Quality Information and Procurement Auction Outcomes: Evidence from a Payment for Ecosystem Services Laboratory Experiment

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Abstract

Conservation auctions are used to procure ecosystem services from private landowners. Bids in these auctions are comprised of both conservation actions and offered prices, though most existing studies of auction performance have assumed that parcel characteristics are exogenously endowed. The choice of conservation action, and uncertainty among participants about the level of ecosystem services provided by different actions, provide an opportunity for auction facilitators to affect auction outcomes through the amount of information provided to participants about the quality of their conservation action. An induced-value laboratory auction experiment is used to explore the impact of access to quality information on the outcome of single-round, multi-attribute conservation procurement auctions. The results indicate that providing participants with information about the quality of their potential conservation actions can increase auction efficiency from 3.4 to 5.0 percentage points, depending on model specification, as measured by the amount of total quality provided for each dollar spent on conservation. This finding differs from recent results in the literature, which have demonstrated a negative relationship between information and efficiency. The novel result stems from inclusion of quality as a choice variable in this study; access to more information results in higher quality submissions, and this effect dominates losses in efficiency due to information rent-seeking. *JEL*: C91, D44, Q28

Key Words: Ecosystem services, information asymmetry, laboratory experiment, procurement auctions, quality auctions

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1 Introduction

The information structure of conservation procurement auctions has attracted significant attention in recent years due to concerns about adverse selection and inefficiencies in the Conservation Reserve Program in the U.S. and other conservation auctions. Asymmetric information between landowners and the buyer regarding the private costs of conservation actions enables landowners to extract payments that are greater than their reservation rate for undertaking conservation actions. Under certain conditions, auctions have theoretically been shown to reduce these information rents by motivating the honest revelation of participants' opportunity costs (McAfee and McMillan (1987); Vickrey (1961)), thereby allowing for the enrollment of more land into conservation for a given budget. However, field evidence from conservation procurement auctions suggests that landholder rents may comprise a significant portion of conservation expenditures (Ulber et al. (2011); Kirwan et al. (2005)). Increasing the efficiency of conservation procurement auctions could have a meaningful impact on ecosystem-service provision; the Conservation Reserve Program in the U.S. uses an auction to allocate almost $2 billion of conservation payments annually.

A promising recent research path has focused on the role of environmental quality information and how it can be used to the buyer's advantage in designing an auction to increase ecosystem service provision under a limited budget. A conservation procuring agency may have an informational advantage in that it has the resources and ability to assess the quality of conservation activities across different parcels of land (Glebe (2013); Cason and Gangadharan (2004); Stoneham et al. (2003)). In contrast, landowners may have relatively less knowledge about the landscape-scale functional processes that generate ecosystem services and the techniques used to estimate the value of these services. In conservation procurement auction experiments where the assessed environmental quality is an exogenous attribute of a submitted offer, withholding this information has been shown to improve auction efficiency by inhibiting the rent-seeking behavior of landowners with high quality proposals (Banerjee et al., 2014; Cason et al., 2003).[1] Within a similar theoretical framework, Glebe (2013) concludes that withholding information about quality can increase the cost-effectiveness of auctions in which there is a fixed number of participants.

We extend this prior work by assessing the implications of quality information revelation in an auction setting where sellers can select both a price *and* a conservation activity. By treating both price and quality as choice variables in the formation of offers, this design more closely reflects field conditions where landholders are given significant latitude in selecting what land and conservation practices they submit for consideration (Claassen et al., 2008). This design violates many assumptions of the standard benchmark auction model (McAfee and McMillan, 1987) and extends significantly beyond prior analytical evaluations of conservation auctions, so we employ an experimental evaluation of the main information treatment effect.

We find that providing auction participants with detailed information regarding the quality of their conservation choice leads to improved auction performance, the opposite finding of studies investigating exogenous quality auctions (Banerjee et al., 2014; Cason et al., 2003). We attribute this finding to the simple inability of landowners' to identify and submit high quality conservation actions when information is withheld, leading to submissions based solely on cost considerations with effectively random quality. The acceptance of lower-quality conservation actions results in an overall decrease in auction efficiency, despite an observed decrease in rent-seeking. This reduction in efficiency from withholding quality information is monotone across greater degrees of quality uncertainty (from known quality value to known quality rank, to unknown quality). The results demonstrate that information restriction is not a panacea for cost-effective conservation procurement and that a greater degree of information revelation in this common procurement auction format can increase

[1] Withholding information from auction participants was also demonstrated to increase procurement auction performance in a more general context (Haruvy and Katok, 2013).

ecosystem-service provision under a budget when quality is a choice variable.

2 Auction Background and Experimental Design

2.1 Procurement Auctions

Conservation auctions are a type of procurement auction, a broad category that covers all auction variants with a single buyer and multiple potential sellers. While the simplest implementation of this type of auction includes price as the only relevant attribute of the good for sale, there are very few instances in procurement where price is the only consideration (Rezende, 2009). In the conservation context, the ecosystem services generated from a parcel of land are an important factor in determining the social welfare contribution of conservation expenditures. Contributions in the areas of auction and mechanism design have significantly improved our understanding of how to promote efficient procurement outcomes when both quality and price matter. Che (1993) demonstrates the optimal design of a two-dimensional (quality and price) procurement auction in which a single item is purchased and buyers are symmetric in terms of their private costs. These scoring auctions, where price and quality are chosen endogenously, have also been investigated under the context of correlated supplier costs (Branco, 1997) and multi-dimensional private information (Asker and Cantillon (2010); Asker and Cantillon (2008)). A key finding of these theoretical models is that an optimal scoring rule does not necessarily reflect the buyer's actual preferences - quality should be weighted less than its inherent value to the buyer. While these theoretical models are derived under conditions that typically do not reflect the conservation context,[2] this finding makes

[2]The "benchmark" auction model assumes bidders are risk neutral and symmetric, their values are independent, and the payment is a function of the cash bid alone (McAfee and McMillan, 1987). Conservation procurement auctions generally consider parcels with differentiated quality, based on asymmetric land endowments across landowners that yield different levels of public benefits, violating the assumption of symmetric bidders. Farmers, the primary landowner typically participating in these auctions, are generally believed to be risk-averse (Chavas et al., 2010), which introduces analytical challenges to optimal auction design (McAfee and McMillan, 1987). Conservation procurement auctions are typically designed to accept multiple contracts under a budget constraint or acreage target that may be unknown to participants, removing a key piece of information that forms the basis of participants' beliefs about their offer's acceptance probability.

it clear that there are opportunities for the buyer to influence auction outcomes by taking advantage of private information in formulating the rules of the auction.

In reaction to the limited insights into policy-relevant auction performance provided by theoretical guidance, recent research has employed simplified modeling approaches, reduced form empirical analyses of field data, and induced-value laboratory experiments to characterize auction performance under different design conditions.[3] Laboratory auction experiments have proven valuable in the design of high-value auctions when theoretical guidance is incomplete or too reductive to capture the salient features of economic phenomena, such as those assessing bandwidth allocation (Banks et al., 2003) and pollution credit (Friesen and Gangadharan, 2013) auctions. Induced-value laboratory experiments are particularly useful in asymmetric information auctions as all parameters are known, an inherent problem of the field setting. Experiments are commonly used to examine the impact of changes in auction design as a complement to game-theoretic approaches (Roth, 2002), and have contributed significant insights to the field design of conservation auctions (Hellerstein, 2010). Relevant to our auction design, Schilizzi and Latacz-Lohmann (2007) and Cason and Gangadharan (2005) find that individualized-price (discriminatory) procurement auctions are typically preferred over fixed-payment programs for conservation, though Schilizzi and Latacz-Lohmann (2007) demonstrate that repetition erodes the efficiency of the allocation - a similar finding to

Additionally, as in Cason et al. (2003) and Banerjee et al. (2014) our study assumes that the scoring rule is non-linear in price and quality, whereas quasi-linearity provides analytical tractability for the theoretical studies mentioned. Taken together, simultaneously capturing the salient features of the field context for conservation procurement extends beyond the bounds of tractable analytical modeling of Nash equilibrium bidding strategies and optimal auction design (Glebe, 2013; Schilizzi and Latacz-Lohmann, 2007).

[3]A fundamental information issue in theoretical modeling of conservation auctions is how participants form probability expectations of winning conditional on their parcels' quality and price. The complexity of the auction design, largely attributable to endowment asymmetry in a context where multiple parcels are accepted under an unknown budget, has typically been modeled with parcel quality and the probability of winning (conditional on score) exogenously entering the objective function of the bidder (Jacobs et al. (2014); Vukina et al. (2008); Kirwan et al. (2005); Latacz-Lohmann and Van der Hamsvoort (1997)). This departure from strategic Nash equilibrium modeling simplifies the decision process for the sake of tractability and is useful in retrospective empirical modeling or for projecting outcomes under the same auction rules. However, the predictions from these models are less applicable to studying changes in strategic behavior under alternative auction designs and information regimes with the goal of improving the allocation and efficiency of procurement.

Rolfe et al. (2009) in a field experiment setting. Beyond auction pricing rules, experimental research has provided insight into relaxing common assumptions about environmental quality, principally on the fronts of spatially-dependent environmental quality (Banerjee et al., 2014; Reeson et al., 2011), and to a lesser extent by treating environmental quality as an endogenous choice variable in the auction (Hellerstein and Higgins, 2010).[4] Lastly, several studies have examined the effect of changing information assumptions and the effect this has on auction efficiency when quality is exogenously determined (Banerjee et al. (2014); Haruvy and Katok (2013); Cason et al. (2003)). These studies all assess multi-round auctions under significantly different auction design assumptions; however, a common theme is that when additional information about quality is provided to sellers, auction efficiency is reduced due to rent-seeking behavior.

Our study examines the impact of withholding quality information from sellers on auction efficiency using information treatments and parameters similar to those of Cason et al. (2003). The quality information treatment in Cason et al. (2003) is conducted in the context of a multi-round auction where participants submit prices on three conservation actions, all of which are evaluated in the auction. We adopt a similar design, with two main modifications: we employ a single-round auction format that is more consistent with field conservation auctions, and we only allow participants to submit one of three potential conservation actions. These modest changes significantly alter the information available to participants in a way that also interacts with the quality information treatment. In a single-round auction, participants cannot conditionally update their beliefs about the comparative strength of their score because they do not have access to the information signals provided by provisionally-accepted offers in a multi-round auction. This uncertainty is compounded when quality becomes a choice variable in the auction, as participants cannot determine whether they are submitting low-quality conservation actions. The impact of access to quality information in

[4] Quality is introduced as a costly choice variable as a treatment in Hellerstein and Higgins (2010), with the main finding that competition drives bidders to incur costly quality improvements to improve their expected returns. This study did not employ a quality information treatment.

this more field-representative setting is unresolved.

2.2 Experimental Design

In this induced-value experiment, participants played the role of landowners reacting to the incentive structure of a conservation procurement auction in exchange for cash payouts based on their performance. Bidders competed against each other by submitting offers composed of: (1) a conservation action and (2) a corresponding price at which they would be willing to undertake that action. A selected action's quality and the offered price are countervailing acceptance criteria: higher quality and lower cost offers are most preferred by the buyer. To maximize earnings, sellers needed to judiciously balance their asking price against the probability of their offer's acceptance.

As in Cason et al. (2003), participants were presented with three potential conservation actions they could choose from, each represented as a colored item for sale, abstracting the decision context from the conservation procurement setting. Actions each had a private cost that was known only to the seller, and an ecosystem service provision level, represented as a quality index, that was known to the buyer and may or may not have been known with certainty by the seller depending on the auction treatment.[5] An auction period began with all participants observing their private information about their three potential conservation actions. Based on this information, the auction proceeded with participants selecting one of the conservation actions and an accompanying price. The submitted offers were then ranked based on the score of the selected conservation action, with score defined as quality/price. Finally, using a discriminatory pricing rule, offers were accepted in decreasing order of their score until the price of the marginal offer exceeded the conservation budget.[6] Individual net

[5]Scientific limitations in monetizing the value of environmental services from parcels has commonly resulted in the use of scientifically informed indices of ecosystem service value (Ribaudo et al., 2001), an approach adopted here to value the quality of parcels.

[6]We employed a budget-constrained versus a target-constrained (such as acreage) format based on prior findings that budget-constrained conservation procurement auctions are more robust to efficiency losses from repetition (Schilizzi and Latacz-Lohmann, 2007).

earnings in an auction period were the difference between the price and the conservation cost for accepted offers and zero otherwise. The only information participants received after each period was if their offer was selected and an updated personal cumulative earnings total.

With the goal of providing general policy guidance for a common environmental procurement auction format, the experiment parameters were not linked to a specific field context, though the random endowment generation process for cost and quality draws and the process of creating cost heterogeneity are based on approaches used in Cason et al. (2003) and Hellerstein and Higgins (2010) to ensure the results here are readily comparable to the broader literature. All costs and prices were denominated in experimental dollars. Each cost draw c_{ij}, for player i and conservation choice j, was drawn from a uniform distribution on support $\{500, 1000\}$ and each quality draw, q_{ij} from a uniform distribution on support $\{50, 100\}$. In each period, one of the conservation actions was randomly selected to receive a cost discount that varied across players. For this conservation action, a third of the players received a discount off of their initial conservation cost of 250, a third received a discount of 125, and the remaining third received no discount.[7] Participants were not made aware of the distributions from which cost and quality parameters were drawn. Each session had twelve participants across four treatments which each consisted of 12 single-round auctions.[8] The budget in each auction period was 4500 experimental dollars, a figure that was constant across treatments and unknown to participants.

The amount of information made available about the quality of potential conservation actions was varied across treatments to assess the effect of information on auction performance.

[7]This additional cost heterogeneity was introduced to assess the extent to which participants gravitated towards a conservation option that provided positive on-farm benefits, such as erosion prevention or pollination services. We chose not to pursue any hypotheses related to this question in this paper, however this variation was retained to capture this important element of field auctions (Vukina et al., 2008).

[8]The same endowments were used across treatments to minimize variance in cross-treatment comparisons. Participants were assigned to different endowments each period, and periods were randomly reordered to ensure that no participant ever received the same endowment twice and that no treatment featured the same period ordering.

In all treatments, participants knew their costs for each action and the buyer knew the true quality value for all actions and participants. In the *Quality Value* treatment, quality information for each conservation action was displayed to the sellers, and the auction proceeded as described above. In the *Quality Rank* treatment, the quality values for each conservation action were not known with certainty; instead, sellers saw a numbered ranking of their three choices based on their respective quality values. This treatment was included to approximate the type of information that landowners may rely on in the absence of provided quality information. While the nominal quality values may not be known with certainty, landowners may have a sense of the relative quality of each choice based on prior auction experience or practical experience in the field. In the *No Quality* treatment, sellers were not provided any information about the quality of their conservation choices. A fourth treatment was also included where participants were ex ante homogeneous with privately known cost and quality values. This was included to assess the potential for additionality under different information treatments, however that analysis is excluded here.

This experiment used a within-subjects design, where participants took part in all treatments. All treatments were featured once in each session, and no person was allowed to participate in more than one session. While this design increases the sample size and minizimizes the error variance associated with participant heterogeneity, it comes at the cost of potential carryover effects between treatments. Participants were given no information about the distribution of parameters and how they may vary, but since the parameterization did not change across treatments they may have been able to infer this based on the order of treatments. However, since the treatment effect is not on knowledge about the distribution of potential quality realizations, but on the random realization itself, this sequencing effect is not expected to bias results.[9] Consequently, we did not counterbalance the treatment sequencing across sessions, instead employing a randomization of treatment sequencing that

[9]To test for the presence of order effects, we conducted all of our regression analyses using a counterbalanced latin square comprised of 4 of the 12 experimental sessions (4, 8, 11, and 12), finding only trivial differences in the parameter and significance estimates. See Tables 8, 9, and 10 in Appendix B.

approximated a counterbalanced design, due to concerns about recruiting enough participants for our desired design.

Behavior in laboratory auctions has been shown to depend on participants' risk attitudes (e.g., Goeree et al. (2002); Kagel and Roth (2002)). Our experiment included a well-established exercise used to elicit participants' risk preferences (Holt and Laury, 2002). Participants were exposed to a series of paired lotteries and asked to reveal their preference for one of the two lotteries. Lottery A resulted in a higher expected payoff than lottery B for the first five paired lotteries. The lotteries had equal expected payoffs for the sixth payout combination, and lottery B had a higher expected payoff for the final five paired lotteries. Previous results indicate that framing has important implications for the reliability of such risk-assessment exercises, so the paired lotteries were presented simultaneously rather than sequentially in an effort to minimize inconsistent responses, although this presentation format is shown to reduce estimates of risk aversion (Garboua et al., 2012). For each participant, the software interface recorded the payout combination at which the individual switched from lottery A to lottery B.

Sessions began with the experimenter reading aloud the instructions (see Appendix A), during which it was emphasized that there should be no communication throughout the experiment. Following the instructions, participants completed the risk exercise. Next, participants engaged in ten practice auction periods, to become familiarized with the software interface and the basic auction design. The cost and quality distributions used in the practice periods differed from those used for data collection, which was noted in the instructions. After the practice periods, the four auction treatments were conducted. Twelve experimental sessions, each with twelve participants, were conducted at Fordham University in the spring of 2014. The 144 unique participants were undergraduates recruited from economics classes and previous laboratory experiments at Fordham. At a conversion rate of 120 experimental dollars to one USD, the mean payment was US$31 per participant, including a fixed $10 show-up payment, for sessions that lasted approximately 90 minutes.[10] This computerized auction was implemented using z-tree (Fischbacher, 2007), automating both the offer submission process and the auction outcomes.

3 Results

3.1 Auction Performance

In procurement auctions where the offered good has only one valued attribute, it is straightforward to calculate how successful the auction was for the procurer based on whether the least costly projects were selected. In our procurement auction, cost and environmental quality are both valued attributes and therefore a different metric is needed to assess auction efficiency. An appropriate measure of auction efficiency in this setting is how much quality is purchased per dollar spent, relative to the optimal quality-per-dollar ratio. This metric has an advantage over total procured quality for measuring efficiency because the budget is rarely binding in auctions for discrete conservation activities, even when it is constant across successive auctions. Subsequently, different expenditures for the optimal set versus the purchased set will yield inconsistent comparisons. Procured quality normalized by expenditures provides a common basis for comparison.

The acceptance algorithm in the auction ranks offers by score and then accepts them successively until the price of the marginal offer exceeds the conservation budget.[11] This approach maximizes quality/cost instead of total quality, subject to a budget constraint. Therefore, the optimal benchmark is constructed in the same way, ranking the endowed scores (q_i/c_i) of all conservation choices and selecting those with the highest scores iteratively

[10] The constant exchange rate between heterogeneously-endowed participants would typically leave some participants with less earning potential than others; however, participants rotated roles so that they each spent an equal number of rounds as each type, equalizing earning potential. Conversion rates were established during pilot testing of the experiment.

[11] The algorithm does not attempt to exhaust the budget by accepting inferior-score offers with prices that are less than the remaining budget.

until the next selection would exceed the budget.[12]

Three different regression models compare the effect of quality information and participant experience on auction efficiency in Table 1.[13] These regressions assume a fixed-effects, session-level error structure to control for unobserved session-level heterogeneity arising from idiosyncratic experimental effects in each session (Fréchette, 2012). The regressions are of the form:

$$Outcome_{tg} = \alpha + \mathbf{X_{tg}}\beta + \mathbf{Z_{tg}}\gamma + \nu_{tg} \tag{1}$$

where t indexes auction periods within a session and g indexes experimental sessions. $\mathbf{X_{tg}}$ is comprised of treatment indicator variables (the indicator for the *Quality Value* treatment is excluded from the regressions) that do not vary across auction periods within a session. The vector $\mathbf{Z_{tg}}$ is comprised of auction characteristics recorded at the period level, including the draws from the cost and quality supports used to determine endowments each period, the period number within a given treatment, which measures participant experience, and interactions between these terms and the treatment indicators.[14]

[12]Because this design results in variation in total expenditures per auction and there exists a dependency between total expenditures and offer variables, the potential exists for treatment comparisons to be confounded by the way information relates to the marginal acceptance criteria. However, the budget is constant, binding, and unknown to participants across all treatments, and can only be inferred via experience in the auction. A one-way ANOVA demonstrates no significant difference in mean expenditures across treatments ($p = 0.692$). Prices are found to decrease with experience across all treatments (Table 3), however an alternate specification of the price function in Table 3 that includes a one-period lagged variable of total expenditures finds no correlation between submitted prices and prior-period expenditures. Together, these results provide compelling evidence that expenditures are stationary across treatments and have no systematic effect on prices across periods or treatments.

[13]None of the presented analyses include results from experimental session 5. This is due to a participant that was submitting prices that were orders of magnitude higher than any observed across all the other sessions. This behavior occurred in one treatment across 5 of 12 auctions, but as high prices are simply non-competitive it is unlikely this meaningfully impacted auction outcomes for other participants. However, due to the magnitude of these prices they significantly change regression results and mean calculations regarding prices. Erring on the side of caution, this session is excluded from the analysis.

[14]Standard errors are clustered at the session level as opposed to the use of a multilevel model because the research question at hand is not interested with session-level effects and because the data is balanced, with the same number of auction rounds in each treatment across sessions (Gelman, 2006).

Stock and Watson (2008) demonstrates that the traditional sandwich variance-covariance matrix is inconsistent in the fixed-effects context when there are more than two time periods, even without serially-correlated errors, which can lead to incorrect inference. The standard errors reported in Table 1 are clustered at the session level and are robust to the concerns put forth in Stock and Watson (2008). While clustered standard errors have desirable large-sample properties (Wooldridge, 2003), inference may be incorrect if the researcher does not acknowledge that the group-level error follows a t-distribution with small samples (Donald and Lang, 2007). The significance of the coefficients estimated in the different models is based on a t distribution with $G - l$ degrees of freedom, where G represents the number of clusters and l represents the number of coefficients estimated in the model, an adjustment suggested by Donald and Lang (2007).[15]

With the *Quality Value* treatment as the reference, model 1 shows that reducing the amount of quality information available results in a corresponding decrease in auction efficiency, as evidenced by the significant negative coefficients on the *Quality Rank* and *No Quality* treatment indicator variables. Relative to the performance of the auction when participants have full information about the quality of their suite of conservation actions, providing participants with only rank information about the quality of these actions reduces the cost-effectiveness of the auction by 2.8 percentage points, representing a loss in efficiency of 3.2% from the *Quality Value* treatment, while completely concealing quality information from auction participants reduces the cost-effectiveness by 3.4 percentage points, or 3.9% relative to the *Quality Value* treatment.[16] The specification in model 1 is identical to a Dun-

[15] In this analysis, $G = 11$ and $l = 2$ in model 1, $l = 4$ in model 2 and $l = 8$ in model 3. The resulting critical t values at the 95% level are 2.306, 2.365, and 3.182 in models 1, 2, and 3, respectively.

[16] The worst possible outcome with this efficiency metric is 40.3% of optimal quality per experimental dollar. This is if the lowest scored conservation actions (limit one per individual) were enrolled at cost. Across all periods this always results in only 4 or 5 accepted submissions – implying that 7 or more people did not submit offers (all of which would have been more competitive). As such it is a very liberal lower bound for efficiency. While the observed efficiency gain from providing information is small relative to the potential range, a 3.4 percentage-point increase in the efficiency of $200 million in expenditures in the Conservation Reserve Program's 43rd general signup would have saved $6.8 million dollars for the same level of ecosystem service provision.

nett's test for comparing multiple treatments to a control (Williams, 1971), demonstrating significant differences in the mean efficiency of these treatments relative to the 87.4% mean efficiency observed in the *Quality Value* treatment. A pairwise comparison using Tukey's HSD test supplements these results by confirming efficiency is higher in the *Quality Value* treatment than in the other treatments; however, a significant difference is not observed between auction efficiency in the *Quality Rank* and *No Quality* treatments ($p = 0.323$).

Quality and cost endowments were drawn from the same distributions for all conservation actions in this experiment, yielding ex ante identical (conditional on cost type) endowments. These draws introduce variation that may confound hypothesis testing. To limit variation across treatments, 12 periods (one treatment worth) of cost and quality endowments for all participants were drawn and reused across treatments. For each new treatment, endowments were first reassigned within a period and then periods were reordered so that no participant ever held the same endowment twice and periods did not occur in the same order across treatments. Model 2 captures the effect of heterogeneous between-period random endowments via the *Endowment* variable, while simultaneously assessing the effect of experience on efficiency. The estimated effects of withholding quality information from auction participants on auction performance are nearly identical to those in model 1, and there are no observable endowment or experience effects.

Model 3 uses interactions between the explanatory variables in models 1 and 2 to explore the possibility that there are treatment-specific experience effects. This specification indicates that the relative inefficiency of the *No Quality* treatment is partially due to decreasing auction efficiency over time. Specifically, every three additional periods in the *No Quality* treatment reduces the cost effectiveness of the auction by approximately 1.26 percentage points relative to the *Quality Value* treatment. This means that in the 12th period of the *No Quality* treatment, the cost-effectiveness of the auction was 5.0 percentage points lower than in the *Quality Value* treatment due to these treatment-specific experience effects. While there are no significant interaction effects in the *Quality Rank* treatment, the effect of providing auction participants with only ranked quality information is responsible for a 5.7 percentage point reduction in cost-effectiveness relative to the treatment with full quality information, when controlling for the treatment-specific impacts of endowment and participant experience. To assess why efficiency decreases with less information, descriptive statistics of auction outcomes are used in the next section as an initial exploration followed by regression models of conservation action selection and offer formation.

3.2 Bidder Behavior

Significant differences in the mean quality of submitted offers are observed across treatments that establish a strictly decreasing level of submitted quality as information decreases (Table 2).[17] Offered quality in the *Quality Value* treatment is greater than that in the *Quality Rank* treatment ($p < 0.001$), and the offered quality in the *Quality Rank* treatment is greater than that in the *No Quality* treatment ($p < 0.001$). Prices across treatments form a less clear pattern. The only significant difference in the average price across treatments is that prices in the *No Quality* treatment are on average less than the prices in the other treatments (*Quality Value* - $p = 0.039$, *Quality Rank* - $p = 0.006$).

The joint effect of these findings is observed in the quality per experimental dollar spent across treatments, where the *Quality Value* treatment mean is significantly higher than that observed in the other two treatments ($p < 0.001$ for both comparisons). Notably, there is no significant difference between the quality per experimental dollar in the *Quality Rank* treatment relative to that in the *No Quality* treatment. Also, as predicted by theory, an increase in the information available to participants is associated with larger profits, as reflected in the observed higher mean profit as treatments become more informative. However,

[17]The experimental design features repeated measures across treatments on the same pool of participants in each session and therefore these pairwise comparisons violate the assumption of independence between groups. This interdependence is addressed by preceding a post-hoc Tukey test with a repeated-measures ANOVA.

while there is an ordering established by the mean values across treatments, the only significant difference is between the average profit in the *Quality Value* treatment and the average quality in the *No Quality* treatment ($p = 0.044$). Taken together, the summary statistics across treatments demonstrate that the higher efficiency in the *Quality Value* treatment is likely due to the higher mean quality of submitted conservation actions, an effect that dominates the offsetting higher mean price in this treatment relative to the two lower-information treatments.

Explaining the behavior that leads to an increase in auction efficiency with an increase in information is complicated by the nature of offer formation. The decision process for a participant in the auction consists of selecting a conservation action from the three potential choices and submitting a price for undertaking the chosen activity. The order in which these two actions are performed is not restricted by the software interface, and decisions are based on both expectations of earnings and beliefs about the probability of an offer being accepted conditional on its score. In the absence of an analytical model for guidance on how to disentangle these endogenous decisions, reduced-form regression models are used to further explain price and conservation action selection.

The conservation action choice regression is a conditional logit model, also referred to as a fixed-effects logit model for panel data. Fixed effects are included to capture unobserved, participant-specific heterogeneity. Conservation action characteristics are used as predictors for the binary dependent variable y_{itg} (1 if action is selected, zero otherwise). The model is:

$$y_{itg} = 1[\alpha + \mathbf{X_{itg}}\beta + c_g + u_{itg} \geq 0] \tag{2}$$

where u_{itg} is distributed extreme value conditional on $\mathbf{X_{itg}}$ and c_g, i indexes conservation actions, t indexes auction periods, and g indexes experiment participants. $\mathbf{X_{itg}}$ is comprised of conservation action characteristics: cost, quality, as well as indicator variables that take on a value of 1 if the action chosen is the minimum cost or the maximum quality of those available. This regression was run independently for each treatment. We cluster standard errors at the session level to allow for unobserved heteroskedasticity and serial correlation within each session. The model results (Table 3) report the marginal effect of each explanatory variable on the probability of selection evaluated at the mean of all explanatory variables under the assumption that the fixed effect is zero.

As described by Neyman and Scott (1948), maximum likelihood estimation of fixed-effects models can lead to inconsistent estimates of structural parameters. This result might suggest limiting our confidence in the marginal effects estimated using the above model. However, recent work suggests that there are certain conditions under which the above model would be preferred to the pooled logit estimator (Greene, 2004). Greene (2004) utilizes Monte Carlo methods to explore the impacts of incidental parameters on small-sample results and finds that the fixed effects estimator is biased upward in small samples, increasing the possibility of type I error, while the pooled estimator is biased downward in small samples. The magnitude of the bias is shown to be greater for the fixed-effects estimator with a limited number of time periods, with the biases of the estimators comparable once there are at least eight time periods. Given these results, we present results of the pooled logit model in table 5 in Appendix B to ensure the robustness of our results. Appendix B also presents table 6, which depicts the mean values of the conservation action characteristics across the selected and non-selected items.

Cost plays a significant role in which conservation choice is selected in all three treatments, and a higher cost leads to a lower probability of selection, *ceteris paribus*. Quality affects choice differently in the three treatments. In the *Quality Value* treatment, the continuous quality information is associated with a greater likelihood of selection when the submitted action has higher quality. In the *Quality Rank* treatment, participants do not observe the continuous quality values and so attention is focused on the action with the highest ranked quality, evidenced by the 14.6 percentage point increase in the likelihood that the action with the highest ranked quality ("Maximum Quality") is selected. In the *No Quality* treatment, participants cannot condition their selection on ranked or continuous quality and therefore react entirely to the cost information, with the lowest cost action acting as a focal point. In this treatment, an action is 24.3 percentage points more likely to be selected if it is the least-cost choice ("Minimum Cost"). These results indicate that by reducing the amount of information about quality, participants are forced to condition their conservation choice on a narrower, and less informative, set of characteristics.

Determinants of the submitted price across conservation choices and participants are identified using a model with participant fixed-effects and standard errors clustered at the session level, similar to that described in equation 2. The estimated model is specified as follows:

$$Price_{itg} = \alpha + \mathbf{X_{itg}}\beta + \mathbf{Z_{itg}}\gamma + c_g + \nu_{itg} \tag{3}$$

where i indexes submitted prices, t indexes auction periods, and g indexes experiment participants. $\mathbf{X_{itg}}$ is comprised of submitted conservation choice characteristics including cost, quality and minimum cost and maximum quality indicators, as given in the selection regression. The vector $\mathbf{Z_{itg}}$ is comprised of participant characteristics. These characteristics include experience (which measures the total number of auction periods in which the individual has participated and can vary from 0 to 47); cumulative profit, which measures the individual's earnings from all previous auction periods; and an interaction term between a risk-aversion indicator variable and the cumulative profit variable.[18] The cumulative profit variable and its interaction with the risk-aversion indicator variable are included to explore the relative importance of risk aversion and biased subjective probabilities of offer acceptance

[18]The risk aversion variable is represented by an indicator variable that takes on a value of 1 if the individual switched from lottery A to lottery B after paired lottery 6 in the Holt and Loury risk attitude assessment test (Holt and Laury, 2002), indicating risk aversion. The risk aversion indicator is time-invariant and is not identified in the model with participant fixed effects.

in determining auction behavior, an unresolved issue in the auction literature (Armantier and Treich, 2009).[19]

Table 4 presents the results of running the above regression model independently for each quality information treatment.[20] Cost plays a significant role in determining prices across all three treatments, with an increase in price of approximately 0.90 experimental dollars for each 1 experimental dollar increase in cost of the selected conservation choice across treatments. While prices are found to significantly increase with quality in the *Quality Value* treatment, participants are unable to condition their offered price on their unobserved quality endowment in the *No Quality* and *Quality Rank* treatments. However, in the *Quality Rank* treatment, participants do react to their ranked quality information. The maximum quality choice of the three potential selections significantly increases the offered price as participants seek profit based on this uncertain signal of quality. In the *No Quality* treatment, cost is the sole focal characteristic. In addition to the positive observed relationship between cost and price, the average price in this treatment decreases by 13.82 experimental dollars if the selected action has the lowest cost of the three potential conservation actions. This is an interesting result that indicates that, in the absence of quality information, participants are not simply basing their price on the nominal cost value but are also reacting to qualitative comparisons of cost between conservation choices. This nuanced view of offer formation is supported by the observation that 31.6% of the time participants do not select the lowest cost option, an unexpected result given that cost and quality are drawn randomly and independently.

Participant characteristics are consistently found to have no impact on the offered price across treatments. Within our chosen participant-level fixed effects specification, none of

[19] To demonstrate the robustness of our findings related to prices, we present the results of model 4 using Wild bootstrapped standard errors, depicted in table 7 in Appendix B, using a methodology described in Cameron et al. (2008).

[20] In this analysis, $G = 11$ and $l = 8$. The resulting critical t value at the 95% level is 3.182.

the time-varying participant characteristics have an impact on price, and we are unable to contribute to the recent literature that subjective beliefs about offer acceptance may be an important determinant of auction behavior (Armantier and Treich, 2009).

4 Conclusion

Asymmetric information in procuring conservation raises both challenges and potential opportunities. While the cost of provision may only be known to landowners, a procuring agency typically has an informational advantage in that it sets the environmental quality scoring rule by both selecting and implementing the assessment of the different ecosystem services of interest. Our study and others find that regulators may be able to affect auction outcomes through strategic control of information regarding the environmental benefits of alternative land-management actions. The important contribution of this paper is the finding that information revelation in an auction context that treats parcel quality as endogenous to offer formation increases efficiency - a new result and a finding contrary to the literature.

Rent-seeking behavior based on greater quality information results in the exclusion of high-quality parcels from the set of conserved parcels and drives losses in efficiency in Cason et al. (2003), even when participants can conditionally update their beliefs about their submission quality through the multi-round auction process. In contrast, when more information was provided to participants in our study, higher profits *and* increasing auction efficiency were observed, with a strict ordering based on the amount of information revealed. While the positive correlation between profits and auction efficiency might seem counterintuitive at first, this result is based on the ability of participants to offer higher quality conservation actions when given access to quality information. When cost and quality are uncorrelated, as was the case in this experiment, offering higher quality is a cost-less strategy for improving the likelihood of offer acceptance in a competitive auction; all else equal submitting higher quality offers raises the efficiency of the auction.

In relation to the efficiency findings of Cason et al. (2003), our unexpected findings appear to originate from making quality a choice variable, versus differences in offer variation driven by rent seeking. In Cason et al. (2003), participants submitted an offer for all three of their potential conservation actions and all of these were evaluated, whereas in this experiment participants only submitted one conservation action from a set of potential actions. The evaluation of all potential conservation actions removes the potential hazard of submitting a low quality action, a situation that is found to reduce the mean quality of submissions under the lower information treatments in our study. While increasing information drove rent-seeking by participants in our study, the extent of this behavior was not sufficient to offset the greater gains made by allowing participants to compete on quality as well as price.

The design of the current experiment resulted in a competitive auction, as roughly half of offers were accepted in any given period. Competition impacts the potential for rent seeking, as does the distribution of endowments across participants. While improving information will allow higher average quality submissions under symmetric quality distributions and independent cost and quality endowments in a competitive auction, relaxing these assumptions may not necessarily yield the same efficiency results. For example, the Conservation Reserve Program accepted nearly 90% of offers in a recent year (USDA, 2012), significantly reducing the incentives for offer competition and resulting in the use of bid caps to avoid over-payment. If rents are maximized at the bid caps for most auction participants, then the additional efficiency gains from revealing information and the associated higher quality submissions should not come at a cost of rent transfer - assuming that cost and quality are independent. Addressing the effect of information revelation in a low-competition environment is a potential avenue for further research, given the importance of the Conservation Reserve Program auction in landscape conservation.

We find that withholding quality information from auction participants makes cost the focal point of offer formation, an issue for conservation auctions that are targeting quality, especially if cost and quality are positively correlated. If low-cost parcels are systematically biased towards a specific type of ecosystem service provision, or are associated with low service provision overall, then not providing quality information may provide an inferior portfolio of services. Ultimately, socially efficient conservation procurement rests on the value of the provided public good exceeding the provision cost. Therefore, from a social welfare perspective, seller rents are a distributional consideration, not an efficiency consideration (Polasky et al. (2014); Claassen et al. (2008)). However, Stoneham et al. (2003) makes the case that conservation budgets reflect the aggregate willingness to pay of a society for environmental services and that a focus on maximizing the efficiency of conservation expenditures is a prudent approach. This rationale is reinforced by the fact that valuing the ecosystem-service provision across the landscape is a major scientific hurdle confronting the conservation science community. As such, it is quite difficult to assess the social return on investment from these programs, let alone the individual contribution of a parcel.

With tightened budgets for government agencies and increased scrutiny of government programs, there is interest among policy-makers to identify auction designs that can obtain the desired level of ecosystem-service provision at the least cost (Hellerstein, 2010). The current results demonstrate that quality information plays a role in the formation of offer strategies and that providing this information increases participants' earnings and the amount of ecosystem services purchased per conservation dollar when quality is a choice variable. Under the assumptions of our study, the provision of quality information to conservation auction participants leads to improved outcomes for both the buyer and the sellers, while avoiding the conservation of inferior quality parcels.

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Table 1: Auction Performance – Percentage of Optimal Cost-Effectiveness Ratio

	Model 1	Model 2	Model 3
Quality Rank Treatment	-0.0283***	-0.0283***	-0.0567*
	(0.0056)	(0.0056)	(0.0169)
No Quality Treatment	-0.0340***	-0.0340***	-0.0262
	(0.0056)	(0.0056)	(0.0158)
Treatment Experience		0.0001	0.0005
		(0.0007)	(0.0011)
Endowment		0.0004	-0.0008
		(0.0007)	(0.0011)
Quality Rank x Experience			0.0029
			(0.0016)
No Quality x Experience			-0.0042**
			(0.0016)
Quality Rank x Endowment			0.0015
			(0.0016)
No Quality x Endowment			0.0030
			(0.0016)
Constant	0.8741***	0.8707***	0.8759***
	(0.0039)	(0.0076)	(0.0115)
Observations	396	396	396

Notes: The dependent variable is the percentage of the optimal cost-effectiveness ratio achieved. The unit of observation is an auction period. The *Quality Value* treatment is the base case. Robust standard errors clustered at the session level are reported in parentheses, and all models include session-level fixed effects. One, two, and three stars indicate 10 percent, 5 percent, and 1 percent significance for a two-tailed hypothesis test after adjusting for the small-sample correction suggested by Donald and Lang (2007), respectively. Wooldridge tests for serial correlation in panel data fail to reject the null hypothesis of no first-order autocorrelation in each model.

Table 2: Descriptive statistics

	Quality Value	Quality Rank	No Quality
Mean quality	82.59	77.60	75.65
	(0.33)	(0.37)	(0.37)
Mean price	682.6	685.8	670.6
	(3.7)	(3.7)	(3.7)
Mean quality/price	0.1268	0.1190	0.1192
	(0.0010)	(0.0009)	(0.0011)
Mean profit	39.92	38.03	35.33
	(1.57)	(1.45)	(1.37)
Mean cost	610.7	617.7	604.7
	(3.8)	(3.8)	(3.6)
Observations	1584	1584	1584
# of accepted offers per auction	6.771	6.715	6.868
	(0.049)	(0.051)	(0.049)
Observations	132	132	132

Notes: All offers. Std. err. in parentheses

Table 3: Behavior – Conservation action selection

	Quality Value	Quality Rank	No Quality
Item Cost	-0.0003***	-0.0002***	-0.0002***
	(0.0000)	(0.0000)	(0.0000)
Item Quality	0.0024***	0.0004	-0.0003
	(0.0004)	(0.0003)	(0.0004)
Minimum Cost	0.1354***	0.1458***	0.2433***
	(0.0117)	(0.0186)	(0.0180)
Maximum Quality	0.2354***	0.0871***	0.0086
	(0.0108)	(0.0244)	(0.0143)
Participant Fixed Effects	Yes	Yes	Yes
Observations	4,752	4,752	4,752

Notes: Results are based on conditional logit models with participant fixed effects in which the dependent variable is an indicator variable denoting whether or not the action was selected. The unit of observation is a conservation action available to an auction participant in a single auction period. Standard errors clustered at the session level are reported in parentheses. One, two, and three stars indicate 10 percent, 5 percent, and 1 percent significance for a two-tailed hypothesis test after adjusting for the small-sample correction suggested by Donald and Lang (2007), respectively.

Table 4: Behavior – Offered Price

	Quality Value	Quality Rank	Unknown Quality
Selected Item Cost	0.8906***	0.9074***	0.9012***
	(0.0162)	(0.0165)	(0.0171)
Selected Item Quality	0.8067***	-0.1515	0.0018
	(0.2063)	(0.0993)	(0.0650)
Minimum Cost	0.5241	1.1303	-13.8175**
	(3.8191)	(3.1566)	(4.4871)
Maximum Quality	2.9428	12.0538***	3.8670
	(3.2269)	(2.7252)	(3.1010)
Cumulative Profit	-0.0466	0.0100	0.0182
	(0.0405)	(0.0118)	(0.0194)
Experience	0.6285	-0.0649	-0.5392
	(1.0717)	(0.6168)	(0.5373)
Risk Averse x Cumulative Profit	0.0329	-0.0178	0.0207
	(0.0353)	(0.0160)	(0.0156)
Constant	61.7110**	133.2608***	140.0185***
	(22.0788)	(26.2177)	(12.7458)
Participant Fixed Effects	Yes	Yes	Yes
Observations	1,584	1,584	1,584

Notes: The dependent variable is the price submitted by an auction participant as a part of her offer. The unit of observation is an auction period. Robust standard errors clustered at the session level are reported in parentheses. The models include participant fixed effects. One, two, and three stars indicate 10 percent, 5 percent, and 1 percent significance for a two-tailed hypothesis test after adjusting for the small-sample correction suggested by Donald and Lang (2007), respectively.

A Instructions

Lottery Period

Welcome to the experiment. This is an experiment in market decision making. If you follow the instructions carefully and make good decisions you will be well-prepared to succeed in today's experiment. In today's session you will participate in a lottery and a series of auctions. Your cash earnings today will consist of a $10 show-up payment, and payments based on your performance in the lottery and auctions.

We will first start with the lottery experiment. We will read through the instructions for the lottery together, and then proceed to the software interface.

In this part of today's session you are asked to make a choice in 11 different paired lotteries. Each lottery has different possible combinations of payoffs. Your task will be to consider each lottery and select A or B using the scroll bar to indicate a preference for taking part in sub-lottery A or sub-lottery B. Consider the payoffs associated with selecting A or B for each of the 10 choices and pick accordingly, as your selection will affect your payoff for completing this task.

After you are finished selecting A or B for the 11 choices, please press "click here to continue." At the end of the experiment when you come to collect your earnings, one of the lotteries will be selected at random to determine your payoff. These payoffs are in $, not experimental dollars like the rest of today's session.

To determine your payoff, when you come to collect your earnings for the experiment at the conclusion of the session, you will:

1. Select a card from shuffled cards numbered 1 - 11 to select the lottery that you will receive your payoff from

2. Select another card from shuffled cards numbered 1 - 10 to determine which of the two payoffs you receive from the chosen sub-lottery.

The payoff from the lottery will be added to your show-up payment and your earnings from the rest of the experiment.

Auction Instructions

We will now proceed into the next phase of today's session, where you will make decisions in an auction environment. During this part of the experiment, you will earn money in experimental dollars. At the end of the experiment, these will be converted to real dollars at a rate of 120 experimental dollars per $1 and you will be paid as you leave. This is in addition to the $10 show-up payment and your lottery earnings.

How you make money

In today's experiment, you will participate in a series of auctions. In each auction period, you will have three types of items to sell: Red, Green, and Blue items. Each item has a cost and quality, which will vary from period to period and across participants. Your values in one period are in no way linked to values in other periods. Your cost and quality values are also not linked. As we move through the instructions, you can refer to the supplemental handout for an example screenshot of the auction software interface.

In each auction period, you must choose one item that you would like to sell and the price (your "offer") that you would like to sell the item at. Your choice of item and your offer are collected via the software interface and are not known to other participants. Do not use a dollar sign when entering your offer through the software interface.

The experimenter (who is the buyer) has a limited budget and likely cannot purchase all items offered by all participants in each auction period. You can sell only one item per period, and if you sell that item, then you must pay that item's cost. If you are able to successfully sell an item in a given period, your earnings in that auction period are equal to the value of your offer minus the cost of the item sold.

$$PeriodEarnings = Offer - Cost$$

Consider the following illustrative example: if your offer of 220 for your Red item is accepted in a given period, and it has a cost of 200, your earnings that period would be 220 – 200 = 20. These period earnings are recorded and added to your "session earnings" before you move on to the next auction period. If you do not sell an item in a period, your earnings are zero for that period; you only pay an item's cost if you are able to sell that item.

Quality

The experimenter, who is the buyer, values higher quality items and uses a scoring rule to help ensure the budget is spent on high quality items. The probability your offer will be accepted is based on both the offer and the quality of the item, in addition to the quality and offers of other auction participants. To rank offers for acceptance, the experimenter turns your offer and quality information into a score using the following rule:

$$Score = Quality/Offer$$

Consider the following illustrative example in the box below. If your offer is 50 and the quality of the item is 10, then your score is $10/50 = 0.2$. Others' items are scored similarly and then they are ranked by their score. Offers are accepted at their offer value until the budget is exhausted.

Participant ID	Rank	Score	Quality	Offer	Accepted	Budget (100 to start)
3	1	0.49	27	55	Y	45
1	2	0.43	9	21	Y	24
2	3	0.29	20	70	N	-54
4	4	0.21	8	38	N	-92

In the above example table, you will see that offers are ranked not on quality or offer, but instead on their score. Items are accepted in order starting from the highest score to the lowest score, until the budget of 100 experimental dollars is exhausted. In the experiment you will not know the budget level or anything about the quality, offers, scores, and acceptance decisions of other participants.

A key thing to understand is that the numbers used in the example are for illustration only and the values and your offer choice may be completely different in the actual experiment.

Changes from the basic setup

You are now familiar with the basic design of an auction period. During the course of the experiment, some of the information on your screen may change.

The change relates to your knowledge of the quality of each of your items. In every auction period, each of your items has a quality assigned to it; however, in some periods the quality value may not be displayed even though it is known to the experimenter. In these periods, the experimenter still assigns a score based on your offer and quality and uses the score to rank bids. However, you will have to make your item choice and offer without this information.

In other periods, each item's quality will be displayed as a rank. As before, the exact number is known to the experimenter and scoring is based on the exact number. However, you will only see the quality rank of the three items.

Item	Quality
Red	27
Green	9
Blue	20

$\Rightarrow$

Item	Quality
Red	1
Green	3
Blue	2

In this example, Red has the highest quality of your three items, so it is ranked first. Blue is second highest, so it gets a rank of two. Green is the lowest quality, so it is ranked third. Again, these numbers are for example only and the values in the experiment may be different.

Summary

A period has the following order

1. Period begins
2. You select an item to sell and enter an offer price to sell that item for
3. All offers are submitted and ranked according to the scoring rule
4. Offers are accepted according to their ranked score until the budget is exhausted
5. You are notified if your offer was accepted and your earnings are calculated automatically and added to your session total
6. Period ends

Questions

How well you understand these rules and procedures are an important determinant of how much you earn in today's session. Think back over the instructions, and if you have any questions, please raise your hand now. We will conduct a practice auction next to give you an opportunity to familiarize yourself with the auction interface. None of the earnings in the practice auction will influence your cash payment today. Once the auctions begin, no talking among participants will be permitted.

B Robustness Checks

Table 5: Behavior – Conservation action selection

	Quality Value	Quality Rank	No Quality
Item Cost	-0.0003***	-0.0002***	-0.0002***
	(0.0000)	(0.0000)	(0.0000)
Item Quality	0.0021***	0.0002	-0.0006
	(0.0003)	(0.0003)	(0.0003)
Minimum Cost	0.1123***	0.1397***	0.2309***
	(0.0111)	(0.0175)	(0.0168)
Maximum Quality	0.2268***	0.0901***	0.0165
	(0.0104)	(0.0240)	(0.0139)
Observations	4,752	4,752	4,752

Notes: Results are based on a pooled logit model in which the dependent variable is an indicator variable denoting whether or not the action was selected. The unit of observation is a conservation action available to an auction participant in a single auction period. Standard errors clustered at the session level are reported in parentheses. One, two, and three stars indicate 10 percent, 5 percent, and 1 percent significance for a two-tailed hypothesis test after adjusting for the small-sample correction suggested by Donald and Lang (2007), respectively.

Table 6: Behavior – Conservation action selection (excluding session 5)

		Quality Value	Quality Rank	No Quality
Item Cost				
	Selected	668.12***	669.38***	660.83***
		(4.26)	(4.18)	(4.13)
	Not Selected	730.00	729.36	733.64
		(2.80)	(2.83)	(2.82)
Item Quality				
	Selected	79.63***	76.55***	75.08
		(0.37)	(0.38)	(0.38)
	Not Selected	72.83	74.37	75.11
		(0.27)	(0.27)	(0.27)
Minimum Cost				
	Selected	0.45***	0.45***	0.50***
		(0.01)	(0.01)	(0.01)
	Not Selected	0.25	0.25	0.23
		(0.01)	(0.01)	(0.01)
Maximum Quality				
	Selected	0.52***	0.40***	0.35
		(0.01)	(0.01)	(0.01)
	Not Selected	0.24	0.29	0.32
		(0.01)	(0.01)	(0.01)
Observations		4,752	4,752	4,752

Notes: Mean values for the characteristics of the potential conservation items are reported. The standard error for each variable is reported in parentheses. A statistically significant difference within each characteristic across selected and non-selected items at the 95% level is indicated by ***.

Table 7: Bidding Behavior – Offered Price

	Quality Value	Quality Rank	Unknown Quality
Selected Item Cost	0.8906***	0.9074***	0.9012***
	(0.0169)	(0.0173)	(0.0178)
Selected Item Quality	0.8067***	-0.1515	0.0018
	(0.2156)	(0.1038)	(0.0679)
Minimum Cost	0.5241	1.1303	-13.8175***
	(3.9899)	(3.2977)	(4.6878)
Maximum Quality	2.9428	12.0538***	3.8670
	(3.3712)	(2.8470)	(3.2396)
Cumulative Profit	-0.0466	0.0100	0.0182
	(0.0423)	(0.0123)	(0.0203)
Experience	0.6285	-0.0649	-0.5392
	(1.1196)	(0.6444)	(0.5613)
Risk Averse x Cumulative Profit	0.0329	-0.0178	0.0207
	(0.0369)	(0.0168)	(0.0163)
Constant	47.4188**	118.5635***	158.9630***
	(22.0003)	(28.3312)	(20.6280)
Participant Fixed Effects	Yes	Yes	Yes
Observations	1,584	1,584	1,584

Notes: The dependent variable is the price submitted by an auction participant as a part of her offer. The unit of observation is an auction period. Robust standard errors clustered at the session level are reported in parentheses. One, two, and three stars indicate 10 percent, 5 percent, and 1 percent significance for a two-tailed hypothesis test using the Wild bootstrapped clustered errors suggested by Cameron et al. (2008), respectively.

Table 8: Bidding Behavior – Offered Price (Order Effects)

	Quality Value	Quality Rank	Unknown Quality
Selected Item Cost	0.9179***	0.9000***	0.9111***
	(0.0174)	(0.0158)	(0.0154)
Selected Item Quality	0.9124***	-0.2138	-0.0456
	(0.1772)	(0.1710)	(0.0418)
Minimum Cost	11.5673	4.5818	-4.6466
	(7.6932)	(6.5649)	(4.8282)
Maximum Quality	6.8038	13.3161***	-0.5648
	(6.8003)	(4.5464)	(1.5454)
Cumulative Profit	-0.0068	0.0468***	0.0026
	(0.0138)	(0.0174)	(0.0451)
Experience	0.0272	-0.6114	0.5948
	(0.5321)	(1.1590)	(0.9328)
Risk Averse x Cumulative Profit	0.0220	-0.0429***	0.0021
	(0.0268)	(0.0130)	(0.0207)
Constant	-9.9675	109.9505***	70.3101***
	(49.9324)	(40.3839)	(9.2058)
Participant Fixed Effects	Yes	Yes	Yes
Observations	576	576	576

Notes: The dependent variable is the price submitted by an auction participant as a part of her offer. The unit of observation is an auction period. Robust standard errors clustered at the session level are reported in parentheses. One, two, and three stars indicate 10 percent, 5 percent, and 1 percent significance for a two-tailed hypothesis test using the Wild bootstrapped clustered errors suggested by Cameron et al. (2008), respectively. The results above are based on sessions 4, 8, 11, and 12, which have balanced treatment ordering.

Table 9: Auction Performance – Percentage of Optimal Cost-Effectiveness Ratio (Order Effects

	Model 1	Model 2	Model 3
Quality Rank Treatment	-0.0272**	-0.0272**	-0.0734***
	(0.0116)	(0.0117)	(0.0239)
No Quality Treatment	-0.0369***	-0.0369***	-0.0284
	(0.0117)	(0.0118)	(0.0171)
Treatment Experience		0.0003	-0.0001
		(0.0006)	(0.0016)
Endowment		0.0004*	-0.0007
		(0.0002)	(0.0010)
Quality Rank x Experience			0.0045
			(0.0038)
No Quality x Experience			-0.0032**
			(0.0013)
Quality Rank x Endowment			0.0026*
			(0.0015)
No Quality x Endowment			0.0019
			(0.0016)
Constant	0.8837***	0.8792***	0.8892***
	(0.0050)	(0.0079)	(0.0130)
Session Fixed Effects	Yes	Yes	Yes
Observations	144	144	144

Notes: The dependent variable is the percentage of the optimal cost-effectiveness ratio achieved. The unit of observation is an auction period. The *Quality Value* treatment is the base case. Robust standard errors clustered at the session level are reported in parentheses, and all models include session-level fixed effects. One, two, and three stars indicate 10 percent, 5 percent, and 1 percent significance for a two-tailed hypothesis test using the Wild bootstrapped clustered errors suggested by Cameron et al. (2008), respectively. The results above are based on sessions 4, 8, 11, and 12, which have balanced treatment ordering.

Table 10: Behavior – Conservation action selection (Order Effects)

	Quality Value	Quality Rank	No Quality
Item Cost	-0.0003***	-0.0003***	-0.0001***
	(0.0000)	(0.0000)	(0.0000)
Item Quality	0.0026***	0.0004	0.0005
	(0.0004)	(0.0006)	(0.0004)
Minimum Cost	0.1114***	0.0939***	0.2173***
	(0.0127)	(0.0182)	(0.0218)
Maximum Quality	0.2613***	0.1059***	-0.0146
	(0.0221)	(0.0370)	(0.0186)
Participant Fixed Effects	Yes	Yes	Yes
Observations	1,728	1,728	1,728

Notes: Results are based on conditional logit models with participant fixed effects in which the dependent variable is an indicator variable denoting whether or not the action was selected. The unit of observation is a conservation action available to an auction participant in a single auction period. Standard errors clustered at the session level are reported in parentheses. One, two, and three stars indicate 10 percent, 5 percent, and 1 percent significance for a two-tailed hypothesis test, respectively. The results above are based on sessions 4, 8, 11, and 12, which have balanced treatment ordering.