Addressing Participant Inattention in Federal Programs: A Field Experiment with the Conservation Reserve Program

Steven Wallander, Paul Ferraro, and Nathaniel Higgins

Voluntary land conservation programs depend upon the willingness of land owners to participate. Since participation requires commitment to long-term contracts, most studies on participation focus on changes to the pecuniary incentives facing land owners. This study presents a large-scale field experiment within the USDA's Conservation Reserve Program (CRP) that examines whether informational outreach, including behavioral nudges, could improve land owners' willingness to participate. The experiment evaluates the impact of three types of reminder letters on the rate at which land is offered into the CRP. We find that for the most well-informed group, farms with expiring CRP contracts, the reminder letters did improve participation. We interpret this result as evidence of inattentive behavior. We do not detect any differences in the estimated treatment effects among the basic reminder letter and the letters augmented with peer comparisons and social norm messaging, nor do we detect any treatment effect among currently unenrolled farms. From a policy perspective, these results imply that the USDA can generate additional CRP offers among farms with expiring contracts at an average cost of $39 per additional offer. Assuming a twenty-five million acre program, reminder letters sent during every sign-up period would result in re-enrollment offers from an additional 420,000 acres. Using simulations based on offers from prior CRP sign-ups, we estimate that these additional offers in the CRP auction would reduce program costs. Depending on the year of simulation, the outreach effort achieves a benefit-cost ratio of between 20:1 and 90:1.

Key words: Land conservation, auction participation, outreach effects, field experiment, inattention, peer comparison.

JEL codes: Q28, D44, D83.

Non-pecuniary interventions offer an appealing approach to public policy, particularly "nudges" that seek to influence outcomes by presenting individuals with information and allowing the individuals the freedom to choose (Sunstein 2014). Some interventions are explicitly based on new ideas from psychology and behavioral economics—such as bounded rationality, framing effects, or loss aversion. Other interventions are based on more traditional notions of information transfer, such as advertising and outreach efforts. In studies that use experimental designs to identify causal effects, informational interventions have been found to affect a wide range of important policy outcomes, such as earned income tax credit (EITC) uptake (Manoli and Turner 2014), school choice (Hastings and Weinstein 2008), and water use (Ferraro, Miranda, and Price 2011). We know of no such published study in the agricultural economics literature.

To expand this research into agricultural economics, we conduct a large-scale field experiment to examine the effectiveness of low-cost, behaviorally-informed outreach

Steven Wallander is an economist with the USDA Economic Research Service. Paul Ferraro is a professor at Johns Hopkins University. Nathaniel Higgins is a senior fellow with the Social and Behavioral Sciences Team of the Office of Evaluation Sciences. The authors wish to acknowledge the assistance of Richard Iovanna of USDA's Farm Service Agency, and Daniel Hellerstein, Chengxia You, and Ryan Williams of the EPA's National Center for Environmental Economics Research Service for critical assistance in developing the data necessary for this study. The authors would also like to acknowledge the helpful comments received from our anonymous reviewers and feedback on preliminary findings from seminar participants at the EPA's National Center for Environmental Economic and the 2014 World Congress of Environmental and Resource Economists. The views expressed in this paper are those of the authors and do not represent the views of USDA, Johns Hopkins University, or the Office of Evaluation Sciences. Any errors or omissions are our responsibility. Correspondence to be sent to: swallander@ers.usda.gov.

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within one of the USDA's largest conservation programs, the Conservation Reserve Program (CRP). In the CRP, owners of eligible cropland enroll their land in ten- or fifteen-year contracts, during which time they agree to establish and maintain conservation covers that provide environmental benefits such as reduced soil erosion, improved wildlife habitat, improved off-site water quality, and carbon sequestration. In exchange, the owners receive annual rental payments from the USDA. As of September 2013, the CRP had about 26.8 million acres enrolled and made more than $1.5 billion in payments annually.

In most voluntary conservation programs, including the CRP, increasing financial incentives would be a reliable, but potentially costly, way to increase program participation. Behavioral economics and psychology research suggest that other approaches can also be effective and less costly. For example, informational outreach may encourage greater participation if farmers are not perfectly well-informed about the program or have not processed all of the available information about all possible uses for their land. The latter is a potential source of "rational" inattention. Informational outreach may also induce greater participation by harnessing pro-social preferences or invoking social norms. Given existing outreach efforts, the relevant policy question is whether the USDA would find it worthwhile to devote additional resources to informational outreach. While theory and empirical studies from other contexts provide some guidance (e.g., Kahnemann 2003, Ferraro, Miranda, and Price 2011), the best guidance would come from evaluating potential interventions within the actual operation of the program.

To accomplish this, we conduct a large-scale field experiment within the General Signup portion of the CRP (explained in more detail below). Three versions of an informational letter, some containing peer comparisons and norm-based nudges, were mailed to 88,533 farms with land eligible for participation in the program. Existing administrative data provided information on program participation for those farms and for a control group of 849,787 farms. We estimate treatment effects on the likelihood of making an offer.

Potential participants, especially those who have participated before, are arguably fully informed about the program. Thus, any effect of the letters is likely mediated by inattention or pro-social preference and norms, rather than any sort of information deficit. We do not attempt to parse whether inattention is rational or otherwise, nor do we attempt to elucidate the way in which pro-social preferences or social norms operate. Instead, we take a pragmatic approach in an attempt to find what interventions work best in an important policy environment (Sunstein 2014; Chetty 2015).

Background

Information about market opportunities is costly to obtain and process, an observation that underlies the bounded rationality approach to behavioral economics (Kahnemann 2003). Even if information is freely available, individuals have been shown to have difficulty attending to many features of information simultaneously (Chetty, Looney, and Kroft 2009). This suggests that providing targeted information may influence choice, even among the well-informed. One possible explanation for such behavior is the concept of rational inattention (Sims 2006; Cheremukhin, Popova, and Tutino 2015), a theory that has found support in lab experiments (Goecke, Luhan, and Roos 2013). Within the context of payments for environmental services from agricultural land, policy makers may be able to improve program outcomes by recognizing and responding to information constraints, whatever their source. In non-experimental settings it is difficult for researchers to isolate the causal impact of information since in most cases it is difficult to discern who has received information and who has not. Our solution is to conduct a field experiment in which the provision of information is randomized across individuals (Bertrand et al. 2010). In developing this field experiment, we consider several different pathways through which program information may influence CRP outcomes.

Informational Nudges and Payments for Environmental Services

The CRP began in 1985 following the passage of the Food Security Act (FSA), and land was first enrolled in 1987. Owners of eligible cropland can offer to enroll their land in ten- or fifteen-year contracts, during which time they agree to establish and maintain conservation covers that provide environmental

benefits such as reduced soil erosion, improved wildlife habitat, improved off-site water quality, and carbon sequestration. In exchange, the land owners receive annual rental payments from the USDA. As of September 2013, the CRP had about 26.8 million acres enrolled, down from a peak enrollment of 36.8 million acres in fiscal year 2007. The expected 2014 fiscal year outlay for CRP is about $1.63 billion, down from a peak of $1.93 billion (not adjusted for inflation) in 2008.[1] Declining program participation is a result of both shrinking authorizations and the difficulty of enrolling land in the program in an era of high commodity prices.

There may be many farmers who are unaware of their opportunity to participate in CRP, or who have not devoted time to evaluating the return to CRP participation against the return to crop production. In such a situation, outreach letters can serve as advertising (Bagwell 2007). Moreover, these letters can be customized to provide content that is tailored to each individual recipient. Individualized letters have been shown to induce energy and water conservation at the household level (Allcott 2011; Costa and Kahn 2013; Ferraro, Miranda, and Price 2011). In the survey response literature, reminder letters are a key component of a well-developed outreach methodology (Dillman and Frey 1974).

Even if farmers are relatively well-informed about the CRP, however, the framing of information about the program may influence how they assess the program. For example, social comparisons have been shown to increase the amount of water conservation (Ferraro, Miranda, and Price 2011) and electricity conservation (Alcott 2011). Since social comparisons could either invoke social norms or provide a heuristic for evaluating opportunity costs, another key finding is that invoking social norms without making social comparisons can also encourage water conservation (Ferraro and Price 2013).

The role of information in determining program outcomes is not widely covered in the literature on payments for environmental services. Program participation is usually modeled as being determined exclusively by factors that influence pecuniary incentives. In addition, most research on existing program outcomes utilizes non-experimental, observational data. For CRP in particular, many studies use econometric models to evaluate the effect that various economic and environmental factors have on the share of cropland enrolled in CRP by county (Plantinga, Alig, and Cheng 2001; Suter, Poe, and Bills 2008) or the likelihood of an individual farm having land enrolled in CRP (Brady and Nickerson 2009). Since enrollment status reflects both the farmer's decision to offer land into the program and the USDA's decision to accept a given offer, other studies isolate the farmer's decision by looking at how economic and environmental factors influence the share of eligible cropland in a county that is offered to the program (Plantinga, Alig, and Cheng 2001; Isik and Yang 2004; Wu and Lin 2010; Wallander et al. 2013). Many of the factors that influence program participation also influence the structure and competitiveness of bids (Kirwan, Lubowski, and Roberts 2005; Vukina et al. 2008). Within the CRP literature, a key innovation in our study is that we are able to evaluate offer behavior at the farm level among eligible farms (as the county-level participation studies had done).

CRP Auction Structure

Most of the land in CRP is enrolled through an auction known as the General Sign-up, which usually occurs once per year.[2] The 2012 General Sign-up is significant because of the large number of expiring contracts, and the amount of acreage in those contracts. About 6.5 million acres of existing contracts were set to expire in 2012, as compared to 3.2 million acres in 2013, and less than 2 million acres per year from 2014–2016. Since all expiring acres are eligible to submit a bid to

[1] See monthly and annual reports for acreage and expected program outlays at the following address: http://www.fsa.usda.gov/FSA/webapp?area=home&subject=copr&topic=rns-css. Our 2013 acreage and 2014 expected outlay numbers come from the September 2013 monthly summary. Our peak acreage and budget number come from an examination of the 2011 and 2009 annual summaries.

[2] Two separate mechanisms are used to enroll privately-owned land in the CRP. The first mechanism is referred to as General Sign-up, which has been used to enroll most of the land area currently in CRP. The second mechanism is referred to as Continuous Sign-up, which enrolls a significant number of contracts, but those contracts tend to cover a much smaller parcel of land. As of July 2013 there were approximately 290,000 CRP contracts covering over 21 million acres that had entered the program through General Sign-up (average contract size of approximately seventy-four acres), and about 410,000 CRP contracts covering 5.5 million acres that had entered the program through continuous signup (average contract size of approximately thirteen acres).

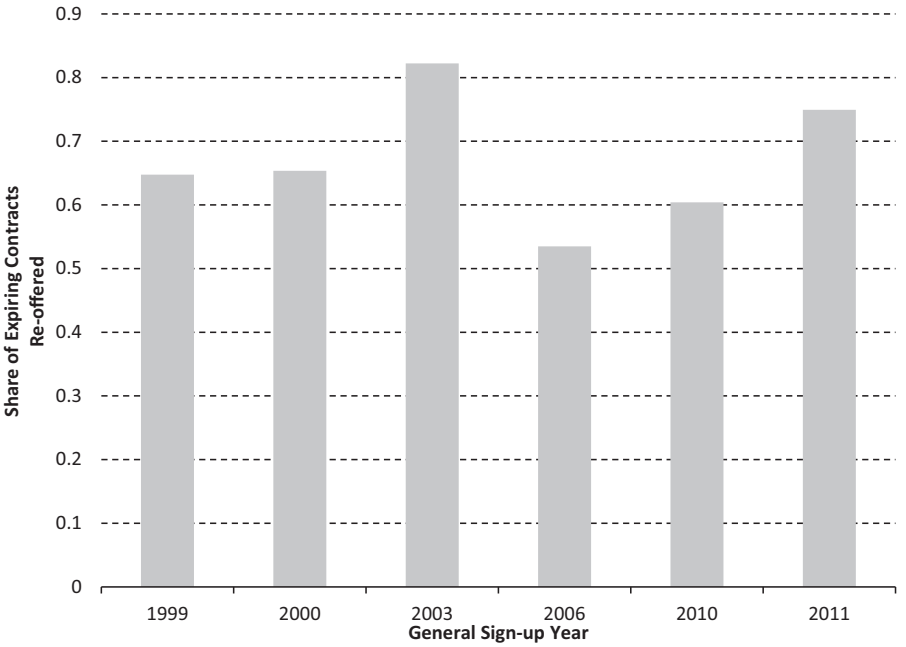

Figure 1. Re-enrollment of expiring contracts

reenroll in the program, the surge of expirations in 2012 provided a unique opportunity to conduct a large-scale field experiment on CRP enrollment decisions.

Interest in enrolling in CRP has fluctuated over time in response to market factors and various aspects of the program design. These movements can be seen in the share of expiring acres that re-enroll in each general sign-up (figure 1). Interest in the program among land owners with unenrolled land is perhaps even more sensitive. While total interest in the program fluctuates over time, there is a clear trend of increasing contract acceptance rates over time (figure 2).[3]

Increasing acceptance rates are particularly notable since the 2008 Food Conservation and Energy Act dramatically reduced the national enrollment acreage cap, and there is continuing pressure for further reductions in the enrollment cap (Stubbs 2012). Given the program constraints, we might assume that competitiveness would be increasing rather than decreasing. One possibility is that high commodity prices are reducing farmer interest in the program (Hellerstein and Malcolm 2011) while also leading to an increase in program costs (Stubbs 2012).

Eligibility for CRP requires that farmland meet a combination of prior land-use and environmental-characteristic thresholds. Fields that are under an expiring CRP contract are, however, fully eligible to submit an offer to re-enroll, regardless of any changes to eligibility criteria since the original enrollment. Unenrolled fields must have been cropped during four years out of the six-year period indicated in the authorizing (most recent) farm act. Unenrolled fields must also either be highly erodible (according to an erodibility index that can be calculated individually for any field), or be located in a Conservation Priority Area, the boundaries of which are set by FSA in consultation with the states. An estimated 212 million acres meet these eligibility requirements (Wallander et al. 2013).

The General Sign-up is a pay-as-bid auction. For a given piece of land, farmers must choose a bid rate, which is the annual "rent" (per acre) that they will receive for the term of the contract if their offer is accepted by the USDA. In addition, farmers must select the conservation practices, such as the type of cover crop to plant on their enrolled acreage. Some more intensive conservation practices

[3] Reliable data on offer rates among eligible but unenrolled land is not available over this full time period. The eligibility requirements have changed over time and it is difficult to determine, among fields or farms not participating, which are eligible for the program and which are not.

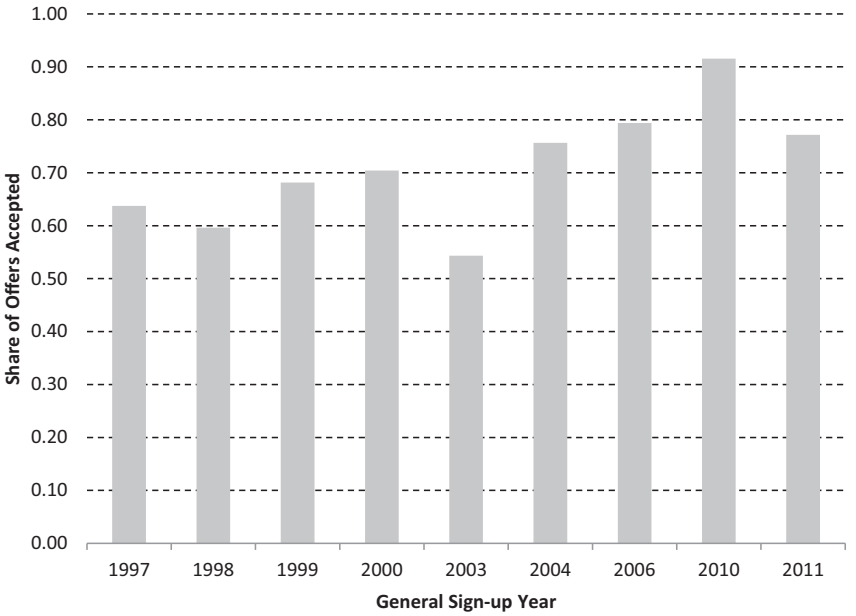

Figure 2. Acceptance rate of general sign-up offers

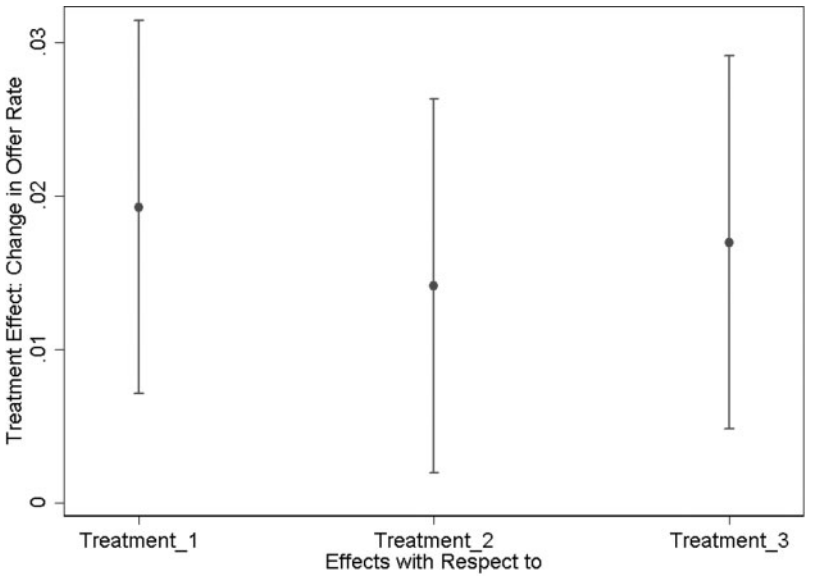

Figure 3. Treatment effect estimates for population of farms with expiring contracts

Note: Treatment effects are from a linear model of offer rates with state-level fixed effects to account for stratification in the experimental design.

are eligible for cost-share assistance and, if they have selected those practices, farmers may also choose whether to accept the cost-share payments.

The General Sign-up is a scoring auction (Asker and Cantillon 2008); bids are ranked based on a single score that collapses all relevant information, such as price and quality, onto one dimension. The score, known as the Environmental Benefits Index (EBI), consists of six factors, five of which rank environmental benefits and one which ranks the contract cost. Many of the factors are determined only by land characteristics. Some of

the factors can be influenced by a farmer's choice of conservation practices, bid rate, and cost share acceptance. Farmers are told their EBI when submitting an offer, with the exception of a "cost factor" that depends in part upon decisions that FSA makes following the closing of each General Sign-up. Most General Sign-up periods are announced approximately a month before the sign-up period opens. During the sign-up period, which usually last for four weeks, farmers can travel to their county FSA offices to complete an offer. Following the closing of each sign-up period, the FSA evaluates the full pool of offers and selects a "cutoff EBI." Offers with an EBI at or above the cutoff are accepted for enrollment in CRP, and lower EBI offers are rejected.

Experimental Design

We set the foundation for our experimental design by distinguishing between two different populations of farmers. For both populations, we determine treatments such that two particular behavioral nudges—a social comparison and a pro-social nudge—are nested within a core treatment that provides only basic information about the program.

Defining the Two Populations

During the March 2012 General Sign-up, the 6.5 million acres with expiring CRP contracts were automatically eligible to submit an offer. We consider these farms the "high information" population primarily because their prior CRP contracts are a clear indication that they understand the bidding and enrollment process. Through their prior involvement, these farms have also revealed a higher willingness to participate in the program, therefore we do not pool the two populations. Prior to our outreach effort, these farms had already received at least one letter from the USDA notifying them that their CRP contracts were set to expire and indicating that they were therefore eligible to submit an offer for re-enrollment. Over recent years, a much greater share of offers comes from land reenrolling than from "new" land entering the program, suggesting that this high information population may be generally more inclined to participate in the program and therefore more likely to be influenced by a reminder letter. These contract expiration letters are qualitatively different from the letters in this study both with respect to timing (sent weeks before the sign-up) and content.

With over 200 million acres eligible for the program but less than 30 million acres enrolled, there is also a large population of farmers with eligible but currently unenrolled cropland. These farms may have other land enrolled in CRP, but they have no land with expiring contracts. We treat this group as a second population, applying the same treatments but conducting a separate analysis. We view this population as consisting of "low-information" farms with respect to CRP. While all of these farms had access to the USDA's broadcast outreach efforts—county office newsletters, USDA website postings, and local and industry news coverage—that precede every General Sign-up, prior to our effort these farms had not received individualized letters informing them about the enrollment period. To identify these farms, we developed a national, field-level database of cropping history, indicators of conservation priority area status, and erodibility index measures to identify all eligible but unenrolled land.

To ensure that the treatment would be uncorrelated with factors that vary across states, such as variations in state-level outreach efforts beyond the national efforts, the assignment of treatments is balanced across states. We stratify each of the two populations by state, and then apply the randomization of treatments within those strata.

Assigning the Treatments

Prior to assigning treatments, we performed a power analysis to determine the number of treatments that the population and budget could support (Royston and Babiker 2002; see appendix for additional details). We nest the three treatments to evaluate the additional impact of adding more information to reminder letters and to test different hypotheses that are prevalent in the behavioral economics literature. Treatment 1, the basic treatment, consists of an informational letter that reminds farmers about the General Sign-up and their eligibility for the program. The key behavioral insight embedded in the letter is a reminder that the General Sign-up period had begun. The following text box was

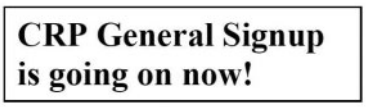

Figure 4. Basic reminder text appearing in all three treatments

located prominently at the top of the letter (figure 4).

Treatment 2 keeps the same content as treatment 1 and adds a side box that told farmers how other "stewards" in their state had provided ecosystem services "for their neighbors" through participation in CRP, suggesting the popularity of CRP. Since prior literature has found that social comparisons that also invoke social norms are most effective (e.g., Hallsworth et al. 2014), we incorporated this version of the comparison in our second treatment. While the stewardship norm is intended to induce greater provision of public goods, that provision occurs through the selection of better practices that also generate a private benefit for the farmer, namely a higher EBI score. Given that this experiment occurs in the context of an auction, treatment 2, by suggesting that individuals other than the recipient are bidding aggressively, could deter the recipient from bidding in the auction (all else being equal, a potential auction entrant would be less likely to bid if other bidders make the prospect of participating less valuable). We customize the information at the state level by calculating the additional EBI score gained through improved practices. The range of additional EBI was from forty-four to eighty-two. The additional points noted in the text below are the 75th percentile of points earned on the EBI through conservation practice selection. The portions of the text that vary by state are highlighted here for emphasis but are not highlighted in the original letter (figure 5).

The third treatment also includes a social comparison, but one with less obviously normative content. The third treatment augments treatment 2 by adding two boxes, both of which signal to farmers the private benefits of enrolling in CRP. The first box emphasizes the stability of CRP payments. The second box uses a peer comparison to signal the regional popularity of CRP. We define the peer group at the state level in order to ensure that the magnitude of participation by

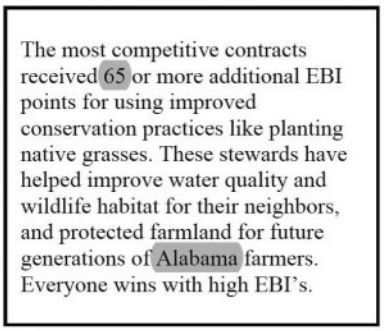

Figure 5. Stewardship norm text appearing in treatements 2 and 3

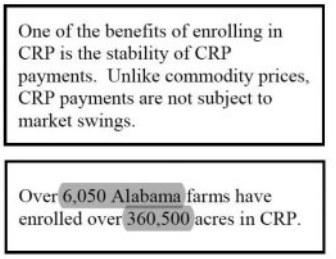

Figure 6. Private benefit and social comparison appearing in treatment 3

other farmers would convey a positive assessment of the benefits of participating in CRP. If we had defined the peer group at the county level, the social comparison may have had greater, more individualized salience (e.g., Hallsworth et al. 2014) but would potentially have signaled lower participation rates in some areas and therefore lower benefits of participation in some of the counties. As before, the portions of the text that are individualized by state are highlighted here but were not highlighted in the letter that went to farms (figure 6). The full version of the treatment three letters for farms with expiring contracts is presented in the appendix (figure A.1).

For high-information farms with expiring contracts, a total of 39,509 letters were sent to 35,127 farms, which are divided into three different treatment groups. Another 11,696 farms serve as the control group. Following the conclusion of the General Sign-up, offer data were linked to the treatment data. A farm operation was coded as making an offer if an offer was made on any of its eligible fields.

Table 1. Summary Statistics for Expiring Contract Population

	Treatment Group			
	0 (Control) No reminder letter	1 Basic reminder letter	2 Social norm ("steward") addition	3 Peer comparison addition
Sampling and treatment				
Sample size (operations)	11,696	11,711	11,699	11,717
Letters sent		13,236	13,091	13,182
Participation outcomes				
Offers	6,720	6,953	6,886	6,929
Offer rate	0.575	0.594	0.589	0.591
***Ex ante* covariates**				
Total crop acres	451.7	443.9	440.3	449.4
	(924.1)	(854.1)	(805.1)	(886.2)
Any continuous contract (0/1)	0.228	0.235	0.239	0.239
	(0.420)	(0.424)	(0.427)	(0.427)
Former EBI on expiring	298.1	298.5	298.4	298.0
	(28.87)	(28.98)	(29.10)	(28.92)
Former Soil Rental Rate (SRR) on expiring	47.90	47.66	47.80	47.80
	(23.27)	(22.73)	(23.02)	(22.86)
Total CRP acres (on all contracts)	181.7	184.8	182.3	184.8
	(298.2)	(303.0)	(298.5)	(308.9)
Total expiring acres	113.5	115.7	114.4	117.1
	(170.1)	(180.7)	(175.4)	(183.1)
Share of county cropland enrolled in CRP	9.483	9.425	9.476	9.460
	(5.560)	(5.576)	(5.547)	(5.562)
Bid structure outcomes				
Acres offered	115.1	114.3	113.4	118.1
	(178.0)	(183.7)	(181.7)	(190.9)
Bid-down (percentage of bid cap)	4.495	4.610	4.637	4.604
	(6.565)	(6.641)	(6.636)	(6.553)
Rent (per acre payment)	72.67	72.00	73.02	72.90
	(45.07)	(44.41)	(45.21)	(45.40)
Total EBI score	266.5	266.7	266.4	266.6
	(45.99)	(47.00)	(47.22)	(47.17)
Practice EBI score	175.3	174.9	175.2	175.3
	(54.37)	(55.50)	(55.68)	(56.26)

Note: The population consists of farms with General Sign-up CRP contracts set to expire in the fall of 2012. As described in the text, expiring contracts were aggregated up to the farm operation level as needed. Data on *ex ante* covariates are drawn from the CRP Contract data. Data on bid structure outcomes are drawn from the CRP Offer data.

In the application of the treatments for the eligible-but-unenrolled population, there was a minor coding error in the mail merge program used to individualize the letters. The error impacted treatments two and three for this population by essentially introducing a typographic error (an added word) into the text boxes containing the social comparison nudge. We take this typographic error into account when interpreting the results for this population. Essentially this coding error inadvertently provided a test of the impact of providing farms with faulty, or noisy, information. The interpretation of the effect of a simple informational letter (treatment 1) for this population is not impacted by the coding error. The appendix includes a more detailed description of the result of this coding error, along with a sample of that letter (figure A.2).

Results

The results for the two populations are presented separately. For each group, we first

Table 2. Bid Structure Outcomes for Expiring Contract Population

Treatment	Total EBI score	Practice EBI score	Bid-down	Rent	Acres offered
1	0.782	0.333	0.108	−0.510	1.843
Basic	(0.692)	(0.814)	(0.091)	(0.437)	(1.670)
reminder	*0.259*	*0.683*	*0.237*	*0.250*	*0.276*
2	0.503	0.501	0.113	0.320	0.685
Social	(0.552)	(0.606)	(0.097)	(0.384)	(2.304)
norm	*0.362*	*0.408*	*0.245*	*0.411*	*0.768*
3	0.360	0.136	0.115	−0.052	**3.811**
Peer	(0.627)	(0.798)	(0.087)	(0.385)	(2.246)
comparison	*0.566*	*0.864*	*0.186*	*0.894*	*0.097*
constant	**262.229**	**164.907**	**5.250**	**72.709**	**66.049**
	(4.868)	(5.961)	(0.412)	(0.270)	(1.339)
	0.000	*0.000*	*0.000*	*0.000*	*0.000*

Note: Each column gives the coefficients from a regression of the outcome variable on the treatment dummy variables with state fixed effects. Clustered standard errors are given in parentheses. P-values are given in italics. Bold values are statistically significant at $p = 0.1$.

present the effect of letters on the offer rate and then present the average difference in bid structures across treatment, which we note are not identified as causal effects. To illustrate the significance of our findings in the context of a large reverse auction, we then present a simulation of outcomes under alterative bid acceptance scenarios. Finally, we explore potential treatment effect heterogeneity.

Treatment Effect for Farms with Expiring Contracts

Summary statistics and offer rates for the expiring contracts population are presented in table 1. The first four rows provide the basic results from the experiment in terms of the population size, number of letters, number of offers, and offer rate for each group. The *ex ante* covariates that are observed in both control and treatment farms are also displayed; these variables demonstrate the covariate balance across treatment arms. All variables are found to be balanced with the exception of the share of farms with land in continuous CRP acres. (See table A.1 in the appendix for the results of the tests).

For high-information farms with expiring contracts, outreach letters increase offer rates. Across treatments 1 to 3, the treatment effects are the differences in offer rates from the control group: 1.9, 1.4, and 1.7 percentage points, respectively. Each treatment effect is statistically significantly different from zero; the p values for the three treatment effects are 0.002, 0.023, and 0.006 for treatments 1, 2, and 3, respectively.[4] We interpret these results as being consistent with the hypothesis that psychological factors such as limited attention are important determinants of program participation.

When looking across letters, however, differences between the treatment effects are not statistically significant. We do not detect any additional effect of augmenting the informational letter with the social comparison or pro-social nudges. Given that there is no statistical difference between treatments, if we pool all three treatments the offer rate among all high-information farms receiving a letter is 59.1%, indicating that the treatment effect of receiving any letter is an increase in the offer rate of 1.68 percentage points. The 95% confidence interval for the pooled treatment effect is 0.69 to 2.68 percentage points

Additional outreach letters are a very low cost way of generating additional offers. The USDA can generate sixteen to seventeen additional offers per 1,120 letters sent, or about one offer for every sixty-seven letters.[5] At a cost of 58.5 cents per letter, which covers the total cost of the printing and mailing, this

[4] These p-values are based on t-tests on the coefficients from a regression of the binary offer variable on the treatment dummy variables and state-level fixed effects. We also added some farm-level covariates that could explain some of the variation in participation. Since the assignment of treatment is random, the only reason to include these covariates is to improve the precision of the treatment effect estimates. The estimates themselves do not change (as expected given randomization of treatment) and the improvements in standard errors are small, and are thus not reported in a new table.

[5] This estimate is made using the pooled treatment effect of 1.68 percentage points and the average of 1.12 letters sent per operation.

amounts to a cost of $39 per additional offer.[6] If a letter was sent to all farms with expiring contracts, it would have boosted participation by 786 bids. In aggregate, if we assume a 25 million acre program and no difference in long-run response to the informational nudge, providing reminder letters to expiring contracts during every sign-up period would result in re-enrollment offers from an additional 420,000 acres.

Effects on bidding behavior are also of interest since the additional information could affect key determinants of the auction outcomes. Conditional on making an offer, farms made decisions about how to structure their bid. The tests of differences in average values for the bidding outcomes variables are shown in table 2. The outcomes that we examine are the average acres per offer, the cost per acre (the "rent"), the bid-down (the percentage difference between the rent and the maximum bid), and the portion of the EBI that can be influenced by the selection of improved conservation practices (the "practice EBI").

Changes in average bid structure are only observable conditional on participation, which has implications for how we interpret the bid structure results. If we think of bid structure as a set of latent variables that are correlated with the probability of making an offer, this challenge becomes clear. Differences in expected bidding outcomes, if observed, between treatments could result from either an actual "intensive margin" change in bidding behavior among farms who would have made an offer even without treatment, or from a change in the composition of the population of farms that makes an offer that would occur if the treatment brings in more "marginal" offers from farmers who have a latent rent, bid-down, or practice EBI that is higher or lower than the average "non-marginal" farmer. For example, if we observe a statistically significant increase in the amount of bid-down, we cannot tell whether that is due to the letter causing farmers to bid more aggressively on average, or whether it is due to an increase in the share of farmers making bids who are willing to bid down more. Econometrically, we cannot experimentally identify the effects of our treatment on bidding behavior, since the probability of assignment to treatment is—as we have shown—correlated with whether or not an offer is made that is correlated with the latent variables underlying observed bidding structure. Table 2 shows the results of regressions that capture differences in these outcomes across treatment groups.

For the average acres per offer, the differences between the treatment groups and the control group are not statistically different for treatments one and two. For the third treatment there is a statistically significant difference of just under four acres per offer. This could indicate that the social comparison nudge encouraged greater acreage on the intensive margin, or it could indicate that the social comparison nudge increased participation among farmers with more eligible land.

For the cost per offer, we look at two variables—total cost per acre and bid-down, which is the percentage reduction in the bid relative to each farm's bid cap. Neither outcome is statistically different across treatment groups. This holds even if we account for the corner solution on bid-down and the large number of offers with zero bid-down by estimating the correlation with treatment using a random-effects Tobit model.

For potential quality differences in contracts, we look at both the total EBI and the portion of the EBI due only to the selection of conservation practices. We fail to detect any differences across treatment groups that are statistically significant.

Treatment Effect for Farms with Eligible but Unenrolled Land

For the low-information farms with eligible but unenrolled fields, a total of 62,196 letters were sent to 53,404 farms; those farms are compared to a control group containing 849,787 farms.

Summary statistics and offer rates for the eligible-but-unenrolled population are presented in table 3. For farms with eligible but unenrolled land, we cannot detect an increase in offer rates. For these farms, offer rates are very low in the control group, about 0.2% (table 3). The point estimates for the offer rates are all less than one offer per 1,000 letters, negative for two of the treatments, and are not statistically significant. A treatment effect of 1.68%—the effect we find in the high-information case—would represent greater than an 800% relative increase in offer rates for this population.

[6] This cost calculation reflects the cost to USDA of printing, folding, and mailing the letters, including postage. This cost does not include the cost of data development and program necessary to identify farms and customize letters. For the population of farms with expiring contracts, these costs are negligible.

Table 3. Summary Statistics for Eligible but Unenrolled Population

	Treatment Group			
	0 (Control) No reminder letter	1 Basic reminder letter	2 Social norm ("steward") addition	3 Peer comparison addition
Sampling and treatment				
Sample size (operations)	849,787	17,794	17,796	17,814
Letters sent		20,696	20,750	20,746
Participation outcomes				
Offers	1,847	34	48	38
Offer rate	0.0022	0.0019	0.0027	0.0021
Bid structure outcomes				
Acres offered	81.80	93.09	55.56	47.40
	(151.8)	(157.3)	(84.29)	(51.51)
Bid-down (percentage of bid cap)	4.290	5.286	3.330	4.472
	(6.64)	(6.33)	(5.83)	(6.57)
Rent (per acre payment)	93.78	84.15	95.69	101.9
	(59.77)	(53.48)	(61.47)	(58.81)
Total EBI score	259.0	266.3	262.1	265.8
	(48.92)	(47.73)	(48.25)	(47.80)
Practice EBI score	180.1	180.4	185.7	190.9
	(57.03)	(64.40)	(53.25)	(47.08)

Note: The population consists of farms with fields that were identified as eligible to participate in CRP but were currently unenrolled. As described in the text, expiring contracts were aggregated up to the farm operation level as needed. Data on bid structure outcomes are drawn from the CRP Offer data.

These results are potentially influenced by a number of factors. First, the low baseline participation rate in the control may indicate that there were errors in the process used to identify program eligibility, or that contact information for these farms, many of who are not as actively engaged with USDA, is more problematic. In fact, approximately 7% of the letters for this group were returned as undeliverable.[7] In addition, the timing of the letters (during the first week of the sign-up period) might not have provided sufficient time for this population to initiate a new offer.[8] Lastly, the coding error described in the appendix may have influenced any effect of treatments 2 and 3 for this population if, upon receiving a letter with a typo in it, these farmers were less responsive to the information. However, treatment 1 for this population did not have such an error, and it has a lower point estimate of an offer rate, which is the opposite of what we might expect if the coding error made the reminder letter less effective. Given our sample sizes, this experiment had sufficient power to detect an approximate doubling of the offer rate, from 0.2% to 0.4%, so the failure to reject the null hypothesis in this context is meaningful from a policy perspective.

Table 4 presents the examination of bidding outcomes for the eligible but unenrolled population. The only statistically significant results are a negative relationship between treatment 2 on bid-down and a negative relationship between treatment 3 and acres offered. The former is possible if the EBI nudge in treatment 2 caused farmers to be less likely to bid-down, although if such substitution were occurring we would expect the practice EBI score to exhibit a positive relationship. The latter effect contrasts with the finding with the other population, perhaps suggesting that the earlier result should be viewed cautiously.

[7] Approximately 4,600 letters were returned as undeliverable, and about 95% of these were from the eligible but unenrolled population. This is not entirely surprising since the USDA tends to have much more up-to-date contact information for farms with currently expiring contracts. We do not attempt to control for returned letters by trimming the populations. It would not be possible to trim the control group (which did not receive letters) and it would limit the external validity of the results since the USDA cannot know in any given outreach effort which letters will be returned.

[8] We are grateful to an anonymous reviewer for highlighting the different impact of timing on the two populations.

Table 4. Bid Structure Outcomes for Eligible but Unenrolled Population

Treatment	Total EBI score	Practice EBI score	Bid-down	Rent	Acres offered
1	4.915	2.335	0.566	−3.109	0.001
Basic	(7.260)	(8.120)	(0.821)	(1.845)	(0.047)
reminder	*0.498*	*0.774*	*0.491*	*0.101*	*0.982*
2	2.798	6.972	−1.567	2.906	−0.028
Social	(5.959)	(5.517)	(0.515)	(2.720)	(0.042)
norm	*0.639*	*0.206*	*0.002*	*0.292*	*0.519*
3	7.106	4.545	0.463	−2.773	**−0.076**
Peer	(6.877)	(6.561)	(0.872)	(5.978)	(0.040)
comparison	*0.302*	*0.489*	*0.596*	*0.646*	*0.061*
constant	**265.319**	**173.700**	**4.463**	**93.854**	**0.177**
	(4.670)	(5.462)	(0.486)	(0.120)	(0.002)
	0.000	*0.000*	*0.000*	*0.000*	*0.000*

Note: Each column gives the coefficients from a regression of the outcome variable on the treatment dummy variables with state fixed effects. Clustered standard errors are given in parentheses. P-values are given in italics. Bold values are statistically significant at $p = 0.1$.

Implications for Program Outcomes

The results of this field experiment demonstrate that reminder letters sent during the General Sign-up auction are effective at encouraging those farms with expiring contracts to re-offer land to CRP. Given the costs of conducting the additional outreach, the question for program managers is whether the benefit of additional offers is sufficient to warrant the expense of the letters. Since the General Sign-up is a multi-unit reverse auction, additional offers would give the program managers a greater pool of offers from which to choose contracts. However, since we do not find a significant impact of letters on the structure of offers, and since program enrollment is constrained, the additional offers may only change the distribution of the final contracts by inducing a minor increase in the EBI (due to a larger pool of offers) and substituting some higher EBI offers for the marginal EBI offers. To evaluate the impact on program outcomes, we conduct a simulation using offer data from three other general sign-ups (sign-ups 39, 41, and 45, which were held in 2010, 2011, and 2013, respectively).[9]

Table 5. Differences Between Re-offers and New Land Offers by Sign-up

	SU 39	SU 41	SU 45
	Summary statistics		
N	50,094	38,715	27,821
Share of re-offers	0.53	0.75	0.83
	EBI		
Constant	274.41***	264.90***	257.25***
	(0.31)	(0.53)	(0.65)
Re-offer (1/0)	−1.66***	−0.77	−0.56
	(0.43)	(0.61)	(0.72)
	Rent		
Constant	81.22***	86.49***	91.52***
	(0.30)	(0.43)	(0.89)
Re-offer (1/0)	−24.89***	−25.26***	11.14***
	(0.41)	(0.50)	(0.98)
	Acres		
Constant	85.84***	83.26***	83.85***
	(0.96)	(1.49)	(1.73)
Re-offer (1/0)	20.27***	18.23***	−18.48***
	(1.31)	(1.72)	(1.90)

Note: Standard errors appear in parentheses. Significance levels: * = 0.1, ** = 0.05, and *** = 0.01.

Given the lack of statistical significance of any treatment effect for the farms with eligible but unenrolled population, we simulate the effect of sending reminder letters to only those farms with expiring contracts in each sign-up. At the very least, this increases the total pool of offers. However, it also changes the distribution of offers to the extent that

[9] The reason that we do not attempt to do a simulation using the data from the 2012 sign-up—comparing the treatment and control groups in our field experiment—is because the selection criteria could not be applied independently to the treatment and control groups in Sign-up 43, and the groups are necessarily pooled for the observed and simulated decision rules. For simulation purposes, the observed offers in the other three sign-ups can function as a pure control group, and we can simulate the effect of a treatment for the entire population in each of those auctions.

Table 6. Simulation Results Using Sign-up 39 Data

Sign Up 39 Absolute Change by Outcome Measure Total cost	Total practice EBI points	Mean practice EBI score	Total accepted acreage
Binding acreage constraint scenario			
−264,446	4,394,399	1	−20
(93,928)	(333,814)	(0)	(313)
Binding budget scenario			
603	4,937,319	1	3,017
(8,820)	(442,917)	(0)	(1,993)
Binding quality (fixed cutoff EBI) scenario			
3,192,841	12,360,559	0	74,436
(168,730)	(746,007)	(0)	(4,281)

Sign Up 39 Percentage Change by Outcome Measure Total cost	Total practice EBI points	Mean practice EBI score	Total accepted acreage
Binding acreage constraint scenario			
−0.1331	0.587	1.0125	−0.0005
(0.0473)	(0.0446)	(0.0777)	(0.0072)
Binding budget scenario			
0.0003	0.6566	1.009	0.0689
(0.0044)	(0.0589)	(0.0764)	(0.0455)
Binding quality (fixed cutoff EBI) scenario			
1.5294	1.6032	−0.0437	1.6295
(0.0808)	(0.0968)	(0.0506)	(0.0937)

Note: Standard deviation appears in parentheses. Grey boxes are outcomes that is subject to the decision rule constraint.

re-offers are systematically different from new-land offers. Table 5 shows the difference in average outcomes between re-offers and new land offers for each of the three sign-ups. While there are some statistically significant differences in EBI, rental payments, and acres per offer between the two groups, these differences are not consistent across the sign-ups.[10] This suggests that increasing the proportion of offers from expiring contracts through use of reminder letters will not have a consistent impact on the distribution of final offers pooled from the two groups.

In the simulation, we assume that new offers—those offers induced by a letter—would be from the same distribution as existing offers. Practically, this means that we are assuming that the letters have the effect of inducing random individuals to participate in the CRP auction. Since our experiment is not designed to allow subgroup analysis, this conservative assumption is warranted; if we had information suggesting that, for example, individuals with lower EBI scores were more likely to be induced to bid, we could incorporate that information into the simulation.

To preserve the joint distribution of the EBI, the rental rates, and the acres per offer, the simulation works by randomly selecting a number of re-offers and replicating them to produce additional re-offers. The simulated auction is then executed exactly as the original auction was, but with more offers (additional details on the role of these variables are provided in the appendix). For each sign-up, the simulation iterates 1,000 times, calculates outcomes at each iteration, and then averages the outcome statistics.

We calculate the outcome statistics under three different acceptance rule scenarios, which reflect the main factors that are likely to

[10] For average total EBI score, re-offers have statistically lower EBI's (by 1.7 points, or about 0.6%) in Sign-up 39 (SU39), but not in the other two sign-ups. Therefore in SU39, re-offers are slightly less likely to be accepted, on average, than new land offers. For average rental payment per acre, re-offers are significantly less expensive, on average, in SU39 and Sign-up 41 (SU41), but significantly more expensive in Sign-up 45 (SU 45). For average total acres per offer, re-offers are significantly larger in SU39 and SU41 but significantly smaller in SU45.

Table 7. Benefit-Cost Ratio Under Binding Acreage Constraint Scenario

	SU 39	SU 41	SU 45
		Summary statistics	
Re-offers	26,550	29,036	23,091
Estimated re-offer rate	0.598	0.704	0.456
Estimated population	44,398	41,245	50,639
Estimated new re-offers	745	692	850
Mailing cost	$25,973	$24,128	$29,624
		Mean estimate	
Change in annual cost	−$264,416	−$55,584	−$116,328
NPV of 10-year change	−$2,342,461	−$492,419	−$1,030,550
Benefit-cost ratio	90.2	20.4	34.8
		5th percentile	
Change in annual cost	−$418,296	−$178,284	−$351,936
NPV of 10-year change	−$3,705,684	−$1,579,418	−$3,117,801
Benefit-cost ratio	142.7	65.5	105.2
		95th percentile	
Change in annual cost	−$119,800	$63,272	$86,052
NPV of 10-year change	−$1,061,308	$560,527	$762,335
Benefit-cost ratio	40.9	−23.2	−25.7

influence FSA's internal deliberations on where to set the EBI cutoff for a given auction.[11] The "Binding Acreage Cap" scenario assumes that FSA could not have accepted any more acres in each sign-up than it actually did. The "Binding Budget" scenario functions in a similar fashion, but assumes that FSA could not have accepted a pool of contracts that would have had a higher total annual rental payment. Finally, the "Binding Quality" scenario assumes that FSA is primarily focused on maintaining a minimum level of environmental benefits on each contract and therefore operates by trying to keep the cut-off EBI the same, regardless of how many offers are accepted. While perhaps the least realistic scenario in terms of how the program is run, the "Binding Quality" scenario gives a sense of how additional offers would impact the program if they are not displacing other offers from acceptance.

Table 6 shows the results of the simulation for signup 39 (SU39). Under the binding acreage constraint scenario, additional offers lead to a 0.13% decrease in costs, a 0.59% increase in total environmental EBI (Env-EBI) points, and a 1.01% increase in acreage Env-EBI points.[12] Under the binding budget scenario, additional offers lead to a 0.66% increase in total Env-EBI points, another 1.01% increase in average Env-EBI, and 0.07% increase in total acreage. Under the binding quality scenario, total annual rental payments increase by 1.53%, total acres increase by 1.63%, and total Env-EBI points increase by 1.60%.

Qualitatively, the effects on the SU41 and SU45 populations are similar. The results for SU41 and SU 45 are provided in the appendix tables A.2 and A.3. Additional discussion of those results is found in the appendix.

We use the changes in total program costs under the binding acreage constraint scenario to calculate a benefit-cost ratio for the simulated reminder letter efforts. Table 7 shows the expected benefit–cost ratio for each sign-up. Benefits reflect only changes in total rental payments and do not reflect any attempt to value improvement in Env-EBI. Since these are (predominately) ten-year contracts, the net present value for the annual savings is therefore 8.86 times the annual

[11] Unlike some reverse auctions, the FSA's acceptance criteria is not made public. Rather than pre-announcing a certain acceptance rate or acreage goal, which would make all of the acceptance uncertainty for an individual farmer come from other farmers' decisions, the FSA tries to balance competing constraints on the program after the signup period has closed and the full pool of offers is settled.

[12] The FSA does not endorse the use of the EBI score as a cardinal measure of benefits. However, since the EBI score functions as an ordinal measure of benefits for comparing one contract against another, we view this approach as a valid way of comparing one pool of offers against another (personal communication, A. Barbarika, FSA).

payments, which is derived using a 2.8% discount rate.[13] The costs reflect the mailing costs for the population in each sign-up and do not reflect the transaction costs of processing new offers, the cost-share payments on accepted offers, or the dynamic effect that increased participation would have over time in making the auction more competitive.

For SU39, the expected reduction in annual rental cost is more than ten times the cost of the letters. For SU45, the reduction is more than twice the cost of the letters. For SU45, the expected reduction is almost four times the cost of the letters. Since these annual savings are captured over all ten years of the contracts, the expected benefit cost ratios are much larger, ranging from 20.4 for SU41, to a remarkable 90.2 for SU39. This result captures both the low costs of sending letters and the importance of receiving additional offers in a multiunit, reverse auction. The context of the auction clearly matters as well. The range of the benefit-cost ratio, over the 1,000 iterations for each auction, is always positive for SU39, but spans negative ratios (increases in annual rental costs) under the SU41 and SU45 auctions.[14]

Heterogeneous Treatment Effects

It could be of interest to the USDA to know if outreach efforts are more effective among some groups than others. In a field experiment setting, identifying heterogeneous treatment effects with respect to variables that were not balanced *ex ante* when drawing the sample requires additional assumptions for econometric identification (Gerber and Green 2012). Due to those restrictions, we do not consider these results a primary finding of this study. Instead, these results are useful for generating hypotheses that could be tested in future experiments.

We focus on three potential dimensions along which treatment effects could be heterogenous: binding county enrollment caps, the rank of farms' exogenous EBI from their prior sign-up, and the farmers' bid-down on their prior contract.

The treatment effect of reminder letters could vary under a binding county enrollment cap if the cap moderates the treatment effect by reducing farmers' likelihood of being accepted and therefore their responsiveness to the reminder letter. At the most extreme, in counties that cannot accept any new offers, we would expect both the control and treatment offer rates to be zero, thus leading to the absence of a treatment effect.

The treatment effect could also vary with other factors that create variation in the probability that a given offer is likely to be accepted. As noted earlier, there are portions of the EBI that a farmer cannot influence. We refer to these as the "exogenous" EBI. For farmers with expiring contracts, the exogenous EBI from their prior sign-up captures the environmental quality of their land that cannot be affected through their bid; we expect that lands with higher EBIs are more competitive and thus would be more positively affected by the treatments. Similar to county caps, it is unlikely that farmers with extremely low exogenous EBI would be accepted to the program and so would not be likely to offer in either the control or the treatment cases.

Expanding a bit beyond the probability of acceptance, the treatment effect is also likely to vary with difference in farmers' underlying (latent) willingness to participate in the program. The variable bid-down reveals both a farmer's interest in participating in the program and the extent to which a farmer feels competitive pressure from the auction, and thus we expect that higher bid-down values will be associated with larger treatment effects.

To detect the presence of heterogeneity, we estimate a probit model of the offer rate (for the expiring contract population) regressed on the pooled treatment dummy variable, the three moderators, and the interactions of the treatment variable and the moderators. We perform an F-test of the joint null hypothesis that the interaction terms are different from zero.

Although we find evidence that the three potential moderators are positively correlated with offer rate, we cannot reject the null hypothesis that the interaction terms are jointly equal to zero (Chi-squared ($df = 9$) = 10.51; $p > 0.311$). Thus, we find no evidence of heterogeneous treatment effects conditional on binding bid caps, exogenous EBI scores or bid-down values.

Failing to reject a null hypothesis of homogenous treatment effects along the dimensions we examine has two implications for

[13] See OMB Circular A-94 (December 2014) for guidance on discounting offered to federal agencies.

[14] We also do not account for county caps in the selection criteria.

outreach efforts that rely on reminder letters. First, this finding suggests that there are not likely to be large returns to trying to target or limit the mailings to a subset of expiring farms. There is not a clear type of farmer who is clearly insensitive, on average, to the reminder letter. Second, for designing future research in this area, these results suggest that interventions that seek to exploit heterogeneous responses to informational nudges in the context of CRP re-enrollment would likely need to devise some method to increase statistical power.

Conclusion

We find that informational letters have an impact on the decision to re-offer land into the CRP. Each additional re-offer induced by the information campaign costs between $24 and $96 dollars (based on a 95% confidence interval of a 0.69 to 2.7 percentage point treatment effect). We detect no effect of these same informational letters on individuals with eligible but unenrolled land. The lack of a precisely estimated treatment effect for the low-information population is potentially related to a number of factors, including the difficulty of identifying eligible but unenrolled fields and the quality of contact information for farms that are not actively engaged with FSA.

Our results suggest that additional informational outreach can have an impact even among a population that is well-informed about the CRP program. The most cost-effective intensity of outreach—the mailing of additional letters, or the pairing of letters and other targeted outreach—is a good subject for continued research.

Perhaps the most surprising thing to consider about the results of this experiment is the magnitude of the decision being influenced. Individuals choosing to re-offer their land to CRP are making (at least) a ten-year commitment with significant financial implications. Other findings in the economics literature also find significant impacts of information provision on extensive-margin decisions (essentially, participation decisions) such as school choice and the choice to claim an EITC, suggesting that inattention can play a significant first-order role in determining outcomes. An interesting future research project might focus on the intensive margin.

We do not attempt to ascribe the effects we observe to a particular behavioral phenomenon, or to a particular behavioral bias. Our results are consistent with a model of inattention, broadly construed. Individuals who do not receive the letter may be responding optimally to cognitive constraints by being attentive to information that they deem most relevant to their enrollment decision (e.g., current corn prices). By drawing attention to features of the re-enrollment decision that land owners were not already focused on, such as the income-smoothing benefits of CRP, we might be relieving attention constraints.

The work here represents a pragmatic approach to using behavioral and experimental economics for policy design. We utilize well-known behavioral interventions that have had success in other domains and test their effectiveness in a unique and important context. From a policy perspective, the treatment effect we estimate for the expiring contract population represents a new baseline to which FSA can compare other outreach activities. The cost-effectiveness of other outreach activities (e.g., workshops, broad advertisements, targeted phone calls or farm visits) can be compared to the cost of obtaining an extra offer through mail-based outreach.

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