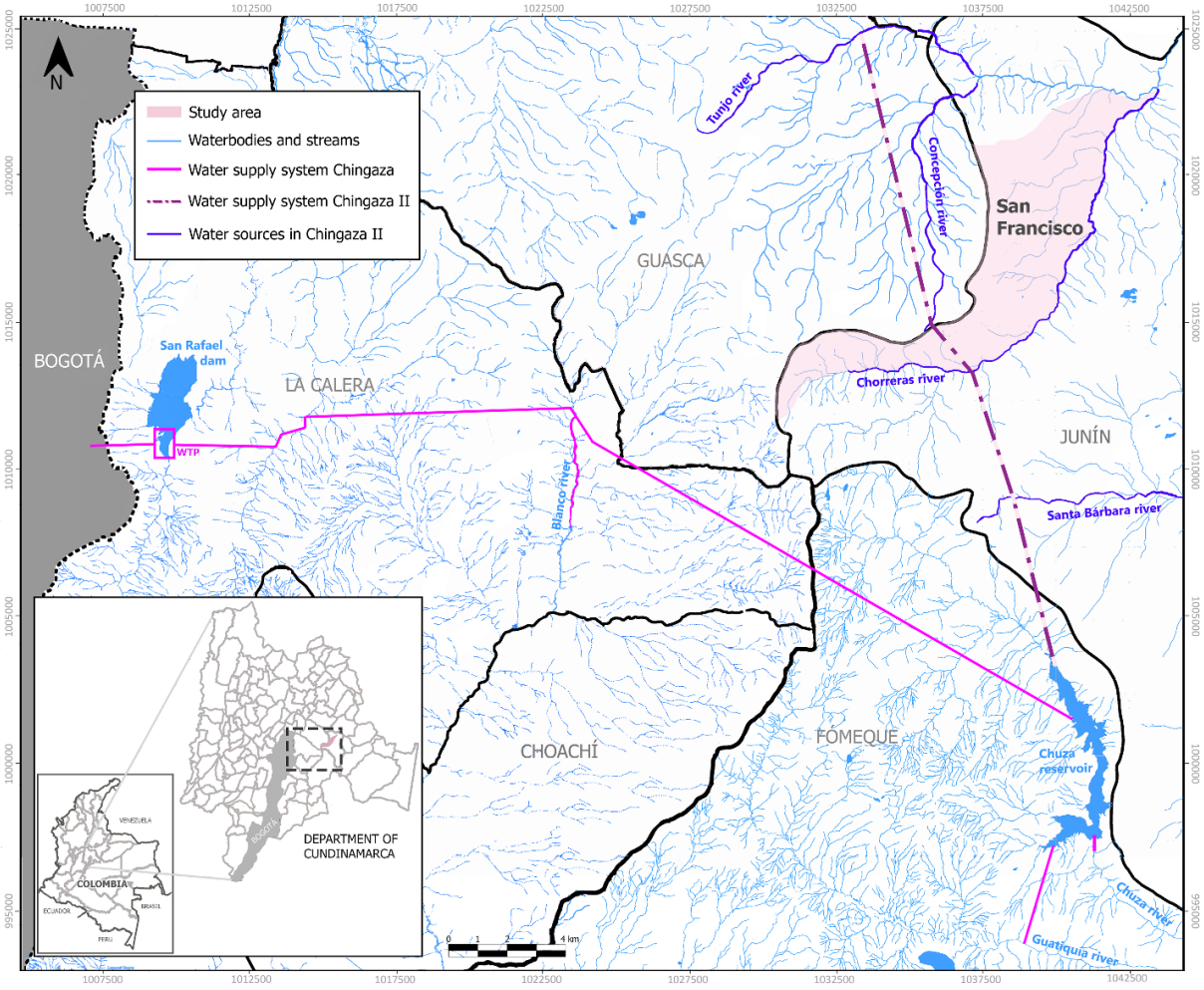

Figure 7. The Chingaza II expansion project. Own elaboration. Data source: Directorate of Spatial Data Infrastructure and Statistics (DIDEE by its Spanish acronym), Government of Cundinamarca. CSR: MAGNA Colombia Bogotá, GRS 1980.

This expansion project was highly controversial among the inhabitants of the municipalities affected by it. Notably, repeated confrontations have led to delays in the expansion project. In particular, local inhabitants fear that the project may threaten their water supply. Furthermore, they believe that the current fees for water use paid by Bogotá's aqueduct company are negligible compared to the costs they must incur by not being able to use their own territory (van der Hammen et al., 2015). Worse still, since the fees are not paid directly to local peasants, they are rarely invested in their territory (ibid). Thus, PES have been proposed as a potential compensation mechanism in this area (Sguerra et al., 2011).

3.3. Methods and research hypotheses

3.3.1. Pre-experimental survey

Between December 2017 and January 2018, we interviewed 60 farmers from different households in the *vereda* San Francisco, municipality of Junín, through door-to-door visits. During each visit, we collected information on sociodemographic characteristics, social and environmental attitudes, and perceptions about the state of the environment. We also asked farmers to compare the people who live in Bogotá to the people who live in their own municipality in terms of how wealthy and needful they are, and to report which group they identified most with (see Appendix A for the exact questions). Farmers were informed that they will be visited again to complete another survey (i.e., the economic experiment) at a later time. To improve the balance between our treatments, we divided our sample into 10 groups based on participants' gender and the sector of the *vereda* in which they lived. Subsequently, we randomly assigned half of the participants in each group to each one of the two treatments. Between February and March 2018 we revisited each farmer to deploy our experiment.

3.3.2. Experimental design

The core of the experiment was a modified dictator game that simulated the negative externality produced by changes in upstream land use on downstream water availability and quality. In the game, farmers were presented with a hypothetical farm containing 8 hectares of upstream forest. Farmers had the opportunity to convert their forest for cattle farming, which was associated with a higher revenue per hectare. However, each hectare of upstream forest produced one unit of downstream water, which was depleted by introducing cattle farming. Hence, when deciding how much forest to conserve, farmers simultaneously decided how much water to produce downstream. To ease farmers' understanding of the game, we referred to money directly instead of using points. The cash equivalent of the farmers' marginal returns was set so that the average payoff covered the area's daily minimum wage (8€). Thus, a hectare of farmland in cattle farming implied a marginal return of 1,50€. We fixed the marginal return of forest conservation at 30% of the marginal return of cattle farming. Thus, a hectare of farmland with forest implied a marginal return of 0,45€. The marginal water benefit for downstream beneficiary households was set to 1,20€ per hectare, for a total social benefit of 1,65€ per hectare. Therefore, forest conservation was socially optimal.

The experiment consisted of three rounds, one of which was randomly chosen to be payoff-relevant. In the first round, farmers played the baseline setting and privately decided the amount of water to provide downstream based on their preferred combination of forest conservation and cattle farming. Farmers marked their decision on a paper form that visually illustrated all possible scenarios and payoffs for both the upstream farmer and downstream household (see Appendix A). In the second round, the PES policy was introduced as a mechanism to increase water provision downstream. Farmers again had to decide the amount of water to provide downstream from a new set of 8 hectares of farmland, which they had to distribute between forest conservation and cattle farming. However, in this round, each farmer received a payment of 0,45€ per unit of conserved forest. The payment level was set below the opportunity cost of forest conservation, which is in line with the empirical observation that many PES programs do not fully cover opportunity costs (Kosoy et al., 2008; Bennett, 2008; Rico García-Amado et al., 2011; Namirembe et al., 2014; Bétrisey et al., 2018; Chervier et al., 2019). Finally, in the third round, the game reverted to the initial setting without the PES policy. From the beginning of the experiment, farmers were aware that

they would have to make decisions over three rounds; however, they were unaware of the conditions of each round.

We implemented two different treatments that were identical in structure and the level of incentives, but which varied in relation to the place of residence of the downstream water beneficiaries. In the *Locals* treatment, beneficiaries were neighbors living in the urban area of the participant's place of residency (i.e., Junín municipality). In the *Outsiders* treatment, beneficiaries were residents of the city of Bogotá. Each participating farmer was randomly paired with one household for which they had to decide how much water they wanted to provide. The amount of water provided was delivered as an actual credit to the water bill of this downstream beneficiary household. Although the downstream households were not present during the experiment, participating farmers were aware of the delivery procedure and had access to a signed letter confirming the agreement between a representative from Osnabrück University and the urban aqueduct of Junín or a leader of the recipient neighborhood in Bogotá, respectively. Table 4 summarizes our treatments and experimental design.

Table 4. Overview of treatments and experimental design.

Name of treatment	Description of place of residence		Description of experimental design		
	Upstream provider	Downstream beneficiary	Round 1 *(pre-policy)*	Round 2 *(during policy)*	Round 3 *(after policy)*
Locals	Junín (Rural)	Junín (Urban)	Externality game without PES policy	Externality game with PES policy	Externality game without PES policy
Outsiders	Junín (Rural)	Bogotá			

3.3.3. *Place identity index*

In addition to information on whether farmers identified more closely with the people who live in their own municipality or those who live in Bogotá, we used information collected in the pre-experimental survey to create a *Place identity* index based on evidence from the literature on predictors of place identity. Beyond physical and geographical factors, place identity can also be predicted by social and sociodemographic factors (Lewicka, 2010). Within the sociodemographic factors, residence length is the most consistent positive predictor (Hay, 1998; Shamai and Ilatov, 2005; Gustafson, 2008; Lewicka, 2005, 2010), followed by homeownership (Bolan, 1997; Mesch and Manor, 1998; Brown et al., 2003). Among the social factors, the strength of community ties is the most consistent positive predictor (Kasarda and Janowitz, 1974; Ringel and Finkelstein, 1991; Mesch and Manor, 1998; Brown et al., 2004; Lewicka, 2005; Bonaiuto et al., 1999, 2006) and is often measured as trust in neighbors (Mesch and Manor, 1998), involvement in social activities (Bonaiuto et al., 1999, 2006), and sense of security (Brown et al., 2003, 2004; Lewicka, 2010).

Since homeownership and voluntary social work may capture additional effects that could bias the effect of social identity (e.g., wealth and pro-sociality), we created an additive index of place identity based solely on farmers' responses regarding whether or not they belonged to a local association (*Association*), whether or not they believed that people in their community can be trusted (*Mutual trust*), whether or not they had lived their entire lives in the municipality (*Native*), and whether their parents had or had not

lived their entire lives in the municipality (*Native ancestors*). Finally, to facilitate the interpretation of our results, we used a median split to dichotomize the former continuous additive index. Thus, the dummy variable *Place identity* had a value of 0 for farmers with weak place identity and a value of 1 for farmers with strong place identity.

3.3.4. Analytical approach and research hypotheses

The present study aimed to examine the impact of place identity on three outcomes: 1) baseline water provision in the absence of PWS; 2) PWS effectiveness; 3) motivational crowding observed after the payments end. For this purpose, we compared water provision between treatments in three stages: pre-policy, policy, and post-policy. In the pre-policy stage, we compared baseline water provision. To analyze the policy and post-policy stages, we used a difference-in-difference approach to study the effectiveness and the crowding effects of the PWS policy. Thus, we compared the change in forest conservation levels once PWS were introduced (i.e., the difference between rounds 2 and 1) and changes in forest levels after the payments ended (i.e., the difference between rounds 3 and 1) between the two treatments. Notably, since we did not include two additional control groups (one for each place identity) without PWS, we could not assess the extent to which social identity was associated with an overall crowding-in or crowding-out effect. Instead, we could only estimate whether the crowding effect was larger or smaller in one treatment relative to the other.

Based on the evidence of people exhibiting parochial altruism, we formulated the following research hypotheses to guide our analysis:

- H0 (*Social identification*): A higher proportion of farmers will identify with the people of Junín rather than with the people of Bogotá.

- H1 (*Pre-policy stage treatment effect*): In round 1, farmers will provide more water in the *Locals* treatment compared to the *Outsider* treatment, on average.

- H3 (*Policy stage treatment effect*): Relative to round 1, farmers' average water provision levels will increase more strongly during round 2 in the *Outsider* treatment compared to the *Locals* treatment.

- H3 (*Post-policy stage treatment effect*): Relative to round 1, farmers' average water provision levels will increase more strongly or decrease less strongly during round 3 in the *Locals* treatment compared to the *Outsider* treatment (i.e., a potential crowding-out effect is weaker—or crowding-in effect is stronger—under the *Locals* treatment).

- H4 (*Heterogeneous treatment effect*): The expected effects in H1, H2, and H3 will be stronger for farmers with strong place identity relative to farmers with weak place identity.

3.4. Results

3.4.1. Social identification

We began by examining farmers' comparisons between the people living in both downstream locations included in this study. Except for one farmer, all farmers identified more closely with the residents of Junín than with the residents of Bogotá (98%). Therefore, this result supports H0 (Fisher's exact test p-

value=0.017). Additionally, 30% of farmers indicated that Junín residents are those with the highest income, while 70% of farmers indicated that those are Bogotá residents instead. Paradoxically, 82% of farmers also indicated that Bogotá residents are those with the most unsatisfied basic needs, while 18% of farmers indicated that those are Junín residents instead. According to farmers, although wages are higher in Bogotá than in Junín, jobs are more unstable and prices of goods and services are also higher. Prior studies have indicated that donations to outsiders increase with recipients' needfulness (Schwartz-Shea and Simmons, 1990, 1991). Since the participants seemed to have mixed images regarding the needfulness of people living in Junín, observed differences in the perceptions of income and unsatisfied basic needs could potentially amplify or counteract the effect of place identity in downstream water provision.

Next, we examined farmers' characteristics regarding predictive variables of place identity. Approximately 63% of the participating farmers lived in the *vereda* for their entire life. Of these individuals, approximately 68% reported that their parents also lived in the municipality for their entire lives. Additionally, approximately 28% of farmers confirmed that they belong to a local association. Finally, approximately 63% of farmers noted that people in their community could be trusted. The resulting continuous additive index of place identity ranged from 0 to 4, with a median of 2. Therefore, those with an additive index of at least 2 were assigned a strong place identity (68%), while those with less than 2 points were assigned a weak place identity (32%).

Table 5. Descriptive statistics by treatment of farmers' perceptions and sociodemographic characteristics related to social or place identity.

Variable name	Description	Outsiders treatment			Locals treatment			Fisher's exact test p-value
		N	n	Proportion n/N	N	n	Proportion n/N	
Identified locals	1 if identifies more with residents of Junín than with residents of Bogotá	30	30	1.000	30	29	0.967	1.000
Income locals	1 if believes residents of Junín have more income than residents of Bogotá	30	9	0.300	30	9	0.300	1.000
Needs locals	1 if believes residents of Junín have more unsatisfied basic needs than residents of Bogotá	30	6	0.250	30	5	0.200	1.000
Association	1 if belongs to a local association	30	10	0.333	30	7	0.233	0.567
Mutual trust	1 if believes people in their community can be trusted	30	19	0.633	30	19	0.633	1.000
Native	1 if has lived entire life in the municipality	30	21	0.700	30	17	0.566	0.422
Native ancestors	1 if parents lived their entire lives in the municipality	30	18	0.600	30	15	0.500	0.604
Place identity	Dichotomized additive index of place identity predictors (median split): 1 if strong place identity	30	22	0.733	30	19	0.633	0.580

Finally, we determined whether farmers' characteristics were similar between treatments. Table 5 presents the distribution of each variable across treatments and the test results for whether statistical differences exist between the two treatments. We determined that our treatments were comparable in

all variables. Descriptive statistics and randomization checks of additional covariates are reported in Appendices B and C. In support of our randomization strategy, we found no statistically significant differences between both treatments for all sets of additional covariates.

3.4.2. Pre-policy stage

We then examined farmers' decisions at the pre-policy stage. Approximately 85% of farmers provided at least one unit of water downstream, which suggests that most participants have a certain degree of other-regarding preferences in accordance with most studies on dictator games (for meta-analysis, see Engel, 2011). The upper panel of Figure 8 summarizes the average units of water provided downstream per round and treatment. During the first round, an average of 3.13 units of water (s.d. = 2.57) were provided downstream in the *Locals* treatment, while in the *Outsiders* treatment, an average of 3.30 units of water (s.d. = 2.12) were provided. Therefore, baseline water provision was 0.17 units lower in the *Locals* treatment compared to the *Outsiders* treatment. However, this difference was not statistically significant (Mann-Whitney U test, p-value=0.632).

As a robustness check, we ran a regression analysis to control for farmers' perceptions of the needfulness of the people living in Junín. Since the dependent variable, water provision, was censored at 0 and 8 units, we opted to use a Tobit regression. Our model included a dummy variable (*Local*) that captured the difference in downstream water provision between the two treatments. *Local* had a value of 1 if the farmer participated in the *Locals* treatment and 0 if the farmer participated in the *Outsiders* treatment. We also controlled for needfulness perceptions and other additional covariates. The regression analysis suggested that water provision was nearly one unit higher in the *Locals* treatment compared to the *Outsiders* treatment (see Table 6, Model I). However, since the coefficient was not statistically significant, our results reject H1.

In terms of the effect of needfulness perceptions, we observed that perceiving local residents (of Junín) as more needful than residents of Bogotá significantly increased downstream water provision. However, we expected that farmers' perceptions regarding the needfulness of the people living in Junín would be particularly relevant in the *Locals* treatment. Therefore, we ran an additional model that included the interaction between the variables *Needs local* and *Income local* with the treatment variable *Local* (see Table 6, Model II). The resulting regressions showed that needfulness perceptions were in fact only relevant in the *Locals* treatment. Notably, water provision to the local residents (of Junín) was 3.3 units larger when participating farmers perceived local residents had more unsatisfied basic needs than residents of Bogotá.

Finally, we analyzed whether the treatment effect was different among farmers with a weak place identity relative to farmers with a strong place identity (H4). Thus, we ran an additional regression model including the interaction between the index variable *Place identity* and the treatment variable *Local t*o capture the difference-in-differences effect of *Place identity* (see Table 6, Model III). In line with our expectations, the coefficient of the interaction was positive and statistically significant. Thus, the treatment effect was 3.5 units larger for respondents with a strong place identity than for respondents with a weak place identity. Therefore, our results support H4 for the differential treatment effect at the pre-policy stage.

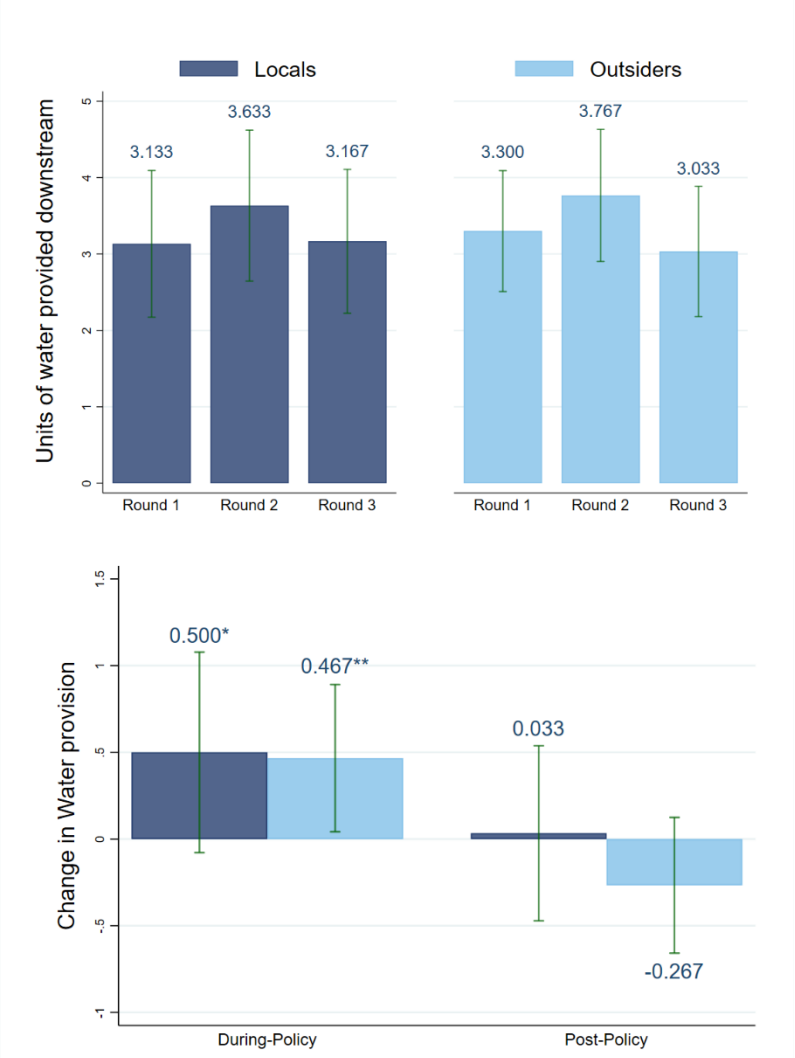

*Figure 8. Average levels and changes in downstream water provision. The top shows the average levels of downstream water provided by round and treatment (dark blue bars for the Locals treatment, light blue bars for the Outsiders treatment). The bottom panel shows the change in forest area between rounds per treatment. Error bars represent the 95% confidence interval. Asterisks indicate statistically significant differences between Round 1 and Round 2/3 using the Wilcoxon matched-pairs signed-rank test (*** p<0.01, ** p<0.05, * p<0.1).*

Table 6. Tobit regression estimates of downstream water provision levels at the pre-policy stage.

Dependent variable: Units of downstream water	Model I		Model II		Model III	
Local	0.920	(0.706)	0.171	(0.848)	-1.727	(1.273)
Place identity					-0.142	(1.008)
Place identity x Local					2.988**	(1.462)
Needs locals	1.909*	(0.985)	0.324	(1.240)	0.716	(1.195)
Needs locals x Local			3.325**	(1.802)	2.655	(1.708)
Income locals	0.626	(0.849)	0.355	(0.982)	0.353	(0.919)
Income locals x Local			0.370	(1.387)	0.685	(1.313)
Constant	2.111	(3.028)	3.143	(2.486)	2.886	(2.370)
Control random assignment	YES		YES		YES	
Control variables	YES		YES		YES	
Observations	60		60		60	
Prob > F	0.001		0.017		0.004	
Pseudo R2	0.097		0.110		0.134	

Note: Control random assignment included gender and *vereda* sector. Control variables included age, education, income, land tenure, and household size. See Appendix D for the full version showing all coefficients. Robust standard errors in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1.

3.4.3. Policy and post-policy stages

Furthermore, we examined the effectiveness of the PWS policy and the effect of ending the payments. As shown in the bottom panel of Figure 8, the introduction of payments significantly increased average downstream water provision relative to round 1 in both treatments (Wilcoxon matched-pairs signed-rank, p-value *Local*=0.059, p-value *Outsiders*=0.025). This increment was nearly 0.5 hectares in both treatments and we found no statistically significant differences between the two treatments (Mann-Whitney U test, p-value=0.858). After ending the payments in round 3, downstream water provision levels returned to a level similar to round 1 in the *Locals* treatment, while they decreased by 0.27 units on average in the *Outsiders* treatment. This suggests a crowding-out effect in the *Outsiders* treatment relative to the *Locals* treatment. Nevertheless, we found no statistically significant differences in changes between the two treatments in terms of water provision (Mann-Whitney U test, p-value=0.465).

We complemented this analysis with an econometric regression. Again, we controlled for needfulness perceptions by including the variables *Needs local* and *Income local* as well as their interactions with the treatment dummy. Given the panel structure of our data, we opted for a random effects Tobit model using a difference-in-differences (DD) approach. Thus, we included two dummy variables (*Policy* and *Post-policy*) that represent the second and third stage, respectively, and we interacted these with the treatment dummy *Local*. Model I in Table 7 also controlled for the variables used in the random assignment of treatments, while Model II also controlled for additional sociodemographic covariates. At

the policy stage, the coefficient of the interaction was nearly zero and not statistically significant, which confirmed the non-parametric analysis. At the post-policy stage, the interaction coefficient suggested that water provision was approximately 0.3 units higher in the *Locals* treatment relative to the *Outsiders* treatment, which confirmed the non-parametric suggestion of a relative crowding-out effect in the *Outsiders* treatment. Again, the coefficient was not statistically significant. Therefore, our results reject H2 and H3.

Table 7. Random effects Tobit estimates of DD models.

Dependent variable: Units of water downstream	Model I		Model II	
Local	-0.333	(0.896)	0.357	(0.850)
Policy	0.598**	(0.279)	0.598**	(0.280)
Policy x Local	-0.004	(0.397)	-0.003	(0.399)
Post-policy	-0.287	(0.280)	-0.286	(0.281)
Post-policy x Local	0.293	(0.397)	0.301	(0.399)
Constant	2.142**	(1.005)	1.988	(2.385)
Control needfulness	YES		YES	
Control random assignment	YES		YES	
Control variables	NO		YES	
Observations	180		180	
Number of id	60		60	
Prob > chi2	0.007		0.000	

Note: Control needfulness included the variables Needs local, Income local, and the interaction of each variable with the treatment variable Local. Control random assignment included the gender and *vereda* sector. Control variables included age, education, income, land tenure, and household size. See Appendix D for the full version showing all coefficients. Standard errors in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1.

Finally, we tested whether the treatment effect was stronger on farmers with a strong place identity in comparison to those with a weak place identity (H4). Thus, we estimated the heterogeneous treatment effect of the PWS, captured by a triple interaction estimate resulting from interacting the treatment, stage, and place identity variables. The resulting regression is summarized in Appendix D.

Figure 9 presents the resulting predicted levels of downstream water provision per round together with the marginal effects of each stage on downstream water provision. The upper panel of Figure 9 shows that predicted levels of downstream water provision were in line with the results of the previous section in support of H4 at the pre-policy stage. In the first round, water provision for farmers with a strong place identity was approximately two times higher in the *Locals* treatment when compared to the *Outsiders* treatment, on the contrary, for farmers with a weak place identity water provision was lower in the *Locals* treatment when compared to the *Outsiders* treatment. The bottom panel of Figure 9 shows that the predicted marginal effect of introducing the PWS at the policy stage was only statistically significant for farmers with a strong place identity in both treatments. Upon examining the marginal effects at the post-

policy stage, we observed that—contrary to expectations—the marginal effect of ending the PWS policy was negative and statistically significant for farmers with a weak place identity in the *Outsiders* treatment. In all other cases, the effect was minimal and not statistically significant.

In line with the predicted marginal effects, the triple interaction coefficients at the policy and post-policy stage were 0.218 units and -0.714 units, respectively (see Appendix D). However, both of them were not statistically significant. Therefore, our results reject H4 for the differential treatment effect at the policy and post-policy stage.

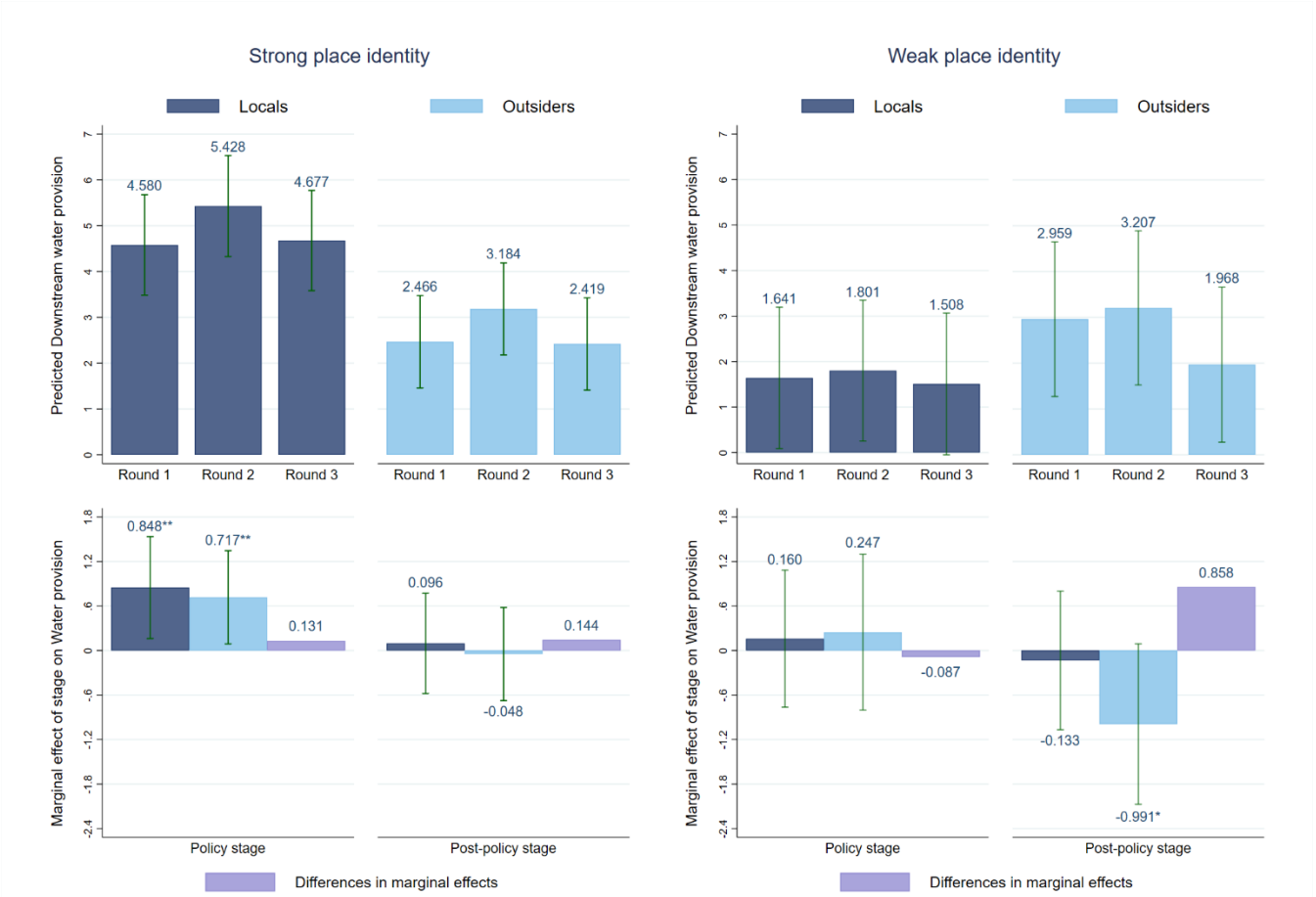

*Figure 9. Per-treatment average predictions of downstream water provision and the marginal effects of stage. The top panel summarizes average predicted levels of downstream water provision per round, treatment, and level of place identity. The bottom panel compares the average marginal effects of stage on downstream water provision between treatments for each group of farmers with either strong or weak place identity. Purple bars measure the difference in marginal effects between treatments. For example, the marginal effect of introducing the PWS policy was +0.848 units of water in the locals treatment and +0.717 units in the outsiders treatment; thus, the difference is 0.848 – 0.717 = 0.131 units. Error bars represent the 95% confidence interval. Asterisks indicate that the average marginal effect was significantly different from zero (*** p<0.01, ** p<0.05, * p<0.1).*

3.5. Discussion

We implemented a modified dictator game in the field to study the effect of the identity of downstream beneficiaries on upstream water provision in the context of the asymmetric appropriation of water in a watershed. In particular, we focused on differences in place identity. In our study, 60 rural farmers from the Colombian municipality of Junín played the role of upstream water providers for passive downstream beneficiaries, with beneficiaries being either residents of the urban area of Junín (*Locals* treatment) or residents of the capital city of Bogotá (*Outsiders* treatment). We assessed the effect of the geographical distance between upstream providers and downstream beneficiaries on three outcomes: 1) baseline (pre-policy stage) water provision; 2) PWS effectiveness (policy stage compared to baseline); 3) water provision after the payments ended (post-policy stage compared to baseline, "motivation crowding").

Our findings contribute to the scarce existing evidence on the relevance of social identity in the context of externalities. Although we did not find an overall effect of the geographical identity of downstream beneficiaries on baseline water provision, sharing the same geographical identity with downstream beneficiaries was relevant to determine baseline water provision downstream among farmers with a strong place identity. Similarly, we did not find evidence of downstream beneficiaries' geographical identities affecting PWS effectiveness or motivational crowding overall. Nonetheless, the PWS policy was able to significantly increase water provision, even among those with already high provisioning levels (i.e., farmers with a strong place identity in the *Locals* treatment). This suggests that the PWS policy complements—rather than substitutes for—social identity motivations when beneficiaries share the same geographical location. Furthermore, our results also provide suggestive evidence of PWS crowding out the intrinsic motivations of farmers with weak place identity who provide water to geographically distant beneficiaries (i.e., *Outsiders* treatment). Therefore, in the absence of a well-defined group of water beneficiaries making payments, PWS programs could be framed in terms of their general benefits to local communities, rather than being framed around benefiting actors within hotspots of water use (e.g., larger cities).

One possible explanation for the unexpected result of PWS crowding out the intrinsic motivations of farmers with weak place identity in the *Outsiders* treatment is that the intrinsic motivations of these farmers might not be related to place identity (i.e., to parochial behavior) and thus could be more susceptible to being substituted by external motivation. For instance, external incentives have previously been associated with a reduction in the internal satisfaction that occurs when donating or with a frameshift from community cooperation to economic reasoning (Rode et al., 2015). Investigating the relationship between identity-related motivations and other types of motivation while also determining the cases in which one prevails over the other could be a promising extension of this work.

One important limitation of our study is our small sample size. While our sample was large for a field experiment in the Global South, it was small in terms of statistical power. In the future, other experiments such as the comparative dictator game (Candelo et al., 2018)—which is based on a within-subject design—could help reduce this limitation. In such a design, participants could be asked to simultaneously decide on water provision for different recipient types. We also encourage future research that studies social identity in water provisioning and uses methodologies that capture finer variations in social closeness. Visual fusion scales, such as the one adopted by Purzycki and Lang (2019), have the advantage of being comparable across samples that vary in literacy, thereby facilitating their implementation in the field.

Finally, we call for caution in assuming that the results could be extrapolated to any context. More studies are needed to check whether the results can be generalized to other settings or may vary with settings. Parochial altruism in our sample was reinforced by the context of water competition between the study area and the city of Bogotá. Therefore, results could be different in another context without a salient competition for water resources. Similarly, needfulness perceptions towards water beneficiaries are also likely to vary between contexts. This is relevant as we also confirmed Schwartz-Shea and Simmons's (1990, 1991) results about the relevance of needfulness perceptions—especially since participants' perceptions about recipients' unsatisfied basic needs significantly increased downstream water provision in the *Locals* treatment.

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Chapter 4 Behavioral spillovers from mixing conservation policies in neighboring areas: An experimental analysis on fairness perceptions toward unequal policies

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Summary

In recent decades, there has been a growing interest in evaluating the effectiveness of conservation programs, including the presence of unintended effects in areas not covered by a program (i.e., so-called "spillovers"). For example, payments for ecosystem services (PES) can have spillover effects via both pecuniary and behavioral channels. While most of the literature on behavioral spillover from PES has focused on those who receive payments, PES can also change the behavior of non-participants by affecting their fairness perceptions. In this study, we analyzed whether PES implementation in one area can have behavioral spillover effects in a neighboring area due to fairness concerns related to unequal policy conditions faced in the two areas. We used data from a lab-in-the-field experiment with 276 farmers in Colombia to compare the behavior of farmers who faced exactly the same policy (or the absence of a policy), but who in one case experienced a disadvantageous condition in relation to their neighbors receiving PES (treatment group) while in the other did not (control group). We found that PES exclusion and simultaneously implementing PES within and outside of a protected area can have negative behavioral spillover. Surprisingly, our findings indicate that the spillover effect does not increase with the level of inequality between neighbors. Only implementing PES in the buffer area of a protected area increases pro-environmental behavior among those within the protected area, even after the policies are removed. We propose that internalization of the positive aspects of the protected area due to the expectation of receiving future payments is a possible explanation for the unexpected effect.

4.1. Introduction

In recent decades, there has been a growing interest in evaluating the effectiveness of conservation programs (Lambin et al., 2014). During the impact evaluation process, the presence of unintended effects in areas not covered by a program has been identified as an important determinant of a program's effectiveness, which is an issue known as spillover (or leakage, slippage, spillage; Angelucci and Di Maro, 2015). Evidence on spillovers from conservation is growing, including the identification of some of the channels through which it may arise (see for review Pfaff and Robalino, 2017). In the context of payments for ecosystem services (PES), for example, negative spillovers may occur when offering monetary incentives eases credit constraints to expand extractive practices to non-enrolled areas (Alix-Garcia et al., 2012) or when the conservation of enrolled areas increases the prices of agricultural commodities with inelastic demands, thereby generating an incentive for additional degradation (Le Velly et al., 2017). However, spillovers do not always go against a program's goals. Increased labor demand for the

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implementation of eco-friendly agricultural projects financed by PES can mitigate degradation pressures in non-enrolled areas (e.g., Giudice et al., 2019 in Peru).

Additionally, spillovers from PES can also involve other policies since conservation programs are always implemented within particular institutional contexts. The direction of such spillovers can vary across contexts (Lambin et al., 2014). In Rwanda, for example, PES had positive spillovers on rule enforcement by reducing negative perceptions of park restrictions, which led communities to engage in the monitoring process (Martin et al., 2014). In contrast, PES spillovers on enforcement translated into greater degradation in Ecuador (Rodríguez de Francisco et al., 2013). Farmers in the area replaced their traditional cycle of crop rotation with continuous cultivation as a preemptive strategy to avoid the risk of being sanctioned for using land that could be considered as undergoing regeneration (Ibid).

PES can also produce behavioral spillovers via non-pecuniary motivations. An increasing body of literature focuses on the conditions under which intrinsic motivations to engage in environmental conservation may be crowded out by the introduction of economic incentives such as PES (for reviews, see Rode et al., 2015; Akers and Yasué, 2019; Ezzine-de-Blas et al., 2019). Empirical evidence is mixed, with some studies showing evidence of crowding out (e.g.,van Hecken and Bastiaensen, 2010; Fisher, 2012; Rico García-Amado et al., 2013) and others showing crowding in (e.g., Bremer et al., 2014; Arriagada et al., 2018; Grillos et al., 2019). The erosion of intrinsic motivations could lead to temporal spillovers that threaten long-term environmental conservation after the incentives provided by PES programs end.

Most of the literature on behavioral spillovers from PES focus on those who receive payments. However, PES can also change the behavior of non-participants through fairness perceptions. Since a significant proportion of people act based on reciprocity (Falk and Fischbacher, 2002), a policy intervention that is perceived to be unfair can be sabotaged. For instance, poorer households that were unable to access a PES program in Vietnam vandalized the coffee plantations of households qualifying for payments (To et al., 2012). In this sense, the implementation of PES adjacent to restricted areas may result in behavioral spillovers on non-recipients facing restrictions. Notably, many PES programs target areas near protected areas (Campanhão and Ranieri, 2019). Moreover, restrictions on human activities within protected areas can have negative impacts on human livelihoods in such areas (Nelson and Hossack, 2003; Andam et al., 2008; Franks and Schreckenberg, 2016). Seeing others being rewarded for similar activities that are imposed on people in protected areas can raise perceptions of unfairness, which could result in resentment, poaching, and antagonism toward protected areas (Mbaiwa, 2005). Furthermore, while PES have been proposed within restricted areas as compliance subsidies in the Global South (e.g., Ezzine-de-Blas et al., 2011 in Brazil; Brimont and Karsenty, 2015 in Madagascar), albeit this implies a legal inconsistency. Despite the relevance of this topic, to the best of our knowledge, no study has addressed whether and how the degree of inequality between neighboring areas—as a consequence of PES implementation within and/or near protected areas with land-use restrictions—leads to behavioral spillovers.

The studies most relevant to the present study include those by Alpízar et al. (2017a, 2017b) on the behavioral spillovers of PES selection criteria. In response to limited budgets and large numbers of eligible providers, it has been recommended that PES programs target areas facing significant threats of environmental degradation (so-called additionality criterion; Wunder, 2005, 2007; Engel et al., 2008). However, Alpízar et al., (2017a, 2017b) showed that if those with the lowest record of environmental donations in dictator games are selected to receive a PES, those with the highest record of environmental

donation reduce their subsequent contributions despite no changes in their pecuniary incentives. In contrast, random selection or the rewarding of prosocial behavior does not result in the same behavioral spillover effect. Accordingly, the authors concluded that the rationale for exclusion is relevant and suggested (but did not test) the consideration of fairness as one of the possible mechanisms that could explain such differential effects. In the present article, we extended Alpizar et al.'s studies in three ways: i) we analyzed PES spillovers on the conservation behavior of individuals who faced a different policy in a neighboring area; ii) we tested whether fairness considerations could moderate behavioral spillover; iii) we evaluated whether behavioral spillover effects remained after the PES program ended.

In particular, we focused on whether PES implementation in one area can have behavioral spillover effects in a neighboring area due to fairness concerns regarding the degree of inequality between the conditions faced in the two areas. We implemented a lab-in-the-field experiment to recreate three scenarios: 1) a high priority area where PES are implemented next to a low priority area that is excluded from PES; 2) a protected area with land-use restrictions surrounded by a buffer area where PES are implemented; 3) a protected area where PES are implemented on top of land-use restrictions, surrounded by a buffer area with only PES. Participants were farmers from the rural area of Junín, a Colombian municipality with enormous water wealth. The experiment was framed around the asymmetric provision and consumption of water between upstream and downstream users in a watershed. Watersheds are often large areas crossing institutional and/or ecological boundaries, which may lead to different policy approaches implemented for different subsets of the ecosystem service providers. In our treatment groups, subjects were told that their area was divided into two and, depending on the hydrological relevance of each area, PES were implemented in one area while one of three policies was established in the other area (i.e., no policy, fine policy, or policy mix of fine and PES). In our control groups, there was no area division and all farms faced one of these three policies. Thus, our experimental design allowed us to compare the behavior of farmers who faced exactly the same policy but in two different contexts: one in which their neighbors faced the same policy (i.e., egalitarian context) and another in which their neighbors received a pure PES policy instead (i.e., disadvantageous context). We analyzed the behavioral spillover from implementing PES in one area on the behavior of farmers in a neighboring area facing a different policy. We did so in two different stages: while the policies were in effect and after they ended.

In what follows, we describe the case study, outline the experimental design, and introduce our research hypotheses. Section 3 presents the results. The discussion and conclusions are presented in the final section.

4.2. Study area and methods

4.2.1. *The municipality of Junín and the Chingaza complex of páramos*

The municipality of Junín, department of Cundinamarca, is located 100 km east of Bogotá D.C. (central Colombia). Cattle farming is the main source of income for many rural households in the municipality and represents an important asset for reducing the risk involved in other agricultural activities. It is characterized by a family production system of traditional smallholders with extensive farming and low profitability. Unattended animals and land, in combination with the use of rudimentary water bowl systems, frequently results in water waste, pollution, and soil deterioration. Approximately 30% of the territory in this municipality is part of the eastern area of the Chingaza complex of páramos, an ecosystem known for the large volumes of freshwater that it stores (Instituto Alexander von Humboldt, 2016). The Chingaza complex supplies 80% of the water demand of Bogotá and nearly half of its area is under the

protection of a strict form of protected area (National Park Chingaza). Approximately 20% of the páramo ecosystem area in Junín (6% of the municipality's territory) is within this protected area. However, there have been reports of cattle presence within the protected area, which points to deficits in the monitoring and enforcement of park rules (van der Hammen et al., 2015). For this reason, PES outside of the protected area are seen as a potential complementary policy relevant to the management of the Chingaza complex (Sguerra et al., 2011).

We worked with farmers from the *veredas*[5] San Antonio, San Rafael, and Santa Bárbara (see Figure 10). Since the study area is not within the páramo area of the municipality, it was unlikely that participating farmers had any prior personal experience with park rules or PES that could bias the results. Yet, their proximity to this context would allow them to identify with the situation framed within the experiments, thereby increasing the relevance of the findings.

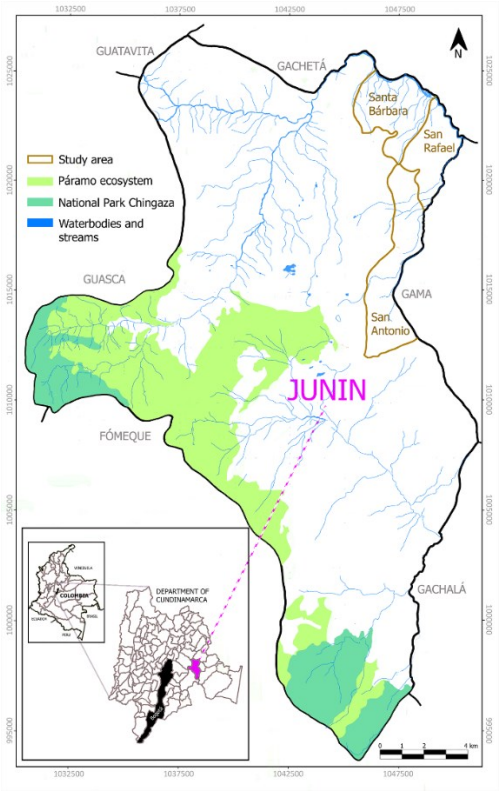

Figure 10. Location of the study area. Own elaboration.
Data source: Directorate of Spatial Data Infrastructure and Statistics (DIDEE by its Spanish acronym), Government of Cundinamarca.

4.2.2. *Pre-experimental survey*

Between February and April 2018—and with the guidance of local leaders—we walked the *veredas* and interviewed 331 farmers, each from a different household. During each interview, we collected

[5] A *vereda* is a territorial administrative subdivision of the rural area of municipalities in Colombia.

information on their sociodemographic characteristics, environmental awareness and vulnerability, and motivations for conservation. We also elicited each farmer's level of risk aversion using an incentivized economic experiment similar to the one developed by Eckel and Grossman (2008). That is, we asked each participant to choose one of nine different lotteries, each of which implied a 50% probability of receiving a low payoff or a high payoff, and where the expected payoff increased linearly with the level of variance between the low and high payoffs. Finally, based on Blanco et al.'s (2011) incentivized elicitation method for fairness preferences, we used an ultimatum game to estimate each farmer's level of disadvantageous inequality aversion (i.e., the minimum acceptable offer) and a modified dictator game to estimate each farmer's level of advantageous inequality aversion (i.e., the minimum egalitarian allocation that makes the dictator prefer to share his endowment equally rather than keep it completely)[6].

Before deploying the survey, we obtained each farmer's written consent to ensure that their participation was voluntary. Farmers were also explained that they would be invited to participate in a group workshop (an experimental session) at a later time. Farmers were randomly assigned and invited to participate in one of the 71 experimental sessions. Approximately 16% (n=55) did not participate, resulting in a total of 276 participants. The sessions were conducted between June and November 2018.

4.2.3. *Experimental design*

We used a modified dictator game to recreate the negative externality that upstream land-use changes produce on downstream water availability. Each farmer was presented with a hypothetical farm with 8 hectares of forest upstream. Farmers had the option to clear each hectare of forest to introduce cattle farming. Notably, conserving forest areas produced clean water for households living downstream. Although cattle farming was associated with a higher revenue per hectare, this implied losing the positive externality downstream.

Water donations from all participating farmers were collected in a fund. The resulting water fund was shared equally among selected beneficiary households as an actual credit on each household's water bill. Downstream water beneficiaries were randomly selected households from the urban area of the municipality of Gachetá, a neighboring municipality. Although beneficiary households were not present during the experiment, participating farmers were aware of the aforementioned procedure and had access to a signed letter confirming the agreement between a representative from Osnabrück University and the manager of the urban aqueduct of Gachetá.

The experiment consisted of three rounds[7]. Each round represented a farming season and only one of them was randomly chosen to be payoff-relevant. Farmers privately decided on their preferred combination of forest and cattle farming in each round by marking their decision on a paper form that visually illustrated all possible choices and corresponding payoffs for both upstream farmers and downstream households (see Appendix A). Posters with visual representations of each form were used to complement instructions and were displayed so that participating farmers could refer to them whenever needed. Once farmers made all of their decisions—and before they knew their outcomes—each farmer

[6] During this part of the survey, each farmer was randomly matched with another anonymous farmer who was being interviewed in another location. First, the farmers informed their decisions in all possible scenarios, i.e. we used a strategy-elicitation method. Subsequently, the interviewers mediated between each pair of farmers reporting the decisions and the corresponding outcomes/payoffs by radio, without revealing the identity of the farmers.

[7] The experiment included a fourth round, the results of which are not relevant for this paper

answered a brief survey about their fairness perceptions regarding a set of environmental policies and their experience during the session (see Appendix A for more details).

In the first and third rounds, all farmers played the described baseline configuration. In the second round, farmers played a control or treatment condition depending on whether other farmers in their same session faced the same incentives or not. More specifically, except for one control condition that continued with the baseline configuration, farmers in the control conditions were told that their farms lay within a high priority area that was critical for downstream water supply and, for this reason, would face a particular environmental policy. In contrast, farmers in the treatment conditions were told that only some of the farms lay within the high priority area and those whose farms were not part of the high-priority area would not face the same environmental policy. Although the outcome of each farmer depended on their individual decisions and was not affected by the performance of their neighbors, the former spatial differentiation evoked a feeling of disadvantage because the policy conditions in one area were more beneficial to farmers compared to those applying in the other area. The exact environmental policies implemented on each condition will be described in the next paragraph. To define which farms were in or out of the high-priority area, each farmer was randomly given a clipboard with an envelope taped to the back before each session began. Farmers were asked not to look at the contents of the envelope until instructed. The envelope contained a piece of paper, the color of which defined the location of the farm in each session. We used a different color combination in each session so that participants could not anticipate the location of their farms or the condition to be implemented in their session based on information provided by participants from previous sessions. This procedure was performed in all sessions, even in those under control conditions in which the envelopes were never used. Figure 11 summarizes our experimental design.

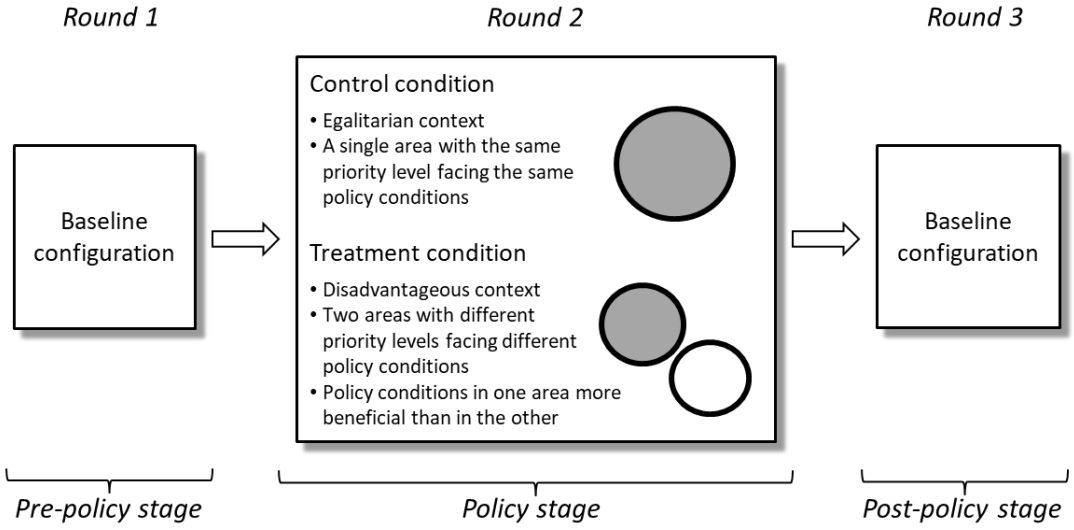

Figure 11. Overview of the experimental design.

We focused on three tangible policy scenarios that could evoke such a feeling of disadvantage because they involve unequal policy conditions between two neighboring areas: 1) a high priority area where PES are implemented next to a low priority area excluded from PES; 2) a protected area with land-use restrictions surrounded by a buffer area where PES are implemented; and 3) a protected area with PES on top of land-use restrictions that is surrounded by a buffer area with only PES. Thus, we implemented

three control conditions and three treatment conditions, respectively (see Table 8). We used the abbreviation *RD*—as in relative disadvantage—to distinguish a treatment condition from a control condition. In the *Baseline* condition, a high priority area was not identified and no policy was implemented in any of the farms, while in the *Baseline-RD* condition, some of the farms were in a neighboring high priority area in which PES were implemented. Likewise, in the *Fine* condition, all farms were within the high priority area in which a fine policy was implemented, while in the *Fine-RD* condition, some of the farms were in an area neighboring the high priority area in which PES were implemented. Finally, in the *Mix* condition, all farms were within the high priority area in which a policy mix of fine and PES was implemented, while in the *Mix-RD* condition, some of the farms were in an area neighboring the high priority area in which only PES were implemented.

Table 8. Overview of treatments and control groups.

Type of condition	Name of condition	Context within condition	Analyzed policy	Neighboring area under PES policy	Number of participants	
					Under analyzed policy	In neighboring area
Control	*Baseline*	Egalitarian	No policy	No	36	0
Treatment	*Baseline-RD*	Disadvantage		Yes	31	24
Control	*Fine*	Egalitarian	Fine policy	No	36	0
Treatment	*Fine-RD*	Disadvantage		Yes	31	27
Control	*Mix*	Egalitarian	Policy mix of fine and PES	No	34	0
Treatment	*Mix-RD*	Disadvantage		Yes	30	27

Note: *RD* = Relative disadvantage. Behavior of subjects in the neighboring area is not part of the analysis.

4.2.3.1. Parameter definitions

To further facilitate farmers' understanding of the game, we referred to money directly instead of using points. The cash equivalent of the marginal returns of cattle farming and forest conservation was set so that the average payoff covered the area's daily minimum wage of COP $30000, which was constrained to using values in multiples of COP $500 to facilitate payments. Thus, a hectare of farmland used for cattle farming implied a marginal return (r_c) of COP $5000. We fixed the marginal return of forest conservation at 30% of the marginal return of cattle farming ($r_f = 0.3r_c$)[8]. Thus, a hectare of farmland with forest implied a marginal return (r_f) of COP $1500. The marginal water benefit for downstream beneficiary households (w) was set to COP $4000 per hectare, for a total social benefit of COP $5500 per hectare.

In the fine policy (*Fine* and *Fine-RD* conditions), farmers had to make an allocation decision similar to that of the baseline—but under land-use regulations. With probability ρ, farmers were sanctioned (S_{Fine}) for every hectare of forest they decided to cut down to introduce cattle farming. Many tropical protected areas in the Global South do not have the capacity to enforce park rules due to budgetary constraints (Bruner et al., 2004; McCarthy et al., 2012). In fact, an inability to enforce compliance with the rules has

[8] The ratio of marginal benefits between forest conservation and cattle farming (f/c) was set based on the percentage of self-consumption participation in the economy of family farming production in Colombia as a benchmark. According to the Ministry of Agriculture and Rural Development of Colombia self-consumption represents 32.6% of the agricultural production of a family, on average (Resolution 464 of 2017).

been reported in several protected areas in Colombia (Nolte, 2015). Nonetheless, we chose to implement a not too weakly enforced fine policy that, while it would not induce a risk-neutral farmer to conserve, would induce a risk-averse farmer to conserve at least part of their forest. This way, the incentive effect of the fine would not dominate the spillover effect of the disadvantageous context (i.e., see Appendix F for more details). Therefore, to ease the understanding of the game we fixed the probability of detection to one in two farmers ($\rho = 0.5$) and set the fine at COP $5000 per hectare. Thus, if the second round was chosen for payoffs, each farmer flipped a coin at the end of the experiment to determine whether their farm had been monitored or not. In case of monitoring, the farmer lost all his cattle farming earnings.

In the setting under a policy mix (*Mix* and *Mix-RD* conditions), farmers had to make an allocation decision similar to that of the baseline—but under a combination of land-use regulations and PES. Again, one in two farmers ($\rho = 0.5$) were sanctioned (S_{Mix}) for every hectare of forest they decided to cut down to introduce cattle farming. Simultaneously, farmers were offered a payment (P) for every hectare of forest they decided to keep. Since a payment that would cover the opportunity cost of conservation implied too weak a fine policy, we looked for a combination of S_{Mix} and P that would still meet the condition of a not too weakly enforced policy mix (see Appendix F for more details). Thus, we chose a payment level (P) of COP $1500 per hectare and reduced the sanction level (S_{MIX}) to COP $3500 per hectare. For simplicity, the PES policy targeting neighboring farmers in all treatment conditions offered the same level of payment per hectare of conserved forest as that offered in the policy mix (P = COP $1500). A summary of parameters is provided in Table 9.

Table 9. Summary of parameters

Parameter	Definition	Fine policy		Policy mix	
		Value	COP $	Value	COP $
L	Hectares per farm	8	-	8	-
r_c	Marginal return forest	1	5000	1	5000
r_f	Marginal return cattle farming	0.3	1500	0.3	1500
P	Payment amount per hectare	0	0	0.3	1500
S	Sanction amount per hectare	1	5000	0.7	3500

Notably, this implies that we changed the severity of the fine policy between the setting under a fine policy (*PA* and *PA-RD* conditions) and the setting under a policy mix (*Mix* and *Mix-RD* conditions). We consider this approach to be more relevant for policy making since implementing PES in protected areas with already high levels of enforcement frequently involves additional concerns (Robalino et al., 2015; Sims and Alix-Garcia, 2016). Furthermore, our goal was not to compare the effectiveness of different instruments but to test whether a disadvantageous context could have spillovers on the effectiveness of each instrument individually. Thus, our analysis consisted of comparing the following pairs of conditions: *Baseline* to *Baseline-RD*, *Fine* to *Fine-RD*, and *Mix* to *Mix-RD*. Also, we excluded from the analysis farmers in the treatment conditions who only faced the PES policy (n = 78) and focused on the behavior of all other farmers (n = 198).

4.2.4. Research hypotheses

Our experimental design allowed us to compare the behavior of farmers who faced the same policy conditions but under different contexts: disadvantageous (*treatment condition*) and egalitarian context (*control condition*). In the egalitarian context farmers faced the same policy conditions relative to their

peers (*control condition*) while in the disadvantageous context farmers faced less favorable policy conditions relative to their peers who received a pure PES (*treatment condition*). We expected a negative spillover effect on the behavior of farmers who faced a disadvantageous context relative to those who faced an egalitarian context. Moreover, we expected this to hold not only during the policy stage but also after the policy is removed. Thus, we formulated the following hypotheses:

- *H1 (Behavioral spillover, policy stage):* On average, farmers in a treatment condition will increase less strongly—or reduce more strongly—their levels of forest conservation in round 2 relative to round 1 when compared to farmers in the respective control condition (i.e., *Baseline-RD* to *Baseline*, *PA-RD* to *PA*, and *Mix-RD* to *Mix*).

- *H2 (Behavioral spillover, post-policy stage):* On average, farmers in a treatment condition will increase less strongly—or reduce more strongly—their levels of forest conservation in round 3 relative to round 1 when compared to farmers in the respective control condition (i.e., *Baseline-RD* to *Baseline*, *PA-RD* to *PA*, and *Mix-RD* to *Mix*).

These hypotheses were motivated by well-established evidence of a substantial proportion of people acting based on reciprocity (Falk and Fischbacher, 2006). Moreover, people are willing to punish those who treat them unfairly and to reward those who treat them kindly (Rabin, 1993). One cause of concern about fairness could be that people care about their relative outcomes compared to others (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000). Therefore, we expected a disadvantageous context to trigger feelings of unfair treatment compared to an egalitarian context, particularly among those who are at a disadvantage vis-á-vis farmers in a neighboring area receiving a pure PES. We formulated further hypotheses to more directly test for these mechanisms. First, we tested for the impact of our treatments on fairness perceptions and the role of fairness preferences therein by formulating the following hypothesis:

- *H3 (Fairness perceptions):* Farmers in a treatment condition are less likely to find the implemented policy setting fair than farmers in the respective control condition.

Therefore, we expect:

- *H4 (Fairness preferences):* The probability that farmers will find the implemented policy setting fair when they face a disadvantageous context compared to when they face an egalitarian context decreases with their level of aversion to disadvantageous inequality.

Second, the behavioral spillover is expected to be stronger among farmers who are averse to disadvantageous inequality compared to those who are not. Thus we formulated the following hypotheses:

- *H5 (Fairness preferences as moderator of behavioral spillover, policy stage):* The effects expected in H1 will be stronger for farmers who are averse to disadvantageous inequality compared to those who are not.

- *H6 (Fairness preferences as moderator of behavioral spillover, post-policy stage):* The effects expected in H2 will be stronger for farmers who are averse to disadvantageous inequality compared to those who are not.

4.3. Results

4.3.1. Descriptive statistics and randomization check

In this section, we first present descriptive statistics from our sample. Cattle farming was the main source of income for approximately 76% of the farmers who participated in the experiments. Nearly all of them (97%) knew of the existence of the National Park Chingaza, even though their farms were not within the protected area. Although approximately 17% had heard of PES programs, none of them reported participating in one. Additionally, 84% of farmers in our sample had some degree of risk aversion. This proportion was higher in comparison to the 76% found in Eckel and Grossman (2008) but is consistent with previous studies that reported higher levels of risk aversion among farmers in low-income countries (Yesuf and Bluffstone, 2009). Lastly, 66% of participating farmers were averse to some degree to disadvantageous inequality. Overall, 98% were averse to advantageous inequality, with 49% of these being extremely averse. Compared to Blanco et al. (2011), farmers in our sample tended to be more averse to advantageous inequality and less averse to disadvantageous inequality. All descriptive statistics for participating farmers are reported in Appendix B.

Before comparing farmers' behavior between each treatment condition and its respective control condition, we first assessed whether farmers in both conditions were indeed comparable (see Appendix C). We used logistic regressions to test for balance among three sets of observed covariates: 1) sociodemographic characteristics; 2) environmental awareness and vulnerability as well as motivations for conservation; 3) preferences regarding disadvantageous inequality aversion and risk aversion. For the three sets of observed covariates, we found no statistically significant differences between the two conditions under the fine policy (*Fine* and *Fine-RD*), nor between the two conditions under the policy mix of fine and PES (*Mix* and *Mix-RD*). However, we found statistically significant differences between the treatment and control conditions under no policy. Farmers in the *Baseline-RD* condition had lived in the *vereda* for less time, had larger homes, trusted the environmental authority less, and had better perceptions regarding the general state of the environment. Therefore, we controlled for these variables in the regression analysis of these conditions.

4.3.2. *Data analysis*

Approximately 90% of the farmers in our sample kept at least one unit of forest in the first round (pre-policy stage), which suggests that most of them had social preferences to conserve the forest. While the average giving in dictator games is typically around 20% of endowment (Camerer, 2003), the baseline average giving in our experiment was approximately 15% higher. On average, 3.9 units of land were allocated to forest during the baseline (s.d. = 2.5) and the median allocation to forest was 4 units.

Figure 12 summarizes the average units of land allocated to forest per round and condition. Compared to round 1, average forest conservation levels increased in round 2 (Policy stage) for all conditions that were under some policy. However, this increment was only significant for the *Fine-RD*, *Mix* and *Mix-RD* conditions (Wilcoxon matched-pairs signed-rank, *Fine* p-value=0.124, *Fine-RD* p-value=0.001, *Mix* p-value=0.001, *Mix-RD* p-value=0.066). In the *Baseline* condition under no policy, average forest conservation levels did not change from round 1 to round 2, suggesting that there was no learning effect. In contrast, average forest conservation levels decreased in the *Baseline-RD* condition. However, this decrease was not statistically significant (Wilcoxon matched-pairs signed-rank, p-value=0.178). In round

3. after all policies are removed, average forest conservation levels only remained significantly higher compared to round 1 in the *Mix* condition (Wilcoxon matched-pairs signed-rank, *p-value* round=0.013).

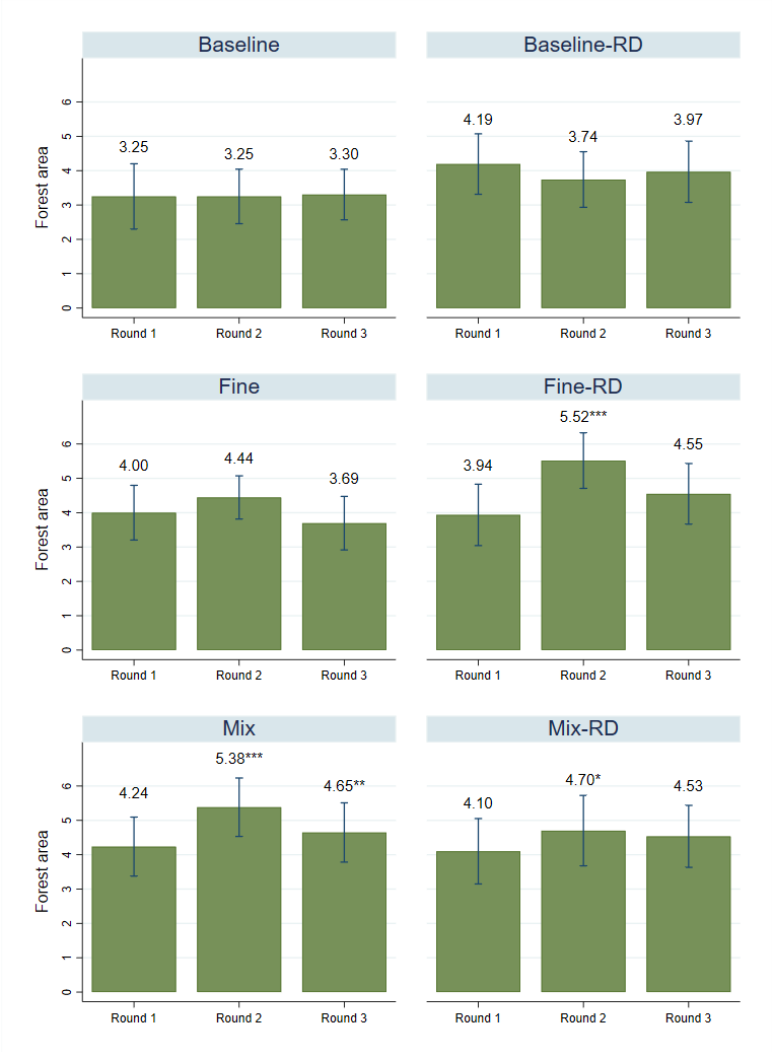

*Figure 12. Average forest conservation levels per round and condition. Asterisks imply statistically significant differences in rounds 2 or 3 relative to round 1 (Pre-policy stage) within each condition using a Wilcoxon matched-pairs signed-rank test (*** p-value <0.01, ** p-value<0.05, * p-value<0.1).*

4.3.2.1. Behavioral spillovers in the policy and post-policy stages

We also tested our hypotheses related to PES having negative spillovers on the behavior of farmers who were at a disadvantage due to being under a different policy. We tested this both while policies were being implemented (i.e., policy stage, H1) and after they were removed (i.e., post-policy stage, H2). We first compared the size of the average changes in rounds 2 and 3 relative to round 1 between each treatment condition and its corresponding control condition. The results for each analyzed policy are presented in Table 10. Thereafter, by taking advantage of the panel structure of our data, we conducted a regression analysis using a difference-in-differences (DD) approach. Since the share of land allocated to forest was censored at 0 and 8 hectares, we conducted a random effects Tobit model for each analyzed policy. Each model included two dummy variables (*Policy* and *Post-policy*) that represented each stage

and another dummy variable that represented the treatment condition (*RD*). We interacted the *RD* variable with the two stage dummies to determine the effect that facing a context of disadvantage had on forest conservation in the policy and post-policy stages. As mentioned in Section 3.1, we included additional covariates as controls for the analysis of the setting under no policy. The results of regression analysis are presented in Table 11.

Table 10. Average behavioral spillovers by stage and analyzed policy condition.

Analyzed policy	No Policy		Fine policy		Policy mix	
Condition	Baseline-RD (n = 31)	Baseline (n = 36)	Fine-RD (n = 31)	Fine (n = 36)	Mix-RD (n = 30)	Mix (n = 34)
Average change in forest area ΔF_{12}	-0.45	0	1.58	0.29	0.60	1.09
Spillover policy stage $\Delta F_{12,T} - \Delta F_{12,C}$	-0.45		1.29**		-0.49	
Average change in forest area ΔF_{13}	-0.22	0.05	0.61	-0.33	0.43	0.37
Spillover post-policy stage $\Delta F_{13,T} - \Delta F_{13,C}$	-0.27		0.94		0.06	

Note: ΔF_{1i} calculates the size of the average change between round i $(i = 2,3)$ and round 1 for each condition. $\Delta F_{1i,T} - \Delta F_{1i,C}$ calculate the average behavioral spillover as the difference in the size of the former average changes between each treatment condition (T) and its respective control group (C). Asterisks imply statistically significant differences based on a Mann-Whitney U test (*** p-value <0.01, ** p-value <0.05, * p-value <0.1).

Here, we discuss our results regarding H1 and H2. We begin with spillover on the behavior of subjects that faced no policy. That is, we compared the behavior of subjects that were excluded from a PES policy (*Baseline-RD*) with that of those who did not face any exclusion (*Baseline*). According to Table 10, farmers in the *Baseline-RD* condition conserved 0.45 fewer units of forest on average in round 2 relative to round 1, while in the *Baseline* condition there was no change in the average level of conservation between rounds 2 and 1. Furthermore, for the post-policy stage, the slight average increment of 0.05 units observed in this control group between rounds 1 and 3 decreased by 0.27 units when farmers faced a disadvantageous context. The direction of these effects is in line with our hypotheses noting that being in a disadvantageous context (i.e., being excluded from PES in this case) reduces conservation. However, none of these relative differences were statistically significant (Mann-Whitney U test, p-value round 2=0.430, p-value round 3=0.329). These results were confirmed by the regression analysis, which showed that although the coefficients for the interaction between *RD* and *Policy* and between *RD* and *Post-policy* in the setting under no policy were negative, they were not statistically significant (see Table 11). Thus, we reject H1 and H2 for farmers that faced no policy.

Next, we examine the spillover effects for the case of the fine policy. Again, we compared the control condition (i.e., the fine policy was implemented for all farmers) to the treatment condition (i.e., neighboring farmers received PES instead). Contrary to our expectations, the results shown in Table 10 suggest that the average changes in rounds 2 and 3 relative to round 1 were always greater for farmers in the *Fine-RD* condition in comparison to farmers in the *Fine* condition. Specifically, we observed a further average increase of 1.29 units of forest in round 2 and of 0.94 units in round 3. Furthermore, unlike in the

Fine condition in which forest area in round 3 decreased compared to round 1, farmers in the *Fine-RD* condition increased their forest area, on average. Although only the relative average increment in round 2 was statistically significant (Mann-Whitney U test, p-value round 2=0.037, p-value round 3=0.155), the regression analysis confirmed the effect for both stages: the coefficients for the interaction between *RD* and *Policy* and between *RD* and *Post-policy* were both positive and statistically significant (at the 5 and 10% levels, respectively; see Table 11). Thus, we reject H1 and H2 for farmers that faced a fine policy. While we found that the disadvantageous context does have a significant spillover on behavior, the direction of the effect is contrary to what we expected.

Table 11. Random effects Tobit estimates of DD models per analyzed policy.

Dependent variable: *Forest area*	No policy		Fine policy		Policy mix	
RD	0.554	(0.701)	-0.069	(0.652)	0.191	(0.764)
Policy	0.111	(0.331)	0.481	(0.412)	1.495***	(0.397)
Policy x RD	-0.611	(0.475)	1.483**	(0.617)	-0.696	(0.581)
Post-policy	0.174	(0.331)	-0.384	(0.413)	0.564	(0.390)
Post-policy x RD	-0.415	(0.476)	1.082*	(0.611)	-0.068	(0.574)
Constant	5.108***	(0.959)	3.924***	(0.532)	3.749***	(0.697)
Control variables	YES		YES		YES	
Observations	201		201		192	
Number of id	67		67		64	
Prob > chi2	0.067		0.000		0.004	

Note: Control variables included in the analysis of No policy included perception of the state of the environment, level of trust in the environmental authority, and some sociodemographic information. Control variables included in the analysis of Fine policy and Policy mix included level of risk aversion (see Appendix D for the full version showing all coefficients). Standard errors in parentheses. Asterisks imply statistically significant coefficients (*** p < 0.01, ** p < 0.05, * p < 0.1).

Finally, we examined the behavioral spillover effects for the case of the policy mix (fine + PES). We compared the control condition (i.e., where one area faced this policy combination) to the treatment condition (i.e., where a neighboring area received only PES but no fine). Compared to when farmers faced an egalitarian context (*Mix* condition), we found that farmers increased their units of forest less in round 2 relative to round 1 when facing a disadvantageous context (*Mix-RD* condition). The average increase of 1.09 units between rounds 1 and 2 observed in the *Mix* condition decreased by an average of 0.49 units in the *Mix-RD* condition (see Table 10). Although this effect is in line with H1, it was not statistically significant (Mann-Whitney U test, p-value=0.351). Additionally, the increase between rounds 1 and 3 observed in the *Mix* treatment was very similar to that of the *Mix-RD* treatment. The coefficients for the interaction between *RD* and *Policy* and between *RD* and *Post-policy* in the regression analysis also went in the hypothesized direction (i.e., they were negative). However, they were not statistically significant (see Table 11). Thus, we reject H1 and H2 for farmers that faced a policy mix.

In summary, while the direction of effects appears to be in line with H1 and H2 for the cases of no policy and policy mix, the effects were not significant, which leads us to reject these hypotheses. For the case of the fine policy, our results also reject H1 and H2 but suggest a significant effect in the opposite direction, (i.e., positive spillover on conservation behavior). Although we further discuss our results in Section 5, we first proceed to examine the impact of experiencing the disadvantageous context on fairness perceptions in the next subsection.

4.3.2.2. Fairness considerations and its role as moderators

We hypothesized that farmers are less likely to perceive a policy as fair when they face it in a disadvantageous context. We first examine the proportion of farmers who found the policy setting implemented in their session fair (see Table 12). We do not report fairness perceptions in the *Baseline* control condition because no policy was implemented. We compared the proportion between treatment and control conditions under the fine policy and the policy mix. For the policy mix, and in line with our expectations, a lower proportion of farmers found the policy fair when they faced it in a disadvantageous context (*Mix-RD* condition) than when they faced it in an egalitarian context (*Mix* condition). Nevertheless, this difference in proportion was not statistically significant. For the case of the fine policy, the results were surprising. The largest proportion of farmers who considered the policy implemented in their session fair was observed in the *Fine-RD* condition, and this proportion was significantly higher than the one observed in the *Fine* condition. Therefore, our results reject H3 and indicate the opposite effect for the fine policy. This suggests that our unexpected result for H1 and H2 among farmers under the fine policy may be due to an unexpected effect of the setting on fairness perceptions. We discuss this result further in Section 5.

Table 12. Proportion of farmers who found the setting implemented in their session fair.

Condition	N	Farmer found it fair that …	Proportion	Fisher's exact test p-value (treatment vs control)
Baseline	36	-	-	-
Baseline-RD	31	others received payments to reduce cattle farming while they were excluded from receiving the payments	0.48	
PA	36	they were fined for cattle farming	0.42	0.087
PA-RD	31	they were fined for cattle farming while others received payments to reduce cattle farming	0.61	
Mix	34	they were fined for cattle farming and simultaneously received payments to reduce cattle farming	0.53	0.401
Mix-RD	30	they were fined for cattle farming and simultaneously received payments to reduce cattle farming, while others only received payments to reduce cattle farming without fines	0.47	

Note: Fairness perceptions in the *Baseline* condition are not reported because no policy was implemented in this condition.

Next, we examine the relationship between fairness preferences regarding disadvantageous inequality aversion (hereafter "fairness preferences") and fairness perceptions toward the implemented policy

setting (H4). We used farmers' estimates of their levels of disadvantageous inequality aversion resulting from the incentivized tasks implemented in the pre-experimental survey (see Section 2.2) to define a dummy variable *DI Averse* that takes the value 1 if a farmer had any degree of aversion to disadvantageous inequality ($DI\ Aversion > 0$) and the value 0 otherwise (see Appendix 6 for more details). In other words, this dummy variable measured whether a farmer had a preference for fair outcomes (in the sense of reduced disadvantageous inequality) or not. Then, we used a probit model to test whether being inequality averse reduces the likelihood that a farmer perceives the implemented policy setting as fair in a disadvantageous context compared to in an egalitarian context. Thus, we look at the interaction between the dummy variables *RD* and *DI Averse*. Since we did not measure fairness perceptions in the *baseline* condition, our analysis focused on the other two policy settings. Table 13 presents the two resulting regressions. In line with our expectations, the coefficients of the interaction between *RD* and *DI Aversion* were negative, which suggests that inequality-averse farmers showed a larger reduction in fairness perceptions when the policy setting was disadvantageous in comparison to farmers who were not inequality averse. However, since this effect was only statistically significant in the fine policy model, our results only support H4 in the case of the fine policy.

Table 13. Probit estimates of fairness perceptions toward the policy setting.

Dependent variable: *Fair*	Fine policy		Policy mix	
DI Averse	0.431	(0.458)	-0.283	(0.425)
RD	1.272**	(0.525)	-0.103	(0.423)
DI Averse x RD	-1.430**	(0.313)	-0.037	(0.682)
Constant	-0.431	(0.275)	0.199	(0.239)
Observations	67		64	
Prob > chi2	0.094		0.763	

Note: Robust standard errors are clustered at the group level and in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Finally, we tested whether having a preference for fair outcomes moderated the behavioral spillover effects (H5 and H6). To achieve this, within each condition, we tested whether the behavioral spillover was stronger on farmers who were inequality averse in comparison to those who were not. Thus, we estimated the heterogeneous spillover effect, captured by a triple interaction estimate resulting from interacting the dummy variable *DI Averse* with the condition and stage dummy variables. The three resulting regressions are presented in Appendix D. Table 14 presents only the triple interaction estimates together with the marginal effects of stage on forest conservation behavior by fairness preferences and condition.

Notably, fairness preferences moderated some of the behavioral spillovers. We first focused on the case of no policy. In line with our expectations, Table 14 shows that the triple interaction estimates for the baseline conditions were negative at both the policy and post-policy stages. However, only the post-policy stage estimate was statistically significant. When we compared the corresponding marginal effects between the two conditions, we observe that in the *Baseline* condition, fairness preferences did not predict changes forest levels. Conversely, in the *Baseline-RD* condition, once the PES policy ended, forest

conservation levels were predicted to significantly decrease among farmers who were inequality averse (compared to the pre-policy stage), while they were predicted to significantly increase in those not averse.

We now move on to the cases of the fine policy and policy mix. Contrary to the case of no policy, all of the triple interaction estimates were positive but only the post-policy stage estimate in the case of the fine policy was statistically significant. When we compared the marginal effects between the *Fine* and *Fine-RD* conditions for the post-policy stage, we observed that farmers who were inequality averse only reduced their forest conservation levels in comparison to their pre-policy stage levels when facing the egalitarian context. Thus, facing a disadvantageous context seemed to alleviate retaliation behavior toward the fine policy among farmers who were inequality averse. This is in line with our previous results of a significant positive spillover effect on conservation behavior in the *Fine-RD* condition when compared to the *Fine* condition (see Section 3.2.1), suggesting that fairness preferences moderated this unexpected result. However, since this effect goes in the opposite direction to our hypotheses, our results lead to rejection of H5 for all cases and only support H6 for the case of no policy.

Table 14. Triple interaction estimates and marginal effects of stage on forest area by fairness preferences and condition.

Dependent variable: *Forest area*		No policy		Fine policy		Policy mix	
Policy stage							
Triple interaction estimate		-0.362	(1.071)	0.636	(1.245)	0.398	(1.179)
Average marginal effects							
Control condition	*Not averse*	0.120	(0.585)	1.028*	(0.566)	1.655***	(0.534)
	Averse	0.104	(0.388)	-0.093	(0.572)	1.296**	(0.590)
Treatment condition	*Not averse*	-0.206	(0.716)	2.152***	(0.659)	0.667	(0.650)
	Averse	-0.583	(0.377)	1.787***	(0.609)	0.899	(0.560)
Post-policy stage							
Triple interaction estimate		-2.169**	(1.069)	2.953**	(1.232)	0.892	(1.161)
Average marginal effects							
Control condition	*Not averse*	-0.025	(0.538)	0.615	(0.565)	0.798	(0.519)
	Averse	0.253	(0.014)	-1.415**	(0.576)	0.260	(0.588)
Treatment condition	*Not averse*	1.222*	(0.156)	0.539	(0.645)	0.362	(0.647)
	Averse	-0.667*	(0.355)	0.834	(0.600)	0.597	(0.551)
Observations		268		201		192	
Number of id		67		67		64	

Note: See Appendix D for the full version showing all coefficients. Standard errors in parentheses. Asterisks imply statistically significant coefficients or marginal effects (*** p < 0.01, ** p < 0.05, * p < 0.1).

4.4. Discussion and conclusion

We conducted a lab-in-the-field experiment to determine whether implementing PES in a specific area could have behavioral spillovers on the behavior of those who are next to this area and end up in a

disadvantageous condition relative to PES recipients. In particular, we analyzed three policy scenarios: 1) a high priority area where PES are implemented next to a low priority area excluded from PES (comparing *Baseline-RD* to *Baseline* conditions); 2) a protected area with land-use restrictions surrounded by a buffer area where PES are implemented (comparing *Fine-RD* to *Fine* conditions); 3) a protected area with PES on top of land-use restrictions, which is surrounded by a buffer area with only PES (comparing *Mix-RD* to *Mix* conditions).

While the direction of the spillover effects on those excluded from PES was found to be in line with our hypotheses, the effects were statistically insignificant. This could be due to Alpízar et al.'s (2017a) impression that PES exclusion is less likely to have spillover effects when PES targeting is based on a farm's location and its relevance to environmental conservation in comparison to other targeting criteria based on the history of conservation efforts. However, although we did not find an overall spillover effect, we observed a negative spillover after the PES policy ended on the behavior of excluded farmers who were inequality-averse farmers. Therefore, another potential explanation for our non-significant result could be that the present study lacked statistical power. While our sample was large for a field experiment in the Global South, it was small in terms of statistical power. In any case, our findings highlight the importance of analyzing behavioral responses once policies expire and the relevance of addressing concerns about equity since these factors could moderate behavioral spillovers.

Our findings also surprisingly suggest that the spillover effect does not increase with the level of disadvantage within each context. Thus, farmers who experienced the most restrictive policy had better conservation outcomes and were more likely to find the policy fair when faced with it in the disadvantageous context (i.e., *Fine-RD* condition) than when faced with it in the egalitarian context (*Fine* condition). Furthermore, even though inequality averse farmers were significantly less likely to find the policy fair in the disadvantageous context, we only observed retaliatory behaviors in inequality-averse farmers in the egalitarian context. Upon examining the reasons why farmers found the setting implemented fair or unfair, we propose Martin et al.'s (2018) "system justification" theory as the main psychological mechanism behind this unexpected effect.

Beyond institutional and ideational reasons, Martin et al. (2018) suggested that people also have a psychological reason to support protected areas—what the author calls "system justification". Although people often express resentment for a protected area because it severely limits their ability to benefit from forest resources, when it is imposed on them to follow the rules of the protected area, they tend to internalize the positive outcomes of supporting the protected area to resolve that cognitive dissonance. When we asked farmers to explain why they considered the fine policy fair, the most frequent answer was the importance of conserving forests for water and the environment. This fits well with the theory of system justification. Given that a significantly larger proportion of farmers found the fine policy fair when they faced it in a disadvantageous context than when they faced it in an egalitarian context, the *Fine-RD* condition must be inducing more system justification than the *Fine* condition. To examine this more closely, we explored the relationship between environmental awareness and the probability of finding the implemented policy setting fair. We found that the probability of finding the fine policy fair in the *Fine-RD* condition increased significantly by 35 points among farmers with greater environmental awareness compared to less environmentally aware farmers. In contrast, in the *Fine* condition, the level of environmental awareness was not correlated with the probability of finding the fine policy fair (see Appendix E for details). This seems to support the proposal that system justification plays a stronger role in the disadvantageous context.

Additionally, 71% of farmers in the *Fine* condition felt that the fine policy was unfair because it did not consider the need for a balance between forest conservation and cattle farming. Of these, 46% considered it legitimate to only sanction those who cut down large amounts of forest and 40% mentioned the need for compensation. These statements are in line with previous findings in the area, which recognized that farmers are aware of the relevance of protecting the forest and the páramo ecosystem, and that by viewing livestock farming as a subsistence activity, they oppose restrictions when there is no effective compensation or viable alternative solutions (Instituto Alexander von Humboldt, 2016). Furthermore, farmers disagree with the implementation of absolute land-use prohibitions since they perceive cattle farming to be part of their identity (Ibid). This could explain why they considered the Fine setting (i.e., where restrictions are imposed in both areas) as unfair. In contrast, upon examining farmers' reasons for finding the fine policy fair in the *Fine-RD* condition, only 17% mentioned the need for a balance between forest conservation and cattle farming. Instead, 67% mentioned that payments should be offered to all participants, while half of them highlighted the fact that they were also conserving the forest. This change in farmer statements also translated into significantly different forest distributions between the two conditions at the policy stage (two-sample K-S test, p-value exact: 0.079). Once the fine policy was implemented, there was an increment in the frequency of choosing an equitable distribution (50-50) between forest conservation and cattle farming in the *Fine* condition, while there was also an increment in the frequency of choosing to conserve all forest area in the *Fine-RD* condition (see Appendix E for details).

Therefore, we propose that the introduction of PES in a neighboring area diverted farmers' attention from defending their right to balance between forest conservation and cattle farming toward signaling their behavior in favor of conservation in order to justify their own right to receive PES. In this sense, the expectation of future payments seems to have increased their obligation to follow the rules, which seems to have led to the greater internalization of the positive aspects of the protected area.

Finally, when PES were implemented on top of land-use restrictions (*Mix* and *Mix-RD* conditions), the spillover effects had the expected negative direction but were not statistically significant. While half of the farmers in the egalitarian context again stressed their right to engage in subsistence cattle farming, 75% of farmers in the disadvantageous context complained about not having the same conditions for everyone. Of these, 58% had explicit feelings of resentment about neighboring farmers receiving payments and demanded that neighboring farmers also be fined for clearing their forests. This suggests that, in this context, PES may no longer be perceived as a rewarding mechanism to conserve forests but as a compensation mechanism for the loss of usage rights. Thus, offering PES to those who are not being regulated raises fairness concerns and triggers retaliatory behavior. In this sense, examining a case in which PES are first implemented in the buffer area and later offered within the protected area could thus be a promising extension of the present work.

In conclusion, our results support our hypothesis that PES could have spillovers on conservation behavior in neighboring areas when they generate a context that is perceived as unfair by policy addressees. However, what is considered to be fair, and—the direction of this spillover effect—is likely to be context-dependent. Our research hypotheses were based on our own logic of what fairness is. However, we found that this logic of fairness was not always aligned with those of the farmers. Thus, we underline the need to comprehend the local logics of fairness before PES implementation.

Chapter 5 Conclusions

In this thesis, I examined whether economic incentives could be more or less effective in the short- and long-term when adjusting non-monetary factors in their design or implementation context. In particular, I evaluated the relevance of three non-monetary factors for the design and implementation of PES.

In Chapter 1, I presented the conceptual framework, summarized the contributions to the literature, listed the research questions, and explained the methodology for addressing them. In each of the three subsequent chapters, I presented each of the research articles of this thesis in detail. In each article, supported by my co-authors, I evaluated the importance of a specific non-monetary factor for policy effectiveness. In particular, in Chapter 2, I examined the role of the policy's framing. In Chapter 3, I studied the relevance of the social distance that exists between ecosystem service providers and beneficiaries. Finally, in Chapter 4, I analyzed the effect of fairness concerns when implementing a policy in an unequal institutional context.

In this concluding chapter, I summarize the key findings of this thesis. I start by answering the research questions posed in Chapter 1 and, to conclude, I provide a critical reflection about the limitations of my research approach.

5.1. Answering the research questions

In order to present the main insights of this thesis, I respond in this section to the research questions posed in Chapter 1 through a summary of the results found in each corresponding chapter. I start with the two research questions addressed in Chapter 2.

RQ1.1: Does changing the terms used to denote the payment affect whether and to what degree PES have a motivational crowding effect in the short or long term?

The results presented in Chapter 2 suggest that although the term used to denote the payment did not have an effect in the short term, it was relevant for motivation crowding in the long term. In particular, framing the payment as a prize that acknowledged farmers' forest conservation achievements was associated with a crowding in effect in the long term, relative to a control framing in which the payment was framed as a payment per hectare of forest conserved. By contrast, framing PES as a compensation for loss and effort incurred during forest conservation was not associated with a crowding effect relative to the control framing. These results support the hypothesis that the choice of words conveys different objectives, meanings and/or expectations for farmers. More specifically, although both framings reduced the focus on economic reasoning, the compensation framing may have increased farmers expectations of future payments (i.e., to demand the right for compensation) while the prize framing may have increased farmers' feelings of competence.

RQ1.2: Does emphasizing other values beyond targeted marketable ecosystem services to justify environmental conservation affects whether and to what degree PES have a motivational crowding effect in the short or long term?

The results presented in Chapter 2 suggest that emphasizing additional values of nature during PES implementation did not have an effect in the short term. In the long term, the results weakly support the hypothesis that an emphasis on the cultural ecosystem services obtained from forests could induce a crowding-in effect relative to a control framing that only emphasized the targeted water services.

Exploratory analyses suggest that by increasing the awareness about the cultural benefits provided by nature, the framing could have been reinforcing the conservation behavior of participants whose conservation motivations were more aligned with the cultural framing manipulation, without detrimental effects in the behavior of those whose motivations were not aligned.

Combining the results on RQ1.1 and RQ1.2, I thus conclude that the framing used to communicate a policy is a relevant and relatively cheap tool that, when properly designed, could increase policy effectiveness.

Subsequently, in Chapter 3, I addressed the following research question:

RQ2: Do the short- or long-term motivational crowding effects of a PES policy depend on the geographical identity of the beneficiaries of ecosystem services?

According to the results presented in Chapter 3, the geographical identity of downstream beneficiaries was not relevant, overall, to the degree of motivational crowding that PES could produce. Indeed, downstream beneficiaries' geographical identity did not affect PES effectiveness in the short or long term. Nevertheless, the results suggest that PES could crowd out intrinsic motivations in farmers with weak place identity who provide water to people living in a different geographical location. By contrast and surprisingly, PES increased water provision in those with already high provisioning levels, i.e., farmers with a strong place identity who provided water to people living in the same geographical location.

Combining these two results, I thus conclude that in the absence of a well-defined group of downstream beneficiaries financing a PES program, it could be better to frame the PES program in terms of the benefits provided to people with whom upstream providers feel more socially identified, rather than in terms of the benefits to actors within hotspots of water use (e.g. larger cities), in order to increase its take-up and effectiveness.

Lastly, in Chapter 4, I addressed the following research question:

RQ3: Are short- or long-term motivational crowding effects in one area affected by whether the same or different policy conditions prevail in a neighboring area? Does the level of inequality between the policy conditions on each area affect the degree to which the above effect occurs?

The results presented in Chapter 4 provide evidence that the motivational crowding effect of a pecuniary incentive in one area could be affected by the policy conditions that prevail in a neighboring area, however, the effects do not necessarily increase with the level of inequality between external interventions. Surprisingly, implementing a weakly enforced fine in a disadvantageous institutional context (i.e., PES were being implemented in a neighboring area) significantly crowded in forest conservation in the short and long term relative to a weakly enforced fine implemented in an egalitarian context. In parallel, this disadvantageous institutional context also increased fairness perceptions towards the fine. In the other two additional settings, where the level of inequality in the institutional context between neighboring areas was less pronounced, having PES in the neighboring area tended to decrease both fairness perceptions and conservation behavior in the long term, as expected. Therefore, the results in Chapter 4 suggest that although the institutional context in which a policy is implemented is relevant to motivation crowding, what is considered to be fair is likely to be context-dependent.

In conclusion, Chapter 4 stress the relevance of considering the institutional context as a relevant factor when defining the targeting criteria for the implementation of an external economic incentive. A special

effort should be made to understand the local logics of equity in order to better anticipate potential fairness concerns about the external economic incentive in each institutional context.

5.2. Reflection on methodological approach

The validity of a study, i.e., whether the results of a study are trustworthy and meaningful, includes two domains: internal and external validity. A high degree of internal validity allows to establish a causal relationship between a treatment and the outcome variable. The less chance there is for "confounding", the higher the internal validity and the trustworthiness of the results. In this thesis, I increased the internal validity of my results relative to previous studies, by fixing the level of incentive between treatments and using a dictator game, in order to disentangle the motivational crowding effect of an external economic incentive from its relative-price effect (see section 1.3). In addition to this, I randomly assigned participants to treatment and control groups, and collected several aspects of information about participants in order to test the randomization process and include covariate controls in cases of randomization failure.

Likewise, a high degree of external validity allows the generalizability and transferability of results, i.e., how applicable the findings are to the real world. In this thesis, I aimed to increase the external validity of my results by working in an area of environmental relevance but with little history of environmental policy interventions (see section 1.5.2). In addition to this, I also framed the lab-in-the-field experiment to recreate a relevant situation that was familiar to participants, and did a tremendous effort to sample all households living in the study area, visiting each household directly and limiting participation to one representative per household to reduce selection bias.

Nevertheless, despite the aforementioned efforts to increase the internal and external validity of this thesis, the results were not exempt of limitations. While the sample was large for a field experiment in the global South, one of the major limitations of my results is the small sample size. Paradoxically, the historical isolation of the study area as a consequence of the civil conflict in Colombia, which I initially saw as an advantage for external validity, actually translated into a great barrier for conducting the research. Although the study area has been free of armed conflict for more than a decade, it is still characterized by a general atmosphere of distrust and suspicion towards outsiders. This could limit the generalizability of the results, since it is possible that there are differences in the levels of social capital and collective action in this area compared to other areas. In the end, we succeeded in convincing a large percentage of households to participate. Still, the duration of the fieldwork was significantly prolonged and, given that participation in the experiment was limited to one representative per household, the sample size obtained was still small in terms of statistical power. Therefore, in the future, it may be preferable to partially sacrifice external validity by allowing two or more household members to participate, in order to ensure a high statistical power.

Furthermore, despite the previously mentioned efforts to reduce selection bias, attrition (i.e., loss of participants) was not avoided. Although the drop out percentage was not very large (between 5% and 16%), the comparison of participants with those who dropped out showed that the latter reported lower levels of trust and cooperation in the pre-experimental survey.

A final limitation on the validity of my results was the general framing of the modified dictator game. While the game managed to successfully recreate a real situation for farmers, it failed to include a crucial aspect about the value of cattle farming to the community: that it is part of participants' social identity.

Since cattle farming is part of the culture and social welfare of the community, a high percentage of participating farmers did not change their decisions during the experiment, many of them anchoring themselves to the decision to equally distribute (50-50) their land between cattle farming and forest. This absence of changes in decisions also influenced the statistical power of the sample by reducing the effect size of the treatment. In the future, a framing that includes subsistence cattle farming could more effectively measure a treatment effect by preventing or reducing the anchoring of decisions.

5.3. Future avenues of research

The findings of this thesis identify specific avenues for future research. In particular, in Chapter 2, I identify two avenues. First, the exploratory analysis suggests that the framing manipulation had a stronger effect on farmers who reported reasons for taking pro-environmental actions that were aligned with the framing manipulation, in line to recently published studies (Lliso et al., 2021) and previous suggestions of environmental relatedness acting as a moderator of the crowding in of intrinsic motivations (Ezzine-de-Blas et al., 2019). However, since farmers' personal statements were only selected from a list of previously defined pro-environmental actions and reasons for taking such actions, they could be poor proxies for competent and social relatedness moderators. Thus, other methodologies, such as the pyramidal response format used by Lliso et al. (2021) to measure human-nature relational models, or the motivations survey used by Moros et al. (2019), could better guide in the future the analysis of the psychological moderators behind motivational crowding effects. Secondly, the results from that chapter also suggest that the compensation framing was ineffective overall: once introduced, it did not increase the average levels of forest conservation relative to the baseline round. A possible explanation for this result is that the level of the incentive could have been perceived as to low to be fair. Since the existence of fair and just relationships between individuals and society has also been suggested to moderate motivational crowding (Ezzine-de-Blas et al., 2019), this could have led the PES policy to be ineffective. Future studies that investigate the interaction between the level of the incentive and the policy framing could help test this hypothesis.

Similarly, the results from Chapter 3 suggest that PES crowded out intrinsic motivations in farmers with weak place identity who provided water to beneficiaries geographically distant to their own location (i.e., people living Bogotá). Furthermore, PES were effective even in farmers with strong place identity who provided already a high amount of water to beneficiaries living in the same municipality before PES implementation. A possible explanation for these results is that intrinsic motivations non-related to social identity and parochialism could be more easily replaced by extrinsic motivation than intrinsic motivations based on social identity. Still, future studies are needed to test this hypothesis. In this chapter, some new methodological avenues were also proposed. For example, the use of other experiments such as the comparative dictator game (Candelo et al., 2018)—which is based on a within-subject design—could reduce statistical power limitations. In addition, the use of visual fusion scales (Purzycki and Lang, 2019) could capture more accurately social closeness between providers and beneficiaries.

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Appendix A Field materials

- **General experiment instructions**

THE EXTERNALITY GAME
BOGOTÁ BENEFICIARIES – PES POLICY WITH NEUTRAL FRAMING

[This experiment will be played individually. Before you start make sure you have all laminated formats: 1) Watershed 2) Baseline – Bogotá 3) PES – Bogotá]

Good morning. We want to first thank you for participating in this study. Because you can earn a considerable amount of money, it is very important that you pay close attention to these instructions. In case there is something you do not understand during the instructions, we invite you to raise your hand and we will be glad to respond to any question. We ask you to turn off your mobile phones to avoid any distractions during this exercise.

This exercise should not last more than one hour. After that, we will prepare your payments, ask you a few more questions and then you can go home with the money you earned. You are free to leave in case you decide to do so, but if you decide to do so, you will decline to receive the earnings from this exercise.

You will have to make several simple decisions during this exercise. All decisions will be maintained confidential and will be made in private. That means that we won't tell what you decided nor what other participants decided.

This exercise tries to recreate a real situation of a farmer's daily life regarding the use of their land. You have been selected to participate in this particular exercise, which is different from other exercises you or other people you know participated in before. Therefore, any comments you may have heard won't necessarily apply to this exercise. Thus, it is important that you make every decision carefully and based only on your own criteria.

You will make decisions in three rounds. Each round represents a different productive season. However, you will only be paid for one of these three rounds. This round will be determined by you at the end of this exercise by selecting randomly a ball from a bag with 3 balls numbered from 1 to 3. For this reason, it is important that you take every decision seriously as you will win your earnings from only one of the rounds.

[From now on make reference to the Watershed laminated form]

This exercise is about water production and its relation to old-growth forest conservation. Each round you will have 8 hypothetical hectares of land with conserved old-growth forest upstream of a watershed. Think of this land as something that you received as an inheritance. Each round, you need to decide how many of these 8 hectares of land with old-growth forest you want to keep conserved and how many of them you want to use for livestock farming.

In order to keep each hectare of old-growth forest well conserved you can only use it to produce a few crops for self-consumption. Thus, each hectare of conserved old-growth forest gives you 0,45 € profit to you *[Show this in the form]*. At the same time, upstream conservation of old-growth forest produces drinking water for human consumption downstream, because it prevents soil erosion and sediment flow.

Thus, each hectare of conserved old-growth forest produces an amount of water that represents 1,20 € profit to the people downstream *[Show this in the form]*.

On the other hand, you can also choose to clear the old-growth forest and instead use the land for livestock farming. Livestock farming is more profitable to you than conserving the old-growth forest as you can sell milk and meat in the market. Thus, each hectare with livestock farming represents 1,50 € profit to you *[Show this in the form]*. However, clearing the old-growth forest upstream reduces water quality for the people that live downstream, as the soil erodes and more sediment falls into water. As you can see in the picture *[Read while showing this in the form]*, as the old-growth forest is being replaced by livestock farming, the soil captures less water and erodes, there are more landslides and water accumulates more and more sediments until it is no longer suitable for human water consumption downstream. Thus, each hectare with livestock farming produces water that is NOT suitable for human consumption downstream, which translates in no profit (0 €) to the people living downstream *[Show this in the form]*.

Based on the later information I have a few questions for you: How much profit does livestock farming give you? *[Wait for participant to answer and then show this in the form]*

How much profit does old-growth forest conservation give you? *[Wait for participant to answer and then show this in the form]*

Is old-growth forest conservation more profitable to you than livestock farming? *[Wait for participant to answer and then show this in the form]* Why? *[Wait for participant to answer and then explain if needed]*

How much drinking water for human consumption, in monetary terms, is produced from livestock farming? *[Wait for participant to answer and then show this in the form]* Why? *[Wait for participant to answer and show this in the form]*

And old-growth forest conservation, how much drinking water for human consumption, in monetary terms, does it produce? *[Wait for participant to answer and show this in the form]* Why? *[Wait for participant to answer and show this in the form]*

Summarizing *[Use the form to summarize]*, old-growth forest conservation upstream keeps water clean and in good quantity for the people living downstream. However, conserving the old-growth forest is expensive for upstream farmers like you since livestock farming is more profitable, it gives you 1,50 € per hectare, while conserving old-growth forest only gives you 0,45 € per hectare.

Any questions so far? *[Wait and see if there are questions.]*

If there are no further questions I'm going to explain in detail who will be the people downstream benefiting from your decisions.

As I said before, you will represent a farmer living upstream. Downstream households will be households from the city of Bogotá. After this exercise is over, we are going to give to one household from the city of Bogotá all the profits resulting from the water produced by you from the total amount of hectares of old-growth forest that you decided to conserve, if any. Remember that we will only choose randomly one of the three rounds to pay you, this will also be the case for the household from the city of Bogotá, who will receive resulting profits from the same round that we payed you for.

For example, let's suppose that in the round we selected to pay, you conserved 5 hectares of old-growth forest. These 5 hectares of old-growth forest multiplied by 1,20 € of water produced by each hectare, produce a total amount of 6,00 € of water. This 6,00 € will be given to a household from the city of Bogotá.

The household that will benefit from your decisions will be randomly selected from a pool of households from the neighborhood Rincón de Bolonia in the city of Bogotá. In order to make sure that the selected urban household receives the water, represented in money, you produced in this exercise, we will credit that amount to the selected household water bill. Think about it as if that household from Bogotá won't have to pay the cost of cleaning the water because you kept the old-growth forest conserved, and therefore the water was clean. In the later example, the selected household would have payed 6,0 € less on next month's water bill. This was just a hypothetical example. There is no right or wrong decision. Please feel free to choose any amount of hectares you want.

Any questions? *[Wait and see if there are questions.]*

If there are no further questions. Now I'm going to explain in detail how this exercise works.

[From now on make reference to the laminated Baseline – Bogotá form]

As mentioned before, you will have to make decisions on three rounds. Each round you will be the owner of 8 hectares of upstream land with conserved old-growth forest that you received as an inheritance. You need to decide how many of these hectares you want to keep with old-growth forest and how many of these hectares you want to switch to livestock farming. In order to make your decision, you will receive a form that looks exactly like this laminated form *[Show the laminated form]*.

This form has all possible 9 options of how you can use your 8 hectares of land *[Show each option in the form]*. On the left side of each possible option you have all your eight hectares of land with old-growth forest or cattle, and below each hectare you have the amount you will earn from it *[Show in the form an example of old-growth forest and another of cattle]*. On top of your eight hectares you can see the total amount of money you will earn given the distribution of old-growth forest and cattle on your farm. If you choose to keep the old-growth forest on all your 8 hectares you will get 3,60 €. As you start clearing the old-growth forest and introducing livestock farming you start winning more money, because livestock farming is more profitable than old-growth forest conservation, up to 12,00 € when you have cleared all old-growth forest and used your 8 hectares for livestock farming *[Use the form to show this]*.

On the right side of each possible option you can see how much water will be produced and how much money you will send to the household from the city of Bogotá. If you choose to keep the old-growth forest on all your 8 hectares you will send the highest amount of water to the household from the city of Bogotá, which corresponds to 9,60 €. As explained before, when you start clearing the old-growth forest and introducing livestock farming you start sending less and less water to the urban households until it reaches 0 € when you have cleared all old-growth forest and used your 8 hectares for livestock farming *[Use the form to show this]*.

In order to let us know the option you want to choose, we ask you to make a cross inside the square below the option you prefer *[Show the square in the form]*. For example, let's suppose I want to use 6 hectares for livestock farming, this means I will leave 2 hectares with old-growth forest. Which option must I choose? *[Wait for participant to answer and then make a cross on the form]* How much money will I earn?

[Wait for participant to answer and show it on the form] How much water represented in money will I send to the household from the city of Bogotá? *[Wait for participant to answer and show it on the form.]*

[Use another marker color] Now, let's suppose I want to use 3 hectares for livestock farming, this means I will leave 5 hectares with old-growth forest. Which option must I choose? *[Wait for participant to answer and then make a cross on the form]* How much money will I earn? *[Wait for participant to answer and show it on the form]* How much water represented in money will I send to the household from the city of Bogotá? *[Wait for participant to answer and show it on the form.]*

Notice that I earn less money now, in comparison to the previous decision of using 6 hectares for livestock farming, given that conserving the old-growth forest is less profitable *[Show it on the form.]* Also notice that now I send more water to the household from the city of Bogotá in comparison to the previous decision of using 6 hectares for livestock farming, since conserving old-growth forest produces clean water for downstream households *[Show it on the form.]*

Summarizing, the form to make your decisions has 9 different options, one for every combination you can have between old-growth forest and cattle on your farm. The number above your farm description is the sum of all earnings per hectare and will be the amount you win in each round. Next to your farm description, you will find the amount of water, represented in money, that the household from the city of Bogotá will get given the combination of old-growth forest and cattle on your farm. As you go down on the form you earn more money and at the same time you send less water represented in money to the household from the city of Bogotá. *[Show all this in the form, highlighting the places with the information of what the participant earns and what the beneficiaries earn.]*

Remember that you will earn money based on your decisions. It is important that we make sure that you understand how this exercise works. Are there any questions? *[Wait and see if there are questions]*

[Delete your decisions from the form] Now, to make sure you understand this exercise, we are going to ask you a few questions. I will read each question at a time. After I read each question you must answer the question and then we'll check if the answer is right or wrong *[Read each question at a time and verify the answers.]*

1. On this exercise, do you earn more money from livestock farming or from old-growth forest conservation?

2. How much money do you earn from each hectare of land you decide to use for livestock farming?

3. How much money do you earn from each hectare you decide to keep with old-growth forest?

4. If you would like to choose a farm with 7 hectares of livestock farming and one with old-growth forest, which option would you have to choose?

5. If you choose option 5. How much money would you earn? How much water, in monetary terms, would you send to a household from Bogotá?

6. If you choose option 2. How much money would you earn? How much water, in monetary terms, would you send to a household from Bogotá?

Are there any further questions? *[Wait and see if there are further questions]*

Now we are going to have a practice round *[Give the practice round format to the participant]*. In this practice round I'm going to tell you which option to choose. For this reason, this round won't be selected for payment and it won't affect your earnings in any way. All I want to show you is the process to fill out the form on each round. Keep in mind that afterwards, when we start the first round you must choose any option, whichever you prefer the most. There is no right or wrong answer. The following is only an example for practice. It was chosen arbitrarily.

Please choose the option where you decide to have 2 hectares with livestock farming and keep 6 hectares with old-growth forest, fold the paper and wait for me to pick it up. Given that this is a practice round, I will check your answer, on the next rounds I will only pick up the paper and I won't look at your answer. *[Wait for the participant to fold the paper and pick it up. Check the answer. If he made a mistake find out what happened.]*

If this practice round counted for the earnings, it would mean that you would have chosen to win 5.70 € by switching two hectares to livestock farming and keeping 6 hectares with old-growth forest. At the same time, you would have sent 7,20 € of water to the household from the city of Bogotá *[Show all this in the form including crossing out option 3]*

Are there any further questions? *[Wait and see if there are further questions]*

If there are no further questions and this exercise is clearly understood, we will ask you to sign the consent form in which I as a researcher from this study, and you as a participant, agree that I have explained how this exercise works and that you are willing to voluntarily participate.

[Distribute the consent forms. Read aloud. Collect the original signed consent form and leave a copy to the participant]

Before we start is important to recall that there will be 3 rounds and to clarify that decisions on each round are independent. Therefore, you can decide whatever you want on each round no matter what you decided on a previous round. Each round you will own a new set of 8 hectares of conserved old-growth forest, and they won't change between rounds, no matter what you decided on a previous round. Again, there are no right or wrong answers. Please decide freely based on your own preferences.

In addition, in order to make the decisions in private I will stand aside and leave you alone to make your decision. Please let me know after you make your decision and don't forget to fold the paper so I won't be able to see what you decided.

We are going to start with the first round of this exercise. Remember that we will randomly choose only one of the rounds to be paid, so please think carefully what you want to do in this round and pay attention to the instructions.

[Distribute the round 1 form]

Remember, the number above your farm description on each possible option represents your total earnings on this round, if chosen *[Show where these numbers are in the form.]* Next to your farm description, you will find the amount of water, represented in money, which you would send to the household from the city of Bogotá. *[Show where these numbers are in the form.]*

Now please make a cross in the box of the option you choose to define how much money you will earn and how much water represented in money you will send to the household from the city of Bogotá. After you make your decision, please fold the form and wait for me to pick it up *[Pick up the form.]*

We are going to start with the second round of this exercise. Remember that we will randomly choose only one of the rounds to be paid, so please think carefully what you want to do in this round and pay attention to the instructions.

[From now on make reference to the laminated PES – Bogotá form]

In this round, the environmental authority, for example Corpoguavio or the city hall, has identified the old-growth forest as a priority to protect the environment and secure water production. Therefore, it has decided to pay farmers an additional amount of money for each hectare of old-growth forest they keep conserved. Thus, each hectare of land you decide to keep under old-growth forest will give you 0,90 €, 0,45 € from self-consumption crops plus 0,45 € from the environmental authority.

In order to make your decision, you will receive a new form that looks exactly like this laminated form *[Show the laminated form.]*

This form has again all possible 9 options of how you can distribute your 8 hectares of land *[Show each option in the form]*. On the left side of each possible option you have all your eight hectares of land with old-growth forest or cattle, and below each hectare you have the amount you will earn from it. As you can see, now you win 0,45 € more than before for each hectare with old-growth forest. Thus, you win 0,90 € instead of 0,45 € for each hectare with old-growth forest. If you choose to keep the old-growth forest on all your 8 hectares you will get now 7,20 €, twice the amount you earned before *[Show this in the PES form by comparing with baseline form]*. As you start clearing the old-growth forest and introducing livestock farming you start winning more money, up to 12,00 € when you have cleared all old-growth forest and used your 8 hectares for livestock farming *[Use the form to show this]*.

The right side of each possible option is exactly the same as before, there you can see again how much water will be produced and how much money you will send to the household from the city of Bogotá. If you choose to keep the old-growth forest on all your 8 hectares you will send the highest amount of water to the household from the city of Bogotá, which corresponds to 9,60 €. And when you start clearing the old-growth forest and introducing livestock farming you start sending less water to the household from the city of Bogotá until it reaches 0 € when you have cleared all old-growth forest and used your 8 hectares for livestock farming *[Use the form to show this]*.

In order to let us know your decision, you need again to cross out inside the square below each one of the numbered options *[Show the square in the form]*. For example, let's suppose I choose option 7 *[Cross it on the form.]* How much money will I earn? *[Wait for participants to answer and show it on the form]* Notice that with the additional payment you get now for each hectare you keep with old-growth forest, you earn more money now in comparison to the same option in the previous round *[Show this by comparing between forms]* How much water represented in money will I send to the household from the city of Bogotá? *[Wait for participants to answer and show it on the form.]*

[Delete your previous decision from the form] Now, let's suppose I choose option 4 *[Cross it on the form.]* How much money will I earn? *[Wait for participants to answer and show it on the form]* Notice again that with the additional payment you get now for each hectare you keep with old-growth forest, you earn more money now in comparison to the same option in the previous round *[Show this by comparing between forms]* How much water represented in money will I send to the household from the city of Bogotá? *[Wait for participants to answer and show it on the form.]*

Remember that you will earn money based on your decision. It is important that we make sure that you understand how this exercise works. Are there any questions?

[Wait and see if there are questions. If not, delete your decision from the form.]

If there are no further questions, let's continue.

Remember that now, for each hectare of old-growth forest you decide to keep you will earn 0,90 €, 0,45 € from self-consumption and 0,45 € additional from the environmental authority.

Please cross out the option you choose to define how much money you will earn and how much water represented in money you will send to the household from the city of Bogotá. After you make your decision, please fold the form and wait for us to pick it up *[Pick up the forms.]*

[From now on make reference again to baseline – Bogotá form.]

We are going to start with the third and final round of this exercise. Remember that we will randomly choose only one of the rounds to be paid, so please think carefully what you want to do in this round and pay attention to the instructions.

In this round, we go back to the initial situation because the environmental authority doesn't have any more money. You need to decide again how many of your 8 hectares you want to keep with old-growth forest and how many you want to switch to livestock farming without any additional payments from the environmental authority. In order to make your decision, you will receive a form that looks again exactly like the form you received in the first round *[Distribute the round 3 format]*.

Remember that you will earn money based on your decision. It is important that we make sure that you understand how this exercise works. Are there any questions? *[Wait and see if there are questions.]*

If there are no further questions, let's continue. Please cross out the option you choose to define how much money you will earn and how much water represented in money you will send to the household from the city of Bogotá. After you make your decision, please fold the form and wait for us to pick it up *[Pick up the form.]*

We have finished this exercise. First we will choose randomly which of the three rounds you will be paid for by using this bag with 3 balls numbered from 1 to 3.

Once we decide the round to be pay, we are going to ask you a few more questions about this exercise, and when we are finished we will give you your earnings.

- **Consent form**

CONSENT FORM – EXERCISE

Place: _____ Date: __/__/_____ Hour starting exercise: ___:___AM/PM

You have been invited to participate in an activity that is part of a research about water management and conservation. Given your experience and knowledge as a farmer, your participation is very important for this research. This research will provide important information for us and will benefit your community as well. The duration of this activity is about two hours. There are no foreseeable risks for your participation as it does not involve experiments with human beings, animals or plants.

At the end of this activity, you will receive an amount of cash according to the decisions you take during the activity. You can earn an amount up to 12,00 €. The financing of this research comes from the University of Osnabrück and other international organizations in Germany. It is part of a doctoral fellowship that studies natural resources management and conservation.

What you earned at the end and your decisions during the activity, **will be confidential. This information will be used just for academic purposes. No external agency will get access to your identity information or your answers and decisions.** All decisions in this activity are made in private and we will keep your decisions confidential, however, we cannot control participants' conversations after the activity is over.

Your participation in this activity is completely voluntary. You may stop this activity at any time. However, if you decide to leave before the activity is over you will not receive what you earned up to that point. Please also understand that we reserve the right to terminate your participation in this activity.

AGREEMENT:

I, _____ state that I understand the information given above and my rights and commitments during the activity, that I will receive money as a donation and therefore this payment does not involve any employment or contractual relationship between the University of Osnabrück and me. I also understand that I can leave the activity at any time declining to receive the money earned in the activity.

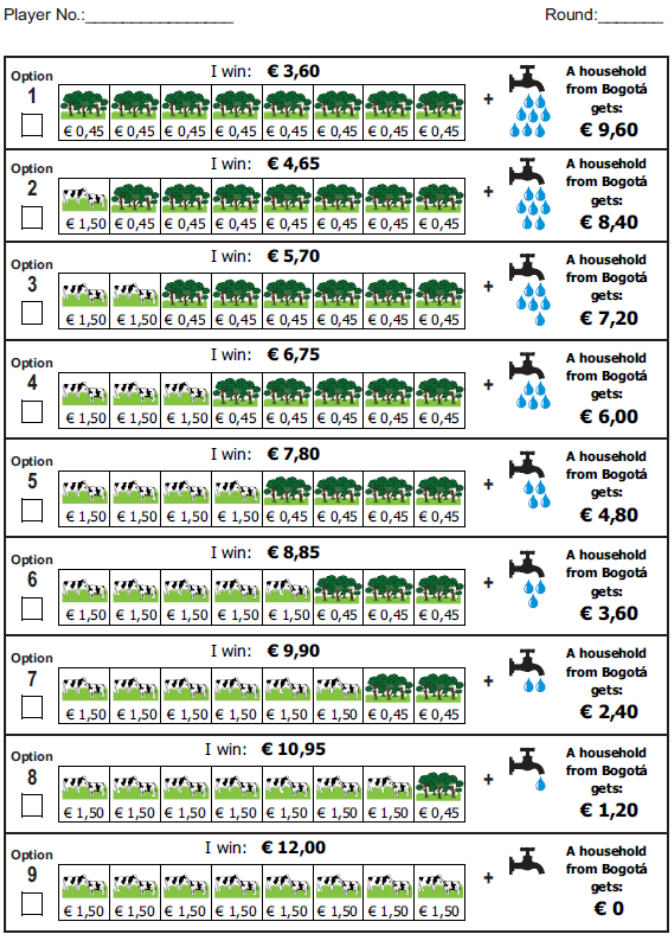
Signed, _____, c.c. _____ de _____.

I, Adriana María Bernal Escobar, PhD student at the University of Osnabrück, certify that this information will be used in a confidential manner and only for academic and community educational purposes. I also certify that we will pay to each participant the amount of money earned during the activity, including those who are not present during the activity but who benefit from the decisions taken by those who are present.

Signed,  , c.c. 1,032'362,820 de Bogotá.

If you have any further questions related to this research, you can contact me at the mobile number 3193308272 or you can send me and email to adriana.bernalescobar@uni-osnabrueck.de.

- **Baseline decision form**



- **Changes in instructions for ES Framing manipulations (only applies to Chapter 2)**

Original paragraph in control group:

In this round, the environmental authority has identified forest conservation as a priority to protect the environment and to secure water provision. Therefore, it has decided to pay farmers …

Additional paragraph in cultural-ES treatment:

Conserving the forest is important for everyone: to you, to your community and to the country as a whole. The magic of the forest holds many secrets and thoughts from our ancestors who protected the territory in the past. Forests are part of our patrimony and it is our duty to protect them for the welfare of our children and of other future generations. They are also a refuge for many animals and plants who rely on our help to conserve their home.

For this reason, in this round, the environmental authority has identified forest conservation as a priority to protect the environment and to secure water provision. Therefore, it has decided to pay farmers …

Additional paragraph in environmental-ES treatment:

Conserving the forest is important for everyone: to you, to your community and to the country as a whole. Forests are a source of biodiversity. They also contribute to reduce the effects of climate change as they capture part of the pollution that damages our air and atmosphere. Forests also help regulate the climate, preventing catastrophic events such as floods and droughts.

For this reason, in this round, the environmental authority has identified forest conservation as a priority to protect the environment and to secure water provision. Therefore, it has decided to pay farmers …

- **Post-experimental survey (only applies to Chapter 4)**

In a scale from 1 (completely disagree) to 5 (completely agree) indicate how much you agree with the following:

- Cattle farming and agriculture must be restricted in order to conserve the environment
- Cattle farming and agriculture must be restricted in order to conserve the environment, and farmers who continue with cattle farming or agriculture on their farms must be fined
- Cattle farming and agriculture must be restricted in order to conserve the environment, and farmers must receive compensation in order to stop with cattle farming or agriculture on their farms

Baseline-RD

- Did you find it fair that others received payments to reduce cattle farming while you were excluded from receiving the payments? Why?

PA

- Did you find it fair that you were fined for cattle farming? Why?

PA-RD

- Did you find it fair that you were fined for cattle farming while others received payments to reduce cattle farming? Why?

Mix

- Did you find it fair that you were fined for cattle farming and that at the same time you received payments to reduce cattle farming? Why?

Mix-RD

Did you find it fair that you were fined for cattle farming and that at the same time you received payments to reduce cattle farming, while others only received payments to reduce cattle farming without fines? Why?

Appendix B Descriptive statistics of participating farmers

- Chapter 2

Socio-demographic characteristics					
Name	*Description*	*n*	*n/N*	*Mean*	*s.d.*
Gender	1 if woman	99	0.63	-	-
Age	Age	-	-	51.66	1.38
Years community	Years living in the community	-	-	43.22	1.75
Education	Years of education	-	-	5.91	3.11
# Kids	No. of kids living in household	-	-	1.01	1.14
# Adults	No. of adults living in household	-	-	2.58	1.18
# cows	No. of own cows	-	-	2.83	2.30
# crops	No. of own crops cultivated	-	-	2.29	1.82
Land owner	1 if owns land	110	0.70	-	-
Land area	Size of own land (hectares)	-	-	1.36	1.74
Savings	Regarding last year savings level: 0 if had to borrow, 1 if used savings, 2 if neither saved nor borrowed, 3 if saved money	-	-	1.52	0.92

Pro-environmental practices and motivations					
Name	*Description*	*n*	*n/N*	*Mean*	*s.d.*
Pro-env actions	Amount of pro-environmental actions implemented in the last year (from a fixed list of 11 items)	-	-	5.64	2.31
Reasons for implementing pro-environmental actions	Reduction of utility rates	68	0.43	-	-
	Reduction of farming expenses	17	0.11	-	-
	Getting a premium price	12	0.08	-	-
	Moral reasons, e.g. ensure the well-being of future generations	102	0.65	-	-
	Environmental reasons, e.g. climate change reduction, increase biodiversity	65	0.41	-	-
Moral motives	1 if solely moral reasons for pro-environmental actions	44	0.28	-	-
Environmental motives	1 if solely environmental reasons for pro-environmental actions	65	0.41	-	-

Fairness preferences regarding inequality aversion					
Name	*Description*	*n*	*n/N*	*Mean*	*s.d.*
Disadvantageous inequality	Level of aversion to disadvantageous inequality (0-5)	-	-	0.74	1.03
Advantageous inequality	Level of aversion to advantageous inequality (0-1)	-	-	0.86	0.27

Environmental awareness and attitudes					
Name	*Description*	*n*	*n/N*	*Mean*	*s.d.*
Lifestyle	5-point likert scale of "it is possible to solve environmental problems without changing our lifestyle"	-	-	2.26	1.61
Capability	5-point likert scale of "It is easy for me to do something to improve the environment"	-	-	3.43	1.69
Jobs	5-point likert scale of "Conservation must be the priority even if it leads to a reduction of employment"	-	-	3.10	1.60
Climate change	5-point likert scale of "Conservation efforts are needed to reduce climate change effects such as droughts and floods"	-	-	4.91	0.43
Bequest	5-point likert scale of "Conservation efforts are needed to ensure well-being of future generations and protect other living beings"	-	-	4.91	0.43
Area with forest	No. of own hectares with forest	-	-	0.12	0.28
Mutual trust	1 if considers people in the community as trustworthy	107	0.68	-	-
Collective action	Index (Cronbach α = 0.62) of how likely (0-2) to get in the vereda:	-	-	1.41	0.58
	Borrowed money back	-	-	1.41	0.64
	Help to find lost animal	-	-	1.35	0.81
	Childcare in case of emergency	-	-	1.52	0.74
Trust env. authority	Standardized index (Cronbach α = 0.67) of perceptions about env. authority	-	-	0.01	0.89
	Level of trust (1-4)	-	-	1.52	1.11
	1 if good perception	101	0.64		
Deforestation vulnerability	How affected (1-4) by deforestation	-	-	1.62	1.02
Water vulnerability	Index (Cronbach α = 0.69) of how affected (1-4) by:	-	-	2.20	0.96
	Water shortage	-	-	2.41	1.25
	Water pollution	-	-	2.12	1.20
	Droughts	-	-	2.02	1.22
Environmental status	Perceived environmental status in the country: 0 if bad, 1 if regular, 2 if good, 3 if very good.	-	-	1.20	0.62

- Chapter 3

Socio-demographic characteristics					
Name	*Description*	*n*	*n/N*	*Mean*	*s.d.*
Gender	1 if woman	36	0.60	-	-
Age	Age	-	-	53.45	17.43
Years community	Years living in the community	-	-	41.77	22.32
Education	Years of education	-	-	5.88	3.17
# Kids	No. of kids living in household	-	-	1.02	1.13
# Adults	No. of adults living in household	-	-	2.65	1.63
# cows	No. of own cows	-	-	2.57	1.85
# crops	No. of own crops cultivated	-	-	3.00	1.96
Land owner	1 if owns land	43	0.72	-	-
Land area	Size of own land (hectares)	-	-	2.13	2.38
Income	Household income per month (1000 $)	-	-	611.29	538.35
Environmental awareness and attitudes					
Name	*Description*	*n*	*n/N*	*Mean*	*s.d.*
Collective action	Index (Cronbach α = 0.62) of how likely (0-2) to get in the vereda:	-	-	0.09	0.61
	Borrowed money back	-	-	1.42	0.67
	Help to find lost animal	-	-	1.46	0.75
	Childcare in case of emergency	-	-	1.60	0.67
Trust env. authority	Standardized index (Cronbach α = 0.67) of perceptions about env. authority	-	-	-0.13	0.83
	Level of trust (1-4)	-	-	1.27	1.00
	1 if good perception	37	0.62	-	-
Water vulnerability	Index (Cronbach α = 0.69) of how affected (1-4) by:	-	-	2.17	1.04
	Water pollution	-	-	2.23	1.18
	Droughts	-	-	1.97	1.31
Area with forest	No. of own hectares with forest	-	-	0.10	0.21
Deforestation vulnerability	How affected (1-4) by deforestation	-	-	1.70	1.09
Lifestyle	5-point likert scale of “it is possible to solve environmental problems without changing our lifestyle”	-	-	2.22	1.55
Capability	5-point likert scale of "It is easy for me to do something to improve the environment"	-	-	3.35	1.71
Jobs	5-point likert scale of "Conservation must be the priority even if it leads to a reduction of employment"	-	-	3.02	1.56
Environmental status	Perceived environmental status in the country: 0 if bad, 1 if regular, 2 if good, 3 if very good.	-	-	1.22	0.61

- Chapter 4

Socio-demographic characteristics					
Name	*Description*	*n*	*n/N*	*Mean*	*s.d.*
Gender	1 if woman	177	0.64	-	-
Age	Age	-	-	55.06	17.10
Years community	Years living in the community	-	-	40.82	23.36
Education	Years of education	-	-	5.84	3.58
Household size	No. of people living in household	-	-	3.25	1.84
Income	Household income per month (1000 $)	-	-	727.07	799.46
# cows	No. of own cows	-	-	2.67	9.80
# crops	No. of own crops cultivated	-	-	3.62	2.21
Land owner	1 if owns land	205	0.75	-	-
Land area	Size of own land (hectares)	-	-	1.92	2.85
Environmental vulnerability					
Name	*Description*	*n*	*n/N*	*Mean*	*s.d.*
Environmental vulnerability	How affected (0-2) by environmental problems in the municipality	-	-	0.86	0.79
Deforestation vulnerability	How affected (1-4) by deforestation	-	-	1.39	0.86
Water vulnerability	Index (Cronbach α = 0.69) of how affected (1-4) by:	-	-	2.31	1.05
	Water shortage	-	-	2.47	1.26
	Water pollution	-	-	2.17	1.15
	Droughts	-	-	2.14	1.15
External motivation to conserve					
Name	*Description*	*n*	*n/N*	*Mean*	*s.d.*
Motivated penalties	4-point likert scale of "I do not cut down the forest for fear of fines"	-	-	1.97	1.15
Motivated payments	4-point likert scale of "I conserve the forest only if I get paid to do it"	-	-	1.37	0.70
Risk aversion					
Name	*Description*	*n*	*n/N*	*Mean*	*s.d.*
Risk	Level of risk aversion (0.097-2.000)	-	-	0.86	0.02

Environmental awareness and attitudes					
Name	*Description*	*n*	*n/N*	*Mean*	*s.d.*
Prioritize environment	Index (Cronbach α = 0.67) of:	-	-	2.84	1.20
	5-point likert scale of "Conservation must be the priority even if it leads to a reduction of employment"	-	-	3.25	1.44
	5-point likert scale of "Conservation must be the priority even if it leads to land-use restrictions "	-	-	2.43	1.33
Lifestyle	5-point likert scale of "it is possible to solve environmental problems without changing our lifestyle"	-	-	3.09	1.63
Capability	5-point likert scale of "It is easy for me to do something to improve the environment"	-	-	4.02	1.33
Area with forest	No. of own hectares with forest	-	-	0.20	0.95
Environmental status	Perceived environmental status in the country: 0 if very good, 1 if good, 2 if regular, 3 if bad.	-	-	1.86	0.60
Collective action	Index (Cronbach α = 0.65) of how likely (0-2) to get in the vereda:	-	-	1.24	0.60
	Borrowed money back	-	-	1.20	0.72
	Help to find lost animal	-	-	1.21	0.83
	Childcare in case of emergency	-	-	1.30	0.77
Trust env. authority	Standardized index (Cronbach α = 0.67) of perceptions about env. authority	-	-	0.01	0.07
	1 if good perception	165	0.60	-	-
	Level of trust (0-3)	-	-	1.54	1.02
Disadvantageous inequality aversion					
Name	*Description*	*n*	*n/N*	*Mean*	*s.d.*
DI Aversion	Level of aversion to disadvantageous inequality (0-5)	-	-	0.74	0.08

Appendix C Randomization checks

Before measuring the treatment effects examined in each chapter, we first checked whether each treatment group was comparable to each control group. In order to do so, we created a dependent variable for each treatment group, taking value 1 for all farmers who participated in the corresponding treatment group and 0 otherwise. Then, we implemented three logistic regressions for each pair of control and treatment groups, one for different sets of observed covariates (e.g., sociodemographic characteristics, environmental awareness and attitudes, and preferences regarding inequality aversion or risk aversion). Finally, we used the Wald test to test the joint significance of all coefficients within each logistic regression. Thus, a non-statistically significant Wald-Chi-Square statistic means that all coefficients (other than the constant term) are zero, therefore, each treatment and control group were comparable because they were not correlated with the observable set of covariates.

For each chapter, the corresponding regressions by set of covariates and treatment are presented below.

- Chapter 2

Logit estimates of socio-demographic characteristics

Variables	ES framing				Payment framing			
	Cultural ES		Regulating ES		Reward		Compensation	
Gender	-0.387	(0.769)	0.326	(0.702)	-0.417	(0.741)	0.040	(0.723)
Age	0.021	(0.023)	-0.054	(0.037)	0.002	(0.030)	-0.040	(0.025)
Years community	-0.008	(0.016)	0.016	(0.020)	0.026	(0.020)	0.025	(0.022)
Education	0.019	(0.100)	-0.192	(0.141)	-0.082	(0.135)	0.109	(0.128)
# kids	0.227	(0.262)	-0.380	(0.360)	0.120	(0.351)	-0.499	(0.358)
# adults	0.321	(0.263)	-0.053	(0.272)	0.086	(0.335)	-0.030	(0.380)
Savings	-0.151	(0.362)	-0.177	(0.426)	0.047	(0.451)	0.009	(0.350)
# of cows	0.093	(0.131)	0.227	(0.148)	0.089	(0.176)	0.524**	(0.207)
# of crops	-0.270*	(0.164)	-0.318*	(0.174)	-0.252	(0.173)	-0.512**	(0.220)
Land owner	0.057	(0.690)	-0.587	(0.911)	0.092	(0.710)	0.472	(0.859)
Land area	-0.324*	(0.194)	0.007	(0.238)	-0.500**	(0.253)	-0.947***	(0.355)
Constant	-0.508	(1.933)	4.389	(2.940)	0.068	(2.484)	1.446	(2.208)
Observations	64		62		60		61	
Prob > Wald chi2	0.699		0.476		0.375		0.144	

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

Logit estimates of inequality aversion

Variables	ES framing				Payment framing			
	Cultural ES		Regulating ES		Reward		Compensation	
Disadvantageous inequality	-0.894***	(0.288)	-0.616**	(0.269)	-0.871***	(0.295)	-0.862***	(0.293)
Advantageous inequality	2.085	(1.493)	-1.966	(1.298)	-0.585	(1.179)	0.208	(0.884)
Constant	-1.008	(1.453)	2.288*	(1.252)	1.329	(1.118)	0.642	(0.803)
Observations	63		60		60		61	
Prob > Wald chi2	0.002		0.011		0.005		0.004	

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

Logit estimates of social and environmental attitudes and perceptions

Variables	ES framing				Payment framing			
	Cultural ES		Regulating ES		Reward		Compensation	
Collective action	0.223	(0.637)	-0.335	(0.555)	-1.605**	(0.634)	-0.906	(0.599)
Mutual trust	0.327	(0.627)	0.494	(0.694)	1.459*	(0.789)	0.663	(0.755)
Trust env. authority	0.278	(0.368)	0.463	(0.364)	0.755*	(0.399)	0.611	(0.381)
Area with forest	-0.276	(0.947)	-0.559	(1.343)	0.272	(0.890)	-0.937	(1.499)
Environmental status	0.0705	(0.180)	0.116	(0.175)	0.0488	(0.179)	-0.00917	(0.245)
Water vulnerability	-0.102	(0.175)	0.186	(0.198)	-0.164	(0.185)	-0.0419	(0.183)
Deforestation vulnerability	0.0979	(0.185)	-0.0485	(0.192)	0.0552	(0.181)	-0.196	(0.218)
Lifestyle	0.266	(0.403)	-0.262	(0.487)	-0.162	(0.587)	-0.525	(0.536)
Capability	-0.242	(0.307)	0.0730	(0.301)	0.425	(0.343)	-0.159	(0.332)
Jobs	-0.105	(0.287)	-0.617*	(0.370)	-0.246	(0.313)	0.411	(0.302)
Constant	-0.146	(1.379)	0.591	(1.382)	1.084	(1.477)	1.947	(1.364)
Observations	64		62		60		61	
Prob > Wald chi2	0.917		0.812		0.410		0.768	

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

- Chapter 3

Logit estimates of socio-demographic characteristics

Variables	Logit estimates	
Gender	0.172	(0.654)
Age	0.013	(0.023)
Years community	-0.011	(0.015)
Education	-0.023	(0.110)
# kids	-0.045	(0.339)
# adults	0.327*	(0.192)
Income	-0.001	(0.001)
# of cows	-0.102	(0.189)
# of crops	0.068	(0.148)
Land owner	-0.820	(0.753)
Constant	0.048	(1.834)
Observations	60	
Prob > Wald chi2	0.685	

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

Logit estimates of place identity predictors

Variables	Logit estimates	
Gender	-0.737	(0.652)
Age	-0.147	(0.604)
Native	-0.493	(0.607)
Native ancestors	-0.446	(0.614)
Constant	0.860	(0.741)
Observations	60	
Prob > Wald chi2	0.650	

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

Logit estimates of social and environmental attitudes and perceptions

Variables	Logit estimates	
Collective action	0.438	(0.494)
Trust env. authority	0.215	(0.402)
Area with forest	-2.667	(1.806)
Lifestyle	0.118	(0.177)
Capability	-0.0562	(0.184)
Jobs	-0.170	(0.187)
Environmental status	0.539	(0.462)
Water vulnerability	-0.180	(0.299)
Deforestation vulnerability	0.170	(0.281)
Constant	0.111	(1.330)
Observations	60	
Prob > Wald chi2	0.789	

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

- Chapter 4

Logit estimates of social and environmental attitudes and perceptions

Variables	No policy		Fine policy		Policy mix	
Collective action	-0.572	(0.493)	0.139	(0.496)	0.910	(0.735)
Trust env. Authority	-0.891**	(0.361)	-0.389	(0.398)	0.281	(0.299)
Own area with forest	-0.570	(0.481)	0.742	(0.920)	1.604	(1.242)
Lifestyle	0.247	(0.196)	-0.268	(0.197)	-0.316	(0.212)
Capability	0.084	(0.232)	0.410*	(0.217)	0.012	(0.214)
Prioritize environment	0.789	(0.515)	-0.062	(0.610)	-0.407	(0.675)
Environmental status	0.344	(0.247)	-0.096	(0.256)	0.265	(0.331)
General env. Vulnerability	0.238	(0.411)	-0.219	(0.503)	-1.102**	(0.474)
Water vulnerability	0.072	(0.423)	0.241	(0.344)	0.002	(0.329)
Deforestation vulnerability	-0.337	(0.257)	0.348	(0.358)	0.164	(0.337)
Motivated payments	0.249	(0.856)	-0.321	(0.387)	0.056	(0.434)
Motivated penalties	0.009	(0.313)	0.326	(0.260)	-0.259	(0.333)
Constant	-3.196*	(1.918)	-1.787	(2.063)	0.448	(2.312)
Observations	67		67		64	
Prob > Wald chi2	0.085		0.662		0.270	

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

Logit estimates of socio-demographic characteristics

Variable	No policy		Fine policy		Policy mix	
Gender	0.708	(0.642)	1.203*	(0.686)	0.629	(0.608)
Age	0.038	(0.030)	-0.003	(0.028)	-0.032*	(0.019)
Years community	-0.043***	(0.016)	-0.0405**	(0.018)	0.006	(0.015)
Education	0.056	(0.114)	-0.059	(0.089)	-0.149*	(0.090)
Household size	0.372*	(0.206)	-0.080	(0.184)	-0.057	(0.168)
Income	0.001	(0.001)	-5.81e-05	(0.001)	0.002***	(0.001)
# of cows	-0.324*	(0.191)	0.174	(0.171)	-0.068	(0.195)
# of crops	0.048	(0.123)	-0.049	(0.142)	-0.057	(0.162)
Land owner	-0.865	(0.611)	0.131	(0.665)	1.087	(0.859)
Land area	0.014	(0.074)	0.053	(0.164)	0.163	(0.164)
Constant	-1.875	(2.314)	1.014	(2.185)	0.021	(1.718)
Observations	67		67		64	
Prob > Wald chi2	0.126		0.232		0.227	

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

Logit estimates of risk and fairness preferences

Variables	No policy		Fine policy		Policy mix	
Disadvantageous inequality	0.515*	(0.284)	-0.112	(0.238)	0.310	(0.249)
Risk aversion	0.223	(0.350)	-0.013	(0.414)	-0.634	(0.451)
Constant	-0.688	(0.418)	-0.060	(0.385)	0.073	(0.444)
Observations	67		67		64	
Prob > Wald chi2	0.142		0.890		0.146	

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

In addition to the previously described randomization check for each set of covariates, we used Fisher's exact tests in Chapter 2 and Chapter 4 to evaluate whether each treatment and control group were comparable in specific covariates. In Chapter 2, we examined how comparable the groups were in terms of reported motivations for pro-environmental actions. The following table summarizes the results of these tests:

Treatment	N	Moral motives		Environmental motives	
		Proportion	Fisher's exact test p-value (control vs treatment)	Proportion	Fisher's exact test p-value (control vs treatment)
Control	30	0.47	-	0.77	-
Cultural-ES	34	0.32	0.307	0.62	0.281
Regulating-ES	32	0.41	0.798	0.62	0.277
Compensation	31	0.48	1.000	0.68	0.570
Reward	30	0.27	0.180	0.50	0.060

In Chapter 4, we examined how comparable the groups were in terms of aversion to disadvantageous inequality. Fairness preferences regarding disadvantageous inequality aversion were elicited by using an ultimatum game. The minimum-acceptable offer was taken as an estimate of the level of aversion to disadvantageous inequality. Farmers who accept all offers that put them at a disadvantage (they earn less compared to the other player) are assumed not to be averse to disadvantageous inequality. All other farmers are assumed not have a certain degree of disadvantageous inequality aversion (the degree of aversion was distributed between a minimum of 0.05 and a maximum of 5). We created a dummy variable named *DI Averse* to classify farmers between those with fairness any level of preferences regarding disadvantageous inequality aversion (dummy variable takes value 1) and those without fairness preferences (dummy variable takes value 0). Finally, we tested whether the groups were comparable in terms of this new variable. The following table summarizes the results of these tests:

Treatment	N	Is averse to disadvantageous inequality (*DI Averse*)	
		Proportion	Fisher's exact test p-value (treatment vs treatment-RD)
Baseline	36	0.69	
Baseline-RD	31	0.77	0.325
Fine	36	0.72	
Fine-RD	31	0.58	0.169
Mix	34	0.59	
Mix-RD	30	0.57	0.531

Appendix D Full version of tables with econometric regressions

- Chapter 2

Based on the difference-in-differences (DD) approach, Table 3 showed the results of four random effects Tobit models (one for each treatment) to estimate the treatment effect of each framing manipulation on forest conservation in the policy and post-policy stages. The full version of this table, including the estimates for the control covariates, is presented below.

Random effects Tobit estimates of difference-in-differences models per treatment

Dependent variable: Forest area	Payment framing				ES framing			
	Reward		Compensation		Cultural-ES		Regulating-ES	
Treatment	-0.532	(0.740)	1.530**	(0.711)	0.679	(0.668)	0.110	(0.675)
Policy	0.598**	(0.277)	0.601*	(0.309)	0.588**	(0.254)	0.601**	(0.302)
Policy x Treatment	0.473	(0.388)	-0.627	(0.427)	0.364	(0.346)	0.446	(0.424)
Post-Policy	-0.289	(0.278)	-0.288	(0.310)	-0.289	(0.256)	-0.290	(0.304)
Post-Policy x Treatment	0.686*	(0.390)	-0.185	(0.426)	0.474	(0.347)	0.304	(0.426)
Environmental motives	0.504	(0.682)						
Disadvantageous inequality	-0.542*	(0.316)	-0.286	(0.275)	-0.387	(0.290)	-0.537**	(0.264)
Advantageous inequality							2.307**	(1.006)
# of cows			-0.0203	(0.161)				
# of crops			-0.265	(0.178)				
Land Area			0.114	(0.183)				
Constant	3.524***	(0.819)	4.123***	(0.793)	3.704***	(0.608)	1.859*	(1.123)
Observations	180		183		189		180	
Number of id	60		61		63		60	
Prob > chi2	0.000		0.005		0.000		0.000	

Note: standard errors in parentheses (*** p < 0.01, ** p < 0.05, * p < 0.1)

- Chapter 3

Table 6 presented the results of three Tobit regressions to test if water provision were different in the pre-policy stage depending on the geographical location of water beneficiaries (i.e., between the two treatments). The first model controls for farmers' perceptions of the needfulness of the people living in the local community relative to outsider community. The second model includes an interaction of farmers' perceptions of needfulness with the treatment variable *Local* because we expected farmers' perceptions regarding the needfulness of the people living in Junín to be more relevant in the *Locals* treatment. The third model controls, in addition to the previously mentioned interaction, for the degree of place identity of each participant. We also controlled for other additional covariates in all models. The full version of this table is presented below.

Tobit estimates of downstream water provision levels in the pre-policy stage

Dependent variable: Units of downstream water	Model I		Model II		Model III	
Local	0.920	(0.706)	0.171	(0.848)	-1.727	(1.273)
Place identity					-0.142	(1.008)
Place identity x Local					2.988**	(1.462)
Needs locals	1.909*	(0.985)	0.324	(1.240)	0.716	(1.195)
Needs locals x Local			3.325**	(1.802)	2.655	(1.708)
Income locals	0.626	(0.849)	0.355	(0.982)	0.353	(0.919)
Income locals x Local			0.370	(1.387)	0.685	(1.313)
Age	-0.050	(0.033)	-0.059*	(0.029)	-0.058*	(0.027)
Gender	-0.235	(0.800)	-0.676	(0.757)	0.103	(0.773)
Education	-0.039	(0.140)	-0.025	(0.136)	0.006	(0.128)
Income	0.001	(0.001)	0.000	(0.001)	0.000	(0.001)
Land tenure	2.432	(0.877)	2.457***	(0.899)	2.782***	(0.893)
Household size	-0.131	(0.178)	-0.128	(0.182)	-0.176	(0.172)
Zone = Centro	1.246	(1.082)	1.667	(1.071)	1.242	(1.019)
Zone = Gonzalez	1.611	(1.148)	2.236**	(1.238)	1.303	(1.239)
Zone = Santa Rosa	-0.950	(0.955)	-0.495	(1.450)	-1.848*	(1.455)
Zone = Valle	2.692**	(1.134)	2.955**	(1.034)	2.497**	(1.000)
Constant	2.111	(3.028)	3.143	(2.486)	2.886	(2.370)
Observations	60		60		60	
Prob > F	0.001		0.017		0.004	
Pseudo R2	0.097		0.110		0.134	

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

In addition to this, Table 7 showed the results of two random effects Tobit models based on the difference-in-differences (DD) approach. These models compared the effect of PWS implementation on forest conservation between treatments in the policy and post-policy stages. The first model controls for the variables used in the random assignment of treatments, while the second model controls for additional sociodemographic covariates. The full version of this table is presented below.

Random effects Tobit estimates of differences-in-differences models.

Dependent variable: Units of downstream water	Model I		Model II	
Local	-0.333	(0.896)	0.357	(0.850)
Policy	0.598**	(0.279)	0.598**	(0.280)
Policy x Local	-0.004	(0.397)	-0.00255	(0.399)
Post policy	-0.287	(0.280)	-0.286	(0.281)
Post policy x Local	0.293	(0.397)	0.301	(0.399)
Needs local	0.477	(1.300)	-0.0434	(1.212)
Needs local x Local	1.976	(1.829)	2.189	(1.742)
Income local	-0.311	(1.032)	0.466	(0.945)
Income local x Local	0.943	(1.495)	0.429	(1.347)
Age			-0.054**	(0.026)
Gender	-0.359	(0.716)	-0.912	(0.728)
Education			0.009	(0.131)
Income			0.000	(0.001)
Land tenure			2.907***	(0.840)
Household size			-0.059	(0.175)
Zone = Centro	1.194	(0.990)	1.938*	(1.028)
Zone = Gonzalez	1.186	(1.216)	2.850**	(1.197)
Zone = Santa Rosa	-1.310	(1.493)	-0.515	(1.406)
Zone = Valle	2.137**	(0.949)	3.185***	(0.989)
Constant	2.142**	(1.005)	1.988	(2.385)
Observations	180		180	
Number of id	60		60	
Prob > chi2	0.007		0.000	

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

Finally, the following table presents the regression analysis of the heterogeneous treatment effect of the PWS. With this analysis, we tested whether the effect of the PWS was stronger on farmers with a strong place identity in comparison to those with a weak place identity. These effects are captured by the triple interaction estimate resulting from interacting the treatment, stage, and place identity variables, which were not statistically significant in both the policy and post policy stages.

Random effects Tobit estimates of triple differences model

Dependent variable: Units of downstream water	RE Tobit estimates	
Local	-1.816	(1.276)
Place identity	-0.493	(1.038)
Place identity x Local	3.432**	(1.473)
Policy	0.247	(0.536)
Policy x Local	-0.088	(0.714)
Policy x Place identity	0.470	(0.625)
Policy x Place identity x Local	0.218	(0.858)
Post policy	-0.991*	(0.552)
Post policy x Local	0.858	(0.729)
Post policy x Place identity	0.944	(0.638)
Post policy x Place identity x Local	-0.714	(0.868)
Needs local	0.406	(1.146)
Needs local x Local	1.511	(1.627)
Income local	0.478	(0.873)
Income local x Local	0.736	(1.253)
Age	-0.054**	(0.025)
Gender	-0.059	(0.727)
Education	0.044	(0.122)
Income	0.000	(0.001)
Land tenure	3.263***	(0.819)
Household size	-0.112	(0.164)
Zone = Centro	1.464	(0.965)
Zone = Gonzalez	1.864	(1.168)
Zone = Santa Rosa	-2.036	(1.387)
Zone = Valle	2.717***	(0.944)
Constant	1.989	(2.267)
Observations	180	
Number of id	60	
Prob > chi2	0.000	

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

- Chapter 4

Based on the difference-in-differences (DD) approach, Table 9 showed the results of three random effects Tobit models (one for each analyzed policy) to estimate the treatment effect of experiencing the policy under a disadvantageous condition on forest conservation in the policy and post-policy stages. The full version of this table, including the estimates for the control covariates, is presented below.

Random effects Tobit estimates of difference-in-differences models per analyzed policy

Dependent variable: Hectares with forest	No Policy		Fine Policy		Policy Mix	
RD	0.554	(0.701)	-0.069	(0.652)	0.191	(0.764)
Policy	0.111	(0.331)	0.481	(0.412)	1.495***	(0.397)
Policy x RD	-0.611	(0.475)	1.483**	(0.617)	-0.696	(0.581)
Post Policy	0.174	(0.331)	-0.384	(0.413)	0.564	(0.390)
Post Policy x RD	-0.415	(0.476)	1.082*	(0.611)	-0.068	(0.574)
Environmental status	-1.188**	(0.514)				
Years community	-0.021	(0.013)				
Household size	0.104	(0.156)				
Trust env. Authority	-0.262	(0.351)				
Risk aversion			0.244	(0.457)	0.548	(0.577)
Constant	5.108***	(0.959)	3.924***	(0.532)	3.749***	(0.697)
Observations	201		201		192	
Number of id	67		67		64	
Prob > chi2	0.067		0.000		0.004	

Note: Standard errors in parentheses, *** p < 0.01, ** p < 0.05, * p < 0.1

Finally, we tested whether having a preference for fair outcomes moderated the behavioral spillover. Thus, within each condition, we tested whether the behavioral spillover was stronger on farmers who were inequality averse in comparison to those who were not. This heterogeneous treatment effect was captured by the triple interaction estimates resulting from interacting the dummy variable *DI Averse* with the condition and stage dummy variables. In Table 13 we only presented the resulting triple interaction estimates, next we present the full version of the three resulting regressions.

Random effects Tobit estimates of the heterogeneous treatment effect per analyzed policy

Dependent variable: Hectares with forest	No policy		Fine Policy		Policy Mix	
RD	0.731	(1.322)	1.504	(1.122)	-0.0761	(1.141)
DI averse	0.569	(0.969)	1.785*	(0.977)	-1.151	(1.030)
DI averse x RD	-0.308	(1.480)	-2.298	(1.403)	0.429	(1.520)
Policy	0.120	(0.585)	1.621**	(0.734)	1.593**	(0.635)
Policy x RD	-0.325	(0.925)	0.921	(1.015)	-0.928	(0.907)
Policy x DI averse	-0.016	(0.702)	-1.601*	(0.867)	-0.165	(0.811)
Policy x DI averse x RD	-0.362	(1.071)	0.636	(1.245)	0.398	(1.179)
Post Policy	-0.025	(0.588)	1.864**	(0.735)	0.952	(0.611)
Post Policy x RD	1.248	(0.921)	-1.058	(1.001)	-0.591	(0.890)
Post Policy x DI averse	0.279	(0.704)	-3.133***	(0.869)	-0.657	(0.792)
Post Policy x DI averse x RD	-2.169**	(1.069)	2.953**	(1.232)	0.892	(1.161)
Environmental status	1.324**	(0.538)				
Living community	-0.022	(0.014)				
Household size	0.099	(0.156)				
Trust env. Authority	-0.227	(0.355)				
Risk aversion			0.373	(0.475)	0.573	(0.576)
Constant	0.954	(1.576)	2.564***	(0.921)	4.407***	(0.893)
Observations	268		201		192	
Number of id	67		67		64	
Prob > chi2	0.077		0.000		0.031	

Note: standard errors in parentheses, *** p < 0.01, ** p < 0.05, * p < 0.1

Appendix E Exploratory analysis

- Chapter 2

As an exploratory analysis of whether the ES framing manipulations performed better in farmers whose reasons for engaging in pro-environmental actions were more aligned to the framing manipulation.

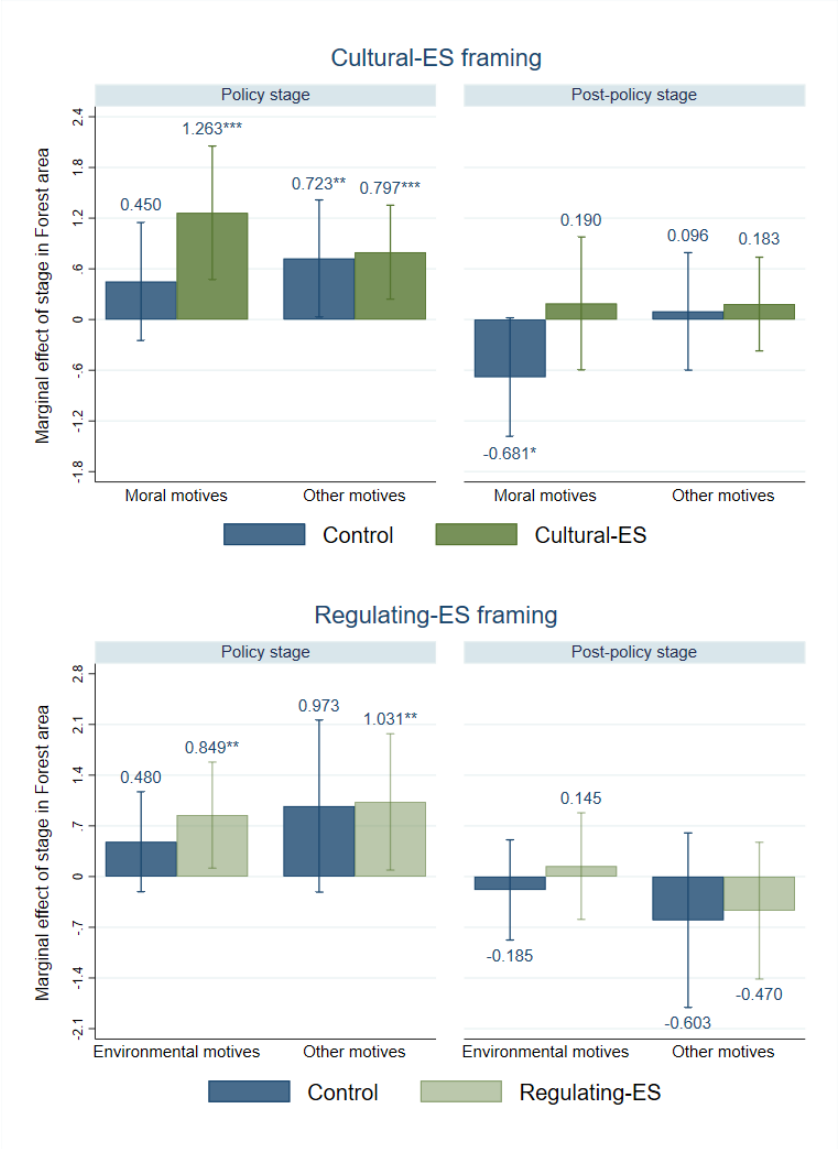
We used the information collected during the pre-experimental survey about farmers' reasons to engage in pro-environmental actions as a proxy for their motivations to conserve the forest. This additional dummy variable "Aligned motives" had a value of 1 if a farmer's reported reasons for engaging in pro-environmental actions included those more aligned with the analyzed framing manipulation (i.e., moral reasons for the framing manipulation in the *cultural-ES* treatment and environmental reasons for that in the *regulating-ES* treatment); otherwise, it had a value of 0. Then, we included an additional dummy variable in the DD model to analyze the heterogeneous treatment effect of the framing manipulation on farmers with aligned motivations. Thus, we interacted the Aligned motives variable with the treatment and stage variables. The following table summarizes the econometric results:

Random effects Tobit estimates of the heterogeneous treatment effect

Dependent variable: Hectares with forest	ES framing			
	Cultural-ES		Regulating-ES	
Treatment	0.994	(0.840)	0.935	(1.164)
Related motives	0.357	(0.898)	0.814	(1.046)
Treatment x Related motives	-0.772	(1.269)	-1.183	(1.395)
Policy	0.723**	(0.354)	0.966	(0.591)
Policy x Treatment	0.075	(0.453)	0.068	(0.755)
Policy x Related motives	-0.273	(0.502)	-0.492	(0.684)
Policy x Treatment x Related motives	0.738	(0.704)	0.512	(0.910)
Post Policy	0.096	(0.355)	-0.602	(0.600)
Post Policy x Treatment	0.087	(0.454)	0.137	(0.762)
Post Policy x Related motives	-0.777	(0.504)	0.415	(0.691)
Post Policy x Treatment x Related motives	0.784	(0.703)	0.372	(0.917)
Level of aversion to disadvantage inequality	-0.386	(0.290)	-0.553**	(0.264)
Level of aversion to advantage inequality			2.205**	(1.002)
Constant	3.526***	(0.733)	1.353	(1.304)
Observations	189		180	
Number of id	63		60	
Prob > chi2	0.000		0.000	

Note: Standard errors in parentheses (*** p < 0.01, ** p < 0.05, * p < 0.1) Note: In Cultural-ES model, 'Related motives' takes on the value "1" if a participant farmer reported moral motives for its pro-environmental actions and "0" otherwise. In Regulating-ES model, 'Related motives' takes on the value "1" if a participant farmer reported environmental motives for its pro-environmental actions and "0" otherwise.

The positive sign of the coefficients of the triple interaction estimate in the policy and post-policy stages suggests a moderation trend. To better summarize this moderation trend, the following figure shows the predicted marginal effects of the stage on forest area based on the former regression analysis:



Error bars represent the 95% confidence interval. Asterisks signify statistically significant marginal effects (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$).

The upper panel shows that in the *cultural-ES* treatment, the marginal effect of introducing the PES policy with a cultural ES framing was predicted to be larger among farmers who reported moral reasons for their pro-environmental actions in comparison to those who reported other actions. On the contrary, the estimated marginal effect of introducing the PES policy with a control framing was only predicted to be statistically significant among farmers who reported reasons other than moral ones for their pro-environmental actions. In general, the largest marginal effect of introducing the PES policy was predicted among those farmers who reported moral reasons and were exposed to the cultural-ES framing. Additionally, once the PES policy ended, our exploratory analysis predicted a negative and statistically

significant marginal effect on forest levels among farmers that were exposed to the control framing and reported moral reasons for their pro-environmental actions. On the contrary, ending the PES policy was not predicted to have a statistically significant marginal effect on the behavior of farmers who reported reasons other than moral ones in the control group or the behavior of any farmer in the *cultural-ES* treatment. Taken together, the results suggest that emphasizing the cultural ecosystem services that forests provide could better prevent potential crowding-out effects in the conservation behavior of morally motivated farmers than a framing that focuses only on the targeted ecosystem services.

Finally, the bottom panel shows little evidence to support a moderation effect in the *regulating-ES* treatment. Baseline levels of forest area among farmers who reported environmental reasons for their pro-environmental actions were predicted to significantly increase in the policy stage for the *regulating-ES* treatment and not significantly change in the control. However, we observed a similar trend among farmers who reported non-moral reasons. Additionally, this exploratory analysis did not predict any statistically significant marginal effects of ending the PES policy on the behavior of farmers in both the control and *regulating-ES* treatments.

- Chapter 4

To support our proposal of a 'system justification' effect (Martin et al., 2018), we explored the relationship between environmental awareness and the probability of finding the policy setting implemented fair in the *Fine* and *Fine-RD* conditions. We first created an index variable adding the variables *Environmental status*, *Prioritize environment* and *Lifestyle* from the pre-experimental survey. Then, based on the median of this index variable, we created the *Environmental awareness* dummy variable to classify farmers between those with high (takes the value 1) and low (takes the value 0) environmental awareness. Finally, we used a Probit model to test whether having a high environmental awareness increased the likelihood that a farmer perceives the fine policy fair in a disadvantageous context compared to an egalitarian context. Thus, we look at the interaction between the dummy variables *RD* and *Environmental awareness*.

We observed that the estimate of the interaction between *RD* and *Environmental awareness* was statistically significant, suggesting a correlation between environmental awareness and the probability of finding the policy setting fair in the *Fine-RD* condition. The average marginal effect of environmental awareness shows that the probability of finding the fine policy fair in the *Fine-RD* condition significantly increased by 35.3 points in farmers with the greater environmental awareness compared to less environmentally aware farmers.

Probit estimates

Dependent variable: Fair	Fine Policy	
Environmental awareness	-0.143	(0.340)
RD	0.194	(0.438)
Environmental awareness x RD	1.238*	(0.673)
Constant	-0.140	(0.269)
Observations	67	
Prob > chi2	0.207	

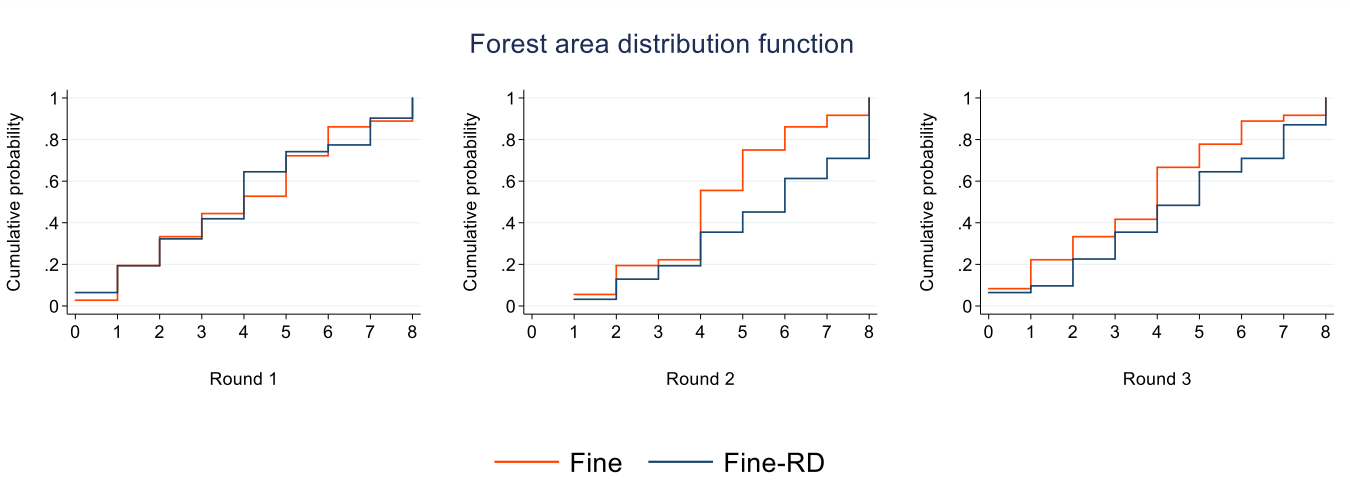
Note: Robust standard errors, clustered on the group level, in parentheses,
*** p<0.01, ** p<0.05, * p<0.1

Marginal effects

Dependent variable: Fair	Average Marginal Effect of Environmental awareness	
Fine-RD condition	0.353**	(0.143)
Fine condition	-0.056	(0.133)

Note: Robust standard errors, clustered on the group level, in parentheses,
*** p<0.01, ** p<0.05, * p<0.1

In addition, we graphed the distributions of the levels of forest conservation per round and fine policy condition and used a two-sample K-S test to evaluate if forest distribution on each round differed between conditions. The two distributions were significantly different in round 2. Choosing to keep 4 units (half of the forest) was the most frequent decision in the *Fine* condition, while choosing to keep 8 units (all forest) was the most frequent decision in the *Fine-RD* condition.



Two-sample K-S test for Round 1, p-value exact: 0.945
Two-sample K-S test for Round 2, p-value exact: 0.079
Two-sample K-S test for Round 3, p-value exact: 0.553
Two-sample K-S test for PA-RD and Mix-RD, p-value exact: 0.678

Appendix F Optimal allocation decisions in Chapter 4

Farmers had to decide how to allocate their land (L) between cattle farming and forest conservation. We denote the number of hectares in cattle farming as x_i. The marginal return of cattle farming (r_c) was higher than the marginal return of forest conservation (r_f). Thus, the baseline allocation decision of each participating farmer resulted in the following payoff function:

$$\pi_i = r_c x_i + r_f(L - x_i)$$

$$\pi_j = \frac{w\sum_i^n (L - x_i)}{n}$$

where $0 \leq x_i \leq L$ and $i = j = n$

Since the marginal social benefit of forest conservation was higher than the marginal return of cattle farming $(r_f + w > r_c)$, forest conservation was socially optimal. However, in the absence of social preferences, the strategy that maximized a farmer's payoffs was to allocate all their land to cattle farming because $r_c > r_f$.

Our interest was in measuring the effect of experiencing the same incentives under different contexts (disadvantageous or egalitarian). We expected a disadvantageous context to trigger feelings of unfair treatment compared to an egalitarian one, particularly among those who care about their relative outcomes compared to others, i.e., those who are averse to disadvantageous inequality. Thus, a farmer who is averse to disadvantageous inequality would include in its utility function the difference between his own payoff (π_i) and that of its neighbors (π_j). In the disadvantageous context, farmers in the neighboring area were always offered an additional payment P per each hectare they kept with forest.

For each analyzed scenario, we show below that in order to minimize the difference between payoffs, the amount of cattle farming x_i^* that each farmer chooses must always be larger than the amount of cattle farming chosen by the neighboring farmers (x_j).

1. *PES exclusion*

Payoff functions:

$$\pi_{j,pes} = r_c x_j + (P + r_f)(L - x_{j,pes})$$

$$\pi_{i,baseline} = r_c x_i + r_f(L - x_i)$$

Difference in payoffs:

$$\pi_{i,baseline} - \pi_{j,pes} = r_c x_{i,baseline} + r_f(L - x_i) - \left[r_c x_j + (P + r_f)(L - x_j)\right]$$

$$\pi_{i,baseline} - \pi_{j,pes} = (r_c - r_f)x_{i,baseline} - (r_c - P - r_f)x_{j,pes} - PL = 0$$

$$x^*_{i,baseline} = \frac{(r_c - P - r_f)x_{j,pes} + PL}{(r_c - r_f)}$$

2. Protected area under land-use restrictions surrounded by buffer area under PES

Payoff functions:

$$\pi_{j,pes} = r_c x_j + (P + r_f)(L - x_{j,pes})$$
$$\pi_{i,fine} = \rho\left[(r_c - S_{fine})x_{i,fine} + r_f(L - x_{i,fine})\right] + (1-\rho)\left[r_c x_{i,fine} + r_f(L - x_{i,fine})\right]$$

Difference in payoffs:

$$\pi_{i,fine} - \pi_{j,pes} = \rho\left[(r_c - S_{fine})x_{i,fine} + r_f(L - x_{i,fine})\right] + (1-\rho)\left[r_c x_{i,fine} + r_f(L - x_{i,fine})\right] - \left[r_c x_{j,pes} + (P + r_f)(L - x_{j,pes})\right]$$

$$\pi_{i,fine} - \pi_{j,pes} = \rho\left[(r_c - S_{fine} - r_f)x_{i,fine} + r_f L\right] + (1-\rho)\left[(r_c - r_f)x_{i,fine} + r_f L\right] - \left[(r_c - P - r_f)x_{j,pes} + (r_f + P)L\right]$$

$$\pi_{i,fine} - \pi_{j,pes} = (r_c - \rho S_{fine} - r_f)x_{i,fine} - (r_c - P - r_f)x_{j,pes} - PL = 0$$

$$x^*_{i,fine} = \frac{(r_c - P - r_f)x_{j,pes} + PL}{(r_c - \rho S_{fine} - r_f)}$$

3. Protected area with PES on top of land-use restrictions surrounded by buffer area under PES

Payoff functions:

$$\pi_{j,pes} = r_c x_j + (P + r_f)(L - x_{j,pes})$$
$$\pi_{i,mix} = \rho\left[(r_c - S_{mix})x_{i,mix} + (r_f + P)(L - x_{i,mix})\right] + (1-\rho)\left[r_c x_{i,mix} + (r_f + P)(L - x_{i,mix})\right]$$

Difference in payoffs:

$$\pi_{i,mix} - \pi_{j,pes} = \rho\left[(r_c - S_{mix})x_{i,mix} + (r_f + P)(L - x_{i,mix})\right] + (1-\rho)\left[r_c x_{i,mix} + (r_f + P)(L - x_{i,mix})\right] - \left[r_c x_{j,pes} + (P + r_f)(L - x_{j,pes})\right]$$

$$\pi_{i,mix} - \pi_{j,pes} = \rho\left[(r_c - S_{mix} - r_f - P)x_{i,mix} + (r_f + P)L\right] + (1-\rho)\left[(r_c - r_f - P)x_{i,mix} + (r_f + P)L\right] - \left[(r_c - P - r_f)x_{j,pes} + (r_f + P)L\right]$$

$$\pi_{i,mix} - \pi_{j,pes} = (r_c - \rho S_{mix} - r_f - P)x_{i,mix} - (r_c - P - r_f)x_{j,pes} = 0$$

$$x^*_{i,mix} = \frac{(r_c - P - r_f)x_{j,pes}}{(r_c - \rho S_{mix} - r_f - P)}$$

The following table describes the amount of cattle farming x_i^* that minimizes the difference between payoffs in each type of scenario analyzed given all possible cattle farming allocation decisions from neighboring farmers (x_j). Values are also approximated given the discrete decision presented in the experiment $(\sim x_i^*)$.

x_j	$x^*_{i,baseline}$	$\sim x^*_{i,baseline}$	$x^*_{i,fine}$	$\sim x^*_{i,fine}$	$x^*_{i,mix}$	$\sim x^*_{i,mix}$
0	3.43	4	12	8	0	0
1	4	4	14	8	8	8
2	4.57	5	16	8	16	8
3	5.14	6	18	8	24	8
4	5.71	6	20	8	32	8
5	6.28	7	22	8	40	8
6	6.86	7	24	8	48	8
7	7.43	8	26	8	56	8
8	8	8	28	8	64	8

Thus, we expect the spillover effect to induce farmers not to conserve their forest. In line with the poor enforcement capacity of many tropical protected areas in the Global South, we implemented a weakly enforced fine policy. However, in order to prevent the incentive effect from dominating the expected spillover effect, i.e., not observing differences between the treatment and control conditions because the fine in the control condition was too weakly enforced. We chose to implement a fine that would not induce a risk-neutral farmer to conserve but would induce a risk-averse farmer to conserve at least part of their forest.

In our experiment, farmers could be sanctioned (S) with probability $\rho = 0.5$, per each hectare with cattle farming x_i, and/or rewarded with a payment (P) per each hectare with forest $(L - x_i)$. This results in the following expected utility function:

$$E(x_i) = \rho U(y_1) + (1-\rho)U(y_2)\ ;\ y_1 = (r_c - S)x_i + \big(r_f + P\big)(L - x_i)\ ,\ y_2 = r_c x_i + \big(r_f + P\big)(L - x_i)$$

where $P > 0$ in the conditions under a policy mix, $P = 0$ in the conditions under a fine policy, $0 \le x_i \le L$, $r_c > r_f + P$ (i.e., the payment does not cover the opportunity cost) and $0 < S \le r_c$ (i.e., farmers cannot lose money during the experiment).

Assuming constant relative risk aversion $\left(U(y) = \frac{y^{1-\sigma}}{1-\sigma}\right)$, we use the power utility function and first find the set of parameters that would not induce a risk-neutral farmer $(\sigma = 0)$ to conserve their forest $(x_i^* > 0)$:

$$\frac{dE(x_i)}{dx_i} = pU'(y_1)\frac{dy_1}{dx_i} + (1-p)pU'(y_2)\frac{dy_2}{dx_i} > 0$$

$$\frac{dE(x_i)}{dx_i} = \rho\big(r_c - r_f - P - S\big)\frac{1}{{y_1}^{\sigma}} + (1-\rho)\big(r_c - r_f - P\big)\frac{1}{{y_2}^{\sigma}} > 0$$

$$r_c - r_f - P - \rho S > 0$$

Next, we find the x_i^* that maximizes expected utility for a risk averse farmer $(\sigma > 0)$:

$$\frac{dE(x_i)}{dx_i} = pU'(y_1)\frac{dy_1}{dx_i} + (1-p)pU'(y_2)\frac{dy_2}{dx_i} = \rho\big(r_c - r_f - P - S\big)\frac{1}{{y_1}^{\sigma}} + (1-\rho)\big(r_c - r_f - P\big)\frac{1}{{y_2}^{\sigma}} = 0$$

$$\frac{y_2}{y_1} = \left(\frac{(1-\rho)(r_c - r_f - P)}{-\rho(r_c - r_f - P - S)}\right)^{1/\sigma} = k$$

$$\frac{r_c x_i + (r_f + P)(L - x_i)}{(r_c - S)x_i + (r_f + P)(L - x_i)} = k$$

$$x_i^* = \frac{(r_f + P)L}{(r_c - r_f - P) + \frac{kS}{k-1}}$$

Since $\left(r_f + P\right)L > 0$, a risk-averse farmer would introduce cattle farming ($x_i^* > 0$) if $\left(r_c - r_f - P\right) + \frac{kS}{k-1} > 0$. This condition requires $k - 1 > 0$:

$$\left(\frac{(1-\rho)(r_c - r_f - P)}{-\rho(r_c - r_f - P - S)}\right)^{1/\sigma} > 1$$

$$r_c - r_f - P - \rho S < 0$$

Which was previously defined as a necessary condition for risk-neutral farmers. Given that $k > 1$ is meet and the payment does not cover the opportunity cost $\left(r_c - r_f - P > 0\right)$, condition 2 is always met. Therefore, the fine is weakly enforced because $x_i^* > 0$ for any level of risk-aversion.

However, depending on a farmer's level of risk aversion there could also be an internal solution. I.e. the policy could still induce some risk-averse farmer to conserve at least some of their forest ($x_i^* < L$):

$$\frac{(r_f + P)L}{(P + r_f - r_c) + \frac{kS}{k-1}} < L$$

$$k < \frac{r_c}{r_c - S}$$

$$\sigma > \frac{ln\left[\frac{(1-\rho)\left(r_c - r_f - P\right)}{-\rho\left(r_c - r_f - P - S\right)}\right]}{ln\left[\frac{r_c}{r_c - S}\right]}$$

Replacing the value of parameters we conclude that, in the fine policy, an internal solution is found for farmers with any level of risk aversion ($\sigma > 0$). In the policy mix, an internal solution is found for farmers with $\sigma > 0.239$.

The following table shows the distribution per type of policy of the levels of risk aversion elicited in the pre-experimental survey (σ), together with the amount of cattle farming that maximizes expected utility per risk aversion level (x_i^*), and the approximate value given the discrete decision presented in the experiment ($\sim x_i^*$).

Elicited risk aversion (σ)	*Fine policy*			*Policy mix*		
	Percentage	x_i^*	$\sim x_i^*$	*Percentage*	x_i^*	$\sim x_i^*$
2.000	6.25	1.09	1	7.46	0.99	1
1.558	14.06	1.43	1	7.46	1.28	1
1.172	7.81	1.93	2	7.46	1.7	2
0.790	9.38	2.93	3	13.43	2.53	3
0.585	15.63	3.96	4	16.42	3.43	3
0.426	9.38	4.98	5	11.94	4.41	4
0.352	15.63	6.01	6	10.45	5.62	6
0.251	12.5	7.16	7	10.45	7.67	8
0.097	9.38	7.99	8	14.93	14.18	8