Environmental and Resource Economics (2019) 73:843–869
https://doi.org/10.1007/s10640-019-00330-1

Performance of Agglomeration Bonuses in Conservation Auctions: Lessons from a Framed Field Experiment

Zhaoyang Liu[1] · Jintao Xu[2] · Xiaojun Yang[3] · Qin Tu[4] · Nick Hanley[5] · Andreas Kontoleon[6]

Accepted: 23 February 2019 / Published online: 9 March 2019

Abstract

The incorporation of an agglomeration bonus payment to encourage spatial coordination in auction mechanisms to allocate payments for ecosystem services (PES) contracts has been explored as a promising innovation that could enhance the effectiveness of PES schemes. Empirical evidence on the performance of this particular design feature is scant, and almost exclusively derived from laboratory experiments using student subjects. This study reports results from a framed field experimental auction allocating PES contracts with and without agglomeration bonus payments using actual forest land owners in rural China as subjects. We find tentative evidence that, in a PES auction that provides agglomeration bonuses, subjects tend to bid less in anticipation of receiving bonus payments when their neighbours are also successful in the auction. In addition, we have mixed findings as to whether the agglomeration bonus is able to induce a bidding pattern in favour of contiguous conservation. The two sets of results convey some encouraging signals of the theoretically postulated cost-effectiveness and conservation efficacy of the agglomeration bonus. Further research from the actual field is warranted in light of the policy significance of this innovative incentive mechanism.

Keywords Agglomeration bonus · Conservation auction · Payments for ecosystem services · Policy impact evaluation · Framed field experiment

---

Electronic supplementary material The online version of this article (https://doi.org/10.1007/s10640-019-00330-1) contains supplementary material, which is available to authorized users.

✉ Andreas Kontoleon
ak219@cam.ac.uk

[1] Institute of Health and Wellbeing, University of Glasgow, Glasgow G12 8RZ, UK

[2] National School of Development, Peking University, Beijing 100871, People's Republic of China

[3] School of Public Policy and Administration, Xi'an Jiaotong University, Xi'an 710049, Shaanxi, People's Republic of China

[4] Centre for Innovation and Development Studies, Beijing Normal University, Zhuhai 519085, People's Republic of China

[5] Institute of Biodiversity, Animal Health & Comparative Medicine, University of Glasgow, Glasgow G12 8QQ, UK

[6] Department of Land Economy, University of Cambridge, Cambridge CB3 9EP, UK

JEL Classification C72 · C90 · Q23 · Q57

1 Introduction

With the rapid proliferation of payments for ecosystem service (PES) schemes worldwide, academics and policy makers have been interested in improvements to various design facets of PES. Two particular areas of PES improvement that have been extensively considered are the enhancement of their cost-effectiveness and their conservation efficacy. Mechanisms for awarding PES contracts via spatially coordinated auctions have recently been explored as a promising design feature that could improve both the cost of delivery and environmental benefits of PES (de Vries and Hanley 2016; Hanley et al. 2012). PES auctions are reverse or procurement auctions which competitively allocate contracts to deliver ecosystem services (ES), favouring bidders who offer to supply these services at a lower price. This scheme is usually believed to be able to deliver cost savings by mitigating 'information rents', namely the potential extra profits to ES suppliers arising from their private information about their own opportunity costs of ES provision (Arguedas and van Soest 2011; Ferraro 2008). A prime example of PES auctions is the US Conservation Reserve Program (CRP) (Stubbs 2014), along with multiple state-level conservation auctions in Australia (Whitten et al. 2013). Further, considerable scientific evidence has emerged showing the enhanced ecological benefits of contiguous—as opposed to dispersed or isolated—conservation (Margules and Pressey 2000; Williams et al. 2012). In order to incentivise contiguous conservation, some PES programmes pay ES suppliers an extra so called 'agglomeration bonus' (in addition to the basic PES payments) to set aside adjacent lands (Parkhurst et al. 2002). Practical applications of this PES design include the Conservation Reserve Enhancement Program (CREP) in Oregon and a Swiss agri-environmental programme that provides a network bonus.[1]

Whereas the conservation auction mechanism is relatively familiar to PES policy makers, the agglomeration bonus is more novel and largely in the test-bed stage. In the work of Parkhurst and Shogren (2007, 2008), the agglomeration bonus was found to be able to induce contiguous conservation in laboratory settings. Subsequent experimental studies further explored the performance of this innovative instrument under different conditions of communication among subjects (Warziniack et al. 2007), different group sizes (Banerjee et al. 2012) with different information flows (Banerjee 2018; Banerjee et al. 2014), and under varying transactions costs (Banerjee et al. 2017; Ferré et al. 2016). Other laboratory studies investigated the agglomeration bonus in auction settings. But those studies mostly concerned the effects of other auction rules, rather than the pure treatment effects of agglomeration bonuses *per se* (in auction settings). For instance, Reeson et al. (2011) explored two auction rules intended to overcome efficiency loss over multiple bidding rounds (whether winners in previous rounds are allowed to modify their bids in later rounds and whether the end-point is known to bidders). Banerjee et al. (2015) tested whether rent-seeking behaviour would be encouraged if the priority to conserve adjacent lands is revealed to bidders. In the study of Krawczyk et al. (2016), spatial coordination

[1] The CREP programme in Oregon aims to restore contiguous riparian buffers along stream habitats by providing a cumulative impact incentive bonus (CIIB) wherever at least 50% of any five-mile section of streambed is put under conservation (USDA 1998). In Switzerland, the Ordinance of Ecological Quality provides additional payments for linking up conserved areas (Krämer and Wätzold 2018).

in conservation auctions is produced by varying the metric by which bids are judged, to increase the chances of bids from neighbours being successful. The study of Fooks et al. (2016) represents a rare example that assessed the pure treatment effects of agglomeration bonuses in conservation auctions.

Most of the empirical evidence to date on the agglomeration bonus comes from laboratory settings using student subjects.[2] However, much less is known about the performance of this PES design aspect within the relevant population of interest (Cason and Wu 2017). This would require undertaking experimental methods in the field using actual professional land holders. Beyond the scientific usefulness of exploring PES design aspects in the field, there is perhaps an even more compelling policy reason for doing so: policy makers often find recommendations based on student samples to be unconvincing and even irrelevant (List 2011; Lusk and Shogren 2007), despite increasingly reported similarities in results given by student subjects and field professionals (Fréchette 2016; Suter and Vossler 2014). It is, thus, imperative that researchers move their *ex ante* policy evaluations into the field so that they can better communicate to policy makers the usefulness of instruments such as PES. This is even more compelling in countries (such as China) where the usefulness of experimental methods in designing policies is still lagging behind.

Our study aimed to address this need by undertaking perhaps the first such framed field experiment with actual ES suppliers in which we tested the performance of the agglomeration bonus in a simulated forest-based PES auction. Subjects were recruited from peasant farmers in rural areas of China who typically held small forest plots. These forest lands were often officially owned by village collectives, but the use rights have been largely allocated to individual peasant households using fixed-term contracts. These peasants, thus, hold 'quasi-private' forest rights and could become ES suppliers in a forest-based PES programme. Many of them indeed had experience of participating in land-based PES programmes. By moving the experimental auction from lab to the field, this study attempts to enhance our understanding of the reaction of actual ES suppliers to agglomeration bonuses in an auction setting.

Our investigation into the spatially coordinated PES auction is especially pertinent to PES policy-making in rural China. Triggered by catastrophic floods in the late 1990s, several influential land-based PES schemes have been rapidly unfolding, such as the Sloping Land Conversion Programme (SLCP) and the Ecological Public-Benefit Compensation Programme (EPBFCP).[3] The designers of these schemes have aspired to enrol more contiguous plots in pursuit of both enhanced ecological benefits and savings in administrative costs that this offers. However, the progress of this agenda has been hindered due to lack of effective policy instruments. The government's previous efforts to promote contiguous enrolment in these PES-akin programmes relied heavily on command-and-control policies, which may have diminished the degree of voluntary participation of Chinese ES suppliers in these PES schemes, undermining their effectiveness.

[2] Fooks et al. (2016) invited 24 farmers to their laboratory experiments, but the majority of their subjects were 96 students. A few other studies relied on alternative methodologies, such as the questionnaire-based choice experiment conducted by Kuhfuss et al. (2015), and the scenario analysis carried out by Drechsler et al. (2016).

[3] The SLCP was implemented to convert roughly 10% of the country's cropland to forests ; the EPBFCP was intended to conserve 30–40% of the country's forestland via strict restrictions on harvesting timber and other forest products. Both programmes have been compensating the rights holders of enrolled lands (Liu et al. 2018).

PES programmes in China have usually targeted remote rural areas where education levels and experience with complex market interactions are limited. Faced with a complex PES mechanism, individuals might have difficulties understanding the incentive mechanisms and hence make economically sub-optimal decisions. If so, the added value of these innovative PES features could be brought into question. This study thus provides one of the first attempts to provide an *ex ante* experimental investigation of the viability and performance of the agglomeration bonus in PES auctions in the context of rural China, in the sense that we draw lessons about the ability of land managers to understand and respond to the complex incentive mechanisms. To achieve this, we first seek to directly discern misperceptions and anomalous bidding patterns by closely scrutinizing the subjects' bids and their responses to a quiz exercise and debriefing questions regarding the auction rules and the rationales behind their bids. Further, we attempt to assess whether our experimental PES auctions that provide agglomeration bonuses are able to deliver the theoretically predicted effects. In developing the study, the authors collaborated with government agencies (primarily with the State Forestry Administration of China). Our intention was to exemplify to Chinese policy makers the usefulness of experimental methods as a tool to inform policy design. Our results (derived from actual forest land owners) would serve as a 'proof of concept' for the potential use of the agglomeration bonus beyond the laboratory setting. So, in sum the contributions of this paper do not only speak to the applied experimental literature (via our findings from exploring the agglomeration bonus in a more realistic field setting), but also speak to the broader policy literature on the development of PES. We add to the academic voices that have argued that in order for these markets to be developed, it is imperative that we produce a critical mass of such 'evidence based' studies (Ferraro and Pattanayak 2006; Baylis et al 2015). Lastly, the policy significance of the study is further heightened by the fact that it takes place in China which is potentially the world's largest ecosystem service market (for carbon, water and forest related services). Hence, understanding how more complex design rules for PES can work in this region is of particular significance for the wider development and proliferation of such market based instruments in developing and transition economies.

The paper is structured as follows. Section 2 presents a theoretical model which describes the optimal bidding strategies in a PES auction, and how these strategies are adapted in response to the introduction of the agglomeration bonus. These theoretical predictions give rise to several testable hypotheses which were examined by a framed field experiment as specified in Sect. 3. Section 4 reports the data, analytical methods and empirical results. The paper concludes in Sect. 5 with a discussion of the key findings of the study as well as the implications for PES policy making in rural China.

2 Theoretical Framework

This section provides a basic theoretical discussion of the optimal bidding strategies of ES suppliers in a PES auction (adapted from Fooks et al. 2016; Latacz-Lohmann and Van der Hamsvoort 1997). This discussion gives rise to theoretical predictions regarding the effects of agglomeration bonuses on the optimal bidding strategies, serving as the theoretical basis of our framed field experiment.

We start with a conservation auction without agglomeration bonuses. In the context of a forest-based PES programme, forest holders deliver ES by conserving their forest resources rather than clear-felling. Suppose forest holder i has private information about the positive opportunity cost of ES provision (δ_i). If forest holder i tenders a bid (b_i) and is selected into the programme, this person will receive a payment that equals to b_i, given a discriminating price auction format. The programme is modelled as a target-constrained procurement auction which aims to enrol a predetermined proportion of eligible forests with the lowest bids.[4] We further assume that the bid is the only selection criterion: the auction accepts bids in ascending order until all contracts are allocated, and the total number of contracts is smaller than the total number of bidders. This is a sealed-bid auction, which does not give a bidder complete information about others' bids. We therefore describe a bidder's perceived probability of being selected as a function of his/her own bid $[P_i(b_i)]$, and the probability decreases as b_i increases. The forest holder is assumed to be risk-neutral, and would thus choose a bid to maximise the expected net benefit of winning the auction:

$$\left(b_i - \delta_i\right)P_i\left(b_i\right) \tag{1}$$

The first order condition of Eq. 1 can be derived by differentiating Eq. 1with respect to b_i. The optimal bidding strategy can then be solved by equating this first order condition to zero:

$$\left(b_i - \delta_i\right)P_i'\left(b_i\right) + P_i\left(b_i\right) = 0, \tag{2}$$

with the second order condition

$$\left(b_i - \delta_i\right)P_i''\left(b_i\right) + 2P_i'\left(b_i\right) < 0, \tag{3}$$

in which $P_i'\left(b_i\right) = \frac{\partial P_i(b_i)}{\partial b_i}$, and $P_i''\left(b_i\right) = \frac{\partial^2 P_i(b_i)}{\partial b_i^2}$. The optimal bid b_i^* satisfies Eqs. 2 and 3. There exists a mark-up above the opportunity cost: $b_i^* - \delta_i = -\frac{P_i(b_i)}{P_i'(b_i)} > 0$.

In the agglomeration bonus scenario, if the holders of two adjacent forest plots are selected into the programme simultaneously, each of them would receive a bonus payment (a) in addition to the basic compensation they bid for, b_i. Suppose forest holder i has a certain number of neighbours (n), and thus expects an agglomeration bonus payment ($a_{in} > 0$) which depends on the enrolment of other bidders and is not determined by bidder i (Fooks et al. 2016). His/her expected net payoff can be expressed as:

$$\left(b_i + a_{in} - \delta_i\right)P_i\left(b_i\right) \tag{4}$$

We can solve for the optimal bidding strategy of this forest holder using the first order condition:

$$\left(b_i - \delta_i\right)P_i'\left(b_i\right) + P_i\left(b_i\right) + a_{in}P_i'\left(b_i\right) = 0, \tag{5}$$

Let $\hat{b}_i^*$ represent the optimal bid that satisfies Eq. 5. Since $a_{in} > 0$ and $P_i'\left(b_i\right) < 0$, we have $\left(\hat{b}_i^* - \delta_i\right)P_i'\left(\hat{b}_i^*\right) + P_i\left(\hat{b}_i^*\right) = -a_{in}P_i'\left(\hat{b}_i^*\right) > 0$. It can thus be proved that $\hat{b}_i^* < b_i^*$, since $\left(b_i^* - \delta_i\right)P_i'\left(b_i^*\right) + P_i\left(b_i^*\right) = 0$ (Eq. 2) and $\left(b_i - \delta_i\right)P_i''\left(b_i\right) + 2P_i'\left(b_i\right) < 0$ (Eq. 3).

That is, forest holders should tender lower bids, on average, when the PES auction includes an agglomeration bonus. Intuitively, the agglomeration bonus creates an additional expected benefit from winning which results in lower bids being offered to enhance

[4] Land-based PES programmes in China typically specify both the budget and targeted acreage based on crude estimates of conservation costs per unit area. For instance, the SLCP intended to afforest 14.67 million hectares of agricultural lands (Xu et al. 2010a). The central government usually assigns disaggregated targets to its local representatives as a critical criterion for assessing their performance in implementing the PES programme. Handing out budgets without explicitly targeted outcomes (which characterises a budget-constrained scheme) is rare in our local contexts.

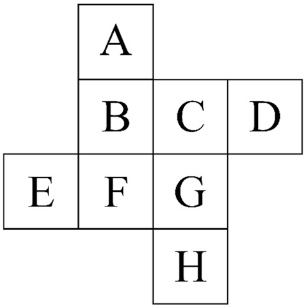

Fig. 1 Spatial configuration of the forest plots in the experiment

the probability of winning. In addition, one bidder would anticipate a downwards shift in others' bids for the same reason, and may hence further bring down his/her own optimal bid. In other words, a bidder in the agglomeration bonus treatment would bid lower not only through the direct effect of the bonus but also indirectly through the expectation that others would as well lower their bids.

Now suppose that forest holder i has more neighbours ($m > n$) and expects a higher agglomeration bonus payment ($a_{im} > a_{in} > 0$). It can be shown that the optimal bid in this scenario would further decrease ($\tilde{b}_i^* < \hat{b}_i^* < b_i^*$). Or in other words, in a conservation auction which offers agglomeration bonuses, forest holders would bid less to enrol forest plots with more neighbours (other conditions being equal). This would in turn increase their likelihood of being selected into the conservation scheme and hence induce contiguous conservation. This mechanism can be contrasted with that analysed in Banerjee et al. (2015) and Krawczyk et al. (2016), who instead use an Environmental Benefits Index to help rank bids. Bids from more connected plots attract a higher score on this Index, and thus are more likely to be successful. In contrast, the scheme investigated here offers an agglomeration bonus to all bidders.

Based on these theoretical findings,[5] the following hypotheses can be proposed:

Hypothesis 1: An agglomeration bonus will induce lower bids in a conservation auction (other conditions being equal).

Hypothesis 2: The agglomeration bonus would promote contiguous conservation by inducing even lower bids for those forest plots with more neighbours (other conditions being equal).

These hypotheses were tested using a framed field experiment conducted in rural China with actual forest holders, as described in the following section.

[5] For brevity, our theoretical discussion assumes that bidders are risk neutral. Alternatively, we can model risk aversion (associated with uncertainty in income) following the study of Latacz-Lohmann and Van der Hamsvoort (1997). It can be proved that Hypotheses 1 and 2 still hold.

3 Experimental Design and Procedures

This section first outlines the general set up of our experiment in order to provide a general understanding of it. We next explain its main design features and implementation procedures in more detail. The full protocol is provided as online supplementary material.

Our experiment consisted of asking subjects to take part in experimental procurement auctions for conservation contracts in groups of eight participants. Each participant was assumed to be in possession of one forest plot when placing their bid. There were eight unique plot types, as presented in Fig. 1. The plots differed in terms of timber values (10,000; 15,000; 20,000; 25,000)[6] and/or the number of adjacent plots (1; 3).[7] All the other attributes of these plots were identical and kept fixed across participants. The timber values were assumed to be fixed through time, in which case it would be economically rational to fell the trees right away in exchange for the corresponding payments. Each participant was asked to bid for a compensation payment that would make him/her willing to wait for 1 year to deforest and receive the timber value, compared to deforesting and receiving the timber value the next day. This payment represented a PES contract aiming to deliver ES by conserving enrolled forests for 1 year. Following the 'strategy method',[8] each participant's actual plot type was unspecified prior to bidding, but would be one of the eight possible types already known to participants. Participants were asked to place a bid for each of the eight possible plots type. After bidding, the actual plot type for each participant was determined randomly. The bids corresponding to the chosen actual plot types were then entered into a one-shot discriminative price auction, with the four lowest bids selected into the simulated PES programme. Finally, our experiment randomly assigned an agglomeration bonus treatment to half of the auction sessions to assess its performance.

[6] All the monetary values mentioned in the experiment were measured in experimental currency units, which were exchanged to Chinese Yuan Renminbi (CNY) at an exchange rate of 1:200 at the end of each experimental session. This was explicitly explained to subjects at the very beginning of each session. The relative merits of using experimental currency units (ECUs) in behavioural experiments remain an open research issue. Some authors argued strongly against this mode of payment (e.g. Davis and Holt 1993), whereas others have found it acceptable in experimental practice (e.g. Drichoutis et al. 2015). In our study, the use of ECUs has enabled us to take advantage of the money illusion effect (Shafir et al. 1997) and to facilitate the implementation of our experiments in an affordable way. During the exploratory pilot work the preceded the final experiment, we designed affordable timber values (around CNY 100 for each subject on average) and framed them in real (monetary) terms. However, these figures were found to be much lower than the actual monetary values of mature trees in our fieldwork locations, which ranged from CNY 10,000–25,000 per mu of forestland held by each subject. In light of that, many subjects in our pilots (mistakenly) assumed that the trees described in our experiments were premature and would become more valuable in the future (although our protocol explicitly ruled this out). They were thus willing to take conservation actions (by delaying deforestation) without any compensation and bid for zero or extremely low compensation. This misunderstanding largely undermined our conservation auctions. In the following round of pilots, we explored using ECUs and described timber values in nominal terms according to actual monetary values (10,000–25,000 ECUs). This effectively addressed the 'premature trees' perception and enhanced the viability of the experiment.

[7] We intentionally avoided mapping each plot type to a particular plot in Fig. 1, so as to reduce the potential impact of any letter preference bias (e.g. some subjects may favour Plot A only because it is labelled with the letter A).

[8] According to existing evidence, subjects tend to give very similar responses between the *strategy* and *direct-response* methods in experimental settings (Brandts and Charness 2011; Kirchkamp et al. 2009). We employed the strategy method so as to obtain more information about subjects' bidding behaviour at relatively lower costs (Kirchkamp et al. 2009; Kirchkamp and Reiß 2011).

Having described the general set up of the experiment we next provide a more detailed discussion of the decision making process and of key design aspects of the protocol. Firstly, every subject's individual bid for each of the eight plot types was made conditional on his/her expectation of the bids for the remaining seven plot types made by the other seven subjects. For example, when Participant 1 (out of a total of 8) was bidding for a conservation contract under Plot Type 1, she/he would consider the expected bids of players 2–8 for Plot Types 2–8. When Participant 1 would next bid for Plot Type 2 she/he would now consider the expected bids of players 2–8 for Plot Types 1 and 3–8. Participant 1 would continue to bid in this manner for the remaining six plot types to formulate a full list of eight bids. The other seven participants would bid in the same way. This process was effective as we did not allow bidders in the same auction group to hold the same plot type. Secondly, the compensation that the subjects bid for was to cover forest holders' opportunity costs of delaying for 1 year the felling of trees in each particular plot (as opposed to deforesting right away). These costs could stem from inflation, time preference and loss of returns which could have been earned by investing the timber income during that year. Therefore, when designing the wording used in the experimental protocol, we consulted previous experimental studies that used auctions to elicit people's time preferences (Deck and Jahedi 2015; Harrison and McKay 2012; Manzini and Mariotti 2014). Subjects were essentially being asked to choose whether to receive a payment *the next day*, versus another payment *after a year plus* 1 *day*. The rationale of adopting this front-end delay was to eliminate credibility bias which might have arisen had we contrasted an *immediate* payment with another payment *after* 1 *year*. In the latter case, subjects might have strongly preferred the immediate payments due to concerns about the credibility of future payments. Adopting a front-end delay design aimed at cancelling out such bias since both options in the choice set were future payments (Carlsson et al. 2012; Harrison et al. 2002).

After the bidding process, we drew lots without replacement so as to obtain the (single) assigned forest plot for each player and informed the subjects about this outcome. Then in each group of eight subjects, the four subjects with the lowest bids were selected into the simulated PES programme, which represented a target-constrained PES auction. They were due to receive the first payments the next day (the PES compensation payments equivalent to the monetary value of their bids), and the second payments 1 year later (the logging profits equivalent to the monetary value of their timber). The other four subjects with higher bids would receive the logging profits the next day. This represented the business-as-usual scenario wherein non-participating forest holders would harvest and sell their timber straight away.[9] The future payments were made by bank transfer or postal order, depending on the subjects' preference. In order to enhance the credibility of actually receiving the future payments, we signed IOU notes in the name of Peking University, which specified the debtor, the creditor, the amount owed and the due date of repayment.[10] Moreover, we recorded the subjects' bank accounts/postal addresses on actual deposit slips/postal orders issued by actual local banks and post offices.

[9] In this standard experimental set up the compensation payments sought by the bidders, or the conservation costs, were the only criterion used for selection into the PES. In comparison, the US CRP and some previous experimental conservation auctions (e.g., Banerjee et al. 2015; Krawczyk et al. 2016) also took into account the expected ecological benefits. We did not incorporate that criterion, which would have made our auction rules overly complex, especially in the presence of the agglomeration bonus. This, however, should be the subject of future work.

[10] This practice was also adopted in the study of Carlsson et al. (2012).

The experiment was a one-shot auction *without* repeated bidding rounds. Such one-shot settings can be commonly observed in field experiments on conservation auctions (e.g., Jack 2013; Messer et al. 2015; Wang et al. 2012). This design is advocated by some researchers who argue that, in repeated auctions, a subject's bids might become 'affiliated to others' instead of representing his/her own opportunity cost (Harrison et al. 2004). This concern may explain some laboratory evidence of a decline in the efficacy of the conservation auction instrument (Schilizzi and Latacz-Lohmann 2007) over repeated rounds.[11] On the other hand, using a multiple round design could help subjects better formulate their optimal bidding strategy (through adaptive learning). As our resources did not allow the use of different treatments on this design element (i.e. both one-shot and repeated bids), we decided to choose the one-shot design mostly on pragmatic grounds. Our pilot fieldwork found that the typical duration of a one-shot session was about 1 h, excluding the follow-up questionnaire survey. In light of this, conducting multiple bidding rounds would expose subjects to considerable survey fatigue and cognitive load, which might at worst undermine the feasibility of our experiment. Further, China's PES programmes in practice are more akin to one-shot schemes. For instance, in the past two decades, the SLCP only had two sign-ups targeting different lands. Therefore, a one-shot auction was also considered to be more relevant for deriving implications for PES policy-making in China.

We then sought to evaluate the effects of adding an agglomeration bonus element in the auction by comparing experimental bids with and without this instrument (hereinafter referred to as the treated and control groups). In the treated group, the agglomeration bonus scheme was implemented to encourage contiguous enrolment. If one forest plot was selected into the PES programme, the forest holder would receive a bonus worth 100 for each border shared with other selected forest plots. The bonus would be paid to the subjects the next day in addition to the compensation they had bid for. In our theoretical model, this is tantamount to setting the value of the parameter a at 100 in the agglomeration bonus scenarios. Following the study of Parkhurst and Shogren (2008), we set the agglomeration bonus at such a level that the total conservation payment in the treated group (the bids plus the bonuses) would be similar to that in the control group (the bids only), based on our pilot fieldwork.[12] As shown by Fig. 1, for those forest plots surrounded by three 'rook neighbours'[13] (B, C, F and G), the agglomeration bonus could potentially add up to 300. For the other plots with one rook neighbour (A, D, E and H), the bonus would be no more than 100. Our theoretical model predicts that rational forest holders would further lower their bids for those forest plots with more neighbours, in anticipation of receiving more agglomeration bonuses. Such plots would thus stand a better chance of being selected into the PES programme, which would in turn promote contiguous conservation.

The experimental protocol was piloted in both laboratory and field settings. We undertook pilots respectively with 32 university students and 24 forest holders from one of the

[11] Another reason for this decline is related to strategic uncertainty in the network of participants (Banerjee et al. 2012).

[12] This was achieved by piloting different levels of bonus. We used the same group size and selection rate throughout all the pilots.

[13] Rook neighbours refer to those adjacent polygons that share at least one common border, as opposed to queen neighbours which could share at least one common border or a corner point. They are defined analogously to the movement of chess pieces.

Fig. 2 Fieldwork locations on a forest map of China. *Notes*: Shaded areas represent forests. Source of the land use map: University of Maryland

counties to be visited in the formal fieldwork. The formal fieldwork was conducted in the summer of 2015 with 192 forest holders (24 auction groups with 8 participants each) in Sichuan province (as shown in Fig. 2). We purposefully selected Sichuan province as our study site, on account of the significance of its forests in the delivery of watershed services and biodiversity. Further, it is a province where the future design of PES schemes has received considerable interest.[14]

Following the stratified sampling method, we randomly sampled four counties in the province, three villages in each county and sixteen subjects in each village. We conducted two experimental auctions in each village with two different groups of subjects, and the agglomeration bonus treatment was randomly assigned to one of the two groups. Half of our auction groups were randomly assigned to the treated and the other half to the control group (so each study arm included 12 auction groups and 96 subjects and obtained a total 768 bids).

Each experimental auction started with an extensive introduction and an exercise session. The enumerators made every effort to ensure against misunderstanding of the auction rules. The subjects then tendered their bids in private, which were recorded by the enumerators. After that, the enumerators debriefed the subjects in an attempt to elicit the rationale behind their bidding decisions. This was followed by attitudinal follow-up questions, cognitive ability questions and socio-demographic questions. Finally, we assembled all participants and publicly drew lots to obtain each subject's forest plot. The corresponding bids of each type of plot were sorted in ascending order and the four lowest bids were selected into the simulated PES programme. Each subject received a show-up fee (CNY 50) in cash

[14] Sichuan is one of the only two provinces that accommodate the upper courses of both the Yangtze River and Yellow River, the two largest rivers of China. Its forests contribute greatly to water inflows to both rivers. Moreover, they provide vital habitats for a variety of endangered species, including the giant panda. It is thus a priority area of ongoing and future forest-based PES in China. These features render Sichuan province an ideal setting for our fieldwork.

Table 1 Definition of variables and descriptive statistics

Variable	Full sample		Anomalous bidders excl.	
	Mean	SD	Mean	SD
Panel 1: Bid (Bidder-level averages)	*Obs. = 192*		*Obs. = 150*	
Bid	1172.31	2625.06	883.10	1600.19
Bid for a plot with 1 neighbour	1158.70	2555.26	875.85	1488.29
Bid for a plot with 3 neighbours	1185.91	2717.36	890.35	1739.63
Panel 2: Covariates (Bidder-level)	*Obs. = 192*		*Obs. = 150*	
Timber value drawn (10^3 CNY)	17.40	5.58	17.10	5.56
Exercise performance Number of correct answers in the exercise session divided by the total number of exercise questions	0.88	0.18	0.87	0.19
Age	47.69	14.86	47.44	15.17
Gender (binary: 1 = male; 0 = female)	0.65	0.48	0.67	0.47
Ethnic minority (binary: 1 = yes; 0 = no)	0.78	0.42	0.77	0.42
Education (years of education attained)	5.62	3.67	5.54	3.74
Household head (binary: 1 = yes; 0 = no)	0.60	0.49	0.61	0.49
CCP[a] member (binary: 1 = yes; 0 = no)	0.22	0.41	0.20	0.40
Village leader (binary: 1 = yes; 0 = no) Membership of the villagers' committee, a villagers' representative or leader of a villagers' group	0.29	0.46	0.29	0.46
Children Number of children (under 16 years old) in a subject's household	0.77	0.83	0.70	0.79
Elderly Number of the elderly (over 60 years old) in a subject's household	0.79	0.82	0.81	0.85
Students Number of students in a subject's household	0.78	0.85	0.75	0.87
Savings (binary, 1 = yes; 0 = no) Whether a subject's household had savings in 2014	0.37	0.48	*0.40*	0.49
Debt (binary, 1 = yes; 0 = no) Whether a subject's household was in debt in 2014	0.31	0.46	0.30	0.46
Market experience (binary: 1 = yes; 0 = no)	0.65	0.48	0.66	0.48

Table 1 (continued)

Variable	Full sample		Anomalous bidders excl.	
	Mean	SD	Mean	SD
Land dispossession (binary: 1 = yes; 0 = no)	0.05	0.22	0.06	0.24
Whether a subject's household lost any lands during 1997–2014 due to land reallocation or expropriation				
Time preference dummy 1 (binary, 1 = yes; 0 = no)[b]	0.23	0.42	0.23	0.42
Time preference dummy 2 (binary, 1 = yes; 0 = no)	0.19	0.39	0.21	0.41
Time preference dummy 3 (binary, 1 = yes; 0 = no)	0.27	0.45	0.27	0.44
Time preference dummy 4 (binary, 1 = yes; 0 = no)	0.31	0.46	0.29	0.46
Risk preference dummy 1 (binary: 1 = yes; 0 = no)[c]	0.34	0.48	0.34	0.48
Risk preference dummy 2 (binary: 1 = yes; 0 = no)	0.17	0.38	0.17	0.37
Risk preference dummy 3 (binary: 1 = yes; 0 = no)	0.48	0.50	0.49	0.50
Trust in future payments (binary: 1 = yes; 0 = no)	0.85	0.35	0.87	0.33
Environmental awareness (binary: 1 = yes; 0 = no)	0.98	0.12	0.98	0.14
Whether a subject considered forest conservation beneficial to the village or the country				
Cognitive skills	0.76	0.20	0.77	0.21
Number of right answers in the cognitive test divided by the total number of cognitive questions				

[a] *CCP* Chinese Communist Party

[b] The subjects were asked to make a hypothetical choice between: A) receiving a payment of CNY 100 the next day, and B) receiving a payment 1 year later. They were asked to specify the amount of the payment in option B) which would make them value the two options as being equivalent, and the responses were censored at 10,000. This is consistent with the fill-in-the-blank elicitation method of our experimental auction. We constructed four dummy variables that indicate the four consecutive intervals of the responses: 'time preference dummy 1' $\in [100, 110]$, 'time preference dummy 2' $\in (110, 180]$, 'time preference dummy 3' $\in (180, 300]$, and 'time preference dummy 4' > 300. We started with 10 intervals cut by the 0.1–0.9 quantiles of the responses (the groups were uneven due to the existence of same values), estimated the bid model using dummy variables representing the 10 intervals, and merged those consecutive intervals that had qualitatively similar estimates

[c] The subjects were asked to make a hypothetical choice between: A) a guaranteed payment worth CNY 100, and B) a 50% chance (by flipping a coin) of receiving a payment. They were asked to specify the amount of the payment in option B) which would make them value the two options as being equivalent, and the responses were censored at 10,000. We constructed three dummy variables that indicate the three consecutive intervals of the responses: 'risk preference dummy 1' $\in [100, 200]$, 'risk preference dummy 2' $\in (200, 500]$, and 'risk preference dummy 3' > 500. These variables were constructed in a manner similar to the time preference variables

immediately. In addition, they earned future payments from the experimental auction worth CNY 88.14 on average. The total average payment for each subject thus amounted to CNY 138.14 (USD 22.10), which roughly equals 1 day's off-farm wage.

4 Results

Before we formally test the hypotheses described in Sect. 2, we first define and describe the variables involved in our analysis, as in Table 1. When selecting these covariates, we referred to previous studies on determinants of people's bidding decisions in PES auctions (Jack 2013). We firstly included standard demographic variables such as 'age', 'gender', 'ethnic minority' and 'education', etc. Further, as described by our theoretical model, the efficacy of the agglomeration bonus stems from people's profit-maximising bidding strategies. We thus controlled for several potential barriers to such optimal decision-making. These barriers include misperceptions about the experiment, cognitive constraints (Fudenberg and Levine 2006), inadequate market experience (List 2003) and insecure property rights (Groom et al. 2010). These are respectively proxied by 'exercise performance', 'cognitive skills', 'market experience' and 'land dispossession' (see Table 1). Moreover, as the subjects in our experiment were essentially making trade-offs between two future payments, their decisions would be inevitably affected by their time and risk preferences. We hence introduced three subjective variables: 'impatience', 'risk aversion' and 'trust in future payments' (see Table 1). In addition, according to standard inter-temporal choice theory, people's savings and debts can be regarded as two objective measures of their time preferences. Lastly, Table 1 summarises these variables both for the full sample and when 'anomalous bidders' are excluded, the latter being those who did not understand the experimental rules (see details further below).

We then tested the balance of these covariates between control and treated groups (for full sample and with anomalous bids excluded). The results of our balance test are reported in Panel 2 of Table 2. The covariates examined are found to be jointly balanced between the control and the treated groups (the p-value for the F-test is 0.89 in the full sample). Only three covariates ('age', 'household head' and 'children') in the full-sample are individually imbalanced at conventional significance levels. These tests suggest that our experimental groups are well balanced across a wide range of socio-economic variables. Any remaining imbalance is what would be expected due to chance.

Furthermore, we assessed the subjects' understanding of the experiment and their levels of rationality in formulating their bids. The findings directly speak to an important research question, namely whether a PES auction that provides agglomeration bonuses is understandable to subjects with limited education and market experience. In addition, this is informative for our subsequent data analysis, which should control for any misperceptions and protests. Before the bidding process, all subjects undertook a quiz type exercise regarding the rules of the experiment. There were 107 subjects (56% of the full sample) that correctly answered all questions in the first attempt, and another 54 subjects (28% of the full sample) that got one question wrong. In the instance of wrong answers, the enumerators would re-explain the rules. Eventually all subjects reportedly understood the experiment. Despite that, after the bidding process, 15 subjects (8% of the full sample) exhibited protest attitudes and/or conspicuous misunderstanding of the rules in response to our open-ended debriefing question about the rationale behind their bids. These subjects can be classified as follows according to their answers: (1) seven

Table 2 Comparison of means between control and treated subjects

Variable	Full sample				Anomalous bidders excluded			
	Treated	Control	Mean difference (trt.—ctrl.)	*p*-value	Treated	Control	Mean difference (trt.—ctrl.)	*p*-value
Panel 1: Bid (Bidder-level averages)	*Obs. = 192*				*Obs. = 150*			
Bid	988.84	1355.77	−366.93	0.33	842.23	921.85	−79.62	0.76
Bid 1 neighbour	960.94	1356.46	−395.52	0.28	828.25	920.97	−92.72	0.70
Bid 3 neighbours	1016.75	1355.08	−338.33	0.40	856.20	922.73	−66.53	0.82
Panel 2: Covariates (Bidder-level)	*Obs. = 192*				*Obs. = 150*			
Timber value drawn	17.34	17.45	−0.11	0.90	16.85	17.34	−0.49	0.59
Exercise performance	0.87	0.88	−0.01	0.65	0.86	0.88	−0.02	0.63
Age	***45.68***	***49.70***	***−4.02****	***0.06***	46.14	48.68	−2.54	0.31
Gender	0.64	0.67	−0.03	0.65	0.67	0.68	−0.01	0.96
Ethnic minority	0.77	0.78	−0.01	0.86	0.75	0.78	−0.03	0.71
Education	5.94	5.30	0.64	0.23	5.82	5.27	0.55	0.36
Household head	***0.51***	***0.69***	***−0.18*****	***0.01***	*0.55*	*0.68*	*−0.13*	*0.11*
CCP member	0.21	0.23	−0.02	0.73	0.22	0.18	0.04	0.57
Village leader	0.24	0.34	−0.10	0.11	0.26	0.32	−0.06	0.39
Children	***0.86***	***0.67***	***0.19****	***0.10***	0.77	0.64	0.13	0.31
Elderly	0.77	0.81	−0.04	0.73	0.82	0.81	0.01	0.90
Students	0.75	0.80	−0.05	0.67	0.70	0.81	−0.11	0.45
Savings	0.34	0.40	−0.06	0.46	0.37	0.43	−0.06	0.47
Debt	0.27	0.34	−0.07	0.30	0.28	0.32	−0.04	0.54
Market experience	0.63	0.68	−0.05	0.45	0.66	0.66	0.00	0.95
Land dispossession	0.05	0.05	0.00	1.00	0.07	0.05	0.02	0.67
Time preference dummy 1	0.21	0.25	−0.04	0.49	0.22	0.25	−0.03	0.69
Time preference dummy 2	0.19	0.19	0.00	1.00	0.23	0.18	0.05	0.44
Time preference dummy 3	0.29	0.25	0.04	0.52	0.26	0.27	−0.01	0.86

Table 2 (continued)

Variable	Full sample				Anomalous bidders excluded			
	Treated	Control	Mean difference (trt.—ctrl.)	*p*-value	Treated	Control	Mean difference (trt.—ctrl.)	*p*-value
Time preference dummy 4	0.31	0.31	0.00	1.00	0.29	0.30	−0.01	0.88
Risk preference dummy 1	0.32	0.36	−0.04	0.55	0.34	0.34	0.00	0.95
Risk preference dummy 2	0.14	0.21	−0.07	0.18	0.14	0.19	−0.05	0.35
Risk preference dummy 3	0.54	0.43	0.11	0.11	0.52	0.47	0.05	0.52
Trust in future payments	0.82	0.89	−0.07	0.22	***0.92***	***0.82***	***0.10***	***0.07****
Environmental awareness	0.98	0.99	−0.01	0.56	0.97	0.99	−0.02	0.53
Cognitive skills	0.77	0.76	0.01	0.67	0.78	0.76	0.02	0.47
Joint covariate balance (*p*-value)	0.89				0.73			

Significant results are highlighted in bold italics (up to the 10% significance level)

Asterisks indicate statistical significance: **p*-value < 0.10, ***p*-value < 0.05, ****p*-value < 0.01

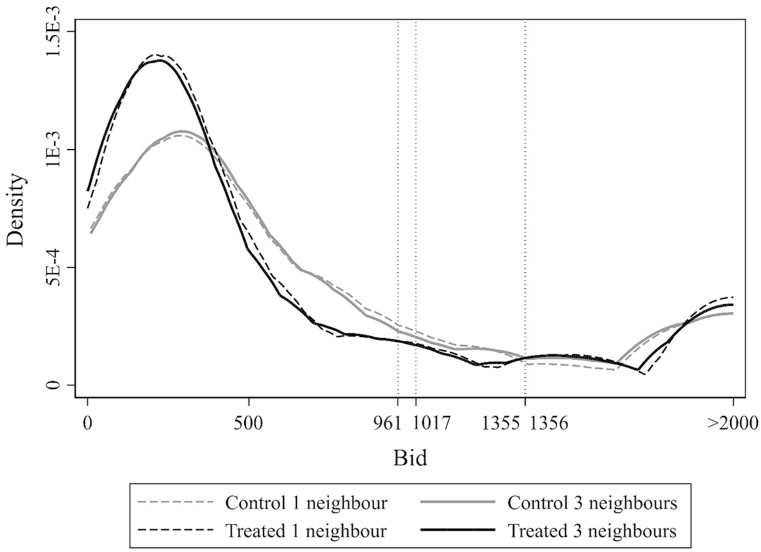

Fig. 3 Distributions of bids by treatment and number of neighbours

subjects thought the value of their trees was going to change after 1 year, whereas the experiment explicitly ruled out this possibility; (2) three subjects intentionally offered extremely high bids to avoid being selected and ignored all other factors (such as the subsidy and the bonus); (3) two subjects had difficulties understanding the experiment; (4) two subjects refused to carefully think and engage with the rules of the experiment; 5) one subject mistakenly believed that the number of neighbours was not relevant in the agglomeration bonus treatment. The responses from all other subjects did not clash with the experimental instructions.

Close scrutiny of the bids revealed that 32 subjects (17% of the full sample) altered their bids non-monotonically for plots with different timber values (the number of neighbours being equal). For example, as the timber value increased, they first bid higher but then lower, or even reversed their bids more than once. This may indicate insufficient understanding of the experiment and/or protest behaviour.

The two types of subjects, hereinafter referred to as 'anomalous bidders', together constitute 22% of the full sample (five subjects belong to both types, so 42 in total). Admittedly, these procedures cannot fully sieve out all the bidders that had difficulties understanding the experiment. Yet, the currently available information only gives indications of misperceptions or anomalies for a minority of the subjects. Table 5 in the appendix compares the auction bids and observable characteristics of anomalous bidders to non-anomalous bidders. It can be seen that anomalous bidders are more likely to have children in their households and less likely to have bank savings. These signs may imply that anomalous bidders are subject to tighter household budget constraints, which may have inclined them to bid much higher, so as to either receive a high 'PES compensation payment', or lose the auction and receive the 'timber value' sooner.

Table 3 Regression results of the bid-level treatment effects of the agglomeration bonus

Dependent variable: *bid*	Full sample		Anomalous bidders excluded	
Explanatory variable:	Model 1	Model 2	Model 3	Model 4
Agglomeration bonus	−203.26 (312.50)	−237.07 (309.24)	***−367.74**** ***(219.65)***	***−377.91**** ***(225.42)***
More neighbours		−7.22 (20.77)		4.51 (33.91)
Agglomeration bonus × More neighbours		69.52 (79.86)		20.88 (100.11)
Timber value		***64.10******* ***(22.54)***		***59.40******* ***(16.32)***
Agglomeration bonus × Timber value		−24.44 (24.76)		***−28.41**** ***(17.01)***
Constant	Yes	Yes	Yes	Yes
Subject-specific covariates (Table 1)	Yes	Yes	Yes	Yes
Village fixed effects	Yes	Yes	Yes	Yes
Enumerator fixed effects	Yes	Yes	Yes	Yes
Obs.	1536	1536	1200	1200
Number of bidders	192	192	150	150
Model significance (*p*-value)	0.19	0.00	0.00	0.00
R^2	0.37	0.38	0.47	0.49

Significant results are highlighted in bold italics (up to the 10% significance level)

Asterisks indicate statistical significance: *p-value < 0.10, **p-value < 0.05, ***p-value < 0.01. Standard errors (clustered at the subject level) are in parentheses

We now explore Hypothesis 1 that the agglomeration bonus leads to lower bids in conservation auctions. Our analysis starts with the treatment effects of the agglomeration bonus on the auction bids obtained from the treated and control groups. Given random assignment of the agglomeration bonus treatment we can validly compare treated and control subjects. Any observed differences in bids should be attributed to the treatment and sampling variability. Panel 1 of Table 2 present differences in unconditional mean bids (using t tests) between the control and treated groups. Figure 3 illustrates the distributions of bids by treatment and number of neighbours. Table 3 reports the regression results at the bid level where we control for the effects of covariates on the observed bids.[15]

Starting with Panel 1 of Table 2, the negative treatment effects on bids indicate that the subjects tend to bid less in the presence of the agglomeration bonus, but this finding is

[15] As mentioned above, the randomisation procedure inherently helps achieve unconfoundedness, implying that any observed differences between the control and treated groups should be attributable to the treatment *per se*. However, owing to finite sample size and sampling variability, this property is only valid on average across samples, not necessarily in any sample (Imai et al. 2008). Therefore, if covariate imbalance is found in a particular sample, as in our case, statistical adjustments should be applied so as to reduce potential bias (Imai et al. 2008; King et al. 2011).

Table 4 Group-level treatment effects of the agglomeration bonus

	Treated mean (SE)	Control mean (SE)	Treatment effect (SE)
Panel 1: Actual experimental auctions			
Payment (without bonus)	***675 (52.22)***	***993.33 (146.01)***	***−318.33** (155.07)***
Payment (with bonus)	1008.33 (86.63)	993.33 (146.01)	15.00 (169.77)
Degree of contiguity	0.42 (0.09)	0.38 (0.07)	0.04 (0.12)
Degree of connectivity	0.65 (0.08)	0.58 (0.07)	0.07 (0.11)
Panel 2: Simulated experimental auctions			
Payment (without bonus)	***702.93 (48.62)***	***986.16 (134.88)***	***−283.23* (143.38)***
Payment (with bonus)	1053.93 (47.33)	986.16 (134.88)	67.77 (142.95)
Degree of contiguity	0.44 (0.02)	0.42 (0.01)	0.02 (0.02)
Degree of connectivity	0.65 (0.02)	0.63 (0.01)	0.02 (0.02)

Significant results are highlighted in bold italics (up to the 10% significance level)

Control obs. = treated obs. = 12. Asterisks indicate statistical significance: **p*-value < 0.10, ***p*-value < 0.05, ****p*-value < 0.01. The 'degree of contiguity' is defined as the number of shared borders between conserved plots divided by the maximum possible number of shared borders (four). On the other hand, connectivity relates to whether two forest plots share a border or if they can be linked to each other through their rook neighbours. For example, as shown by Fig. 1, forest plots C and E are considered to be connected since they can be linked to each other through their rook neighbours B and F. For the conserved plots in each group, the 'degree of connectivity' is defined as the number of connected plots divided by the maximum possible number of connected plots (four). We focus on the highest level of connectivity if there are multiple clusters of conserved plots in one group

statistically insignificant. We used each subject's average bid for the analysis instead of the eight raw bids to address within-subject correlation.[16,17]

Table 3 presents treatment results in a regression setting. Models 1 and 2 report results from different specifications using the full sample. Both models were estimated using all the bids, but we have addressed within-subject correlation by clustering the standard errors at the subject level. Further, both models control for a variety of covariates, *and* village and enumerator fixed effects. As can be seen in Model 2, the coefficient of 'agglomeration bonus' is still negative but statistically insignificant. The estimate has a 95% confidence interval ranging from −847 to 373, which implies that in a nontrivial proportion of instances bidders would not necessarily bid higher if agglomeration bonuses are provided.

[16] The treatment effect estimates would be negative and statistically *significant* if all the raw bids are indiscriminately used. But the statistical significance may have been overrated, as there likely exists unobserved correlation among the eight bids tendered by the same subject, which may have misleadingly underestimated the standard errors (Cameron and Miller 2015).

[17] Admittedly the bid-timber value ratios are indeed much lower compared to other time preference elicitation studies in rural China (e.g. Carlsson et al. 2012). We checked the subjects' responses to our debriefing question and found that many subjects regarded the interest rates of bank savings or the subsidy rate of the SLCP programme as a reference point, and made some adjustments according to the auction rules (i.e. only lower bids were going to be selected) and the treatments (i.e. timber values, the agglomeration bonus and the number of neighbours). This may not strictly conform to the time preference elicitation nature of our experimental auctions. But this is likely to be how farmers would react to a conservation auction in our study area. In addition, we find no indication of correlation between such reference dependence and the agglomeration bonus treatment. This implies that we are likely to be able to difference out such reference dependence when assessing the efficacy of the agglomeration bonus, which would give rise to valid estimates of the treatment effects.

We found qualitatively similar findings from the most parsimonious specification that excludes all the control variables and only contains the agglomeration bonus treatment and other aspects of the experimental design (timber value, number of neighbours and their interaction terms with the agglomeration bonus treatment).

However, recall that we discerned 42 anomalous bidders from people who have clear misperceptions about the auction mechanism and protestors. We re-estimated the bid models using a subsample that excluded these 'anomalous bidders', giving rise to Models 3 and 4 in Table 3. The coefficient of 'agglomeration bonus' in Model 4 is statistically significant at the 10% level, and notably larger in size: the coefficient translates into a 43% reduction in the mean bid of the subsample that excludes anomalous bidders. Further, the negative and significant coefficient of the interaction term between the 'agglomeration bonus' and 'timber value' variables suggest that non-anomalous subjects' bids are reduced to a greater extent for plots with higher timber values. The subsample estimates conform to our theoretical prediction, yet may not be representative of the population we wish to study (although we find no covariate imbalance between the full sample and the subsample).

Moreover, we tested Hypothesis 1 at the auction group level. As shown by Panel 1 of Table 4, the group-level conservation payment in the treated groups (excluding the bonus payment) is considerably lower than that in the control groups. This result is in line with that of Fooks et al. (2016). After accounting for the bonus payment, total conservation payments are practically no different between the control and treated groups. However, these findings should be taken with caution, as they were derived from our experimental auctions that only used one random draw from each subject's eight bids. To check the robustness of the finding, we simulated all possible auction sessions using the recombination approach (Margolis and Shogren 2004). We first generated all possible combinations of bids that could have been drawn within each original auction group. With Permutations ($n=8$, $k=8$) we generated 40,320 combinations per group, and *967,680* in total across all groups. We next worked out the auction outcome of each combination that matches our actual experimental auctions, and calculated each combination's total payment, which was averaged over all combinations of each original auction group. The group means were then used to estimate the treatment effects. As can be seen in Panel 2 of Table 4, the simulation results are highly similar to those derived from actual experimental auctions. These findings provide supporting evidence for Hypothesis 1 at the auction group level, but admittedly cannot substantiate the 'no-regrets' property of the agglomeration bonus, namely it has the potential to encourage contiguous conservation without significant additional budgetary costs. Indeed, the mean equivalence tests presented in Table 4 suggests that we cannot reject the null hypothesis that on average the control and treated groups have equal costs (accounting for the agglomeration bonus payments for the treated groups). Despite that, if we test whether the costs of the treated groups are 10% higher than the control group, we cannot reject the null hypothesis either (H_0: costs of treated groups—costs of control groups ≥ 99.33, p-value $= 0.31$). In light of that, we cannot rule out the possibility that the agglomeration bonus would incur nontrivial additional costs.

We next explore the conservation implications of the agglomeration effect. We expect the agglomeration bonus to induce even lower bids for forest plots with more neighbours. This would increase their probability of being selected into the PES programme and thereby promote more contiguous conservation (Hypothesis 2). Going back to Panel 1 of

Table 2, we firstly look at the difference in the treatment effect as the number of neighbours increases. We have no significant findings. On average the agglomeration bonus treatment does not reduce auction bids to a greater extent for plots with more neighbours (−338 for plots with three neighbours whereas −396 for plots with one neighbour). The difference (57) is relatively small (less than 5% of the mean bid) and statistically equal to zero (p-value = 0.92).

In our regression models (Table 3), the dependence of the treatment effects on the number of neighbours is captured by the interaction term between the 'agglomeration bonus' and 'more neighbours' variables. For the full sample, the coefficient of this interaction term in Model 2 is highly similar to the result of the unconditional mean comparison. Upon the removal of the 42 anomalous bidders, the counterpart coefficient in Model 4 becomes even smaller (2% of the subsample's mean bid) and remains equal to zero in a statistical sense.[18]

Despite these findings, one might wonder whether the agglomeration bonus treatment has delivered any group-level improvements in the spatial coordination of conservation. Table 4 reports two numeric indicators of the contiguity and connectivity of the conserved forest lands from each group. Both indicators are formally defined in the notes below Table 4. The contiguity and connectivity indicators of the treated groups are both over 10% higher than the control groups, but the differences are statistically insignificant. In addition, recall that such results might be random as our experimental auctions only used one draw of each subject's eight bids. We thus resorted to recombination procedures to make full use of the information from our data. Panel 2 of Table 4 shows that if all possible combinations of bids are taken into account, the improvements in the contiguity and connectivity indicators would be rather limited in size (both below 5%), and still statistically insignificant. The group-level results somewhat contrast with student experiments, which typically find enhanced contiguity of conservation in the agglomeration bonus treatment (e.g. Parkhurst and Shogren 2008), although they are not strictly comparable to this study as they did not assess the treatment effect in auction settings. Fooks et al. (2016) also found higher levels of contiguous conservation in a conservation auction that provided agglomeration bonuses, but the statistical characteristics of this result was not reported.

Altogether, our experimental results (based on field data) find no discernible evidence regarding the agglomeration effect postulated by Hypothesis 2, either at the bid level or the group level. It is possible that the subjects did not thoroughly understand the connection between the amount of potential bonus payment and the number of neighbours. Indeed, we carefully explained the experiment to the subjects and assessed the robustness of our results by excluding bidders that clearly showed that they misperceived the protocol or were protesting. But these procedures may not suffice to completely rule out the impact of misperceptions and protests. For instance, we conducted a series of quantile regressions, which find that the estimates for the 0.75 and 0.9 quantiles notably deviate from our expectations, whereas those for the 0.25 and 0.5 quantiles are largely in line with our expectations. After dropping the highest 10% of bids (or using log transformed bids as a means to reduce the impact of extreme bids), we find that the agglomeration bonus indeed leads to lower bids for plots with more neighbours, which is in line with Hypothesis 2. Some bidders may have intentionally tendered extraordinarily high bids in protest since they were not allowed to opt-out (Lusk

[18] A less restrictive approach is to compare the main treatment effect estimates given by models estimated separately for plots with different numbers of neighbours. We have explored this approach and found qualitatively similar results.

and Shogren 2007).[19] Perhaps they were not completely identified by our protest diagnosis. But of course there could be other reasons underlying the different results given by this subsample and the full sample. For instance, if the majority of the subjects did understand the experiment, a subject in the agglomeration bonus treatment would bid lower for plots with one neighbour not only through the direct effect of the bonus but also indirectly through the expectation that others would further lower their bids for plots with three neighbours. This would lessen the effect of having more neighbours in the agglomeration bonus treatment.

In addition, the group level agglomeration effect is also affected by changes in other determinants of the offered bids, such as timber values. Table 4 shows that the coefficient of 'timber value' is positive and strongly significant in all the regression models. This implies that the subjects tend to bid higher for forest plots with higher timber values, which in turn decreases their likelihood of being selected. Therefore, when the agglomeration bonus comes into play, it is ambiguous whether forest plots with more neighbours *and higher* timber values are more likely to be selected relative to those with fewer neighbours *but lower* timber values. This counterbalancing effect has likely hindered contiguous conservation in our experiment. The implication is that, in real landscapes, the spatial correlation patterns of the opportunity costs of conservation take on additional importance in determining the conservation effectiveness of an auction with an agglomeration bonus.

5 Conclusion

Auction mechanisms and the agglomeration bonus have been promoted in the literature as two promising innovations of PES schemes. Few papers have tried to combine these design elements in an empirical setting (Krawczyk et al. 2016). In this paper, we test such a combination in a real life setting. Although the agglomeration bonus has attracted widespread interest, many unanswered questions remain about the factors which determine its cost-effectiveness and ecological impact (de Vries and Hanley 2016). So far, empirical evidence in this regard is almost exclusively derived from laboratory experiments using student subjects. Understanding of how such a bonus scheme would be communicated and understood by actual forest holders or farmers is very limited indeed. One way forward is to further test-bed this instrument in field settings. This study undertook perhaps the first framed field experiment to investigate the performance of the agglomeration bonus in a PES auction. Further, we chose a policy context, rural China, that has further reaching implications for the development of PES programmes, given the potential size of ecosystem service markets in China and the sheer number of people they are bound to affect.

We conducted the experimental sessions in rural areas of Sichuan province, China, and randomly recruited actual forest holders as subjects. Our empirical results provide a mixed picture of the cost-effectiveness and conservation efficacy of the agglomeration bonus in an auction setting. First, we find that bidders in the agglomeration bonus treatment generally bid less (irrespective of the number of neighbours), in anticipation of receiving the bonus

[19] Had an opt-out option been provided, the vast majority of our participants would have preferred not to bid so as to minimise the time and cognitive efforts required for decision making. The propensity to choose the status quo in decision making has been widely observed (Boxall et al. 2009) and is particularly dominating in our local contexts. Admittedly, the absence of an opt-out option has likely introduced noise, since many subjects may have intentionally tendered extraordinarily high bids (Lusk and Shogren 2007). Yet, allowing subjects not to bid would substantially undermine the viability of our experiment.

payment. But this finding is only statistically significant in a subsample that excludes 42 bidders (22% of the full sample) who exhibited evident misunderstanding of the experiment and/or protest attitudes. However, counterpart estimates from the full sample are statistically insignificant, and we should be cautious about generalising lessons drawn from a subsample which may not be representative of the targeted population. We find that group-level total bids are significantly lower (by over 28%) in the agglomeration bonus treatment. After taking into account the bonus payments, group-level total costs of the treated groups become similar to the control groups, but our data cannot statistically reject the possibility that the agglomeration bonus may incur nontrivial additional costs (e.g. by 10%).

Second, the agglomeration bonus is expected to induce lower bids for forest lands with more neighbours, since the expected bonus payment increases with the number of neighbours. These forest lands are thus more likely to be selected into the PES programme, which would promote contiguous conservation. We have no significant findings in this regard, either at the bid level (with or without anomalous bidders) or the group level, and therefore the jury is still out when considering findings using actual forest land users (i.e. non-student subjects) in an actual PES field setting. This is likely attributable to the highest 10% of bids, which may represent another type of protest as bidders were not allowed to opt out. Upon the removal of the highest 10% of bids, we find significant evidence in line with the hypothesised nexus between the agglomeration bonus' effect on bids and the number of neighbours. But the different findings derived from this subsample and the full sample may relate to factors other than protest bids. Additional research is warranted on whether spatially-targeted and/or spatially coordinated auctions deter potential participants due to the complexity of enrolment rules. We also noted the importance of considering the spatial correlation in opportunity costs of conservation (participation) across the landscape, since this spatial pattern will determine the additional environmental benefits which an agglomeration bonus can deliver.

The mixed findings from our lab-in-the-field experiment nevertheless convey some encouraging signals in favour of the joint use of PES auctions and the agglomeration bonus in the developing world. Further research is warranted in light of the policy significance of this innovative incentive mechanism. It is particularly pertinent in contexts where ambitious blanket conservation policies are often adopted on the basis of their apparent simplicity and low cost, even though incentive compatible policies are potentially more viable and effective. For example, recently the State Forestry Administration of China has announced plans to impose an outright ban (for an unspecified length of time) on commercial logging in *all* natural forests (as opposed to planted forests) during the country's 13th Five Year Period (2016-2020) (Forest Trends 2016; State Forestry Administration 2016). These natural forests account for 58% of the total area of the country's forests and 81% of the total standing volume. Villages and individuals own 45% of these natural forests, whereas the state (governmental bodies and state-owned enterprises, etc.) holds the remaining 55%. This would perhaps constitute the largest conservation restriction plan of its kind in the world. Under the scheme the government would only offer a nominal uniform compensation rate (CNY 15 per mu per year) to owners of communal or private natural forests (whereas the median bid in our experiment is CNY 350). The limited amount of compensation reflects the immense size of the scheme. As a substantial proportion of forest holders are effectively enrolled against their will, there is considerable uncertainty on the efficacy of this grandiose plan.

A PES-type scheme was proposed as an alternative for conserving the entirety of these communal and private natural forests, but was eventually abandoned due to being perceived as prohibitively expensive. Yet, there are problems in over-stretching a country's limited financial resources in an attempt to conserve *all* natural forests. This is likely to be an inferior policy option compared to concentrating conservation actions in priority areas, chosen on the basis of ecological significance and opportunity cost. In such a setting, a PES auction with an agglomeration bonus could provide a more efficient, cost effective and viable alternative to both a command-and-control logging ban approach and a uniform payment scheme. These potential benefits, however, come at the expense of a more complex design and implementation burden, and thus higher transactions costs (Banerjee et al 2017) and the expectation of reduced rates of participation (Rolfe et al 2018). This necessitates additional academic endeavours to further assess the viability of this PES feature for rural forest land owners in less developed regions with limited education and market experience. Our study provides a helpful start in this direction as—despite its inconclusive results—it serves as a useful 'proof of concept' for the potential use of the agglomeration bonus in an important policy context such as China.

Our study also served as a valuable 'stake-holder' engagement exercise in that it aimed to show Chinese policy makers how experimental methods can be used as a tool to design new policies. By applying experimental methods in the field with actual land owners (and not student subjects) we were able to curb the scepticism that is attached to pure laboratory studies and have helped pave the way for a wider acceptance of such methods in policy circles in China (such as the State Forestry Administration of China).

Many of the broader lessons we learned on conducting field experiments in China are similar to those we have encountered in other countries. For example, it is imperative to engage with stakeholders and policy-making agencies from the beginning of the design of the experimental protocol. It is also vital to take under account local ethnic and cultural specificities (in our cases we used local dialects in the protocol where needed, while we also employed local translators). It is also important to adapt experimental protocols so that the reliability of the experiment is enhanced in the eyes of the participants (for example, in our case we used the more cumbersome and costly approach of postal orders and bank transfers to make future payments as opposed to the more convenient but less trustworthy option of leaving payments to be distributed by local authorities). Yet, our work also allowed us to draw some more idiosyncratic lessons specifically for the case of rural China. Firstly, policy-making agencies appear to be more centralised compared to other regions of the world that we have worked in (e.g. Africa, South East Asia, Latin America) while at all the same time non-governmental agencies seem to have less leeway to operate within. This somewhat curtails the prospects and flexibility of conducting scientifically robust and independent large-scale randomised controlled field studies. Yet, on the other hand, our experience suggests that there is very strong interest from Chinese policy makers in evidence-based and results oriented policy-making approaches. There is also a strong preference on their behalf for methods that have a high degree of realism and which rely less on purely theoretical predictions or on empirical methods that are either based on contrived samples (such as student samples) or statistical methods that evoke restrictive (and often untestable) assumptions (such as quasi-experimental methods). Hence, we found that there is significant appetite for experimental studies that are more directly related to specific policy settings and challenges and which include the actual people that would be affected by a potential policy change.

Secondly, although the influence of policymakers in China may appear to limit the flexibility and independence of conducting large scale behavioural experimental studies, our experience suggests that can have a positive side for pursing better, more targeted experimental protocols. We found that collaborating with government agencies and stakeholders ensured higher participation rates and greater local community cooperation compared to most other countries we have worked in. This could also be attributed to local cultural and social reasons. It could however ensure that randomisation processes and samples of participants are of very high quality. Also, the puissance of policy makers in China may actually augment the scope for exploring more intricate and innovate experimental hypotheses and designs than perhaps researchers could consider elsewhere (whilst remaining within ethical experimental boundaries). Taken together, our experience from this study suggests that there is significant potential for future policy oriented experimental field studies in China.

Acknowledgements The authors gratefully acknowledge the very helpful comments from delegates at the 6th World Congress of Environmental and Resource Economists (June 25–29, 2018, Gothenburg, Sweden), the Conference on Behavioral & Experimental Agri-Environmental Research (October 14–15, 2017, Shepherdstown, USA) and the 18th BIOECON Conference (September 14–16, 2016 Cambridge, UK). We would also like to particularly thank for the comments provided by Prof Christian Vossler, Prof Paul Ferraro, Dr Leah Palm-Forster, Prof Heidi J. Albers and two anonymous reviewers of the Special Issue 'Behavioral & Experimental Agri-Environmental Research' to be published in Environmental & Resource Economics. Further, the authors are grateful for the financial support from the British Academy (Reference: SG121920) and the Environment for Development Initiative. Last but not least, the authors would like to express sincere appreciation to the State Forestry Administration of China, Peking University and Sichuan University, for their engagement and assistance in the design and implementation of this study.

Appendix

See Table 5

Table 5 Comparison of means between anomalous and non-anomalous bidders

Variable	Non-anomalous bidders		Anomalous bidders		Mean difference	*p*-value
	Mean	SD	Mean	SD		
Panel 1: Bid (Bidder-level averages)	*Obs. = 150*		*Obs. = 42*			
Bid	***883.10***	***1600.19***	***2205.18***	***4625.70***	***−1322.08******	***0.00***
Bid 1 neighbour	***875.85***	***1488.29***	***2168.87***	***4585.79***	***−1293.02******	***0.00***
Bid 3 neighbours	***890.35***	***1739.63***	***2241.49***	***4683.98***	***−1351.14******	***0.00***
Panel 2: Covariates (Bidder-level)	*Obs. = 150*		*Obs. = 42*			
Timber value drawn	17.10	5.56	18.45	5.58	−1.35	0.17
Exercise performance	0.87	0.19	0.90	0.13	−0.03	0.28
Age	47.44	15.17	48.57	13.83	−1.13	0.66
Gender	0.67	0.47	0.57	0.50	0.10	0.22
Ethnic minority	0.77	0.42	0.81	0.40	−0.04	0.56
Education	5.54	3.74	5.90	3.46	−0.36	0.57
Household head	0.61	0.49	0.55	0.50	0.06	0.45
CCP member	0.20	0.40	0.29	0.46	−0.09	0.24
Village leader	0.29	0.46	0.29	0.46	0.01	0.92
Children	***0.70***	***0.79***	***1.00***	***0.90***	***−0.30*****	***0.04***
Elderly	0.81	0.85	0.71	0.74	0.10	0.49
Students	0.75	0.87	0.86	0.78	−0.11	0.48

Table 5 (continued)

Variable	Non-anomalous bidders		Anomalous bidders		Mean difference	*p*-value
	Mean	SD	Mean	SD		
Savings	***0.40***	***0.49***	***0.26***	***0.45***	***0.14***	***0.10***
Debt	0.30	0.46	0.33	0.48	−0.03	0.70
Market experience	0.66	0.48	0.62	0.49	0.04	0.62
Land dispossession	0.06	0.24	0.02	0.15	0.04	0.35
Time preference dummy 1	0.23	0.42	0.21	0.42	0.02	0.80
Time preference dummy 2	0.21	0.41	0.12	0.33	0.09	0.20
Time preference dummy 3	0.27	0.44	0.29	0.46	−0.02	0.81
Time preference dummy 4	0.29	0.46	0.38	0.49	−0.09	0.28
Risk preference dummy 1	0.34	0.48	0.36	0.48	−0.02	0.84
Risk preference dummy 2	0.17	0.37	0.19	0.40	−0.02	0.72
Risk preference dummy 3	0.49	0.50	0.45	0.50	0.04	0.64
Trust in future payments	0.87	0.33	0.79	0.42	0.08	0.16
Environmental awareness	0.98	0.14	1.00	0.00	−0.02	0.36
Cognitive skills	0.77	0.21	0.74	0.16	0.03	0.50
Joint covariate balance (*p*-value)	0.40					

Significant results are highlighted in bold italics (up to the 10% significance level)

Asterisks indicate statistical significance: *p-value < 0.10, **p-value < 0.05, ***p-value < 0.01

References

Angrist JD, Pischke J (2009) Mostly harmless econometrics: an empiricist's companion. Princeton University Press, Woodstock

Arguedas C, van Soest DP (2011) Optimal conservation programs, asymmetric information and the role of fixed costs. Environ Resour Econ 50(2):305–323

Banerjee S (2018) Improving spatial coordination rates under the agglomeration bonus scheme: a laboratory experiment with a pecuniary and a non-pecuniary mechanism (nudge). Am J Agric Econ 100(1):172–197

Banerjee S, Kwasnica AM, Shortle JS (2012) Agglomeration bonus in small and large local networks: a laboratory examination of spatial coordination. Ecol Econ 84(December):142–152

Banerjee S, de Vries FP, Hanley N, van Soest DP (2014) The impact of information provision on agglomeration bonus performance: an experimental study on local networks. Am J Agric Econ 96(4):1009–1029

Banerjee S, Kwasnica AM, Shortle JS (2015) Information and auction performance: a laboratory study of conservation auctions for spatially contiguous land management. Environ Resour Econ 61(3):409–431

Banerjee S, Cason TN, de Vries FP, Hanley N (2017) Transaction costs, communication and spatial coordination in payment for ecosystem services schemes. J Environ Econ Manag 83(May):68–89

Baylis K, Honey-Rosés J, Börner J, Corbera E, Ezzine-de-Blas D, Ferraro PJ, Wunder S (2015) Mainstreaming impact evaluation in nature conservation. Conserv Lett 9(1):58–64

Boxall P, Adamowicz WL, Moon A (2009) Complexity in choice experiments: choice of the status quo alternative and implications for welfare measurement. Aust J Agric Resour Econ 53(4):503–519

Brandts J, Charness G (2011) The strategy versus the direct-response method: a first survey of experimental comparisons. Exp Econ 14(3):375–398

Carlsson F, He H, Martinsson P, Qin P, Sutter M (2012) Household decision making in rural China: using experiments to estimate the influences of spouses. J Econ Behav Organ 84(2):525–536

Cason TN, Wu SY (2017) Subject pools and deception in agricultural and resource economics experiments. Retrieved from http://www.krannert.purdue.edu/faculty/cason/papers/Sub_pools_deception.pdf

Davis DD, Holt CA (1993) Experimental economics. Princeton University Press, Princeton

de Vries FP, Hanley N (2016) Incentive-based policy design for pollution control and biodiversity conservation: a review. Environ Resour Econ 63(4):687–702
Deck C, Jahedi S (2015) An experimental investigation of time discounting in strategic settings. J Behav Exp Econ 54:95–104
Drechsler M, Smith HG, Sturm A, Wätzold F (2016) Cost-effectiveness of conservation payment schemes for species with different range sizes. Conserv Biol 30(4):894–899
Drichoutis AC, Lusk JL, Nayga RM (2015) The veil of experimental currency units in second price auctions. J Econ Sci Assoc 1(2):182–196
Ferraro PJ (2008) Asymmetric information and contract design for payments for environmental services. Ecol Econ 65(4):810–821
Ferraro PJ, Pattanayak SK (2006) Money for nothing? A call for empirical evaluation of biodiversity conservation investments. PLoS Biol 4(4):482–488
Ferré M, Engel S, Gsottbauer E (2016) Can agglomeration payments induce sustainable management of peat soils in Switzerland?—A computerized framed experiment. Retrieved from: http://www.bioecon-network.org/pages/18th_2016/Ferre.pdf
Fooks JR, Higgins N, Messer KD, Duke JM, Hellerstein D, Lynch L (2016) Conserving spatially explicit benefits in ecosystem service markets: experimental tests of network bonuses and spatial targeting. Am J Agric Econ 98(2):468–488
Forest Trends (2016) China's logging ban in natural forests: impacts of extended policy at home and abroad. Retrieved from: http://www.forest-trends.org/documents/files/doc_5145.pdf
Fréchette GR (2016) Experimental economics across subject populations. In: Kagel JH, Roth AE (eds) The handbook of experimental economics. Princeton University Press, Princeton
Fudenberg D, Levine DK (2006) A dual-self model of impulse control. Am Econ Rev 96(5):1449–1476
Groom B, Grosjean P, Kontoleon A, Swanson T, Zhang S (2010) Relaxing rural constraints: a 'win-win' policy for poverty and environment in China? Oxf Econ Pap 62(1):132–156
Hanley N, Banerjee S, Lennox GD, Armsworth PR (2012) How should we incentivize private landowners to 'produce' more biodiversity? Oxf Rev Econ Policy 28(1):93–113
Harrison J, McKay R (2012) Delay discounting rates are temporally stable in an equivalent present value procedure using theoretical and area under the curve analyses. Psychol Rec 62(2):307–320
Harrison GW, Lau MI, Williams MB (2002) Estimating individual discount rates in Denmark: a field experiment. Am Econ Rev 92(5):1606–1617
Harrison GW, Harstad RM, Rutström EE (2004) Experimental methods and elicitation of values. Exp Econ 7(2):123–140
Imai K, King G, Stuart EA (2008) Misunderstandings between experimentalists and observationalists about causal inference. J R Stat Soc Ser A (Stat Soc) 171(2):481–502
Jack BK (2013) Private information and the allocation of land use subsidies in Malawi. Am Econ J Appl Econ 5(3):113–135
King G, Nielsen R, Coberley C, Pope JE, Wells A (2011) Avoiding randomization failure in program evaluation, with application to the medicare health support program. Popul Health Manag 14(S1):S-11–S-22
Kirchkamp O, Reiß JP (2011) Out-of-equilibrium bids in first-price auctions: wrong expectations or wrong bids. Econ J 121(557):1361–1397
Kirchkamp O, Poen E, Reiß JP (2009) Outside options: another reason to choose the first-price auction. Eur Econ Rev 53(2):153–169
Krämer JE, Wätzold F (2018) The agglomeration bonus in practice—an exploratory assessment of the Swiss network bonus. J Nat Conserv 43:126–135
Krawczyk M, Bartczak A, Hanley N, Stenger A (2016) Buying spatially-coordinated ecosystem services: an experiment on the role of auction format and communication. Ecol Econ 124(April):36–48
Kuhfuss L, Préget R, Thoyer S, Hanley N (2015) Nudging farmers to enrol land into agri-environmental schemes: the role of a collective bonus. Eur Rev Agric Econ 43:609–636
Latacz-Lohmann U, Van der Hamsvoort C (1997) Auctioning conservation contracts: a theoretical analysis and an application. Am J Agric Econ 79(2):407–418
List JA (2003) Does market experience eliminate market anomalies? Q J Econ 118(1):41–71
List JA (2011) Why economists should conduct field experiments and 14 tips for pulling one off. J Econ Perspect 25(3):3–15
Liu Z, Gong Y, Kontoleon A (2018) How do payments for environmental services affect land tenure? Theory and evidence from China. Ecol Econ 144:195–213
Lusk JL, Shogren JF (2007) Experimental auctions: Methods and applications in economic and marketing research. Cambridge University Press, Cambridge

Manzini P, Mariotti M (2014) A case of framing effects: the elicitation of time preferences. Retrieved from: http://www.st-andrews.ac.uk/~wwwecon/repecfiles/4/1405.pdf
Margolis M, Shogren JF (2004) Implementing the efficient auction: initial results from the lab. Econ Lett 84(1):141–147
Margules CR, Pressey RL (2000) Systematic conservation planning. Nature 405(6783):243–253
Messer KD, Ferraro P, Allen W (2015) Behavioral nudges in competitive environments: a field experiment examining defaults and social comparisons in a conservation contract auction. Retrieved from: http://www.bioecon-network.org/pages/17th_2015/Messer.pdf
Parkhurst GM, Shogren JF (2007) Spatial incentives to coordinate contiguous habitat. Ecol Econ 64(2):344–355
Parkhurst GM, Shogren JF (2008) Smart subsidies for conservation. Am J Agric Econ 90(5):1192–1200
Parkhurst GM, Shogren JF, Bastian C, Kivi P, Donner J, Smith RBW (2002) Agglomeration bonus: an incentive mechanism to reunite fragmented habitat for biodiversity conservation. Ecol Econ 41(2):305–328
Reeson AF, Rodriguez LC, Whitten SM, Williams K, Nolles K, Windle J, Rolfe J (2011) Adapting auctions for the provision of ecosystem services at the landscape scale. Ecol Econ 70(9):1621–1627
Rolfe J, Schilizzi S, Boxall P, Latacz-Lohmann U, Iftekhar S, Star M, O'Connor P (2018) Identifying the causes of low participation rates in conservation tenders. Int Rev Environ Resour Econ 12:1–45
Schilizzi S, Latacz-Lohmann U (2007) Assessing the performance of conservation auctions: an experimental study. Land Econ 83(4):497–515
Shafir E, Diamond P, Tversky A (1997) Money illusion. Q J Econ 112(2):341–374
State Forestry Administration (2016) The 13th five year plan for Forestry (in Chinese). Retrieved from: http://www.forestry.gov.cn/uploadfile/main/2016-5/file/2016-5-19-4e0699f79b4b4a2ab03843684dd32c76.pdf
Stubbs M (2014) Conservation reserve program (CRP): status and issues. Retrieved from: http://nationalaglawcenter.org/wp-content/uploads/assets/crs/R42783.pdf
Suter JF, Vossler CA (2014) Towards an understanding of the performance of ambient tax mechanisms in the field: evidence from upstate New York dairy farmers. Am J Agric Econ 96(1):92–107
USDA (1998) Agreement between the USDA commodity credit corporation and the state of oregon and clean water services of the County of Washington State of Oregon: concerning the implementation of a conservation reserve enhancement program. Retrieved from: http://www.fsa.usda.gov/Assets/USDA-FSA-Public/usdafiles/Conservation/PDF/orcrepagmnt120904.pdf
Wang X, Bennett J, Xu J, Zhang H (2012) An auction scheme for land use change in Sichuan Province, China. J Environ Plann Manag 55(10):1269–1288
Warziniack T, Shogren JF, Parkhurst G (2007) Creating contiguous forest habitat: an experimental examination on incentives and communication. J For Econ 13(2–3):191–207
Whitten SM, Reeson A, Windle J, Rolfe J (2013) Designing conservation tenders to support landholder participation: a framework and case study assessment. Ecosyst Serv 6(December):82–92
Williams KJ, Reeson AF, Drielsma MJ, Love J (2012) Optimised whole-landscape ecological metrics for effective delivery of connectivity-focused conservation incentive payments. Ecol Econ 81(September):48–59
Xu J, Tao R, Xu Z, Bennett MT (2010a) China's Sloping Land Conversion Program: does expansion equal success? Land Econ 86(2):219–244
Xu J, White A, Lele U (2010b) China's forest tenure reforms: impacts and implications for choice, conservation, and climate change. The Rights and Resources Initiative, Washington