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Forest conservation policy and motivational crowding: Experimental evidence from Tanzania

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ABSTRACT

It has been hypothesized that the effectiveness of payments for ecosystem services (PES) programs could, in some cases, be undermined by motivational crowding out, the detrimental interaction between new material incentives and payees' pre-existing intrinsic incentives. Of particular concern is the possibility for motivational crowding out to linger longer than the PES program itself. We use a modified, forest conservation-framed dictator game to test for potential persistent motivational crowding out among famers in the East Usambara Mountains, Tanzania, a global biodiversity hotspot. We apply four stylized policy treatments: an individual payments type PES, where farmers are compensated individually for donations they make to a recipient group (an action representing forest conservation); a collective payments PES, where a group of farmers are compensated as a whole for their donations; and two mandated levels of contribution, low and high, backed by penalties. The PES treatments did not induce significant, persistent motivational crowding, and the mandate treatments showed some evidence of a positive effect (motivational crowding in) beyond the policy period. We also found that motivational crowding in and motivational crowding out tendencies coexist within our sample, and that the sample subsets exhibiting these behaviors can be predicted by socio-demographic and farm characteristics.

1. Introduction

Payment for ecosystem services (PES) is an increasingly popular environmental policy approach where behavior is incentivized by conditional, material rewards (Engel et al., 2008; Ferraro and Kiss, 2002; Kemkes et al., 2010). While there are considerable theoretical advantages of PES over more traditional command and control or suasion approaches (Pagiola et al., 2005), the feasibility of PES is highly dependent on the particular socio-economic, political, cultural and biophysical context in question (Jack et al., 2008; Kemkes et al., 2010).

One element of context that must be considered is the target community's prevailing environmental attitudes and sources of motivation. The imposition of new incentives may interact with the pre-existing motivation structure that governs behavior. This occurs due to the existence of two distinct sources of motivation: extrinsic (for example, material rewards, penalties, and social recognition) and intrinsic (for example, enjoyment, interest and satisfaction) (Ryan and Deci, 2000). PES policies attempt to manipulate behavior by providing material incentives, a form of extrinsic motivation, and similarly, mandate approaches change extrinsic motivation by threatening penalties for non-compliance (Fehr and Rockenbach, 2003). In some cases it is possible for the new extrinsic motivators to either undermine or reinforce the existing intrinsic motivators (Bowles, 2008). If intrinsic incentives are undermined it is likely that the policy will under-achieve the expected environmental benefit, or lead to a net negative environmental effect (motivational crowding out), while reinforcing or augmenting intrinsic incentives could lead to a net positive effect (motivational crowding in). Of particular concern is the possibility for motivational crowding effects to linger longer than the policy itself (Reeson and Tisdell, 2008; Gneezy and Rustichini, 2000). Extrinsic incentives could alter agents' preferences and hence crowd in or out intrinsic motivation long after the original stimulus has been removed, an effect we refer to as *persistent* motivational crowding. This concern is relevant to PES given that many programs to date have operated for limited periods in pilot projects, based on finite funding arrangements (Farley and Costanza, 2010; Pagiola et al., 2007).

There is a considerable literature on various facets of the motivational crowding phenomenon (reviewed briefly in Section 2), which suggests possible causal mechanisms, as well as the contexts in which it occurs. Within the environmental policy literature, a number of authors have raised the hypothesized danger of motivational crowding out for PES (for example, Corbera, 2012; Farley and Costanza, 2010; Muradian et al., 2013). However, there remains a recognized lack of empirical evidence on the extent to – and conditions in which – PES may be adversely affected (Muradian et al., 2013; Rode et al., 2015; Wunder, 2013). Furthermore, the small number of empirical studies on this topic

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suggests that motivational crowding in under PES is also possible (see for instance d'Adda, 2011; Narloch et al., 2012; Vollan, 2008). The interaction between PES incentives and pre-existing motivation thus appears context dependent and in need of further empirical research (Muradian et al., 2013; Rode et al., 2015; Wunder, 2013).

In this study we use a forest conservation-framed field-lab experiment – a modified dictator game – to test for motivational crowding under four stylized forest conservation policies: an individual payments type PES, where farmers are compensated for any donation they make to a recipient group (an action representing forest conservation); a collective payments PES, where a group of farmers are compensated as a whole for their donations; and two mandated levels of contribution, low and high, backed by penalties. In doing so we aim to provide an experimentally controlled comparison of behavior between two commonly utilized PES types, as well as a comparison to more traditional regulatory based policies. While we are interested in motivational crowding that occurs under the policy treatment itself, we test specifically for persistent motivational crowding – that occurring after a policy is revoked. To the best of our knowledge this is the first field experiment to do so for both rewards and mandates.

The study context is the East Usambara Mountains, an area of montane subtropical forest in North East Tanzania. The East Usambara forests are recognized as one of the world's biodiversity hotspots, meaning that they support extremely high biodiversity yet face considerable threats from deforestation (Myers et al., 2000). Despite past conservation efforts, the threat from land clearing for smallholder agriculture and timber harvesting is ongoing. We undertook our experiment with East Usambara landholders, selected from two village districts with high potential for deforestation. The framing used presented participating farmers with choices thematically similar to, and described in terms of, their long term land management choices. At the time of this study, authorities and non-governmental organizations were exploring the potential for using PES to reduce clearing in the area (WWF, 2010), a prospect improved by the recent development of REDD+ (Reducing Emissions from Deforestation and forest Degradation) institutions in Tanzania.

In aggregate, we found no evidence for *persistent* motivational crowding in or out under either an individually or collectively paid PES treatment. Under the individually paid PES, we found evidence of partial crowding out during the policy imposition (as expected), however, the net average donation to the recipient group elicited by the PES incentive remained positive. The collective PES treatment was ineffective at eliciting donations. We found evidence for motivational crowding in under the mandate treatments, suggesting that such policies can act as positive signals regarding the community's or regulators' expectations. Notably, this effect persisted beyond the life of the treatment. We follow our aggregate results with a heterogeneity analysis. We found that motivational crowding in and motivational crowding out tendencies coexist within our sample, and that the sample subsets exhibiting these behaviors can be predicted from socio-demographic and land management characteristics.

These results and others are presented and discussed in Sections 5 and 6. Prior to this, Section 2 presents a brief synopsis of motivational crowding theory and a review of recent empirical work. Sections 3 and 4 describe the study site, and the experimental design and methods of analysis, respectively.

2. Review of Relevant Literature

2.1. Mechanisms for Motivational Crowding

Disparate strands of research, in both social psychology and economics disciplines, have given rise to alternative theories of motivational crowding. Given the existence of comprehensive surveys elsewhere (see for instance, Frey and Jegen, 2001; Bowles and Polania-Reyes, 2012; Festré and Garrouste, 2015), we provide here a relatively brief summary. The earliest theoretical explanation of motivational crowding, detailed in the social psychology literature, is based on Cognitive Evaluation Theory (CET) (Deci and Ryan, 1985). CET posits that individuals derive intrinsic motivation from feelings of *control* and *competence*. External stimuli which reduce these, say by undermining autonomy or reducing the possibilities for individual cognitive development, reduce intrinsic motivation and thus dedication to the task at hand. Self-Determination Theory (SDT), also developed by Deci et al. (1999), extends CET to incorporate a third factor: *social relatedness*, which states that individuals are further motivated by the quality of their relationships with others, and their self-perception of how others perceive them. This leads to the possibility that extrinsic incentives can impair self-esteem. For example, in the presence of external incentives, others may no longer be able to determine whether an action reflects an individual's character or their self-interest, and thus reduce their positive peer acknowledgement accordingly. Importantly, SDT theory proposes that extrinsic motivation may vary considerably in its relative autonomy. For instance, processes of *internalization* and *integration* describe the incorporation of the behavior and underlying value of an external incentive into an individual's "sense of self" (even while the motivation remains technically extrinsic) (Ryan and Deci, 2000). This gives rise to the possibility that the extrinsically motivated action will continue beyond the duration of the extrinsic incentive itself. The utility of SDT has been widely acknowledged, although debate remains in regards to whether extrinsic monetary rewards (as opposed to sanctions) should be expected to enhance or undermine individual control, and in what conditions (see meta-analyses by Cameron et al., 2001; Deci et al., 1999; and the summary survey by Festré and Garrouste, 2015).

This social psychology literature remained distinct from the economics literature on motivational crowding until more recently (Festré and Garrouste, 2015; Frey and Jegen, 2001). Although this is arguably due primarily to scholarly divisions, it is also because the implications of motivational crowding out are difficult to reconcile with key microeconomic tenants (for example, the relative price effect) in a traditional utility maximization framework such as a basic principal-agent model. Doing so has required the relaxation of the 'separability assumption' – that different sources of motivation can and should be considered independently (i.e. are additive) within a utility function. Bowles (2008) and Bowles and Polania-Reyes (2012) provide a number of theoretical explanations for why the separability assumption may fail. These include *framing effects* - incentives may change perspectives on a decision situation, leading to a change in what an individual considers to be 'appropriate' behavior. Profit maximizing behavior, for instance, may result from the imposition of a market, where more other-regarding behavior previously dominated. Similarly, *information content effects* may occur when new incentives convey information about the assumptions held by the policy administrators or society. These mechanisms are incorporated into an expanded principal-agent model based on utility maximization framework by Benabou and Tirole (2003). We leave further discussion of motivational crowding theory – beyond that required to interpret our results – to the introductory chapter of this special issue.

Empirical evidence for motivational crowding, particularly that from laboratory experiments, is abundant, although there exists some disagreement regarding conditions in which it is likely to occur (see meta-analyses by Cameron et al., 2001; Deci et al., 1999; and the literature surveys by Frey and Jegen, 2001; Bowles and Polania-Reyes, 2012). Given the existing reviews we again do not provide a detailed review and instead focus on the small literature specific to environmental management, which comprises mainly of studies using framed field experiments.[1] At least three studies find notable crowding results with regard to penalties. Using a common pool resource (CPR) game with rural Colombian villagers, Cardenas et al. (2000) found that a weakly enforced mandate crowded out social cooperation in the

[1] See Harrison and List (2004) for a taxonomy of economics experiments.

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context of firewood exploitation. Velez et al. (2010) also used a CPR game and found some evidence of motivational crowding out of pre-existing social cooperation among Colombian fishers. Jack (2009) used an investment game to represent a PES program in Kenya. She found that a weak enforcement mechanism caused a decrease in the investment made by upstream participants (which persisted after removal of the enforcement mechanism).

These experiments used penalties to motivate behavior; PES relies on rewards. To our knowledge, only three field experiments have formally tested motivational crowding under rewards, and none have tested for persistent motivational crowding. Vollan (2008) investigated the impact of rewards (and penalties also) in a CPR game with graziers in Namibia and South Africa. He found no evidence of crowding out from rewards (but some crowding out associated with penalties in high trust situations). Narloch et al. (2012) compared individual and collective PES type rewards in a public goods game played with farmers in Bolivia and Peru. They found that individual rewards crowded in reciprocity to others' contributions. Kerr et al. (2012) used an experiment in which participants had to undertake the actual environmental task in consideration: litter collection (in Mexico) and tree planting (in Tanzania). No conclusive evidence for motivational crowding was found, however dissatisfaction was higher among participants who received low payments than among those who were not paid. This small literature on environmental management and motivation suggests that externally set mandates and monetary incentives often – but not always – reduce intrinsic motivation during the treatment. It also suggests, tentatively, that mandates are more likely to do so than rewards, however studies with controlled comparisons are few. Research on persistent effects (i.e. those beyond the treatment period) is largely absent.

It should be noted that the above described studies used a CPR, public good or trust game, due to the stylized similarities between these games and the environmental problems these authors consider (firewood collection, fishing, water quality and grazing). We consider a dictator game to be a more appropriate representation of our socio-ecological situation.[2] At least three studies have similarly used a dictator game to investigate elements of motivational crowding in an environmental policy context (Alpízar et al., 2013; d'Adda, 2011; Kits et al., 2014). Of these, Kits et al. (2014) is notable in presenting evidence for persistent motivational crowding out. Their laboratory dictator game (with a student sample) simulated a conservation auction, a rewards allocation mechanism quite distinct from those considered elsewhere. We return to a number of these papers in our discussion.

3. Study Site Description

3.1. The East Usambara Mountains

The East Usambara Mountains, located in North Eastern Tanzania, form part of the Eastern Arc Mountain Range. They support humid subtropical forest in the wetter areas and deciduous woodland in drier areas, with an elevation gradient contributing to a diverse array of forest ecosystems (Burgess et al., 2007). The Eastern Arc Mountains have developed what is thought to be the highest endemic species density (per 100km^2) of any known ecosystem worldwide, and is a recognized 'Global Biodiversity Hotspot', a grouping of the most valuable and vulnerable ecosystems (Myers et al., 2000). Of the Eastern Arc forests, the East Usambara site is recognized as being one of the most biologically important (Burgess et al., 2007).

The human population of the East Usambara Mountains is growing rapidly due to high birth rates and local immigration. Most household income is generated by smallholder agriculture, which accounts for approximately 88% of employment (commercial estate farming accounts for a further 11%). Incomes are on average lower than those for the rest of Tanzania (Reyes, 2008).

3.2. The Agro-ecological Issue

The East Usambara forests have suffered from land clearing and logging (Newmark, 2002; Hall et al., 2009), with 57% of the original forest cover lost, mostly in the past 35 years (Newmark, 2002). A major, ongoing cause is smallholder agriculture. Original forest is thinned for within-forest cultivation of cardamom (*Elettaria cardamomum*), an important cash crop. Over time, the remaining tree cover is removed as yields fall due to nutrient deficiencies, and the field is used for cropping. A common conversion is to sugarcane, although conversion to perennial spices (cloves, cinnamon) or annual food crops (cassava, bananas, yams) also occurs. Like cardamom, these second stage crops also suffer from nutrient deficiencies over time, and eventually many plots are abandoned to woody weeds which limit forest regeneration. Of the remaining forest in the East Usambara Mountains, approximately 26% has already been planted with cardamom, meaning that the process of land conversion is underway (Reyes et al., 2006).

Intervention to protect the remaining forest could target several stages in the deforestation process. Farmers could be encouraged (via rewards or penalties) to maintain existing stands of original forest. Alternatively, or in addition, farmers could be encouraged to maintain existing agroforestry operations instead of converting to open land crops (such as sugarcane).[3] Although original forest provides the highest biodiversity benefits, maintaining agroforests (particularly those operated under recently developed 'improved' agroforestry practices) would be preferable to complete forest loss (Leonard et al., 2010). The principles of motivational crowding investigated in this study are likely to apply to both points of intervention.

4. Data and Methods

4.1. Experimental Approach

250 participants were randomly selected from village registries in the villages of Kwezitu and Shambangeda. These villages were selected for their relatively high numbers of farmers with forest or agroforest on their farms. Participation was conditional on being a farmer (land owner or manager) with primary or joint-primary decision-making responsibility. Participation rates were high (over 90% of available, invited farmers).

Ten farmers at a time took part in each 3 h session, which took place in a hall or house within the two villages. Farmers were divided randomly into two groups, one a group of dictators and the other a group of passive recipients. Each dictator farmer was presented with a stylized 'farm', consisting of 6 cards each associated with a cash value of between 20 and 60 Tanzanian shillings (TZS) (1500 TZS equaled approximately one U.S. dollar in 2010). The total value of each farm (which served as the original endowment) was thus between TZS 190 and 250 (USD 0.13–0.17). Each side of a card featured an illustrated depiction of a land use choice: sugarcane (representing private benefit) or forest (representing public, environmental benefit). Dictator farmers were then asked which cards they wished to keep for themselves (i.e. convert

[2] CPR games model situations in which a participant's resource use diminishes the resources available for others, but cooperation can increase aggregate resource availability. This does not characterize situations in which the benefits of environmental improvement are highly diffuse and non-competitive between ES providers, as is the case for biodiversity or carbon sequestration (Kits et al., 2014). In such situations, pro-conservation decisions entail a sacrifice, but are not strategic decisions to increase overall profits through cooperation. Such a situation instead corresponds broadly with the rules of the DG, in which a participant (the dictator) is presented with an endowment, and is asked to divide the endowment between himself/herself and a recipient participant. We describe our modified dictator game in Section 4.1.

[3] Which of these options (if any) is preferable would depend on community willingness, financial resources and a detailed assessment of the ecological benefits of both types of forest, data beyond the scope of this study. We do note, however, that PES for agroforestry should also make payments for original forest conservation to avoid perverse incentives.

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to sugarcane), if any, and which they wished to donate to the recipient farmer group (i.e. maintain as forest), if any.[4] Farmers had to physically remove, turn over and reattach cards on their decision board, placing them on either their sugarcane side, or their forest side, to make their decision (a process we considered analogous to choosing land use on each plot of a farm). We explicitly framed the experiment in these terms to resemble a key land use decision that farmers face. A realistic forest-conservation frame is expected to increase the policy relevance of findings relative to an abstract laboratory environment (Harrison, 2004; Handberg and Angelsen, 2015). Decisions were kept confidential and no communication between farmers was permitted. Donations from farmers were paid to the passive recipient group as a whole so monetary outcomes could not be associated with any one individual.[5]

Farmers played this baseline setup for 8–10 rounds,[6] before a treatment 'policy' was introduced without warning. The policy period continued for 8–10 rounds before the game reverted without warning to the initial setup for a final 8–10 rounds. Groups that did not face a treatment policy formed the control, and played the baseline setup for the whole game.[7] In all cases slightly different (randomly distributed) endowments were used (i.e. different 'farm' configurations), so each farmer played with differently valued cards in each round. This was to encourage farmers to consider their decision and associated tradeoffs each time, rather than simply repeating a past play. Each farmer played only one treatment (or the control).

Policy treatments took the form of either one of two reward schemes (imitating the basic principles of PES) or one of two mandate schemes (imitating the basic principles of environmental regulations). The individual PES treatment provided dictator farmers with compensation for each card they donated to the recipient group at a flat rate equal to the average value of the cards (TZS 40).[8] In the collective PES treatment, the same reward was placed in a central pool and divided equally between dictator farmers.[9] In both cases, the endowment donated to the recipient group was equal to the total value of the donated cards, as in the baseline procedure. PES payments were made on each player's running score sheet at the end of every round. Experimenters explained verbally, using a standard script, the payment being made and the reason for the payment, to each farmer individually (although care was taken not to reveal specific choices to other farmers to maintain anonymity).

The mandate treatments required a minimum donation to be made by each farmer in order to avoid risking a penalty. In the high mandate treatment, a donation of TZS 140 was required while in the low mandate treatment a donation of TZS 50 was required. This amounted to 55–75% and 20–26% of endowments respectively (variation due to different endowment amounts). The penalty for non-compliance (applicable in the mandate treatments) consisted of a fine equal to twice the discrepancy between the amount actually donated and the required donation (TZS 140 or 50). Two dictator farmers (out of the total of five) in each round were randomly selected for auditing, and if found to be in violation of the requirement, were penalized. Participants were aware of who was being audited, but the result of that audit (the imposition of a penalty or otherwise) was kept confidential. Although the penalty was double the payment discrepancy, the chance of audit was less than half (a 2 in 5 chance). Hence the expected value of compliance was deliberately set lower than the expected value of non-compliance.

The probability of audit used in the mandate treatments is higher than in other published studies. (For instance, Vollan (2008) used 20%, Cardenas et al. (2000) used 6.25% and Velez et al. (2010) used 10% in their field experiments on motivational crowding). The higher proportion was used firstly to improve the robustness of conclusions (a stronger regulation sends a more authoritative signal and is thus more likely to violate a participant's sense of *control*, see Section 2.1). Secondly, farmers in the East Usambara Mountains are familiar with strong enforcement of environmental regulations, particularly since the creation of the Amani Nature Reserve in 1997. Researchers typically use a partial enforcement rate to simulate the low quality of enforcement typical of environmental regulation in developing country contexts (Cardenas et al., 2000). However, the success of enforcement and the widespread understanding of regulations (as determined through discussions with landholders and local forest officers) in this area meant that we considered a higher likelihood of auditing to be more suitable.

Payouts were made confidentially at the end of the three hour session. The average payout was TZS 5200 (approx. USD 3.70). (For comparison, the local daily wage rate for farm labor was estimated to be TZS 2000 in 2009 (Bullock et al., 2011)).

4.2. Sample Profile

A short questionnaire was administered to dictator farmers (the game's decision makers) after the experiment to collect demographic, land use and attitudinal information (Table 1 and Fig. 1). Average land size owned or managed is 6.29 acres, with an average of 2.82 acres of agroforestry and 0.53 acres of original forest.

An important baseline characteristic is the depth of pre-existing other-regarding behavior and environmental attitudes. Motivational crowding out is considered to be more likely in situations where pre-existing pro-social or pro-environmental tendencies are strong, i.e. in those places where intrinsic motivation is likely to be high. In our setting, attitudes specifically related to forest conservation are most important. Our brief survey finds the strength of forest conservation attitudes ambiguous (Fig. 1). However, supporting evidence is provided by Morgan-Brown et al. (2010) who surveyed conservation attitudes and actions in Kwezitu village (one of our study sites). Pro forest conservation attitudes are also suggested by the practice of community-based forest management and joint forest management in the study region. These require voluntary village-based committees to set and enforce local regulations, with some assistance from government. In addition, the East Usambara region has received an unusually high level of attention from non-profit organizations that have, in at least some cases, successfully motivated improved environmental management without direct incentives or coercion (Vihemäki, 2009). There are thus grounds

Table 1
Summary demographic characteristics of sample (active 'dictator' participants, $N = 125$).

	Mean	St. dev
Gender (proportion male)	0.66	–
Age	44.78	13.95
Born in village (proportion)	0.32	–
No. adults in household	3.18	1.60
No. children in household	2.87	1.75
Self-reported annual income (USD)	790	852

[4] A simple notion of ecosystem services was used to explain this setup. Farmers were told that "… [donating money by keeping forests] represents the fact that when farmers leave forest on their farms, it benefits other people." Informal discussions with participants suggested that this is well understood by farmers in our study region.

[5] This introduced to our experiment the possibility of free riding, which is realistic given the environmental situation at hand. Individuals may care about a particular environmental outcome, yet behave on the expectation that others will bear the cost of action.

[6] A slight variation in the number of rounds (8–10) was used to prevent participants from anticipating the conclusion of the period.

[7] To maintain consistency in game proceedings between the control and treatment, control groups' play was similarly interrupted (with original instructions reiterated) after the initial (pre-policy) and middle (policy) period. For control and treatment groups alike, passive recipients were present when dictators received instructions, so dictators knew that recipients knew the terms of the game. Research assistants were not advised as to when and what the policy changes would be before they were announced to the group as a whole.

[8] Consequently, some cards were worth more and some were worth less than this amount, representing the common situation in which a flat PES payment may overcompensate or undercompensate for an action across heterogonous land parcels. This flat payment approach to PES is different to a reverse auction PES where payments try to match the opportunity cost of action (Kits et al., 2014).

[9] Hence farmers could receive a proportion of the PES payment even without donating. This allowed for possible free-riding, not only with regard to the overall outcome (the payment made to the passive recipients) but also with regard to the PES payments received by dictator farmers.

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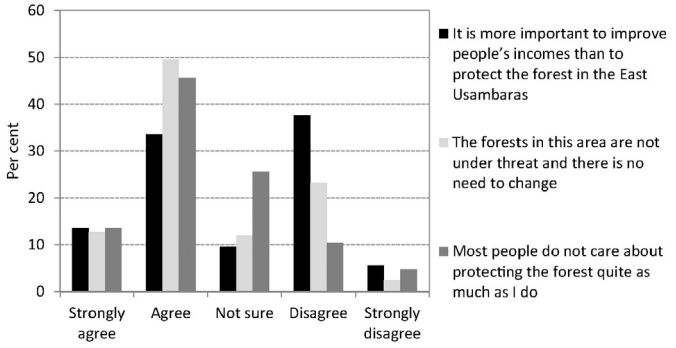

Fig. 1. Respondents' environmental attitudes (active 'dictator' participants, $N = 125$).

for believing that there exist at least some pro-environmental attitudes which could conceivably interact with new policies. It should be noted that this experiment measures other-regarding behavior - the donations to the passive group in the dictator game, to be exact. We use this as a *proxy* for forest conservation behavior given that we cannot manipulate and measure forest conservation behavior directly. However we expect that the game's forest framing helps to align these attitudes – other-regarding attitudes (e.g. care for the community) and forest conservation attitudes – in the game context.

4.3. Empirical Analysis

The experimental procedure described above gives panel data that can be analyzed using a difference in differences model. We specify the model as:

$$y_{iP}^{j} = \alpha_0 + \alpha_1 P_1 + \alpha_2 P_2 + \alpha_2 T_j + \beta_1 P_1 T_j + \beta_2 P_2 T_j + v_i^j + \varepsilon_{iP}^j \quad (1)$$

where y_{iP}^{j} is the amount of endowment donated by individual i in policy period P under treatment T_j. P_1 and P_2 are dummy variables representing the three policy periods ($P_1 = 1$ indicates the policy period, $P_2 = 1$ indicates the post-policy period). T_j is a dummy variable where $T_j = 0$ for the control subjects and $T_j = 1$ for subjects in treatment j. Hence β_1 represents the additional proportion of the endowment donated during the policy period, and β_2 represents the additional proportion donated in the post policy period, for the treatment group j. To account for the panel nature of the data we use individual random effects, $v_i^j \sim N(0,\sigma_v^2)$ with an idiosyncratic error term $\varepsilon_{ip}^j \sim N(0,\sigma_\varepsilon^2)$.

Any differences between the treatment groups except that caused by the policy is controlled by the non-interacted treatment dummy variable. Given that the treatment groups were selected randomly we expect this to be non-significant. The non-interacted policy dummy variables control for any change in generosity over time unrelated to the imposition of the policy to the treatment group.

The model specification described above assumes that the vector explaining policy impact is homogenous across individuals. One method for considering heterogeneous preferences is to include socio-demographic variables explicitly (accounting for known sources of heterogeneity). However, it is also possible to consider the inclusion of unobservable sources of heterogeneous preferences using a latent class model (LCM). An LCM can be considered a semi-parametric version of a random parameters model, one in which parameters are distributed discretely. This allows for the identification of distinct 'classes' of respondents, and does not require any assumption on the distribution of parameters, beyond the number of classes. Membership in a particular class, s, is based on a latent membership likelihood function (M_{is}^{*}) of socio-demographic characteristics. The latent membership function has both an observed ($A_s X_i$) and unobserved component (ϵ_{is}):

$$M_{is}^{*} = A_s X_i + \epsilon_{is} \quad (2)$$

Table 2
Variables hypothesized to be determinants of amount donated in dictator game.

Variable	Description
Policy	Round is part of the during-policy period. Policy is applied to the treatment group (dummy, 1 = policy period).
Post policy	Round is part of the post-policy period group (dummy, 1 = post-policy period).
Treatment	Participant is a member of the treatment group (dummy, 1 = member of treatment group).
Land	Land area owned or managed by participant (acres)
Gender	Male/Female (dummy, male = 1)
Age	Participant's age (years)
Local to village	Participant was born in the village currently lived in (dummy, 1 = born in village)
No. children	Number of children in participant's household.
Possessions index	Number of possessions owned by participant's household selected from a set list (radio, motorcycle, mobile phone, cow, bicycle, television) (0–6 index).

where A_s is a coefficient vector specific to segment s that is associated with the observable determinants (X_i) of individual i's membership. If errors are assumed to be identically, independently distributed as type 1 extreme values, the probability of individual i belonging to segment s is given by a multinomial logit:

$$P_i(s|X_i) = \frac{e^{A_s X_i}}{\sum_{s \in S} e^{A_s X_i}} \quad (3)$$

Selection of the number of classes is not guided by formal criteria; instead class selection is based on log likelihood statistics and information criteria, and plausibility of results given the size of membership classes and the size of standard errors (Boxall and Adamowicz, 2002; Scarpa and Thiene, 2005).

Variables included in both standard and latent class models are presented in Table 2. In all analyses, the first round in each period was dropped to avoid including spurious choices made while participants became accustomed to the game rules.

5. Results

5.1. Contemporaneous Impact of Policy Treatments on Donations

Individual PES and both mandate treatments were successful at eliciting higher donations during the policy period (Table 3). The difference in donated amounts between the treatment and control groups (referred to hereafter as the 'premium'), ranged from 6.3% under the individual PES treatment to 22.5% under the high mandate treatment. Percentages are reported relative to the initial endowment amount to allow for consistent comparisons across treatment groups.

The collective PES treatment generated no premium during the policy period. This was likely due to a free rider effect, given that under the rules of this treatment, dictators could keep their own endowment yet still receive a share of the compensation made for other dictators' donations. The individual PES treatment generated a relatively small premium, 6.3%. The altruism demonstrated in the pre-policy period and by the control group (quantified by the donation of approximately 35% of their endowment on average) appears to have been largely replaced by financially incentivized 'donations'. Farmers donated an average of 41% of their endowment, an amount very close to the profit maximizing amount[10] (42% on average, or between 28 and 61% depending on the particular endowment). This suggests that extrinsic incentives

[10] Farmers could profit maximize under this treatment by donating their low value cards (<40 TZS) and receiving for them flat compensation of 40 TZS, equivalent to farmers who conserve only their low value land under a flat rate PES program. As expected, the individual PES treatment players kept a higher proportion (an increase of 3.54%) of their high value cards relative to control group players during policy-period rounds.

Table 3
Random effects panel estimates of difference in differences models for four simulated policy treatments.

	Standard PES		Collective PES		Mandate–high		Mandate–low	
Dep. var. = donation	Coef.	Std. error	Coef.	Std. error	Coef.	Std. error	Coef.	Std. error
Constant	0.369	0.035***	0.369	0.038***	0.369	0.037***	0.369	0.041
Policy	−0.020	0.014	−0.020	0.014	−0.020	0.014	−0.020	0.013***
Post policy	−0.044	0.014***	−0.044	0.014***	−0.044	0.014***	−0.044	0.013***
Treatment * policy	0.063	0.020***	0.028	0.020	0.225	0.022***	0.094	0.021***
Treatment * post policy	−0.017	0.020	0.026	0.020	0.103	0.022***	0.065	0.021***
Treatment	0.014	0.049	−0.020	0.056	−0.043	0.058	−0.001	0.063
Adjusted R-squared[a]	0.023		0.004		0.07		0.017	
Number of obs.	1392		1272		1128		1152	
Number of participants	59		53		47		48	

*** Significant at $\alpha = 0.01$ level.

[a] Total explanatory power of the regression models in Table 3 is relatively low (R-square 0.004–0.07), likely due to the high variability of human behavior in an experimental field-lab setting and relatively small sample sizes. Given that our focus is on causal inference rather than predictive modelling, our key conclusions remain valid. However, results should be read as average treatment effects rather than predictions of the behavior of a particular individual in a particular institutional regime.

(money) and intrinsic incentives (altruism, sense of responsibility to recipient farmers) in this context are largely non-additive, as defined by Bowles (2008). This is known in the psychology literature as the 'traditional' crowding effect, and is expected in cases where altruism drives initial donations (Frey and Stutzer, 2006).

Both mandate treatments generated a premium during the policy period. The donation amount required in order to comply with the mandate was TZS 140 in the high treatment and TZS 50 in the low treatment, representing 48–56% and 17–20% of farmers' endowments respectively. In the high treatment, the requirement was designed to be 'binding' in the sense that it would be higher than what participants on average contributed voluntarily. The low treatment was designed to be less than the average amount contributed voluntarily. A further design feature incorporated into both mandate treatments was that the expected value of compliance was slightly less than the expected value of non-compliance (see Section 4.1).

From this, two noteworthy results arise. Based on the expected value alone we would expect neither mandate treatment to have an impact from a purely self-interested perspective. As the fine imposed is 200% of the donation discrepancy (the difference between the donation and the amount required) yet the chance of being audited is only 40%, the expected value of non-compliance is 120% of the expected value of compliance. The positive response seen can only be explained by non-pecuniary influences. These could include a framing effect (see Section 2.1), where the newly imposed policy context signals what is considered appropriate behavior by other players or by the

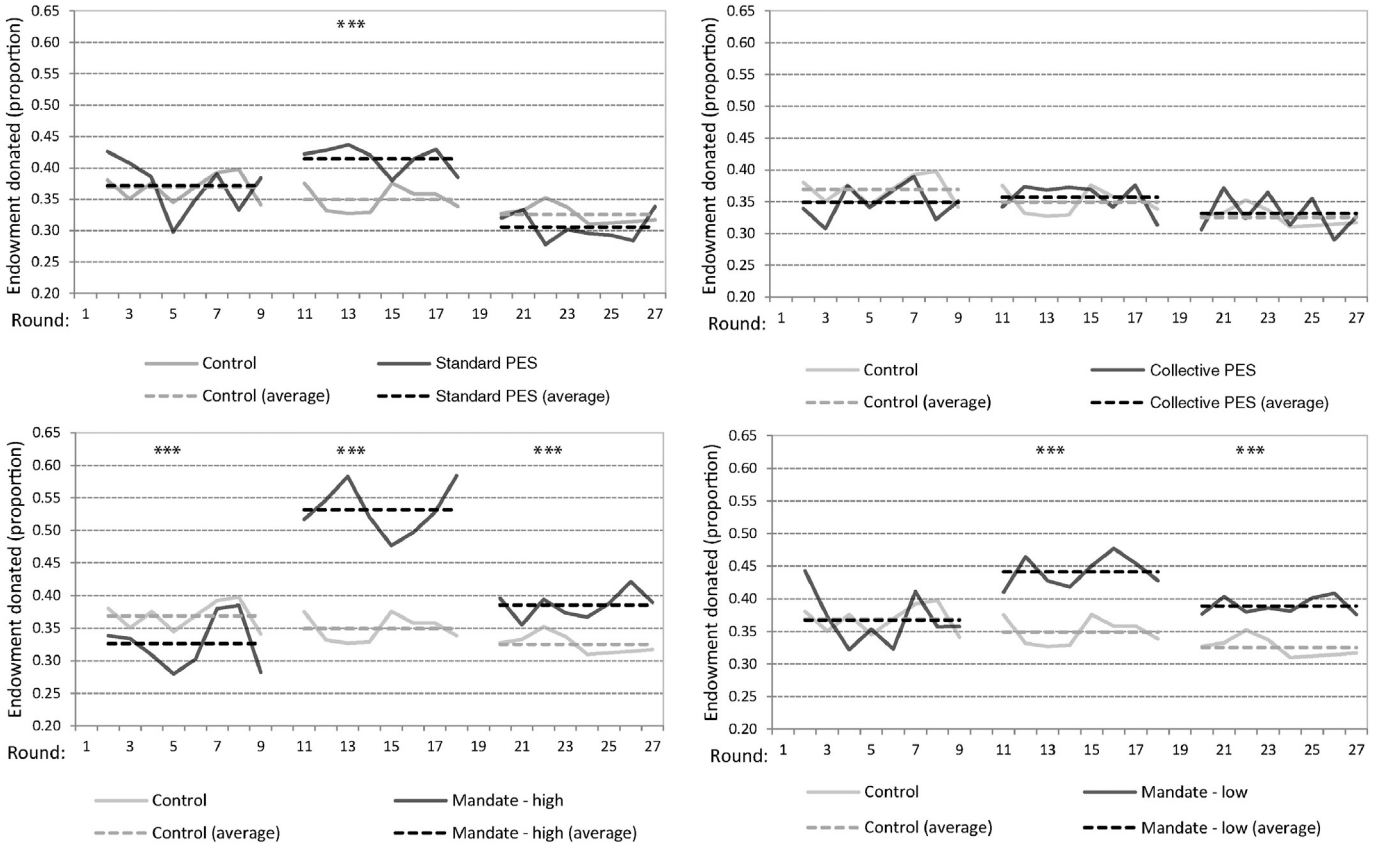

Fig. 2. Comparison of treatment and control groups across periods. First section: pre policy, second section: during policy, third section: post policy. Y-axis is the proportion of endowment donated to the recipient group in each round. *** = significant difference between treatment and control at $\alpha = 0.01$ level.

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experiment's administrators. This effect was quite pronounced: Participants did not only comply with these mandates, in the case of the low mandate, they went well beyond its requirements. Although the low mandate demanded only 17–20% of the participant's endowment, average donations were 44% of the endowment, significantly higher ($p < 0.01$) than the control group (which had average donations of 35%). Hence there is some evidence of motivational crowding in as a result of the mandate policy simulations. Donation amounts were higher than they would have been under either strict compliance with the mandate, or under intrinsic incentives alone (as indicated by comparison with the control).

5.2. Persistent Impact of Policy Treatments on Donations

The variable treatment * post policy remained non-significant under both PES policy scenarios and hence there is no evidence for persistent motivational crowding under these treatments. Following the PES policy period, amounts donated returned to a level comparable to that under the control treatment. In the case of the mandate treatments, amounts donated fell from their peak achieved during the policy period, however remained at levels significantly higher ($p < 0.01$) than the control. The high mandate policy delivered an ongoing 10.3% premium while the low mandate policy delivered an ongoing 6.5% premium. The post-policy period lasted for approximately the same length of time as the policy period (8–10 rounds), and while it is of course not possible to claim that preferences have changed permanently, there is no sign of a decrease over the span of the post policy period. This suggests there has been a preference change that has caused motivational crowding in of intrinsic motivation. The results reported in this section are evident in Fig. 2 which presents graphically the average proportion of endowment donated under each treatment across time.

5.3. Heterogeneity Analysis

The results presented above consider each participant to have identical unobserved preferences. To the extent to which the sample is drawn randomly from the population, this is a fair assumption for the purposes of predicting the response likely to be made by the population in total (i.e. the average treatment effect). However, the collection of demographic, land use and attitudinal information (see Fig. 1, Table 2) allows us to explore responses made by subsections of the sample also. Panel latent class models with two classes were estimated for this purpose (Table 4).

Immediately apparent are strong bifurcations in responses to the policy treatments. In the case of the individual PES treatment, 27% of the subsample (class 1) shows no statistically significant response, either during the policy period or in the post-policy period. 73% of the subsample (class 2) shows a negative response (motivational crowding out) in the post-policy period as well as a tendency to donate more in general. The average of these two sets of coefficients, weighted by the class probabilities, gives the aggregate model (Table 3) (which shows no evidence of significant motivational crowding out in the post policy period).

The distinction between classes is more marked in the mandate treatments. The high mandate appears to be particularly polarizing, with 62% of the subsample (class 1) exhibiting motivational crowding

Table 4
Panel linear regression latent class model with two classes.

	Standard PES		Collective PES		Mandate–high		Mandate–low	
Dep. var. = donation	Coef.	Std. error	Coef.	Std. error	Coef.	Std. error	Coef.	Std. error
Latent class model 1								
Constant	0.694	0.035***	0.492	0.028***	0.485	0.028***	0.576	0.025***
Policy	0.014	0.041	−0.011	0.032	−0.022	0.033	0.026	0.038
Post policy	−0.122	0.035***	−0.075	0.034**	−0.083	0.034**	−0.088	0.038**
Treatment * policy	−0.002	0.058	−0.006	0.051	0.084	0.053	−0.020	0.073
Treatment * post policy	0.026	0.054	0.015	0.054	0.207	0.056***	0.108	0.072
Treatment	−0.022	0.038	0.038	0.034	−0.058	0.039	0.113	0.044**
Sigma	0.171	0.018***	0.236	0.013***	0.241	0.010***	0.217	0.012***
Latent class model 2								
Constant	0.246	0.016***	0.206	0.016***	0.184	0.013***	0.234	0.014***
Policy	−0.009	0.019	−0.009	0.021	−0.006	0.017	−0.012	0.018
Post policy	−0.009	0.020	0.000	0.022	0.011	0.018	−0.010	0.018
Treatment * policy	0.032	0.025	0.029	0.028	0.426	0.029***	0.117	0.027***
Treatment * post policy	−0.058	0.026**	0.038	0.029	−0.064	0.027**	0.026	0.026
Treatment	0.033	0.018*	−0.073	0.021***	0.000	0.019	−0.047	0.018**
Sigma	0.139	0.007***	0.104	0.006***	0.084	0.006***	0.124	0.005***
Probabilities for class membership (class 1)								
Constant	−1.530	0.544***	−2.433	0.595***	−2.817	0.675***	−1.929	0.594***
Land area (acres)	−0.001	0.001**	−0.029	0.025	−0.002	0.001	−0.067	0.035*
Gender (male = 1)	0.381	0.181**	1.139	0.205***	0.205	0.237	0.437	0.215**
Age (years)	0.009	0.006	0.022	0.009***	0.058	0.012***	−0.004	0.009
Born in village	0.351	0.198*	0.298	0.230	−0.030	0.254	−0.158	0.280
No. children	−0.074	0.053	0.118	0.052**	−0.014	0.070	0.356	0.082***
Possessions index	−0.008	0.067	0.227	0.103**	0.225	0.097**	0.239	0.083***
Prior class probabilities at data means for LCM variables								
Class 1	0.27		0.55		0.62		0.33	
Class 2	0.73		0.45		0.38		0.67	
Diagnostic statistics								
No. obs.	1391		1272		1128		1152	
No. participants	59		53		47		48	
Deg. freedom	21		21		21		21	
Log likelihood function	172.484		147.076		171.675		156.219	
AIC	−302.967		−252.151		−301.350		−270.437	
BIC	−192.959		−144.036		−195.758		−164.403	

* = significant difference between treatment and control at α = 0.1 level, ** = significant at α = 0.05 level, *** = significant at α = 0.01 level.

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Table 5
Socio demographic characteristics associated with each latent class of respondents for three different treatments[a]. Italicized characteristics increase the probability of a farmer being in that particular class for a given treatment. Statistically significant responses to policy treatments are indicated in bold. Classes are independent across treatments (i.e. there is no relationship between class 1 under one treatment and class 1 under another treatment).

	Standard PES	Mandate–high	Mandate–low
Latent class 1	No policy response No post-policy response No change in donation amounts *Smaller land owners/managers* *Male* *Born in village*	No policy response ***Positive post-policy response** No change in donation amounts *Older* *More possessions (wealth proxy)*	No policy response No post-policy response ***Higher donation amounts** *Smaller land owners/managers* *Male* *More children in household* *More possessions (wealth proxy)*
Latent class 2	No policy response ***Negative post-policy response** ***Higher donation amounts** *Larger land owners/managers* *Female* *Not born in village*	No post-policy response ***Negative post policy response** No change in donation amounts *Younger* *Less possessions (wealth proxy)*	***Positive policy response** No post-policy response ***Lower donation amounts** *Larger land owners/managers* *Female* *Fewer children in household* *Less possessions (wealth proxy)*

[a] The collective PES treatment is omitted due to limited statistically significant results.

in during the post-policy period and 38% of the subsample (class 2) exhibiting motivational crowding out. The motivational crowding in effect is particularly strong, with a 20.7% premium. The crowding out effect is milder, at −6.4%. The net effect for the whole treatment is motivational crowding in (Table 3). The low mandate treatment has a less marked bifurcation. 33% of the subsample (class 1) shows no response during or after the policy imposition, while 67% of the subsample (class 2) is responsible for the motivational crowding in response during policy imposition that is evident in the aggregate results. Neither class under the collective PES treatment shows any evidence of response during or following the policy. By including socio-demographic variables in the LCM we can estimate probabilities of an individual falling in one class or another based on their individual specific characteristics. Farmers with larger landholdings, women, and farmers not born in the village are more likely to exhibit crowding out behavior under the individual PES, while younger, poorer farmers are more likely to be crowded out by the high mandate (Table 5).

6. Discussion

The social psychology literature (see Section 2.1) states that motivational crowding out is likely when a treatment is seen as restrictive (i.e. diminishes individual control) while motivational crowding in is likely when a treatment is seen as enabling (i.e. facilitates the ability of an individual to make a desirable choice) (Cameron et al. 2001; Deci et al., 1999). Both types of treatment can also signal expectations regarding what is and is not desirable behavior, and thus reframe decisions in ways leading to outcomes not well predicted by a standard profit maximization framework (Bowles, 2008).

The slight positive impact of individual rewards (during the policy period) is not indicative of motivational crowding out, and suggests that this intervention was not seen as restrictive by participants in aggregate. However, the relatively small positive treatment impact, combined with the fact that the average amount donated was close to the average profit maximizing amount, suggests that the reward incentive largely replaced the preexisting intrinsic motivation. Although this is not a demonstration of net motivational crowding out, as the treatment effect remained positive (Frey and Stutzer, 2006), it is suggestive of non-separable motivation sources. The resultant effort is less than the sum of effort expected from each source of motivation in isolation: profit maximization of the monetary incentive, and the preexisting non-monetary motivation (as seen in the control). The lack of persistent crowding suggests no lasting change to preferences.

Like Narloch et al. (2012), we found collective rewards to be ineffective in inducing greater donation amounts, likely due to the potential for free-riding. This result is supported by a choice experiment survey undertaken in the same study area (Kaczan et al., 2013). In this study, no interest was found among farmers for undertaking forest conservation in return for (hypothetical) collective payments to a village fund. This result is also supported by field experiment and choice experiment findings by Kerr et al. (2012). They performed an experiment in which Mexican villagers were asked to collectively undertake litter collection in return for collective payment – a request which was unmet in cases of low trust. They also report that a (hypothetical) collective payment, to a local school, was ineffective at eliciting garden work among Tanzanian villagers. Nevertheless, we consider collective PES (and similarly, collective mandates) to be worthy of future study. Collectively-applied interventions could harness positive social relations between community members, thus maintaining motivation through the psychological need of *relatedness* (see Section 2.1). In collective situations, the common nature of payments and penalties means that other-regarding behavior is still required and thus reputational benefits to that behavior are maintained. Similarly, any intrinsic benefits ('warm glow') accruing to other-regarding behavior are not diminished. Our experiment failed to shed light on these predictions, as motivational crowding out in a rewards framework was not apparent in the aggregate model. However, in situations where motivational crowding out is apparent under individual rewards or penalties – in circumstances that reduce control or feelings of competence – moving to a collective framework (with increased social relatedness) may represent a partial remedy.

With regard to the mandate treatments, there is some theoretical support for the motivational crowding in we observe. In cases where the regulations are (1) considered fair, and (2) considered likely to encourage others to behave in a socially beneficial manner, enforcement may attract the support of agents who are more comfortable operating in a fair and controlled policy environment (Fehr and Rockenbach, 2003; Reeson and Tisdell, 2008). We hypothesize (although cannot test) that farmers were comfortable with the required donation amounts, likely due to the fact that they prevented free riding (because donations were pooled before being divided evenly among recipients for payment). The mandates ensured that the passive group – members of whom were often known by those in the dictator group – received some payment, and that all members of the dictator group contributed to that payment.

The positive implications of the mandates for the passive players, and the likely existence of social links between passive and dictator players, may explain the difference between these findings and those elsewhere in the literature. Vollan (2008) (using a CPR game), Cardenas et al. (2000) (also using CPR) and Cardenas et al. (2011) (using an asymmetric appropriation game) found evidence of motivational crowding out due to a regulation treatment, a result they ascribed to the 'restrictive' nature of the intervention. These games do not have outside players (passive recipients) like the dictator game.

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Another possible reason for our positive mandates finding (a lack of motivational crowding out, in aggregate) may be the particular legal context of the East Usambara region. Environmental regulations, although imperfectly enforced, are relatively comprehensive due to the recognized environmental value of the area, and long-standing presence of environmental organizations and agencies (Vihemäki, 2005). For example, rules (with steep penalties) govern the cutting of particular timber tree species, and the harvesting of fuelwood in forest reserves is tightly controlled. Forestry officers are present in the area due to proximity to nature reserves and department of forestry facilities. It is plausible that exposure to – and some level of support for – this environmental regulation leads to higher compliance than seen in other framed experimental studies. Variation due to the social or cultural environment is well recognized in the experimental literature (Velez et al., 2010).

Two additional results are noteworthy with regard to the mandate treatments. Neither mandate is worth obeying from a dictator player's pure self-gain perspective, given that the expected value of non-compliance is greater than the expected value of compliance (see Section 4.1). As suggested by Cardenas et al. (2011), and as predicted by theory (Bowles, 2008; Bowles and Polania-Reyes, 2012) these results may indicate that the mandates were perceived by players to be signaling a socially appropriate donation amount, or reframed the need to donate as a more urgent cause than previously thought. Given that participants not only complied with these mandates, but went considerably beyond them, we expect that some element of reframing occurred in addition to signaling and in addition to behavior change due to the direct monetary incentive. These information signals and perspectives could plausibly persist through time despite the policy relaxation in subsequent rounds. At least two other studies present similar over-compliance results from mandates (see Lopez et al., 2012; Velez et al., 2010). In our findings and in these similar findings, it is likely that the mandate provided not only an incentive but information and framing which participants were willing to act on.[11]

Our experiment deliberately tested the motivational crowding impacts of rewards (PES) and mandates in independent treatments, to provide an experimentally controlled comparison of behavior under these two broad policy approaches. We wish to note (1) a caveat, and (2) an important difference between this experimental setup and PES in practice. With regard to point (1), we recognize that our PES treatments could contain implicit perceived or real social penalties. Farmers' failure to provide 'adequate' donations (in the opinion of the recipients) could lead to social sanctions after the game. However, this is not unlike real environmental actions under a PES which could be influenced by the possibility of social sanction from community members along with the loss of monetary reward in the case of non-compliance. With regard to point (2), most PES programs are implemented in a context in which environmental regulation (and not just social convention) already exists. In the East Usambaras, farmers under any future PES program would make choices based simultaneously on potential incentive payments and the existing environmental regulations. It is plausible that the imposition of PES, perhaps through a framing effect, could change the effectiveness of existing regulations even for farmers *not* covered by the new PES. This possibility appears largely unstudied.

7. Conclusion

We wish to highlight two points. First, neither PES nor mandate treatments were associated with *persistent* motivational crowding out. Intrinsically motivated donations were not diminished by the imposition and removal of a policy simulation. In fact, for the cases of the mandate treatments, there is evidence for persistent motivational crowding in, where donation amounts remained high even after the policy simulation had concluded. Notable was the way in which the mandate itself was more significant than the mandated amount, with even an inconsequential mandate impacting behavior. To our knowledge this is the first field study that considers persistent impacts of both rewards and mandate policies. Broadly speaking, our study helps address the claim made by Wunder (2013), that there is a lack of evidence supporting the concern that motivational crowding will undermine PES. Our results do not contradict this claim. Although we see evidence of a considerable substitution between intrinsic and extrinsic motivation under an individual PES treatment, the net effect was positive (i.e. the individual PES treatment increased contributions, albeit by less than the amount expected if motivations were additive).

Second, we find that a particular experimental treatment can invoke significantly different responses from subsets of the same sample, even when that sample is relatively homogenous in terms of key socio-demographic characteristics. We find that both high and low mandates caused persistent motivational crowding in and motivational crowding out simultaneously. Given that an individual's likelihood of falling into either a motivational crowding in or motivational crowding out subsample can be partially explained based on his/her socio-demographic characteristics, it may be possible for policy to be targeted at particular subsections of a population which are most likely to respond in the desired manner (i.e. by showing motivational crowding in). This is a possibility raised previously (Broch and Vedel, 2012), however the relatively weak explanatory results differentiating farmers between categories means further research on techniques to do so is required. In addition, consideration would need to be given to the potentially detrimental effects of such discrimination. For instance, Alpízar et al. (2013) used a field experiment with Costa Rican farmers to show that PES targeting criteria (which render eligible some farmers and exclude others) can have adverse consequences on the behavior of those who are excluded. They found that targeting farmers who were likely to conserve less in the absence of the program (i.e. 'rewarding the unmotivated') reduced the intrinsic motivation of those excluded (i.e. those not targeted). Restricting participation based on geographic region did not. This suggests that if policy makers attempt to avoid motivational crowding out by targeting farmers less susceptible to the effect, they could reduce intrinsic motivation among other farmers. Thus targeting to avoid motivational crowding out is likely to work only when targeting criteria are considered fair.

We recognize that disparities between the experimental situation and a real policy are large, and the results of this study alone are insufficient grounds on which to base policy changes in the East Usambara study region. One important disparity is the difference in beneficiary group in the game compared to that in reality, despite the forest game framing used. Farmers in the game made a sacrifice for the sake of other farmers. In reality, farmers practicing conservation at the expense of profits are making a sacrifice for an environmentally concerned national and global community. A useful extension to this research would be to improve the direct comparability of the experiment and PES policy. One component of this could be to use a more distant beneficiary (people in a nearby town, for instance, who benefit from the area's hydrological services), while another would be to use real environmental tasks rather than donations (for example, see Kerr et al., 2012, whose experiment required Tanzanian farmers to plant trees in return for payment). In addition our study does not directly consider image concerns: a desire to maintain or build reputations for prosociality. Monetary rewards are known to decrease the image value of some activities such as community volunteerism (Carpenter and Myers, 2010). Farmers in each session group of 5 did face scrutiny from their recipient group so some level of group image concern remained, yet it is plausible that in conditions of direct peer observation,

[11] We note that an explanation based on social determination theory (see Section 2.1) would require increased feelings of autonomy, competence, and/or possibly relatedness (the latter from, for example, a hoped-for improved social connection with recipient counterparts) in this setting. Our results would also suggest that persistence of those feelings beyond the treatment period is required. Given that we lack evidence on such psychological pathways we do not speculate further on an SDT explanation for this finding.

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responses may have been more strongly negative. Within the limits set by this and other experimental stylizations, however, these results suggest that temporary rewards are not incompatible with persistent altruistic or pro-social behavior. In addition, mandates enforced with penalties, can, in the right form, can have a complementary effect on pre-existing voluntary behavior, both during and after their imposition.

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