Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01215

Behavioral Biases among Producers: Experimental Evidence of Anchoring in Procurement Auctions

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Abstract

Experimental research in behavioral economics focuses on consumer behaviors. Similar experimental research on profit-maximizing producers is rare. In three field experiments involving commercial agricultural producers in the US, we detect evidence of anchoring in competitive auctions for conservation contracts related to nutrient and pest management that were worth, on average, nearly nine thousand dollars. In these auctions, the value of the starting cost-share bid was randomized to be either 0% or 100%. When the starting value was 100%, final bids were 46% higher, on average. We find weak evidence that experience with conservation contracts may modestly attenuate the anchoring effect.

JEL Codes: D91

Keywords: Randomized Controlled Trial, Behavioral Anomalies, Defaults, Producers, Choice Architecture, Replication

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For funding support, the authors thank the United States Department of Agriculture National Institute of Food and Agriculture (2019-67023-29854; 2017-67024-26278), Economic Research Service (59-6000-4-0064; 58-6000-1-0037), and the Office of the Chief Economist (58-0111-18-003). For implementation and research assistance, the authors thank Mark Masters and Linda Means from the University of Albany; Kristin Rowles from Policy Works; J. Dustin Tracy from Augusta University; Sean Ellis, Michael Griner, Nicholas Janusch, Kaitlynn Ritchie, Olesya Savchenko, and Madeline Valinksi from the University of Delaware; Marisa Henry, Ben Meiselman, Panayiota Petrou-Zeniou, and Katie Tam from Johns Hopkins University.

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1 Introduction

Introductory microeconomics is typically divided into consumer and producer theory. In introductory psychology, however, no such distinction is made: a behavioral theory applies equally to individuals making personal decisions about their retirement savings as it would to individuals who own a business and make decisions about input expenditures. Thus, in principle, behavioral economic theories about systematic and predictable deviations from the traditional economic model should apply equally to both consumers and producers. Empirical support for that assertion, however, is equivocal.

Among consumers – i.e., utility-maximizing decision-makers – the experimental evidence in support of behavioral economics is extensive. However, the power of behavioral economic theories to explain the behavior of profit-maximizing producers may differ for at least three reasons. First, producers typically compete in markets, and market experience could eliminate behavioral anomalies (Alevy et al. 2015; List 2003; List 2011). Second, the stakes associated with producer decisions are often much greater than the stakes associated with consumer decisions in laboratory and field studies. When the consequences of participants' choices are limited, participant attention and cognitive effort during decision-making may be low, thus making consumers more prone to behavioral biases (Levitt and List 2007; although see Enke et al. 2020). Third, producers engage in repeated transactions in the same choice context, which often gives them greater experience and expertise in their decision contexts than consumers. The domain expertise and familiarity of producers with the choice context may help them avoid behavioral biases. Whether any of these three reasons moderate the power of behavioral economic theories to explain producer behaviors is an empirical question.

The evidence in support of behavioral economic theories among producers is almost exclusively non-experimental (e.g., Beggs and Graddy 2009; Camerer et al. 1997; Coval and Shumway 2005; Gao et al. 2018; McAlvanah and Moul

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2013). We discuss the limitations of this non-experimental literature and the need for experimental evidence in the following section.

We study producers' behaviors in competitive auctions in which we can determine whether the bidders are susceptible to one of the most intensively studied behavioral "anomalies" among consumers: anchoring (Furnham and Boo 2011). An anchor in an auction is an arbitrary value in the decision environment that affects the bidding decision. Anchoring bias in judgment implies that once "anchored" to an arbitrary value, bidders tend to make numerical estimations that remain close to the value of the anchor (Jacowitz and Kahneman 1995; Tversky and Kahneman 1974). The bidders in our sample are experienced commercial agricultural producers in the United States. In competitive procurement auctions, producers competed for contracts that related to on-farm nutrient and pest management and cost nearly \$9000 per farm, on average. The auctions were embedded in agri-environmental programs that, as in most agri-environmental incentive programs in the US, were run as "cost-share" programs. In cost-share programs, the procurer and the bidder share the total cost of contract implementation, i.e., the bids are expressed as the percent of the total cost that the producer is willing to pay, varying from 0% to 100%. In the auctions, bidders made cost-share offers (bids) for contracts that provide them with cash assistance for implementing technologies or land-use practices that provide private and public benefits (an impure public good).

The anchor in our study is the starting bid viewed by producers, which was randomly assigned to be 0% for half of the participants and 100% for the other half. If the bidders in the auctions are fully informed, cognitively unbounded, profit-maximizing producers, the starting bid value should have no impact on final bids. Producers' varying levels of experience participating in conservation cost-share programs also allowed us to estimate whether experience with conservation contracts moderated any anchoring effect (i.e., whether an anchor has a greater impact on inexperienced bidders). Moreover, given concerns about the replicability of social science experimental research

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in general (Camerer et al. 2018) and anchoring effects in particular (Maniadis et al. 2014), we report results from three different procurement auctions that have similar structures but differ in the bidder pools, the contracts on which bidders make offers, and the years in which they were conducted.

We find that, on average, bidders who were shown a starting bid value of 100% submitted final cost-share bids that were 8 percentage points higher than bidders who were shown a starting bid value of 0% (95% confidence interval (CI) [4, 12]), which equates to a 46% increase in the average bid. Based on the average total contract cost, the high anchor induced producers to contribute an additional $565 toward the cost of implementation. Although the point estimate for the treatment effect is larger among inexperienced bidders (9 vs 7 percentage points), we cannot reject the null hypothesis of equality among the two groups. However, we do see heterogeneity in the estimated treatment effects across auctions, ranging from 3 to 28 percentage points. This heterogeneity highlights the importance of replication, both to increase the precision of the estimated effect and to avoid exaggeration bias in published literature.

Like prior studies on anchoring, we cannot isolate the mechanisms through which the anchor affects behavior. The starting bid value may have affected behavior subconsciously through, for example, a cognitive adjustment process or consciously through, for example, a perception that it provided an informative signal about the optimal bid. Alternatively, strategic bidders could view the anchors as signals of the auction organizers' preferences, which may influence bidder actions if they believe they are in a dynamic game with the auction organizers who may reciprocate in the future by providing more benefits to successful bidders. Thus, one should not infer that the observed anchoring effect necessarily implies a violation of profit maximization by the producers.

In the next section, we describe how our study contributes to the behavioral science literature. In the third and four sections, we describe the experimental designs and report the estimated treatment effects from the auctions individ-

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ually and overall. We conclude with a discussion of the implications of our results for the validity of behavioral economic theories applied to producers and for programs and policies aimed at changing choice architectures for producer decisions.

2 Behavioral Literature on Producers

Experimental evidence of behavioral biases among producers is rare, particularly for commercial producers in high-income nations that may be the best match for the archetype of a profit-maximizing or cost-minimizing producer who operates in a competitive environment.

Complementary experimental evidence among producers is important because, in non-experimental studies, it is challenging to distinguish between behavioral deviations from traditional economic theory and contextual deviations from a study-specific theory (e.g., are agents present biased or are there unobserved constraints or a misspecified objective function?). For example, in an observational study of competitive auctions in which experienced wholesale dealers bid on used cars, Lacetera et al. 2016 report that auctioneers could influence the auction outcomes. The authors speculate that these auctioneer effects arise from auctioneers exploiting behavioral biases among car dealers by anchoring bidders to reference points, creating bidding frenzy, fear of loss, and rivalry. However, these auctioneer effects are inferred from regression coefficient estimates on auctioneer dummy variables, which may also capture unobserved heterogeneity in the auction contexts associated with different auctioneers. In a similar example, Farber 2015 reexamines the work of Camerer et al. 1997 on New York taxi drivers' labor supply decisions being reference dependent. Using the complete record of all trips taken by NYC taxi drivers between 2009 and 2013, Farber 2015 finds little evidence of reference dependence and concludes that the results are consistent with neoclassical optimization models of labor supply. Similarly, DellaVigna and Gentzkow 2019

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observe suboptimal pricing decisions among US retail chains but acknowledge the identification challenge stating that these deviations from traditional economic theory may arise because of factors that are not behavioral biases (e.g. brand image concerns). Prior non-experimental studies of producers have identified anchoring effects among bookmakers in Australian horse racing (McAlvanah and Moul 2013), collectors at art auctions (Beggs and Graddy 2009), and institutional bidders in Chinese auctions for initial public offerings (Gao et al. 2018). However, isolating anchoring effects is challenging in studies that cannot exogenously manipulate the anchor.

Prior experimental studies on behavioral biases among producers suffer from at least one of two issues. First, in publications that study efforts to "nudge" producers using messages (e.g., social comparisons, reminders), the messages are sent from regulatory authorities (e.g., Holz et al. 2020; Mascagni et al. 2018). In such cases, it is unclear if the post-message higher compliance rates are a deviation from traditional economic theory or a reasonable response to a perceived change in the probability of audits and penalties. Second, in experiments run in business contexts, the human subjects are not necessarily best characterized as "producers." Many of the experiments studying the behavioral economics of organizations randomize treatments within organizations rather than across them, i.e., they study the behaviors of employees or customers who affect an organization's profits rather than behavior of the organization itself (e.g., Alevy et al. 2007; Drehmann et al. 2005; Haigh and List 2005; see reviews in Duxbury 2015a; Duxbury 2015b). Yet whether the incentives of these decision-makers are aligned with the incentives of the owners of the means of production is unclear. In other words, these experimental subjects may not be best characterized as profit-maximizing producers. Scholars have also used experiments to test behavioral nudges to improve farmer decision-making in developing countries (e.g., Duflo et al. 2011). But the decisions of farmers in these countries are often modeled using agricultural household production models due to the inseparability of consumption and production decisions (Ahn et al. 1981) and none of the authors claim that their subjects

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https://doi.org/10.1162/rest_a_01215

are profit-maximizing producers. Physicians who run their own practices may be best characterized as producers, but many of the behavioral science experiments that have physicians as subjects either use hospital physicians, who may be better characterized as employees, or do not describe the contexts under which the physicians make their decisions (e.g., see list of experiments with doctors in the review paper by Wang and Groene 2020).

In our experiments, we study business owner-operators, rather than employee representatives of a business, which ensures that the objectives of the experimental decisionmakers are aligned with the objectives of the business – in other words, it ensures we are studying the behaviors of producers rather than of agents in a principal-agent framework. Studying owner-operators also offers another advantage for interpreting experimental results. Group decision-making has been reported to be more consistent with traditional economic theories than individual decision-making (Kugler et al. 2012). Thus, regardless of whether decisionmakers are producers or consumers, decisions by groups may differ from decisions made by individuals. By design, we eliminate a simple "group decision-making effect" as a rival explanation to a "producer effect" because we study owner-operators rather than more complex business organizations with hierarchies and bureaucracies.

3 Auction Experimental Design

The producers in our study have ample experience with production operations and long-term exposure to competitive pressures in both their normal business operations and in the high-stakes auctions that are the focus of our study. Most commercial farmers in the US experience tight operating profit margins in highly competitive agricultural markets (Hoppe 2015; Hoppe 2014) and regularly participate in auctions (e.g., farm implements, land, livestock etc.). When economists model US commercial farmers, they model them as profit-maximizing producers. For example, in 2020, the American Journal of

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Agricultural Economics published 11 articles that include a model of US commercial farmer decision making. Of the 11 articles, 8 (73%) modeled farmers as profit-maximizing producers. The producers in our study had operated their commercial farms for an average of 23 years and farmed an amount of land above the national average (National Agricultural Statistics Service 2020).

Agricultural production generates negative externalities, including water pollution and habitat loss (Tegtmeier and Duffy 2004). To reduce these externalities, government agencies and non-governmental organizations have created programs that give producers a financial incentive to adopt on-farm conservation practices to reduce soil erosion and prevent water pollution. Many of the practices are assumed to be impure public goods benefiting both the producer and the public. Thus, producers are expected to bear part of the cost of implementing the measures. From 2014 to 2020, the US Department of Agriculture and its partners operated conservation cost-share programs with a total budget of $38.7 billion[1].

In the cost-share programs in our study, producers made cost-share offers (bids) for contracts that would provide them with cash assistance for implementing technologies or land-use practices that provide private and public benefits (an impure public good). The technologies and practices were aimed at nutrient (fertilizer) and pest management. For example, farmers could choose to install in-ground filters (a technology) or vegetative buffer strips (a practice), which retain farm soil (private benefit) and remove fertilizer runoff before it enters and pollutes surface waters (public benefit).

[1] USDA Economic Research Service website: https://www.ers.usda.gov/topics/natural-resources-environment/conservation-programs/

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3.1 StewardShares I and II: Auctions for Nutrient Management Practices

In 2014, the University of Delaware developed the StewardShares program[2], which incentivized Mid-Atlantic agricultural producers to adopt practices and technologies that reduce soil losses and nutrient run-off that pollute surface waters and coastal areas (Carpenter et al. 1998). The StewardShares program offered producers cost-share contracts for nine practices: six riparian buffers along surface water boundaries to intercept runoff, two in-stream filters to remove phosphorous pollution, and the demolition and remediation of abandoned poultry houses that leach fertilizer into waterways. Extension agents selected those practices because prior studies had shown that they effectively reduced nutrient runoff and improved water quality. To avoid competing with other agricultural cost-share programs, the StewardShares program selected variations of the practices that were not eligible for federal, state, or local programs at the time (e.g., StewardShares offered forest buffers of 15 feet and 30 feet in width when other programs required 50 feet or more).

Through StewardShares, commercial agricultural producers who were large enough to be subject to nutrient management regulations were invited to bid in first-price, sealed-bid online auctions. The first auction (StewardShares I) occurred in 2014 and the second (StewardShares II) in 2016. Though not as widely used as posted-price mechanisms, auctions have been used by some agri-environmental cost-share programs in the Mid-Atlantic region. For instance, the Delaware Agricultural Lands Preservation Foundation's incentivized auction for agricultural easements has been operating for more than 20 years (Messer and Allen 2010). Bids placed in StewardShares auctions represented the "percentage cost-share" producers were willing to contribute. For instance,

[2]The full program name was the Agricultural Values, Innovation, and Stewardship Enhancement program, or AgVISE program. However, after the third auction was run, a commercial company that had trademarked the AgVISE acronym issued a cease-and-desist letter and the parties agreed to an alternative shortened name of the program: StewardShares.

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if the cost to install a phosphorus filter was $20,000 and the producer submitted a cost-share bid of 25%, the producer agreed to pay $5,000 and the StewardShares program would pay the remaining ($15,000). The minimum bid was 0% (StewardShares paid the full cost) and the maximum bid was 100% (the producer pays the full cost). The producer could also choose to not place a bid on a practice. For StewardShares I, 356 producers went to the online auction interface to learn about the auction and 121 of those producers placed bids. For StewardShares II, 181 producers went to the online auction interface to learn about the auction and 57 of those producers placed bids. More details on recruitment can be found in the Appendix Table A3.

For each practice on which the producer wished to bid, the producer chose the number of units (e.g., feet of riparian buffer, number of phosphorous filters) and submitted a single cost-share bid. Per-unit costs to implement the measures in each state were established through negotiations with local firms that agreed to install the measures at the negotiated prices. Producers were given the prices and names and contact information for the firms that would install each measure. They were further informed that they could bid on as many contracts as they wished, treating each contract as independent. Producers were told that the total program budget for the auction (the amount the program would pay) was $40,000 for StewardShares I and $100,000 for StewardShares II.

Instructions for the auctions noted that the submitted bids on all practices would be pooled and ordered from highest producer cost-share to lowest producer cost-share. Then, starting with the highest bidder, the contracts would be awarded until the program budget was exhausted. When the expenditure required by the next-highest bid exceeded the remaining budget, the program would move down through the list to the highest affordable (within-budget) bid and award that contract. This process was repeated until no within-budget bids remained or the budget was exhausted.

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As mentioned above, when considering their bids, the participants could choose the quantity of a practice to implement (number of feet of buffer, number of phosphorus filters, number of poultry houses to remediate) and the computer interface would automatically display the total cost of implementing it. Below the display of the total cost was a cursor-controlled slider that participants used to select the amount of their cost-share. Moving the slider displayed the producers' dollar cost for the displayed percentage and the dollar value paid by the StewardShares program. Participants could revise the quantity and cost-share percentage as many times as they wished before finalizing their bids.

The starting bid value displayed on the slider was randomly assigned across subjects to either 0% or 100%. Each participant was presented with the same starting position for every practice offered, so approximately half of the participants were shown a starting cost-share value of 100%. Figure A1 in the Appendix shows examples of bid screens presented to participants during the three experiments. The participants were not aware of the random assignment of the starting bid values. Table A1 in the Appendix reports the descriptive statistics of the treatment and control groups and the covariate balances between them. As expected, given the sample sizes and treatment randomization, the differences in covariate values across the two groups are no greater than would be expected by chance.

3.2 StewardShares III: Auction for a Feral Hog Trapping System

Non-native feral hogs are one of the most widespread and destructive invasive pests in the United States. They destroy crops, prey on small livestock, and compete with native wildlife for food, costing an estimated $1.5 billion per year

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for control efforts and damages[3]. Feral hogs also adversely affect soil quality, causing nutrient runoff into nearby streams, thus undermining water quality (Siemann et al. 2009). To control feral hogs on agricultural lands, trapping systems are recommended (Ellis et al. 2020; West et al. 2009).

In 2016, StewardShares III implemented a cost-share program for a state-of-the-art trapping system from a private firm, Jager Pro[4]. Along with the usual hardware required for a feral hog trap, the system start-up package included a remote-controlled gate, a motion-activated camera, and one year of remote camera and gate cellular service. For a pre-negotiated price of $4,000, Jager Pro experts installed and trained the producers to use the system. The program ran from October 2016 through December 2018. Some producers were recruited both in person at agricultural expositions and via mailed invitations and others were recruited solely by mail (see Table A3 in the Appendix for recruitment details). While nearly all of the StewardShares I and II participants were from the mid-Atlantic region of the United States, participants in StewardShares III were largely from the Southeastern region.

The design of the StewardShares III auction was similar to the design of StewardShares I and II. First, participants were given information about the hog trapping system and the auction. They then chose whether to submit a cost-share bid for a single trap in a discriminative price, sealed-bid auction. Bids were submitted using the same slider format and randomly assigned starting bid values of 0% or 100%. Similar to StewardShares I and II, producers could choose to not place a bid. For StewardShares III, 1095 producers went to the online auction interface to learn about the auction and 482 of those producers placed bids. Table A2 in the Appendix provides the descriptive statistics of the treatment (100% anchor) and control (0% anchor) groups and

[3]Data from the U.S. Department of Agriculture website (www.aphis.usda.gov/aphis/resources/pests-diseases/feral-swine/feral-swine-damage). Researchers at the University of Georgia estimate that the overall cost may be closer to $2.5 billion annually (https://www.cnbc.com/2018/08/03/hogs-run-wild-but-usda-doubling-efforts-to-fight-problem.html)

[4]A brief video of the program can be found at www.youtube.com/watch?v=ikszxgNCgiA

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the covariate balances between them. As expected, given the sample sizes and treatment randomization, the differences in covariate values across the two groups are no greater than would be expected by chance.

3.3 Estimand and Hypotheses

We first estimate the average treatment effect of the anchor in each auction. Then we estimate an overall treatment effect using the pooled producer-level bid data. The auctions can be pooled in a meta-analysis because they all use the same treatment and auction structure and vary only by the invitation procedures and the contracts on which producers made bids. Thus, the experiments can be viewed as replications. Given the variations in the auctions, they are not so-called "pure" replications; instead, they test the same construct in analogous but different decision environments.

In addition to estimating the average treatment effect of the anchor in each experiment, we pose two hypotheses for testing in the experiment:

Null Hypothesis 1: The submitted bid, on average, is not affected by the anchor starting bid value (the average treatment effect of the starting bid value is zero).

Based on results from prior behavioral studies of consumers, the alternative hypothesis is that a 100% starting bid value leads to higher average bids than a 0% starting value (the average treatment effect of moving the starting bid value from 0% to 100% is positive). Although the hypothesis is one-sided, we subject it to a two-tailed hypothesis test to be conservative in our statistical inferences.

Null Hypothesis 2: The average effect of the starting bid value is not affected by producers' prior experience with conservation incentive contracts.

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Based on prior studies suggesting that behavioral "anomalies" are attenuated by experience, the alternative hypothesis is that the average effect of the starting bid value is reduced when producers have more experience. Although this hypothesis is one-sided, we subject it to a two-tailed hypothesis test to be conservative in our statistical inferences.

To test Hypothesis 2, we estimate conditional average treatment effects for subgroups of experienced and inexperienced bidders. To measure their experience with the kinds of contracts offered in the auctions, we added two questions to the post-auction survey completed by all participants. The first question asked participants if they had ever participated in the Conservation Reserve Program (CRP), which uses a procurement auction to allocate about US$2 billion/year for conservation contracts (temporary land retirement and some land enhancements). The second question asked participants if they had ever participated in any other federal, state, or local conservation incentive program, nearly all of which are run as cost-share programs but use posted prices (share percentages) rather than auctions to allocate contracts. We use the answers to these questions to define previous experience as participation in one of the three categories of programs: (1) the Conservation Reserve Program (CRP), which uses an auction to allocate funds for conservation contracts, (2) any other federal, state, or local conservation incentive program, nearly all of which are run as cost-share programs but use posted prices (share percentages), or (3) StewardShares I. Table A6 in the Appendix provides details about the sample of bidders with various types of previous experience of participating in these three categories.

3.4 Estimation

The outcome variable is a participant's cost-share bid, which was expressed as the fraction of the total cost of implementing the contract or practice. The

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values of the variable thus lie in the range [0, 1]. After presenting the distribution of bids by treatment arm, as well as their mean values and confidence intervals, we use a regression estimator to estimate the average treatment effect, the conditional average treatment effect (experience), and the standard errors of the estimates. We estimate the overall effect from all three experiments using a meta-analysis with the pooled bidder-level data. No bidder observations from any of the three experiments were dropped for any of the analyses.

Since the bids take values in the range [0, 1], we estimated average treatment effects using a generalized linear model with a binomial distribution and a logit link function, also called a fractional logit model. Papke and Wooldridge 1996 showed that the fractional logit estimator performs better than other methods with continuous [0, 1] variables because it accounts for the boundedness of the dependent variable, capturing non-linearity in the data and yielding a better fit than linear estimation models.

Formally, we model the participants' cost-share bids Y_i as a function of the treatment variable T_i and other participant characteristics $\mathbf{X_i}$:

$$g\left\{E(Y)\right\} = \beta_1 T + \gamma \mathbf{X}, \;\; Y \sim F \tag{1}$$

where $g(.)$ is the link function and F is the distributional family. Our main estimation results use a logit link function with a binomial distribution:

$$logit\left\{E(Y)\right\} = \beta_1 T + \gamma \mathbf{X}, \;\; Y \sim Bernoulli \tag{2}$$

To increase the precision of the estimates, the regression specification includes the participant characteristic variables (shown in Tables A1 and A2 in the Appendix), and categorical variables for their state of residence and auction item. In the pooled sample, we also add categorical variables for the StewardShares auction (I, II, or III). As shown in Table A5 in the Appendix, our results were similar when employing ordinary least squares (OLS). Results

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from both GLM and OLS remain similar with and without controls. Since every participant can bid on more than one item, we cluster standard errors at the bidder level.

To estimate the conditional average treatment effect of market experience, we augment the regression model in equation 2 by including an interaction term of the treatment variable with a binary indicator taking a value of one when the participant had previous experience with conservation programs and a value of zero otherwise. A negative coefficient on this interaction term would be consistent with the hypothesis 2 alternative, which predicts that the average treatment effect is lower for participants with previous experience with enrolling in conservation programs.

Unlike an auction in laboratory experiments in which participants are typically required to submit bids, the StewardShares auctions, like all field auctions, allowed participants to choose not to bid on a contract. The participants were randomly assigned to either treatment or control when they arrived at the auction landing page after agreeing to participate in the auction. The design of our experiments mitigates possible endogenous selection bias because the participants had to decide whether they wanted to bid on a practice or technology before seeing the slider starting value for that contract. However, participants could change their minds after landing on the bidding screen, return to change their response, and decline to bid. If the randomly assigned bid starting value on the screen affected the likelihood of participants placing bids, our estimator could potentially be biased. We tested for this potential selection effect using observations from the participants who viewed the contract information screen (bidders and non-bidders). In a logistic regression model, we regressed a binary variable for placing an eligible bid on the treatment variable T_i. Table A4 in the Appendix presents the results of this estimation. We found no evidence that the treatment had an effect on the extensive margin of a participant's likelihood of placing a bid.

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4 Results

Effect of Starting Bids on Final Bids: Data Visualization

Panel A of Figure 1 shows sample distributions of the cost-share bids in each StewardShares program and for the pooled data. The density plots for the 0% starting bid values skew toward lower cost-shares relative to the density plots for the 100% starting bid values. As shown in Panel B of Figure 1, the mean bids are higher for bidders who faced the 100% starting value anchor rather than the 0% starting value anchor.

Effect of Starting Bids on Final Bids: Regression Estimates

Covariate-adjusted estimated average effects of the anchor treatment from the regression estimators are presented in Figure 2. The corresponding regression table is presented in Table A5 of the Appendix. The table also shows estimates using GLM and OLS, with and without controls. The coefficients remain stable across all specifications. To make interpretation easier, we converted the coefficients on the treatment variable from the regressions to their average marginal effects. These estimates are similar to the simple differences in the means shown in Figure 1.

Consistent with the alternative Hypothesis 1, the estimated average treatment effects in the individual auctions and in the pooled dataset are positive. In StewardShares I, the average bids by participants shown a starting value of 100% are an estimated 7.6 percentage points higher than the average bids by the participants shown a starting value of 0%. In StewardShares II and III, the estimated average effects are 26.0 percentage points and 2.5 percentage points, respectively.

The overall estimated effect using the pooled data is an increase of 8 percentage points (95% CI [4.4, 11.7]) which equates to a 46% increase in the

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average bid. Given the average cost of the implemented projects on which participants placed bids, this estimated treatment effect is equivalent to an average difference of $565 in the final bids that arises from the change in the position of the starting value on the slider.

Effect of Market Experience on the Treatment Effect

Consistent with the alternative Hypothesis 2, the point estimate on the interaction of the anchor and experience is negative. Prior experience with conservation contracting, on average, attenuated the average effect of the anchor treatment (see Figure 3; Table A6 in the Appendix presents the corresponding results in tabular form). However, the difference in the anchoring effect for experienced and inexperienced participants is small (2.1 percentage points) and imprecisely estimated (95% CI [-0.04, 0.09]). Therefore, we cannot reject the null hypothesis that the effects for the two groups are equal. We can, however, conclude that market experience does not eliminate the anchoring effect: we can reject the null hypothesis of zero anchoring effect for both subgroups.

5 Conclusion

In three procurement auctions with commercial farmers who have, on average, two decades of experience in the competitive US agricultural market, we detect an anchoring effect from the starting bid value. Prior experience with conservation contracts like the ones in the auction may have a modest moderating influence on anchoring (the estimated effect of experience is small but imprecisely estimated), but it does not eliminate the anchoring effect.

We know of only two published behavioral field experiments whose subjects are agricultural producers in high-income countries. In the US, Wallander et al. 2017 study the effects of a reminder message about a sign-up period for a government program and the same reminder message augmented with one of

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two social comparison messages. The subjects in this experiment were a mix, in unknown proportions, of operators (farmers) and non-operating landowners (non-farmers) who rent their land to operators. The authors report that, for a subgroup of recipients, the messages boosted participation by about 1.5 percentage points, with no difference in treatment effects detected across the messages (suggesting the reminder alone drove the increase in participation). The study does not differentiate treatment effects by operators and non-operators. In France, Chabé-Ferret et al. 2019 study the effects of social comparisons on irrigation use by farmers. They cannot detect a treatment effect, but given the variance in their outcome measure, their design may be underpowered to detect the typical effect sizes found in social comparison experiments (i.e., <0.10 SD).

Our results suggest that behavioral science-inspired interventions can be effective in programs aimed at influencing experienced profit-maximizing agents in competitive environments. In the specific context of agri-environmental programs, the results imply that program administrators could make inexpensive changes to decision environments to generate greater environmental benefits under limited budgets. If our estimated effect size were generalizable to other cost-share programs, the cost-savings for environmental agencies from changing the starting bid value would be substantial. From 2014 to 2020, the US Department of Agriculture and its partners spent more than $38 billion on conservation cost-share programs. The 8-percentage point improvement in the share offered by producers scaled to USDA's $38 billion in expenditures represents savings of over $3 billion.

With only three experiments, we cannot offer any insights into the potential sources of heterogeneity in the estimated effects across experiments. The effect of the starting bid may have been smaller in StewardShares III because of the simpler decision environment in that auction. In StewardShares III, participants considered only one contract rather than the nine contracts offered in StewardShares I and II. However, the recruitment methods and the geographic regions from which the two sets of samples were drawn also dif-

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fered, as did the attributes of the contracts and the year in which they were conducted. An experiment that could randomly vary those features would be complex and expensive. We must instead wait for additional field experiments with producers to obtain insight into the drivers of heterogeneous responses to anchors or other changes in the choice architecture of auctions. However, our replication of the anchor treatment in three auctions allows for greater confidence in the internal and external validity of the estimates of average anchoring effects.

Our results may not generalize to other behavioral anomalies or to other producer groups. For example, our sample comprised owner-operated commercial farms. Organizationally more complex producers that comprise groups of individuals, particularly groups situated in bureaucracies with standardized practices, may behave differently.

Despite these limitations, our study has important implications for behavioral economics and for policy. For behavioral economics, it suggests, as others have argued, that theories from behavioral economics may be as relevant to the decisions of profit-seeking agents as they are to the decisions of utility-seeking agents. For policy, it suggests that the reported successes from inexpensive changes to the decision environments of consumers may be generalizable to the decision environments of producers.

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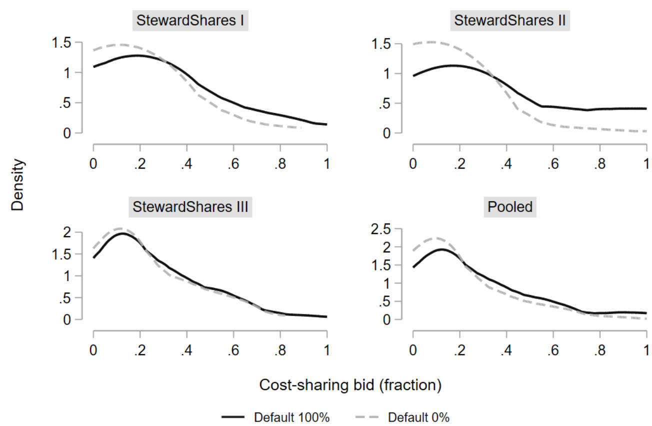
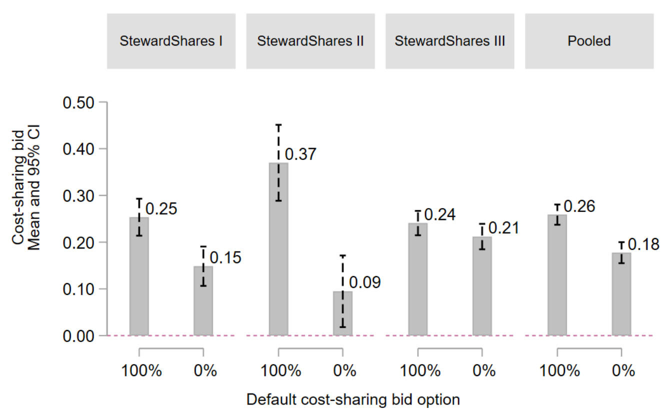
Tables & Figures

Figures

FIGURE 1: Descriptive Plots of Outcome Variable: Density and Mean of Cost-Share Bids Placed by Participants (measured as a fraction of total project cost)



(A) Kernel density distribution of cost-sharing bids



(B) Mean Cost-Share Bid Values in the Treatment Group (100%) and Control Group (0%)

Notes: Panel (A) shows kernel density plots for the cost-share bids expressed as the fraction of the total project cost borne by the bidder. Panel (B) shows the mean values of the bids with 95% confidence intervals indicated by the dashed lines. The numbers above each bar report mean bid values.

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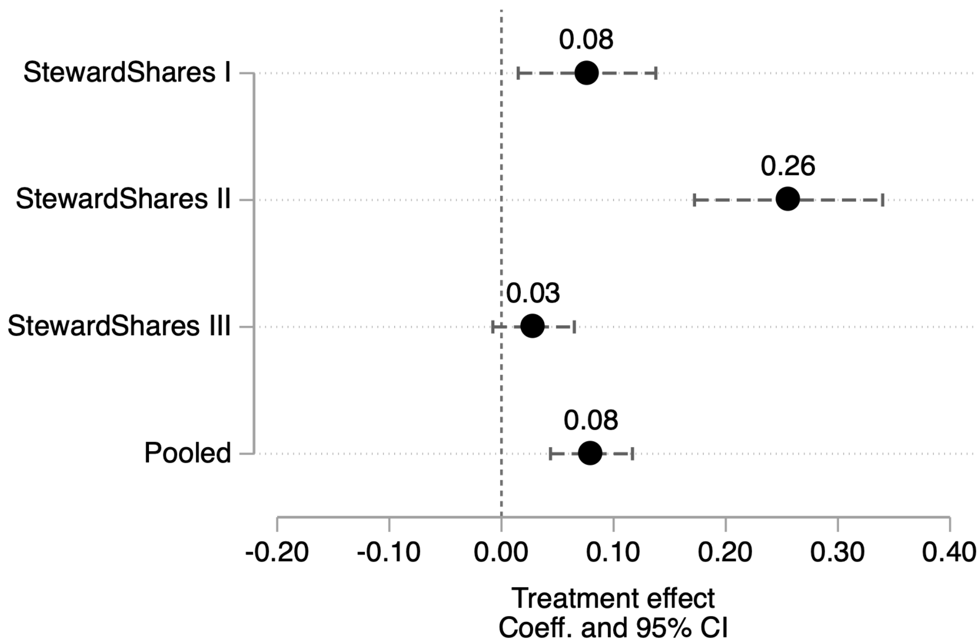
FIGURE 2: Average effect of a starting bid value of 100% rather than 0% on participants' cost-share bids: Dependent variable is the bid expressed as a fraction of total cost



Notes: The figure shows the average marginal effects of the treatment on participants' bids and associated 95% confidence intervals. The estimates are from separate GLM fractional logit models with standard errors clustered by bidder. All specifications control for participant characteristics, state and auction item. The pooled regression specification also control for auction number (I, II or III).

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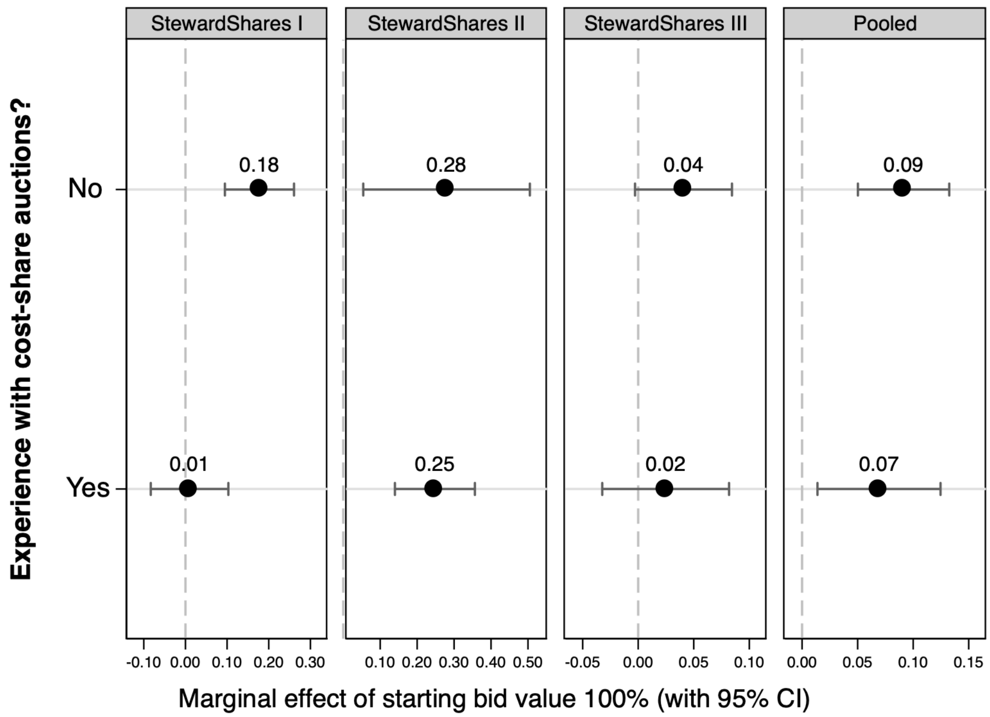
FIGURE 3: Moderating Effect of Experience with Conservation Contracting on the Treatment Effect of the Starting Bid Value



Notes: The figure shows the average marginal effects of the treatment on bids and associated 95% confidence intervals for participants with and without previous experience participating in conservation contracting. We estimate separate GLM fractional logit models for the auctions and the pooled sample. We add a dummy variable for previous experience and its interaction with the treatment in equation (2). All specifications control for participant characteristics, state and auction item. The pooled regression specification also control for auction number (I, II or III). The confidence intervals are based on robust standard error estimates clustered at the bidder level. Differences in the treatment effects and their associated 95% CIs are: StewardShares I: 0.17 [0.04, 0.30]; StewardShares II: 0.03 [-0.20, 0.26]; StewardShares III: 0.02 [-0.05, 0.09]; Pooled: 0.02 [-0.04, 0.09].

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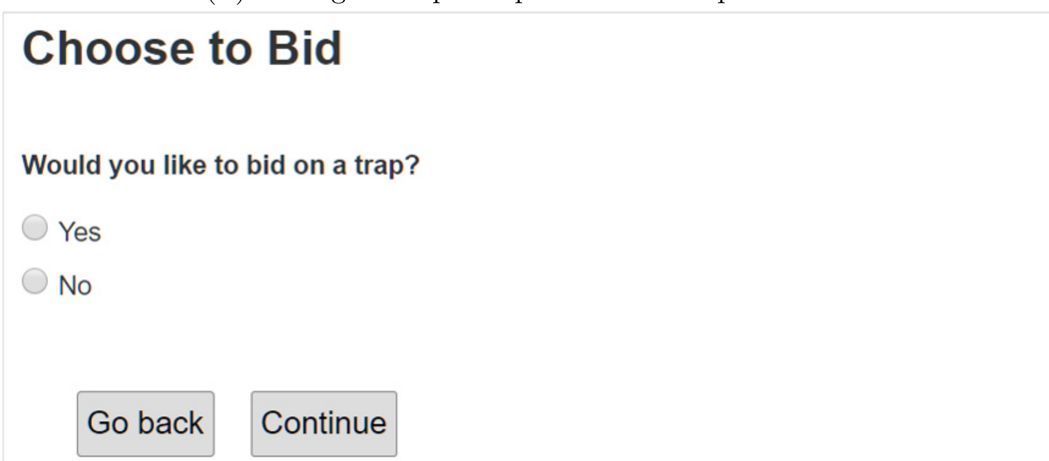
Appendix

Appendix Figures

After seeing an instructional video about the Hog trap technology and how the cost-share auction works, participants in StewardShares III saw the following interactive auction screens.

Figure A1: Bid Screen for Auction Participants

(a) Asking if the participant wanted to place a bid



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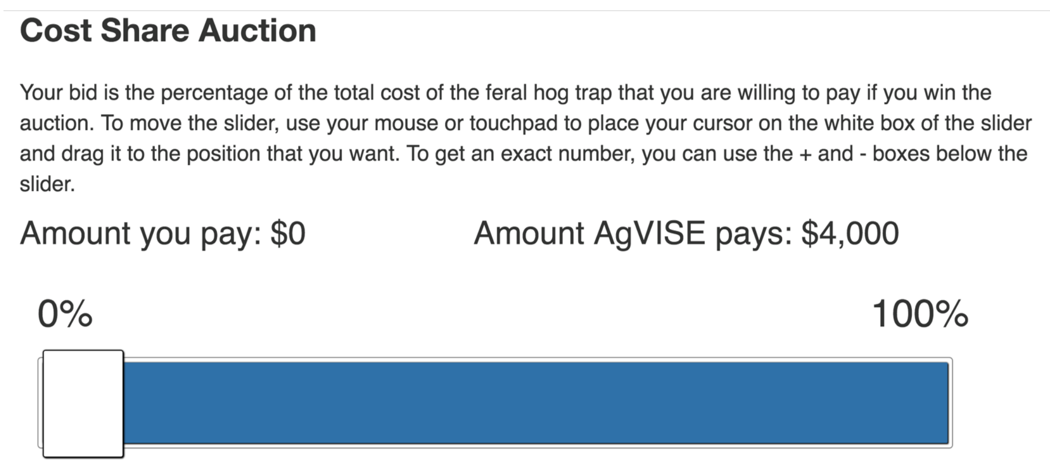
(B) Control group participants saw the slider starting at 0%



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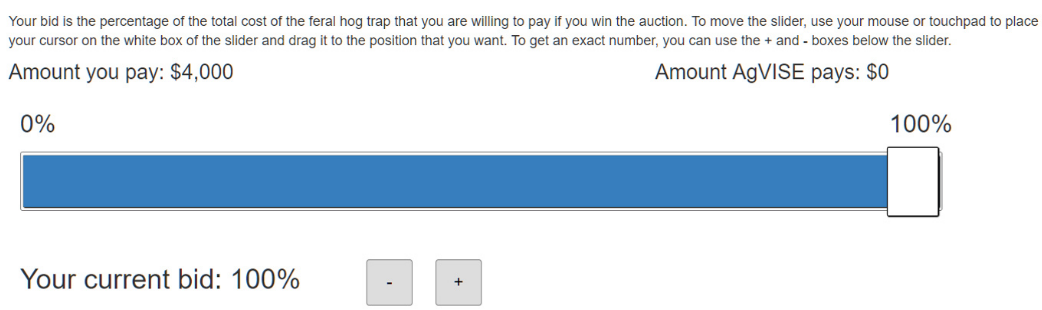
(c) Treatment group participants saw the slider starting at 100%



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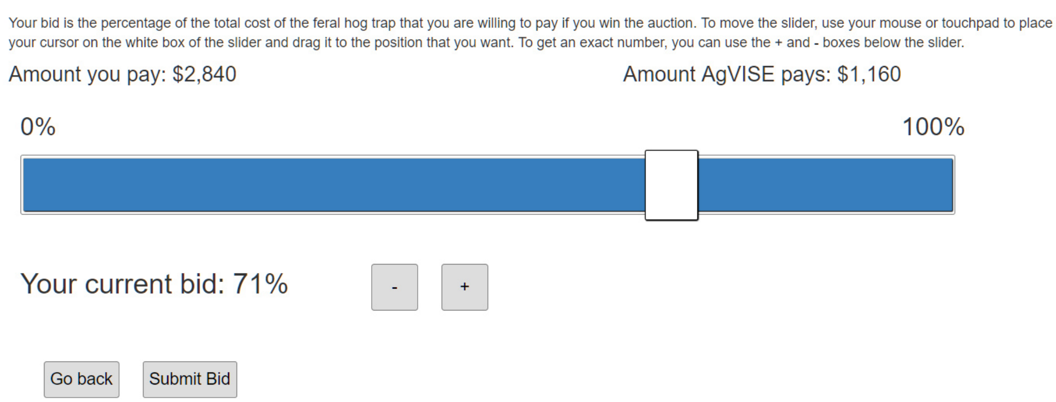
(D) Example of a possible bid



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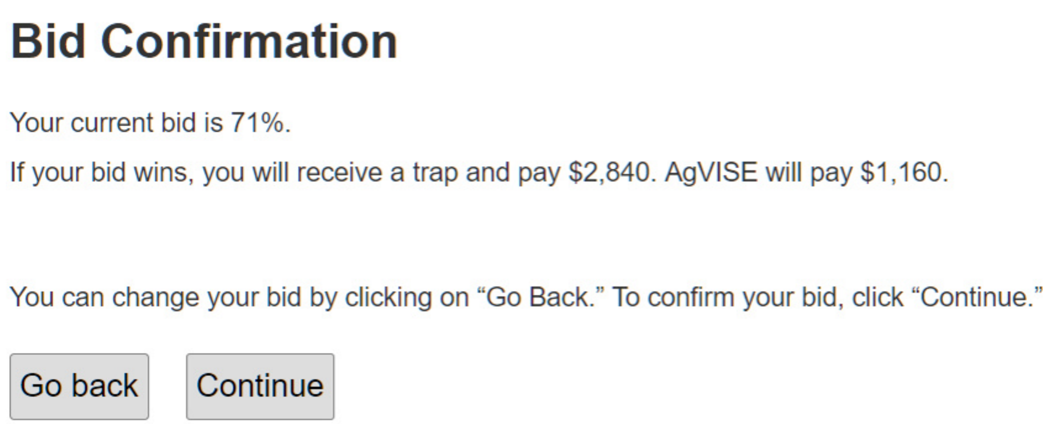
(E) Bid confirmation page before submission



Bid Confirmation

Your current bid is 71%.

If your bid wins, you will receive a trap and pay $2,840. AgVISE will pay $1,160.

You can change your bid by clicking on “Go Back.” To confirm your bid, click “Continue.”

Go back Continue

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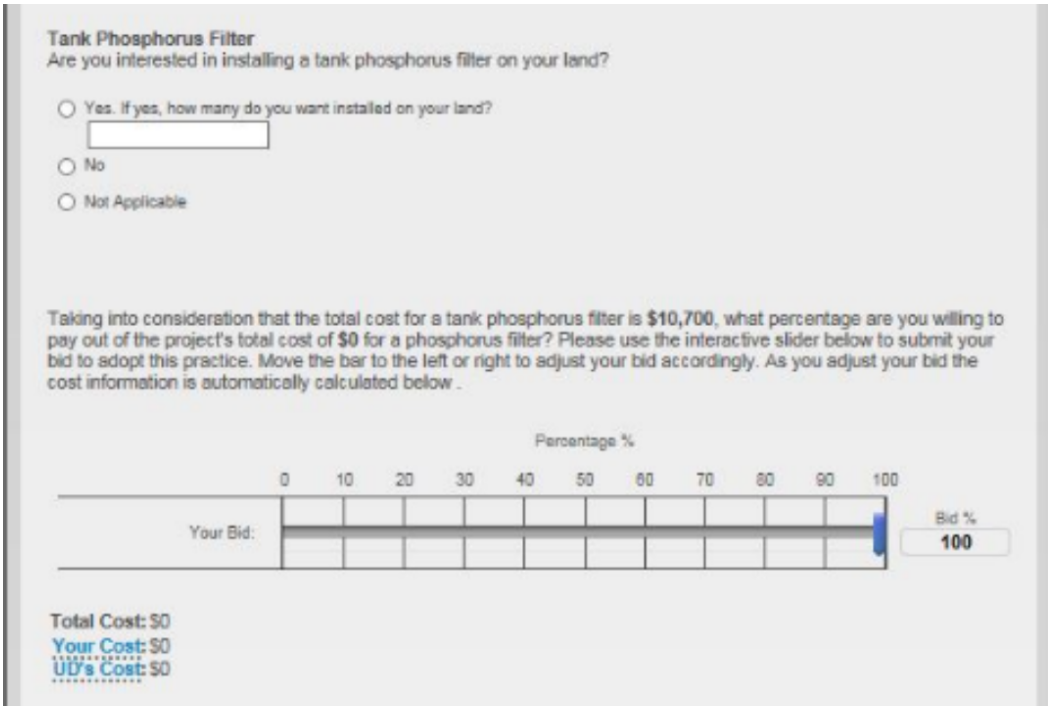
(F) Example of a screenshot of StewardShares I and II auctions



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Appendix Tables

Table A1: Descriptive Statistics for StewardShares I and II

Panel A: StewardShares I (2014)

Variable	(1) Starting Bid = 0% Mean/SD	(2) Starting Bid = 100% Mean/SD	(3) Total Mean/SD
Age	55.02 (12.10)	53.51 (11.18)	54.26 (11.62)
Number of years in agriculture	31.10 (16.45)	30.44 (16.74)	30.77 (16.53)
Gender (1 = Female; 0 = Male)	0.18 (0.39)	0.13 (0.34)	0.16 (0.37)
Owns land (1 = Yes; 0 = No)	0.95 (0.22)	0.95 (0.22)	0.95 (0.22)
Participate in CRP (1 = Yes; 0 = No)	0.12 (0.32)	0.08 (0.28)	0.10 (0.30)
Any other conservation program (1 = Yes; 0 = No)	0.50 (0.50)	0.43 (0.50)	0.46 (0.50)
Any conservation program (1 = Yes; 0 = No)	0.55 (0.50)	0.44 (0.50)	0.50 (0.50)
Total agricultural land (acres)	600.38 (1594.33)	390.32 (700.66)	494.48 (1227.34)
Acres owned	386.31 (1549.15)	183.02 (335.30)	283.83 (1116.49)
Acres leased	214.07 (431.92)	207.30 (584.10)	210.65 (512.17)
Acres under row crops	251.93 (507.38)	117.15 (265.41)	183.98 (407.89)
Participant in Ag. Week (1 = Yes; 0 = No)	0.30 (0.46)	0.23 (0.42)	0.26 (0.44)
N	60	61	121

Notes: Descriptive statistics shown are for the sample of participants who placed bids. Note that each participant could bid on more than one technology. The F-test statistic for the test of joint equality of the covariates is 0.92 (p = 0.52). CRP stands for the US Conservation Reserve Program. "Any other conservation program" refers to participation in any non-CRP federal, state or local programs. "Any conservation program" refers to participation in either CRP or any other conservation program. Ag Week is a week-long agricultural event at the University of Delaware (where some producers were recruited).

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https://doi.org/10.1162/rest_a_01215

Panel B: StewardShares II (2016)

Variable	(1) Starting Bid = 0% Mean/SD	(2) Starting Bid = 100% Mean/SD	(3) Total Mean/SD
Age	59.32	60.21	59.77
	(11.77)	(10.32)	(10.96)
Number of years in agriculture	34.89	36.45	35.68
	(17.17)	(18.79)	(17.87)
Gender (1 = Female; 0 = Male)	0.25	0.17	0.21
	(0.44)	(0.38)	(0.41)
Owns land (1 = Yes; 0 = No)	0.86	0.97	0.91
	(0.36)	(0.19)	(0.29)
Participate in CRP (1 = Yes; 0 = No)	0.21	0.17	0.19
	(0.42)	(0.38)	(0.40)
Any other conservation program (1 = Yes; 0 = No)	0.43	0.76	0.60
	(0.50)	(0.44)	(0.49)
Any conservation program (1 = Yes; 0 = No)	0.50	0.79	0.65
	(0.51)	(0.41)	(0.48)
Total agricultural land (acres)	306.18	430.93	369.65
	(514.60)	(832.75)	(691.65)
Acres owned	118.11	162.38	140.63
	(125.52)	(241.09)	(192.76)
Acres leased	188.06	268.55	229.01
	(459.18)	(674.37)	(575.06)
Acres under row crops	152.24	410.11	283.44
	(281.31)	(837.24)	(636.83)
N	28	29	57

Notes: Descriptive statistics shown are for the sample of participants who placed bids. Note that each participant could bid on more than one technology. The F-test statistic for the test of joint equality of the covariates is 1.36 ($p = 0.23$). CRP stands for the US Conservation Reserve Program. “Any other conservation program” refers to participation in any non-CRP federal, state or local programs. “Any conservation program” refers to participation in either CRP or any other conservation program.

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https://doi.org/10.1162/rest_a_01215

Table A2: Descriptive Statistics for StewardShares III

Variable	(1) Starting Bid = 0% Mean/SD	(2) Starting Bid = 100% Mean/SD	(3) Total Mean/SD
Age	47.70	50.73	49.28
	(14.28)	(14.19)	(14.30)
Number of years in agriculture	25.47	27.12	26.34
	(16.09)	(16.17)	(16.14)
Gender (1 = Female; 0 = Male)	0.09	0.12	0.11
	(0.29)	(0.32)	(0.31)
Owns land (1 = Yes; 0 = No)	0.97	0.93	0.95
	(0.18)	(0.25)	(0.22)
Participate in CRP (1 = Yes; 0 = No)	0.27	0.28	0.27
	(0.44)	(0.45)	(0.45)
Any other conservation program (1 = Yes; 0 = No)	0.30	0.31	0.30
	(0.46)	(0.46)	(0.46)
Any conservation program (1 = Yes; 0 = No)	0.47	0.44	0.45
	(0.50)	(0.50)	(0.50)
Total agricultural land (acres)	1138.21	2171.34	1678.35
	(1563.76)	(14985.15)	(10890.84)
Acres owned	585.81	645.80	617.17
	(1109.72)	(984.54)	(1045.47)
Acres leased	552.40	1525.54	1061.18
	(1066.54)	(14974.74)	(10853.34)
Acres under row crops	488.77	456.92	472.12
	(1134.84)	(1041.97)	(1086.25)
Acres damaged by hogs	343.21	458.44	403.45
	(826.56)	(1135.21)	(1000.53)
Any hog damage in past year? (1 = Yes; 0 = No)	0.90	0.86	0.88
	(0.29)	(0.35)	(0.32)
N	230	252	482

Notes: Descriptive statistics shown are for the sample of participants who placed bids. The F-test statistic for the test of joint equality of the covariates is 1.64 (p = 0.08). CRP stands for the US Conservation Reserve Program. "Any other conservation program" refers to participation in any non-CRP federal, state or local programs. "Any conservation program" refers to participation in either CRP or any other conservation program.

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Table A3: Participant Recruitment Details for StewardShares I, II, and III

For StewardShares I and II, the University of Delaware sent invitation letters to all producers who had nutrient management plan registered with the state. According to National Agricultural Statistics Service, the state of Delaware has 2,300 operational farms (National Agricultural Statistics Service 2020). From a Freedom of Information Request filed by the University of Delaware, we received a list of 2,083 names and addresses from the Delaware Department of Agriculture for producers enrolled in Delaware's Nutrient Certification Program. For StewardShares I, we sent an invitation letter to all 2,083 producers. The mailing revealed that that some of the addresses were not valid. Thus, in StewardShares II, we dropped 120 names from the original list and sent invitations to the remaining 1,963 producers on the list. For StewardShares I, 356 producers went to the online auction interface to learn about the auction. Among those, 121 producers placed bids. For StewardShares II, 181 producers went to the online auction interface to learn about the auction. Among those, 57 producers placed bids. For StewardShares III, 1095 producers went to the online auction interface to learn about the auction. Among those, 482 placed bids. The table below shows the details of the recruitment for StewardShares III including the period of recruitment and eligibility criteria.

Auction Period	**Recruitment**	**States**	**Eligibility Criteria**
October 18 to October 31, 2016	• SunBelt Ag Expo, Moultrie, GA • Internet • E-mail recruitment via producer associations • Hand-outs provided for distribution at producer meetings	AL, GA, FL	• 18 years old • Experience with problems with hogs on your land • Produce/ sell more than $1,000 in ag products per year • One participant per household
January 18 to March 31, 2017	• Internet only auction with e-mail and hand-outs to support recruitment as described above		
October 16 to October 31, 2017	• SunBelt Ag Expo, Moultrie, GA • Internet • E-mail and hand-outs to support recruitment as described above	AL, FL, GA, LA, MS, SC, TN, TX	• 25 years old • Experience with problems with hogs on your land • Produce/ sell more than $1,000 in ag products per year • One participant per household
November 27 to December 8, 2017	• Amarillo Farm and Ranch Show, Amarillo, TX • Internet • E-mail and hand-outs to support recruitment as described above		
September 5, 2018 to December 7, 2018	• SunBelt Ag Expo, Moultrie, GA • Internet • Hard copy mailings to addresses of farmers in target states acquired from FarmMarketiD • E-mail and hand-outs to support recruitment as described above	AL, AR, FL, GA, LA, MS, NC, OK, SC, TN, TX	• 25 years old • Experience with problems with hogs on your land • Produce/ sell more than $1,000 in ag products per year • One participant per household

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https://doi.org/10.1162/rest_a_01215

Table A4: Effect of treatment on the likelihood of participant placing a bid

	(1) StewardShares I	(2) StewardShares II	(3) StewardShares III
Treatment (starting bid value at 100%)	0.004	-0.003	0.008
	(0.015)	(0.018)	(0.027)
Control group:			
- Mean	0.08	0.07	0.42
Observations	3,195	1,611	1,082

Notes: The table shows the marginal effect of the treatment from fractional logit regression models. Each column is a separate regression. The dependent variable takes a value of one if bidder placed a bid and zero otherwise. All specifications control for participant characteristics, state and auction item. Robust standard error estimates shown in parentheses are clustered at the bidder level.

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01215

TABLE A5: Treatment effect of starting bid anchor on cost-sharing bids

Panel A: GLM Estimates

	StewardShares I		StewardShares II		StewardShares III		Pooled	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Starting Anchor at 100%	0.11	0.08	0.27	0.26	0.03	0.03	0.08	0.08
	[0.03,0.18]	[0.02,0.14]	[0.11,0.43]	[0.17,0.34]	[-0.01,0.06]	[-0.01,0.06]	[0.04,0.12]	[0.04,0.12]
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Panel B: OLS Estimates

	StewardShares I		StewardShares II		StewardShares III		Pooled	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Starting Anchor at 100%	0.11	0.09	0.27	0.29	0.03	0.03	0.08	0.08
	[0.03,0.18]	[0.01,0.16]	[0.09,0.46]	[0.16,0.43]	[-0.01,0.06]	[-0.01,0.07]	[0.04,0.12]	[0.04,0.12]
Control group:								
- Mean	0.15	0.15	0.09	0.09	0.21	0.21	0.18	0.18
- Std. dev.	0.19	0.19	0.17	0.17	0.20	0.20	0.20	0.20
Observations	256	256	102	102	483	482	841	840

Notes: Table shows the average marginal effects of the treatment on participants bids. Each column shows results from a separate regression estimated using GLM fractional logit in Panel A, and OLS models in Panel B. Dependent variable is cost-share bid expressed as fraction with range [0,1]. Specifications with controls include participant characteristics, state and auction item fixed effects. Participant characteristics include age, gender, number of years in agriculture, indicator for CRP participation, total agricultural land and area under row crops for StewardShares I and II (indicator for owning any land in the case of StewardShares III). Regressions for the pooled sample also control for auction number (I, II, or III). In addition, regressions for StewardShares I (columns 1 and 2) and StewardShares III (columns 5 and 6) also control for cross-treatments not used in this study. These treatments were randomized and are orthogonal to the anchoring treatment shown here. As a result, excluding these cross-treatment controls does not change the estimates. 95% CI shown in square brackets account for clustering at individual bidder level. Results in Panel A, Columns (2), (4), (6) and (8) correspond to the estimates shown in Figure 2.

Review of Economics and Statistics Just Accepted MS.
https://doi.org/10.1162/rest_a_01215

TABLE A6: Moderating effect of experience with conservation contracting on the treatment effect of starting bid anchor

	(1) Experience = CRP or Other Pooled Sample	(2) Experience = CRP Pooled Sample	(3) Experience = SS I SS II Sample
Anchor 100% × No Experience	0.09	0.08	0.32
	[0.05,0.13]	[0.04,0.12]	[0.16,0.48]
Anchor 100 % × Experience	0.07	0.08	0.21
	[0.01,0.12]	[-0.01,0.17]	[0.09,0.33]
(Experience - No Experience) × Anchor			
Estimate	-0.02	0.00	-0.11
CI	[-0.09,0.04]	[-0.09,0.10]	[-0.32,0.10]
Observations	840	840	102

Notes: Table shows the average marginal effects of the treatment on participant bids - for participants with or without experience. Each column shows results from a separate regression estimated using GLM fractional logit. The dependent variable is cost-share bid expressed as fraction with range [0 , 1]. Definition of experience in column (1) is those who have experience with CRP or any other cost-share conservation program. Experience in column (2) is defined as those who have participated in CRP , while in column (3), it is defined as SS II participants who had previously also participated in SS I. All specifications control for participant characteristics, state, auction item fixed effects and auction round fixed effects (SS I, II or III). 95% CIs shown in square brackets account for clustering at individual bidder level.