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Enhancing Spatial Coordination in Payment for Ecosystem Services Schemes with Non-Pecuniary Preferences

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Abstract

The environmental benefits from Payment for Ecosystem Services (PES) schemes can often be enhanced if private land managers are induced to enrol land in a spatially coordinated manner. One incentive mechanism which has been proposed to achieve such spatial coordination is the agglomeration bonus, a two-part payment scheme which offers a pecuniary (financial) reward for decisions that lead to greater spatial coordination of enrolled land. However, farmers respond to a range of motives when deciding whether to participate in such schemes, including non-pecuniary motives such as a concern for the environment or social comparisons. This study implements a de-contextualised laboratory experiment to test the effectiveness of the agglomeration bonus when non-pecuniary motives are explicitly incorporated into the decision-making environment. We capture intrinsic preferences for the public good dimension of environmental improvement through a real donation to environmental charities and examine the relative impact of a group-ranking nudge. The experimental results show that the agglomeration bonus does indeed improve participation and spatial coordination when non-pecuniary motives are accounted for, but that its performance is not enhanced by the nudge.

Keywords: Spatial Coordination; Agglomeration Bonus; Coordination games; Nudge; Social comparison; Laboratory experiments; Environmental preferences.

JEL codes: C91; C92; Q15; Q18; Q57

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Introduction

Spatial coordination of land enrolment is often a key determinant of the environmental effectiveness of Payment for Ecosystem Service (PES) schemes, since the provision of ecosystem services and the conservation of biodiversity can depend on the spatial configuration of land use (Goldman, Thompson and Daily, 2007; Wünscher, Engel and Wunder 2008; Polasky et al., 2014; Cong et al., 2014; Fooks et al., 2016). Examples of environmental objectives benefitting from spatial coordination of participants include flood alleviation through wetlands enhancement, the protection of riverbanks, the creation of wildlife corridors, and species re-introductions. Encouraging spatial coordination of participating farmers can indeed be key to realising the full potential environmental benefits of PES schemes.

Parkhurst et al. (2002) proposed the use of an "Agglomeration Bonus" (AB) to tackle this spatial coordination problem. Uniform payments made to land managers choosing to participate in the PES scheme are topped-up with an additional payment if the enrolled plot is contiguous to a plot enrolled by at least one other land manager. Typically, the total contiguity bonus paid to an individual landowner is increasing in the number of direct neighbours who participate in the scheme (e.g., Banerjee et al., 2014). The coordination game created by the AB can generate multiple Nash equilibria in pure strategies, which can then be ranked in terms of Pareto dominance and risk dominance[1] (Parkhurst et al., 2002).

[1] A Nash equilibrium is a Pareto dominant equilibrium when no player can increase his utility without reducing the utility of at least one of the other players. A Nash equilibrium is Pareto dominant if it is Pareto superior to all other Nash equilibria in the game and is therefore always preferred. A Nash equilibrium risk-dominates another Nash equilibrium if it less risky for all players.

Many studies have evaluated the performance of the AB under a range of circumstances through the use of laboratory experiments with student subjects[2]. The AB has shown to have positive effects on spatial coordination, being able to achieve a range of target spatial configurations of enrolled land (Parkhurst et al., 2002; Parkhurst and Shogren, 2007). However, a number of authors have found that, over time, subjects increasingly converge on the risk dominant outcome, implying a coordination failure in the provision of ecosystem services or biodiversity conservation (Banerjee, Kwasnica and Shortle, 2012; Banerjee et al., 2014, 2017). Banerjee, Kwasnica and Shortle (2012) find that the number of subjects in their local networks affects the performance of the AB, with a smaller number of players on the network achieving greater spatial coordination. Banerjee et al. (2014) show that higher rates of coordination on a Pareto-dominant pro-environmental land use equilibrium are obtained when more information is provided to each individual on the choices of other participants in their local network in previous periods. However, spatial coordination on this pro-environmental choice still declines over time, so that an equilibrium with *all* players choosing pro-environmental land use does not occur on any local network. Banerjee et al. (2017) find that the transactions costs of enrolling in the AB scheme help determine its success in achieving spatial coordination, and also show the importance of communication opportunities between players.

Most relevant to the current paper, Banerjee (2018) compares the effects of changing both the pecuniary incentives involved in participation (the size of the two payment rates for the AB – the participation payment and the spatial bonus) and a non-pecuniary incentive. This non-pecuniary incentive consists of information on the past behaviour of players outside an individual's local network, so that this information is of no strategic value to each participant. Since this information does not change the expected monetary payoffs, it can be considered an

[2] Other studies have made use of simulation models (e.g., Iftekhar and Tisdell, 2014) and field experiments with farmers (Liu et al., 2019).

information nudge. The author finds that changes in payment rates and the introduction of this nudge both improve spatial coordination. However, the experimental design used in Banerjee (2018) does not reward players for any non-pecuniary motives in their choice to participate, and assumes players directly benefit in monetary terms (as incorporated in their payoffs) from the group's coordination on the pro-environmental land-use equilibrium. We believe that players also have intrinsic motivations to contribute to an environmental public good, even in an experimental game setting. This also echoes observations that some farmers value their contribution to the environment and are prepared to forego profits for the sake of environmental protection, even if they do not draw any direct financial benefits from these pro-environmental actions (Gasson, 1973; Willock et al., 1999; Maybery et al., 2005). We therefore propose in the present paper a new experimental protocol that enables us to capture players' non-pecuniary motives to contribute to a public good. We then test a behavioural nudge, which establishes an explicit comparison across groups of players relative to their aggregate environmental contribution.

Indeed, aside from Banerjee (2018), all experimental tests of the AB to date have focussed solely on participants' relative monetary payoffs from choosing whether or not to participate in a PES scheme. However, a substantial literature to date, which we review in the next section, has shown that farmers base their decisions on whether to enrol in PES schemes on both pecuniary and non-pecuniary considerations (Howley and Ocean, 2020). If farmers do indeed act on their own preferences for environmental conservation, then the experimental setting in which AB-type schemes are evaluated should explicitly recognise this.

In this paper, we design a de-contextualised laboratory experiment whereby participants (mainly students) can be rewarded both according to their non-pecuniary preferences for the environment as well as through monetary payments for their choices to participate. Using a donation mechanism, we tie participation decisions to real donations to an environmental charity, thus rewarding non-pecuniary, pro-environmental motives for participating. We then evaluate how a non-pecuniary *mechanism* – a nudge – affects the performance of the AB. The nudge we test seeks to activate a social comparison incentive (initially described in the social psychology literature by Festinger, 1954) by providing information on how a player's group has performed in terms of coordination and environmental contribution, compared to other groups in the same experimental session. This is accompanied by an appreciative comment, which is expected to reinforce the positive value attached to being ranked first as a group.

The novelty of our study is thus to measure how well the AB works in achieving economically efficient spatial coordination on a local network when non-pecuniary motives are explicitly recognised and rewarded; and how well a nudge works in this setting, either on its own or in combination with an AB. Unlike Banerjee (2018), we find that our nudge does not improve the performance of the AB in this setting.

We test two research questions. The first is: does the AB achieve spatial coordination in land use choices when non-pecuniary motives are explicitly allowed for in an experimental laboratory setting? The second is: can a nudge based on group ranking improve the performance of the AB? In the next section we review evidence and argument on the determinants of participation in PES schemes. Based on the finding that non-pecuniary motives are one reason why some farmers participate, we then discuss how these kinds of motives can be "captured" in the laboratory.

2. Motivations for participation in PES schemes and how to represent these in the laboratory

Most economic research on PES schemes assumes that producers (e.g., land managers) compare the financial costs of participating in a scheme (e.g., the opportunity costs of reducing pesticide applications) with the financial payments offered by the buyer of the PES contract. However, beyond the comparison of individual financial payoffs, an emerging empirical literature suggests a broad set of motivations of land managers, with factors such as preferences for environmental protection, altruism, and moral considerations as being important (Michel-Guillou and Moser, 2006; Sheeder and Lynne, 2011; Sorice et al., 2011; Mzoughi, 2011; Banerjee and Shogren, 2012; Mills et al., 2017; Bottazzi et al., 2018; Rolfe et al., 2018; Streletskaya et al., 2020, Pannell and Claasen, 2020; see Dessart et al., 2019 for a review). For example, Sheeder and Lynne (2011) found that farmers' moral concerns about farming impacts on the environment were linked to participation in soil conservation measures; Vanslembrouck et al. (2002) found that awareness of the environmental benefits of agri-environment measures increased intention to participate for farmers in Belgium; Howley and Ocean (2020) found that giving farmers the opportunity to display their "green credentials" increased the likelihood that they would maintain conservation practices once their PES contract payments had ended; Palm-Forster et al. (2017) show that pro-environmental attitudes were important in explaining decisions to adopt sustainable arable farming techniques in the USA.

If personal concerns for environmental quality are important determinants in explaining pro-environmental behaviour such as participation in PES schemes, then the laboratory environment within which such schemes can be studied should reflect this. However, measuring the effects of such motives in laboratory experiments is not straightforward, since protocols are often de-contextualised and subjects are anonymous. Some researchers have captured pro-environmental motivations in the laboratory through donations to environmental charities. For example, Clot, Grolleau and Ibanez (2016) use an adapted dictator game to mimic pro-environmental behaviours, where subjects are asked to state how much of their endowment they are willing to give to an environmental charity. Others have used donation to charities to study the "warm glow" of giving proposed by Andreoni's (1990) model of impure altruism (Crumpler and Grossman, 2008). We make use of this idea by implementing an experimental design whereby subjects' choice of opting for a less profitable option can generate a real monetary donation paid by the experimenters to an environmental charity of the subject's individual choice. By making this contribution for real, we can capture the non-monetary motivations of the players when asked to contribute to a public good. We interpret the size of donations as equivalent to the change in the supply of an environmental public good from which individuals may derive utility, in addition to the monetary payoffs from their choices. As emphasized by Nyborg (2018), this contribution to utility can be interpreted as the "benefits from keeping a self-image as morally responsible" (p. 415).

As well as preferences for environmental quality, farmers may also have social preferences. That is, their utility may depend partly on the well-being of others, on what others think of their own behaviour, and how they compare their own performance with the performance of others along some relevant domain. Such social preferences have been documented in a number of theoretical (Le Coent, Préget and Thoyer, 2021) and empirical studies (Villamayor-Tomas et al., 2019; Lopes et al., 2020). The existence of such social preferences paves the way for the use of social comparison nudges[3]. Nudges are interventions that aim to change the behaviour of economic agents by changing the framing or informational context of economic decisions, without changing the expected financial payoffs from alternative actions and without regulating their actions (Croson and Treich, 2014). One attractive feature of nudges is that they are far less costly to implement than financial incentives. Even if their expected effect is generally small, they may be very cost-effective, especially if applied to a large population. As we noted earlier, Banerjee (2018) found that the introduction of a simple information nudge, where information on the past choices of people outside the individual's local network was provided to subjects, improved the performance of the AB.

[3] An alternative conceptual support for the use of social comparison nudges is the idea of identity-based utility (see, for example, Lequin et al., 2019).

In our experiment, we focus on nudges emphasizing group ranking. This provides additional information and constitutes a different framing from the experiment in Banerjee (2018). A number of empirical studies show that households (Alcott 2011; Ferraro and Price, 2013) and farmer groups (Chen et al., 2009; Kuhfuss et al., 2016; Chabe-Ferret et al., 2019) react to information on what their peers are doing in terms of their own environmental performance or effort. This farmer-specific evidence nests within an already abundant literature evaluating the impact of comparative performance nudges on individual behaviour in a wide range of circumstances. Group ranking has been employed in public good games to reduce free riding issues (Gunnthorsdottir and Rapoport, 2006; Tan and Bolle, 2007) and in coordination games as a way to address coordination failure (Bornstein, Gneezy and Nagel 2002; Riechmann and Weimann, 2008). We build on this literature to set up a nudge based on *group ranking* rather than solely on information on the actions of individuals belonging to the same group (Banerjee et al., 2014) or on information on other groups' performance (Banerjee, 2018). Since land managers are often part of a local group defined by their location (e.g., a watershed, a village community, a farming cooperative), we hypothesize that land managers may be sensitive to the way their own group performs in terms of environmental contributions relative to the performance of neighbouring groups (van Dijk et al., 2015). We emphasize here that this information does not change the financial pay-off of players, although it may change their utility, and therefore their choice.

Finally, we note that the subject pool for the experiment reported here is composed of students and members of the general public, not farmers. Cason and Wu (2019) review the circumstances under which student subject pools are more desirable to use than professionals. Most relevant to our paper is their argument that "*students are the most appropriate subjects for scientific research questions closely tied to economic theory*" (Cason and Wu, 2019, p. 743). Here the theory concerns the effectiveness of a monetary incentive for coordination when non-pecuniary preferences are potentially helping to determine the choice of strategy; and the interaction of this incentive with a nudge based on social comparisons, through the comparison of alternative treatments. We are not trying to predict how a particular population of farmers would actually respond to a specific type or level of monetary incentive, nor how this population would respond to a specific nudge in absolute terms. We are also not trying to estimate the non-pecuniary preference parameters of this population. Indeed, Peth and Musshoff (2020) show that results from experiments using German students and German farmers are comparable in terms of the relative performance of treatments, but not in absolute terms. We also recognise that it is challenging to capture social motivations or to generate a social norm in a de-contextualised laboratory experiment in which subjects are anonymous. We therefore place ourselves in a rather harsh environment to observe a potential effect of a comparative nudge. Nevertheless, we believe that our laboratory experiment is a good tool for testing our two research questions, because, by being de-contextualised, it provides a strong level of control and excellent internal validity (Thoyer and Préget, 2019).

3.Modelling Framework

Consider a finite set of land managers $i = 1, \dots, N$ who can manage their land in two alternative and mutually-exclusive ways, labelled X, Y. Land management option X refers to pro-environmental or conservation land management adopted under a PES scheme (for example, an agri-environmental measure in the Common Agricultural Policy), whilst Y indicates that the land is managed for conventional agricultural production. Following previous studies (e.g., Banerjee, Kwasnica and Shortle, 2012; Banerjee et al., 2014), we assume that land use option Y generates lower environmental benefits but greater agricultural revenue, *r*, compared to land use X, i.e., $r(X) < r(Y)$. To keep the payoff structure simple and transparent, we assume that

$r(X)$ includes the PES payment but remains lower than $r(Y)$, so that the payment does not fully compensate for all revenue losses associated with the adoption of conservation practices.

Land managers' participation in the PES scheme (i.e., the choice of X instead of Y) is expected to generate an environmental improvement, e, which is considered a public good (such as the value of higher groundwater quality, better flood protection, or enhanced biodiversity) from which both the land manager *and* wider society benefit. This point is crucial, and distinguishes our modelling and experimental approach from previous literature on the AB. The environmental benefit generated is conditioned on spatial coordination by neighbouring land managers in the adoption of pro-environmental land management, X. To emphasize the importance of spatial coordination, we assume that the environmental benefit is *only* produced if at least one of each land manager's direct neighbours also adopts X. Let n_i be the number of land manager i's direct neighbours who choose X. We thus assume that the aggregate environmental benefit generated by land manager i when choosing X is proportional to the number of direct neighbours also choosing X and equals en_i.

To facilitate an effective delivery of the environmental public good, and as long as the monetary value of the environmental benefit generated outweighs the loss in net revenues from choosing Y over X (i.e., $e > r(Y) - r(X) > 0$), it is the policymaker's objective to foster contiguous adoption of land use X in order to maximize social welfare. To this end, the policymaker can incentivize two neighbouring land managers to choose X with an agglomeration bonus. If n_i neighbours choose X, land manager i will receive a bonus bn_i when choosing X. The bonus is proportional to the environmental benefits generated through land management choices by neighbouring land managers.

If land managers only consider the monetary payment (agricultural revenue plus bonus payment) and do not take into account how their land use choice impacts on the supply of the environmental public good, the *monetary* payoff $p_i(\sigma_i)$ of land manager $i = 1, \dots, N$ adopting land use strategy $\sigma_i = X, Y$ reads as:

$$(1) \quad p_i(\sigma_i) = r(\sigma_i) + b(\sigma_i)n_i$$

where $b(X) = b \geq 0, b(Y) = 0$.

At this stage, and in the absence of an AB (b = 0), a single Nash equilibrium exists when all players choose *Y*, since $r(X) < r(Y)$. On the other hand, when a bonus is offered to induce land managers to adopt land use *X*, and assuming that the bonus is sufficiently large to ensure that the income difference between *X* and *Y* is covered,[4] this gives rise to a coordination problem with two Nash equilibria: one in which all land managers choose X (the Pareto dominant equilibrium) and one where all land managers choose Y (the risk dominant equilibrium).

However, as noted before, land managers may not only consider their financial gains following the payoff function specification in Eq. (1) but may also feel concerned about the impact of their management practices on the environment. In such a case, an increase in the quality of the environment –in the supply of the environmental good *e* – increases their utility. Consequently, we may observe situations where farmers choose pro-environmental land management practices (with or without coordinating with their neighbours), even though they do not derive direct financial gains from this, or even loose out.

One may also consider that land managers derive utility from choosing X independently of the choice of their neighbours, a "warm glow" effect (Andreoni, 1989, 1990), even though no significant environmental benefit might be generated. Indeed, what might be important to land managers is to do their best in choosing X for their self-esteem and/or to signal they are

[4] Formally, if we would assume that all land managers have *N*-1 neighbours (i.e., they are all spatially connected) then the condition on *b* would be: $b \geq \left(r(Y) - r(X)\right)/(N-1)$.

"responsible citizens," and perhaps to induce others to choose X as well. Taking into account these two non-pecuniary components, alongside the monetary income, Eq. (1) can be rewritten as an indirect utility function with monetary and non-monetary components, the former being weighted by $\delta_i > 0$ reflecting land manager i's marginal utility of income:

$$(2) \qquad U_i(\sigma_i) = \delta_i(r(\sigma_i) + b(\sigma_i)n_i) + a_i e(\sigma_i)n_i + w_i(\sigma_i)$$

where parameter a_i reflects land manager i's homegrown preference for the environment and w_i reflects the warm glow preference. The first two elements on the right-hand side of Eq. (2) are monetary-derived utility, whereas the third and fourth are non-monetary utility components. Besides, we assume that $e(X) = e > 0,\ e(Y) = 0, w_i(X) \geq 0$ and $w_i(Y) = 0$.[5]

As reflected in Eq. (2), we assume that the non-monetary utility component relating to environmental preferences of farmer i , $a_i e(\sigma_i)n_i$, is proportional to the environmental benefit they generate with their choice of X, which also depends on their neighbours' choices. This assumption is in line with the findings of an empirical study by Lawley and Yang (2015), who investigate the spatial interactions among neighbouring land managers in the context of conservation easements in Canada. In contrast, the warm glow effect for land manager i, $w_i(\sigma_i)$, depends only on their own choice of *X*.

Since a utility function such as (2) includes both environmental and warm glow preferences, then some land managers might prefer to choose X over Y even without the offer of a monetary bonus (b = 0). We therefore account for the empirical observation that conservation practices are sometimes adopted by land managers even in the absence of a payment mechanism to compensate for the loss of income (e.g., Michel-Guillou and Moser, 2006; Mills et al., 2017; Palm-Forster, Swinton and Shupp, 2017). This situation then

[5] Note that this utility function does not include the aggregate level of the environment or income; we are only interested in the difference in utility $U_i(Y) - U_i(X)$.

constitutes a coordination problem and, depending on the value of the behavioural parameters, multiple Nash equilibria can exist both with and without the bonus.

As stated in the introductory section, one objective of this article is to test the impact of a social comparison nudge on land managers' behaviour in the AB experiment. We speculate that such a nudge may induce more land managers to coordinate on the Pareto dominant Nash equilibrium, X, by impacting their utility without changing their monetary payoffs. The nudge consists of information on the ranking of performance of a specific land manager's group relative to the performance of other groups, assessed by the level of environmental benefits generated at the group level. This ranking is complemented by a "congratulation message" when the group is ranked first, which also provides information about injunctive aspects of group behaviour, i.e., the "*perception of what most people approve or disapprove of*" (Cialdini, Kallgren and Reno, 1991) to avoid a "boomerang" effect[6] (Alcott, 2011).

In our model, the *N* land managers are partitioned in *K* groups. Each land manager i, $i = 1, \dots, N$, belongs to a single group k, $k = 1, \dots, K$. We expect that belonging to a group *k* which is ranked higher in terms of environmental benefits generated will increase land managers' utility compared being in a lower-ranked group. This is because they would perceive themselves and the group as performing better in producing the environmental good, and value the social reward of this outcome being recognised through the announced ranking. This sensitivity to social reward is heterogeneous and captured in Eq. (3) by function f_i, which is increasing with the rank. Group *k*'s rank ($rank^k$) depends on the choice of player *i* belonging to group $k(\sigma_i^k)$, the choices of all other players of the same group $k(\sigma_{-i}^k)$, but also on the choice

[6] A boomerang effect can occur when the best performing individuals or groups react to a social comparison nudge by decreasing their environmental contribution to adjust to what the majority of others do. This effect would counter the intended effect of the nudge, that is to give all individuals a non-monetary incentive to improve their contribution to the environment (by choosing X).

of other groups' players (σ_j^{-k}). So, $rank^k = g\left(\sigma_i^k, \sigma_{-i}^k, \sigma_j^{-k}\right)$. A player may choose X to increase the probability of reaching a higher ranking depending on their individual social preferences. We consequently adjust the utility function (2) of an individual i belonging to group k as follows:

$$(3) \qquad U_i^k(\sigma_i^k) = \delta_i^k\left(r(\sigma_i^k) + b(\sigma_i^k)n_i^k\right) + a_i^k e(\sigma_i^k)n_i^k + w_i^k(\sigma_i^k) + f_i(rank^k)$$

Note that we do not impose $f_i(rank^k)$ to be positive.

We hypothesise that the choice of X by each individual increases the likelihood of creating more environmental benefits within a group, hence increasing the probability of achieving a higher rank in the inter-group competition. Eq. (3) shows that when land managers display pro-environmental and/or pro-social behaviour, then some might prefer to choose X over Y, either to satisfy their environmental preferences, or as a warm glow effect, or as a response to the social comparison, or any combination of the three motives. Whether the provision of such a nudge actually affects individual utility levels and influences choices is something that can be tested.

4. Experimental Design and Procedures

Modelling the spatial connectivity between land managers requires the imposition of a specific spatial structure on subjects. We follow the local network structure used by Banerjee, Kwasnica and Shortle (2012), Banerjee et al. (2014) and Banerjee et al. (2017) where subjects are arranged on a circle. The main advantage of utilising a circular network configuration is its symmetry, with each subject having the same number of *direct* neighbours, i.e., one on the left-hand side and one on the right-hand side. Given a circular network setting, the number of a subject's direct neighbours (n_i) choosing X can be 0, 1 or 2. Note, however, that a subject is *indirectly* linked to all other subjects on the network through their direct neighbours. Another advantage of employing a fixed and symmetric network structure is that all subjects in the experiment face the same degree of strategic uncertainty (Alós-Ferrer and Weidenholzer, 2008). Although a land manager may know what type of land management their *direct* neighbours' practice, they may not fully know the decisions of the *indirect* neighbours. That is, the social interactions among more distant neighbours on the network tend to be weaker (e.g., Lawley and Yang, 2015). Consequently, a symmetric network structure of a given size allows us to identify the impact of a nudge on spatial coordination and hence the environmental benefits without having to worry about confounding factors such as subjects being able to extract rents because of their specific position on the network as in Iftekhar and Tisdell (2017). Therefore, in this experiment, under each treatment, subjects are placed around a circular network in groups of 6 ($N = 6$). Each session included three groups of 6 subjects, where each subject is asked to choose between action X or action Y. Monetary payoffs depend for each participant only on the behaviour of the members of their group (not on members of other groups in the experiment).

To represent the environmental benefits of farming practices, subjects were told that the choices they would make during the experimental session could generate a donation to an environmental charity[7]. The environmental charities implement actions from which subjects can benefit, but which mainly generate a benefit to the wider society. In this setup, subjects who want to behave pro-environmentally can choose X at the cost of a lower individual monetary payoff, just like some land managers decide to participate in PES scheme due to non-pecuniary motivations. Subjects were not given the choice to retain the donation for themselves, since we want to mimic their contribution to a public good. We assume here that

[7] After reading the instructions and before the start of the experiment, subjects had to choose one charity to which their donation would be sent. The choice included one international charity (World Wildlife Fund), two French national charities (France Nature Environnement; Fondation Nicolas-Hulot pour la Nature et l'Homme), and one local charity (Ouvre-Tête).

the donation made to environmental charities will in practice contribute to environmental quality and will therefore increase the subject's utility if they care for the environment. The donation was placed in an envelope at the end of each session in the presence of the subject. The experimenters subsequently sent the total amount of donations to the corresponding charities and transferred to the subjects a confirmation of their donations by e-mail. Apart from using specifically designated environmental charities, the rest of the experiment was decontextualized.

The introductory section of this paper sets out two research questions:

(1) Does the AB achieve spatial coordination in land use choices when non-pecuniary motives are explicitly allowed for in lab experimental conditions?
(2) Can a nudge based on group ranking improve the performance of the AB in such a setting?

To investigate these questions, we created an experimental design with the following treatments: a control treatment without the AB and without the nudge (CT), a treatment with the AB alone (AB), and a treatment with the AB and the nudge (AB-N). We also consider an intermediate treatment where the nudge is introduced without the AB (N). This is shown in Table 1. In order to obtain a balanced number of independent observations across treatments, we had 6 groups participating in treatment CT and AB, and 6 sessions of 3 groups in treatment N and AB-N (see table 2). Indeed, in the control and AB treatments, an independent observation is obtained at the group level since individual decisions are dependent of those belonging to the same circular network in previous rounds, while in the treatments including the social comparison nudge (N and AB-N), an independent observation is obtained at the session level since the performance of other groups is communicated, introducing correlations between an individual's choice in *t* and choices of other participants to a same session in previous rounds.

A total of 288 students participated in the experiment: 16 sessions with 18 subjects each were run between April and September 2016 at the LEEM (Economic Experimental Laboratory of Montpellier) in France. Each session was composed of 15 periods during which subjects encountered the same choice problem under the same treatment within the same group and kept the same neighbours. After each period, each subject was informed of their own monetary payoffs, the donation generated given their choice, and the choices of their two direct neighbours. No communication was allowed within groups or between groups. At the end of a session, 4 periods were randomly selected, and subjects were paid their average payoff for these 4 periods. The actual donation made to the charity was the average donation generated in these 4 periods. The session ended with a short questionnaire to collect data on individual characteristics. The sessions lasted a maximum of 2 hours and the average payment received by subjects was €11.88, in addition to a participation fee of €2 for students and €6 for members of the general public. A total of €1,972 have been donated to the four environmental associations, in accordance with subjects' choices.

Balancing tests of individual characteristics between treatments are provided in Appendix 2. They show that the groups are balanced for most individual characteristics with the exception of age and the frequency at which participants usually make donations to environmental charities. Since these 2 factors may influence respondents' choice of *X*, which is associated with the potential donation, over *Y* in the experiment, we include these two variables as controls in our models (section 5.2). Results hold with and without these control variables. More information on the experimental design is provided next.

Control Treatment (CT)

In line with the modelling framework described in section 3, subjects can choose X and receive a revenue of $r(X) = 7$, or they can choose Y and receive a higher revenue equal to $r(Y) = 13$. When choosing X, a subject can generate a donation $d(X) = 8$ if one of their neighbours also chooses X, or $2d(X) = 16$ if both neighbours choose X. The value of the donation generated by choosing X, $d(X)$, is set to 8, so that coordination with a single neighbour is enough to achieve a Pareto improvement, i.e. the value of the environmental benefits outweighs the loss in agricultural revenues, $e(X) = d(X) \geq r(Y) - r(X)$. The parameter values are reported in Table 3 and payoffs for the control treatment are shown in Table 4.

As discussed in the previous section, in the control treatment, if subjects' utility functions do not include any pro-social or pro-environmental component ($a_i = w_i = 0$ for all i), then the unique Nash equilibrium is reached with all subjects choosing Y. However, given a choice of X, if the value that subjects gain from the non-pecuniary components $a_i e(X) n_i + w_i(X)$ as described in Eq. (2) is greater than the difference of revenue between Y and X, then multiple Nash equilibria can exist.

Agglomeration Bonus Treatment (AB)

In this treatment we implicitly introduce the AB by increasing the monetary payoff of choosing X when neighbours choose X as well. If only one neighbour chooses X, the subject receives $r(X) + b = 7 + 3 = 10$; when 2 neighbours choose X, the subject receives twice the bonus payment, $r(X) + 2b = 7 + 2(3) = 13$; if none of a subject's neighbours chooses X, then no bonus is received and the monetary payoff is only $r(X) = 7$, as in the control treatment. Importantly, as shown in Table 3, we adjust the rate of the AB so that the individual monetary payoff of strategy X, when the two neighbours also choose X, equals but does not exceed the payoff of strategy Y. Table 5 includes the payoffs for the AB treatment. By making both Nash equilibria monetary-payoff equivalent, we ensure that any difference observed in behaviours can be attributed to non-pecuniary motivations. Indeed, pecuniary motivations alone cannot be the driver of subjects' choice of X, as this choice will not lead to an increased monetary payoff, even when coordination with both neighbours is achieved.

In the absence of pro-social or pro-environmental preferences ($a_i = w_i = 0$), there are two Nash equilibria of equal individual payoffs: one where all subjects choose X and one where all subjects choose Y, the later being the risk dominant equilibrium. However, if at least one subject displays a strictly positive a_i or w_i, then multiple Nash equilibria exist: "all subjects choosing X" becomes the Pareto dominant equilibrium while "all subjects choosing Y" remains the risk dominant equilibrium.

Nudge Treatment (N)

This treatment is similar to the control treatment (CT) in terms of payoffs (see Table 4), but now subjects are "nudged" through an inter-group comparison. Before the start of the first period, subjects are told (as explained in the experiment instructions[8]) that after each period, each subject will be informed of the ranking of their group in terms of total donations generated for the environmental charities relative to the two other groups in the room. The group who generated the highest donation during a period received the following message at the end of that period: "Well done, your group is ranked first in terms of donations." This includes an injunctive norm (judgment of "well done") as well as a comparison to the other groups. The second (*third*) group received the message: "Your group is ranked second (*third*) in terms of donations." In case two groups generated the same level of donations during a period, then they were ranked according to the number of subjects who chose X, where the group with the highest number of subjects choosing X obtained the highest ranking accordingly.[9]

[8] The experiment instructions can be found in the Appendix.

[9] This is the case where a donation of 32 is generated by a group with four contributors (X-X-Y-X-X-Y) and another group with three neighbouring contributors (X-X-X-Y-Y-Y). When groups could not be discriminated based on their donations or the number of subjects choosing X, then they were considered equal and given the same ranking. In this case, they were ranked first if the third group was worse off, or third if the third group was better. When the three groups in a session were equal, they were arbitrarily all ranked first if they had all chosen X (to "reward" pro-environmental behaviour), but third if at least one subject in a group had chosen Y.

Nudge plus Agglomeration Bonus Treatment (AB-N)

In treatment AB-N, the payoffs are the same as in the AB treatment (see Table 5), but additionally subjects are nudged in the same way as in the nudge treatment (N). In this treatment, if subjects display environmental preferences and/or sensitivity to group ranking, multiple Nash equilibria exist with all subjects choosing X being the Pareto dominant equilibrium and all subjects choosing Y being the risk dominant equilibrium.

5. Results

To address whether the AB leads to higher participation and enhanced spatial coordination under our experimental protocol explicitly recognising and rewarding non-pecuniary motivations, we will compare the Control (CT) to the AB treatment (AB). To test whether a nudge would improve the performance of the AB, we will compare the Nudge (N) to the AB and Nudge (AB-N) treatment. Additionally, since we use a two-by-two between-subject design, we are also able to compare the performance of a nudge when applied alone to that of the agglomeration bonus applied alone (N versus AB). Below we first present the treatment comparisons in terms of participation, coordination and efficiency outcomes (Section 5.1), followed by an analysis of the individual strategies (Section 5.2).

5.1 Effect of Treatments on Participation, Spatial Coordination and Efficiency

Let us first look at the effect of the various treatments on participation, reflected by the number of subjects choosing X, and the extent of spatial coordination on X, reflected by the level of environmental benefits produced at the group level through the lens of the total amount of environmental donations (recall that such donations are only made when at least some positive level of spatial coordination over choosing X occurs).

Interpretation of behaviours under the control treatment CT

As predicted, the control treatment CT displays the lowest levels of participation (choice of X), which ranges from 15 to 40 percent (Figure 1). Since choosing X leads to lower individual payoffs, this result clearly indicates that a share of subjects do gain utility from the potential donation to an environmental charity (high a_i) and/or the warm glow feelings associated with choosing to play X themselves (high w_i). However, the proportion of subjects choosing X decreases over time, with groups tending towards the risk dominant equilibrium. This picture also emerges when we consider spatial coordination (Figure 2), as the level of coordination quickly declines toward zero in the control groups (treatment CT). This indicates that the choice of X is mostly led by environmental preferences (reflected by a_i), which relies on coordination, rather than by warm glow effects (reflected by w_i). A subject displaying a strictly positive w_i derives additional utility from choosing X no matter what the environmental outcome might be. Therefore, this warm glow should not fade away over time as it is not affected by one's neighbours' choices. In contrast, the environmental gain component of utility depends on the choices made by a subject's neighbours. If neighbours repeatedly choose Y, then the environmental benefit is not created and choosing strategy X becomes less attractive for those who care about the environmental good. However, we cannot discard the hypothesis that other behavioural factors may also explain the choice of X, as, for example, the cultural background of participants or the Hawthorne effect (or observer effect; McCambridge, Witton and Elbourne, 2014) that may lead subjects to choose more the pro-environmental choice X than they would do without the awareness of being observed. Nevertheless, the prevalence of this behavioural bias is, a priori, equal in all four treatments and should not affect insights into our research questions which rely on the comparison of behaviours under alternative treatments.

Effect of the AB when accounting for non-pecuniary motivations

Comparing the control treatment (CT) and the AB treatment (AB) reveals that the AB increases participation (Figure 1) and enhances coordination (Figure 2 presents the average of individual donations, which is an indicator for spatial agglomeration). This effect is statistically significant (see Table 6). This result accords with findings from previous experimental articles (Parkhurst et al., 2002; Banerjee et al., 2014). Additionally, our results show a robust effect of the AB under less favourable payoffs settings. Indeed, in previous studies the expected individual monetary payoff of choosing *X* and reaching the Pareto efficient Nash equilibrium is higher than choosing the risk dominant strategy, *Y*. However, in our protocol the level of the bonus payment is such that the individual payoffs under the risk dominant Nash equilibrium (all-*Y*) and the Pareto efficient Nash equilibrium (all-*X*) are *equal*, making strategy *X* even less attractive when looking at monetary payoffs only. The preference for strategy *X* under this payoff structure can be explained by the donation that is generated by a coordinated choice of *X* and subjects' environmental preferences for this donation, as no increase in individual financial payoffs can be expected from choosing *X* over *Y*. The AB and donations induce subjects to coordinate on the Pareto optimal Nash equilibrium instead of selecting the risk dominant Nash equilibrium by triggering subjects' extrinsic and intrinsic motivations to choose *X* (through the AB and through the use of a real donation to charities, respectively).

Introduction of the nudge and its effect on the AB performance

The group ranking nudge seems to slightly improve the situation for both participation and spatial coordination when introduced without an AB (N versus CT). However, Mann-Whitney tests, comparing the average proportion of subjects choosing *X* and the average donation generated by subjects in both treatments, show that the differences between treatments N and CT are not significantly different from zero (see Table 6). This suggests that the ranking component of the hypothesized subject's utility function has little average effect on choices.

Despite this non-significant average effect, however, we see that the nudge has a significant but small impact on participation and coordination for some of the periods, including the first. This indicates that the prospect of being ranked does have an effect, even when the outcome of the ranking is still unknown. This is encouraging, as it means that some improvement in coordination could be obtained at low cost by simply announcing (credibly) that groups will be ranked, and by signalling relative group performance.

When comparing treatments N and AB, we find a statistically significant enhanced performance of the AB over the nudge, which is not surprising given previous results. What is more interesting is our research question 2 and the comparison of treatments AB versus AB-N. We aimed at testing whether a nudge would "supercharge" the positive effect of the AB on both participation and spatial coordination by providing groups with feedback on their relative performance in terms of donations (and thus of environmental outputs). However, the results suggest that the nudge combined with the AB has a slightly negative effect (see Figures 1 and 2), although it is not statistically significant (see Table 6). The reason why the nudge fails to improve the AB performance is investigated in more detail in the analysis of individual choices (5.2).

Efficiency Analysis

We define a variable representing a group's net benefits, denoted $groupB$, as the total benefits produced at the group level (comprising both individual payoffs and donations) net of the budgetary costs linked to AB payments. From a policymaker's perspective this variable embodies the total benefits produced (agricultural production value plus environmental benefits) from which we deduct the public spending (AB payments). This can be used as a proxy for net social welfare produced at the group level. Formally this reads: $groupB = \sum_i (r_i + n_i(d - b))$ with i members of the group.

We can analyse the efficiency of a treatment as its capacity to induce spatial coordination and to generate the greatest net benefit at the group level. Under all treatments, the maximum net benefit is obtained when all subjects coordinate on X without any public subsidy (agglomeration bonuses). Numerically this amounts to $groupB_{max} = 6 \times (13 + 16 - 6) = 138$. Conversely, the minimum net benefit that can be produced is reached when there is no spatial coordination, implying no neighbours coordinate on choosing the same land use strategy.[10] In this case the group net benefit is: $groupB_{min} = (3 \times 7) + (3 \times 13) = 60$. We can subsequently define the standardized average efficiency of a treatment as follows:

$$(4) \quad efficiency = \frac{1}{3}\sum_{k=1}^{3}\frac{groupB^{k} - groupBmin}{groupBmax - groupBmin} \in [0,1].$$

A treatment is fully efficient if *efficiency* = 1, meaning that the groups under this treatment generated the maximum net benefit. It is fully inefficient if *efficiency* = 0, meaning that all the groups in this treatment generated the minimum net benefit possible.

A comparison of the treatments' relative efficiency brings an additional perspective to the results. Figure 3 shows that the AB increases the efficiency score from 0.19 in the absence of incentives (CT) to 0.71 (AB). We also observe that the AB yields greater efficiency than the nudge only. Group efficiency is also significantly improved (see last row in Table 6 by the introduction of a nudge only (N) compared to the control treatment (CT). This is due to the fact that the nudge bears no budgetary costs (under the assumption that providing information feedback to groups is costless). The efficiency comparison between treatments AB and AB-N displays no significant difference.

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[10] On a circular network comprising 6 subjects, this case represents an alternating pattern of choices at the group level (i.e., *X-Y-X-Y-X-Y*).

5.2 Analysis of Individual Strategies

In order to analyse how treatments impact individual player decisions, and to verify that the results are robust when controlling for imbalanced characteristics between treatment groups, we follow Banerjee et al. (2012, 2014) by using random effects probit regressions. We estimate the treatment effect ΔT in:

$$(5) \qquad \sigma_{it}^{*} = \alpha + \Delta T + \theta n_{it-1} + \gamma t + u_i + \varepsilon_{it}$$

where $\sigma_{it} = X$ if $\sigma_{it}^{*} > 0$, and $\sigma_{it} = Y$ if $\sigma_{it}^{*} \leq 0$. The probability that subject *i* chooses action X in period *t* $(t > 1)$ depends on treatment T, period t, and neighbours' choices in the previous period, n_{it-1}[11]. Further, α is a constant, γ is a parameter to be estimated, u_i are individual random effects and ε_{it} is the error term.

The analysis at the individual subject level confirms the results based on the descriptive statistics of outcomes (Table 7), when controlling for age and frequency of donations to environmental charities in participants' daily life. The AB (Treatments AB and AB-N) significantly increases subjects' probability to choose strategy X, whereas the nudge alone (N) has no significant effect. Subjects' choices of X are also significantly and positively related to their direct neighbours' choice of X in the previous period, this last result holds for all treatments. With successive rounds, the tendency to choose X declines over time, as shown by the negative value of coefficient *t,* mirroring the result from Banerjee et al. (2014).

Using a random effects probit model, we analyse the four treatments in a pooled model. Table 8 shows that in treatments AB and AB-N the influence of neighbours' choices is significantly higher than in treatments CT and N. This confirms our interpretation that the AB induces strategic behaviour (playing X and signalling to neighbours that playing X is a winning strategy), thus strengthening the positive effect of environmental preferences. Indeed, while

[11] The model is implemented on the data from period 2 to 15 as it incorporates neighbours' choice in the previous period.

the neighbours' influence in treatments CT and N only relies on a small number of subjects who exhibit sufficiently strong environmental preferences (high value of a_i) to compensate for the loss of revenue, the AB induces additional individuals with relatively lower values of a_i to choose X. Note that the choice of X in this situation remains motivated by the perspective of a donation, since the individual monetary payoffs related to choosing X do not exceed the monetary payoff under the risk dominant strategy, Y, even when receiving the AB.

To gain more insight into the functioning of the nudge, and to better understand the results obtained in treatment AB-N, we analyse the effect of subjects' group ranking in the previous period ($t-1$) on their choice of X in period t for the two treatments that include the nudge (N and AB-N). Results of these random effect probit models are presented in Table 9. We find that in most cases the group ranking announced in period $t-1$ has a significant influence on subjects' choices in period t. Interestingly, the effect differs depending on the treatment. When the nudge is used on its own (treatment N), being ranked third rather than first significantly increases a subject's probability of choosing X in period t (significant at 10% when ranked second). When the nudge is used in combination with the AB (treatment AB-N), then being ranked second rather than first in period $t-1$ has a negative effect on subjects' probability to choose X in period t (significant at 10%), whilst the effect of being ranked third is not significant.

These individual strategies can be interpreted as responses to social norms[12] that are being transmitted through the group ranking information. The analysis of treatment N shows

[12] Social norms can be defined as "shared understandings of how individual members should behave in a community" (Chen et al., 2009, p.11812). These encompass both what an individual understands the actions of others in some relevant peer group to be, and what they believe is expected of them by members of this group (Abbott, Nandeibam and O'Shea, 2013). If individuals derive disutility from diverging from a social norm, then providing information of this kind can be expected to change behaviour if the weight the individual places on the

that the expected positive impact of social comparison exists mostly when the group is lagging behind in terms of coordinating on X. In other words, being ranked first and being congratulated for being first has less effect on subjects' participation decisions than being ranked second or third. Despite this differential effect of ranking detected in the Nudge treatment, the overall effect is not enough to generate a significant influence of the nudge on participation or coordination (Table 7). The few participants willing to give up some of their individual payoff to generate a donation seem to be sensitive to the ranking included in the nudge but they remain a minority of participants. The positive impact of neighbours' choice of X can be explained likewise: it reinforces the social norm (playing like others) but it also increases the likelihood of generating the environmental good via the donation, something that subjects motivated by environmental outcomes are sensitive to.

In contrast, when the nudge is used in conjunction with the AB (treatment AB-N), the information of being ranked second has a negative effect on participation. This might be because the nudge also carries information on the probability of coordination failure. Indeed, the CT shows that only a minority of participants are willing to give up some of their payoff to generate the donation, therefore the majority of participants who choose X under the AB and AB-N treatments are those who are willing to take the risk of trying to coordinate with their neighbours to generate the donation, on the condition that their individual payoff remains the same as when choosing Y. When combined with an AB, the comparative nudge thus has a counteractive effect: subjects use the information conveyed by the nudge as strategic information on their chances of being paid the bonus, and therefore of being able to maintain the same individual payoff. A low ranking shows that coordination is not achieved in some parts of the network. Indeed, the ranking conveys information which is not provided in the

opinions of others or their own selfish concern for social ranking is strong enough (Czajkowski, Hanley and Nyborg, 2015).

control (CT) and the AB treatments (AB): although each subject is always informed of his neighbours' choices, they do not know the choices of their indirect neighbours. The ranking provides indications on the behaviour of indirect neighbours and can be used by the subject to try to anticipate his neighbours' choices in the next round.

6. Discussion

Recent developments in the literature on PES show that (i) private land managers can be encouraged to spatially coordinate their actions using an agglomeration bonus (AB) which rewards participation with neighbours; (ii) farmers' decisions to join a PES scheme rely on both pecuniary and non-pecuniary motives; and (iii) behavioural interventions such as nudges can improve participation in PES schemes. This article brings these ideas together to address two issues. First, how can we design an experimental protocol that accounts for subjects' non-pecuniary motivations for participating in a public good donation scheme in a laboratory setting with students and members of the general public, and what effects does the AB have when it is introduced? Second, can an inter-group comparison nudge be used to improve the performance of the AB, or indeed substitute for it, when such non-pecuniary motives are explicitly allowed for?

Regarding the first point, we have used donations to environmental charities by participants in the experiment (or rather, the utility that participants get from these donations) to represent a wide range of potential non-pecuniary motives which farmers might have for participating in PES schemes, such as pride in being a good steward of their land for future generations, or a sense of duty to conserve biodiversity on their land, or a selfish pleasure from a higher local level of environmental quality (such as better water quality, or higher quality wildlife habitats). Our results show that the AB can be expected to significantly increase the level of participation and spatial coordination. These results echo previous findings in the literature. An important result of this paper is that these results hold even where subjects do not benefit themselves directly from their contribution to the environmental public good.

The second conclusion relates to the inter-group comparison nudge. Announcing rankings based on relative group performance in terms of environmental benefits generated is not enough to improve spatial coordination on its own. This is at odds with results from Banerjee (2018), who finds that providing information on another group's choices and AB earnings increased the proportion of subjects choosing the Pareto dominant strategy (pro-environmental land use), and that coordination was sustained over time; as well as some of the theoretical social norm literature (e.g., Rege, 2004). However, this is in line with the results obtained by Bornstein, Gneezy and Nagel (2002) who show that a ranking increases coordination only if it has payoff consequences. Although we do obtain some efficiency gains with the nudge alone due to the fact that it bears no budgetary costs, these gains remain relatively small[13] and we fail to significantly improve the performance of the AB by adding the nudge. Indeed, the strategic information included in the information to participants on the ranking of their group appears to be stronger than the positive effects of the social norm on the desired outcome. Note that, the power of the kind of nudge used here may be smaller in the lab than in real life, where actual social groups are embedded in common institutions shared life histories, especially since the "game setting" of our experiment makes more salient the strategic component of subjects' choices, rather than the environmental and social implications of their choices.

At this stage, we can only suggest explanations which will need to be investigated further with a different protocol. Ranking groups while also offering an AB seems to indirectly provide

[13] The assumption of low costs could be challenged in real world settings since announcing the relative success of various groups of farmers would induce administrative and communication costs (see, for instance, Banerjee et al. (2017) for an experimental study on the role of transaction costs in an AB setting).

information on what other members of a subject's group choose, leading a subject to adjust their behaviour towards choosing the risk dominant equilibrium when informed that their group is not performing well in terms of coordination. The fact that providing ranking information has a significant effect only on least-performing groups in the nudge only treatment (N) is suggestive of variation in how the nudge is interpreted by players. However, we are not able to dis-entangle the multiple possible effects of the nudge: all that can be observed is its overall impact.

7. Conclusions

The main contribution of this paper to the PES literature is that we explicitly incorporate non-pecuniary motives in testing the performance of incentives designed to improve spatial coordination of pro-environmental actions. Such spatial coordination has been argued by many ecologists to be important for the achievement of environmental targets. However, since participation is voluntary, only an unlikely spatial pattern of covariance between ecological benefits and economic opportunity costs will achieve this coordination, unless additional incentives are put in place which reward coordination. Such incentives are typically thought of as being monetary – thus the "standard" agglomeration bonus pays extra when neighbours participate with neighbours. Yet if farmers are also motivated by a concern for the environment as a public good, the ecological benefits of spatial coordination could in themselves generate a utility payoff which is non-pecuniary in nature. This is the type of reward that our experiment incorporates through the use of donations that are made to environmental charities if subjects spatially coordinate. Our results show that the AB is still effective in such an environment, but that a behavioural nudge based on group comparison of participants does not seem be effective, either on its own, or in conjunction with the pecuniary rewards offered by the agglomeration bonus. Moreover, we find suggestive evidence that the effect of social norm information on one’s group coordination success relative to other groups depends on whether or not a monetary incentive is also in place to encourage coordination.

Appendix 1: Experiment Instructions (Treatment AB-N)

(Translated from French)

Note: the parts between brackets [...] are adjusted according to the treatment. The remainder of the instructions is the same for all treatments.

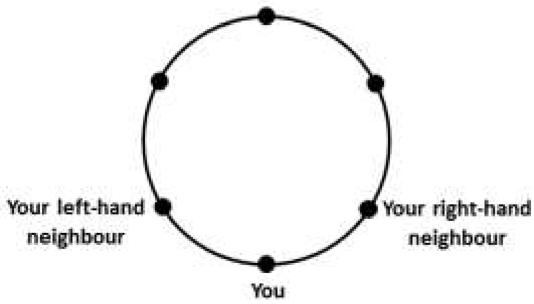
The objective of this experiment is to study decision making. Please read carefully the instructions, they will help you understand the experiment. When all participants have read the instructions, one of the experimenters will read them aloud. Your decisions will be treated anonymously. Your will enter your choices in the computer you are now facing. You will be paid according to the decisions you and the other participants make during the experiment. You will be paid in cash at the end of the experiment. Your choices might also generate donations to environmental charities. The amount donated will depend on your choices but also on the other participants' choices.

From now on, please remain silent. If you have a question, raise your hand and an experimenter will come to respond to your question in private.

General principle of the experiment

At the start of the experiment, the central computer will randomly create three groups of 6.

All participants in your group, including yourself, are virtually arranged on a circle (see figure below):



You therefore have two neighbours: a right-hand and a left-hand neighbour. Your group members, as well as your two neighbours, will remain the same until the end of the experiment. However, you will not be able to identify the other members of your group, and they will not be able to identify you.

This experiment will have 15 periods. In each period you will have to choose between two options: X or Y. Your payoff for a period depends on your choice and that of your two neighbours.

Your choice can also generate, additionally to your private payoff, an environmental benefit, represented here by a donation to an environmental charity. This donation is only generated if **you and at least one of your neighbours** choose X. If you and **both** your neighbours choose X, the donation is doubled.

More precisely, by choosing X:

- If both your neighbours choose X, you earn [13€] and generate a 16€ donation,
- If one neighbour chooses X and the other Y, you earn [10€] and generate a 8€ donation,
- If both your neighbours choose Y, you earn 7€ and generate no donation.

By choosing Y, you earn 13€ but will not generate any donation.

The table below displays your payoff and the donation potentially generated resulting from your choice, presented in rows (X or Y), and from the choices of your two neighbours, presented in columns (XX, XY or YY).

		Your neighbours' choices		
		Both choose X	One chooses X, the other Y	Both choose Y
Your choice	X	Your payoff: [13 €] Donation generated: 16 €	Your payoff: [10 €] Donation generated: 8 €	Your payoff: 7 €
	Y	Your payoff: 13 €	Your payoff: 13 €	Your payoff: 13 €

This table is the same for all participants and will remain the same throughout the experiment.

<u>The donations</u>

The total amount donated will be calculated at the end of the experiment according to your choices and these of the other participants. The experimenters commit to transfer all donations generated during this experiment to the charities of participants' choice.

Before the first period, you will be asked to choose one of the following environmental charities. We will send the donations generated by your choices to the charity you select. You will only be asked to do this choice once for the all experiment.

- **WWF** (leading world organisation for nature protection),
- **Fondation Nicolas Hulot pour la Nature et l'Homme** (French, apolitical charity for planet Earth protection),
- **France Nature Environnement** (federation of French charities for the protection of nature and the environment),
- **Ouvre-Tête** « Social Alternative and Ecological Solidarity » (charity created in 2006 by students from the University of Montpellier).

If necessary, you will find further information on these charities at the end of these instructions.

<u>Practically</u>

In each period, options X and Y will l appear at the bottom of your screen and you will have to click on your choice. You can change your choice as often as you want. When you click on "confirm", your choice will be definitive, and you will not be able to change it any more for the current period.

At the end of the period, when all participants have confirmed their choices between X or Y, you will be informed, for the current period, of:

- Your left-hand neighbour's choice,
- Your right-hand neighbour's choice,
- Your payoff,
- The donation generated by your choice, if any.

[*For treatments N and AB-N:*

Additionally to this individual information, each group will be informed of its ranking relatively to the two other groups in the room. This ranking depends on the total amount of donations generated at the group level. The first group (rank 1) is the group that generated the most donations and the last group (rank 3) the group that generated the least donations during the period. If too groups are equal, they will be ranked according to the number of participants who chose X. Therefore, for an equal level of donations, the group with the most members choosing X will be ranked before the other one.]

A table will display a summary of this information for all periods since the beginning of the experiment.

Payment of payoffs and donations

At the end of the 15 periods, 4 periods will be randomly selected and will determine the value of your payoff and of the donation generated by your choices. This selection is random, so please do pay close attention all along the experiment. You will be paid the average of your payoffs for these 4 periods. Similarly, the donation that will be sent to your selected charity will be the average of the donations generated during these 4 periods. You will only be paid in cash, and privately, your individual payoff. The donation will be placed in the envelope corresponding to your selected charity under your supervision. A proof of the actual donations to the charity will be circulated by email after the experiment.

Before we start, please answer to a short questionnaire to check your understanding of the instructions. Your responses to this questionnaire will have no consequences on your final payoff or on the donations.

If you have a question, please raise your hand.

Appendix 2: Balancing tests and participants characteristics in each treatment group

	T = CT	T = AB		T = N		T = AB-N	
	n = 36	*n = 36*		*n = 108*		*n = 108*	
	Mean (Std. Dev.)	**Mean (Std. Dev.)**	**p-value (t-test[1])**	**Mean (Std. Dev.)**	**p-value (t-test[1])**	**Mean (Std. Dev.)**	**p-value (t-test[1])**
Year of birth	1991.72 (6.505)	1988.47 (9.834)	0.103	1988.3 (9.338)	0.043	1989.91 (7.976)	0.219
	Percent.	**Percent.**	**p-value (Chi2 test)**	**Percent.**	**p-value (Chi2 test)**	**Percent.**	**p-value (Chi2 test)**
Gender: female	50.00%	66.67%	0.151	0.57%	0.439	49.07%	0.923
Participants							
Students	80.56%	69.44%	0.276	64.81%	0.078	73.15%	0.374
General public	19.44%	30.56%		35.19%		26.85%	
Previous participation to experiments	88.89%	91.67%	0.691	87.04%	0.771	90.74%	0.745
Frequency of donations to environmental charities							
Never	75.00%	41.67%	0.004	59.26%	0.092	53.70%	0.034
Less than once a year	25.00%	41.67%		31.48%		36.11%	
More than once a year	0.00%	16.67%		9.26%		10.19%	
Frequency of donations to other charities							
Never	50.00%	38.89%	0.472	34.26%	0.198	37.04%	0.377
Less than once a year	33.33%	33.33%		37.96%		39.81%	
More than once a year	16.67%	27.78%		27.78%		23.15%	
Volunteers for charity							
Never	36.11%	41.67%	0.271	34.26%	0.555	31.48%	0.175
Less than once a year	36.11%	19.44%		28.70%		24.07%	
More than once a year	27.78%	38.89%		37.04%		44.44%	

[1] *two-sided t-test, H0: difference = 0*

The tests test for differences between each treatment (AB, N, AB-N) and the control treatment (CT)

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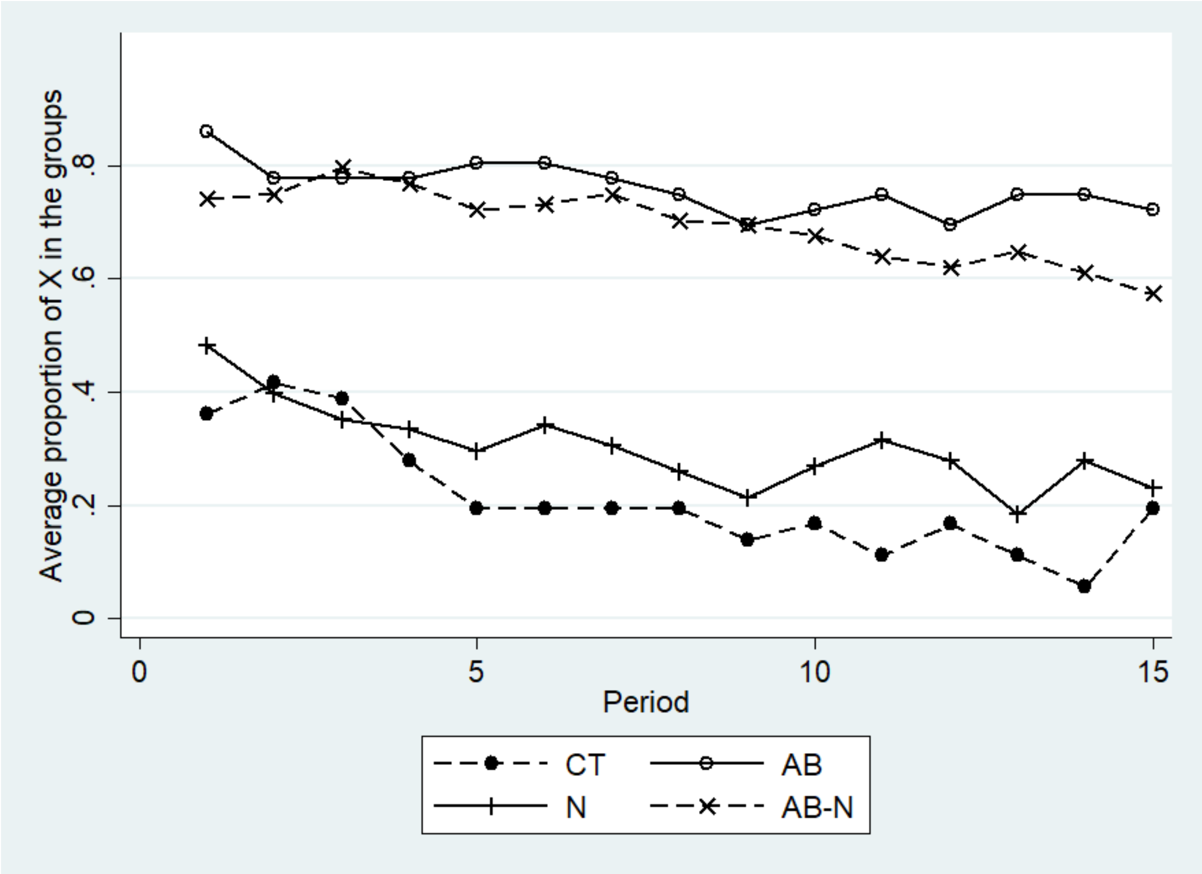

Figure 1: Average proportion of subjects choosing *X* by period and treatment

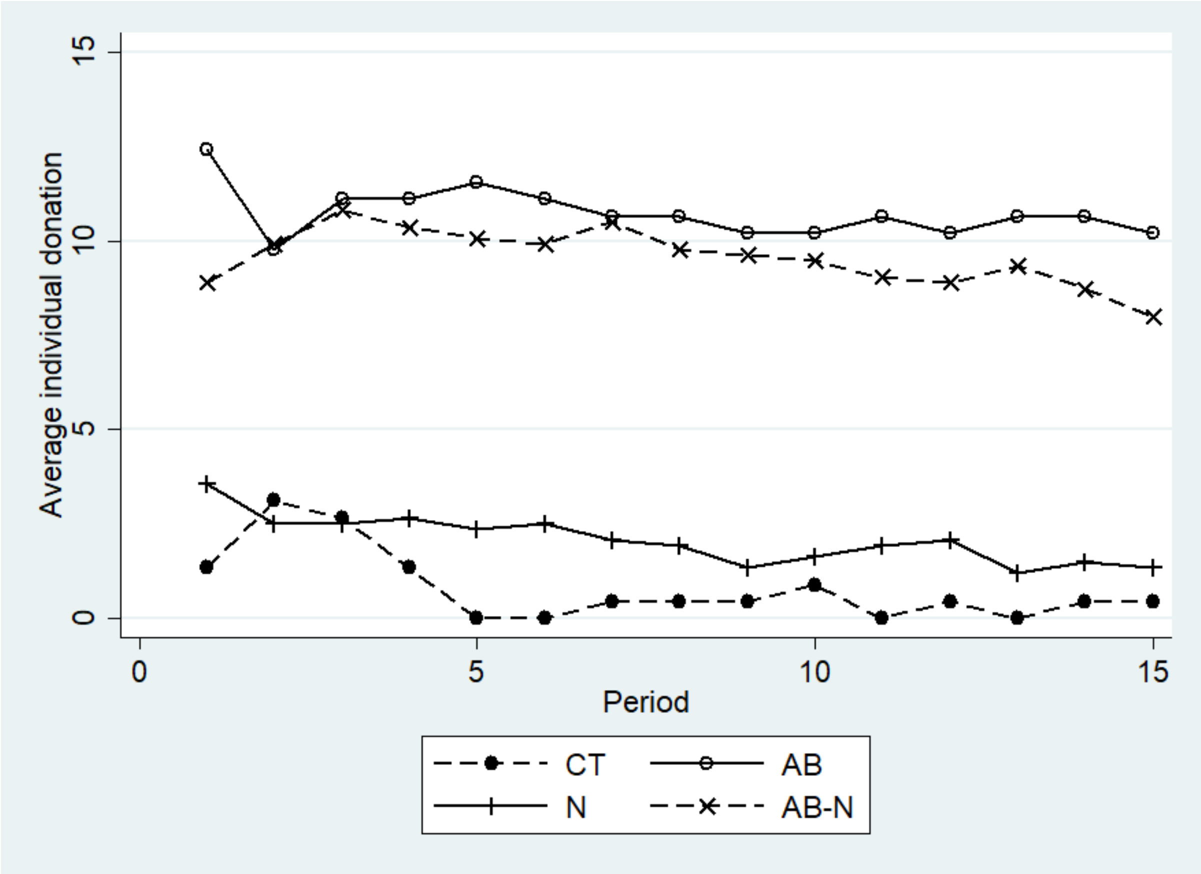

Figure 2: Average individual donation by period and treatment

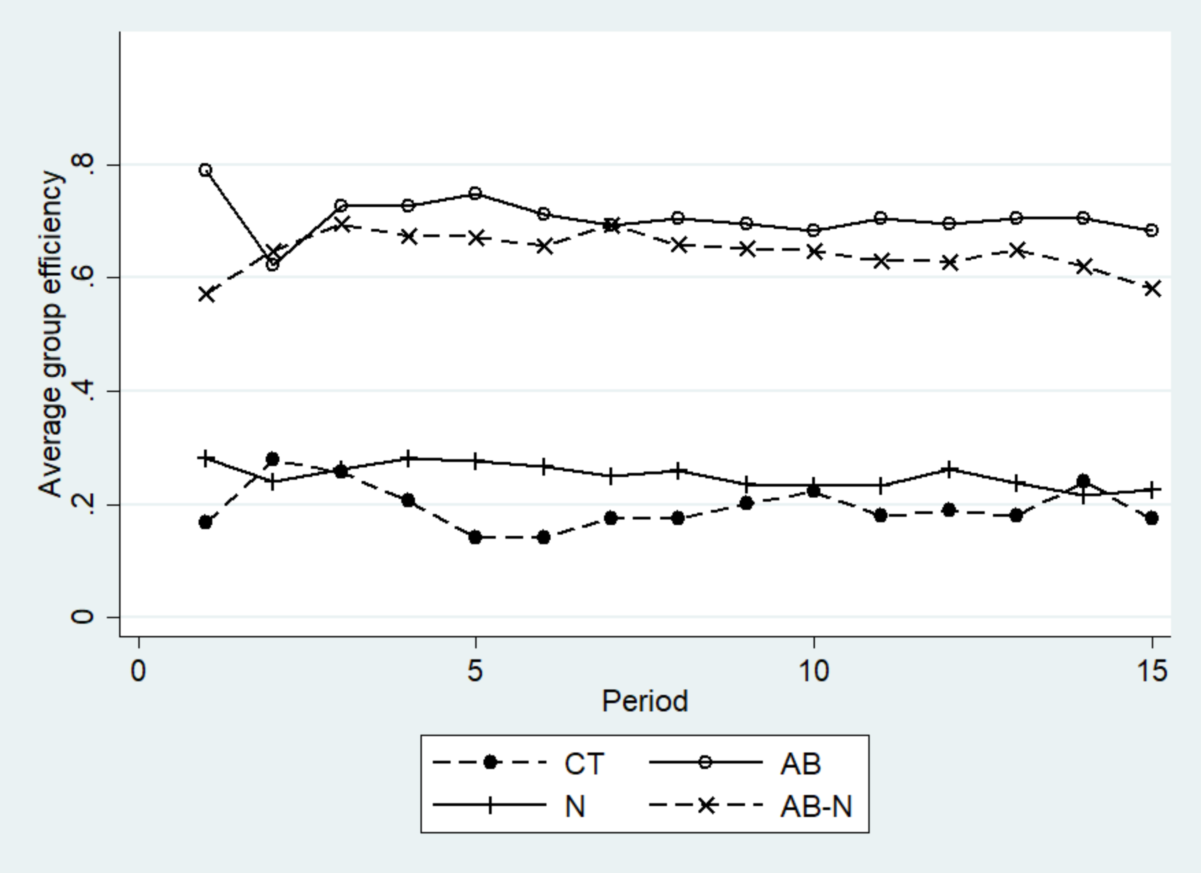

Figure 3: Average efficiency by period and treatment

Table 1: Treatments

		Nudge	
		NO	YES
Agglomeration bonus	NO	CT	N
	YES	AB	AB-N

Table 2: Number of Sessions, Groups and Subjects per Treatment

Treatment	Number of participants
CT	2 sessions, 3 groups of 6 subjects each = 36 participants
AB	2 sessions, 3 groups of 6 subjects each = 36 participants
N	6 sessions, 3 groups of 6 subjects each = 108 participants
AB-N	6 sessions, 3 groups of 6 subjects each = 108 participants
Total	16 sessions, 288 participants, **6 independent observations per treatment**

Table 3: Parameter Values

Parameters	X	Y
Revenue (r)	€7	€13
Agglomeration bonus (b)	€3	€0
Donation (d)	€8	€0

Table 4: Payoffs Control Treatment (CT) and Nudge-only Treatment (N)

		Your Direct Neighbours' Choices		
		Both choose X	One choose X, the other chooses Y	Both choose Y
Your choice	X	Your payoff: €7 Donation generated: €16	Your payoff: €7 Donation generated: €8	Your payoff: €7
	Y	Your payoff: €13	Your payoff: €13	Your payoff: €13

Table 5: Payoffs AB Treatment (AB) and AB plus Nudge Treatment (AB-N)

		Your Direct Neighbours' Choices		
		Both choose X	One chooses X, the other chooses Y	Both choose Y
Your choice	X	Your payoff: €13 Donation generated: €16	Your payoff: €10 Donation generated: €8	Your payoff: €7
	Y	Your payoff: €13	Your payoff: €13	Your payoff: €13

Table 6: Results - Treatment Effects

	Mean Value (*Standard Deviation*)				Mann-Whitney test results: Prob > |z|			
Variable	CT Control	AB Agg. Bon.	N Nudge	AB-N AB + Nudge	CT vs AB	CT vs N	AB vs N	AB vs AB-N
Number of independent observations	6	6	6	6				
Share of *X* choices	0.21	0.76	0.30	0.70	0.006***	0.262	0.016**	0.423
	(0.10)	*(0.29)*	*(0.14)*	*(0.19)*				
Donation (€/subject)	0.80	10.76	2.07	9.56	0.007***	0.150	0.016**	0.631
	(0.89)	*(5.83)*	*(1.57)*	*(4.07)*				
Efficiency	0.19	0.71	0.25	0.65	0.007***	0.078*	0.016**	0.631

Note: *** H0 rejected with 99% confidence level, ** H0 rejected with 95% confidence level, * H0 rejected with 90% confidence level, H0 is the hypothesis that both mean values are equal.

Table 7: Effect of Treatments on Individual Choices of *X* (Random Effects Probit Model)

Variable	Treatment AB	Treatment N	Treatment AB-N
T (ref CT)	2.328***	0.227	1.894***
n_{it-1}	0.485***	0.318***	0.970***
t	-0.064***	-0.053***	-0.070***
Birth year	-0.0001	-0.009	-0.021
Freq. env. don.	0.921**	0.142	0.077
_cons	-2.100	16.427	39.391
Lnsig2u _cons	1.046**	0.668***	1.174***
Statistics			
N	*1008*	*2016*	*2016*
Sample	*72*	*144*	*144*
ll	*-321.37*	*-831.00*	*-584.16*
aic	*656.74*	*1676.01*	*1182.32*

Note: Dependent variable Y = 1 if participant chooses X, 0 otherwise; *p<0.1; **p<.05; ***p<.01; Standard errors clustered by independent observation. *Lnsig2u* is the panel level variance component.

Table 8: Influence of Neighbours' Choices by Treatment

Variable	Coefficient
AB (ref CT)	1.734***
N (ref CT)	0.299
AB-N (ref CT)	0.593
n_{it-1}	0.387***
n_{it-1} * AB	0.624**
n_{it-1} * N	-0.110
n_{it-1}* AB-N	1.190***
t	-0.054***
Birth year	-0.009
Freq. env. don.	0.164
_cons	-16.041
Lnsig2u _cons	1.023
Statistics	
N	*4032*
Sample	*288*
LL	*-1287.18*

Note: Dependent variable Y = 1 if participant chooses X, 0 otherwise; *p<0.1; **p<.05; *** p<.01; Standard errors clustered by independent observation. *Lnsig2u* is the panel level variance component.

Table 9: Ranking Effect on Choice of *X* in treatments N and AB-N (All Periods Included)

Variable	Treatment N	Treatment AB-N
Ranked Second $(t-1)$ (ref: ranked first)	0.219*	-1.004*
Ranked Third $(t-1)$ (ref: ranked first)	0.281**	-0.823
n_{it-1}	0.359**	1.330***
t	-0.045***	-0.080***
_cons	-1.073***	0.598
Statistics		
N	*1512*	*1512*
Sample size	*108*	*108*
LL	*-612.74*	*-341.40*
AIC	*1237.48*	*694.79*

Note: Dependent variable Y = 1 if participant chooses X, 0 otherwise; *p<0.1; **p<.05; ***p<.01; Standard errors clustered by independent observation.