Performance of conservation auctions: Does the pre-existing institution matter?

Abel-Gautier Kouakou
Institute of Environmental Systems Science, Osnabrück University, Germany

Elisabeth Gsottbauer
Institute of Public Finance, Innsbruck University, Austria

Stefanie Engel
Institute of Environmental Systems Science, Osnabrück University, Germany

Abstract

This paper presents experimental evidence that the performance of institutions governing conservation outcomes depend on the order in which these institutions are introduced. We study a conservation setting where conservation contracts to landowners can be distributed by different allocation mechanisms: a fixed-price scheme or a conservation auction. Our data shows that subjects who had experienced a fixed-price scheme before a conservation auction is in place adjust their bids in the auction according to the observed fixed-price level. This in turn hampers the budgetary cost-effectiveness of conservation auctions when compared to auctions without a pre-existing institution in place. Multiple round bidding cannot attenuate this behavioral bias. However, these effects are significant only in a setting where the fixed price is high. Moreover, despite the behavioral bias induced by the pre-existing institution, the auction still performs better than a fixed-price scheme.

Keywords: Payments for ecosystem services, conservation auction, fixed-price scheme, information rent, budgetary cost-effectiveness, reference-dependence, behavioural and experimental economics.

1. Introduction

Conservation auctions have become an attractive way of allocating conservation contracts to farmers (Jack, 2013; Jindal, Kerr, Ferraro, & Swallow, 2013; Rolfe, Windle, & Mccosker, 2009; Stoneham, Chaudhri, Ha, & Strappazzon, 2003; Ulber, Klimek, Steinmann, Isselstein, & Groth, 2011). They are a form of payments for environmental services (PES) (Engel, Pagiola, & Wunder, 2008). One of the main advantages of conservation auctions over more frequently used fixed-price schemes to allocate conservation contracts is that auctions are able to deal with the problem of asymmetric information[1] arising between the conservation agency and farmers/landholders (Ferraro, 2008; Latacz-Lohmann & Van der Hamsvoort, 1997) and ultimately can enhance cost-effectiveness of conservation programs (Jack, 2013; Schilizzi & Latacz-Lohmann, 2007, Windle & Rolfe, 2008). However, others have also explored potential pitfalls in the design of conservation auctions, such that conservation auctions may lose their competitive edge in case of repetition (Schilizzi & Latacz-Lohmann, 2007; Windle & Rolfe, 2008). Furthermore, Lundberg et al. (2018) argue that the relative effectiveness compared to fixed-price schemes depends largely on the context in which auctions are implemented including variability in the correlation between ecosystem services and landscape characteristics, cost heterogeneity and available budget size of the program. In this paper, we study whether the previous implementation of a fixed-price scheme may impact the cost-effectiveness of a conservation auction, that is whether a kind of path-dependence in provision of conservation activities by landowners exists.

Many existing large-scale conservation programs such as the ones targeting tropical deforestation in Costa Rica and Mexico as well as agri-environmental programs in Europe traditionally use fixed-price contracts for contract allocation (Wunder, Engel, & Pagiola, 2008). As auctions are increasingly considered an efficient allocation mechanism for conservation contracts to private landowners, programs in these countries may seek to implement procurement auctions to tender payments for ecosystem service contracts. A number of scientific studies have argued for such a change by demonstrating potential gains in cost effectiveness (Wünscher, Engel, & Wunder, 2008 for Costa Rica; Alix-Garcia, de Janvry & Sadoulet, 2008 for Mexico; Armsworth et al., 2012 for the UK). We experimentally examine whether the existence and level of a pre-existing fixed-price scheme continues to influence bids in conservation settings governed by a procurement auction, despite being no longer decisive

[1] The problem of asymmetric information describes the case in which a conservation agency know less than farmers or landowners as farmer's opportunity cost of participating in a conservation programs is private information.

for an individual's decision problem. We hypothesize that a pre-existing environmental service price may affect the benchmark against which participants perceive and evaluate conservation auctions, that is, a participant may use such prices as reference points.

Reference points have been found to adversely affect many economic behaviours including, amongst others, the declaration of taxes (Bruttel & Friehe, 2014), trust and cooperation (Bohnet and Huck, 2004; Bruttel and Friehe, 2011), valuation of housing prices (Einiö, Kaustia, & Puttonen, 2008; Genesove & Mayer, 2001), and the time investors hold on to stocks (Weber & Camerer, 1998). Yet, others have shown that repeated decision-settings and market competition may eliminate behavioral anomalies such as the impact of non-conscious reference points (e.g. Braga, Humphrey, & Starmer, 2009; Dijk, van Soest, & Ansink, 2013; Knetsch, Tang, & Thaler, 2001 List, 2003; Loomes, Starmer, & Sugden, 2003; Tufano, 2010). Translated to our setting, such reference-dependent or path-dependent[2] behaviour may affect farmers' bids and thus possibly compromise the performance of conservation auctions and ultimately even eliminate the relative cost-effectiveness advantage of the auction.

To answer the question how conservation auction performance is affected by the previous existence of a fixed-price scheme, we conduct a controlled laboratory experiment with 180 university students in Osnabrück (Germany), which provides us the opportunity to exogenously vary the existence and level of such a fixed-price scheme and assess its impact on the economic performance of conservation auctions. In our induced-value[3] and decontextualized experiment we exogenously vary the pre-existing environmental service price, using the midpoint and the upper bound of the distribution of opportunity costs as realistic benchmarks for possible policy price settings (Wunder, Engel, & Pagiola, 2008). In addition, we implement a baseline (control) treatment in which no such previous service price exists.

Our results indicate that the performance of conservation auctions depend on the level of the pre-existing environmental service price. When the price in a pre-existing fixed-price scheme is high, participants' average bids increase and the budgetary effectiveness of the conservation auction decreases. This effect is not attenuated by repeating the auction. However,

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[2] Reference-dependence means that people evaluate outcomes relative to reference points, while path dependence means that people's decisions depend on previous decisions made. Here we use the terms 'reference-dependent' and 'path-dependent' interchangeably and thus do not enter into a debate about semantic differences.

[3] In an induced-value experiment, experimental subjects are given pre-assigned values for a fictitious good. In our setting, this means that participants are pre-assigned opportunity cost for a parcel of land. This method stands in contrast to other experimental markets for real goods in which participants are readily assigned field goods such as coffee mugs.

these effects are not statistically significant for the case where the fixed-price scheme was based on average opportunity costs. Our paper thus provides evidence that the performance of conservation auctions can depend on the institutional context in which the auction is being implemented, and we recommend that policy makers account for this when designing conservation auctions.

The remainder of the paper is organised as follows. Section 2 summarises related literature. Section 3 presents the experimental design. Section 4 details our research hypotheses. Section 5 presents the results. Finally, in section 6, we discuss our findings and conclude the paper.

2. A review of related literature

Our paper explores the potential of reference- or path-dependent behavior in conservation auctions. A number of behavioral economic studies in other market and trade settings have analyzed related issues. For example, Genesove & Mayer (2001) and Einiö et al. (2008) find sellers' behavior in the housing/apartment market to be heavily influenced by the previous nominal purchase price which sellers seem to use as reference point. Both studies indicate that the purchase price of the old apartment heavily influences decisions to move and acquire new apartments. Findings are explained according to Prospect Theory (Kahneman and Tversky, 1979), which stipulates that loss-averse agents might consider the original purchase price to be a reference point in their value function. Another example includes research indicating that reference points can affect investors' choices of selling and keeping stocks (Grinblatt & Keloharju, 2001; Odean, 1998; Weber & Camerer, 1998). In particular, many investors have been found to hold on to stock market losers too long in their quest to avoid losses. There is also a broader literature in experimental auction valuation that shows that endowing auction participants with a good significantly influences their valuation of that good (e.g. Kahneman and Tversky, 1979; Horowitz and McConnell, 2002). This phenomenon is referred to as the endowment effect (Kahneman et al., 1991) and usually explained by Prospect Theory and loss aversion. With respect to conservation auctions, Dijk et al. (2018) confirm the existence of an endowment effect in a laboratory procurement auction.

The aforementioned studies examine the possibility of reference points impacting economic decision-making and bidding behavior. As we also plan to investigate the effect of repeated market trials or a repeated-round auction, we also consider evidence for the mitigation of behavioral anomalies via repeated market exposure (Braga et al., 2009; Corrigan et al., 2011; Cox & Grether, 1996; Knetsch et al., 2001; List & Shogren, 1999; Loomes et al., 2003; Plott, 1996; Dijk et al., 2018). In fact, most of these studies test two hypotheses: the price following hypothesis (also called the shaping hypothesis), which refers to the fact that agents adjust their bids towards the price observed in a previous market period (Loomes et al., 2003), and the market experience hypothesis (also called the market discipline hypothesis or the refining hypothesis), which refers to the fact that market experience over time may induce individuals to reveal or understand their real preferences and eliminate anomalies over time (Loomes et al., 2003; Plott, 1996). Results are mixed. While some studies find evidence in favor of the price following hypothesis (Corrigan & Rousu, 2006; List & Shogren, 1999) or the market experience hypothesis (Cox & Grether, 1996; Loomes et al., 2003), others do not (Braga et al., 2009; Knetsch et al., 2001). In the context of conservation auctions, Dijk et al. (2018) find support for the market experience hypothesis, showing that repetition is able to attenuate the endowment effect, i.e. the effect of initial endowment and its effect on auction results. In contrast to our experiment, van Dijk et al. (2018) investigates the ownership of a good, in their case, subjects are able to participate in an auction and sell an item they just have been endowed with. In our experiment, we are interested in the phenomenon if the pure possibility of obtaining a contract via the participation in a fixed-price scheme in a previous period has an impact on auction efficiency later on. Finally, our paper links to a broader literature on using laboratory and field experiments to study conservation auction design questions and their effect on auction performance. Most of the literature has focused on a direct comparison of conservation auctions to fixed price schemes (Schilizzi & Latacz-Lohmann, 2007; Windle & Rolfe, 2008; Stohneman et al., 2003). Others have focused on studying more precisely the effect of information provision and entry conditions on the performance of auctions (Cason et al., 2003; Fooks, Messer, & Duke, 2015). We are, to our knowledge, the first study to examine the impact of a pre-existing fixed price scheme on participant's bids and auction performance.

3. Experiment

We first describe our general conservation set-up and explain our experimental design and treatments. We also provide details on the implementation of the experiment. Appendix C contains a translated version of the experimental instructions.

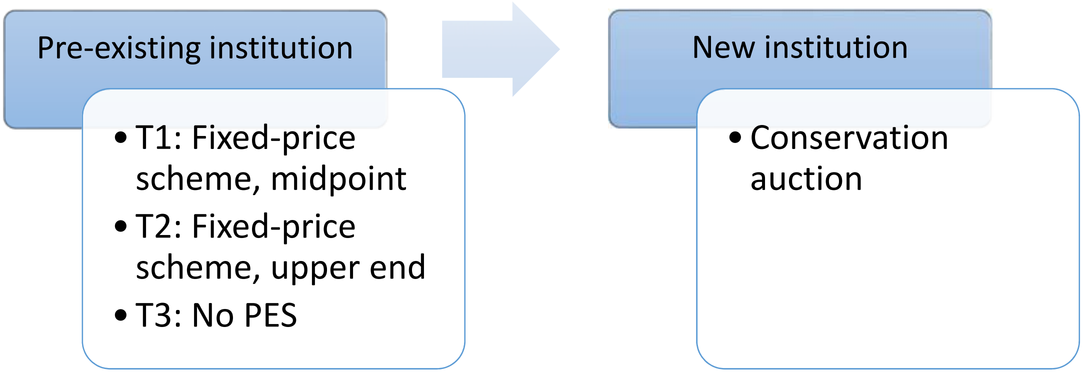
3.1 The conservation setup

As our research is motivated by the use of auctions to allocate PES contracts, we elaborate our conservation setup using a typical PES framing. Note, however, that all experiments were conducted in a context-free environment, using neutral language without mentioning PES, conservation or ecosystem service terminology.

We consider a representative agent (or farmer), who runs a conventional activity under the status quo state and obtains a profit π_0. The conventional activity may be replaced with an environmentally friendly one yielding a lower profit π_1, with $\pi_0 > \pi_1$. In other words, the representative agent's opportunity cost of replacing the conventional activity with the environmentally friendly one is: $\pi_0 - \pi_1$. In order to promote the environmentally friendly activity, an implementing agency can incentivize agents to adopt the environmentally friendly activity through a conservation payment, which we refer to, for the purpose of this paper, as "PES". The implementing agency can allocate such payments either via a fixed price scheme or via a discriminatory auction[4]. Please note that both schemes are budget constrained. Here we focus on a case of ex-ante equivalence. This means both allocation mechanisms have the same available budget. To be able to study the impact of the sequencing of institutions on the performance of conservation auctions, we implement the fixed-price scheme and the discriminatory auction sequentially. That means, we have a pre-existing fixed-price scheme before the discriminatory auction, as can be seen in Figure 1. We also consider two levels of a possible pre-existing price: the upper bound and the midpoint of the distribution of opportunity costs. We do so because we assume that policymakers or the implementing conservation agency will have some information about the upper bound and the midpoint of the distribution of opportunity costs, and will use it as starting point to set the payment level, which is in line with evidence on existing PES (Wunder, Engel, & Pagiola, 2008). In addition, we include a control treatment in which no pre-existing environmental service price exists.

[4] Note that a discriminatory auction is analogous to a first-price sealed-bid auction. We select winners from the lowest to the highest until the budget is exhausted, while winners are paid their exact bids.

Figure 1: Sequential implementation of different PES allocation mechanisms



3.2 Experimental design and treatments

The main aim of the experiment is to answer whether a pre-existing fixed price scheme affects participants bids and hence the performance in a subsequent conservation auction. To establish a clean causal relationship, we also aim to control for a range of participant characteristics and preferences measurements which have been shown in the past to affect participant bidding in auction environments (Isaac & James, 2000; Latacz-Lohmann & Van der Hamsvoort, 1997; Van den Bos, Talwar, & McClure, 2013). Therefore, and before the start of the main experiment, we collect incentivized measures for individual risk and social preferences. In order to measure risk preferences we use an adapted setup of the certainty equivalence test (CET) (see, for example, Dohmen & Falk, 2011). For an individual-level assessment of social preferences, we make use of the equality equivalence test (EET) developed by Kerschbamer (2015). In short, the EET consists of a set of mini dictator games in which participants need to make trade-offs between their own and someone else's welfare. Test results of the EET allow to classify subjects into major social preference types including efficiency-minded, inequality-averse, inequality-loving and spiteful. To collect information on a range of demographic characteristics including gender and study major, participants need to fill out a short exit survey at the end of each experimental session. In addition, subjects were asked to indicate their fairness perception of the presented auction mechanism and also completed a personality type test in the form of the Big Five (John, Donahue, & Kentle, 1991; John, Naumann, & Soto, 2008).

The main experiment consists of 2 stages. Before it starts all subjects were assigned to groups of 10 participants in order to keep group size constant across stages and experimental sessions. We applied an induced-value set-up, meaning that each subject was randomly assigned a different opportunity cost in the form of an individual 'Chip' cost. Note again that we performed a decontextualized experiment, i.e., the instructions did not contain any terminology related to conservation or opportunity cost of conservation. In our setting an individual Chip stands for a parcel of land and an individual chip cost represents the corresponding opportunity cost of conservation. Every participant was assigned one Chip and its according Chip cost. Chip costs were uniformly drawn from an interval between 10 and 300 and subjects were told that chip costs were spread uniformly along the given interval.[5]

Table 1 summarizes our 2-stage experimental design and treatments. Stage 1 includes the provision of conservation contracts using a fixed-price scheme which allocates contracts randomly (i.e. via a lottery) to participants, and stage 2 is a subsequent procurement auction in which participants are asked to submit their bids to be able to receive one of the contracts. In our between-subject design, our treatments vary the existence and price level of the fixed-price environment in stage 1 including (i) a control condition with no previous-fixed price scheme, (ii) an upper-bound condition in which the stage-1 price-level of the fixed price scheme is set at 300 (i.e. the upper bound of the distribution of Chip costs), and (iii) a midpoint condition in which the price-level of the stage 1 fixed-price scheme is set at 155. In our set-up we use a total conservation budget of 1200 points.[6] Under the fixed price scheme, subjects are randomly selected until the budget is exhausted. Subjects get a positive payoff (the difference between the fixed price and their Chip costs) if they are selected. Otherwise they get nothing. Under the upper bound condition, all subjects are eligible, whereas under the midpoint condition, only subjects who have a Chip cost below or equal to the fixed price are eligible. Note that after completion of stage 1 and before the start of stage 2, all participants are informed about the outcomes of stage 1 including whether they were randomly selected or not. Note also that stage 1 does not involve an active decision of the participants. Subjects are informed about their Chip costs, the budget size, whether they are randomly selected and their payoffs. They do not know the other members of their groups.

Stage 2 simulates the provision of conservation contracts using a conservation auction. In the design of the conservation auction we closely follow other set-ups used in previous experiments examining conservation auction design issues (Schilizzi & Latacz-Lohmann, 2007). Subjects were asked to offer their Chips to the buyer via submitting claims (or bids). They were also told that not all of them would be able to sell their Chips to the buyer as claims could only be compensated until the budget was exhausted. Subjects were informed about the

[5] This means, any number between 10 and 300 has an equal chance of being drawn from the interval and that all members of the same group have the same probability to obtain a specific Chip cost.
[6] This means we can keep a similar degree of competition as in the original paper of Schilizzi & Latacz-Lohmann (2007).

budget size. Subjects were also informed that claims would be ranked from lowest to highest and then selected starting from the lowest claim until the budget was exhausted. This meant that in our settings, subjects were competing with the other 9 participants in their group in order to get their claim accepted. If their claim was accepted, they were paid the difference between their claim and their individual Chip cost. This auction set-up was repeated for 10 rounds. All participants were informed about the outcome of each round before the next round, specifically about whether they got selected to obtain a conservation contract and their payoffs.

Table 1: Experimental stages and treatments

Treatment / Stage	**Stage-1**	**Stage-2**
Control (N=60)	No PES	Conservation auction
Upper-Bound (N=60)	Fixed-price = 300	Conservation auction
Mid-Point (N=60)	Fixed-price = 155	Conservation auction

Note: N is the number of participants. In each treatment, we have 6 groups of 10 participants.

3.3. Procedural details

The experimental sessions took place at the laboratory for economics research (LaER), University of Osnabrück, Germany, using SoPHIE (Hendriks, 2012) as software platform and university students as subjects. We ran 9 sessions with 20 subjects in each session, which implies a total of 180 subjects (18 groups of 10 subjects). Only one randomly selected part, including the preference experiments, was relevant for cash payments at the end of the experiment. A typical session lasted about one hour and the average cash payment amounted to 10.22 Euros.

4. Hypotheses

We formulate our research hypotheses, based on our literature review (see Section 2) on reference- and path-dependent behavior and the impact of repeated trials on bidding in auctions.

First, evidence on the impact of reference points in price (e.g., Genesove & Mayer, 2001) and market settings (e.g., List & Shogren, 1999) in which price bids follow the price observed in a previous market period, suggests that subjects may adjust their bids in the conservation auctions according to the price-level observed in the fixed-price scheme. Also, evidence on repeated market and auction trials posit, that multiple bidding rounds may attenuate the effect of subjects adjusting their bids according to the price-level observed in the fixed-price scheme.

H1.1 ('reference point')*: If there is a pre-existing fixed-price scheme, subjects use the pre-existing price as reference point for bidding in the conservation auction.*

H1.2 ('market experience')*: Multiple bidding rounds attenuate such adjustments in bidding over time.*

Second, according to loss aversion (Camerer & Loewenstein, 2004; Kahneman & Tversky, 1979; Tversky & Kahneman, 1991) which refers to the fact that people value more losses than gains, we expect that, in our conservation auction setting, subjects who were selected by the pre-existing fixed-price scheme will collect more information rents (i.e. the difference between the selected bid and subject's opportunity cost) than those who were not selected by the fixed-price scheme. In fact, subjects who were selected to participate in the fixed-price scheme, obtained a contract and an information rent in stage 1.[7] We hypothesize that due to loss aversion, subjects may try to conserve their obtained information rent, although it may decrease their chance of getting a new contract in the conservation auction.

H2 ('loss aversion'): *Subjects who have been selected by the pre-existing fixed-price scheme get more information rents under the auction than those who were not selected by the fixed-price scheme.*

Third, according to the reference point effect (Genesove & Mayer, 2001; Loomes et al., 2003) and the loss aversion hypothesis (Camerer & Loewenstein, 2004; Kahneman & Tversky, 1979; Tversky & Kahneman, 1991), we expect that conservation auctions are less efficient when there is a pre-existing fixed-price scheme. In evaluating the efficiency of conservation auctions, three criteria are used: the number of contracts awarded, the information rents obtained and the budgetary cost-effectiveness.

H3.1 ('performance I'): *When there is a pre-existing fixed-price scheme, conservation auctions are less efficient than when there is no pre-existing fixed-price scheme.*

H3.2 ('performance II'): *Repeated-round conservation auctions do not eliminate behavioral anomalies, meaning that conservation auctions remain less efficient when they are preceded by a fixed-price scheme.*

[7] Except in the case where chip cost just equalled the fixed price.

5. Results

We first present descriptive statistics on subject's behavior in the different parts of the experiment (section 5.1). We then test our research hypotheses (sections 5.2 and 5.3).

5.1. Descriptive statistics

Among the 180 subjects who participated in our experiment, eight subjects made inconsistent choices in the risk-preference elicitation task and seven subjects made inconsistent choices in the distributional preference elicitation task. This leaves a total of 168 subjects with consistent choices in both tasks. We focus our analysis on these 168 subjects.[8]

Table 2 presents a summary of descriptive statistics. Panel A reports individual risk preferences and Panel B shows the frequency of distributional (or "social") preference types. Panel C shows the number of subjects and the average opportunity cost assigned in the induced-value experiment. Also, note that all variables are reported for each of our treatments.

Table 2: Descriptive statistics

	Upper-bound (T1)	**Mid-point (T2)**	**Control (T3)**
Panel A: Risk attitude			
Risk index*	0.51 (0.12)	0.49 (0.16)	0.48 (0.12)
% of risk averse subjects	87	87	93
Panel B: Distributional (social) preferences			
% of efficiency-minded (altruistic) subjects	71	67	66
% of inequality averse subjects	15	16	18
% of spiteful subjects	9	12	11
% of inequality loving subjects	5	5	5
Panel C			
Number of subjects (N)	55	57	56
Budget size (points)	1200	1200	1200
Average opportunity cost	158.8 (80.2)	133.2 (77.5)	129.5 (73.7)

Note: Standard deviations in parentheses. *Risk index: For rational subjects, the risk index ranges from 0.1 (if a subject always chooses the safe payoff) to 1.0 (if a subject chooses the safe payoff only in the last decision problem). A higher value of the risk index means a lower degree of risk aversion. The proportion of risk averse subjects indicates the share of rational subjects who prefer safe options that are lower than or equal to the expected value of the lottery (50 points).

[8] However, our results remain robust including also the subjects with inconsistent choices.

With respect to risk-aversion, we find that across treatments, the majority of our subjects can be classified as risk-averse (Chi-square test for 3 independent samples: p=0.570). Likewise, the majority of our subjects can be classified as efficiency minded (altruistic) (71% for treatment 1, 67% for treatment 2 and 67% for treatment 3) (Chi-square test for 3 independent samples: p=0.997). This is in line with Balafoutas, Kerschbamer, & Sutter (2012) who find that 71% of their subjects are efficiency minded. Also, there is no statistically significant difference in the average of assigned opportunity costs in our induced-value experiment (Kruskal-Wallis one-way analysis of variance: p=0.1057). We can therefore conclude that if we observe a difference between our treatments, this can be attributed to the existence and/or level of the pre-existing fixed price scheme.

5.2. Impact of pre-existing fixed-price schemes on bids and information rents

We first examine the impact of our treatments on subjects' bids (hypotheses 1.1. and 1.2). We find the following.

RESULT 1.1 ('reference point')*: Exogenously provided reference prices influence subjects' bids in conservation auctions (in round 1 and across all 10 rounds). This effect is, however, not significant in the case where the fixed price is the midpoint of the distribution of opportunity costs.*

Support for Result 1.1. First, we compare average bids in round 1 across conditions with a high (upper-bound) and low (mid-point) fixed-price in stage-1 to our control condition without any fixed-price scheme. We find that the average bid in round 1of treatment 'Upper bound' (263) is significantly higher than the average bid in the control condition (224) (Mann-Whitney U test, two-sided, p = 0.207; Kolmogorov-Smirnov two-sample test, one-sided, p = 0.086; t-test, one-sided, p = 0.072), whereas the average bid in round 1 of treatment 'Midpoint' (221) is not statistically different from the control condition (Mann-Whitney U test, two-sided, p = 0.906; Kolmogorov-Smirnov two-sample test, one-sided, p = 0.711; t-test, one-sided, p = 0.583). Second, we compare average bids across all rounds and across conditions with a high (upper bound) and low (midpoint) fixed-price in stage-1 to our control condition without any fixed-price scheme. We find that the average bids in treatment 'Upper bound' (237) and treatment 'Mid-point' (220) are higher than the average bid in our control condition (208), although the difference between midpoint and the control is not statistically significant (control vs upper bound: Mann-Whitney U test, two-sided, p = 0.010, t-test: two-sided test, p = 0.029;

control vs mid-point: Mann-Whitney U test, p = 0.2690, t-test: one-sided test, p = 0.170).[9] Figure 2 provides further evidence in support for result 1. We also use OLS regressions to check the robustness of our result (see table 3), and confirm that round 1 and average round bids in the 'Upper-bound' treatment are pushed upwards by the pre-existing institution when compared to a situation without such an institution, although the effect is once again not significant for the 'Mid-point' treatment. Our results lead us to confirm H1.1 (reference point effect) with the qualification of the effect being significant only in the 'Upper-bound' condition. However, the reference point effect is not valid for the case where the fixed price is the midpoint of the distribution of opportunity costs.

RESULT 1.2 ('market experience'): *Multiple bidding rounds do not attenuate subjects' adjustments in bidding over time.*

Support for Result 1.2. We compare average bids across last five rounds and across conditions with a high (upper bound) and low (midpoint) fixed price in stage-1 to our control condition without any fixed-price scheme. We find that the average bids of last five rounds of treatments 'Upper bound' (240) and 'Midpoint' (227) are significantly higher than that of the control condition (205) (control vs upper bound: Mann-Whitney U test, two-sided, p = 0.000; Kolmogorov-Smirnov two-sample test, two-sided, p = 0.001; t-test, one-sided, p = 0.018. Control vs midpoint: Mann-Whitney U test, two-sided, p = 0.129; Kolmogorov-Smirnov two-sample test, two-sided, p = 0.014; t-test, one-sided, p = 0.092). Second, we compare the average bids of the last round across treatments. We also find that the average bids of the last round of treatments 'Upper bound' (252) and 'Midpoint' (239) are significantly higher than that of the control condition (194) (control vs upper bound: Mann-Whitney U test, two-sided, p = 0.001; Kolmogorov-Smirnov two-sample test, two-sided, p = 0.001; t-test, two-sided, p = 0.019. Control vs midpoint: Mann-Whitney U test, two-sided, p = 0.128; Kolmogorov-Smirnov two-sample test, two-sided, p = 0.025; t-test, two-sided, p = 0.041). The OLS regressions also helps confirm our results (see table 3). Therefore, we reject H1.2 (market experience).

[9] In addition, we find that bids in our treatments are statistically different from pre-existing price levels 300 and 155, respectively (treatment 1: t test: two-sided test, p = 0.000; treatment 2: t test: two-sided test, p = 0.000).

Figure 2: Average bids across rounds and treatments

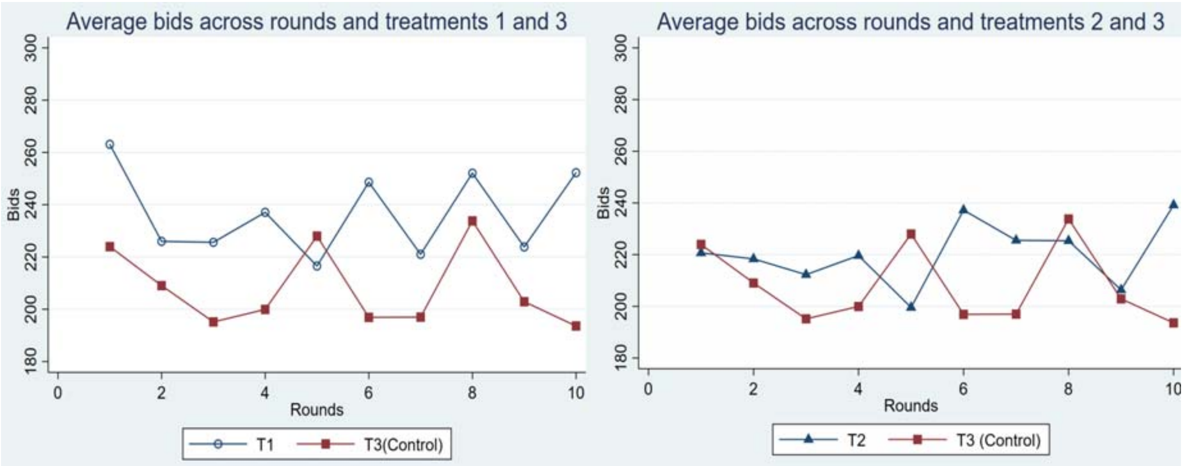


Table 3: OLS regression of subjects' bids on the pre-existing price level

Model	Model 1 (round 1)		Model 2 (round 10)		Model 3 (round 1-10)	
	Coefficient	p-value	Coefficient	p-value	Coefficient	p-value
Opportunity cost	0.780***	0.000	0.663***	0.008	0.626***	0.000
T1 (Upper-bound)	32.860*	0.064	48.616**	0.030	17.045*	0.079
T2 (Midpoint)	-0.403	0.967	38.309**	0.049	10.25304	0.286
T3 (Control)	------------------------------		------------------------------		------------------------------	
Constant	41.478	0.378	-111.98	0.399	54.623	0.232
Socioeconomic characteristics	YES		YES		YES	
N	150		150		150	
R^2	0.3549		0.2449		0.488	

Note: The table reports ordinary least squares estimates (heteroskedasticity-robust and cluster-robust standard errors in parentheses). Models 1 and 2 use first-round bid and tenth-round as dependent variable, whereas model 3 use average bid over 10 rounds as dependent variable. Opportunity cost is subject's opportunity (chip) cost. T1 is a binary variable that takes the value 1 if the pre-existing price is the upper bound of the distribution of opportunity costs and 0 otherwise. T2 is a binary variable that takes the value 1 if the pre-existing price is the midpoint of the distribution of opportunity costs and 0 otherwise. And T3 is a binary variable that takes the value 1 if there is no pre-existing price before conservation auctions and 0 otherwise. Socio-economic characteristics include age, gender, income, distributional (or "social") preference traits, risk index, and fairness perception of the auction. ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

We next examine H2, which states that subjects who have been selected by the pre-existing fixed-price scheme collect higher information rents under the auction than those who were not selected by the fixed-price scheme. We find the following:

RESULT 2 ('loss aversion')*: Subjects who have been selected by the pre-existing fixed-price scheme make lower bids and get higher information rents than those who were not selected by the fixed-price scheme. However, this effect is not significant in the case where the fixed price is the upper bound of the distribution of opportunity costs.*

Support for Result 2. Figure 3 displays average bids between T1 and T2, based on the outcome of the pre-existing fixed-price scheme. It is straightforward to note that, in T1, there is no statistically significant difference, in terms of average bid, between subjects who were selected by the fixed-price scheme (241) and those who were not (233) (Kolmogorov-Smirnov test: one-sided test, p = 0.859; t-test: one-sided test, p = 0.653). However, in T2, we do find a statistically significant difference. Subjects who were selected by the pre-existing fixed-price scheme have lower bids (183) than those who were not selected (271) (Kolmogorov-Smirnov test: two-sided test, p = 0.000; t-test: two-sided test, p = 0.000). This result leads us to confirm H2 (loss aversion). However, the loss aversion hypothesis is not valid for the case where the fixed price is the upper bound of the distribution of opportunity costs. A potential explanation is that information rents under the fixed-price scheme were higher under T2 than under T1.
When we compare information rents collected, we find that, in T1, there is no difference between subjects who were selected by the fixed-price scheme and those who were not selected (T1: Kolmogorov-Smirnov test, one-sided, p = 0.359; t-test, one-sided, p = 0.470), whereas, in T2, subjects who were selected by the fixed-price scheme collect more information rents than subjects who were not selected by the fixed-price scheme (T2: Kolmogorov-Smirnov test, two-sided, p=0.000; t-test, two-sided, p = 0.000). Furthermore, an econometric analysis of the impact of being selected by the pre-existing fixed-price scheme on subjects' bids confirms our result (see table 4). When we control for subjects' opportunity costs, subjects who have been selected by the pre-existing fixed-price scheme under the 'Midpoint' treatment are more likely to have a lower bid in the auction than those who were not selected by the fixed-price scheme. However, this effect is not significant in the case where the fixed price is the upper bound of the distribution of opportunity costs.

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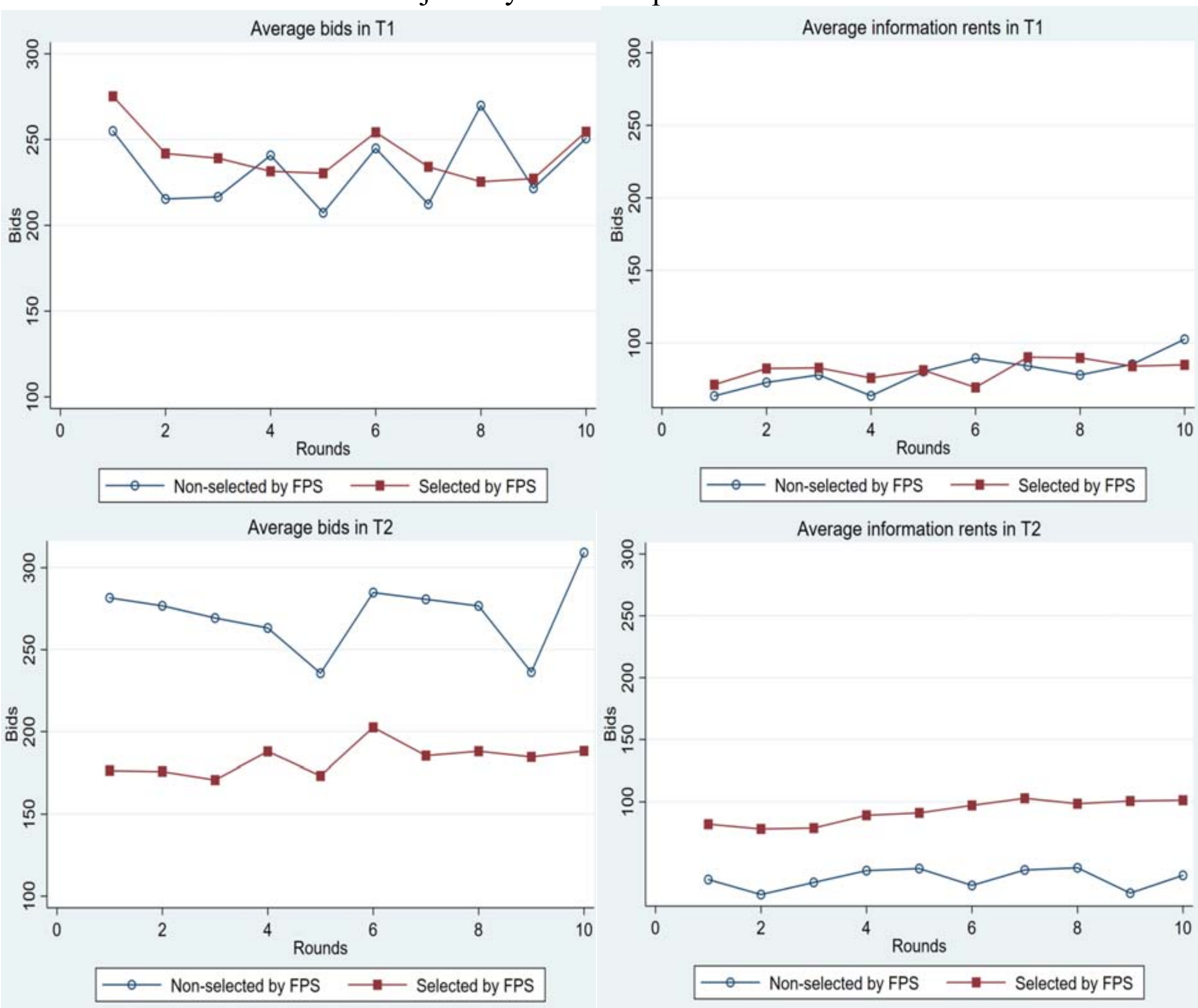
Figure 3: Average bids and information rents across treatments, selected and non-selected subjects by the fixed-price scheme.



Note: FPS means fixed-price scheme.

Table 4: Pooled FGLS regression of subject's bid on the fact of being selected by the fixed-price scheme

Model	T1		T2	
	Coefficient	p-value	Coefficient	p-value
Opportunity cost	0.732***	0.000	0.295**	0.014
Selected by FPS	6.592	0.787	-36.019**	0.026
Constant	130.826***	0.000	194.198***	0.000
N	550		570	
Prob > Chi2	0.000		0.000	

Note: The table reports feasible generalised least squares estimates (Cluster-robust standard errors in parentheses. We place no restrictions on the error correlation over round for individual *i*.). The dependent variable is subject's bid. Opportunity cost is subject's opportunity cost. Selected by FPS is a binary variable that takes the value 1 if the subject was selected by the pre-existing fixed-price scheme and 0 otherwise. ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

5.3. Impact of pre-existing fixed-price schemes on performance of conservation auctions

Regarding the performance of conservation auctions, we use the following criteria for our evaluation: the number of contracts awarded (i.e. the more contracts awarded, the better[10]), the information rent (i.e. the lower the information rent, the better), and the budgetary cost-effectiveness (i.e., the lower the payment per unit of ecosystem service provision, the better). For the interpretation of results in terms of ecosystem services, we assume here that each chip sold under the auction implies one unit of ecosystem services provided. As stated earlier, the information rent is calculated as the difference of the obtained payment from the auction and subject's assigned opportunity costs. Table 5 summarises all performance criteria across treatments. To the degree applicable, it also provides comparable values for the performance of the fixed-price schemes across all three treatments.

Table 5: Performance of conservation auctions across treatments

	Upper-bound (T1)		**Mid-point (T2)**		**Control (T3)**
	Fixed-price	Auctions	Fixed-price	Auction	Auction
Average bid	-	236.60 (16.23)	-	220.41 (12.42)	208.02 (15.01)
Average number of contracts awarded	22	30 (1.08)	33	35 (2.22)	35 (0.73)
% of contracts awarded	40	54	58	61	62
Average information rent	137.36 (15.94)	73.11 (10.42)	74.42 (46.73)	76.17 (8.08)	79.07 (4.68)
Payment per unit of ecosystem service ('budgetary cost-effectiveness)	300	191.34 (4.34)	155	178.63 (4.93)	177.19 (3.89)

Note: Values are averages across all ten rounds, in the case of auctions. Standard deviations in parentheses. % of contracts awarded is the proportion of subjects selected in each treatment.

We first analyse the impact of our treatments on the proportion of contracts awarded. We find the following.

RESULT 3.1.1 ('proportion of contracts – performance I'): *The pre-existence of a fixed-price scheme does not affect the proportion of contracts awarded by conservation auctions.*

RESULT 3.2.1 ('proportion of contracts – performance II): *Repeated-round conservation auctions do not affect the proportion of contracts awarded.*

[10] We assume that environmental impacts per unit of land conserved are equal across farmers, so that a larger number of contracts implies higher environmental effectiveness.

Support for Results 3.1.1 and 3.2.1. Table 5 reports the proportions of contracts awarded by each allocation mechanism in each treatment. We compare the proportion of contracts awarded in round 1 by conservation auctions in our control treatment (T3) with that in treatment 'upper-bound' (T1) and 'mid-point' (T2). We find that, in round 1, there is no statistically significant difference (T1 vs T3: Chi-square test for two independent samples: two-sided, p = 0.511; Fisher exact probability test: one-sided, p = 0.321. T2 vs T3: Chi-square test for two independent samples: two-sided, p = 0.789; Fisher exact probability test: one-sided, p = 0.471). This result leads us to reject H3.1 if we use the proportion of awarded contracts as criterion for evaluating the performance of conservation auctions. We do the same analysis for the last nine rounds. We find that, from round 9 to round 10, there is no significantly significant difference between the contract awarded by each treatment across rounds (T1 vs T3: Chi-square test for two independent samples: two-sided test, p = 0.395 for round 2 and 0.218 for round 10; T2 vs T3: Chi-square test for two independent samples: two-sided test, p = 0.790 for round 2 and p = 0.760 for round 10).[11] Our result leads us to reject H3.1 and H3.2 if we use the proportion of awarded contracts as criterion for evaluating the performance of conservation auctions.

We then examine the impact of the pre-existence of a fixed-price scheme on the level of information rent collected in conservation auctions. We find that:

RESULT 3.1.2 ('information rent – performance I')*: The pre-existence of a fixed-price scheme does not affect the level of information rent in conservation auctions.*

RESULT 3.2.2 ('information rent – performance II')*: Repeated-round conservation auctions do not affect the level of information rent.*

Support for Results 3.1.2 and 3.2.2. Again, we provide a between-treatment comparison to evaluate hypothesis 3, now focusing on information rents. We find that, in round 1, the information rent collected in auctions without a pre-existing fixed-price scheme (T3) is not different from the conditions including a pre-existing fixed price (T1 and T2) (T3 vs T1: Mann-Whitney U test: two-sided, p = 0.400, Kolmogorov-Smirnov two-sample test: one-sided, p = 0.903. T3 vs T2: Mann-Whitney U test: two-sided test, p = 0.755, Kolmogorov-Smirnov two-sample test: one-sided test, p = 0.674). Likewise, when we consider the average information rent collected over 10 rounds, we also find that there is no difference between treatments (T1 and T2) and the control condition (T3 vs T1: Mann-Whitney U test: two-sided test, p = 0.579, Kolmogorov-Smirnov two-sample test: one-sided test, p = 0.930 T3 vs T2:

[11] The result holds for every round. We find the same result, using the Fisher exact probability one-sided test.

Mann-Whitney U test: two-sided test, p = 0.794 , Kolmogorov-Smirnov two-sample test: one-sided test, p = 0.873).

We do the same analysis for the last five rounds. When we consider the average information rent collected over the last five rounds, we find that there is no difference between treatments (T1 and T2) and the control condition (T3 vs T1: Mann-Whitney U test: two-sided test, p = 0.799, Kolmogorov-Smirnov two-sample test: one-sided test, p = 0.788. T3 vs T2: Mann-Whitney U test: two-sided test, p = 0.864, Kolmogorov-Smirnov two-sample test: one-sided test, p = 0.613). Likewise, in the last round, there is no difference between treatments (T1 and T2) and the control condition (T3 vs T1: Mann-Whitney U test: two-sided test, p = 0.541, Kolmogorov-Smirnov two-sample test: one-sided test, p = 0.269. T3 vs T2: Mann-Whitney U test: two-sided test, p = 0.990, Kolmogorov-Smirnov two-sample test: one-sided test, p= 0.591).

A further econometric regression (see table 6 model 1), using the pooled dataset of three treatments (T1, T2 and T3), indicates that T1 and T2 do not affect the information rent collected. This result leads us to reject H3.1 and H3.2 if we use information rent as criterion for evaluating the performance of conservation auctions.

Finally, we evaluate the impact of the pre-existence of a fixed-price scheme on payment per unit of ecosystem service in conservation auctions. We find that:

RESULT 3.1.3 ('budgetary cost-effectiveness – performance I')*: The pre-existence of a fixed-price scheme increases the payment per unit of ecosystem service in conservation auctions. The effect is low for the case where the fixed price is the midpoint of the distribution of opportunity costs.*

RESULT3.2.3 ('budgetary cost-effectiveness – performance II')*: Repeated-round conservation auctions do not attenuate the effect on the budgetary cost-effectiveness.*

Support for Results 3.1.3 and 3.2.3. Our between-treatment comparison indicates that, in round 1, payments per unit of ecosystem service in conservation auctions are significantly higher when the pre-existing fixed-price is the upper bound of the distribution of opportunity costs (T1 vs T3: Mann-Whitney U test: two-sided test, p = 0.224, Kolmogorov-Smirnov two-sample test: two-sided test, p = 0.074; T2 vs T3: Mann-Whitney U test: two-sided test, p = 0.396, Kolmogorov-Smirnov two-sample test: one-sided test, p = 0.454). When we consider the average payment across rounds, we find the same result (T1 vs T3: Mann-Whitney U test: two-sided test, p = 0.004, Kolmogorov-Smirnov two-sample test: two-sided test, p = 0.006; T2 vs T3: Mann-Whitney U test: two-sided test, p = 0.206, Kolmogorov-Smirnov two-sample test: one-sided test, p = 0.072).

We do the same analysis for the last five rounds. Using the average payments of last five rounds, we find that payments per unit of ecosystem service in conservation auctions are still significantly higher when the pre-existing fixed-price is the upper bound of the distribution of opportunity costs (T1 vs T3: Mann-Whitney U test: two-sided, p = 0.223, Kolmogorov-Smirnov two-sample test: one-sided, p = 0.071; T2 vs T3: Mann-Whitney U test: two-sided test, p = 0.378, Kolmogorov-Smirnov two-sample test: one-sided test, p = 0.084). However, in the last round, there is no difference between treatments and the control condition (T1 vs T3: Mann-Whitney U test: two-sided, p = 0.114, Kolmogorov-Smirnov two-sample test: one-sided, p = 0.622; T2 vs T3: Mann-Whitney U test: two-sided test, p = 0.378, Kolmogorov-Smirnov two-sample test: one-sided test, p = 0.232).

We confirm our results using the pooled dataset of all three treatments (T1, T2 and T3) and regressing the payment per unit of ecosystem service on treatments. As can be seen in table 6 (model 2), the payment per unit of ecosystem service significantly increases under T1 (as compared to the control), while the effect is not statistically significant under T2. In summary, we confirm H3.1 and H3.2 for T1 when using payment per unit of ecosystem service as criterion for evaluating the performance of conservation auctions, while the effect is weaker for T2.

Table 6: Pooled OLS regression of information rent collected and payment per unit of ecosystem service.

Model	**Model 1** (information rent collected)		**Model 2** (payment per unit of ecosystem service)	
	Coefficient	p-value	Coefficient	p-value
T1	-6.27991	0.170	10.009***	0.002
T2	-3.164822	0.437	0.677	0.801
T3	------------------	------------	----------------------	------------
Constant	YES		YES	
N	1,006		1,006	
Prob > F	0.3770		0.005	
R-squared	0.002		0.013	

Note: The table reports ordinary least squares estimates (robust standard errors are used for the t-test). Model 1 uses the information rent collected as dependent variable, whereas model 2 uses the payment per unit of ecosystem service as dependent variable. T1 is a binary variable that takes the value 1 if the pre-existing price is the upper bound of the distribution of opportunity costs and 0 otherwise. T2 is a binary variable that takes the value 1 if the pre-existing price is the midpoint of the distribution of opportunity costs and 0 otherwise. And T3 is a binary variable that takes the value 1 if there is no pre-existing price before conservation auctions and 0 otherwise. The coefficients indicate changes from T3 (control). ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

For T1, we also compare the performance of the auction to the performance of the fixed-price scheme to check whether the alleged performance gains of introducing an auction prevail despite the behavioral biases shown above. For this, we compare the performance indicators for T1 in Table 5 between the fixed-price scheme and the auction. We find that under the auction the number and proportion of contracts (and thus ecosystem service provision in our setting) is higher (t-test: one-sided test, p = 0.000) information rents are lower (Wilcoxon matched-pairs signed-ranks test: two-sided, p = 0.000; t-test: two-sided, p = 0.000) , and budgetary cost-effectiveness is improved (Wilcoxon matched-pairs signed-ranks test: two-sided, p = 0.000; t-test: two-sided, p = 0.000) as compared to the fixed-price scheme. So, while performance is poorer than in a setting where no fixed-price scheme was previously installed, the auction still seems worthwhile implementing.

6. Discussion and conclusions

The purpose of this paper is to examine the impact of pre-existing fixed-price schemes on subjects' bids and the performance of conservation auctions. We use the midpoint and the upper bound of the distribution of opportunity costs as pre-existing prices, which may be viewed as realistic benchmarks for policy settings. Regarding behavioural responses, we find that the reference price provided by the fixed-price scheme influences subjects' bids in conservation auctions and this effect is not attenuated by repeating the auction. Moreover, we find that these behavioural responses have implications for the budgetary effectiveness of the auction: the pre-existence of the fixed-price scheme increases the amount spent per unit of ecosystem service in conservation auctions. However, all of these effects are significant only when the fixed price is set at a high level, in our case at the upper bound of opportunity costs.

We also find that subjects who were selected by the pre-existing fixed-price scheme tend to make lower bids in the conservation auction than those who were not selected by the fixed-price scheme and obtain higher information rents. This effect, which we attribute to loss aversion, is valid in the case where the pre-existing price is the midpoint of the distribution of opportunity costs. However, in the case where the pre-existing price is the upper bound of the distribution of opportunity costs, bids are not significantly affected by being selected or not in the fixed-price scheme, and information rents of those previously selected are actually lower. This may be due to the following. Information rents in the fixed-price scheme were lower when the price was set at the upper bound than when it was set at the midpoint. In addition, the type of opportunity-cost subjects that are selected by each fixed-price scheme differs. In fact, when the pre-existing price is the middle point of the distribution of opportunity costs, the fixed-price scheme is more likely to select low-opportunity-cost subjects than high-opportunity-cost subjects, while the upper-bound scheme provides equal selection opportunities to all subjects.

Previous studies have called for allocating conservation contracts by using auctions (Jindal et al., 2013; Porras, Barton, Miranda, & Chacón-Cascante, 2013; Schilizzi & Latacz-Lohmann, 2007; Stoneham et al., 2003; Windle & Rolfe, 2008). Our study indicates that the pre-existing institution and/or the pre-existing environmental service price can matter for the performance of conservation auctions. In our case, conservation auctions may perform less well when there is a high pre-existing fixed-price scheme than when there is not, although our analysis still finds an overall performance gain from implementing the auction compared to a fixed-price scheme. Our results imply that it is important for policymakers to take the pre-existing institution into account when evaluating the opportunities for implementing an auction, . From an academic perspective, our research confirms the price following hypothesis, especially when the fixed price is high.

Two caveats are in order when considering the results. First, our study was conducted in a laboratory, using university students as subjects. Second, our parametric setting, although close to Schilizzi & Latacz-Lohmann (2007), is not inspired by a real case study. Further empirical research should examine the impact of pre-existing fixed-price schemes on conservation auctions in a real PES program. Loss aversion may be stronger among actual landholders than among university students. This could increase the behavioral effects found in this paper. It remains to be studied whether the effect can be large enough to override the general performance gains from implementing auctions.

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Appendix

A. Preferences measurement

Table A1: The Certainty Equivalence Test (CET)

Row Nr.	Alternative LEFT	Your choice		Alternative RIGHT
	You receive			You receive
1	**10 points for sure**	LEFT ◯	◯ RIGHT	**50% of 100 points** and **50% of 0 point**
2	**20 points for sure**	LEFT ◯	◯ RIGHT	**50% of 100 points** and **50% of 0 point**
3	**30 points for sure**	LEFT ◯	◯ RIGHT	**50% of 100 points** and **50% of 0 point**
4	**40 points for sure**	LEFT ◯	◯ RIGHT	**50% of 100 points** and **50% of 0 point**
5	**50 points for sure**	LEFT ◯	◯ RIGHT	**50% of 100 points** and **50% of 0 point**
6	**60 points for sure**	LEFT ◯	◯ RIGHT	**50% of 100 points** and **50% of 0 point**
7	**70 points for sure**	LEFT ◯	◯ RIGHT	**50% of 100 points** and **50% of 0 point**
8	**80 points for sure**	LEFT ◯	◯ RIGHT	**50% of 100 points** and **50% of 0 point**
9	**90 points for sure**	LEFT ◯	◯ RIGHT	**50% of 100 points** and **50% of 0 point**
10	**100 points for sure**	LEFT ◯	◯ RIGHT	**50% of 100 points** and **50% of 0 point**

Source: Adapted from Balafoutas, Kerschbamer, & Sutter (2012), Dohmen & Falk (2011) and Dohmen et al. (2010). Note: Since the expected value of the lottery is 50 points, risk averse subjects should prefer the safe options that are smaller than or equal to 50 points over the lottery. Risk loving subjects should choose the lottery when the safe option is greater than 50 points.

Table A2: The Equality Equivalence Test (EET)

Row Nr.	LEFT		Your choice		RIGHT	
	You receive	Other person receives			You receive	Other person receives
1	**80 points**	**130 points**	LEFT ◯	◯ RIGHT	**100 points**	**100 points**
2	**90 points**	**130 points**	LEFT ◯	◯ RIGHT	**100 points**	**100 points**
3	**100 points**	**130 points**	LEFT ◯	◯ RIGHT	**100 points**	**100 points**
4	**110 points**	**130 points**	LEFT ◯	◯ RIGHT	**100 points**	**100 points**
5	**120 points**	**130 points**	LEFT ◯	◯ RIGHT	**100 points**	**100 points**
6	**80 points**	**70 points**	LEFT ◯	◯ RIGHT	**100 points**	**100 points**
7	**90 points**	**70 points**	LEFT ◯	◯ RIGHT	**100 points**	**100 points**
8	**100 points**	**70 points**	LEFT ◯	◯ RIGHT	**100 points**	**100 points**
9	**110 points**	**70 points**	LEFT ◯	◯ RIGHT	**100 points**	**100 points**
10	**120 points**	**70 points**	LEFT ◯	◯ RIGHT	**100 points**	**100 points**

Source: Adapted from Balafoutas et al. (2012) and Kerschbamer (2015).

B. Experimental Instructions

Instructions – Experiment 1 (in German)

Herzlich Willkommen und Danke für Ihre Teilnahme!

Bitte lesen Sie sich die Anleitung aufmerksam durch. Die Höhe des Geldbetrags, den Sie während des Experiments verdienen, ist abhängig von Ihren eigenen Entscheidungen sowie denen der anderen Teilnehmenden. Das Experiment und die gesamte Interaktion zwischen Ihnen und den anderen Teilnehmenden findet über Computer-Terminals statt und wird circa 60 Minuten dauern.

Es ist Ihnen nicht gestattet, mit anderen Teilnehmenden zu kommunizieren. Verstoßen Sie gegen diese Regel, müssen Sie ohne Bezahlung vom Experiment ausgeschlossen werden. Bitte beachten Sie, dass es weder den wissenschaftlichen MitarbeiterInnen noch den anderen Teilnehmenden möglich sein wird, Ihre anonym getroffenen Entscheidung Ihrer Person zuzuordnen**.** Sollten Sie während des Experiments Fragen haben, geben Sie dies bitte mit einem Handzeichen bekannt. Wir werden dann zu Ihnen kommen und Ihre Frage bei gedämpfter Lautstärke entgegennehmen. Anschließend wird die Frage laut beantwortet, sodass alle Teilnehmenden ebenfalls informiert sind.

Grundlegender Ablauf des Experiments

Das Experiment setzt sich aus mehreren *Teilen* zusammen. In jedem Teil werden Sie gebeten, eine oder mehrere Entscheidungen zu treffen. Bitte beachten Sie, dass Ihre Entscheidungen in den verschiedenen Teilen des Experiments vollständig unabhängig voneinander sind und dass das Ergebnis Ihrer Entscheidungen Ihnen erst zum Ende des Experiments mitgeteilt wird.

Auszahlung

Ihre Auszahlung am Ende des Experiments berechnet sich wie folgt: Sie erhalten 5 Euro, unabhängig von Ihren Entscheidungen im Experiment. Des Weiteren erhalten Sie eine Zahlung abhängig von jenen Entscheidungen, die Sie in *einem* per Zufallsprinzip ausgewählten *Teil* des Experiments getroffen haben. Während des Experiments wird Ihr Verdienst in „Punkten“ (Währungseinheit des Experimentes) berechnet. Am Ende des Experiments wird Ihr Verdienst nach dem folgenden Wechselkurs in Euro umgerechnet:

1 Punkt = 0,10 €.

Ihre Auszahlung erhalten Sie in bar nach Ende des Experimentes gegen Vorlage Ihres Teilnahmecodes.

Anleitung: ***Teil A***

In diesem ersten Teil des Experiments müssen Sie **10 Entscheidungen** treffen. Jede Ihrer Entscheidungen besteht aus der **Wahl zwischen einer LINKEN und einer RECHTEN Alternative**.
In jeder der 10 Entscheidungssituationen erhalten Sie über die LINKE Alternative einen sicheren Verdienst. Der Verdienst der RECHTEN Alternative ist hingegen abhängig vom Zufall.

Beispiel: Sie könnten gefragt werden, ob Sie die LINKE Alterative bevorzugen, mit der Sie definitiv 70 Punkte erhalten. Oder ob Sie die RECHTE Alternative wählen möchten, bei der Sie mit einer Wahrscheinlichkeit von 50% 100 Punkte und mit einer Wahrscheinlichkeit von 50% 0 Punkte erhalten. Sie müssten sich dann für eine der beiden Alternativen entscheiden. Dieses Entscheidungsproblem würde wie folgt auf Ihrem Bildschirm dargestellt sein:

LINKE Alternative	Ihre Wahl		RECHTE Alternative
Sie erhalten			Sie erhalten
70 Punkte mit Sicherheit	LINKS ◯	◯ RECHTS	**mit 50%iger Wahrscheinlichkeit 100 Punkte und** **50%iger Wahrscheinlichkeit 0 Punkte**

In diesem ersten Teil des Experiments werden Sie **insgesamt 10 solcher Entscheidungen** treffen. Ihr Verdienst aus diesem Abschnitt wird wie folgt bestimmt:
Am Ende des Experiments wird für jede und jeden TeilnehmerIn **eine der 10 Entscheidungssituationen** per Zufallsprinzip ausgewählt und der Verdienst entsprechend der getroffenen Wahl bestimmt. Zur Anschauung: Würde per Zufallsprinzip die oben vorgestellte Entscheidungssituation ausgewählt und sollten Sie hier die RECHTE Alternative gewählt haben, so erhielten Sie mit einer Wahrscheinlichkeit von 50% 100 Punkte und mit einer Wahrscheinlichkeit von 50% 0 Punkte. Ob Sie nun tatsächlich 100 oder 0 Punkte erhalten, wird erneut per Zufallsprinzip durch den Zentralrechner bestimmt.

Bitte melden Sie sich per Handzeichen, wenn Sie eine Frage haben. Wir kommen dann zu Ihnen und werden alle Ihre Fragen individuell beantworten.
Wenn Sie die Anweisungen verstanden haben, klicken Sie bitte auf „weiter.

Anleitung: ***Teil B***

In diesem Teil des Experiments müssen Sie **10 Entscheidungen** treffen. In jeder der Entscheidungssituationen bilden Sie mit einer oder einem anderen Teilnehmenden ein Paar. Diese oder dieser wird im Folgenden als *„die andere Person"* bezeichnet. „Die andere Person" wird Ihnen per Zufallsprinzip durch den Zentralcomputer anonym zugeteilt.
Jede Ihrer Entscheidungen besteht erneut in der **Wahl zwischen den Alternativen LINKS und RECHTS**. Jede Alternative ist mit Konsequenzen für Sie und „die andere Person" verbunden.

Beispiel: Sie könnten gefragt werden, ob Sie die LINKE Alternative bevorzugen, durch die Sie 110 Punkte erhalten und die „andere Person" 130 Punkte erhält. Oder ob Sie die RECHTE Alternative wählen möchten, bei der Sie 100 Punkte erhalten und die „andere Person" 100 Punkte. Sie müssen sich dann für eine der beiden Alternativen entscheiden. Dieses Entscheidungsproblem würde wie folgt auf Ihrem Bildschirm dargestellt:

LINKS		Ihre Wahl		RECHTS	
Sie erhalten	Die andere Person erhält			Sie erhalten	Die andere Person erhält
110 Punkte	**130 Punkte**	LINKS ◯	◯ RECHTS	**100 Punkte**	**100 Punkte**

In diesem zweiten Teil des Experiments werden Sie **insgesamt 10 solcher Entscheidungen** treffen. Ihr Verdienst aus diesem Teil wird wie folgt bestimmt:
Am Ende des Experiments **wird der Zentralcomputer per Zufallsprinzip bestimmen, wessen Entscheidungen – Ihre oder die der „anderen Person" – herangezogen werden**. Als nächstes wird der Computer per Zufallsprinzip **eine der 10 Entscheidungen** dieser Person auswählen und diese Entscheidung ausführen. Sollte beispielsweise per Zufallsprinzip die oben vorgestellte Entscheidungssituation ausgewählt und Ihre Entscheidung herangezogen werden und Sie hätten die LINKE Alternative gewählt, so erhielten Sie 110 Punkte und „die andere Person" 130 Punkte.

Bitte melden Sie sich per Handzeichen, wenn Sie eine Frage haben. Wir kommen dann zu Ihnen und werden alle Ihre Fragen individuell beantworten.
Wenn Sie die Anweisungen verstanden haben, klicken Sie bitte auf „weiter".

Anleitung: ***Teil C***

Zu Beginn von Teil C wird der Zentralcomputer alle Teilnehmenden zu Gruppen von 10 Personen zusammenfassen. Sie werden im Laufe von Teil C immer in dieser Gruppe von 10 Personen bleiben.

In diesem Abschnitt erhält jedes der 10 Mitglieder Ihrer Gruppe (Sie mitinbegriffen) einen Chip, den Sie einem Käufer – dem Computer – anbieten können. Jeder Chip kostet einen gewissen Betrag. Die Höhe der *Kosten* eines Chips werden zufällig **zwischen 10 Punkten und 300 Punkten** gewählt. Jede der Zahlen zwischen 10 und 300 hat dabei eine gleich hohe Wahrscheinlichkeit, gewählt zu werden. Für alle Mitglieder Ihrer Gruppe besteht die gleiche Wahrscheinlichkeit, einen Chip mit bestimmten Kosten zu besitzen. Die Höhe der Kosten Ihres Chips wird Ihnen auf Ihrem Computerbildschirm mitgeteilt. Für den restlichen Verlauf von Teil C werden die Kosten Ihres Chips gleichbleiben.

Auswahl der GewinnerInnen:

Zufall: Der Computer wählt GewinnerInnen durch einen Zufallszug aus, bis sein Budget in Höhe von **1200 Punkten** ausgeschöpft ist. Wird diese Regel angewendet, **ist jedes individuelle Angebot mit einem Preis in Höhe von 300 Punkten festgelegt.**

Das bedeutet:

- Ob Sie tatsächlich ausgewählt sind, wird zufällig vom Zentralcomputer bestimmt.

Ihr Verdienst:

Ihr Verdienst in diesem Abschnitt wird wie folgt berechnet:

- Wenn Sie per Zufallsprinzip ausgewählt werden, so erhalten Sie 300 Punkte minus der Kosten Ihres Chips.
- Wenn Sie nicht ausgewählt werden, so erhalten Sie einen Verdienst von 0 Punkten.

Beispiel: Angenommen, Ihr Chip kostet 50 Punkte; Der Computer bietet einen Preis von 300 Punkten. Ihr Verdienst in diesem Teil wäre dementsprechend 250 Punkte (also 300 - 50) wenn Sie per Zufall ausgewählt werden.. Ansonsten beträgt Ihr Verdienst 0 Punkte in diesem Teil des Experimentes.

Zusammenfassung:

- Ihre Gruppe besteht aus 10 Teilnehmenden (Ihnen und 9 anderen)
- Jede und jeder Teilnehmende erhält 1 Chip
- Jeder Chip ist mit *Kosten* verbunden. Die Höhe der Kosten wird zufällig im Intervall zwischen **10 und 300 Punkten** bestimmt.
- Wenn Sie per Zufallsprinzip ausgewählt werden, so erhalten Sie 300 Punkte minus der Kosten Ihres Chips.
- Wenn Sie nicht ausgewählt werden, so erhalten Sie einen Verdienst von 0 Punkten.

Bitte melden Sie sich per Handzeichen, wenn Sie eine Frage haben. Wir kommen dann zu Ihnen und werden alle Ihre Fragen individuell beantworten.Wenn Sie die Anweisungen verstanden haben, klicken Sie bitte auf „weiter.

Anleitung: ***Teil D***

Teil D des Experimentes besteht aus mehreren Runden. Wenn Teil D als auszahlungsrelevant ausgewählt wird, wird eine der Runden per Zufallsprinzip ausgewählt um Ihren Verdienst zu bestimmen.
Die Kosten Ihres Chips, und die Kosten der Chips der anderen Mitglieder in Ihrer Gruppe bleiben die gleichen wie im vorherigen Teil des Experimentes.

Teil D, Runde 1

Ihre Aufgabe:

Sie können dem Käufer unter Angabe eines *Preises* Ihren Chip anbieten – gleiches gilt für die anderen 9 Mitglieder Ihrer Gruppe. Bitte berücksichtigen Sie: Der von Ihnen angegebene Preis kann nicht unter den Kosten Ihres Chips liegen.

Auswahl der GewinnerInnen:

Rangfolge: Der Computer wird dann alle 10 Angebote ihrem Preis nach von niedrig nach hoch ordnen. Angefangen beim niedrigsten Angebot akzeptiert er der Rangfolge entlang solange Angebote, bis sein Budget in Höhe von **1200 Punkten** ausgeschöpft ist.

Bitte beachten Sie, dass Sie niemals die Höhe der Chipkosten der anderen Teilnehmenden, die zu Ihrer Gruppe gehören, erfahren werden. Umgekehrt erhalten auch die anderen Teilnehmenden keine Informationen über die Höhe Ihrer Chipkosten.

Das bedeutet:

- Sie **konkurrieren** mit den anderen 9 Teilnehmenden Ihrer Gruppe darum, dass Ihr Angebot akzeptiert wird.
- Je niedriger Ihr Angebot, desto höher die Wahrscheinlichkeit, dass Ihr Angebot akzeptiert wird.

Ihr Verdienst:

Ihr Verdienst in diesem Abschnitt wird wie folgt berechnet:

- Wenn Ihr Preis akzeptiert wird, erhalten Sie den Preis minus den Kosten Ihres Chips.
- Wenn der von Ihnen angegebene Preis nicht akzeptiert wird, erhalten Sie 0 Punkte.

Beispiel: Angenommen, Ihr Chip kostet 50 Punkte und Sie bieten Ihn für 200 Punkten an. Ihr Verdienst in diesem Abschnitt wäre somit 150 Punkte (also 200 - 50), aber ausschließlich wenn Ihr Angebot angenommen wird. Ansonsten erhalten Sie 0 Punkte in diesem Abschnitt.

Zusammenfassung:

- Ihre Gruppe besteht aus 10 Teilnehmenden (Ihnen und 9 anderen)
- Jede und jeder Teilnehmende erhält 1 Chip
- Jeder Chip ist mit *Kosten* verbunden. Die Höhe der Kosten wird zufällig im Intervall zwischen **10 und 300 Punkten** bestimmt.
- Der Käufer wird alle Angebote ihrem Preis nach, angefangen beim niedrigsten bis zum höchsten, sortieren. Erneut beginnend mit dem niedrigsten Angebot wird der Computer solange Angebote akzeptieren, bis sein Budget in Höhe von **1200 Punkten** erschöpft ist.
- Wenn Ihr Angebot akzeptiert wird, so erhalten Sie den Preis Ihres Chips minus der Kosten Ihres Chips.
- Wenn Ihr Angebot abgelehnt wird, erhalten Sie 0 Punkte.

Bitte melden Sie sich per Handzeichen, wenn Sie eine Frage haben. Wir kommen dann zu Ihnen und werden alle Ihre Fragen individuell beantworten.Wenn Sie die Anweisungen verstanden haben, klicken Sie bitte auf „weiter.

Teil D, Runde 2

Runde 2 läuft genauso ab wie Runde 1. Bitte melden Sie sich per Handzeichen, wenn Sie eine Frage haben. Wir kommen dann zu Ihnen und werden alle Ihre Fragen individuell beantworten. Wenn Sie die Anweisungen verstanden haben, klicken Sie bitte auf „weiter.

Teil D, Runde 3

Runde 3 läuft genauso ab wie Runde 2. Bitte melden Sie sich per Handzeichen, wenn Sie eine Frage haben. Wir kommen dann zu Ihnen und werden alle Ihre Fragen individuell beantworten. Wenn Sie die Anweisungen verstanden haben, klicken Sie bitte auf „weiter.

Teil D, Runde 4

Runde 4 läuft genauso ab wie Runde 3. Bitte melden Sie sich per Handzeichen, wenn Sie eine Frage haben. Wir kommen dann zu Ihnen und werden alle Ihre Fragen individuell beantworten. Wenn Sie die Anweisungen verstanden haben, klicken Sie bitte auf „weiter.

Teil D, Runde 5

Runde 5 läuft genauso ab wie Runde 4. Bitte melden Sie sich per Handzeichen, wenn Sie eine Frage haben. Wir kommen dann zu Ihnen und werden alle Ihre Fragen individuell beantworten. Wenn Sie die Anweisungen verstanden haben, klicken Sie bitte auf „weiter.

Teil D, Runde 6

Runde 6 läuft genauso ab wie Runde 5. Bitte melden Sie sich per Handzeichen, wenn Sie eine Frage haben. Wir kommen dann zu Ihnen und werden alle Ihre Fragen individuell beantworten. Wenn Sie die Anweisungen verstanden haben, klicken Sie bitte auf „weiter.

Teil D, Runde 7

Runde 7 läuft genauso ab wie Runde 6. Bitte melden Sie sich per Handzeichen, wenn Sie eine Frage haben. Wir kommen dann zu Ihnen und werden alle Ihre Fragen individuell beantworten. Wenn Sie die Anweisungen verstanden haben, klicken Sie bitte auf „weiter.

Teil D, Runde 8

Runde 8 läuft genauso ab wie Runde 7. Bitte melden Sie sich per Handzeichen, wenn Sie eine Frage haben. Wir kommen dann zu Ihnen und werden alle Ihre Fragen individuell beantworten. Wenn Sie die Anweisungen verstanden haben, klicken Sie bitte auf „weiter.

Teil D, Runde 9

Runde 9 läuft genauso ab wie Runde 8. Bitte melden Sie sich per Handzeichen, wenn Sie eine Frage haben. Wir kommen dann zu Ihnen und werden alle Ihre Fragen individuell beantworten. Wenn Sie die Anweisungen verstanden haben, klicken Sie bitte auf „weiter.

Teil D, Runde 10

Runde 10 läuft genauso ab wie Runde 9. Bitte melden Sie sich per Handzeichen, wenn Sie eine Frage haben. Wir kommen dann zu Ihnen und werden alle Ihre Fragen individuell beantworten. Wenn Sie die Anweisungen verstanden haben, klicken Sie bitte auf „weiter.