



# Informational nudges in conservation auctions: A field experiment with U.S. farmers<sup>☆</sup>

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## ABSTRACT

In the U.S., the fifth largest “crop” is vegetation dedicated to environmental conservation. Over 22 million acres of perennial covers are planted on environmentally sensitive land enrolled in U.S. Department of Agriculture’s Conservation Reserve Program (CRP), one of the largest agricultural conservation programs in the world. About half of CRP lands are enrolled through a complex reverse auction called the General Signup. The communication of program options to participants could have an important behavioral impact. Psychologists have found that information presentation in complex decision environments can interact with the bounded rationality and cognitive biases of decision makers. We tested two changes in the status quo CRP decision environment using an incentivized, lab-in-the-field experiment with 701 prior General Signup participants. First, program participants typically make an active choice over which cover practice to plant and how much of a discount to offer, where the discount is a reduction in their annual program payment. Changing that default to an opt-out, high-scoring offer resulted in a 13 percentage point increase in selection of the best practice and a 48 percent increase in the average discount. In the actual CRP, that increase in discounting would reduce total program costs by about \$30 million per signup. Second, shifting to real-time updating of offer scores modestly reduced the frequency with which participants revised their offers, suggesting a reduction in transaction costs. From a policy perspective, these results suggest that small changes in conservation auctions could both improve the quality of conservation practices and reduce program costs.

## 1. Introduction

One of the largest government programs in the world for the procurement of privately provided environmental services on rural lands is the U.S. Department of Agriculture’s (USDA) Conservation Reserve Program (CRP). Landowners enrolled in the CRP receive annual rental payments for establishing and maintaining an approved vegetative cover, rather than farming the land, during a fixed contractual period that is typically ten to fifteen years. To procure a wide range of environmental services with minimal disruption to food and commodity production, the CRP targets lands with low productivity and high environmental sensitivity. Movement of cropland into and out of the

CRP has been a major component of agricultural land use change in the U.S. over the past thirty five years (Hendricks and Er, 2018).

Given its multiple objectives, the CRP’s enrollment process is a complex mix of multiple, multi-layered enrollment mechanisms (Hellerstein, 2017). Historically, most land has enrolled through the CRP’s General Signup, a national, multi-unit, sealed-bid reverse auction. Offers to the General Signup are ranked on both quality and cost. Participants can increase the probability that their offer is accepted by agreeing to a higher-quality conservation practice, by offering a discount (i.e.: accepting a lower annual rental payment), or by combining both types of offer improvements.

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<sup>1</sup> This enrollment figure includes over 2 million acre enrolled in the CRP Grasslands initiative, which targets grazing lands that are the most likely to convert to cropland.

**Table 1**  
CRP General Signups.

Year	Offers	Accepted (%)	Accepted acres	Average rent (\$/acre)	Total cost (million \$)
2022	22,919	87	2,071,495	51.54	1,068
2021	22,603	88	1,877,631	52.66	989
2020	56,788	91	3,418,782	54.75	1,872
2016	26,279	18	407,416	62.55	255
2013	27,821	87	1,684,030	64.28	1,082
2012	47,934	88	3,875,848	51.24	1,986
2011	38,715	77	2,826,389	48.19	1,362
2010	50,094	92	4,343,452	46.08	2,001

Notes: Total offers, accepted acres, and average rent were obtained from [www.fsa.usda.gov](http://www.fsa.usda.gov). Average rent for 2021 and 2022 were obtained on request from USDA. Acceptance rate was calculated as percent of offers. Total cost is the total procurement 'value' of the auction assume that all contracts are 10-year contracts and is calculated in nominal terms with no discounting. Total cost includes only annual rental payments and does not reflect the additional federal spending on cost share for the establishment of conservation cover.

A key recurring policy question is how the complex reverse auction process in the General Signup could be modified to encourage producers to offer higher quality practices and greater discounts on their rental payments, either of which would improve the cost-effectiveness of the CRP (Kirwan et al., 2005; Hellerstein et al., 2015; Lundberg et al., 2018; Cramton et al., 2021; Pratt and Wallander, 2022). The CRP's annual budget is about USD \$2 billion (Stubbs, 2022) and approximately 22 million acres (9 million hectares) are currently enrolled in the program.<sup>1</sup> The average General Signup receives tens of thousands of offers and ranges from \$0.255 billion to \$2.001 billion (nominal) (Table 1). Given the scale of the CRP General Signup, one potential avenue for reducing program costs or increasing program benefits would be implement more modest changes in the information architecture while holding the more complex auction design constant.

Psychologists and behavioral economists have found that the way information is presented in complex decision settings can interact with the bounded rationality and cognitive biases of decision makers to affect the final outcomes (Yoeli et al., 2017). Changes in the presentation of choice information can provide behavioral "nudges", including through interaction with traditional economic incentives, when they are designed to encourage decisions in a particular direction (Congiu and Moscati, 2022). While many informational nudges are commonly found to have the intended effects across multiple domains, those effects are not always present and can sometimes induce unintended reactions (Hummel and Maedche, 2019; Mertens et al., 2022). Increasingly, many informational nudges are being suggested for public policy because of very low implementation costs (Benartzi et al., 2017). However, implementation costs for nudges are not well documented (Carlsson et al., 2021).

In this study, we experimentally tested whether small changes in the General Signup auction's decision environment can lead to policy-relevant changes in farmer offers. Using a stylized version of the enrollment software to create a simulated (artefactual) CRP auction, we estimated the impacts of two behavioral interventions—(i) a high-quality default starting offer; and (ii) live updates on the offer score—on cover practice and discount choices.

The high-quality default offer allowed us to test whether participants anchor to the default when constructing their final offer. In the context of our study, the default offer of a cover practice and discount level was pre-filled on the decision screen. If participants are cognitively constrained and unfamiliar with the program, they may be more inclined to submit an offer near the anchor. However, these participants have submitted offers in prior signups and some research has found that decision makers with greater experience in a decision setting have been found to be less influenced by anchoring (Alevy et al., 2015).

The live updating on the offer score allowed us to test whether the current program design attenuates responsiveness to the underlying auction incentives. In the current program, participants only receive their offer scores after constructing an offer and proceeding forward in the submission process. In our study, the live score, or real-time, information automatically updated to reflect whatever was entered into the decision screen. The behavioral intervention of providing real-time information on the quality of an offer means that it is less costly for a participant to update based on that information.

## 2. Background and literature

This research contributes to two branches of the literature related to agricultural conservation program design. First, there is an ongoing expansion of behavioral economic research within the space of agricultural conservation and sustainability (Balmford et al., 2021). This study examines whether the informational nudges found to be effective in other contexts are also effective in the context of agricultural conservation auctions. Second, experimental methods are a common approach for studying questions about conservation auction design (Schilizzi, 2017). To provide greater external validity for testing the effects of behavioral interventions, this study uses an auction design that closely follows the CRP auction design and a subject pool comprised of agricultural producers with prior experience in the CRP General Signup auction, a group that comprises the majority of offers in most sign ups.

### 2.1. Behavioral nudges: Anchoring, defaults, and information

Nudges can have large effects on behavior; in a meta analysis of 100 experiments using nudges, Hummel and Maedche find that two thirds of the effects are statistically significant and the median effect size is 21% (Hummel and Maedche, 2019). Nudges can be an important tool for policy makers as relatively small interventions with the potential to adjust behavior without significantly changing the incentives or choice options for decision makers (Benartzi et al., 2017). Behavioral interventions are increasingly recognized as an important tool for improving the sustainability of food production systems (Ferrari et al., 2019). However, a number of studies have argued that nudges may be much less effective than thought if meta-analyses are adjusted to account for the potential impacts of publication bias and other factors (Maier et al., 2022; Szasz et al., 2022).

Defaults are choices or settings which require an individual to make a deliberate action to deviate from the default. Status quo bias describes the reluctance of a decision-maker to change from the default. A good example of this is the dramatic differences in organ donor registration between countries that have an opt-in or an opt-out system (Johnson and Goldstein, 2003). Defaults have been shown to have large behavioral impacts in a number of contexts, including to increase voluntary contributions (Messer et al., 2007, 2013), reducing over-prescription of opioids (Chiu et al., 2018) and tipping behavior for taxi rides (Haggag and Paci, 2014).

Anchoring is a cognitive bias that describes a tendency to rely too heavily on the first piece of information available when making a decision. It is a well-studied and well-documented behavioral bias in the literature (Kahneman, 2011). In the context of consumer goods, psychologists and behavioral economists have reported that anchoring can influence valuations (Ariely et al., 2003; Maniadi et al., 2014). Results from these studies, and their interpretations, have generated disagreement about the stability of consumer preferences (Maniadi et al., 2014; Enke et al., 2021). There is further disagreement if anchoring influences inexperienced consumers' valuations more than experienced consumers (Alevy et al., 2015; Clark and Ward, 2008; Löfgren et al., 2012). There is some evidence of the impact of anchoring on market outcomes fading over time (Alevy et al., 2015). Anchoring has also been shown to change behavior across many contexts, including: willingness-to-pay for environmental action (Li et al., 2021), the value of bids

on initial public offering auctions (Gao et al., 2018), farmers' bidding practices (Holst et al., 2015), and others (see a review in Furnham and Boo (2011)). In reviewing the literature for this study, we only identified one other study that tested defaults within a conservation program enrollment setting using farmers as study participants (Ferraro et al., 2022).

Other nudge strategies include additional information and feedback. Feedback nudges have been shown to have long and persistent effects on water consumption (Chabe-Ferret et al., 2019). Information and social comparisons increase compliance with water protection rules (Peth and Mußhoff, 2020). In experiments simulating stochastic business environments, decision making performance is significantly and negatively impacted by delays in providing feedback information (Diehl and Stermann, 1995). In learning settings, feedback-timing effects have been predicted to influence outcomes through an error correction mechanisms, although experimental research suggests that a reinforcement or 'spacing' mechanism is a better fit for observed results (Smith and Kimball, 2010). The form of information feedback, graphical versus tabular, can also influence performance in dynamic decision making settings (Atkins et al., 2002).

## 2.2. Experimental papers on conservation auctions

Experiments are a common approach for studying auctions and auction-like games with hundreds of studies featured in major literature reviews (e.g.: Dechenaux et al., 2015). Within that literature, there are several dozen studies that focus specifically on conservation auctions (Schilizzi, 2017). These studies are predominately lab experiments. Lab-in-the-field or field experiments remain relatively uncommon due in part to the challenges of implementing experiments within actual programs (Wallander et al., 2017; Rosch et al., 2021).

Auction structure is an important topic in this literature. Pricing structure within an auction is predicted to have an important impact on bidding behavior. Several studies compare pay-as-bid (discriminatory pricing) and uniform pricing. Early research found that pay-as-bid pricing was more cost effective (Cason and Gangadharan, 2004), but subsequent research found that uniform pricing was more cost effective if the experiment also captured compliance decisions (Kawasaki et al., 2012). Reverse auctions, such as CRP, also commonly use bid caps, which are the counterpart to reserve prices in forward auctions. Other research has found that bid caps set too close or too far from expected reserve rates can reduce cost effectiveness (Hellerstein et al., 2015; Cramton et al., 2021).

Another line of research in the conservation auction experimental literature examines the impact of withholding ranking information as a mechanism for reducing information rents. Some of these studies found that withholding information on ranking can reduce information rents (Cason and Gangadharan, 2004; Messer et al., 2014; Banerjee et al., 2015), while other studies found that withholding ranking information can also reduce benefits (Conte and Griffin, 2017).

Other key issues covered in these studies include the way in which the dynamic of repeated auctions can improve net benefits even while increasing rents (Fooks et al., 2015), the prevalence of adverse selection in these auctions (Arnold et al., 2013), incentives for offer quality improvement (Banerjee and Conte, 2018), the impact of using benefit-cost ratio ranking (Iftekhar and Tisdell, 2014; Fooks et al., 2015), multiple, interacting auctions (Tisdell and Iftekhar, 2013), and the role of communication and trust between participants and program administrators (Vogt et al., 2013).

Most of the studies on conservation auction experiments referenced above have relied on student populations. In a methodological shift, experimental research related to questions of agricultural decision-making are increasingly recruiting farmers as study participants to address concerns over external validity (Holst et al., 2015; Takeda et al., 2015; Palm-Forster et al., 2017), which has been identified as an important issue for research that seeks to inform agricultural policy (Rosch et al., 2021; Palm-Forster and Messer, 2021).

## 3. Experimental design

Our study used a lab-in-the-field experiment that, in the terminology of Harrison and List (2004), was both a framed field experiment and an artefactual field experiment. It was a framed field experiment because the choice environment is modeled to very closely resemble an actual CRP General Signup. It was an artefactual field experiment because the subjects are farmers and farmland owners who participated in prior CRP General Signups. These two features of our experimental design improved our ability to generalize from the experimental findings to the actual policy setting of real world conservation auctions.

### 3.1. Development, review, approval, and preregistration

The study was initiated as a collaboration between researchers at the USDA Economic Research Service, the USDA Farm Service Agency (FSA), and the Center for Behavioral and Experimental Agri-environmental Research. The early stages of development involved an assessment of the current CRP General Signup enrollment process and software, followed by the development of the study enrollment software.

Our detailed experimental design and analysis plan went through a multi-stage internal USDA approval process. As part of that approval, the plan received an external peer review by researchers working on conservation auction experiments. Since the project was developed as a federal data collection effort, the study design also went through United States Office of Management and Budget (OMB) review under the requirements of the Paperwork Reduction Act. The approval for the information collection request was finalized on May 9, 2022 (OMB Control Number 0536-0078). Following the approval of the information collection request, the study design was preregistered on June 10, 2022 (See Appendix A.1). A growing literature argues that preregistering study designs is an important tool for ensuring research quality and replicability (Nosek et al., 2018; Ferraro and Shukla, 2020).

### 3.2. Population and recruitment

Since any behavioral intervention in the CRP enrollment process would potentially influence individuals who had opted into the program, the target population for causal inference was likely future CRP participants. In recent CRP General Signups, the large majority of offers have come from rural landowners that are re-enrolling in the program. Based in part on this observation, the recruitment pool for this study consisted of rural landowners and farmers who were affiliated with one of the 79,391 offers made in two of the most recent General Signups (2020 and 2021).<sup>2</sup> This recruitment pool had the added advantage of having high familiarity with the CRP enrollment process, which should give the study conservative estimates of behavioral interventions since expertise with a decision making setting has been found to reduce sensitivity to behavioral interventions.

To determine a target sample size, we conducted a power analysis on the rental rate discount decision. In recent CRP General Signups, our review of program administrative data found that the average discount on all offers (relative to the offer-specific bid caps) was about 4.2 percent. At that average discount decision, a minimum detectable effect of about 0.2 percentage points at 80 percent power would require between 1,000 and 1,500 observations. (See Appendix A.2) The goal of obtaining sufficient power at this level was motivated by the fact that in the context of a 1 to 2 billion dollar ecosystem service procurement auction, a 0.2 percentage point increase in discounts offered would save the program 20 to 40 million dollars.

<sup>2</sup> The 2022 General Signup had not been completed at the time of drawing the sample.

Based on an expected response rate of 10 percent, a sample of 11,000 participants were randomly selected from the administrative data on the prior CRP General Signups. The full list of participants was filtered to exclude estates and duplicate addresses. The sample was drawn as a simple random sample. Participants were recruited through a series of mailings directing them to the study website. Participants were notified that they were being asked to participate in an auction to study alternatives to the CRP General Signup structure. Prior research has found that recruiting farmers for experimental research can run into several challenges that often lead to low study participation rates (Weigel et al., 2021). For this study, recruitment materials reflected the role of USDA in the study, which, along with other factors, was designed to maximize response rates (see Supplemental online materials for recruitment materials.)

### 3.3. Choice environment, induced values, and payoff calculation

The software interface used for this study was designed based on current software used by the FSA to enroll landowners in the CRP program. The experimental software underlying the web-interface was designed on the SoPHIELabs Platform. (Software screen shots are provided in the supplemental online material.) In a deviation from the actual program, study participants interacted directly with the software themselves. In an actual CRP General Signup, farmers and landowners travel to their county FSA office and work with USDA staff who enter offers into the software and provide guidance on the enrollment process. Since this experiment does not include that principal-agent interaction, the study includes an instructional video and a number of graphics to improve comprehension.

The study auction provides a set of marginal incentives calibrated to induce offer choices that resemble the observed outcomes in past CRP auction. As in the actual CRP General Signup, participants are asked to make two choices for each offer. They choose the quality of their offer by choosing from a menu of conservation cover practices. They also choose the price of their offer by selecting a payment (“rental rate” in the program terminology) that is constrained by a field-specific bid cap (“soil rental rate” in the program terminology). Each offer is scored and ranked using a metric called the Environmental Benefits Index (EBI). As in the actual General Signup, participants can increase their likelihood of having an accepted offer by selecting improved cover practices, a higher discount on the rental rate, or a combination of both. Most prior studies on conservation auctions have focused only on the price or rental rate decision, whether using lab experiments (Cason and Gangadharan, 2004; Conte and Griffin, 2017) or econometric models (Kirwan et al., 2005). Those studies that have included both quality and price have observed important trade-offs in participant decision-making (Banerjee and Conte, 2018).

To reduce respondent burden and to simplify the calibration process, the study makes a number of simplifications to the auction incentives. Within the actual CRP General Signup, participants can choose between many cover practices and can choose combinations of practices (Pratt and Wallander, 2022). For this study, we simplified the cover practice choice set into four options that reflect the spectrum of most common choices in the actual program. For these four cover types or practices, we assign points and costs similar to the actual program (with establishment costs annualized) adjusted to produce a consistent Nash equilibrium in offer structure (Table 2). As in the actual program software, the points associated with each practice are revealed in the selection menu in the study software. The study software adds information on prices and the total EBI score, which is not included in the actual program software and is communicated through the interaction with the county staff. The study software also takes a more explicit approach to communicating the relative ranking of the practices by using the terms ‘basic,’ ‘good,’ ‘better,’ and ‘best.’ While the EBI points and average costs in the actual program imply such an ordinal ranking,

**Table 2**

Available practice EBI points.

Practice name	CRP Code	Auction parameters	
		Points	Cost
Basic (non-native grass)	CP1	13	\$2
Good (native grass)	CP2	43	\$6
Better (wildlife habitat)	CP4d	65	\$10
Best (rare and declining habitat)	CP25	90	\$15

Note: These practice types, points, and total are based on analysis of actual offer data and EBI rules.

the ordering is not so explicitly communicated to participants in the actual program.

Participants also can get additional EBI points by offering a discount on their annual payment. The discount is calculated as a percentage of the bid cap (i.e. the difference between the bid cap and the offer, divided by the bid cap). In the actual CRP this “cost factor” includes points proportional to the absolute annual payment and points proportional to the relative annual payment (i.e.: adjusted for the maximum bid cap). For this study, we use a simplified version of the actual (non-linear) ranking score. Participants with fields having a low, medium or high bid cap receive, respectively, 3, 5, or 7 points per percent discount. Since the EBI cost factor in the General Signup is not revealed to participants in the actual program enrollment process, we do not include this scoring structure in the training video or in the software. However, study participants can see the effect of offering a discount on their total EBI score.

$$\text{Discount EBI} = X \times \text{Discount} \times 100 \quad \text{where } X = \begin{cases} 3 & \text{low bid cap} \\ 5 & \text{medium bid cap} \\ 7 & \text{high bid cap} \end{cases}$$

In addition to the marginal incentives described above, the study uses assigned field characteristics to induce variation in offers resembling the choices observed in the actual program. The assignment of field characteristics varies across rounds, which allows the study to estimate the responsiveness to underlying program incentives using a mix of within-participant and across-participant variation. Each field is assigned three characteristics that influence its relative baseline competitiveness in the auction and the potential gains (information rent) that the participant can make through program enrollment:

- The land EBI: a portion of the endowment EBI score that reflects the environmental sensitivity of the land;
- A bid cap: the upper limit on the annual payment for that field, which also contributes to the endowment EBI;
- A reserve rate: the “outside option” return on the land.

The first two factors combine to form a total EBI endowment. As in the actual program, this feature of our study design means that some fields start with a higher probability of being accepted because they have a higher land EBI, a lower bid cap, or a combination of the two (Pratt and Wallander, 2022). These fields with higher endowments should be likely to make less aggressive offers in an effort to extract more information rent from the program (Kirwan et al., 2005). There were three different bid caps and three different environmental scores, which combine to generate nine possible fields (Table 3). These values are based on the 25th, 50th, and 75th percentiles in bid caps (Bid Cap EBI) and environmental scores based on land characteristics (Land EBI) from the past five General Signups. By avoiding the tails of the EBI endowment and bid cap distributions, this study design avoids excessive clustering in which a large share of participants have an expected probability of acceptance near either zero or 100 percent.

The study provided monetary payments to participants. A minimum participation fee of \$10 served as a recruitment incentive. The opportunity to earn a larger payment was constructed to mimic the actual program incentives in terms of the net gains from being accepted into the program. This structure is standard practice to reduce hypothetical bias in auction experiments (Falk and Heckman, 2009). In the actual CRP, participants receive an annual payment equal to their offered rental rate if their offer is accepted. Since the program is voluntary, economic models of participation typically assume that participants have a net gain from the program that is positive, meaning that their rental payment minus their cost of implementing the cover practice is greater than some “reserve” rate. Since many of the factors that can impact reserve rates – risk preferences, retirement from farming, conservation attitudes – are difficult to replicate in an artefactual lab experiment, the monetary payments for this study are calculated based on net gains relative to assigned reserve rates. The reserve rate was presented to participants as “net income from your field without CRP participation”. The rate was set to provide sufficient room (maximum possible gain) for participants to both offer at least some discount and select some practice upgrades without having zero or negative net returns. Specifically, the per-acre reserve rate values were calculated as the field-specific bid cap minus \$15 dollars (the cost of the best practice) and minus nine percent of the bid cap.<sup>3</sup> Receiving an additional payment would only occur if a participant’s offer was accepted. As participants offered larger discounts or selected more expensive cover practices, their expected probability of getting a payment would increase, but the value of that payment (their per-acre net return times the common field size of five acres) would decrease. Offers with negative net returns were given a final payment of ‘zero.’ This particular feature of the study design maximizes the marginal incentives attached to the offer improvement decisions. However, it also means that the study does not impose “loss-framed incentives” by, for example, treating the participation fee (or the reserve rate) as an endowment that is reclaimed if participants make offers with negative net returns (Di Mauro and Maffioletti, 2004; Zhang et al., 2019). The lack of loss-framed incentives and the relatively low stakes of the experiment might overly incentivize (or at least not disincentivize) excess bidding in relation to the reserve rate.

The auction in the experiment cleared like a standard CRP General Signup clears. Study participants had a window of about five weeks during which they could log into the study website and submit their offers in each of three rounds. After the participation window closed, one of the three rounds was randomly selected to be the true auction pool. While the actual CRP is subject to an acreage enrollment constraint, the study auction was subject to a budget constraint. The offers in the selected round were sorted and accepted, beginning with the highest scoring offer until the budget constraint was met. All of the accepted offers would receive their \$10 participation payment plus their net gains from their offer. The rejected offers would only receive a \$10 participation payment.

The Nash equilibrium of the optimal strategic offers for each of the nine field types 3 was calculated using a numerical model of expected net returns. This model was used for calibration of the study and is not incorporated into the analysis.<sup>4</sup> The model calculated the expected net return for each field for each possible offer in terms of the four cover practices and discount in whole percentages. The expected probability of offer acceptance was represented with a logistic distribution with

<sup>3</sup> For example, for the lowest bid cap of \$50, the assigned reserve was calculated as  $\$50 - \$15 - 0.09 \times \$50 = \$30.50$ .

<sup>4</sup> Experimental studies of auction design sometimes compare deviation from the Nash equilibrium in their analysis. That is a useful approach when the control and treatment design each have separate Nash equilibria. We are not aware of auction equilibrium theory that incorporates the effects of behavioral interventions, and so we do not have separate Nash equilibria outcomes predicted for each treatment group.

**Table 3**  
Field types and nash equilibrium.

A. Total EBI endowments				
Bid cap	Bid cap EBI	Land EBI		
		Low	Average	High
		88	120	171
\$50/acre	97	185	217	268
\$100/acre	68	156	188	239
\$150/acre	40	128	160	211

Note: The land EBI in the actual CRP auction is the sum of six different EBI subfactors, which reflect physical characteristics such as soil erodibility. In this experiment, there is no underlying calculation and the values are based on historical CRP offer data. The bid cap EBI in the actual signup is calculated by the formula  $a \times \frac{(b - \text{bid cap})}{b}$  with coefficients that are determined after the auction has closed. While this means that the bid cap EBI is technically not known to participants during the signup, the actual values have historically remained nearly constant. For this experiment, bid cap EBI values are drawn from recent auctions.

B. Nash equilibrium practice choice				
Bid cap	Bid cap EBI	Land EBI		
		Low	Average	High
		88	120	171
\$50/acre	97	Good	Good	Basic
\$100/acre	68	Better	Better	Good
\$150/acre	40	Best	Best	Better

Note: The Nash equilibrium of the optimal practice choice for each of the nine field types was calculated using a numerical model of expected net returns.

C. Nash equilibrium discount (percent)				
Bid cap	Bid cap EBI	Land EBI		
		Low	Average	High
		88	120	171
\$50/acre	97	7	2	1
\$100/acre	68	3	1	0
\$150/acre	40	2	0	0

Note: The Nash equilibrium of the optimal discount offer for each of the nine field types was calculated using a numerical model of expected net returns.

location parameter calculated as the equilibrium “cut off EBI” and a scale parameter based on the standard deviation of historical CRP General Signup cut off EBI scores. The cutoff EBI was adjusted to find the equilibrium outcome at which it was consistent with the distribution of equilibrium offers across the nine field types and an acceptance rate that met the budget constraint (44 percent).

Participants were assigned to a sequence of three different fields over the three rounds of constructing offers. (See Fig. 1.) This design ensured that we had within-participant variation in bidding incentives, which allows for the estimation of the score updating treatment effect (explained in the analysis section below). Using only three rounds with a single field in each round serves two purposes. First, it reduces respondent burden by only requiring participants to make a total of six decisions (price and cover) over the course of the entire session, in addition to the responses to the practice round and the post-decision survey questions. Second, it would have been problematic to vary the treatments within participants because the types of behavioral interventions being studied – defaults and information timing – should not be evident to participants. Varying treatments across rounds would have required controlling for the sequence in which the treatments were assigned, reducing the statistical power benefit of implementing within-participant treatments. In our review of the prior literature, most conservation auction experiments asked participants to make offers in between eight and 65 auctions, many of which might consist of multiple rounds. In other studies, participants are usually assigned a single item (e.g., a field as in this study) on which to make an offer, but in some cases participants are assigned multiple items. The combination of auctions, rounds, and multiple items can significantly increase the number of offer decisions required by the average participant over the course of the study. The most extensive conservation auction studies

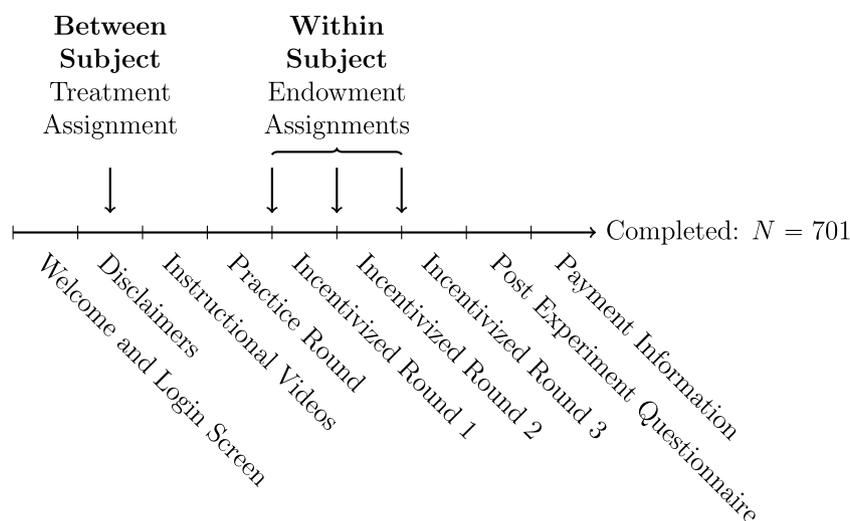


Fig. 1. Experiment timeline. This timeline illustrates the order of stages within the study. The assignment of between-subject treatment and within-subject endowments is indicated by the down arrows.

required 91 (Banerjee et al., 2015), 108 (Cason and Gangadharan, 2004), and 130 decisions to be made by participants (Fooks et al., 2015). So in contrast to most prior conservation auction experimental studies, this study relies on a smaller number of participant choices and therefore requires the larger sample described in the previous subsection.

### 3.4. Treatments

This study randomly assigns two treatments using a 2-by-2 design. This creates four possible treatments, which are assigned at the participant level. That assignment remains constant across all four rounds (including the practice round, as illustrated in Fig. 1).

1. **Treatment 1: Default High-Score Offer** Participants were randomly assigned to either an “active choice” condition (the status quo control group) or a “default offer” condition (the treatment group). In the “active choice” condition, which is based on the current CRP enrollment software, auction participants started with no pre-selected option for cover choice or annual rental payment. In the “default offer” condition, participants started with an offer consisting of a high quality cover (“Best”) and high discount (9 percent) for the annual rental payment, which they can change (i.e., an opt-out default, see Fig. 2).
2. **Treatment 2: Information (Real-time Updating on Offer Score)** Participants were also randomly assigned to a “score updating” condition (treatment) or a “post-decision information” condition (the status quo control). In the “post-decision information condition”, which is how the current CRP signup software functions, participants only saw their ranking score on the final offer submissions screen, after having made their cover practice and annual rental rate choices. In the “score updating” condition, participants saw their score updated immediately as they changed their choices on the offer selection screen (Fig. 2). This score also was repeated on the final offer submission screen. As in the current software, all participants had an option to navigate back to the earlier offer selection screen to revise their choices. The assigned condition remained constant across rounds for each participant.

The two-by-two treatment design allows for each treatment to be independently assigned to 50 percent of the population. As noted in the study pre-registration, we do not estimate the interaction effect. Since the two treatments have different types of expected effects on decision making – as noted below – the interaction effect is not of theoretical

interest. These two treatments were selected among the many possible behavioral interventions in the information environment. Not having to estimate the interaction effect reduces the number of hypotheses being testing and therefore increases the overall statistical power of the study.

As described in the previous subsection, participants were also assigned one of nine field types, which varied each round for each participant. To ensure that each round had a balanced population of the nine field types, participants were assigned a sequence of field types at the beginning of the experiment rather than being randomly assigned a type at the beginning of each round. There were 729 possible sequences of the nine fields across the three experimental rounds ( $9^3$ ). To also ensure that each participant had variation between low, medium, and high endowments across their three rounds, we assigned field characteristics in two stages. First we assigned one of six possible sequences of broad starting EBI categories (low, medium, and high). Then we randomly selected one of the three starting EBI scores and associated bid cap within each category.

For treatment assignment, we sought to have the four treatments equally balanced within each of the six sequences. We accomplish this by assigning treatment and sequence jointly at the beginning of the experiment, which means that there were 24 possible assignments. Balance of the treatments and endowments is presented in Appendix A.3.

### 3.5. Hypotheses and analysis plan

As specified in the pre-analysis plan, this study uses an economic experiment to answer seven research questions about whether providing a default offer or information updating to potential CRP enrollees can affect the overall characteristics of the enrollment offers. The seven questions are summarized in Table 4.

For the analysis of this experiment, there are ten different average treatment effects of interest (Table 5). Nine of the effects arise from the interaction of three outcomes (total endogenous EBI points, the discount decision, and the cover practice choice) and the treatments. The default treatment has a single effect for each outcome because it is expected to shift the average decisions. The score updating treatment has two effects for each outcome because it is expected to interact with the EBI endowment for the primary effect and to possibly shift the average decisions for the secondary effect. The tenth effect is the simple effect of the score updating treatment on the average propensity to navigate backwards from the final offer screen in order to revise the offer.

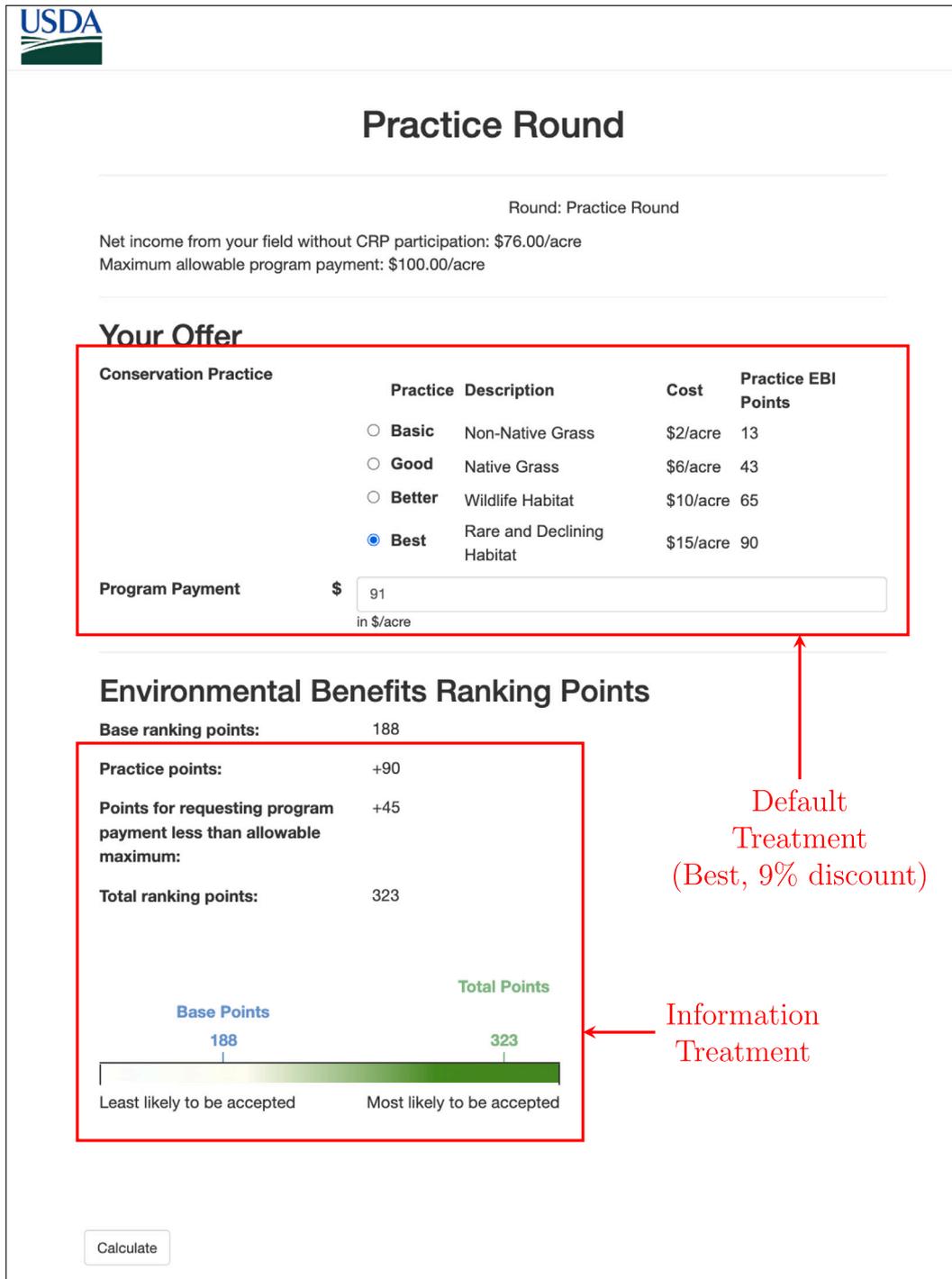


Fig. 2. Screenshot of decision screen with treatment components outlined.

Since there are only ten treatment effects of interest, we do not adjust the test statistics for multiple hypothesis testing. The primary consideration for this choice is that the models capture nested decisions. The discount and cover choices are the two components that feed into the endogenous EBI score. Therefore the number of truly independent hypothesis tests is lower.

To increase the precision of our estimated treatment effects, we estimate each effect in a regression framework. That framework is also useful because the hypothesis tests for live score updating require an interaction of the treatment with the endowment EBI. The default treatment effect is captured through the coefficient on the treatment

indicator variable,  $Default_i$ . The primary score updating effect is captured by the interaction between the EBI endowment and the treatment indicator variable,  $Info_i$ . The hypothesis in this case is that the treatment will strengthen the effect of the EBI endowment. The secondary treatment effect of the score updating effect is the coefficient on the treatment indicator variable  $Info_i$  itself.

To estimate the treatment effects on the endogenous EBI outcome (Eq. (1)), we use a Tobit model. A Tobit model is appropriate because the endogenous EBI outcome is censored at the lower end, which is the score (13 points) from selecting the basic cover choice with no discount. This approach of giving positive points to minimum offers reflects actual practice within the CRP General Signup. While there is

**Table 4**

Research questions.

H1 default treatment
1. Will the average score (endogenous EBI, reflecting both choices) be higher?
2. Will the average discount be larger?
3. Will higher quality practices (best cover choice) be more common?
H2 information treatment
1. Will there be increased responsiveness (average score) to the auction incentives?
2. Will the average discount be larger for lower endowment fields (and smaller for higher endowment fields)?
3. Will lower endowment fields be more likely (and will higher endowment fields be less likely) to offer higher quality practices?
4. Will there be fewer offer revisions?

**Table 5**

Treatment effect estimates.

Dependent variable	Anchor	Information (Interaction)	Information (Direct)
Endogenous EBI	$\tau_{1,1}$	$\tau_{1,2}$	$\tau_{1,3}$
Percent discount (%)	$\tau_{2,1}$	$\tau_{2,2}$	$\tau_{2,3}$
Cover (1/2/3/4)	$\tau_{3,1}$	$\tau_{3,2}$	$\tau_{3,3}$
Revised offer (0/1)		$\tau_4$	

also possible censoring at the upper end in terms of maximum possible EBI points, we do not expect to observe a binding constraint there. All specifications use round fixed effects and individual clustered standard errors.

$$\text{EndoEBI}_{it} = \Phi(\alpha_1 + \tau_{1,1}\text{Default}_t + \tau_{1,2}(\text{Info}_i \times \text{ExogEBI}_{it}) + \gamma_1\text{ExogEBI}_{it} + \tau_{1,3}\text{Info}_i + \eta_{1,t}\text{Round}_t + \epsilon_{1,it}) \quad (1)$$

The discount decision was estimated using a Tobit model given censoring at zero. Based on a pre-analysis of administrative data from prior CRP signups, there could be as many as half of offers with no discount.

$$\text{Discount}_{it} = Y(\alpha_2 + \tau_{2,1}\text{Default}_t + \tau_{2,2}(\text{Info}_i \times \text{ExogEBI}_{it}) + \gamma_2\text{ExogEBI}_{it} + \tau_{2,3}\text{Info}_i + \eta_{2,t}\text{Round}_t + \epsilon_{2,it}) \quad (2)$$

The cover decision is a discrete choice and so is modeled using an ordered probit with the treatment effects included in the latent variable construction.

$$\mathbb{P}(\text{Cover}_{it}) = \Psi(\tau_{3,1}\text{Default}_t + \tau_{3,2}(\text{Info}_i \times \text{ExogEBI}_{it}) + \gamma_3\text{ExogEBI}_{it} + \tau_{3,3}\text{Info}_i + \eta_{3,t}\text{Round}_t + \epsilon_{3,it}) \quad (3)$$

As a test of whether knowledge of the final score is truly constrained in the control condition for the score updating treatment, we collected data on whether participants use the “back” buttons to navigate to earlier screens and revise their offers prior to finalizing their offer structure. We estimated the using a probit model and a binary indicator ( $\text{Revised}_{it}$ ) if the participant  $i$  revised their offer in round  $t$ .

$$\mathbb{P}(\text{Revised}_{it} = 1) = \Omega(\alpha_4 + \tau_4\text{Info}_i + \zeta\text{Default}_t + \gamma_4\text{ExogEBI}_{it} + \eta_{4,t}\text{Round}_t + \epsilon_{4,it}) \quad (4)$$

**4. Data**

The final data set used for the analysis consists of 1,925 offers submitted by 701 study participants. This excludes participants who did not complete all three rounds and the final survey questions. The final data set additionally excludes the rounds in which there were offers with a negative net final value (see Appendix A.4 for detail on the full and positive value sample of the data and robustness check on the analysis). This smaller sample size of usable observations results in lower power relative to the original power analysis. The minimum detectable effect with the sample size of the experiment is closer to 0.4

percentage points. The final auction acceptance rate was 100 percent. The auction cleared on a budget constraint, and the high acceptance rate was primarily due to the prevalence of large discounts in submitted offers. Additionally, a bit less than 10 percent of the participants who completed all three rounds opted out of receiving any payment. Among all participants, including those dropped from the final sample, the minimum program payment was the \$10 participation payment, the average payment was \$41.76 (including the participation payment), and the maximum payment was \$142.50 (also including the participation payment).

**4.1. Descriptive statistics**

The two components of the offer, cover choice and the discount (as a share of the bid cap) on the offer, have distributions that vary by treatment group (Fig. 3).

In Panel 3(a), we see that the least common practice for all groups was the lowest-scoring “basic” practice, and the most common practice for three of the four groups was the second-highest scoring “better” practice. In the two histograms for the high score default, the share of offers who ultimately chose the “best” cover choice is higher than for the two histograms showing the distribution of cover types without the default treatment. The differences in the distribution of cover choices between the live score updating treatment groups and the information control groups are not as large, although there may be slightly lower adoption of the “basic” practice with the information treatment.

In Panel 3(b), the distributions of percent discount reveal stark differences between the groups. For the two top histograms, showing the groups with the control of an active choice default, the most common offer has no discount. For the two treatment groups with the default of a 10% discount, the most common offer is a 10% discount. Notably, in the positive value sample the top two distributions also show more discounts over 10%, suggesting that, relative to the Nash Equilibrium, there is greater discounting than expected and potentially an anchoring effect toward the default from both directions. The difference between the status quo information groups (the two left histograms) and the two right hand histograms (the live score updating treatment) is not as great, although there are more offers with 10% bid down in the group that only received the information treatment.

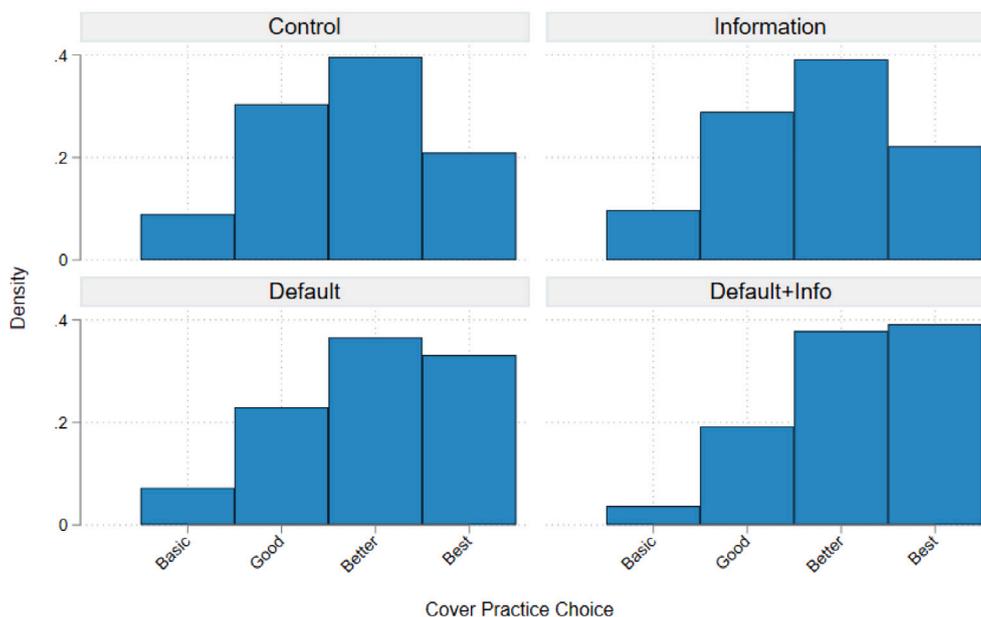
Table 6 presents the mean and standard deviation of the four key outcome variables of interest: endogenous EBI points, discount, cover practice choice, and the number of offer revisions. Each variable is presented first with observations pooled across all treatments, and then at the treatment level. See Appendix A.3 for an overview of the positive net value data and related robustness checks. The average endogenous EBI is highest for the treatments with a default offer. The binding maximum possible endogenous EBI is 740 points. The 153 observed maximum endogenous EBI is a result of using the positive net return offers. A field with a high bid cap of \$150 with a 9% discount (63 points) and the best practice (90 points) would therefore receive 153 endogenous EBI points. No other combination could exceed this with a positive net return. The average discount offered is also highest for the treatments with a default offer. See Appendix A.3 for a discussion of the discount levels observed in the actual CRP. The default treatments also led to better cover practice choice (shown here as the mean of the ordered numeric variable. The average number of revisions is between 0.4 and 0.6 (out of a maximum of 6) for all four groups.

**5. Results**

**5.1. Primary results**

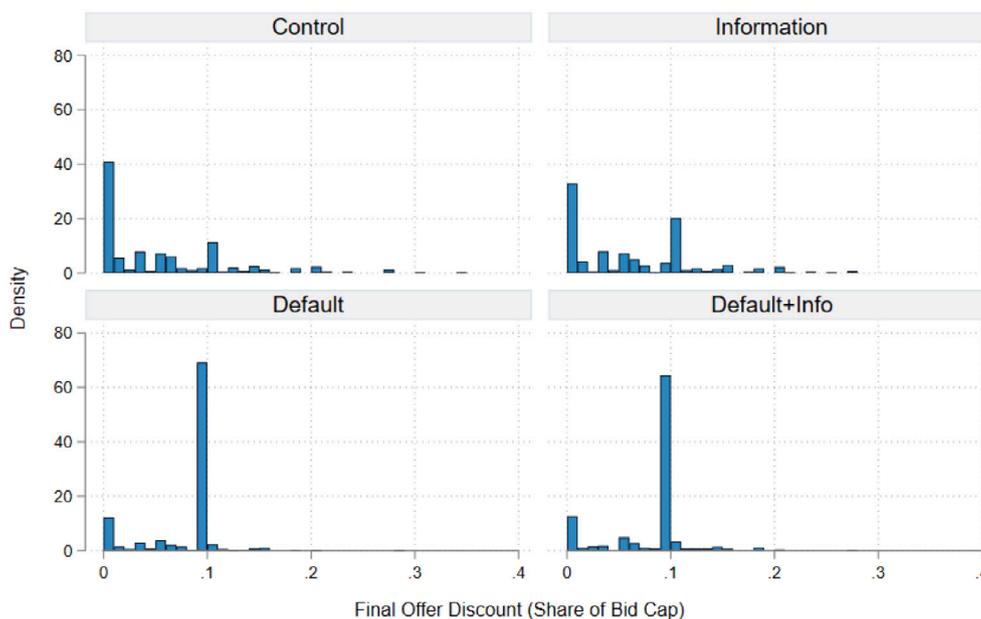
We present the estimating equations (1) through (4) using the positive net value data set in Table 7. As noted above, the regressors include indicator variable for both treatment effects, an interaction of the information timing treatment with the endowed (starting) EBI

(a) Histogram of Cover Choice by Treatment. Default Treatment Cover is “Best”



Graphs by Treatment

(b) Histogram of Discount by Treatment. Default Treatment Discount is 9%



Graphs by Treatment

Fig. 3. Variables of interest: Cover choice and discount.

points, the endowed EBI points as a separate continuous variable, and round fixed effects.

Column (1) shows the coefficients for a Tobit model of the endogenous EBI score, which is a measure of offer improvement that combined both the discount offered and the cover practice chosen. The treatment of a high scoring default results in a higher scoring final offer, which is consistent with the research hypothesis that the opt-out default would lead to anchoring. Participants with a higher number of endowed EBI

points tend to offer fewer additional points, a strong indication that participants understood and responded to the basic auction incentives. The first measure of the information timing treatment effect comes from the interaction terms. The coefficients indicate that, contrary to the research hypothesis, providing live updating of the EBI score does not increase responsiveness to the EBI endowment. The second measure of the information treatment timing effect is the coefficient on

**Table 6**  
Descriptive statistics of variables of interest by treatment.

	Treatment	Mean	Std. Dev.	Min.	Max.	Obs.
Endogenous EBI	Pooled	97.2	32.6	13	153	1925
	Control	84.1	32.4	13	153	391
	Information	88.7	31.7	13	153	401
	Default	102.6	31.5	13	153	618
	Default + Info	107.3	29.7	13	153	515
Discount (percent)	Pooled	6.9	5.0	0	35	1925
	Control	5.3	6.5	0	35	391
	Information	6.2	6.0	0	27	401
	Default	7.6	3.5	0	28	618
	Default + Info	7.8	3.8	0	27	515
Cover (0/1/2/3)	Pooled	1.9	0.9	0	3	1925
	Control	1.7	0.9	0	3	391
	Information	1.7	0.9	0	3	401
	Default	2.0	0.9	0	3	618
	Default + Info	2.1	0.8	0	3	515
Revisions (count)	Pooled	0.5	1.2	0	6	1925
	Control	0.6	1.4	0	6	391
	Information	0.5	1.0	0	6	401
	Default	0.6	1.2	0	6	618
	Default + Info	0.4	1.0	0	6	515

**Table 7**  
Regressions results.

	(1) Endogenous EBI	(2) Discount	(3) Cover choice	(4) Revisions
Default treatment	18.74*** (2.010)	0.0281*** (0.00429)	0.375*** (0.0693)	-0.0314 (0.0819)
Info treatment × Endowed EBI points	0.0219 (0.0257)	0.00000936 (0.0000536)	0.000390 (0.000964)	-0.000548 (0.00134)
Info treatment	0.336 (5.336)	0.00310 (0.0100)	0.0511 (0.199)	-0.0907 (0.274)
Endowed EBI points	-0.276*** (0.0178)	-0.000129*** (0.0000346)	-0.00393*** (0.000642)	-0.00132 (0.000858)
Constant	138.9*** (4.088)	0.0726*** (0.00739)		-0.309 (0.188)
Round fixed effects	Yes	Yes	Yes	Yes
Observations	1925	1925	1925	1925

Standard errors in parentheses.

Note: Individual Cluster Standard Errors Included (Multiple Rounds per Individual).

\*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ .

the indicator variable, which suggests little impact of the information treatment on the overall improvement of the average offer.

Columns (2) and (3) present the models for the components of the offer, the discount on the rental rate and the cover choice, respectively. The discount rate, like the endogenous EBI, uses a Tobit model. The cover choice uses an ordered probit model. The coefficients for both models reflect the same type of results seen in the endogenous EBI equation. The high-scoring, opt-out default leads to anchoring on both the discount and the cover choice. Both choices are responsive to the endowed EBI points. The information treatment effect does not result in greater responsiveness for either choice.

Column (4) presents the binary (probit) model of the indicator of whether participants revised their offer by using the “back” button on the software after arriving at the final offer submission screen. The sign on all coefficients is negative, but only the endowed EBI points is statistically significant. This suggests that participants who knew their offer was at the higher end of the total score were less likely to seek improvements in the offer before submitting it.

Given the non-linear nature of all of the models, the marginal treatment effects cannot be directly observed from the coefficient estimates reported above. We estimate the marginal treatment effect of the default in a set of semi-elasticities and change in predicted probabilities for each of the outcome variables, shown in Table 8. The anchoring effect on the endogenous EBI points is 19.9 percent, with a 95 percent confidence interval of 15.6 to 24.2 percent. The anchoring effect on the discount is an increase of 48.2 percent. This represents

an expected increase in the discount of about 2.4 percentage points. Since the average CRP General Signup (see Table 1) procures contracts with about \$1.3 billion in total annual rental payments, that scale of an treatment effect in the actual program would result in a savings of over \$30 million per signup. The effect on cover choice is broken down by type. As noted above (see Fig. 3(a)), not all participants in the treatment group stick with the “best” practice for their final offer. Under the default treatment there is an expected increase in the adoption of the best practice of 12.5 percentage points and decreases of 5.2 percent and 8.2 percent in the basic and good practices, respectively. Lastly, the default treatment did not have a clear impact on the likelihood of offer revisions with a 95 percent confidence interval with a range of negative 6.2 percentage points to positive 4.2 percentage points.

The main research hypothesis for the effect of live score updating is the prediction that reducing the lag on information feedback to offer selection will have a reinforcement effect. This means that testing for a treatment effect involves comparing participant responsiveness to the auction incentives with and without the live score updating treatment. In the design of this auction, all of the variation in auction incentives comes from the endowment EBI. Table 9 presents the estimated effect of each decision outcome with respect to the endowment EBI for the control and treatment. Included in the direct effect is the hypothesis that providing live updating will reduce the revision of offers. For all outcomes, there is significant overlap in the confidence intervals, leading to the finding that live score updating did not make participants more responsive to the underlying auction incentives.

**Table 8**  
Anchoring: Marginal effects of default treatment.

Outcome	Effect	95% confidence interval
<i>Percent Change</i>		
Endogenous EBI	19.9	(15.6, 24.2)
Discount	48.2	(30.4, 65.9)
<i>Percentage Point Change</i>		
Cover Choice		
Basic	-5.2	(-7.2, -3.1)
Good	-8.2	(-11.2, -5.1)
Better	0.8	(-0.2, 1.8)
Best	12.5	(8.1, 17.0)
Revision of Offer	-1.0	(-6.2, 4.2)

Note: Marginal effects calculated from regressions presented in Table 7.

The secondary research hypothesis for the effect of live score updating is that the direct effects will, like the anchoring effect, shift the distribution of offers. Table 10 presents the marginal effects of treatment effect for these outcomes. There is some indication that the live score updating results in higher scoring offers, with a 4.9 percent increase in the endogenous EBI. However, the confidence intervals for the effect on the two components of offer improvement – discount and cover choice – span negative and positive effects. The direct effect does show a 6.3 percentage point reduction in the likelihood of offer revisions, which suggests that live score updating might reduce transaction costs by helping participants identify their optimal choice sooner.

## 5.2. Post-experiment survey responses

The post-experiment survey questions provide a means to check a number of assumptions built into the design of this study. A key motivation of this line of research is the finding in the behavioral economics literature that experience with an information setting tends to attenuate or eliminate the effectiveness of behavioral interventions, such as changes in the information setting. Table 11 part (a) shows that most study participants have considerable experience with the CRP General Signup. More than 60 percent had participated in three or more actual signups. A small share of study participants (3 percent) reported that they had not participated in any prior signups. Since the signup software allows offers to have multiple people associated with them, this might indicate that these individuals were associated with prior offers that were completed by someone else, or they might have responded to the study invitation that was sent to someone else in their household.

Table 11 part (b) provides a measure of whether the structure of having the participants submit their own offers, without the assistance of a county agent, is a significant deviation from the actual program. A bit more than one-fifth of participants (22.85 percent) report that they tend to rely primarily on the input of the county agent. Only 16.94 percent rely primarily on their own preferences when making actual program offers. These findings suggest that while the majority of participants play a key role in making program offers – either independently or with input from the county agent – in the actual auction the principal–agent interaction is an important part of the decision making environment that is not replicated in this study or other experimental research on conservation auctions. Limited information is available on whether county agents use any sort of default when presenting enrollment options to program participants. While CRP program materials emphasize the importance of practice upgrades to improve ranking, examination of the data of actual CRP offers, shows that most offers from farmers only choose modest improvements in cover practices (Pratt and Wallander, 2022).<sup>5</sup>

<sup>5</sup> An anonymous reviewer raised the question of whether county agents could be using high scoring defaults similar to those employed in this study.

Table 11 part (c) gives the responses to a series of questions that examine task comprehension and an assessment of the extent to which the study design adequately reflects the actual signup. Most participants felt that the study closely resembled the actual CRP General Signup. Only about one-fourth of participants felt that the enrollment process was not similar to the actual program. Over 71 percent of respondents felt that the field characteristics were realistic and understandable. Only about one-third (31.96 percent) of participants felt that the smaller set of cover choices was unrealistic. Most participants (79.46 percent) also reported that they comprehended the study and understood the task of making an offer. However, the results related to the lack of a principal–agent component to the signup were mixed. A large majority (81.26 percent) of participants appreciated being able to construct their own offer, but a majority (62.93 percent) also reported that they missed having the county agent's advice.

Table 11 part (d) reports the results on whether the adopted strategies in this study match the assumptions built into the study design, particularly the calibration through the Nash equilibrium and the use of monetary incentives. Slightly more than half of participants (50.58) reported adopting a strategy of managing the trade off of net returns and probability of acceptance, which is the assumption of expected profit maximization at the core of this study and most experimental work on conservation auction design. The remaining participants were split between focusing mostly on the probability of acceptance (27.89 percent), focusing mainly on the net return (12.45 percent), or some other approach (8.99 percent).

## 6. Policy implications

Reverse auctions are an important and common policy tool for many programs, including for agri-environmental programs and payment for ecosystem services. Billions of taxpayer dollars are spent through these auctions, so improving the cost-effectiveness of these programs can have meaningful impacts. In this study, we test two behavioral nudges using a lab-in-the-field experiment built around one of the world's largest such programs, the General Signup for the USDA's Conservation Reserve Program. Testing the effects of behavioral nudges in the context of such program serves two purposes. First, if behavioral nudges are effective, they may reveal ways in which the current program design is hampered by participants who are either not fully informed or fully engaged in evaluating their program choices. Second, administering agencies may be able to act on these insights and employ such nudges in the actual program. In the CRP General Signup, participants submit offers to enroll and compete on price (annual payment) and quality (conservation cover practice).

Using a simulated CRP general signup auction and a population of prior participants of the actual land preservation program, we test for an anchoring effect of a default high-ranking offer and a focusing (reverse-attenuation) effect of information timing. Understanding how program participants respond to behavioral nudges embedded in a CRP-type enrollment process is important for increasing the efficiency of the CRP program. The CRP represents a large fraction of overall conservation spending by the USDA, and, because of the scale of the program, even a small effect on enrollment could generate significant benefits. As noted above, if the experimental default effects on offer discounting were observed in the actual program, it would amount to

If true, this would mean that the high scoring default treatment, rather than the active-choice control, is more representative of baseline conditions in the actual program. To indirectly test this hypothesis, we examined the offer data from the most recent CRP General Signup, specifically the number of scenarios submitted prior to selection of the final offer. Over 79 percent of offers relied on only a single scenario. In combination with the prevalence of zero discounts and basic cover practices, this suggests that county agents are not commonly running high scoring scenarios as defaults.

**Table 9**  
Information updating effects — Primary.

Outcome	Control		Live Score Updating	
	EBI Effect	95% CI	EBI Effect	CI
<i>Percent change</i>				
Endogenous EBI	-60.2	(-68.6 , -51.9)	-52.5	(-60.6 , -44.3)
Discount	-46.2	(-74.0 , -18.4 )	-38.9	(-67.4 , -10.5)
<i>Percentage point change</i>				
Cover choice				
Basic	11.6	(7.2 , 16.0)	8.7	(4.7 , 12.7)
Good	16.2	(11.0 , 21.3)	15.0	(9.1 , 20.9)
Better	-3.1	(-5.9 , -0.3)	0.2	(-2.1 , 2.5)
Best	-24.7	(-32.4 , -17.0)	-23.9	(-33.2 , -14.6)
Revision of offer	-8.8	(-19.8 , 0.2)	-10.8	(-22.4 , 0.7)

Note: Marginal effects calculated from regressions presented in Table 7.

**Table 10**  
Information updating effects — Secondary.

Outcome	Effect	95% confidence interval
<i>Percent Change</i>		
Endogenous EBI	4.9	(0.9 , 9.0)
Discount	8.4	(-4.9 , 21.8)
<i>Percentage point change</i>		
Cover choice		
Basic	-1.7	(-3.5 , 0.1)
Good	-2.7	(-5.6 , 0.2)
Better	0.2	(-0.3 , 0.7)
Best	4.3	(-0.3 , 8.8)
Revision of offer	-6.3	(-11.4 , -1.2)

Note: Marginal effects calculated from regressions presented in Table 7.

reducing the costs of the program by over \$30 million, for the average General Signup. This cost savings would also be accompanied by an increase in environmental benefits due to greater adoption of higher scoring cover practices. These savings could either save taxpayer money or enable more conservation on agricultural lands for the same amount of money.

### 7. Conclusions

The results of this study have significant implications for the policy and program design of the CRP. However, policy makers should be aware of a couple caveats to these results. For instance, we do not consider dynamic effects, because the results of one year’s signup will likely impact the behavior in subsequent signups. Increasingly competitive offers would likely spill over into the auction through program participants’ beliefs about the likelihood of acceptance conditional on EBI score. This could imply even greater program improvements due to such behavioral intervention.

Beyond the potential for direct and dynamic impacts on program outcomes, this research highlights some additional considerations for CRP specifically and conservation auctions more broadly. Prior literature has found two factors that influence the effectiveness of behavioral nudges. Experience can attenuate or eliminate the effectiveness of nudges. In contrast, complexity, such as that found in the CRP General Signup, tends to increase the effectiveness of nudges. We find large effects of a high scoring default treatment despite the experience of our participant pool with the decision making environment. This suggests that the complexity of the CRP signup, despite the many simplifications made for this study, potentially creates costs for the program by reducing participant comprehension or attention. We do find that participants are responsive to the basic program incentives, but future research could further explore whether program complexity is hindering the intended effects of program design.

Another important insight from this study is that the standard approach to experimental research on conservation auctions may be

**Table 11**  
Post experiment questionnaire.

(a) Prior CRP Signups (enrollment periods)			
	Percent		
None	3.21		
1 to 2	35.04		
3 to 4	31.32		
5 or more	30.42		
(b) Decision making in CRP Signups			
	Percent		
Rely on the county agent	22.85		
My own preferences	16.94		
Interact with the county agent	60.21		
(c) Study comprehension and assessment statements			
	Agree	Disagree	Do not know
This enrollment process was similar to the CRP General Signup.	57.64	26.32	16.05
I understood the task of making an offer in each round.	79.46	14.25	6.29
The field characteristics were realistic and understandable.	71.50	19.26	9.24
The small set of cover choices was easy to understand.	86.26	10.78	2.95
The smaller set of cover choices seemed unrealistic.	31.96	46.98	21.05
I appreciated being able to construct my own offer.	81.26	8.09	10.65
I missed having the input of the county agent.	63.93	22.59	13.48
I appreciated not having to travel to the county office.	50.45	38.00	11.55
(d) Offer strategy for this study			
	Percent		
Balance the net return to my offer against the likelihood of getting accepted	50.58		
Maximize the net return to my offer	12.45		
Maximize the chance that my offer would be accepted.	27.98		
Different strategy or did not really use a consistent strategy.	8.99		

impacted by the absence of the interactions between producers and field agents who sometimes act as intermediaries between the producers and the conservation programs (e.g., county agents in the CRP). In the CRP, for example, those interactions may involve the use of anchors and defaults, in which case the implication of our experiment would not be that USDA could make the program more cost-effective by incorporating high-scoring defaults, but rather that field staff's current use of such defaults may have made the program more cost-effective. However, the prevalence of zero-discount and basic cover practices in the actual CRP offer data suggest that either few county agents currently use high-scoring defaults or that the defaults used are not effective. Future research could survey county agents to provide data on the approaches they traditionally take to constructing offers when working with program participants. If that research revealed that many county agents are simply starting participants off in the enrollment process with low-scoring offers, the future experimental research on defaults could utilize a control group with a low-scoring default rather than an active choice default. Future research could also test a treatment where defaults are set to the Nash equilibrium solution rather than to a uniformly high-scoring offer. The latter would be difficult to implement in the actual program since calculating the Nash equilibrium individual solutions would require information on participant opportunity costs that are generally only known to the participant. However, it is possible that FSA county agents take a heuristic version of this approach when interacting with program participants. Ultimately, the most compelling test of the changes to the information environment considered in our study would be to randomly embed these changes into a regular General Signup for the CRP. In addition to estimating the effect of the new defaults on the attributes of the bids submitted, one should also seek to estimate whether the new defaults lead to higher shares of producers exiting the program early, perhaps as a result of offering more practices or a higher discount than would be privately optimal.

### CRedit authorship contribution statement

**Steven Wallander:** Conceptualization, Methodology, Software programming, Validation, Formal analysis, Investigation, Resources, Data curation, Writing, Supervision, Visualization, Project administration, Funding acquisition. **Laura A. Paul:** Conceptualization, Methodology, Software programming, Validation, Formal analysis, Investigation, Data curation, Writing, Supervision, Visualization, Project administration. **Paul J. Ferraro:** Conceptualization, Methodology, Validation, Resources, Writing, Supervision, Funding acquisition. **Kent D. Messer:** Conceptualization, Methodology, Validation, Resources, Writing, Supervision, Funding acquisition. **Richard Iovanna:** Conceptualization, Methodology, Validation, Resources, Writing, Supervision.

### Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

## Appendix A. Appendices

### A.1. Pre-registration

**Title:** Conservation Auction Behavior: Effects of Default Offers and Score Updating

**Registration type:** OSF Pre-Registration

**Date registered:** June 10, 2022

**Date created:** June 10, 2022

**Blind reviewer link:**

[https://osf.io/fm8xt/?view\\_only=c2372624e7b14b5fb0775bd921020584](https://osf.io/fm8xt/?view_only=c2372624e7b14b5fb0775bd921020584)

### A.2. Power analysis

For the opt-out default treatment effect on discounting, we conducted a simple power analysis of a difference in two means around an expected average discount of 4.2%. For a sample size of 1,000, the minimum detectable effect at 80 percent power is 0.2 percentage points, or an increase to from 4.2 to 4.4% discount. In other words, if the default increases the discount from 4.2% to 4.4%, then four out of five runs of the experiment would successfully reject the null hypothesis of no effect on discount at the Type 1 error rate (alpha) of 0.02. We used an alpha of 0.02 (rather than 0.05) to take a more conservative approach given that the study tests multiple hypotheses. We conduct the power analysis as a two-tailed test because it is possible that the default could induce practice improvements that are offset by reductions in discount. We selected a very small 0.2 percentage point increase in discount as the desired minimal detectable effect because given a budget of \$2 billion per year would result in a program savings of \$40 million per year, which would be a meaningful saving in program costs.

In the experiment, the smaller sample size of usable observations results in lower power relative to the original power analysis. The minimum detectable effect with the sample size of the experiment is closer to 0.4 percentage points. Any failures to reject the null hypotheses are therefore subject to more uncertainty.

### A.3. Respondent balance check and positive net value data set

Of the 11,000 previous CRP participants who were invited to participate in this study, 1,064 initial respondents logged in to the website to begin the study. [Table A.1](#) presents a comparison the initial survey respondents to the non-respondents according to data on their offers in the recent CRP General Signups. For the characteristics on the fields offered, there were not statistically significant differences in terms of three characteristics: whether the offered field was a for a re-enrollment, the average Land EBI points, and whether the offered field was in a wildlife priority zone. Study respondents did tend to have slightly smaller average field size (53.1 versus 63.7 acres) and somewhat higher bid caps (\$113 per acre versus \$105 per acre). The higher bid cap suggests that study respondents might have historically offered more improvements than non-respondents. The comparison of offer structure characteristics confirms this. Study respondents had historically offered somewhat higher average discounts (3.3% versus 3.0%) and cover practices that scored slightly higher (73 versus 70 cover EBI points). There was not a statistically significant difference between the two groups in offer acceptance rate.

A total of 285 respondents exited the website prior to completing the study, leaving 779 respondents who completed all three rounds and the concluding survey questions. Those responses constitute the “full data set” for this study. Initial analysis of the full data set revealed that an unexpectedly large proportion of offers had a negative net value due to extremely high discounts being offered on the rental rates. (Section 3.3 in the main text provides details on how negative net values arise.) From the full data set, 412 out of the 2,337 submitted offers have negative final values. These offers are irrational in the sense that the participants were clearly not attempting to maximize the expected value in terms of the monetary payout from the study. Since the costs of cover practices were capped at the cost of the “best” practice, these negative value offers arose from high discounts being offered on the annual rental rates ([Fig. A.1](#)).

Discounts for these negative net value offers are excessive in both prevalence and in levels, raising concerns about the external validity of the full data set for making inference about the actual CRP General Signup. When compared to the Nash Equilibrium analysis, in which the highest expected discount was 7 percent, the observed discounts well beyond 50 percent are suggestive of respondents either misunderstanding the incentives or being subject to hypothetical bias because

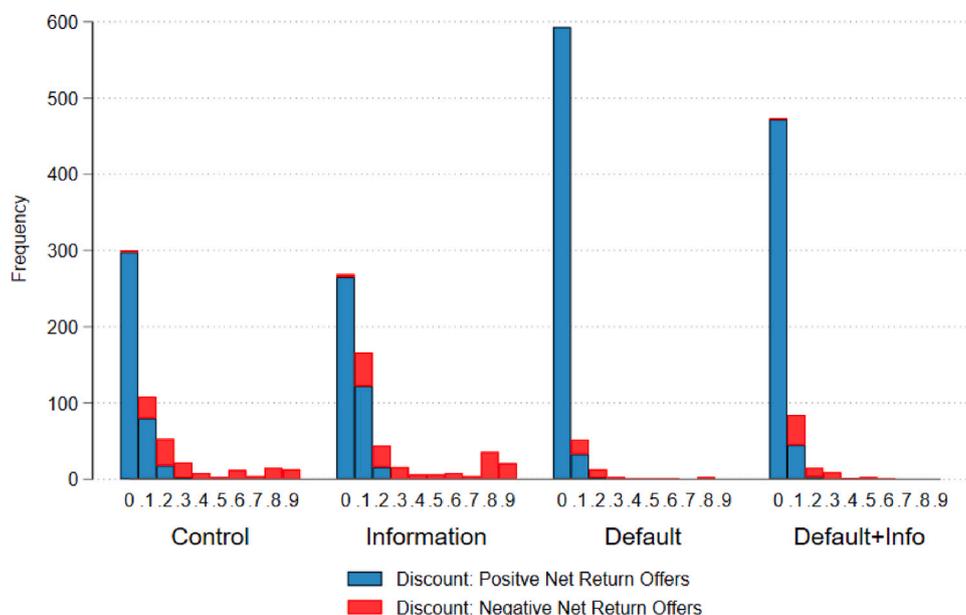


Fig. A.1. Discount rates.

**Table A.1**  
Prior CRP General Signup covariate balance check.

Variable	Respondent	Non-respondent	Difference
<b>Field characteristics</b>			
Field size (acres)	53.1 (89.5)	63.7 (121.2)	<b>-10.6</b> [-2.82]
Re-enrollment (%)	71.4 (45.2)	71.0 (45.4)	0.4 [0.28]
Bid cap (\$/acre)	112.56 (63.18)	105.02 (64.42)	<b>7.54</b> [3.75]
Land EBI (points)	116.4 (50.0)	115.2 (48.0)	1.2 [0.80]
Wildlife zone (%)	48.3 (50.0)	47.4 (50.0)	0.9 [0.58]
<b>Offer structure</b>			
Discount (%)	3.3 (5.8)	3.0 (5.7)	<b>0.3</b> [1.79]
Cover EBI (points)	73.0 (34.1)	70.0 (33.5)	<b>2.9</b> [2.81]
Accepted offer (%)	89.6 (30.5)	90.2 (30.0)	-0.6 [-0.60]

Notes: For each of the eight offer covariates, the table gives the mean and standard deviation statistics, with the later in parentheses. The last column provides the difference in the means with the *t*-statistics in brackets below each difference. Bold values indicated differences that are statistically significant at (two-tailed) *p*-value of 0.05 or lower.

the financial stakes for the study were too low. In the historical CRP General Signups used draw the sample for this study, only 0.58 percent of offer had a rental rate discount in excess of 20 percent. In the full data sets, 10.31 percent of offers had rental rate discounts in excess of 20 percent (Table A.2). To address the problem of excess discounts, we restrict the primary analysis to the 1,925 offers with positive net value, which provides a data set in which only 1.09 percent of offers had discounts exceeding 20 percent. This “positive net value data set” much more closely mirror the upper end of discounts in the actual program participation.

Since the study found that the discount offered was subject to an anchoring effect, restricting the analysis to the positive value data set does impact the balance on the assignment of treatment. As shown in the first column of Table A.3, for all respondents, including those who exited early, the treatment assignment is proportional: near 25 percent for all four treatment groups. For the full data set, the participants in

**Table A.2**  
Discount comparisons between CRP Signups (54 and 56), the positive net value sample, and the full sample.

Discount	Actual CRP	Pos. Net value data set	Full data set
0% (No discount)	75.36	19.79	16.30
0.01 – 5%	3.87	15.43	12.71
5.01 – 10%	6.55	55.53	47.41
10.01 – 15%	8.06	5.51	7.10
15.01 – 20%	5.59	2.65	6.16
20.01% or greater	0.58	1.09	10.31
N	66,608	1,925	2,337

**Table A.3**  
Treatment assignment balance in full, finished, and positive sample.

	Incl. Non-Finishers (%)	Full (%)	Pos. net value (%)
Control	25.00	22.59	20.31
Information	24.34	24.39	20.83
Default	26.03	28.11	32.10
Default + Info	23.97	24.90	26.75
Treatment not assigned	0.66	0	0
Unique participants	1,064	779	701
Round observations	2368	2337	1925

the control group where somewhat less likely complete the experiment, which is reflected in column 2. The treatment assignment of the positive net value data set is skewed toward the high scoring default groups as shown in the last columns of Table A.3.

As a check on the randomness of the assignment of treatment, we also check the balance of covariates across the treatment groups. The mean and standard deviation values for the covariates are shown by group in Table A.4. Of the 48 pairwise t-tests on between groups, only 4 returns statistically significant differences in mean covariates from prior CRP signups. This analysis is for the full sample. When conducting the same tests of balance across treatment groups within the positive net value sample, only 1 of the 48 pairwise t-tests is statistically significant. These tests show that treatment is effectively randomly assigned and there is no evidence for systematic differences between treatment groups.

**Table A.4**  
Treatment balance check on prior CRP General Signup covariates.

Variable	Control	Information	Default	Both
<b>Field characteristics</b>				
Field size (acres)	52.2 (86.4)	51.5 (75.1)	59.0 (108.9)	49.9 (83.7)
Re-enrollment (%)	70.3 (45.8)	72.3 (44.8)	71.0 (45.5)	72.5 (44.8)
Bid cap (\$/acre)	115.00 (64.01)	110.95 (63.63)	114.37 (63.64)	109.29 (61.52)
Land EBI (points)	118.6 (48.1)	112.8 (50.9)	117.3 (51.2)	116.3 (48.1)
Wildlife zone (%)	<b>51.4</b> (50.1)	48.2 (50.1)	<b>41.9</b> 49.4	<b>51.4</b> (50.1)
<b>Offer structure</b>				
Discount (%)	2.9 (5.3)	3.4 (6.0)	3.5 (6.0)	3.3 (5.8)
Cover EBI (points)	72.5 (33.9)	72.3 (35.5)	73.7 (32.8)	73.6 (34.1)
Accepted offer (%)	<b>91.9</b> (27.3)	<b>84.6</b> (36.2)	<b>91.5</b> (27.9)	89.9 (30.2)

Notes: For each of the eight offer covariates, the table gives the mean and standard deviation statistics, with the later in parentheses. Bold values indicated means that are statistically different from at least one other treatment pool at a (two-tailed)  $p$ -value of 0.05 or lower.

#### A.4. Robustness and heterogeneous treatment effects

To illustrate the impact of using the positive net value data set for the primary results, the following analysis presents a set of robustness checks using the full data set and an exploration of heterogeneous response with respect to the anchoring effect. First, we present the regressions results using the full data set and the same econometric models as used in the main text. These results illustrate the robustness of most results with the exception of the anchoring effect on the discount decision. The direction of the average anchoring effect on the discount decision switches between the two data sets. Second, to illustrate the reasons for this switching, we present a quantile regression, which illustrates that the lack of robustness in the discount decision is due to the interaction of the anchoring effect with the excess discounting. Third, we present a transformed discount rate decision, in which the dependent variable is the absolute value of the distance from the default. This analysis demonstrates that, the estimated anchoring effect of the default on the discount decision is smaller in absolute terms when we use the positive net value data set because the effect of the default on negative net value offers is so large.

##### A.4.1. Full data set regression results

The results from Eqs. (1)–(4) with the full data set are presented in Table A.5. In Column (1) of Table A.5, the estimation for the Endogenous EBI (the points allocated based on the cover choice and discount), we see that the default treatment and information treatment do not have significant effects. In the case of the default treatment, this could be due to the strong positive effect on cover choice and the strong negative effect on discount rate (reducing the large discounts that resulted in a negative final value). If the presence of the information nudge indeed attenuates the impact of the endowment, we would expect the coefficient on the interaction term to be positive — offsetting the negative impact of endowment on offer EBI points. With the full data set, it is, and it is significant at the 0.1 level. In Column (2) of Table A.5, we see the perhaps surprising result that the default treatment *decreases* the discount rate. As we introduced in the Data section, this captures the strong anchoring effect *away* from extreme discount levels that result in a negative final value (offers that lose money). We dig deeper into this result below using a positive value sample removing the money-losing offers. The coefficient on the Information treatment is insignificant. Endowment has a similar negative and significant effect on the discount. The interaction term remains positive but is small. In the case of the discount, however, there is the suggestion of learning with a negative coefficient on the

round fixed effect. In Column (3) of Table A.5, the ordered probit regression on cover choice indicates a strong positive effect of the default treatment. The information treatment, on the other hand, has no effect on cover choice. The endowment has a negative effect on cover choice, which is consistent with the expectation that participants will rent seek if they have more desirable land. The interaction term on the endowment and info treatment has no effect. Round fixed effect is not significant, indicating that cover choice did not change over time. Column (4) of Table A.5 indicates that only endowment had a significant effect on the number of revisions, with higher endowments resulting in fewer revisions.

##### A.4.2. Full data set using a quantile regression approach

The effect of anchoring pulling in both directions (up from low discount offers, down from extreme high discount offers) becomes more salient in Table A.6 which presents results from quartile analysis of the discount. The above specification estimates the conditional mean of discount over the treatment indicators and endowment. This table presents the 25th percentile, median, and 75th percentile of discount on those variables. Column (1) of Table A.6 is an Ordinary Least Squares estimation for comparison. Column (2), presents the 25th percentile of endogenous EBI, and for that quartile we see that there is a strong positive effect of the default offer on discount (0.06 percentage point increase). This captures the strong anchoring effect *up* from low discount rates. The median discount quartile has no effect from the default. This barely changes from the full data set. In Column (4) of Table A.6, we see a significant and large negative coefficient on discount, highlighting what we saw in Table A.5: that the default rate reduces extreme discount offers that would be unprofitable to the farmer. Not presented in the table, but of interest — for the 90th percentile of discount offers conditional on endowment, the effect of the 0.09 default would decrease the offers by nearly 50 percentage points. The coefficient on the Information treatment is insignificant throughout.

##### A.4.3. Normalized discount outcome

One method to try to understand the strength of the anchor pulling in both directions is to create a normalized discount outcome by taking the absolute value of the difference of a discount offer and the default of 0.09. Fig. A.2 presents a histogram of all discounts offered in our Full Data Set across all treatment groups. As highlighted in the above section, the long right tail is noticeably different from what is observed in the actual CRP auction. In this histogram we have denoted the 0.09 default discount that some of the treatment groups had pre-filled in the decision screen.

**Table A.5**  
Regressions with full data set.

	(1) Endogenous EBI	(2) Discount	(3) Cover Choice	(4) Revisions
Default treatment	-3.299 (2.977)	-0.0606*** (0.0137)	0.325*** (0.0629)	0.0758 (0.0746)
Info treatment	0.0740* (0.0309)	0.000167 (0.000122)	0.000798 (0.000884)	0.000756 (0.00118)
× Endowed EBI points	-6.193 (6.354)	-0.0101 (0.0262)	0.00783 (0.181)	-0.330 (0.240)
Info treatment	-0.307*** (0.0211)	-0.000337*** (0.0000804)	-0.00338*** (0.000605)	-0.00208** (0.000778)
Constant	172.0*** (4.929)	0.204*** (0.0208)		-0.230 (0.166)
Round fixed effects	Yes	Yes	Yes	Yes
Observations	2337	2337	2337	2337

Standard errors in parentheses.

Note: Individual Cluster Standard Errors Included (Multiple Rounds per Individual).

\*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ .

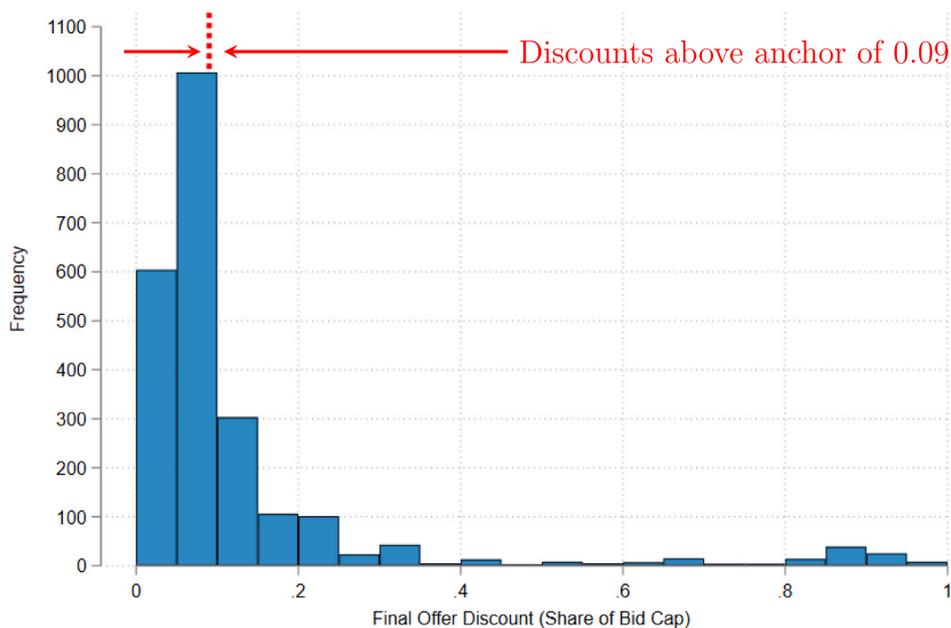
**Table A.6**  
Quantile regression results on discount.

	(1) OLS (Mean)	(2) Q 0.25	(3) Q 0.50	(4) Q 0.75
Default treatment	-0.0775*** (0.0125)	0.0604*** (0.00810)	4.25e-18 (0.00412)	-0.1000*** (0.00902)
Info treatment	0.000102 (0.0000958)	0.0000119 (0.0000446)	1.03e-18 (0.0000243)	-3.82e-18 (0.0000304)
× Endowed EBI points	-0.000659 (0.0226)	0.00218 (0.00698)	-1.95e-16 (0.00500)	8.67e-16 (0.00639)
Info treatment	-0.000128* (0.0000592)	-0.000185** (0.0000708)	-5.32e-19 (0.0000159)	8.01e-19 (0.0000188)
Constant	0.192*** (0.0175)	0.0533** (0.0185)	0.0900*** (0.00509)	0.190*** (0.00979)
Round fixed effects	Yes	Yes	Yes	Yes
Observations	2337	2337	2337	2337

Standard errors in parentheses.

Note: Individual Cluster Standard Errors Included (Multiple Rounds per Individual).

\*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ .



**Fig. A.2.** Illustration of creating the absolute value of the difference between discount offered and the default discount of 0.09.

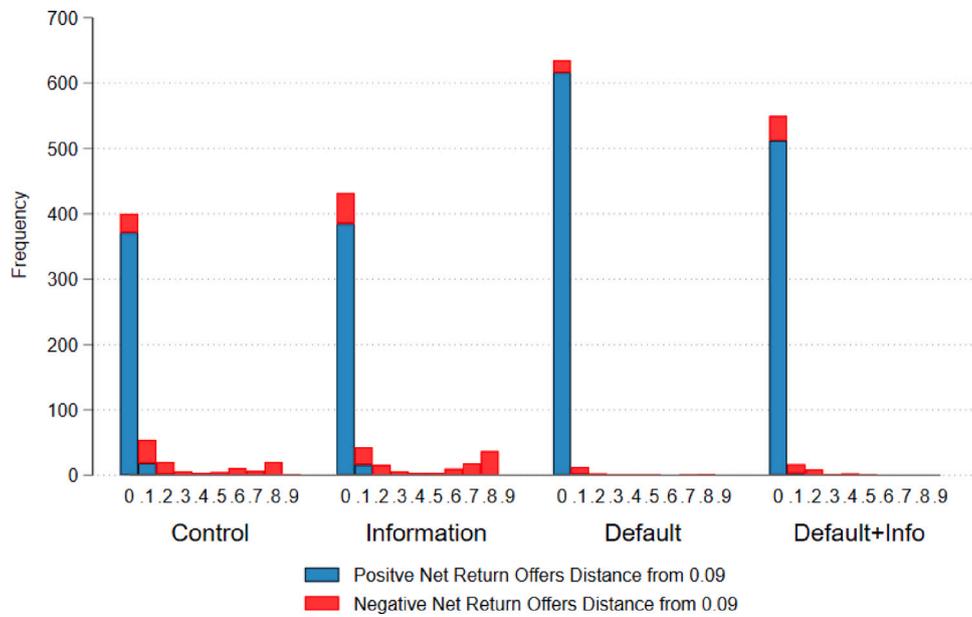


Fig. A.3. Discounts normalized by distance from default of 0.09.

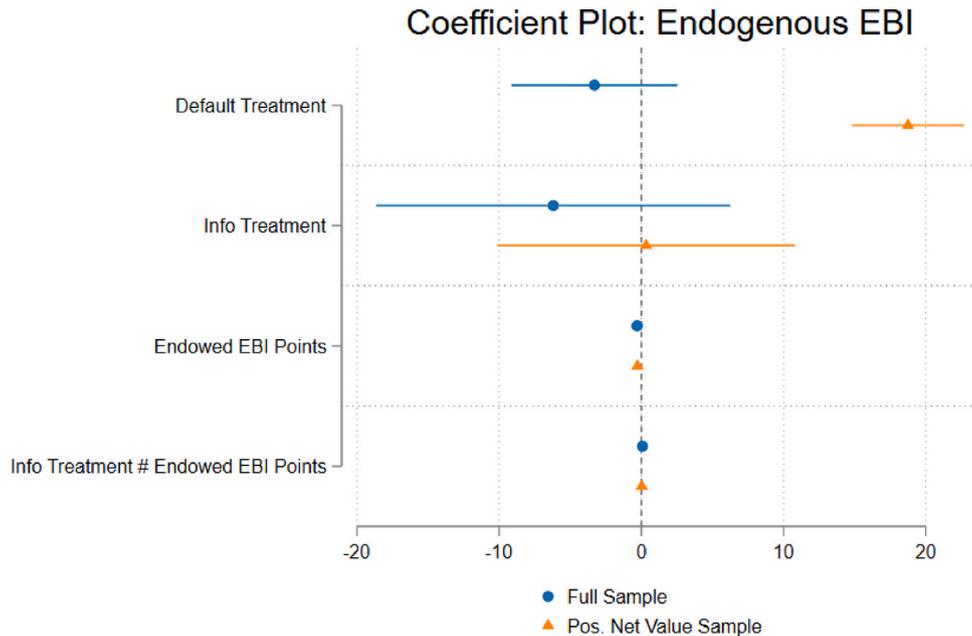


Fig. A.4. Coefficient plot of endogenous EBI.

Fig. A.3 presents the frequencies of the new normalized outcome values. It is possible that a positive net return offer was greater than the default of 0.09, and similarly that a negative net return offer was less than the default of 0.09. However, as is illustrated in the stacked bars in the [0,0.1) and [0.1,0.2) ranges, there are few negative net return offers in those first two bars. What is helpful about this outcome, however, is that it removes the directionality from the impact of the discount. In other words, the sign reversal of the effect of the default on discount rates highlighted in the quantile regression results now would be represented along a positive range.

These comparisons are particularly interesting with our normalized value of discount. The results from Table A.7 are presented now in Fig. A.7. As was stated above, now the sign of the two effects is the same, but the positive net value data set represents the more conservative estimate of the anchoring effect of the default on discounts.

As discussed above, the sign of the coefficient on the default flips between the positive net value data set and the full data set, and again in the quantile regression between the 25th percentile and the 75th percentile, conditional on endowment. This is due to the long tail of extreme discounts, or “over-bidding” of some participants. If we redo the analysis with the normalized discount presented above, we show that the sign of distance from default does not flip and instead shows that size of anchoring effect in the positive net value data set is a conservative estimate of the anchoring impact of the default. Thus, not only is use of the positive net value data set is 1) more conservative of an estimate of the default effect, (2) it avoids making inference based on discounting behavior well outside of what is observed in the actual program and is likely related to the absence of loss-aversion incentives in the experiment.

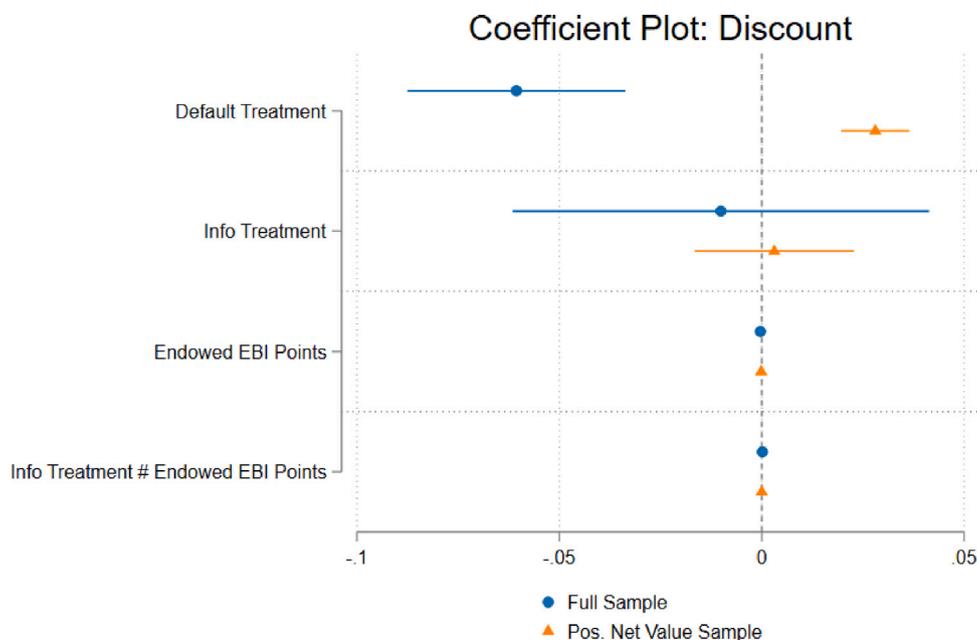


Fig. A.5. Coefficient plot of discount rate.

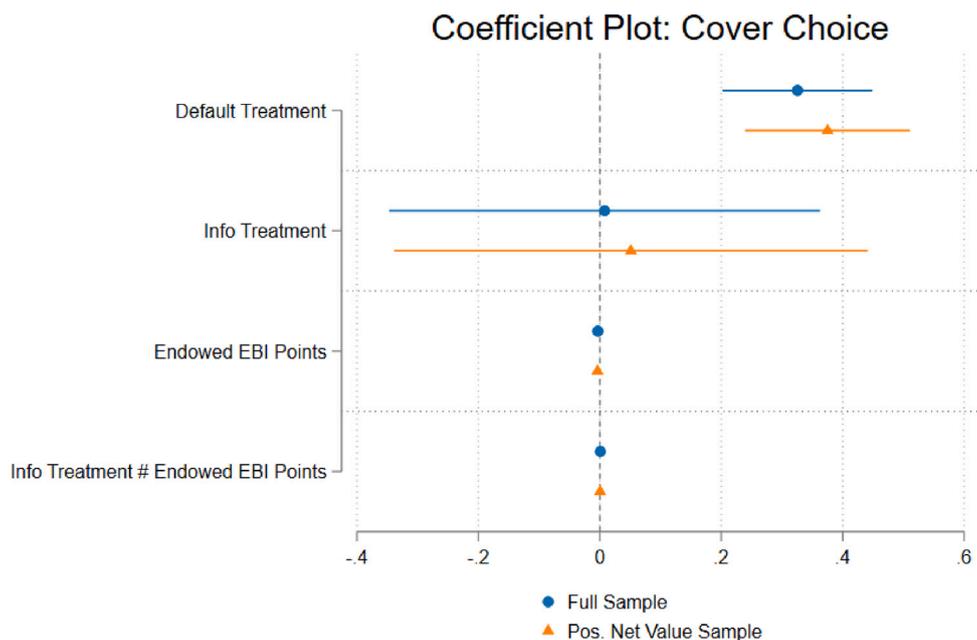


Fig. A.6. Coefficient plot of cover choice.

These regression results using the normalized measure of discount. If the offers are close to the discount because of the discount, then the coefficient on “Default Treatment” should be of a similar magnitude but opposite sign than that of the constant. The larger the constant is, the farther the average of control group offers is from the default of 0.09. Results are presented in Table A.7. Column (1) presents the tobit model of the normalized discount (lower limit of 0) using the Full Data Set. Column (2) presents results from the preferred Positive Net Value data set and Column (3) presents results from the excluded Negative Net Value dataset. The key takeaway across all three is that indeed, the effect of the default is to pull offers closer to that level. Additionally, the estimate from the Positive Net Value is a conservative estimate of the overall effect of the default on offers.

A.4.4. Coefficient plots to compare the model estimates from the positive net value data set and the full data set

A helpful way to visualize the differences in the estimates between the Positive Net Value data set and the Full data set is using coefficient plots of the estimates presented throughout this paper. In the plots below, we compare the coefficients of the Full data set with those of the Positive Net Value data set. To begin, in Fig. A.4 the coefficients and confidence intervals from Table A.5 (Positive Net Value data set) are presented with blue circles, while the coefficients and confidence intervals from Table 7 (Full data set) are presented with the orange triangles. A striking difference is visible in the first rows of the effect of the default treatment on Endogenous EBI: the positive net value sample is significantly greater than zero, while the full data set estimate is not different from zero. A look at Figs. A.5 and A.6 quickly illustrates that the difference in endogenous EBI is largely from the difference in

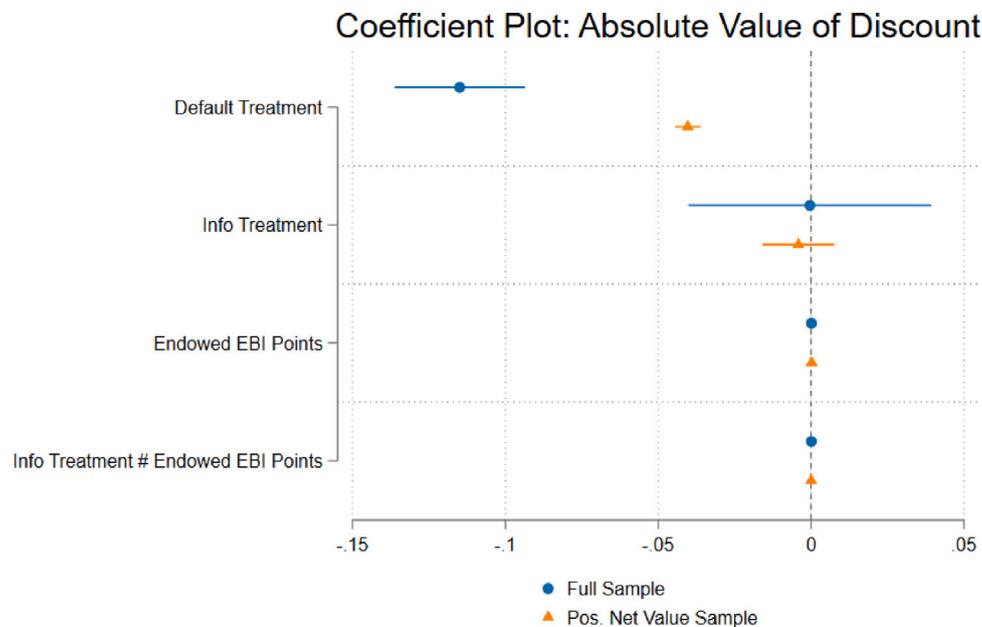


Fig. A.7. Coefficient plot of absolute value of the difference between the offered discount rate and the default discount rate.

Table A.7  
Normalized discount regression results.

	(1) Full	(2) Positive net value	(3) Negative net value
Default treatment	-0.115*** (0.0109)	-0.0404*** (0.00217)	-0.229*** (0.0366)
Info treatment	0.0000586 (0.0000834)	0.00000813 (0.0000304)	-0.000465 (0.000444)
× Endowed EBI points			
Info treatment	-0.000435 (0.0203)	-0.00418 (0.00600)	0.110 (0.0901)
Endowed EBI points	0.0000922 (0.0000506)	0.000129*** (0.0000202)	0.000820* (0.000347)
Constant	0.124*** (0.0154)	0.0341*** (0.00435)	0.196** (0.0668)
Round fixed effects	Yes	Yes	Yes
Observations	2337	1925	412

Standard errors in parentheses.

Note: Individual Cluster Standard Errors Included (Multiple Rounds per Individual).

\*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ .

discount and not from the differences in cover choice. The effect of the anchoring default seems to pull the sample in both directions between the two data sets. The robustness of the cover choice is in part because the default cover choice was already “best”.

### Appendix B. Supplementary data

Supplementary material related to this article can be found online at <https://doi.org/10.1016/j.foodpol.2023.102504>.

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