



Lottery Incentives and Resource Management: Evidence from the Agricultural Data Reporting Incentive Program (AgDRIP)

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Abstract

To manage resources effectively in an agri-environmental context, policymakers need information about on-farm management practices and ecological conditions. This information is often accessible to agricultural producers but not to policymakers. However, little is known about how best to structure incentives for voluntary reporting. In other contexts, lotteries are often used to provide an incentive for voluntary data reporting. This article provides evidence about the efficacy of lottery (stochastic) incentives relative to fixed (deterministic) incentives. Based on two field experiments embedded in a data reporting program for agricultural producers, we estimate that lottery incentives reduced program enrollment between 28% and 62% relative to fixed incentives. A novel feature of our study is a comparison between fixed incentives and actuarially equivalent lotteries with explicitly communicated probabilities, which allows us to rule out an effect size of actuarially equivalent lotteries larger than +15% relative to fixed incentives.

Keywords Citizen science · Field experiment · Lottery incentives · Randomized controlled trial · Resource management

1 Introduction

To achieve public environmental goals on private agricultural lands, policymakers need data related to on-farm ecological conditions and economic decisions. For example, data on the presence or absence of threatened species can guide the design of conservation incentives (Reeling et al. 2019), data on variation in fertilizer applications can guide water pollution enforcement (Lungarska and Jayet 2018), and data on irrigation use can guide the design of pumping fees for reducing stress on aquifers (Krishnamurthy 2017).

The views and analysis expressed here are those of the authors and do not necessarily represent the views or policies of the U.S. Department of the Treasury.

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One option for collecting ecological data is through remote sensing. Technological advances like satellite imagery hold the promise of collecting large-scale data with low marginal cost (Gorelick et al. 2017). However, data gathered remotely is often measured with error and requires careful validation when used for causal inference (Jain 2020). Furthermore, there are ethical and political issues associated with collecting data on private lands without the landowners' consent. And for some types of data—the presence or absence of species, fertilizer applications, and groundwater pumping, for example—remote sensing is unlikely to yield accurate estimates.

An alternative or complement to remote sensing is to include agricultural producers directly in data collection. Citizen science—large-scale data collection by volunteers—has been especially successful in the field of biodiversity conservation research (Dickinson et al. 2010). Nurturing intrinsic motivation has been a particularly effective recruitment strategy when data collection coincides with pre-existing hobbies like bird-watching or when citizens experience data collection as a game, with a friendly user interface and game-like incentive structures (Xue et al. 2016). However, agricultural producers are a particularly challenging group to motivate (Kuhfuss et al. 2016; Weigel et al. 2020), and the challenge has been intensifying as measured by declining response rates to surveys conducted by the National Agricultural Statistics Service (Johansson et al. 2017). Although many surveys of agricultural producers have historically been uncompensated, researchers and policymakers are increasingly open to using financial incentives to motivate volunteers.

We assess the effectiveness of lottery incentives for obtaining voluntary data reports from agricultural producers. Lottery incentives are ubiquitous; for example, in retirement savings (Gertler et al. 2018; Kearney et al. 2011), academic achievement (Levitt et al. 2016; Luiselli et al. 2002), worker attendance (Pedalino and Gamboa 1974; Hassink and Koning 2009), and medication adherence (Kimmel et al. 2012; Volpp et al. 2017). Traditional lotteries are undeniably popular (McCaffery 1994; Garrett and Sobel 1999), and program administrators use lottery incentives hoping to harness that appeal to reduce program cost. However, why and when lottery (stochastic) incentives can be more effective than fixed (deterministic) incentives is not well understood. Competing theoretical models predict the superiority of either lottery incentives or fixed incentives. Empirical assessments of lottery incentives have produced mixed results depending on the context (Filiz-Ozbay et al. 2015; Halpern et al. 2011).

We report results from two field experiments that recruited agricultural producers to a voluntary data reporting program. In 2018 and 2019, agricultural producers with water withdrawals in Georgia and Colorado were sent mailings inviting them to participate in the Agricultural Data Reporting Incentive Program (AgDRIP). The producers were asked to enter their water meter readings once per month in a smartphone or browser-based app that was developed specifically for this program and enabled producers to track their own water use over time. Financial incentives were offered to all producers in both experiments.

The structure of the financial incentives was randomly assigned in order to assess the effectiveness of lottery incentives relative to fixed incentives. The experimental treatments are summarized in Fig. 1. The lottery incentive was a chance each month to win a large cash prize—\$2000 in the 2018 experiment and \$1000 in the 2019 experiment. Producers who were offered the lottery incentive also received a small fixed payment each month (\$10 in 2018 and \$5 in 2019). Producers who were not offered the lottery incentive received a larger fixed payment each month (\$30 in 2018 and \$15 in 2019). Most producers who were offered the lottery incentive were not explicitly informed about the probability of winning, but some producers

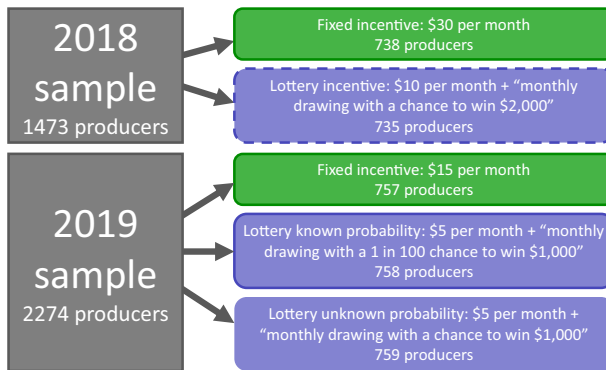


Fig. 1 Visual summary of experimental design

who were offered the lottery incentive in 2019 were promised a “1 in 100” chance of winning the prize each month, which made it actuarially equivalent to the fixed incentive.

In both experiments, lottery incentives were less effective than fixed incentives for recruiting agricultural producers to the data reporting program. Pooling the results from both experiments, we estimate that lottery incentives reduced registration by 45%, with a 95% confidence interval between -62 and -28% . The point estimate of the treatment effect for lottery incentives was negative in all lottery treatment groups—i.e. whether or not producers knew the exact probability of winning. In the actuarially equivalent lottery with explicitly communicated probability of winning, we rule out an effect size larger than $+15\%$ relative to fixed incentives.

To our knowledge, our study is the first to compare lottery incentives to fixed incentives in the context of agricultural producers. We contribute to the large literature on lottery incentives for survey completion, which generally argues that lottery incentives are inferior to prepaid fixed incentives (Singer and Kulka 2002; Singer and Ye 2013), but rarely includes a comparison of actuarially equivalent fixed incentives (Halpern et al. 2011; Levitt et al. 2016) when making that assertion. We also contribute to the smaller literature on lottery incentives for contributions to public goods, which typically finds that lottery incentives are effective (Fabbri et al. 2019; Naritomi 2019; Goette and Stutzer 2019). One reason our study makes a contribution to these literatures is that the incentives in our study are large relative to most lottery incentives for survey participation and contributions to public goods.

In our judgment, fixed incentives are more promising than lottery incentives for motivating agricultural producers to participate in large-scale data collection. Program administrators are always looking for ways to save costs, and harnessing the popularity of lotteries using lottery incentives sounds like a plausible strategy. However, based on the results of these experiments and on our assessment of prior literature, we believe lottery incentives are unlikely to be better than fixed incentives for incentivizing data collection about on-farm ecological conditions and economic decisions.

2 Background

Justification can be found in the economic theory literature to expect that either incentive structure—lottery incentives or fixed incentives—might induce higher participation. Fixed incentives are predicted to be more effective in expected utility models in which agents are risk averse, and lottery incentives are predicted to be more effective in many non-expected utility models in which agents distort small probabilities upwards (Starmer 2000; Prelec 1998; Tversky and Kahneman 1992).

To build intuition for the payment mechanisms in the experiment, we describe a simple model of an agricultural producer choosing whether to participate in a voluntary data reporting program. The payoff to the producer for participating is expressed by a simple lottery, obtaining a positive payoff x with probability p and obtaining 0 with probability $1-p$. We focus on a comparison between two lotteries: P1 is the degenerate lottery of a certain \$10 payoff $(p_1, x_1) = (1, 10)$, and P2 is a lottery that obtains a \$1000 payoff with one percent probability, $(p_2, x_2) = (0.01, 1000)$.

The producer is assumed to make choices to maximize weighted utility. The decision weight corresponding to a probability is expressed by the function $\pi(p)$, and the subjective value (utility) of a payoff is expressed by the function $u(x)$. We assume that $\pi(\cdot)$ and $u(\cdot)$ are strictly increasing, $\pi(0) = 0$, and $\pi(1) = 1$. Weighted utility W —the value placed by the producer on an incentive package—is the product of the subjective value of the potential payoff and the decision weight placed on the probability: $W(p, x) = \pi(p)u(x)$. Decision weights like this are featured in many non-expected utility models (Starmer 2000). More complex models that accommodate multiple nonzero payoffs and maintain first order stochastic dominance, e.g. rank-dependent prospect theory (Tversky and Kahneman 1992), yield the same intuition in the context of our choice problem, so we use this simpler model to convey the intuition. (We focus on the simpler model, but the intuition that comes out of their more complex model has the same implications for evaluating the fixed incentives relative to the lottery incentives in our experiments.)

Whether lottery incentives or fixed incentives will induce higher participation depends on the relative shapes of $u(\cdot)$ and $\pi(\cdot)$. Higher curvature of $u(\cdot)$ leads to higher risk aversion and a stronger preference for fixed incentives. Higher curvature of $\pi(\cdot)$ leads to higher distortions of small probabilities and a stronger preference for lottery incentives.¹

Empirical evidence about the effectiveness of lottery incentives is mixed. Many studies find that lottery incentives are effective in various contexts (e.g. Porter and Whitcomb 2003; Björkman Nyqvist et al. 2016; Naritomi 2019), and many others find the opposite (e.g. Leung et al. 2002; Levitt et al. 2016; Volpp et al. 2017; DellaVigna and Pope 2017). Two contexts that are most relevant to our setting are lottery incentives for survey completion and lottery incentives for contributions to public goods.

The voluntary data reporting program in our experiments can be interpreted as a repeated monthly survey requiring the effort of physically going to a water meter in order to obtain the answer to a single question: What is your water meter reading? Survey research generally finds that postpaid incentives, including lottery incentives, are less conducive to eliciting responses than fixed prepaid incentives or no financial incentives (Singer and Kulka 2002; Singer and Ye 2013). However, few of the studies of lottery incentives give any indication to respondents of the probability of winning the lottery (Halpern et al.

¹ Appendix A expands on the theoretical motivations introduced here.

2011). In the absence of any information about what the respondents believed about the probability of winning, the results are difficult to interpret.

The data reporting program can also be interpreted as a contribution to a public good, where the public good is water resource management. There is evidence that financial incentives can displace intrinsic motivation to provide public goods (Gneezy et al. 2011), but sufficiently strong lottery incentives could overcome whatever loss there is in intrinsic motivation. For example, bundling lottery incentives into purchasing tickets for public transit was effective for improving compliance with transit fees (Fabbri et al. 2019), lottery incentives were effective for increasing blood donations (Goette and Stutzer 2019), and lottery incentives were effective for motivating third party tax reporting (Naritomi 2019).

Public and private data collection programs are increasingly open to offering financial incentives to agricultural producers. Response rates to the Agricultural Resource Management Survey (ARMS), which is conducted by the U.S. Department of Agriculture, have been low and declining in recent decades (Johansson et al. 2017). To mitigate the potential for bias from low response rates, researchers have experimentally varied financial incentives to learn what is effective for inducing higher participation on the ARMS and other surveys of U.S. farmers (Beckler and Ott 2007; Glas et al. 2019). Private data collection is also being incentivized, including invitations to farmers from corporations that frame sharing data as a revenue stream for the farmer (Powell 2017).

Agricultural producers tend to be more risk averse than average in laboratory settings, but laboratory measurements do not seem to be reliable for predicting behavior outside of the laboratory, where producers seem to be more risk tolerant than average. For example, Hellerstein et al. (2013) elicit risk preferences using standard laboratory techniques then compare predicted on-farm behaviors (based on the elicited risk preferences) to actual on-farm behaviors and find substantial disparities.

3 Experimental Design

To shed light on the appeal of lottery incentives relative to fixed incentives, we conducted two field experiments with agricultural producers in two consecutive years. Producers were sent mailings in 2018 and 2019 that described financial incentives for participating in a voluntary data reporting program for one year (10 months of reporting), and the structure of the financial incentives was randomly assigned.

The experiments were pre-registered through the Open Science Framework. The experimental design, analysis plan, power analysis, and code used for random assignment of the incentives is available at <https://osf.io/x6m7b> for the 2018 experiment and at <https://osf.io/9ejg2> for the 2019 experiment.

3.1 Recruitment

The 2018 and 2019 experimental samples comprise agricultural producers who owned irrigated land in Georgia or Colorado and had one or more water meters. Georgia producers were identified using a database maintained by the State of Georgia and available to researchers at Albany State University. Colorado producers were identified using databases maintained by the Plains Groundwater Management District and the W-Y (Washington and Yuma) Groundwater Management District in Colorado and made available to researchers at Colorado State University. Using these sources, 1473 producers were sent mailings in 2018

February 2, 2018

Dear Firstname Lastname,

We want to improve water management.

Data on water use and agricultural production informs efficient farming practices and helps manage our shared water resources. Researchers at Albany State University, supported by the U.S. Department of Agriculture, are launching a new program that offers financial incentives for the voluntary provision of information on agricultural water use. The program seeks to assess how specific incentive approaches influence reporting behavior.

You will be paid for your participation.

Participation in AgDRIP, the Agricultural Data Reporting Incentives Program, is strictly voluntary. If you participate, you will be asked to submit a water meter reading each month, March–December 2018. Monthly text message reminders will inform you when to submit your meter reading using the user-friendly AgDRIP website or smartphone app. Once during the program, you will also be asked to submit basic information on your crop type, acreage, and yield. If you own irrigated land in the Flint River Basin but do not currently farm, please forward this invitation to your tenants.

For reporting the reading on your water meter, you will be paid \$10 once per month on the enclosed prepaid Visa card and entered into a monthly drawing with a chance to win \$2,000.

You will be paid \$30 just for completing the registration process, plus \$10 per month during the reporting period, March–December. In addition, a bonus of \$2,000 will be paid each month to one farmer that is randomly selected from the group of farmers who submit reports in that month. We have enclosed with this letter a prepaid Visa card that can be used at ATMs and anywhere Visa cards are accepted. We will reload the card throughout the program in order to deliver money quickly and easily to you.

Register now to receive \$30 on your prepaid Visa card.

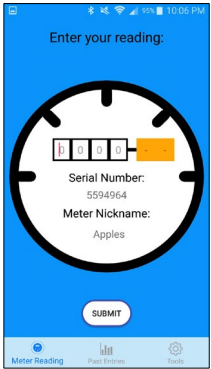
Visit our website www.agdrip.org and enter your personal code: **word123**. Enrollment in AgDRIP is limited to 200 participants and priority will be given to farmers who complete the registration process early. For more information, see the AgDRIP website (www.agdrip.org) or contact us by email (info@agdrip.org) or call us at (229) 430-2900.

Sincerely,

Mark Masters
Director, Georgia Water Planning & Policy Center
Albany State University

Reporting your water meter reading with a smartphone app is easy!

1. Register at www.agdrip.org using the code **word123**. You will be given a username and password for the app.
2. Search for “AgDRIP” in the App Store (if you have an iPhone) or Google Play (if you have an Android).
3. Download and install the AgDRIP app.
4. Log in using the username and password from www.agdrip.org.
5. Enter your meter reading.



For reporting the reading on your water meter, you will be paid \$10 once per month on the enclosed prepaid Visa card and entered into a monthly drawing with a chance to win \$2,000.

No smartphone? No problem! You can use the username and password to report a meter reading at www.agdrip.org.

Fig. 2 Example letter, 2018. Two-page letters were sent in February of 2018 and 2019 with the two pages on the front and back of a single sheet of paper. The main difference among treatment groups was a “treatment message” that appeared twice in the letter, once in a prominent box on the front and once at the bottom of the second page. An enclosed prepaid Visa card was attached to a second sheet of paper that provided instructions for activating the card. The exact wording of all treatment messages is in Appendix Table B.1, and the 2019 letter is in Appendix Figure C.1

and 2274 producers were sent mailings in 2019. The 2018 sample was smaller because it was limited to producers in the Flint River Basin in Georgia and the Plains Groundwater Management District in Colorado. The 2019 sample includes producers from the 2018 sample as well as producers from elsewhere in Georgia and Colorado.²

Water is more abundant in the regions of Georgia included in the sample than in the regions of Colorado included in the sample. For example, Dougherty County in Georgia gets around 45 inches of rainfall per year, while Colorado’s Kit Carson County gets around 15 inches of rainfall per year.³ Groundwater is likewise generally more abundant and accessible in the karst topography of the Georgia sample area as compared to Colorado. Corn is a major crop in both regions. Other common crops in Georgia are cotton and peanuts, and other common crops in Colorado are alfalfa and winter wheat. Appendix Tables B.12 and B.13 report that on average Colorado producers in the sample had more withdrawals and used more groundwater than Georgia producers. Around three quarters of the withdrawals in the sample in Georgia were groundwater withdrawals, and the rest were surface withdrawals or well-to-pond combination withdrawals. All withdrawals in the sample in Colorado were groundwater withdrawals.

² Within Georgia and Colorado, the regions added in 2019 were very similar agriculturally to the regions present in both 2018 and 2019, with slightly more reliance on withdrawals for irrigation in the added regions in both states. See Appendix Tables B.12 and B.13.

³ Average annual precipitation from 2000 through 2020 at Burlington Carson Airport and Southwest Georgia Regional Airport, calculated from data obtained from noaa.gov.

A recruitment letter inviting participation in the water-reporting program was sent to each producer in February of the sample year. Figure 2 shows an example letter. The letter explained (1) why gathering information about water use is useful, (2) who sponsored the program, (3) what the recipient was being asked to do, and (4) how the recipient would be compensated. The program was called the Agricultural Data Reporting Incentives Program, or AgDRIP for short. This branding and acronym were used in recruitment materials, an informational website, and an app.

Producers were asked to use the smartphone or web-based app to report their water meter reading once per month for March through December, which covers the agricultural watering season. The recruitment materials included a screenshot of the app, which was developed specifically for use as a part of this reporting program.⁴

The letters asked producers to indicate their willingness to participate in the program by registering online using a personalized access code. Producers were offered a cash bonus simply for completing registration. The 2018 letter advertised the bonus as \$30, and a subsequent postcard raised the 2018 bonus to \$100 for completing registration by March 28.⁵ The 2019 letter advertised the bonus as \$30 for completing registration by February 20 or \$15 for completing registration by March 1.

Compensation for registering and for reporting meter readings was provided through a prepaid Visa card enclosed with the invitation letter.⁶ The research team loaded the cards remotely to deliver payments promptly, typically within three days of registration or reporting.

Producers were sent reminder postcards two to three weeks after the initial letter. Appendix Figure C.2 shows an example postcard. The reminders reiterated the compensation structure and the enrollment instructions. In 2018 a reminder phone call was made to producers in Georgia approximately four weeks after the letters were sent, and another reminder postcard, shown in Appendix Figure C.3, was sent approximately six weeks after the letters were sent to producers in both Georgia and Colorado. In 2019 no phone calls were made, and a second reminder postcard was sent approximately nine weeks after the letters were sent. The decision not to make phone calls in 2019 was based on the perception by the research team that the registration yield from phone calls in 2018 was not worth the cost of labor resources.

3.2 Experimental Treatments

Before the invitations were mailed, each producer was randomly assigned to a financial incentive treatment. A treatment message summarizing the financial incentive was

⁴ There was particular interest in Georgia about the potential for more widespread data collection using the mobile app. The first thing users saw after logging into the app was a data entry screen that resembled a water meter but with blank spaces where the numbers would be. Users had access to two other “screens”: (1) a tools screen where they could customize nicknames for their water meters and the appearance of the meter on the data entry screen, and (2) a history screen where they could view the meter readings they had submitted in prior months.

⁵ We chose to increase the size of the bonus because registration was lower than we anticipated. All 2018 registrants were given \$100 for registration even if they registered when the advertised bonus was only \$30.

⁶ The prepaid Visa card was chosen as a payment delivery channel to be as close as administratively feasible to cash. Previous research has found that substitutes for cash elicit lower response rates than cash with the same face value (Teisl et al. 2006). Since the producers who chose to participate in 2018 also received an invitation in 2019, they were asked to keep their prepaid Visa cards from the prior year, and their 2019 invitation letters did not enclose a card.

emphasized in a prominent box on the letter and repeated in the reminder postcards. The financial incentive treatments are summarized in Fig. 1, and the exact wording of the treatment messages is reported in Appendix Table B.1.

In 2018, there were two financial incentive treatments: a fixed incentive and a lottery incentive. The fixed incentive (control) group was offered \$30 per monthly report. The lottery incentive group was offered \$10 per monthly report plus a monthly chance to win \$2000.⁷ At the end of each month, a producer was randomly selected from the set of producers who submitted reports that month to receive the \$2000. To give producers a basis for forming beliefs about the expected value of incentive payments, the letter indicated that enrollment was limited to 200 participants.

Other than the enrollment limit, the probability of winning the monthly drawing was not explicitly communicated to participants in the 2018 lottery incentive group. Because the letter indicated that enrollment was limited to 200 participants, producers could have inferred that the expected value of the lottery incentive conditional on submitting a report was at least \$20 (\$10 fixed payment plus a 1 in 200 chance of winning \$2000).

Noting that participants in AgDRIP 2018 did not know the exact probability of winning the lottery, which may have affected their willingness to participate and submit reports, the AgDRIP 2019 experiment was designed to analyze the effect of an explicit probability of winning the lottery. In particular, AgDRIP 2019 compared a lottery incentive to a fixed incentive with the same expected value. Although there are many prior studies examining the effectiveness of lottery incentives in various contexts, we are aware of only two that compare lottery incentives to fixed incentives with the same expected value. Lottery incentives with an expected value of \$20 were more effective than fixed incentives in promoting savings behavior in a laboratory experiment with college students (Filiz-Ozbay et al. 2015). In promoting survey responses among clinicians, lottery incentives with an expected value of \$5 were less effective than unconditional fixed incentives (i.e. the clinician received the fixed incentive regardless of whether they responded to the survey) and about equally effective to conditional fixed incentives (i.e. the clinician received the fixed incentive only if they responded to the survey) (Halpern et al. 2011). The AgDRIP 2019 experiment adds to this short list.

There were three financial incentive treatments in 2019: a fixed incentive, a lottery incentive with a clearly communicated probability of winning, and a lottery incentive with no indication of the probability of winning. The fixed incentive (control) group was offered \$15 per month for reporting readings from one water meter. Those in the treatment group involving a lottery with known probability were offered \$5 per monthly report and a “1 in 100 chance to win \$1000” for reporting readings from one water meter. Those in the treatment group involving a lottery with unknown probability were offered \$5 per monthly report plus a chance to win \$1000 for reporting readings from one water meter, with no indication of the odds of winning, not even an enrollment limit. An additional fixed incentive of \$5 per monthly report for readings from a second meter was offered to all producers.⁸

⁷ The lottery incentive thus includes a fixed component. We are not aware of previous literature examining mixed incentives with both fixed and lottery components.

⁸ The 2019 experiment thus incentivized reporting on two meters, whereas the 2018 experiment incentivized reporting on only one meter. Simplicity was prioritized in the design of the 2018 experiment, and for the 2019 experiment the research team felt that the potential boost in incentivized data collection was worth the added complexity of the incentive structure.

The \$1,000 prize was awarded to exactly one producer per month. All producers in either lottery treatment group who submitted reports were eligible to win. An algorithm that maintained the promised 1-in-100 chance to win for producers in the lottery-with-probability treatment group selected the winner at random from the set of eligible producers.

Preferences between fixed and lottery incentives may depend on the stakes involved (Fehr-Duda et al. 2010). The expected value of the lottery incentive in the 2018 experiment was at least \$20 (\$10 fixed component plus a 1 in 200 chance to win \$2000), and the expected value of the lottery incentive in the 2019 experiment was \$15 (\$5 fixed component plus a 1 in 100 chance to win \$1000). The stakes in our experiments (\$20 and \$15) were thus comparable to the previous studies with comparisons between fixed incentives and actuarially equivalent lottery incentives (Filiz-Ozbay et al. 2015; Halpern et al. 2011).

The stakes in our experiments were larger than in most recent studies evaluating the efficacy of incentivizing farmers to respond to surveys. Recent examples of smaller incentives include \$0.56 worth of chocolate (Fairweather 2010), \$2 (Glas et al. 2019; Avemegah et al. 2021), and up to \$10 (Arora et al. 2020). We are only aware of one study that has looked at substantially larger amounts, ranging up to \$100, for a short survey (Weigel et al. 2020). The \$20 incentive used by USDA to increase response rates for its core Agricultural Resource Management Survey in 2004 (Beckler and Ott 2007) was a bit larger than the incentives in our experiment after adjusting for inflation.

3.3 Random Assignment

To ensure covariate balance, the treatments were block randomized on (1) state of operation, (2) type of withdrawal (groundwater, surface water, or well-to-pond), (3) number of registered water meters, (4) average annual water use in the five years before treatment, (5) whether a producer's mailing address was outside of the meter's water district, and (6) whether the producer had withdrawn water in the preceding calendar year. In the 2019 experiment, treatment was also block randomized on whether the producer had registered for AgDRIP in 2018. Covariate balance is reported in Appendix Tables B.2 and B.3, which show no significant differences across treatment groups.

3.4 Outcome

The primary outcome is whether producers registered for the program. The primary outcome in the pre-registration plan was whether producers used their access code, and the research team subsequently decided that program registration was a more meaningful outcome. Completing registration required slightly more effort—in addition to using the access code, registration consisted of completing a registration form and logging in to a smartphone or web-based app. The substantive conclusions about the difference between fixed incentives and lottery incentives is the same regardless of which outcome is used, so we focus on the more meaningful outcome and report the pre-registered outcome in Appendix Table B.4. A secondary outcome is the number of monthly reports submitted by producers.

We chose registration rather than the number of monthly reports as the primary outcome because (1) we preferred a primary outcome that could be measured even if enrollment was too low to follow through with the program as planned, (2) we expected registration to be the step on the path to information collection where the most producers are

lost, i.e. non-enrollment is a bigger obstacle than non-compliance,⁹ and (3) registration is a cleaner outcome than the number of monthly reports in the sense that the number of monthly reports depends on both the fraction of producers who register and the compliance behavior of producers who self-select into program participation.

4 Analysis

The AgDRIP 2018 experiment tested the hypothesis that the lottery incentive affected the registration rate relative to the fixed incentive. We test this hypothesis by estimating the treatment effect using an ordinary least squares regression. We regress an indicator (dummy) variable for whether producers registered on an indicator variable for treatment status and the vector of blocking variables used in random assignment: $registered_i = \alpha_0 + \alpha_1 lottery_i + \gamma blockingvariables_i + \epsilon_i$.¹⁰ We test the null hypothesis that the lottery incentive has no effect on registration relative to the fixed incentive, $\alpha_1 = 0$. The power analysis for the 2018 experiment, which is included in the pre-registration materials, estimated that the experiment had 80% power to detect a 5 percentage point treatment effect from a 15% registration rate.

The AgDRIP 2019 experiment tested two hypotheses: (1) whether the lottery with known probability (*LKP*) had an effect on participation relative to a fixed incentive (*F*), and (2) whether the lottery with known probability (*LKP*) had an effect on participation relative to the lottery with unknown probability (*LUP*). Each hypothesis was tested against a two-sided alternative. We estimate treatment effects by regressing an indicator variable for whether producers registered on a treatment indicator and the vector of blocking variables used in random assignment: $registered_i = \beta_0 + \beta_1 LKP_i + \beta_2 LUP_i + \gamma blockingvariables_i + \epsilon_i$. The null hypotheses tested are $\beta_1 = 0$ and $\beta_1 = \beta_2$. We adjusted the significance levels for multiple-hypothesis testing to control for a false discovery rate of 5% using the Benjamini–Hochberg procedure (Benjamini and Hochberg 1995). The power analysis for the 2019 experiment, which is included in the pre-registration materials, estimated that the experiment had 80% power to reject at least one of the null hypotheses.

In addition to the comparisons between lottery and fixed payments, the registered experimental designs in both years also describe a second round of random assignment after producers completed registration. We randomly assigned whether registrants would receive the fixed component of their payment before or after submitting their reports. The outcome variable was the fraction of reports submitted, which we call the compliance rate. The next section reports results from the participation experiments (lottery versus fixed payments).¹¹

⁹ Postal mailings tend to produce low response rates with agricultural producers, on the order of five percent or less (Weigel et al. 2020). On the other hand, we expected compliance to be strong as a result of feelings of reciprocity following the registration payment, similar to the operative mechanism with prepaid survey instruments (Singer and Kulka 2002; Singer and Ye 2013).

¹⁰ We include the blocking variables as covariates to adhere to the registered pre-analysis plan and to follow Bruhn and McKenzie (2009), who recommend that analysis of random experiments controls for the method of randomization.

¹¹ Appendix Figure C.4 reports results from the compliance experiments (prepaid versus postpaid), noting that our power analysis concluded that the compliance experiments were underpowered.

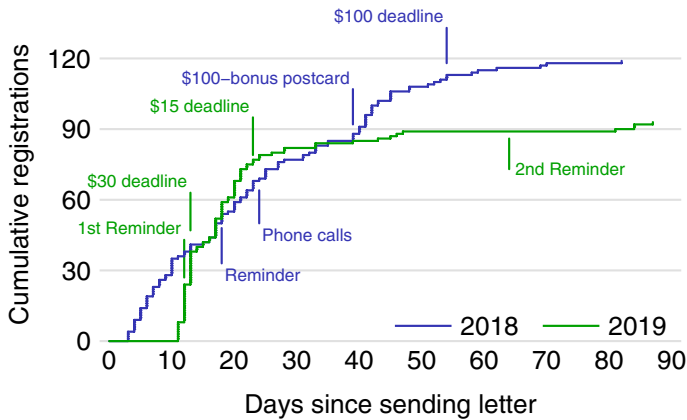


Fig. 3 Cumulative registrations in 2018 and 2019

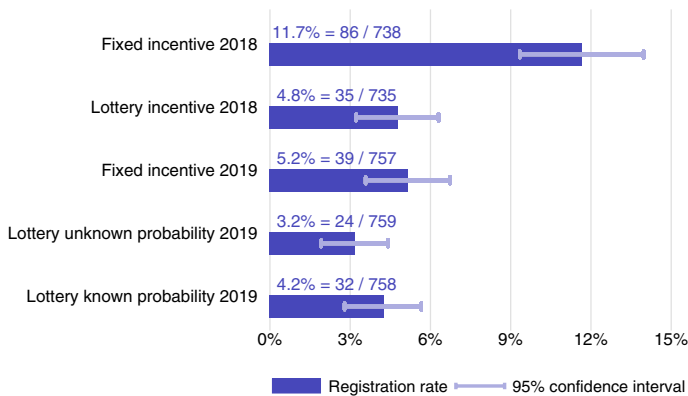


Fig. 4 Registration rate by participation treatment

5 Results

In 2018, 121 of the 1473 producers (8%) completed registration, while in 2019, 95 of the 2274 producers (4%) completed registration. The lower registration rate in 2019 is likely related to the smaller financial incentives offered for both registration (\$30, down from \$100 in 2018) and participation (\$15 per month, down from \$30 in 2018). Figure 3 shows that registrations clustered just before registration payment deadlines, and that there was an acceleration in registrations just after the \$100-bonus postcard was sent in 2018.

In both years, producers in the fixed incentive groups registered at a higher rate than producers in the lottery incentive groups. Figure 4 shows that 11.7% of the fixed incentive group and 4.8% of the lottery incentive group registered for AgDRIP in 2018. Figure 4 also shows that 5.2% of the fixed incentive group, 4.2% of the lottery with known probability group, and 3.2% of the lottery with unknown probability group registered for AgDRIP in 2019.

Table 1 Effect of lottery treatments on registration rate

	(1)	(2)	(3)	(4)
Lottery	– 0.069*** (0.014) [– 0.097, – 0.041]		– 0.015** (0.007) [– 0.029, – 0.001]	– 0.038*** (0.007) [– 0.052, – 0.024]
Lottery unknown probability		– 0.020** (0.008) [– 0.037, – 0.004]		
Lottery known probability		– 0.010 (0.008) [– 0.027, 0.006]		
Sample	2018	2019	2019	Pooled
R– squared	0.02	0.34	0.34	0.17
Observations	1,473	2,274	2,274	3,747
Model	OLS	OLS	OLS	OLS
Controls	X	X	X	X
Mean of dep. var. in F	0.117	0.052	0.052	0.084
p-val on L=F	0.000		0.035	0.000
p-val on LKP=LUP		0.230		
p-val on LKP=F		0.220		
p-val on LUP=F		0.015		

This table estimates the treatment effect of experimental mailings on producer registration rate using ordinary least squares regressions

F ≡ Fixed; L ≡ Any lottery; LKP ≡ Lottery with known probability; LUP ≡ Lottery with unknown probability

Standard errors are in parentheses and 95% confidence intervals are in brackets. The variables used to block treatment assignment are included as covariates: state, type of withdrawal, number of withdrawals, 5-year volume quintile, and recent water use. In the 2019 sample, prior participation was also a covariate. In the pooled sample, an indicator variable is also included for year. Standardized effect size bounds are calculated by dividing estimates or 95% confidence bounds by the mean of the dependent variable for the fixed incentive. Coefficients on control variables reported in Appendix Table B.5

* $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$

5.1 Treatment Effects

Table 1 reports the estimated treatment effects from the regressions described in the Analysis section. Column 1 shows that the lottery incentive offered in AgDRIP 2018 reduced program participation by 6.9 percentage points relative to the fixed-payment incentive, with a 95% confidence interval of a reduction between 4.1 and 9.7 percentage points. We therefore reject the pre-registered null hypothesis in the 2018 experiment of no effect. Relative to the average registration rate of 11.6% in the fixed incentive group, the lottery treatment reduced participation between 35 and 83%.

Columns 2 and 3 report the results from AgDRIP 2019. We estimate in column 2 that the lottery incentive with known probability reduced registration by 1.0 percentage point relative to the fixed incentive (95% CI $[-2.7, +0.6]$). This is a large treatment effect relative to the registration rate of 5.2% in the fixed incentive group, but it is imprecisely estimated (standardized point estimate is a 19% reduction, 95% CI $[-52, +12]$). We cannot reject zero treatment effect, and so we fail to reject the first pre-registered hypothesis in the 2019 experiment.¹²

The second pre-registered hypothesis in the 2019 experiment compares the two lottery incentives. We estimate in column 2 that the lottery with known probability treatment raised registration by 1.0 percentage point relative to the lottery with unknown probability treatment (95% CI $[-2.6, +0.6]$). Again, this is a large treatment effect relative to the registration rates (standardized point estimate is a 24% reduction, 95% CI $[-63, +15]$), but the 95% confidence interval does not exclude zero so we fail to reject the null hypothesis of no difference.¹³ We do not know exactly what producers in the lottery with unknown probability treatment believed about the probability of winning, but based on the lower registration rate it seems reasonable to suppose that they expected the probability of winning was lower than it turned out to be (greater than 1-in-35 for that treatment group). This supposition is consistent with pessimistic expectations in models of decision under ambiguity (Gilboa and Schmeidler 1989).

Although the lottery with probability was not statistically different from the fixed incentive, there is strong evidence that, overall, lottery incentives were less effective in recruiting producers than fixed incentives. First, if we group the two lottery treatments in 2019 together (Table 1 column 3), the estimated treatment effect of either lottery incentive is negative and statistically significant ($p < 0.05$). Second, if we pool the 2018 and 2019 samples and compare all lottery incentive treatments to all fixed incentive treatments (Table 1 column 4), the effect of lottery incentives is statistically significant ($p < 0.01$).¹⁴

If we use the 95% confidence interval on the estimated treatment effect from the pooled sample to create a plausible range for the effect size, the lottery incentive reduced the registration rate between 2.4 and 5.2 percentage points relative to the fixed incentive, which corresponds to a reduction in the fixed-incentive registration rate between 28 and 62%.

¹² The direction of the smaller measured effect in 2019 than in 2018 is consistent with the logic of Fehr-Duda et al. (2010); stakes were larger in 2018, and lottery incentives elicited a lower response rate relative to fixed incentives in 2018.

¹³ Participation in AgDRIP in 2018 may have influenced 2019 participation decisions. We do not attempt to examine the causal impact of previous participation, and the fact that some producers in the 2019 sample participated in 2018 does not interfere with the identification of the treatment effects that we are examining. In all regressions that include the 2019 sample, we include a prior-year participation indicator as a covariate to adjust for differences between producers who previously participated in AgDRIP and producers who did not. The inclusion of the prior-year participation variable results in a notable increase in the model R squared in the models that include 2019 data.

¹⁴ There were differences between the 2018 experiment and the 2019 experiment, including the size of the registration bonus payment and the expected value of the monthly payments. We do not attempt to examine the causal impact of those differences, and those differences do not interfere with the identification of the treatment effects that we are examining. In the pooled regression, we include year as a covariate to adjust for differences between the 2018 experiment and the 2019 experiment.

5.2 Reporting

The goal of the financial incentives was to motivate producers to report their water meter readings. The AgDRIP app allowed producers to submit reports on any number of meters, but incentive payments were not made for every report. Producers received incentives to report on only one meter in 2018 and up to two meters in 2019. For comparability across samples, we examine the number of reports per registered meter, with a maximum of one report per month in 2018 and a maximum of two reports per month in 2019.

Enrolled producers submitted on average 7.7 reports per registered meter in the 2018 sample and 7.2 reports per registered meter in the 2019 sample, both with a maximum of 10 reports per registered meter. We do not see strong evidence that the financial incentive treatment had any impact on the rate at which producers submitted reports. Across both samples, producers in lottery incentive treatments submitted on average 7.4 reports per registered meter and producers in the fixed incentive treatments submitted on average 7.6 reports per registered meter.¹⁵

Most producers who registered for AgDRIP submitted reports regularly. Appendix Tables B.6 and B.7 show that more than 70% of registered producers submitted at least seven reports per registered meter. Fewer than 7% of registrants declined to submit a single report after registering, which suggests that the vast majority of producers who registered either intended to participate in the program by submitting reports or were motivated to do so as a form of reciprocity in exchange for the registration payment. The distributions of reports per registered meter were similar across treatment groups.

5.3 Selection into Water Use Reporting

In many applications, researchers and program administrators want sample data to be representative of a population or to know how to adjust the sample so that it is representative. Therefore, we investigate whether lottery incentives and fixed incentives induced different types of producers to submit water use reports. This portion of the analysis was not pre-registered and should be considered exploratory.

Because lotteries involve risk, one natural hypothesis in this context would be that lottery incentives attracted a relatively more risk tolerant subset of producers compared to fixed incentives. Roe (2015) shows that agricultural producers in general appear to be more risk tolerant than the general population in the United States.

We do not have supplemental measures of risk tolerance for the experimental samples, but we can compare producers who registered in the lottery incentive groups to producers who registered in the fixed incentive groups using (1) administrative data, (2) baseline survey data, (3) water reporting data, and (4) endline survey data. Appendix Table B.8 shows that producers who registered in the lottery incentive group on average had more water withdrawal permits, used more water historically, were more likely to use groundwater, reported more acreage, and were slightly younger. Of these differences, only the difference in reported acreage in the 2018 sample was statistically significant, and that difference is

¹⁵ Appendix Figure C.5 shows the reports per registered meter separately by each of the five treatment arms. None of the differences are statistically significant at the 5% level. Appendix Figure C.6 shows that producers with two registered meters submitted fewer readings per meter than producers with only one registered meter.

Table 2 Cost effectiveness by treatment

	Fixed 2018	Lottery 2018	Fixed 2019	Lottery 2019
Producers	738	735	757	1517
Registrants	86	35	39	56
Reports	653	283	478	621
Mailing cost per producer				
Letter, postcards, postage	\$2.55	\$2.55	\$2.55	\$2.55
Payment card	\$4.00	\$0.00	\$4.00	\$0.00
Total	\$6.55	\$2.55	\$6.55	\$2.55
Registration payment per registrant	\$100.00	\$100.00	\$30.00	\$30.00
Total mailing cost	\$4,834	\$1,874	\$4958	\$3868
Total incentive cost (actual)	\$30,325	\$26,670	\$6680	\$14,900
Fixed payment per report	\$33.27	\$0.00	\$11.53	\$0.00
Lottery payment per month (adjusted)	\$0.00	\$2000.00	\$0.00	\$1,000.00
Total incentive cost (adjusted)	\$30,325	\$23,500	\$6,680	\$11,680
Total cost (adjusted)	\$35,159	\$25,374	\$11,638	\$15,548
Total cost per report (adjusted)	\$53.84	\$89.66	\$24.35	\$25.04

This table calculates cost per report with one adjustment relative to the actual experiments. The adjustment is that it eliminates the fixed component of the lottery incentives under the assumption that this would obviate the need to send payment cards to every single producer because mailing a single check per month would be adequate. Even assuming that change would have no influence on the registration or reporting rate, the fixed payments were still more cost effective

entirely driven by one outlier producer who reported acreage more than triple any other producer in the sample and declined to answer the acreage question on the endline survey.

In both 2018 and 2019, producers who registered in the lottery incentive group were more likely to report wanting to improve water resource management and less likely to report wanting to receive financial compensation as motivations, but neither of those differences is significant at the 5% level. In 2018, producers who registered in the lottery incentive group were significantly more likely to report wanting to track water use as a motivation, but, in 2019, producers who registered in the lottery incentive groups were less likely to report that as a motivation.

Thus, we do not find evidence in our data that different types of agricultural producers selected into water use reporting depending on whether they were offered fixed incentives or lottery incentives, but more research on this would be needed to draw strong conclusions.

5.4 Administrative Cost Per Report

Using standard economic logic, the ubiquitous presence of lottery incentives implies that they are fulfilling some need. We investigate the possibility that the prevalence of lottery incentives is related to administrative convenience rather than popular appeal relative to fixed incentives. If lottery incentives are, dollar-for-dollar, less appealing than fixed incentives but also less costly to administer, they could be more cost-effective overall for a program administrator.

To assess the cost effectiveness of lottery incentives relative to fixed incentives, we construct a counterfactual that removes the costs of distributing payment cards from the administrative costs of the lottery incentive treatments. In this case, it is clear that the lottery is cheaper to administer, relative to fixed payments, if the lottery induces sufficiently high participation. The counterfactual asks what would have happened if AgDRIP had offered a pure lottery incentive (with no fixed component). In that case, we assume it would have been feasible to make prompt payments to an individual lottery winner via postal mail and avoid the cost of distributing payment cards to every producer in the sample. The analysis uses observed registration and compliance rates, which further assumes that eliminating the fixed component of the lottery treatments would have had no impact on registration or compliance rates.

In the counterfactual where the lottery treatment was administered without distributing payment cards, we estimate that the fixed incentive treatment still would have achieved a lower cost per report, including administrative costs. Table 2 breaks down the components of this calculation. We calculate a cost per report of \$53.84 for the fixed incentive treatment in 2018 and a cost per report of \$89.66 for the lottery incentive treatment in 2018. The cost per report in 2019 was \$24.35 for the fixed treatment and \$25.04 for the lottery treatments.

5.5 Motives for Participation

Participants in the experiments were invited in December of the sample year to complete an end-of-season survey about their reasons for participating in the program. Producers were paid \$10 for completing the end-of-season survey. The survey, shown in Appendix D, asked respondents to rate statements about motives for participating in AgDRIP in terms of their agreement or disagreement on a five-point Likert scale and asked some additional open-ended questions. Around 55% of the sample completed the end of season surveys—64 of 121 registered participants in 2018 and 55 of 95 registered participants in 2019.

The most common motivation cited by participants was improving management of water resources. Across both years, 90% of respondents agreed or strongly agreed with the statement, “I chose to participate this year because I felt it was important for improving the management of water resources in my area.” A majority of respondents also agreed that they were motivated by tracking water use (78%) and receiving financial compensation (76%). Comparing these three motives and given the opportunity to add other motives, 47% of respondents, a plurality, said that improving management of water resources was their strongest reason for participating.

The survey included three Likert-scale questions that related compensation to effort, fairness, and motivation. Appendix Tables B.9 and B.10 show that responses were similar to all three questions: between 72 and 83% of respondents agreed or strongly agreed that the compensation system was fair and motivated them to exert the effort required to submit reports.

5.6 Impact of Winning on Subsequent Compliance

In a study of lottery incentives offered to citizens reporting tax receipts in Brazil, Naritomi (2019) found that lottery incentives were effective in the sense that people who had won the lottery subsequently reported their tax receipts more frequently. This is a retrospective

effect and not the usual forward-looking way in which incentives operate in a standard economic model.

In an exploratory analysis, we find no evidence that winning the lottery in one month improved subsequent reporting rates. To test this question, we regress a dummy indicator variable for whether a report was received on one-month lagged dummy indicator variables for whether a report was received and whether the producer won the drawing: $reported_{i,t} = \eta_0 + \eta_1 won_{i,t-1} + \eta_2 reported_{i,t-1} + \epsilon_i$. The estimated “treatment effect” of winning the lottery (expressed by η_1) is -3.0 percentage points, with a 95% confidence interval of -18.6 to $+12.6$ percentage points. See Appendix Table B.11. This exploratory analysis had low power due to AgDRIP awarding only 20 prizes across the two seasons. In an alternative specification where the regressor is an indicator for ever having won rather than an indicator for having won in the immediate previous month, the estimated “treatment effect” is $+3.0$ percentage points, with a 95% confidence interval of -4.4 to $+10.4$ percentage points.

Eight of the ten producers who won a drawing in 2018 registered for AgDRIP in 2019. The 2019 registration rate among those producers who won a drawing in 2018 (80%) was thus higher than the registration rate among the 109 producers who participated in 2018 but did not win a drawing (50%) and much higher than the registration rate among the 2155 producers who did not participate in 2018 (1.5%). However, all ten of the producers who won a drawing in 2018 were highly compliant in the sense that they submitted at least eight out of ten monthly reports. The 2019 registration rate among the 78 highly compliant producers (who submitted at least eight reports in 2018) was 60%, which is still lower than the registration rate among producers who won a drawing but not statistically significant at the 5% level (95% CI from -12.7 to $+52.2$ ppt).

5.7 Subgroup Analyses

We examine whether some subgroups had larger treatment effects than others in several exploratory analyses. Appendix Tables B.14 and B.15 show that similar estimates are obtained when the sample is restricted to Georgia producers or Colorado producers, respectively. Appendix Table B.16 shows that lottery incentives, especially lottery incentives with unknown probability, had a stronger dampening effect among previous participants than among producers who had not previously participated. We suspect that producers who had previously participated paid more attention to the invitation and to the financial incentives than producers who had not previously participated, which suggests that the dampening effect may also be more pronounced in other contexts where the financial incentive is especially salient to decisionmakers. To avoid problems with multiple hypothesis testing and false discovery, we urge caution in drawing strong conclusions from the subgroup analyses, and we interpret them as being informative about promising directions for future research.

6 Discussion

Even though we failed to reject the two null hypotheses in the 2019 experiment, we believe our results point to the inferiority of lottery incentives. The two hypotheses in the 2019 experiment were likely underpowered—our power analysis in anticipation of the 2019 experiment used outcomes from 2018 as a guide without adequately adjusting for the dampening effect that reducing incentive magnitudes would have on registration. We

believe a replication with power to detect a 15% reduction in registration rates from an actuarially fair lottery incentive would be likely to find a statistically significant negative effect. Furthermore, the lottery treatments in these experiments had a fixed component—to the extent that the treatment effect of the lotteries was blunted by being mixed incentives rather than pure lotteries, the measured treatment effect may be attenuated relative to pure lottery incentives.

One counterfactual to consider is what would have happened if the treatment was administered as part of the registration payment rather than the compliance stage, e.g. instead of paying producers \$30 for registration, offer a 1-in-100 chance of \$3000. We suspect that such a registration lottery would have yielded similar results to what we observed in the lottery treatment, but it is possible that a lottery payment up front would have been more salient.

In our view, program administrators would be well served by offering fixed incentives to motivate participation by agricultural producers in data collection programs. Producers are well positioned to report data about ecological conditions and economic decisions on private agricultural land, which can be used to shape resource management policy. Fixed incentives appear to be more effective than lottery incentives at recruiting producers to participate in the data collection process. Furthermore, we find no evidence of selection into water use reporting by treatment, i.e. we find no evidence that producers who responded to fixed incentives were systematically different from producers who responded to lottery incentives.

While survey researchers generally find that prepaid incentives outperform postpaid incentives, many surveys continue to use lottery incentives, which are necessarily postpaid. The results we present suggest that little would be lost by utilizing prepaid incentives rather than lottery incentives.

More broadly, AgDRIP demonstrates that developing smartphone apps is a feasible method for facilitating large-scale data collection from agricultural producers. When data that is accessible to agricultural producers would be helpful for informing the design of policies—e.g. policies related to irrigation, pollution, or conservation—enlisting and incentivizing agricultural producers as data-collectors should be in the policy-making toolbox.

7 Conclusion

In the context of a voluntary irrigation water reporting program for agricultural producers, we used two randomized controlled trials to estimate the effect of lottery incentives relative to fixed incentives on program participation. Pooling the experimental data, we conclude that lottery incentives induced significantly lower registration rates than fixed incentives. Fixed financial incentives appear to be more promising than lottery incentives as a strategy for program administrators seeking to include agricultural producers in large scale data collection. Lottery incentives were less effective in an absolute sense and also less cost-effective, adjusting for lower administrative costs of distributing payments to fewer producers.

One important contribution of our study is to provide experimental evidence on the difference in responses to fixed incentives and actuarially equivalent lotteries, a difference that has not been explored in the context of agricultural producers. Participation under the actuarially equivalent lottery incentive was lower than under a fixed incentive, and we can

rule out differences that would imply that the actuarially equivalent lottery incentive was a substantial improvement relative to the fixed incentive, as some behavioral economics models would predict. However, we could not rule out zero difference—i.e. the 95% confidence interval for the estimated difference in participation under the two contracts includes zero. Thus a replication with more statistical power would be necessary to claim definitively that an actuarially equivalent lottery incentive is worse than a fixed incentive.

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Declarations

Human Subjects Research The experiments reported in this paper were approved by the Institutional Review Board at Albany State University under the title “Understanding Agricultural Water Use Behavior Through Randomized Controlled Trials.” IRB 00004776, project 1041657.

Exclusivity This paper has not been submitted elsewhere in identical or similar form, nor will it be during the first three months after its submission to the Publisher.

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