THREE ESSAYS ON HETEROGENEITY AND ASYMMETRIC INFORMATION IN LAND ECONOMICS

by

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INFORMATION IN LAND ECONOMICS

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ABSTRACT

Social planners use different policy tools to correct market failures arising in land economics. Sometimes, social planners use reverse auctions to retire or reserve farmland from production. Other times, social planners offer incentive contracts for the adoption of best management plans. Taxes may be used to influence landowners' development decisions, too. Heterogeneous attributes of landowners as hidden information create challenges to social planners in designing cost-effective policy tools. Landowners have better information and make use of this advantage to maximize private benefits, which limits the performance of these policies, measured by different metrics. The three essays evaluate those policy tools and investigate the heterogeneity and asymmetric information problem in land economics.

The first essay investigates heterogeneous land develop decisions under two property taxes. Land value taxation (LVT) is thought to enhance the efficiency of property taxation by raising revenue without distorting land improvement incentives. A related set of results suggest that LVT promotes economic development and reduces urban sprawl. With heterogenous induced-value setting, this research constructs a spatial economic model of a virtual urban area to extend the only existing LVT experiment under behavioral economics settings, in which participants act as property developers who invest to build "out" (by employing more land) and build "up" (by adding building floors) in a dynamic setting. In addition, the experiment uses a voting treatment with information nudge, in which participants could vote for their preferred tax plan, capturing LVT's political acceptability in the laboratory. Experiment results

show that experiment participants adhere to their optimal tax preference and building choices for most of the time and LVT does lead to higher earnings to landowners, higher tax revenues, and denser cities as the theoretical models predict. However, experiment participants' voting choices can also be swung by information nudge that does not affect their earnings directly.

Reflecting on the low participation rates of certain incentive programs, the author suspects that rigid eligibility requirements and strict rules stop farmers from participating in and further limit the performance of these programs. A theoretical model is constructed to compare the performance of fixed payments and reverse auctions under different levels of flexibility. Heterogeneous willingness-to-accept (WTA) is introduced as hidden information. Non-additional choices are defined as choices with negative WTA, which are used to discover the potential trade-off between flexibility and additionality. A neutral framed lab experiment with induced-value is designed and conducted to support the model. Preliminary experiment results show that program flexibility affects program performance through the mechanism and through participants' behavior change. A more flexible contract leads to higher cost-effectiveness, but also more non-additional behaviors. Although the internal validity of the experiment is significant from regression analysis, the external validity is questionable as other neutral framed lab experiments.

To enhance the external validity and address the asymmetric information problem in an empirical setting, the third essay investigates the effects of cost-share payments on the adoption of cover crops in Maryland and Ohio. A three-stage theoretical framework is constructed to explain the sequential outcomes of planting cover crops, cost-share enrollment, and the share of acreage under cover crops as the intensity of adoption. This research used a double-selection model with incomplete classification to test and correct the potential selection bias in estimating the intensity of adoption. The model is estimated with survey data from Maryland and Ohio in the 2017-2018 planting season. Controlling the self-adoption rate as the counterfactual of paid-adoption, the cost-share programs in Maryland is estimated to increase cover crop adoption by additional 21.85 percentage points of farmland for enrolled farmers on average, while the estimated effects in Ohio is lower at 19.03 percentage points, which can be attributed to the lower per acre payments and a payment cap. Furthermore, an out-of-state estimation predicts a 27.74 percentage points increase in acreage if Ohio can employ a program similar to the one in Maryland.

Although the three essays are constructed under different contexts, the economic theory of asymmetric information and heterogeneity connects the three essays. The first essay introduces an experiment design testing information nudge in the landowners' tax preference and land development decisions. The second essay shows how rational landowners with heterogeneous WTA as hidden information response to different policy tools in a lab environment and how policy tools perform with respect to behavior change. The second essay brings the asymmetric information problem from the hypothetical lab environment in the first essay to the farm fields and predicts the effectiveness of cost-share programs using survey responses from farmers. The combination of lab experiments and empirical method contributes to a better understanding of the asymmetric information and heterogeneity problem in land and agricultural economics.

Chapter 1

NUDGING LAND VALUE TAXATION: A SPATIALLY EXPLICIT EXPERIMENT WITH ENDOGENOUS INSTITUTIONS

1.1 Introduction

Many researchers believe the land tax (LVT) is superior and more efficient than uniform property taxation (UPT), in part, because it promotes a distortionless, more-intensive use of land (Pollack and Shoup 1977; DiMasi 1987; Plassmann and Tideman 2000; England and Ravichandran 2010; Banzhaf and Lavery 2010; Chapman, Johnston, and Tyrrell 2009; Choi and Sjoquist 2015; Gemmell, Grimes, and Skidmore 2017) without regressivity (Bowman and Bell 2008; Plummer 2010; Choi and Sjoquist 2015) or with correctible levels of regressivity (England and Zhao 2005). An essential corollary to the intensity-efficiency advantage is that LVT also prevents "sprawl." Sprawl is a value-laden word, but the meaning to an economist is that land use develops without negative externalities. Just as LVT "gets prices right" with respect to the intensity of vertical improvements, LVT also can help get prices right horizontally. LVT's horizontal advantage arises not from internalizing negative externalities but instead from maximizing the economic potential of land near the central business district (CBD) and reducing incompatible land uses.

Greater-than-anticipated land investment, capital/land ratios, and population density have become metrics for the spatial land-use advantage of LVT. The relevant studies use different models, data, and/or metrics to assess the spatial impacts of LVT, so it is not necessarily clear how to synthesize the results (though Banzhaf and Lavery 2010 offer the most comprehensive work). Pollack and Shoup (1977) offered an early approach to the investment-intensity question, arguing that LVT could increase improvements by up to 25 percent. DiMasi (1987) used a general equilibrium model to show that a version of LVT increases improvements per unit of land and population density. Plassmann and Tideman (2000) offered empirical evidence that land tax municipalities had significantly higher levels of construction (measured with building permits). Banzhaf and Lavery (2010) offered strong, empirical evidence that a version of the land tax reduces sprawl because it increases housing units but not housing size. Importantly, Banzhaf and Lavery (2010) argued that perceived inconsistencies in the LVT-sprawl papers (c.f. Choi and Sjoquist 2015 vs. Song and Zenou 2006) may be because different metrics are being confounded; LVT has an improvement effect, a density effect, and a dwelling-size effect (also see England, Zhao, and Huang 2013 for a study of these effects with varying property tax). On balance, the literature suggests that moving from UPT to LVT will reduce sprawl and increase the intensity with which land is used (Brueckner and Kim 2003; Choi and Sjoquist 2015).

This paper starts with a spatial-economic model and experiment of a stylized city, where there is predicted efficiency[1] and density advantage for LVT relative to

[1] Different from other economic research in which "efficiency" is mostly used to describe the status of a society when the overall social welfare is maximized, this work uses "efficiency" in a limited way.

UPT. Experiments are new to LVT research, and the first experiment paper (Duke and Gao 2018) offered a land investment model with some endogenous institutional choice treatments, i.e., experiment participants were able to vote for their preferred tax institution while making their land-investment choices. This paper offers a new model with three further innovations. First, the new model is spatially explicit, which allows estimation of how tax institutions affect sprawl. Second, some of the endogenous institutional choice treatments expose participants to a nudge prior to voting, which tests whether "cheap-talk" norms can encourage adoption of LVT. Third, the distribution of landowner types is varied to see how varying the majority (or minority) in favor of LVT affects nudge effectiveness, which tests whether voting patterns and nudges tend to perform differently when LVT is majority-preferred.

Nudges have been used and studied in economic research even before Thaler and Sunstein's book, *Nudge: Improving Decisions About Health, Wealth, and Happiness* (2009). They defined a nudge as "...any aspect of the choice architecture that alters people's behavior in a predictable way without forbidding any options or significantly changing their economic incentives." Economic researches have proved that nudges affected people's behavior or choices in lab experiments (Alm et al. 1999, Marks et al. 1999, Croson and Marks 2001, Feld and Tyran 2002, Kroll et al. 2007, Messer et al. 2013) and field experiments (Costa and Kahn 2013, Zarghamee et al. 2017). In lab experiments, some research tested how nudges change people's compliance with tax payments or donations to public goods. For example, Alm et al.

Although this is a compromise in order to simplify the calculation and experiment process (explained in detail later), it offers the researcher a more straight-forward way to explain the advantage of LVT.

(1999) found that group communication as “cheap talk” before voting for tax rate increased tax compliance. The design of the experiment matters a lot in lab experiments because the decisions are made in a hypothetical and also highly controlled environment. A minor difference may incur significant changes in participant’s choices. Messer et al. (2013) found that experiment framing, voting, and participants’ endowment had significant effects on voluntary contribution to public goods. Field experiment studies, which is less hypothetical but introduces more noise in analysis, also show significant effects of nudges. Costa and Kahn (2013) showed that information nudges were a cheap way to reduce electricity usage although the effects vary by peoples’ political attitudes. Zarghamee et al. (2017) found that framing (status quo bias), social norms (group vote), and mood all changed people’s charity donations from three field experiments. This research here introduces a new political economy perspective in using nudges to influence people’s preference for tax mechanisms.

The political economy perspective is informative because prior research suggests that LVT faces a vast array of political objections (c.f. Fischel 2015, p. 15; Youngman 2016, p. 193). LVT objections were explored by Bourassa (2009, pp. 195-6) who found ethical objections (such as opposition to taxing unrealized capital gains), objections from policy change “losers,” and objections to too-dense development. Plummer (2009) investigated perceived inequities from tax incidence. Duke and Gao (2018) found that despite a general tendency to be more efficient than UPT, LVT may trigger over-investment among some homeowners because of positionality. This paper develops an entirely new experiment to remove some drivers of LVT effectiveness identified in Duke and Gao (2018), including positionality and regressivity corrections, to develop a purer test of nudges without previously identified behavioral confounds. The theoretical model with parameterization replicates the theoretical support for LVT that LVT generates higher efficiency and more compact cities under landowners' optimal choices. Although the density of a city may affect the life and welfare of its residents, the research uses the density as a separate measurement from efficiency. The experiments results complement the theoretical results and herein show the efficiency and density outcome under possible sub-optimal decisions. Furthermore, the experiment also reveals that participants sometimes vote for the tax that does not maximize their individual earnings ("earnings-irrationality"), and group patterns and nudges can affect institutional choice.

1.2 Theoretical Framework

This section explains the spatial-economic model that governs the experiment and leads to predictions. The model explores how cities grow spatially under different tax plans with a stylized monocentric circular urban area similar to the one used by DiMasi (1986). The urban area develops outward from an existing central business district (CBD), and the expansion of the city is represented by a set of concentric rings, denoted by $i \in \{1, 2, 3, \ldots, I\}$. i here not only denotes the ring number but also denotes the linear distance from a building site to the CBD. To further simplify the model, rays emanating from the CBD, denoted by $j \in \{1, 2, 3, \ldots, J\}$, are used to represent the directions or roads of development. Figure 1 shows an example of four rings and five directions. Undeveloped land parcels are available for building at the intersection of rings and directions. J developers own one of the J roads each and will develop along their specific roads. Thus, each potential development locations can be indicated by a unique pair of (i, j). The model also assumes a predetermined urban boundary of ring I and undevelopable agricultural land is beyond it. In each period $t \in \{1, 2, 3, \ldots, T\}$, the landowner decides which location i to place a single development unit, which is stylized as a "brick." Each landowner is endowed with one brick at the beginning of period t. B_{ijt} denotes the number of bricks at location i for landowner j at period t. B_{ijt} as a symbol of the building also incurs revenue, cost, property value, and thus property taxes introduced later. All locations start with zero bricks, i.e. $B_{ij0} = 0$. The theoretical model here is designed largely to fit into the lab experiment introduced later. By granting one brick per period, the researcher overcame the need to have a separate production function, input costs, and complex constraints. The stylized concept of placing a brick was described in terms of a Lego© block as a salient symbol of land improvement.

1.2.1 Revenue and Costs

The choice problem involves revenue, operating and maintenance costs, and taxes. Bricks, as a symbol of a building, at location (i, j) generate revenue for its own j as $R_{ijt} = \alpha_0 B_{ijt}^{\beta}/i$, expressed in experimental dollars. All else equal, bricks generate more revenue when bricks are closer to the CBD. Bricks on the same ring i generates the same revenue regardless of the owner. Later, β is parameterized to generate increasing marginal revenue with each additional brick at a given location. The operating and maintenance (O&M) costs arising from brick placement accrued in the period of placement and in each successive period: $C_{ijt} = (a_1 B_{ijt} + a_2 B_{ijt}^2)/i$. The parameters a_1 and a_2 are the sole source of induced heterogeneity in the experiment, with five types (see values used in the experiments in Table 2). For all parameters, costs increase at an increasing rate with bricks at any one location. Also, costs also decrease as the distance to the CBD increases.

1.2.2 Property Values and Property Taxes

Similar to Duke and Gao (2018), property values (PV) are defined as the sum of land values (LV) and improvement values (IV). $PV_{ijt} = LV_{ijt} + IV_{ijt}$. Improvement values stand for the values of establishments built upon a piece of land or location. This model uses revenue, R_{ijt}, generated from bricks as a representation of improvement values. The model of land values majorly depends on two factors: the location of a piece of land, and capitalization of building activities on and around that land. For each location (i, j) at period t, its land value is calculated as:

$$LV_{ijt} = (\lambda_1 B_{ijt} + \lambda_2 B_{i-1,j,t} + \lambda_3 B_{i+1,j,t} + \lambda_4 B_{i,j+1,t} + \lambda_4 B_{i,j-1,t})/i \quad (1.1)$$

where λ_1 to λ_4 are non-negative coefficients. The denominator i indicates a decreasing trend of land values with respect to the distance to the CBD. A location further away from the CBD will have lower LV, all else equal. The numerator captures the building activities and the land value capitalization process. Adding bricks, i.e. increasing building activities, will contribute to the value of a piece of land. This capitalization process takes a similar but stronger stand compare to Duke and Gao (2018). The land value of location (i, j) not only captures the amenity of establishments on (i, j), with a multiplier of λ_1 but also building activities around it. To be more specific, building activities on the same road but one ring inward, $B_{i-1,j,t}$, and one ring outward, $B_{i-1,j,t}$ will contribute to the land value of location (i, j) with multipliers of λ_2 and λ_3. Buildings on the same ring i but neighboring roads will also add values to the location (i, j) with a coefficient of λ_4. Thus, LV_{ijt} is only positive if: (1) there is a brick at a location; and/or (2) there is a brick at a location adjacent to it. The CBD is assumed to have a fixed number of bricks ($B_{0jt} > 0$) at any period t, so locations on ring 1 will always have a positive land value to start. The agricultural area has zero bricks. The property values form the basis of taxation introduced next.

Property taxes change the benefit-cost analysis of the landowners and thus the optimal choice of building activities, i.e., the location to put on the endowed brick in each period. Two tax systems are studies here. LVT assesses each location on land values alone: $Tax_{ijt}^{LVT} = \tau_{LVT}LV_{ijt}$. UPT taxes at the same rate for land values and for improvements values, which are simplified as the revenue from bricks at each location: $Tax_{ijt}^{UPT} = \tau_{UPT}(LV_{ijt} + R_{ijt})$. Tax revenue is also used as a metric to compare the two tax schemes later. Because tax revenue is a transfer—even if it is not costlessly transferred—the tax institution that generates more revenue is superior than the one that generates less, all else equal. Because tax revenue can be spent by governments for public goods, which further enhances social efficiency. This reasoning suggests that an LVT tax surplus, relative to UPT, captures one important social efficiency advantage for LVT.

1.2.3 The Long-Run Optimization of Landowners' Choice

Landowner j's choice problem is to decide the location i for one brick in each of period t in a limited time cycle of T periods. Let the choice variable $L_{jt} \in \{1, 2, 3, \ldots, I\}$ be an indicator of which one of the i locations was selected. L_{jt} determines the increment of bricks on location i as X_{ijt} such that $X_{ijt} = 1$ if $i = L_{jt}$ and 0 otherwise. The optimization problem for each landowner j is to maximize earnings E_j, which is a summation of revenue, cost, and property taxes, subject to the constraint of a single brick available in each period. This problem can be written as:

$$\max_{L_{jt}}: E_j = \sum_{t=1}^{T} \sum_{i=1}^{I} (R_{ijt} + C_{ijt} - Tax_{ijt}) \tag{1.2}$$

$$\text{s.t.} \quad B_{ijt} = B_{ijt-1} + X_{ijt}$$

$X_{ijt} = 1$ if $i = L_{jt}$ where $L_{jt} \in \{1, 2, 3, \ldots, I\}$ in any t

$B_{ij0} = 0$ for $i > 0$

The first constraint shows how bricks increase based on those placed in the previous period. The second constraint indicates that there is only one brick available in a period and it will be added to the location of the landowners' choice. The final constraint indicates that each time cycle starts with no bricks at any locations. After substituting R_{ijt}, C_{ijt}, and Tax_{ijt} in the objective function with the equations discussed above. The optimization problem reduces to a series of location choices of L_{jt}. Note that (1) the author is not trying to solve the optimization problem theoretically but to construct a theoretical framework that serves the experiment introduced later, (2) the inclusion of time requires a long-run equilibrium that maximizes the overall earnings of T periods, and (3) thus, the solution, if solved, will be a series of numbers that follows a certain order, not a single number. The author next parametrized all coefficients to meet time and budget requirements of this research, and the cognition ability of human participants in an experiment session. With the parametrization introduced later, the author was able to calculate the optimal solution to a limited version of the optimization problem here.

1.2.4 Parameterization

Table 1 shows numerical values of coefficients used in the parameterization process. The parameterization first controls the number of rings, landowners, and periods. After numerical simulations with different values, the author finalizes the coefficients as shown in Table 1.

The parameterization of five landowner types reflects the modeling decisions by the researcher, who sought predicted treatment effects on efficiency, spatial development patterns, and voting behavior. However, the parameters hold several connections to real-world landowners. First, buildings on the same ring will earn the same market prices regardless of which type builds. Also, given that each type gets one brick each period, then the types have the same endowment; one could think of very similar “box” buildings where it is easy to scale them to have small or large footprints and short or tall height. The formulation of production as a “brick” further simplifies the output of land use to be a homogeneous production unit. Second, heterogeneous costs assume that some types have a comparative advantage in O&M; these are ordered by this skill from best (*Type1*) to worst (*Type5*). *Type1* has the best human capital or technology for O&M such that they can service any building at any given location at a lower cost than the other types. These simplifications relate best to a real-world setting in which relatively similar, scalable commercial or residential buildings—such as office buildings, apartment buildings, warehouses, etc.—could be placed at various distances from the CBD. More “bricks” at any location could be thought of as more intensity or vertical development—such as taller buildings.

Type1 is labeled as an “LVT lover” because this type has the best O&M technology for higher buildings, and therefore will be more profitable from more intensity at any location relative to the other types. They “love” LVT in that this tax does not penalize the intensive building pattern they prefer (except the property tax incurred by land value capitalization, which is at a much smaller scale). In contrast, *Type4* is a “UPT lover” because their technology is not skilled like *Type1* at O&M. *Type4* profits more when land and improvements are taxed at the same rate because they tend to have lower relative costs when there is little intensity at a location. The real-world landowner of *Type4* would have lower-than-average improvements, such as a one-story office park, while *Type1* would have higher intensity. *Type2* and *Type3* are intermediate cases. *Type2* is a marginal "LVT lover" who benefits slightly from LVT, while *Type3* is a marginal "UPT lover" who benefits slightly from UPT. The behavior of these two marginal types will be important in the analysis because they have the lowest opportunity cost for an earnings-irrational tax institution. *Type5* is interesting because its O&M technology is so poor that it is indifferent between LVT and UPT in terms of location choices, though it earns more under UPT. This type may relate to low-intensity commercial land uses that have large parking lots. Their O&M costs increase too rapidly if they add any density, so their profitability comes from having minimal possible improvements. They have the largest earnings preference for UPT because the tax rate on land is lower than LVT and they, in effect, can free ride on the public goods funded through the tax paid by their neighboring owners who have more improvements. Although the *Type1* "LVT lovers" make the most efficient use of land closer to the CBD, their comparative advantage in O&M of bricks is unique and cannot simply be replicated by the other types. So, the social planner's problem is to devise a system of tax incentives that maximizes the net social product of the five types in the city. As explained below, the distribution of types varies so that some sessions have only one *Type1*, while others have two or three.

Note that the coefficient of λ_4 works as a "switch" of landowner interdependence. If $\lambda_4 = 0$, each landowners' building activities are independent and one's choice will not affect neighboring landowner's land values. If $\lambda_4 > 0$, there is interdependence between the land values of neighboring landowners. This reflects a "spill-over" effect of building activities across roads and this effect is especially stronger in denser cities. For simplicity, this effect is named "side-effects" and experiment session without side-effects ($\lambda_4 = 0$) is hereafter referred to as independent sessions. Experiment sessions with positive λ_4 are named “interdependent sessions”. The author started with independent sessions in which $\lambda_4 = 0$, such that land values on one road evolve independently[2] of decisions on other roads. Land value at any location *i* is only affected by *j*’s own choices. Thus, there is no negative capitalization externality in these sessions because *j*’s own choices of brick location determine the land values and, thus, taxes. Note that the CBD is parameterized to have a fixed number of six bricks. This effect on location $i = 0$ is external to participant *j*, it (1) is not an externality that arises from participant -*j*’s choices; (2) affects all participants in the same way; and (3) simply puts a baseline, the nonzero land values on locations on ring 1. Until an endogenous voting process is introduced, the optimal choices in the independent sessions are known with certainty[3].

1.2.5 Predicted Behavior and Treatments on Efficiency and Compactness

Table 2 shows the optimal choice set for each of the five landowner types. As discussed above, because of the intertemporal design of the theoretical model, the optimal choice set is a set of locations order by period *t* as $\{L_{j1}, L_{j2}, L_{j3}, L_{j4}\}$ for any

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[2] A modeling challenge is to manage the degree of interaction among participants. Obviously, voting allows the participants to interact. However, based on Duke and Gao (2018), the researchers were concerned that land capitalization interdependency and tax redistribution could overpower the political economy results. As such, this paper reports an experiment with independent capitalization and no tax redistribution; tax revenue is compared by treatment but not constrained to be revenue-neutral.

[3] One might object that there is no salvage value in this model, which would allow one to capture appreciated land value. This modeling simplification is, in part, due to the practical need to make the decision problem as simple as possible for experiment participants. However, this simplification also can be justified in that land and bricks are supplied to the participants for free. Conceptually, the decision setting could be a manager, who takes over a going concern for four decision periods and where all management incentives reduce to the aforementioned revenue, cost, and tax processes. These incentives are net of the cost of capital.

participant *j*—all of which can be expressed as location decisions *i* for simplicity. The optimal choice set leads to the highest earnings derived by a landowner in a round indicated by the variable, $Earnings_j$ in the objective function of equation (1.2). A related variable, $GroupEarnings = \sum_{j=1}^{J} Earnings_j$, adds up the city earnings over four periods. In this stylized model, the earnings variables measure efficiency, capturing all wealth other than the tax revenue created by a city in a round.

The choice set also leads to a spatial distribution of building patterns. Many indices of sprawl exist, but this research developed a simple measure based on the metric used in gravity models of trade. Road compactness measures the spatial density in terms of distance from the existing CBD:

$$Comp_{jt} = B_{0t} * \sum_{i}^{I}(B_{ijt}/i) \tag{1.3}$$

Given the current parametrization, $Comp_j$ ranges from 6 to 24. When all bricks are in location 1, $Comp_j = 24$ and $Comp_j = 6$ when all bricks are in location 4. The higher the value of *Comp*, the more compact is the landowner's "road." The multi-person "city" also can be represented by a simple average of the individual road compactness values: $CityComp = (\sum_{j=1}^{J} Comp_j)/J$. As Table 2 shows, *Type1* ought to have the most compact (*Comp* = 18) investment under LVT, i.e., {1,1,2,2}, followed by types 2-4, i.e., {1,1,2,3} or {1,1,3,2}, *Comp* = 17. All types ought to pursue the same building pattern under UPT, i.e., {1,2,3,4} with *Comp* = 12.5. *Type5* has no spatial treatment effect in that the optimal choice for LVT and UPT is {1,2,3,4}. $Comp_j$ vary considerably less than $Earnings_j$ because earnings are designed to be induced values that governs experiment participants' decisions. As shown in Table 2, *Type1* and *Type2* earn more from their optimal choices when taxed with LVT, while the other three types earn more with and thus prefer UPT. Table 2 presents the induced earnings advantage under LVT for each type.

1.3 Experiment Design and Treatments

The parameterization and optimization results in the theoretical framework build the context and lay the foundation for the experiment design and analysis. First, at the individual level, the above work designs a building game in which student participants can follow the rules to maximize earnings. This is a setting to ensure basic rationality assumption. Second, the researcher is interested in how the participants' choices or behaviors are affected by earning-independent treatments, especially, information nudges. Detailed treatments and hypothesis are explained below. Third, the researcher is also interested to see how the tax institutions affect the group or the city overall. Although the group-level results can be calculated from individual results under the optimal condition, they may show different patterns when the optimal choices or rational assumptions are violated. The experiments investigate possible outcomes by allowing deviations and uncertainties in choices.

To start with, the experiment is programmed with equations and parameters from Table 1. Thus, five participants play as landowners in each round and each round is treated as a time cycle of development with $T = 4$ periods. All references to the tax plans in the experiment used neutral language whereby UPT was "Tax Plan 1" and LVT was "Tax Plan 2." Table 2 shows the set of five types with heterogeneous choices, resulting earnings and compactness, and a fixed payment. Paid at the end of a round, the fixed payment balanced the expected earnings among the types when earnings-rational behavior is pursued. Although the instructions informed the participants that they would receive fixed payments, the payment levels ought not to affect their behaviors because they do not affect marginal earnings from decisions. The five types were labeled by color within the experiments so that the participants would not infer any ordering.

1.3.1 Group heterogeneity

The researcher examines how the heterogeneity of types affects the efficiency, compactness, and voting outcomes from the tax institutions. This is made possible by changing the distribution of landowner types in each group. *TypeDistA* had one of each type (1-5). This distribution predicted a 2:3 ratio of participants who “win” from LVT, so UPT should win in an earnings-rational vote. Nevertheless, *GroupEarnings* and *CityComp* are higher in LVT. So, LVT is potentially Pareto efficient, but will not be selected by earnings-rational voting. This 2:3 contrast between what is individually versus socially optimal replicates Duke and Gao’s (2018) experimental set up, which sought to capture the received knowledge on the U.S. experience with LVT. To wit, LVT is thought to make society wealthier but tends to be voted down by communities that use it. The current paper extends prior work, in part, with two new voting distributions (3:2, and 4:1). *TypeDistB* had two *Type1* participants and no *Type5*, so earnings-rational voting is 3:2 for LVT. *TypeDistC* had three *Type1* participants and no *Type4* or *Type5*, so earnings-rational voting is 4:1 for LVT. All three distributions led to an outcome where LVT was socially efficient (with 64.63, 233.63, and 394.89, additional *GroupEarnings* respectively). Type distributions did not vary within sessions.

1.3.2 Voting Treatments

The experiment makes the tax institution endogenous in six of eight rounds by allowing participants to select their preferred tax institution by a majority vote. A voting round begins with either LVT or UPT. Participants vote in the first period whether to switch to the other tax institution before they made their location choice. If a majority of 3, 4, or 5 voted to switch, then the present period and the remaining periods of the round will have the new tax institution. If 0, 1, or 2 voted to switch, then one more period of the initial tax is played, and another vote is conducted in the following period. The starting tax institution was alternated for each round. Three variables derive from the voting treatments. First, *Vote* is an indicator of whether the round was played with a voting treatment; when *Vote* = 0, the tax is exogenous. Only rounds 1 and 2 of any session have *Vote* = 0. Second, *LVTStart* indicates whether the initial period started in LVT. In any session, four rounds will start in LVT and four in UPT. This variable enables statistical tests of anchoring. Third, *LVTPeriods* $\in \{0,1,2,3,4\}$ measures the number of periods in a round under LVT. In voting rounds the tax institution can switch, so one must track how much of the round was under LVT.

1.3.3 Information Treatments

Some voting rounds (rounds 5-8) used an information treatment. Thaler and Sunstein (2009, p. 6) defined a nudge as "...any aspect of the choice architecture that alters people's behavior in a predictable way without forbidding any options or significantly changing their economic incentives." The information treatment was set up like a nudge because it does not affect earnings, but it merely communicates a normative claim about the overall city welfare—a claim that simply can be ignored.

The nudges are framed as a way to encourage participants to vote for LVT because it is socially “superior” for the city. Four different nudges were developed through a broad review of writings about LVT. Both messages were framed positively and negatively. The positive framing was (UPT is Tax Plan 1 and LVT is Tax Plan 2):

PosInfo1: Tax plan 2 leads to higher total earnings for the City. This is true even though some color types earn more under Tax plan 1 and some earn more under Tax plan 2.

PosInfo2: Under Tax plan 2, revenue from buildings of all sizes—from 0 bricks to 4 bricks—pays the same uniform tax rate of zero. This is because all taxes are imposed on the land and no taxes are imposed on revenue from buildings. This allows builders to add as many bricks as they wish without penalty.

The first nudge is an earnings statement, while the second combines earnings and tax incentives; it captures tax incentives in that all “pay the same uniform tax rate” and individual earnings in that there is no distortion associated with improvements. Two “mirror” nudges were developed that reframed the nudges negatively about UPT. The researchers were concerned about order effects on these treatments because once information is provided, it cannot be taken away. As such, only a positive or negative framing was used in a given session. The *PosInfo1* or *NegInfo1* rounds always came first; therefore, the second nudge has a cumulative effect of the first and second nudge.[4]

[4] Participants were unaware of the socially efficient tax until the information treatments. Thus, the nudges revealed not only normative claims about LVT but also what was best for the city. Although the researchers do not expect participants to make earnings-irrational decisions, participants may have a meta-utility function. It is possible that a “UPT lover” (types 3, 4, or 5, and especially type 3 who is closest to indifference) may vote for LVT out of a sense of altruism, knowing that the group is better off even though that type personally loses from LVT. This is not an unrealistic framing of how nudges are expected to affect behavior.

1.4 Experiment Sessions, Data Structure, and Hypotheses

Data were collected at the University of Delaware's Center for Experimental and Applied Economics. The z-Tree software was used (Fischbacher 2007) with 10 tablet computers linked to an administrator computer. Student participants were largely undergraduate business and economics majors, though others were recruited when sessions did not fill. The University of Delaware Institutional Review Board approved the protocol. Participants completed informed consent, read paper instructions, watched an instructional presentation, asked questions, and were trained in two unpaid practice rounds. The practice rounds helped participants learn how the interface works and the basic aspects of the game. The first practice round had no taxation, so participants simply placed four bricks. The second practice round then introduced taxation with a different set of tax rates.

Participants completed eight rounds of four periods each.[5] New instructions were distributed with every treatment. Although participants played eight rounds, only one was randomly selected for payment in order to magnify the incentives from each decision. A session took 1.5-2.0 hours to complete. The exchange rate for experimental dollars was 17: $1 and a participant earns $25 on average. To prevent bankruptcy in the experiment, the administrator gave a participation incentive of $5 to all who completed the experiment. A post-experiment survey showed that

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[5] Participants saw the following information during a period in the experiment. The first screen showed private information for the participant describing the status quo building and the costs and revenue of each potential brick placement in the current period. The participants made a choice on this screen. The second screen showed the results of the participant's choice and also the building pattern of four other participants. The building pattern was simply a table of five rows (for each participant) and four columns (for their locations). Each cell in this table had a number of bricks placed. Third, in voting treatments, there were two preliminary screens first asking for the individual vote and second seeing the results of the group vote.

respondents tended to make earnings-rational decisions but also paid attention to provided information.

Although highly simplified, there are 4^4=256 possible brick placement choice sets in the four-period round. As such, the research team provided participants with a printout of the earnings from each of 256 possible choices; a different printout was provided for each type. This table had the top five earning decisions for LVT and for UPT highlighted, so participants could quickly identify high-earning choices.

1.4.1 Experiment Sessions and Data Structure

The experiment devised an ordering of the treatments over nine independent sessions, which would minimize confusion, not attempt to "remove" any information already introduced and prevent any order effects. Five sessions received two positive nudges, while four received two negative nudges. Each session consisted of two cities run simultaneously with the same treatments. Participants played one LVT and one UPT round in: (1) a nonvoting treatment; (2) voting with no information; (3) voting with nudge I; and (4) voting with nudge II. Every participant started the paid rounds with two non-voting treatments—one with each tax institution. This is a 1x2=2 design. Then, each participant would play all (3x2=6) of the voting treatments: (no information, nudge I, nudge II) x (UPT, LVT). The order of the information treatments was always none, then I, then II. However, the UPT and LVT orders were highly randomized over six possible orders. Table 3 lists the full experiment design and the data collected in each session.

1.4.2 Hypotheses on Efficiency and Compactness

The model framework is parameterized to motivate the theoretical hypotheses recognized in the literature that LVT generates more aggregate wealth and a more compact city than UPT if all participants follow the optimal choice sets. Wealth creation is measured with *GroupEarnings* and tax revenue. Table 4 shows the predicted *GroupEarnings* and tax revenue if all participants follow the optimal choice set. Although voting and information treatments are introduced in later rounds, the building choices constitutes a necessary part for the induced values. Thus, the hypothesis on efficiency and compactness serves as a necessary condition for the voting and nudge hypothesis introduced later. If these hypotheses are verified to be true, it supports arguments that the participants understand the game and make earnings-rational building decisions. Without this economic decision model in the background, the results on the following analysis fall into general claims of information nudge which are too broad to fit into the land tax context.

In addition to the tax systems, LVT and UPT, the following three factors also contribute to the differences in earnings, tax revenue, and compactness. First, type distribution. This is a direct result of the parameterization process. If a group has more "LVT lovers" as in *TypeDistC*, the group earnings should be higher, and more building activities happen near the CBD. Second, the existence of voting treatment. The Voting treatment gives participants the choice to choose the tax system that earns them more money, although one player's vote may not change the tax mechanism. Thus, participants' votes may change the aggregate earnings indirectly. The tax treatments require more complex controls because, in the voting treatments, the tax institution can switch in any periods of 1-4. The author uses variable *LVTPeriods* ϵ $\{0,1,2,3,4\}$ to indicate the number of periods played under LVT. Third, information treatments. On one hand, the information treatments nudge the participants towards LVT, and the parameterized model has higher *GroupEarnings* under LVT for all type distributions. On the other hand, nudges are cheap talks. Thus, one expects that the coefficient on the indicators for these treatments (*PosInfo1*, *PosInfo2*, *NegInfo1*, *NegInfo2*) will be positive or have no effect. The first treatments explicitly mention *GroupEarnings*—so one might anticipate a positive coefficient. Further, one expects the second nudge to have either a larger substantive effect or be more likely to have a positive effect than the first nudge because it introduces a second dimension (equity) and it has a potential cumulative impact with the first nudge.

The same explanations can be applied to the hypothesis of city compactness or *CityComp*. The logic associated with the hypotheses is similar because *CityComp*, like *GroupEarnings*, was parameterized to increase with LVT for all type distributions.

1.4.3 Hypotheses on Group Votes and Individual Votes

A set of hypotheses explore participants' votes and this analysis is conducted at both the group level and individual level. The individual level analysis relies on the variation induced by the type distributions. The dependent variable in a regression of 285 group votes is the number of votes, *LVTVotes* $\in \{0,...,5\}$, in favor of LVT within the five-person city and within any period that a vote is held.[6] One expects that *LVTVotes* increases in *TypeDistB* and *TypeDistC* because these distributions have

[6] Though there were 576 periods of data, all periods in rounds 1 and 2 had no votes. Thus, there are only 432 possible voting periods (6 rounds x 4 periods x 2 cities x 9 sessions). Further, although voting may occur in up to four periods per round, once a vote to change tax institutions occurs there are no more votes held. Of the 432 possible voting periods, only 285 had votes. These 285 are the units of analysis for the regression explaining *LVTVotes*.

more types who favor LVT relative to *TypeDistA*. *LVTStart* also could lead to higher *LVTVotes* because of an anchoring effect. The nudges promote LVT, so all should have a positive impact on *LVTVotes* and the second nudge treatments should have larger substantive impacts because of the cumulative effect; however, as argued above, there also could be no impact because the nudges are cheap talk. The variable *Round* controls for experience.

Analyses on individual votes test: (1) whether experiment participants vote for the tax regime that maximizes their individual earnings or whether they are motivated by other factors; and (2) whether a nudge alters voting behavior. Data are available for votes from each of the five participants from the 285 voting periods, or 1,425 observations. First, *VoteForLVT* indicates whether or not the participant voted for LVT, and this variable can be explained with a series of controls on the type, order effects, and information treatments. Interactions test whether the nudges affect types differently. Second, *ExpectedVote* is an indicator of whether the participant voted in-line with earnings-rationality. If *ExpectedVote* = 1, then the individual voted for LVT or UPT and that was earnings-rational. *Type1* and *Type2* should select LVT, while the others should select UPT. When *ExpectedVote* = 0, then the participant may be following another decision rule that can be picked up by behavioral economics. A model explains *ExpectedVote* with controls on the types, on the round number (*Round* $\in \{3, ..., 8\}$), and on the period number (*Period* $\in \{1, ..., 4\}$). One does not expect the coefficients on these variables to affect *ExpectedVote* because participants ought to be earnings-rational, regardless of round or period. However, any earnings-irrational behavior should occur with *Type2*, *Type3*, and *Type4* because they have the lowest opportunity cost of voting against earnings-rationality. As such, interactions of these types and the nudges may be significant.

1.5 Results

Table 4 shows a quick summary of group level experiment data compared with theoretical predictions at the optimal level, with the first panel showing tax revenue, the second panel showing group earnings, and the third level showing city compactness. Column *LVTPeriods* = 0 shows the average results in rounds conducted entirely under UPT, while *LVTPeriods* = 4 shows the average results in rounds conducted entirely under LVT.

Three-quarters of the independent sessions were conducted with endogenous tax institutions. If majority voting were allowed and all participants voted with earnings-rationality, then all rounds in *TypeDistA* would be in the UPT cell and all rounds in *TypeDistB* and *TypeDistC* would be in the LVT cells. However, institutions were assigned exogenously in rounds 1 and 2. So, 3 of 8 observed LVT rounds for *TypeDistA* were mandated for LVT; similarly, 3 of 10 UPT rounds for *TypeDistB* and 3 of 6 UPT rounds for *TypeDistC* were mandated for UPT. The remaining 5 of 8, 7 of 10, and 3 of 6, respective rounds under earnings-irrational taxes are endogenously selected, i.e., they reflect voting against at least one participant's earnings-rationality. In addition, in three rounds, the participants played one period of LVT (*LVTPeriods* = 1). In total, 18 of 144 rounds (12.5 percent) played more than one period in an endogenous tax institution against predicted voting. About half of deviations went toward UPT (55.5 percent), while the remainder went towards LVT. The deviations could reflect one or more behavioral drivers, such as (1) errors in earnings-rational choice; (2) political/ethical objections; and (3) the impact of nudges.

Another aspect of earnings-irrational voting is the difference with which it is observed across type distributions. *TypeDistC* only deviated in 6.3 percent of rounds in contrast to 16.7 percent for *TypeDistA* and 14.6 percent for *TypeDistB*. A reason for this is the number of *Type1* participants in each type distribution. *Type1* loved LVT and had the largest incentives. *TypeDistC* has three *Type1* participants who can use their majority to force the selection of LVT. In contrast, *TypeDistA* and *TypeDistB* have no majority with an earnings advantage of that magnitude. *Type2* through *Type5* have LVT earnings advantages from -40.83 to +29.19. *Type3* is especially close to indifference with an LVT earnings advantages of -18.81. Thus, one expects more uncertainty in the decision making (lower cost of voting against earnings-rationality) with *TypeDistA* and *TypeDistB*.

1.5.1 Efficiency and Compactness Results

Table 4 presents the predictions and experimental data. First, as predicted by the theoretical model, LVT consistently generated more tax revenue, *GroupEarnings*, and *Citycomp*. Thus, there is support for one part of the efficiency hypothesis and the necessary condition of building an economic background is satisfied. Second, in half of the treatments, experiment results were statistically indistinguishable from the predictions. The insignificant differences can be a result of a small sample because the group level analysis uses aggregate data from a group of five participants. The author sees the insignificance as evidence that participants understood the game well and were following the rules to make optimal decisions accordingly. Third, in the other half of the treatments, experiment results have significant, although small, differences compared to the absolute values of predictions. These differences are results of participants' deviations from the optimal choices and help to explain how the aggregate results change when participants do not behave optimally.

Starting with group earnings shown in the second panel of Table 4, the observed *GroupEarnings* are either significantly lower than the predicted value or have no insignificant difference. This is because the predicted value shows the highest values a group can possibly get and deviation from the optimal choices can only result in lower earnings. Tax revenue and *CityComp* shows deviations in both directions. As the first panel of Table 4 shows, *TypeDistA* averaged slightly more tax revenue than predicted under UPT, while *TypeDistB* and *TypeDistC* averaged slightly less revenue than predicted under LVT. Thus, the point estimates suggest that UPT tends to generate more revenue than predicted.

0 offers a controlled econometric test explaining the drivers of *GroupEarnings*. The model has high explanatory power. *LVTPeriods* has a positive impact, so earnings are higher with more periods under LVT. Starting with LVT does not alter earnings, so there is no anchoring effect. The net effect of LVT is strongly positive; for instance, four periods of LVT would increase earnings by approximately 200 (at point estimates). The impact of the control variables, capturing heterogeneity and other non-treatment impacts, was as expected. The main effects of *TypeDistA* and *TypeDistB* were induced in the model to earn less than *TypeDistC*, the reserved category, and the results show this. The magnitudes of the coefficients are roughly similar to the induced value structure. In addition, *Round* was not significant in the *GroupEarnings* model, suggesting no evidence of order effects. Combined with the small or insignificant difference between experimental results and predictions, the insignificance of *Round* also suggests that there are almost no learning effects in the

location decisions. The author believes that participants understood how to follow the optimal choices well after training and practice rounds. This is important because it differentiates the later voting and information treatments and potential human errors introduced in the experiment basis.

The coefficients on *Vote* suggest that voting increases earnings—probably because it allows a group to select its preferred tax, which generates higher earnings for the majority of the group. For *TypeDistC*, one clearly expects the option to vote would increase group earnings because this distribution has three "LVT lovers," who can control the majority outcome and one marginal "LVT lover." Earnings are higher under LVT for all type distributions, but *TypeDistC* earns considerably more under LVT than the other distributions. Having this voting advantage, 4:1, allows *TypeDistC* to increase earnings by approximately 110 when allowed to vote. The result on *TypeDistB* and voting show that these combined effects tended to produce no extra earnings (*Vote* and *TypeDistB***Vote* approximately cancel each other out). As discussed above, *TypeDistB* should have preferred LVT 3:2 and should have earned more under LVT. However, this distribution was the most likely to select the earnings-irrational tax. *TypeDistA* tended to earn more when allowed to vote. This was surprising because *TypeDistA* was induced to vote for UPT, which would have lower *GroupEarnings*. Data on group voting results showed that this group tended to vote too often for LVT: 3 times when *LVTPeriods* = 1 and 5 times when *LVTPeriods* = 4. This may explain why *TypeDistA***Vote* did not match the hypothesis of a negative effect on earnings.

The third panel of Table 4 compares the experiment results to predictions on the average compactness of the cities, by round. The model predicted that LVT would have cities 3-4 units more compact than the same cities under UPT; however, the experiment results either matched this prediction or exceeded it. Table 4 also shows that in all UPT treatment averages, the city averaged less compactness than predicted. Under LVT, it was less compact in two of three treatments. Together these results suggest a slightly greater tendency for UPT, relative to LVT, to cause sprawl. Comparing the observed outcomes for each type distribution in Table 4 shows that the average point-estimate difference of 3.46-4.75 in City*Comp* from the tax treatments was roughly the same magnitude as predicted. In *TypeDistC*, LVT produced a greater *CityComp* advantage than expected (*TypeDistC*) because the average *CityComp* under LVT was statistically indistinguishable from the prediction, while the average *CityComp* under UPT was statistically less than the prediction.

As with the efficiency results, these observed differences in behavior from the type distributions likely reflect differences in the magnitude of incentives; *TypeDistC* has three *Type1* participants and two marginal types (*Type2* and *Type3*), so a majority have the clearest incentives about where to build and what tax plan to select. In contrast, *TypeDistB* has only two *Type1* participants and *TypeDistA* has only one *Type1*. Collectively, the *TypeDistA* and *TypeDistB* sessions are more likely to face uncertainty in their decision making and, thus, one expects the resulting average compactness to be less in line with predictions than *TypeDistC* sessions. A more complete causal explanation of these differences is shown in the regression results that explain *CityComp*.

The second column of Table 5 presents a regression that explains *CityComp*. As with group earnings, the coefficients on the type distribution controls tend to be significant while *Round* is not, but the anomaly is *TypeDistB*. A reason why the

TypeDistB coefficient lacks significance may be that (as in Table 4) a large number (N=10) of 48 rounds were playing in UPT, with 7 selected by a city against the majority interest. Table 5 shows that LVT leads to more compactness because an additional LVT period (*LVTPeriods*) leads to approximately 1 point of *Comp*, which matches the theoretical prediction in Table 2.[7] The interaction terms, including nudges, showed no impact. Voting has a modest effect on compactness ($\beta^{Vote} = 0.53$).

The collective results (Table 4 and Table 5) on compactness suggest that participants were either making decisions that resulted in as much or less compactness than predicted; there was no evidence of behavior leading systematically and substantively to more compactness than predicted. There were several situations most likely to produce predicted compactness. First, when the land characteristics have more "LVT lover" types. These settings would be more likely to observe compact building patterns and this tendency would be higher under LVT. Second, the results suggest that the nudges do not lead to more compactness, even though the second nudges advocated compact brick placement. Third, LVT tended to produce more compactness than UPT at levels predicted or at levels slightly exceeding predictions. This provides some experimental evidence to the longstanding claim that LVT does indeed reduce sprawl, and the behavioral evidence suggests that the impact may even be larger than expected.

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[7] The prediction (from Table 2) was, on average, LVT would lead *TypeDistA* to 3 more *CityComp* points, *TypeDistB* to 3.9 more *CityComp* points, and *TypeDistC* to 4.1 more *CityComp* points. The coefficients show a similar treatment effect, where four LVT periods lead to roughly 3.42 *Comp* points for *TypeDistA* ($4\beta^{LVTPeriods} + \beta^{TypeDistA}$) and about 4 for *TypeDistB* ($4\beta^{LVTPeriods} + \beta^{TypeDistB}$) and *TypeDistC* ($4\beta^{LVTPeriods}$).

1.5.2 Group Voting Behavior and Nudges

Table 6 presents the group voting data averaged by treatment and the predicted votes (2 votes for *TypeDistA*, 3 for *TypeDistB*, and 4 for *TypeDistC*). In the no-information treatments, votes for LVT were statistically indistinguishable from predictions for all three type distributions. However, there was some evidence that nudges affected votes in the information treatments. Surprisingly, for *TypeDistA*, two nudges (*PosInfo1* and *NegInfo1*) statistically lowered votes for LVT. It is not clear why the first nudges lowered *LVTVotes*, but the second nudge statistically raised the votes back to the predicted level. Thus, there is some evidence that the cumulative nudges worked in the sense that they overcame the negative effect of the first nudges. The unexpected effect of the first nudges might be associated with the type distribution, where LVT lovers are in the minority.

The results were considerably different for *TypeDistB* and *TypeDistC*. Voting patterns largely matched predictions; votes for LVT under 7 of 8 information treatments increased statistically above the predicted level. The substantive impact was important, too, averaging about 0.5 more votes out of five above the predicted level. The average number of votes, though, did not tend to increase between the first and second nudge—only in *NegInfo2* with *TypeDistC* did the average votes statistically increase after the first nudge. *TypeDistB* and *TypeDistC* were "over responsive" to the nudges. But the predictions were that the groups would already select LVT when voting was available, so the extra LVT votes do not affect efficiency and compactness. In sum, evidence shows that some of the participants voted in favor of LVT. Participants tended to vote in line with their incentives (a majority in favor of

LVT in two of three type distributions), and their votes in favor of LVT tended to exceed expectations when using the nudges.

Table 7 offers two regressions to explain *LVTVotes*. Model 1 implies that all nudges raise votes for each type distribution.[8] Model 2 shows that the effects of nudges are more nuanced, however, so the remaining discussion focuses on model 2. The results suggest a substantial anchoring effect ($\beta^{LVTStart} = 0.82$), whereby almost 1 of 5 possible votes can be expected when beginning a round with LVT. This is surprising in that the induced values do not predict any anchoring, but it is a common behavioral effect observed in experiments. There also is a counterbalancing order effect ($\beta^{Round} = -0.17$), whereby fewer LVT votes are found as rounds increase and participants gain experience.[9] The coefficients on the type distributions match expectations. The model predicted one extra vote from using *TypeDistB* rather than *TypeDistA* and, though $\beta^{TypeDistB} = 0.64$ is less than one, the interactions account for more of the difference. Similarly, *TypeDistC* generates almost two more LVT votes, as predicted. The nudges largely worked as expected, raising *LVTVotes*. The first positive nudge only increased votes among *TypeDistB* and *TypeDistC*. The first

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[8] The main-effects coefficients in model 1 have some overlapping confidence intervals. A Wald test shows that the coefficients on *PosInfo1* and *NegInfo1* are statistically indistinguishable (p=0.41). Also, *PosInfo2* and *NegInfo2* are indistinguishable (p=0.57). Thus, the research did not identify that one framing (positive or negative) would be more effective in promoting LVT. However, differences were found in the cumulative effects. Thus, the second nudge creates a larger increase in LVT votes than the first ($\beta^{PosInfo2} > \beta^{PosInfo1}$ with p=0.10 and $\beta^{NegInfo2} > \beta^{NegInfo1}$ with p=0.01). This result was expected because there was an anticipated cumulative effect. Similarly, the effect of the second information treatment of one type (positive or negative) was larger than the first information treatment of the other type: ($\beta^{PosInfo2} > \beta^{NegInfo1}$ with p=0.02 and $\beta^{NegInfo2} > \beta^{PosInfo1}$ with p=0.06). Another unreported analysis shows that *LVTVotes* did not tend to decline differently over time with the type distributions. This cannot be shown in the regression because of multicollinearity.

[9] The participants can start to learn about the type distribution starting in round 3 because the voting results are revealed on the second screen in the period.

negative nudge only increased votes among *TypeDistB*. It is not surprising that the first nudge worked more effectively on *TypeDistB* and *TypeDistC* because these distributions had the most LVT lovers. The second nudges increased *LVTVotes* for all types, and the effect was even greater for *TypeDistB*. This cumulative effect was expected. However, the magnitudes of the nudge effects are surprising. The first positive nudge produced a point estimate of 0.76-0.78 more *LVTVotes* out of 5 for *TypeDistB* and *TypeDistC*. The second positive nudge produced another 0.72 more *LVTVotes* for *TypeDistA* and *TypeDistC* and 1.21 more *LVTVotes* for *TypeDistB*. The negative nudges produced a similar pattern of effects. These large substantive effects were surprising given that the nudges did not alter earnings-rationality and they did not tend to change group-level efficiency or compactness.

Collectively, the voting results show that groups favor LVT only when the underlying incentive structure has a majority of LVT lovers (*TypeDistB* and *TypeDistC*). In other words, LVT was preordained to be the most efficient for the "city," and yet the results suggest that groups only select when the majority of participants win from LVT. There is no evidence of a universal objection to LVT. Without nudges, groups vote with earnings-rationality rather than equity. This first result implies that efforts to promote LVT require a greater understanding of the heterogeneity of "types" among landowners. A second result, however, is that nudges can increase LVT support. When the majority favors LVT, nudges increase group votes for LVT even higher than expected. Some participants who should have supported UPT had a systematic tendency to vote against earnings-rationality and in favor of LVT when the nudge was used. The nudges further interacted with the distribution of types. When the LVT lovers were a majority, the nudge on average pushed one UPT lover to vote against earnings-rationality about half the time. This was a surprising result, which may arise from psychological processing of conforming to social norms and minority status. As before, this suggests efforts to promote nudges may be best targeted in situations where the majority would “win” from LVT. The nudges systematically increased votes for LVT, as was seen in table 5 on group voting. Further, the second nudge had a larger effect than the first, which probably reflects the cumulative impact of the messaging rather than the framing.

1.5.3 Individual Voting Behavior and Nudges

Individual data reveal more about voting against earnings-rationality and responsiveness to nudges. Table 8 presents the *VoteForLVT* regression of 1,425 observations on whether a participant voted for LVT. *LVTStart* increases the likelihood of an LVT vote by 20 percentage points, but this anchor was counterbalanced by an experience tendency; all else equal, each additional round increases the probability of voting for UPT by 4 percentage points. The main effects on the individual type controls indicate the likelihood of an LVT vote in a no-information voting treatment. Not surprisingly, *Type1* is the most likely to vote for LVT. The earnings-rationality incentive was less strong for *Type2*, who had a lower tendency to vote for LVT relative to *Type1* by 11 percentage points. The intuition for this effect is that, although *Type2* always “wins” under LVT, it does not win as much as *Type1* and thus has a lower opportunity cost of earnings-irrationality. *Type3*, *Type4*, and *Type5* were 63 to 77 percentage points less likely to vote for LVT than *Type1*, but the prediction was 100 percent. So, these UPT-loving types are revealing a substantive tendency to support LVT against earnings-rationality.

The analysis of the individual votes show some interesting results on nudges. First, the main-effects coefficients the nudges worked to increase LVT votes. The first nudge on average increases the probability of an LVT vote by 14 to 15 percentage points, while the second increases it by an additional 21-24 percentage points. These main effects are robust across the first three types. The interactions of type and nudge further show different tendencies among *Type4* and *Type5*. *PosInfo1* has a weaker effect on *Type4*, and *PosInfo2*, *NegInfo1*, and *NegInfo2* have weaker effects on *Type5*. The intuition, once again, relates to the opportunity cost of voting against earnings-rationality. *Type2* and *Type3* are close to indifference, so the nudges are more likely to promote LVT to these types while *Type4* and *Type5* are less responsive. Overall, all types respond to nudges. If social planners seek to promote LVT for efficiency reasons, then they should target nudges at voters on the margin of LVT support (*Type2* and *Type3*). Each nudge increases the probability of an LVT vote by 14-24 percentage points.

Table 8 also offers a complementary regression to explore earnings-rational voting directly with *ExpectedVote*. The model has a low explanatory power, which actually indicates high-quality data because there should be few systematic patterns to earnings-irrational choice, i.e., deviations should be largely random. The *Round* coefficient suggests experience leads to a slightly greater likelihood of a rational choice—approximately 4 percentage points more per round played. However, there is a counter effect, where within each round, each additional period played leads to 5 percentage points less likelihood of a rational choice. This result could be because of fatigue, boredom, or an urge to try out different votes as the periods increased. These two effects were statistically significant, and it is important to control for them, but they do not invalidate the other results. The most important result in the *ExpectedVote* model is that *Type2* and *Type3* have a 13-15 percent statistical tendency to deviate from earnings-rational choice. As discussed above, *Type2* and *Type3* are closest to indifference between LVT and UPT, so they face the lowest cost to deviation. Second, the interactions show that *PosInfo1* and *NegInfo2* reduce the likelihood that *Type3* makes a rational choice, while *NegInfo1* and *NegInfo2* reduce the likelihood that *Type4* makes a rational choice; both of these messages thereby promote LVT. These interactions make sense because *Type3* and *Type4* were the two UPT-lovers closest to indifference.

Collectively, the individual voting results show that the participants with the most extreme types (*Type1* who loves LVT and *Type5* who loves UPT) tended to vote with earnings-rationality. However, the marginal types showed some systematic tendencies to vote against individual earnings—even when not treated with a nudge—and this is especially important for *Type3* and *Type4* because they individually prefer UPT, though LVT makes society wealthier. Thus, swaying *Type3* and *Type4* is the key to maximizing social efficiency. The evidence further showed that most nudges increased LVT votes for most of the types, and the second nudge had a larger effect than the first. Few differences were found between positive and negative framing, however; The negative framings caused three statistical divergencies from earnings-rationality among *Type3* and *Type4,* but the positive framing only affected *Type3*. That said, the impact of these results is tempered by the raw data on the group voting that suggested the nudges did not systematically alter group voting in *TypeDistA* (see Table 6) when UPT was preferred by a majority. In other words, the nudges work, but systematically in settings where the majority was already incentivized to support LVT.

This is important because other researchers expressed concern that even when LVT is best for society, voters reject it. Thus, nudges can potentially help voters make the optimal decision.

1.6 Policy Implications and Conclusion

This induced-value experiment explored the political economy of land tax institutions, particularly how those taxes affect efficiency and sprawl when endogenously selected. Using a partial measurement of efficiency from the perspective of landowners' earnings and the cities' tax revenue, the experiment results on LVT's advantage on efficiency and compactness closely match predictions, but the deviations also show the benefits of using behavioral economics. Although participant choices lead to slightly less efficiency and compactness than predicted, the results thus offer evidence that LVT leads to more efficiency and less sprawl.

The most interesting results relate to endogenous institutions and nudges. The group voting results showed that without a nudge, voting tended to be earnings-rational. This included *TypeDistA*, which was induced to support UPT against social efficiency. Thus, there was little evidence of an earnings-irrational, equity-based rejection of LVT at the group level. However, the individual voting data on the no-information treatments were more nuanced. There are slight tendencies of 13-15 percent among those on the margin (*Type2* and *Type3*) to vote against earnings-rationality. If one looks only at *Type2*, then the experimental evidence would indeed suggest an ethical objection to LVT as suggested in the literature. However, the entirety of the behavioral evidence shows that there are tendencies to deviate from earnings-rationality—tendencies that increase as the opportunity cost of earnings-irrationality falls. Therefore, LVT promoters may benefit from anticipating a modest, substantive level of political objections—particularly as UPT is the status quo in most U.S. jurisdictions and therefore LVT acceptance will require a number of marginal LVT lovers to understand that they are truly better off under a new tax institution.

The lack of systematic tendencies in the no-information voting treatments is desirable from an experimental standpoint because it allows a purer test of the nudge treatments. The researchers developed two nudges and framed them either positively (in favor of LVT) or negatively (against UPT) in a between-subjects design. The group-level voting results were partially negative. When a majority of the group prefers UPT, the nudge did not tend to switch the majority to favor LVT. However, when the majority favored LVT, the nudge enhanced that majority beyond predictions. Thus, LVT implementation depends on the relative size of the LVT-winning group. The individual-level analysis of voting offers stronger evidence about how the nudges worked. The nudges tend to increase LVT votes overall and, especially, among the two types (*Type2* and *Type3*) on the margin of preference between LVT and UPT. These two types are the key "swing votes" in the sense that they can determine the selected tax and they face the lowest cost from earnings-irrational voting. The nudges even encouraged *Type4* to select LVT more frequently. So, this evidence is promising in suggesting that education efforts on LVT are likely to pay off with some types. Nudges help the marginal LVT lovers make an earnings-rational decision and the marginal UPT lovers make an earnings-irrational decision; collectively these two decisions will tend to promote social efficiency.

The collective voting results, therefore, are mixed about LVT promotion. On the negative side, the nudges do not have a robust effect that consistently switches a group vote among a UPT majority to be in favor of LVT. Also, the nudges have a relatively small overall substantive effect on the probability of LVT support. On the positive side, the results suggest that the nudges work as expected in several situations; they are especially effective at increasing support for LVT when the majority would benefit from LVT. This result suggests that the nudges can be politically useful in situations where LVT is relatively favored. Also, the experiment finds some evidence that there is a small tendency for nudges to alter votes among UPT-lovers on the margin. Thus, there ought to be some situations where nudges "swing" votes to LVT among a group that would otherwise support UPT.

Obviously, this experiment is built upon a highly stylized model, but it does mimic some fundamental processes at work in actual land markets. The most extreme assumption in this model is that decisions made by one individual lead to independent land-value capitalization. This assumption was pragmatic—to make the model more understandable to the participants and ensure that the researchers could solve for behavioral predictions. Other assumptions are less severe, such as the model parameterization where LVT would be more efficient and lead to less sprawl than UPT. The literature supports this approach, and it is difficult to conceptualize a model of this type where LVT would lead to less efficiency and more sprawl. Nevertheless, the magnitude of the efficiency and compactness advantages can be adjusted with the parameterization; therefore, the large efficiency and compactness advantages[10] found

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[10] The results showed that groups will be unlikely to reach the efficient frontier. Small deviations from optimal choice are going to be made either by mistake or for some non-earnings reason. As such, the optimum will not tend to be achieved, and yet the results suggest no systematic pattern whereby the deviations are larger in LVT than in UPT. Moreover, there was evidence that in some situations UPT may even have a systematic tendency to deliver more-than-expected sprawl. This result means that policy makers gain some confidence, though not certainty, that LVT will deliver on the promises of efficiency and compactness—as long as the underlying distributions of types in the real world match the setting developed here.

for LVT are not the primary messages. Instead, the model allowed the researchers to test, first, whether the efficiency and compactness advantages would be as great or greater than predictions when using a behavioral economics setting—a setting that allowed for patterns of behavior that do not perfectly align with earnings-rationality. Second, the setting allows the researchers to explore how group heterogeneity affects endogenous institutional choice with and without nudges.

How does group heterogeneity relate to the real world? Consider one case: The majority of landowners in a jurisdiction tend to have preferences that match *Type1* and *Type2*, i.e., the owners currently have a relative skill in intensively developing land. This might be a situation where agglomeration economies for intensive development exist. The evidence herein suggests that LVT is the most efficient tax institution. There will be large efficiency and sprawl cost of using UPT. Voting for the jurisdiction's preferred tax will tend to lead to LVT—as long as the owners understand their earnings under both taxes—and nudges can help increase this support for LVT, on the margin. One possibility for future research is to examine what would happen to voting if residents had an incomplete understanding of their future earnings under the alternative tax regime, which would typically be LVT as UPT is the status quo tax. Such research could further explore whether public-good type efforts to provide this information helps groups see the advantage of LVT.

Another case might be one in which a city is distressed with very little remaining economic activity in the CBD and more activity sprawling into the suburban communities. This may be a market signal that the region has a comparative disadvantage in intensive development. In terms of this paper's model, this situation might match *TypeDistA* where LVT would create more "losers" than "winners" but the overall effect would be efficient. If LVT were proposed, there likely would be substantial opposition. Voting may not deliver LVT, and nudges may not work to encourage this community to adopt LVT. A further concern, but one that goes beyond the model, is that there may be some communities where LVT is not the most efficient tax institution. If a community has a dramatic comparative disadvantage in intensive development, then LVT possibly would even fail to promote efficiency. There is probably some lesson in the pattern of status-quo density for one to infer the relative advantage/disadvantage the community would have in further promotion of intensive land use.

A final case may be a city that is already densely developed with vibrant economic activity. Presumably, this signals the profitability of intensity, so LVT could potentially enhance this profitability further. The already-dense city could resist the urge to sprawl and develop greater density. Voting should lead to LVT, and the nudges would help reach this efficient outcome.

A limitation of this study is that the experimental evidence probably is quite sensitive to assumptions about the land-market structure. If land market decisions do not capitalize independently, is a systematic tendency introduced that invalidates these results? Agricultural land ought to be largely independent of improvements in the sense that the price when agriculture is a highest-and-best-use derives from the commodities produced rather than a neighbor’s investment. In urban settings, there are pervasive interdependencies, such as economies of agglomeration, and land values are constantly adjusting to changes in economic activity among proximate parcels. That said, it would be incorrect to interpret this paper’s results as speaking against LVT. The model was built without a negative capitalization externality, and it did not allow for a corresponding increase in the profitability of the buildings from adjacent development. Thus, the improvement values are artificially constrained to be fixed to make the experimental conditions understandable to participants.

Chapter 2

EVALUATING PES CONTRACT COST-EFFECTIVENESS, ADDITIONALITY, AND FLEXIBILITY: EVIDENCE FROM LABORATORY EXPERIMENTS

2.1 Introduction

Two problems for social planners running purchase of ecosystem services (PES) programs are how to best handle heterogeneous benefits and costs from different projects and how to best address hidden information on farmers' willingness-to-accept (WTA). Social planners use different contracting mechanisms to seek, with a limited budget, how to select the "right" projects with minimum payments, i.e., payments that just cover a farmer's minimum WTA, which is molded and phrased as net private cost (NPC) in this study. A commonly used mechanism, fixed payments (FP), which offers uniform payments to farmers, may be easy to administer but unavoidably transfer public funding to low NPC farmers as information rents. Microeconomic theory on information asymmetry suggests that reverse auctions (RA), in which farmers compete to obtain payments by offering different prices, could not only increase contract performance by reducing information rents, but also offers social planners the power to set selection rules with specific goals (such as maximize social welfare, enrollment, or environment benefits). Empirical studies (Stoneham et al. 2003; Horowitz et al. 2009) and laboratory researches (Hailu and Schilizzi 2004;

Schilizzi and Latacz-Lohmann 2007; Glebe 2008; Arnold et al. 2013) show that reverse auctions could increase the performance, measured by cost-effectiveness, of PES programs under certain circumstances. Reverse auctions could be interpreted as a more flexible contract mechanism compare to fixed payments. However, the term flexibility (defined later) conveys more meanings and this research uses lab experiment to investigate PES program performance, measured by different criteria (defined later), under different levels of flexibility.

To avoid ambiguity and indeterminateness in reasoning, this research first discusses the term – flexibility, then enumerates and reviews related studies under the context of PES programs. Flexibility generally means the ability to make changes or to deal with changing or different situation. Flexibility in contracting, when approached from a legal perspective, is deemed to be suspicious because it may undermine the authority and normality of contracts. However, when approached from a business-oriented perspective, flexibility is viewed as an essential attribute (Soili et al. 2015). Under the context of PES programs, with terms borrowed from Schilizzi (2017), flexibility can be considered as a “causal factor” which affects the “outcome effects” of PES contracts. Although Schilizzi (2017) did not define the term “flexibility” specifically, he agreed that flexibility related implementation rules, such as single auction vs. repeated auctions, single bidder vs. group bidders, and single item vs. multiple items, would affect the performance of PES programs, especially, of those fulfilled by reverse auctions. Iftekhar et al. (2012) argued that combinatorial auctions, which allow for simultaneous submissions of bids on combinations of projects, as a flexible version of simple auctions, could exploit spatial synergies and improve program performance. This is supported by an experiment by Said and Thoyer (2007), who found that allowing combinatorial bidding enhanced the allocation efficiency of reverse auctions. Fooks et al. (2015) found that a more flexible and dynamic auction setting leads to higher overall social benefits as well as lower offers from participants. On the contrary, Iftekhar and Tisdell (2014) found that a more flexible, multiple offers auction not only increases rent extraction but also generates fewer benefits compared to a single offer auction. Another way to study flexibility is through transaction costs. A more flexible program may have one effect that lowers transaction costs (easier for participants to accept or adopt a familiar practice) and possibly, another adverse effect that raises transaction costs (more time and effort to study and choose among different practices). Either way, Palm-Foster et al. (2016) showed that auction performance will be affected by transaction costs. Thus, researchers have not reached an agreement on whether increasing program flexibility improves PES program performance—in part because flexibility could be defined in different ways. This study approaches flexibility from a more "business-oriented" perspective, which treats flexibility as an attribute affecting the performance of PES contracts. Further, the author defines flexibility specifically as the number of different PES options available to participants, i.e., fewer options as stringent cases while more options as flexible cases.

With the definition above, flexibility, as a causal factor, affects the performance of PES contracts through not only the mechanism itself but also through participants' behaviors. Farmers' participation decisions and offering behaviors as the "intermediate outcome" (Schilizzi 2017) determine how the mechanism works. In addition to lab research results, empirical studies provided evidence on the differential behaviors and showed that farmers prefer more flexible programs and require greater financial incentives under less flexible situations (Ruto and Garrod 2009; Espinosa-

Goded et al. 2010; Christensen et al. 2011). This result can be decomposed into two levels and inspires two questions. First, in terms of participation and enrollment, will a rational participant be more likely to join a PES program if the program is more flexible? This has not been tested in lab experiments because most experiments focus on participants' offer behaviors after participation. This research goes one step further to investigate subjects' participation decisions before making offers. Second, in terms of offer behaviors, will a more flexible program dampen participants' rent-seeking behaviors because of the freedom to choose their preferred options or promote participants' greed to make use of the mechanism and maximize private benefits? Either case could happen, and experiment results could reveal which case dominates.

Either through effects on participation decisions or through offer behaviors, flexibility will affect the performance of the PES programs measured by different criteria. As Schilizzi (2017) pointed out, auction performance can be ranked differently and "performance trade-offs" should be considered when multiple criteria are used. With different criteria available, it is important to select certain criteria because they reflect the social planners' goal and, essentially, provide a consistent way to compare alternative programs. Whitten et al. (2017) summarized twelve lessons in designing different criteria and pointed out that a good rule of thumb was to consider both benefit and cost heterogeneity. Duke et al. (2013) showed how PES programs suffered when all commensurately measured benefit and cost data were not considered. A summary of some commonly used criteria in Table 9 shows that most researchers focus on budgetary cost-effectiveness (Schilizzi and Latacz-Lohmann 2012; Messer et al. 2017) instead of efficiency because full efficiency is generally precluded by limited budget (Duke et al., 2013).

In addition to the criteria showed in Table 9, another key metric to evaluate PES program performance, additionality, has been urged by some researchers (Kurkalova et al. 2006; Engel et al. 2008; Maron et al. 2013). Kurkalova et al. (2006) empirically addressed the severity of the non-additionality problem with a finding that 86% of PES payments went to existing BMP adopters under a fixed payment mechanism, which is a loss of public funding. Although additionality has been studied in some research areas such as biodiversity conservation (Ferraro and Pattanayak 2006; Maron et al. 2013), water quality trading (Duke et al. 2014), and conservation tillage (Kurkalova et al. 2006), it is less investigated in PES auctions. As stated by Duke et al. (2014), "Additionality is the idea that an ecosystem service from a management practice currently is not provided or would not have been provided in the absence of a new policy institution seeking to increase service provision. Non-additionality, then, will be defined as an ecosystem service provided prior to the policy, but that is claimed to be an environmental improvement outcome of the policy." Non-additionality can be explained by an adverse selection (hidden information) model, where farmers with negative NPC claim payments from the PES program. Using laboratory experiments, Arnold et al. (2013) showed that reverse auctions outperform fixed payments but were less efficient than a screening mechanism because those "wrong" or non-additional participants were adversely selected. In a lab experiment, the non-additional practices or farmers could be modeled as those with negative NPC. Almost all existing auction experiments assume positive NPC to avoid the potential complication of non-additionality. This may lead to an over-estimation of the benefits acquires by the PES programs because the environmental benefits from non-additional practices should have been achieved even without any extra payments.

This work addresses the potential trade-off between flexibility and additionality in PES programs. Although prior empirical researches show that farmers prefer more flexible programs, allowing high levels of flexibility may not increase the performance of PES programs as expected because of non-additionality. The rationale behind this argument is that when farmers are given the flexibility to choose among multiple practices, rent-seeking behaviors may lead them to choose non-additional practices (options with negative NPC in this study). In that case, a large portion of the social planners' budget goes to information rents instead of covering the costs of additional projects. To test the performance of different mechanisms with varying levels of flexibility, the author uses a lab experiment with different levels of flexibility and heterogeneous NPC (both negative and positive). By controlling the number of options available to participants, the experiment controls not only competition intensity (Schilizzi 2017), but also flexibility. Therefore, as the first lab experiment with negative NPC in auctions, this paper will contribute to the existing knowledge by testing hypotheses on how flexibility affects the performance of fixed payments and reverse auctions when additionality is considered. Table 10 provides a summary of hypotheses.

2.2 Theoretical model

Assume six types of heterogeneous farmers denoted by i, $i \in \{1, \ldots, 6\}$. Each farmer has ten options available denoted by j, $j \in \{1, \ldots, 9, 10\}$, where option 1 to option 9 stand for nine specific PES practices, and option 10 stands for status quo or no adoption of any practices. Each option comes with a specific net private cost

(NPC_{ij}), which is the difference between private cost (PC_{ij}) and private benefit (PB_{ij}), i.e., $NPC_{ij} = PC_{ij} – PB_{ij}$. NPC_{ij} is positive for most *i*, *j* combinations and stands for the minimum payments required to induce adoption. NPC_{ij} could be negative, or in other words, some practices could be privately beneficial to farmers. A rational farmer supposes to adopt a negative NPC option even no extra payments are offered, as the case of self-adoption. Choosing a negative NPC option can also be interpreted as a non-additional behavior when the farmer gets paid from the PES program. All the PES practices are assumed to have a positive external benefit (EB) and EB_{ij} is assumed to be known by the social planner as public information. NPC_{ij} is assumed to be private information to the farmers, but the distribution (average) of NPC_{ij} for certain option *j* is known to the social planner. Figure 2 shows the decisions and interactions between the social planer and a representative farmer, where the social planner uses either fixed payments or reverse auctions to induce farmers' adoption of PES practices to provide environmental benefits.

2.2.1 Farmers' decision under fixed payments

Under the fixed payment (FP) scheme, a social planner offers a flat rate payment, *Pay*, to eligible option(s). The payments are the same for all the eligible options across all farmers. Use dummy variable e_j to stand for the eligibility of option *j* (e_j = 1 if option *j* is eligible for payments, and 0 otherwise). Facing the fixed payments, a rational farmer *i* will then choose an option *j* to maximize net benefit.

$$\max_{z_{ij}}: \sum_{j=1}^{J} z_{ij}\left(e_j * Pay - NPC_{ij}\right) \tag{2.1}$$

$$\text{s.t.}: \sum_{j=1}^{J} z_{ij} = 1$$

where $z_{ij} = 1$ if farmer *i* choose option *j*. The objective function of equation (2.1) represents farmer *i*'s pursuit for maximizing net benefits or earnings as the difference between fixed payments and net cost. The constraint of equation (2.1) indicates that the farmer can choose one and only one practice.

2.2.2 Farmers' decision under discriminatory price auction

Under reverse auctions, and more specifically, discriminatory price auctions (DPA), the social planner asks farmers to make offers for specific options, denoted by $offer_{ij}$, and selects farmers with the lowest unit offer b_{ij} (offer per unit of the external benefit calculated as $\frac{offer_{ij}}{EB_{ij}}$) first until when the budget is exhausted. This selection rule based on the unit offers instead of overall offers ensures cost-effectiveness at the unit level. A rational farmer *i* will choose an option j and make an offer $offer_{ij}$ for that option to maximize expected net benefits.

$$\max_{z_{ij},b_{ij}}: \sum_{j=1}^{J} z_{ij} \left[\left(e_j * offer_{ij} - PNC_{ij}\right) P\left(e_j * \frac{offer_{ij}}{EB_{ij}} \leq \beta\right) + PNC_0 \left(1 - P\left(e_j * \frac{offer_{ij}}{EB_{ij}} \leq \beta\right)\right) \right] \tag{2.2}$$

where β is the last accepted unit offer and $P\left(e_j * \frac{offer_{ij}}{EB_{ij}} \leq \beta\right)$ stands for the probability that farmer *i*'s offer for option *j* is accepted by the social planner. β is determined jontly by the social planner's budget and offers from all farmers. The objective equation (2.2) above extends the theoretical analysis of reverse auctions by Latacz-Lohman and Hamsvoort (1997) in two ways. First, this model considers a selection rule based on the unit offer instead of the overall offer for a conservation practice. This extension can potentially increase the cost-effectiveness of the auction when the outcome is evaluated by total EB generated for a given budget. Second, this model extends farmers' choice from one practice or option to multiple options. In this case of multiple options, famers will need to consider a new tradeoff among the options (discussed later). Normalize $PNC_0 = 0$ (the net cost is 0 if the offer is rejected and the farmer does not adopt any PES) to get:

$$\max_{z_{ij},b_{ij}}: \sum_{j=1}^{J} z_{ij} \left[\left(e_j * offer_{ij} - PNC_{ij}\right) P(e_j * \frac{offer_{ij}}{EB_{ij}} \leq \beta) \right] \qquad (2.3)$$

$$\text{s.t.}: \sum_{j=1}^{J} z_{ij} = 1$$

Equation (2.3) shows the first tradeoff between offer and the probability of winning. A higher offer for a certain option increases the earnings of the farmers ($offer_{ij} - PNC_{ij}$) if they win the auction, but a higher offer also reduces the probability of winning ($P(e_j * \frac{offer_{ij}}{EB_{ij}} \leq \beta)$) conditional on the external benefits of the option (EB_{ij}). The second tradeoff involves the choice among options. With the simple case of one option, a farmer will only need to consider making a "wise" offer for it. However, with multiple options available, a farmer will need to compare the possibility of winning and earnings for multiple options.

Not all options are eligible to enter the auction ($e_j = 1$ for eligible option j), but the farmers can still choose an ineligible option as self-adoption. In that case, $e_j = 0$ and $P\left(e_j * \frac{offer_{ij}}{EB_{ij}} \leq \beta\right) = 1$. It represents the case when a farmer is willing to adopt a practice even though there are no payments. This could happen to those farmers who have negative *NPC* options, and this is important for the purpose of modeling the case of the early adopters or self-adoptions.

Latacz-Lohmann and Van der Hamsvoort (1997) provide an optimal solution based on assumptions of bidder's expectation. However, this work is not aimed at solving the optimal bidding strategy theoretically but at guiding the following design and implementation of the experiment.

2.3 Experiment Design

The experiment sessions, approximately ninety minutes each, were conducted with 132 undergraduate participants (mostly in business and economics majors) at the University of Delaware Center for Experimental and Applied Economics. Participants were randomly assigned to one of eleven sessions with two groups in a session and six participants in each group. Decisions were made on individual tablet computers with private screens linked to an administrator computer using z-Tree software (Fischbacher 2007). The University of Delaware Institutional Review Board approved the protocol and informed consent was obtained.

Participants completed informed consent, read overview instructions, watched an instructional presentation, and asked questions. For each treatment (defined later), separate instructions were distributed, and participants were trained over a practice round before they make any decisions. At the end of the experiment, participants completed a survey and were paid based on their choices. "Experiment dollars" are used as tokens of monetary earnings during the experiment and were converted to U.S.

dollars at a rate of 450: $1. Participants' earnings varied by the induced values and were $18 on average.

The neutral framing design assigned participants as sellers who produce and sell a simplified "Product A" in each round, which they always sold to get base payments of 360 experiment dollars (this is used to prevent a bankrupt situation in case that participants make irrational decisions or mistakes and lose money from the experiment). Participant i was asked to add a specific option j (1 to 10) to Product A with a cost of NPC_{ij}, which will be paid by the buyer either through fixed payments or reverse auctions. Participants could always see the ten options with corresponding NPC and EB no matter which options were eligible for payment.

Table 11 summarizes the treatments. Three flexibility levels, i.e., one-paid, three-paid, and nine-paid options, combined with two payment mechanisms, i.e., fixed payments and reverse auctions, give a total of six combinations (denoted by FP1, FP3, FP9; DPA1, DPA3, DPA9) and each of them is repeated three rounds. One round of No-Payment (NP) was conducted at the beginning of each session as the control. This is a within-subject design that all subjects went through nineteen rounds and made decisions under the seven treatments above. The NP round stands for a situation where no incentive is offered. Although the result seems straightforward, it is necessary to start from it, not only for comparison but also to get a salient response with incentives introduced later. Under fixed payments (FP1, FP3, and FP9), tablet computers calculated the potential earnings as extra payments (0 if not eligible) minus NPC. Participants were asked to choose an option based on given information. They were specifically informed that their earnings would be higher if they chose the option with higher potential earnings. As mentioned above, this study interprets the number of paid options as different levels of flexibility. The ten options are always shown to participants but only a randomly selected subset of them are paid, i.e. one randomly selected option is offered an extra payment (if this option is chosen by the participant) under FP1, three under FP3, and nine under FP9. If participants chose an eligible option, their earnings would be calculated as base payments plus extra payments minus NPC. Otherwise, their earnings would be base payments minus NPC.

Under reverse auctions, participants were asked to choose an option first. If participants chose an eligible option, they would need to make an offer for it (they were given one chance to change their offer). If they chose an ineligible option, they did not need to make an offer and their earnings would be calculated as base payments minus NPC. This mimics a self-adoption case when a farmer chooses to adopt a practice without entering the auction. Tablet computers calculated then ranked unit-offers (calculated as offer divided by EB) and selected the lowest one first until the budget was not enough to pay the next offer. Unit-costs (calculated as NPC divided by EB) were shown in addition to NPC and EB for each option to help participants choosing among different options. The budget was determined endogenously and was also shown to participants when they made decisions.

The nineteen rounds in the eleven sessions resulted in 404[11] group level observations. There were 418 observations originally, but fourteen of them were lost because of program malfunction. To prevent a potential order effect, six sessions were conducted with the order of fixed payments first, then reverse auctions, and the other five sessions were conducted the other way around.

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[11] The group level data include twenty-two observations for NP; sixty-six for each of FP1, FP3 and FP9; sixty for DPA1; sixty for DPA3; sixty-four for DPA9

2.4 Results

The primary interest of this research is to test how flexibility, which is interpreted and modeled as the number of options eligible for payments, affects PES program performance. The treatment effects work through both the group level and individual level. The direct effect comes at the group level from different mechanisms, which select program participants and pays them differently. At the individual level, the treatments may have an intermediate effect because individuals' behaviors change according to different mechanisms, which will contribute to the group level outcomes. The direct effect and the intermediate effects combined lead to the final outcome. The final impact at the group level is reported and discussed first, and then a detailed analysis of individual behaviors and choices is presented.

2.4.1 Group level data analysis

Figure 3 summarizes the external benefits (EB) acquired under fixed payments and reverse auctions when only one option is given to participants. Each group replicates a simple comparison of fixed payments and reverse auctions. Although the author did not have all data completely for the 9 options because of the randomization process (reverse auction with option 4 and fixed payments with option 6 were not used in the experiment, and thus unable to compare under these two cases), we can still observe that, consistent with the theoretical prediction, reverse auctions out-perform fixed payments under option 2, 5, 7, 8, and 9 (the total height of the bar is higher for DPA than FP). This result also contributes to the main hypothesis in this paper that increasing flexibility increase the performance of PES contracts. Reverse auctions can be interpreted as a more flexible contract scheme than fixed payments because the participants are given the freedom to make an offer and ask for an "ideal" payment, instead of a dichotomous choice of accepting or rejecting a fixed payment. However, reverse auctions do not outperform fixed payment when option 1 or option 3 is the only eligible option. This can be explained by non-additional behavior as the EB acquired under these two cases are mostly nonadditional. (This is reflected by the height of dotted bar as nonadditional EB under FP-1, DPA-1, FP-3, and DPA-3, while dashed bar stands for additional EB). Note that option 1 and option 3 are nonadditional by design for certain participants while option 4 to option 9 are additional options to all participants. Having non-additional (negative NPC) options may change participants' behavior in a reverse auction because any positive offer could give them a positive earning when they win. They can get a higher payment under reverse auctions than under fixed payments and use up the budget while some participants are crowded out of the auction because their high adoption cost prevents them from making lower offers and winning.

Group level data evaluating PES program performance were analyzed by a fixed-effect regression shown in Table 13. Estimations of coefficients for variables *3-paid* and *9-paid*, which stand for different levels of flexibility, are significant across different regressions which supports the hypothesis that flexibility affects PES program performances. Consistent with preliminary results shown in Figure 4, total EB is significantly higher under FP9 and FP3 than FP1, which means that increasing flexibility increase EB (and adoption) under fixed payments. However, although relaxing the contract from a 1-paid scheme to a 3-paid scheme increases EB by $402.17, the 9-paid treatment only increases EB by additional $14.94 (the 9-paid treatment increases EB by $417.11 compare to the 1-paid treatment). Although not fully tested, this result implies a potential non-linear relationship or diminishing marginal effect of contract flexibility and contract performance. Considering the research and administrative work for the social planner when offering a fully flexible contract like the 9-paid scheme, results here suggest that a medium level flexible (with 3 options) fixed payment scheme may be a most cost-effective one.

Although more flexible schemes outperform the strict one under fixed payments, the results are reversed under auctions. Both Figure 4 and regression results in Table 13 indicate that less EB was acquired under DPA3 and DPA9 compared to DPA1. In addition, although DPA1 outperforms FP1 for option 2, 5, 7, 8, and 9, DPA3 and DPA9 acquire less EB compared to their corresponding fixed payments schemes, FP3 and FP9. This result indicates that more flexible reverse auctions cannot increase the performance of PES programs measured by EB. This result can be explained in two directions. First, under strict schemes as FP1 or DPA1, the budget is tight, and limited participants would take the fixed payment (3 out of 6 subjects in a group would accept the fixed payment by design). Figure 5 shows that the average adoption rate is 3.43 out of 6 under FP1 while DPA1 induces more adoption (4.05 out of 6). So higher EB under reverse auctions are explained by higher adoption rates. Second, under 3-paid and 9-paid schemes, the high-cost options drive up the budget required to induce adoptions. Under a fixed payment scheme as FP3 or FP9, participants face a simple problem of choosing the highest earnings (shown as "Potential Earning" on participants' screens) and can get positive earnings from adopting a low-cost option. Figure 5 shows that the average adoption rate is 5.53 under FP3 and 5.98 under FP9. However, the adoption rate is lower under reverse auctions, 4.43 under DPA3 and 4.56 under DPA9. The low adoption rate can be a result of over-offering. Later regression results show that participants tend to make higher offers under more flexible auctions and also with higher budget (budget is always shown to subjects). Those low-cost participants in advantage request higher payments and get accepted. Those high-cost participants are prevented from winning and adopting because the budget is mostly used up to pay the lower offers.

Figure 6 introduces three other measurements of PES performance in addition to total EB acquired by the PES programs. The social planner may also care about the fiscal efficiency of the program such as budget, budget leftover, and information rents. The budget does make a difference, although not reflected by the histogram. The nonlinear relationship between budget and the performance of mechanisms are detected for Total EB, information rents and social welfare shown by Table 13. Higher budget leads to higher EB (both Total EB and Total additional EB) but also higher information rents. Not all budget is spent, and budget leftover stands for the money that supposed to be used for inducing adoption but fails to because of rejections of participants in fixed payments or reverse auctions. There is less budget leftover under more flexible treatments for both fixed payments and auctions because adoption rates are higher under more flexible treatments. DPA1 results in less budget leftover compare to FP1. However, DPA3 and DPA9 have higher budget leftover compared to FP3 and FP9 respectively. This result is consistent with the analysis on EB or adoption above. DPA1 results in more adoption compared to FP1 but DPA3 and DPA9 results in less adoption. More flexible schemes also incur significantly higher information rent for both fixed payments and reverse auctions. Regression on information rents from Table 13 shows that a 3-paid scheme generates $200.96 more information rents while a 9-paid scheme generates $260.79 more. Although DPA1 generates higher information rents compared to FP1, DPA3 and DPA9 have lower information rents compared to FP3 and FP9. In addition, total NPC is lower under more flexible treatments which indicates that participants choose less costly options given multiple options (proved by individual data analysis discussed later).

Total EB measures the benefit side of the PES programs while social planner' fiscal efficiency measures the cost. A new variable of social welfare is introduced as subtracting total NPC of those adopted practices from total EB acquired to measure the "net benefits" of these programs. Note that NPC stands for net private cost, which includes privates benefits to farmers, social welfare measures the total impact those PES programs have on the society. Figure 7 and regression (4) in Table 13 shows how different treatments affect social welfare. First, more flexible schemes do increase social welfare as social welfare is much higher under 3-paid and 9-paid schemes. Second, consistent with the measurement of EB, reverse auctions lead to higher social welfare than fixed payments when there is only one option eligible for payments. When there are 3 or 9 eligible options, social welfare is lower under reverse auctions. This result can also be explained by the adoption rate. Any option adopted will generate positive social welfare by design, i.e., EB_{ij} - $NPC_{ij} > 0$ for any i, j. Thus, more adoptions mean higher social welfare. DPA1 leads to more adoptions than FP1 while DPA3 and DPA9 result in fewer adoptions than FP3 and FP9 respectively.

The analysis above points out that non-additionality plays a key role in evaluating these payment schemes. It is not only that these non-additional adoptions or EB could have been there even without the PES payments, but also that participants with non-additional options may game the system and reduce the performance of these programs. Figure 4 shows that a larger share of EB comes from non-additional adoption when all 9 options are offered under either fixed payments or reverse auctions. This indicates that non-additionality problem may be worse under more flexible treatments for both fixed payments and auctions. To test if this hypothesis is true, a dummy variable *non-additional* is added to the group level regressions (*non-additional* equals 1 if a non-additional option, either option1, option2, or option 3, is eligible for payment). Although the estimation is not significant in regressing total EB, the total additional EB is $396.5 lower if a non-additional option is eligible for payment. The non-additional choices also reduce the social planner's fiscal efficiency because regression (3) shows that information rents are $320.97 higher when a non-additional option is eligible for payments. However, social welfare is $218.13 higher if a non-additional option is eligible. This is because that the non-additional options have negative NPC, or in other words, the non-additional options' private benefits exceed their private costs and will add up to EB, such that social welfare is much higher when they are adopted. On the contrary, the additional options with positive NPCs will be subtracted from EB when calculating social welfare.

2.4.2 Individual-level data analysis

As the experiment interface shows all ten options through different treatments, explaining the treatment effect is complicated by uncertainty in participants' behavior. Under fixed-payments, participants' choice is relatively straight-forward, and they simply need to choose the highest earnings option with certainty. However, under reverse auctions, in addition to the trade-off between earnings and probability of winning, the experiment also introduces a second level of trade-off among different options discussed above in the theoretical part. Although the theoretical analysis above mathematically modeled subjects' behavior, the model was not theoretically solved and there is no analytical model to solve the endogenous decisions. Facing a similar problem, Conte and Griffin (2017) used reduced-form regression to explain selection and offer behavior. Using a similar method, the research analyzes option choice by a conditional logit (fixed effect logit) model for panel data as shown by equation (2.4).

$$y_{ijt} = 1\big[\alpha + c_i + \beta_1 NPC_{ijt} + \beta_2 EB_{ijt} + \beta_3 Paid_{ijt} + \beta_4 Non\text{-}additional_{ijt} + \beta_5 Non\text{-}additional_{ijt} * 3\text{-}paid + \beta_6 Non\text{-}additional_{ijt} * 9\text{-}paid + \mu_{ijt} \geq 0\big] \tag{2.4}$$

where *i* indexes participants, *j* indexes option one to ten. $Paid_{ijt}$ equals 1 if the option is eligible for payment in a treatment and $non\text{-}additional_{ijt}$ take a value of 1 if the option is a non-additional option (the option has a negative NPC).

Regression was run independently for the pooled data and the three payment schemes, no payment (NP), fixed payments (FP), and reverse auctions (DPA). Table 14 reports the marginal effect on the probability of selection evaluated at the mean of all explanatory variables. Not surprisingly, the NPC of an option is significantly negative across different regressions, which means that participants are more likely to choose lower cost options. Participants are also more likely to choose a higher EB option, especially under the no-payment base scenario. EB should be exogenous to participants' decisions under no-payment and fixed-payments since EB does not affect their earnings. It makes more sense that participants are more likely to choose a high EB option because they were informed that the auctions are based on the offer/EB ratio. With similar NPC, a high EB option will lead to a higher probability of winning. Participants also respond to the eligibility rule significantly. When an option is eligible for payment, it is at least 24.07 percentage points more likely to be selected. The marginal effect of eligibility rule under reverse auctions (29.31 percentage point) is higher compared to that under fixed payment (24.07 percentage point). Participants show more interest in joining the program under reverse auctions possibly because of the hope of winning and getting a positive earning. Under fixed payments, they know the result of their choice clearly and may turn down payments when it is low. However, they may take the chance in making a profitable offer under reverse auctions although their offers may be rejected. Option10 as the base or the choice of staying at the status quo is controlled in all regressions and turns out to be significant in all regressions.

The primary parameters of interest are β_4, β_5, and β_6. β_4 can be used to derive the marginal effect of being a non-additional option (negative NPC) on the probability of selection. Table 14 shows that the estimation of β_4 is significantly positive for the pooled data. However, regressions for different payment mechanisms show that the non-additional attribute is not significant alone. The significant marginal effect under no payments is major because that when no options are paid, choosing a negative NPC option will earn participants money. While the dummy variable alone is not significant under fixed payments and even negative under reverse auctions. This is possible because when the payment comes into play for other additional options, the earnings of taking a payment exceeds the earnings of taking a non-additional option. However, the interaction term of non-additionality and flexibility levels turns to be significantly positive for both fixed payments and reverse auctions (the likelihood of choosing a non-additional option is 9.12 percentage point higher under a 9-paid fixed payment scheme; 6.13 percentage point higher under a 3-paid auction and 7.51 higher under a 9-paid auction). So, a more flexible payment scheme increases the chance that participants choose non-additional options.

The non-additionality problem not only comes from that participants are more likely to choose non-additional options, but also that participants may make use of their advantage to seize higher earnings in the form of information rents. Table 15 shows regression results on individuals offers and information rents acquired. Several insights could be drawn from Table 15. First, as the endogenous budget are calculated by the computer program and are shown to participants during the experiment, the estimation of *Budget* is significantly positive across different regressions. Participants make higher offers and earn higher information rents when they observe a higher budget. Second, variable *3-paid* are significantly positive in regression (5) to (8) which indicates that participants on average make higher ($25.24) offers and earns $21.61 more information rents under a more flexible scheme. Although participants do not make significantly higher offers under 9-paid auctions (estimation of *9-paid* is not significant in regression (5) and (6)), they do earn $20.31 more information rents. This is mostly because that given all 9 options, participants are more likely to choose a low cost (low NPC) or negative cost (negative NPC) option while maintaining the same offer level.

As predicted by theoretical models, regression (5) shows that participants make higher offers for higher NPC and higher EB options. However, when the non-additionality attributes are controlled as in regression (6), estimation on NPC is not significant anymore while the interaction term of *PositiveNPC* and *NPC* is significantly positive. *PositiveNPC*[12] equals 1 if the option's NPC is positive and indicates that the option is additional. This means that participants make offers based on both NPC and EB when they choose an option with positive NPC. However, their offer only depends on EB when they choose an option with negative NPC. This is mostly because that participants use NPC as an anchor for their offers and add a markup as earnings when their offers are accepted. However, when participants choose a negative NPC option, they could get positive earnings as long as their offers are accepted. NPC does not serve as an anchor for their offers anymore. A higher offer does not suffice higher earnings because higher offers are more likely to be rejected. Analysis of participants' earnings, calculated as accepted offer minus NPC, reflects the number of information rents transferred to participants. Estimations of *3-paid* and *9-paid* are consistently positive in regression (7) and (8) which support the hypothesis that more flexible schemes lead to higher information rents. Estimations in regression (7) and (8) shows that information rents are negatively related to NPC and positively related to EB. So higher cost participants earn less even though their offers are accepted. On the contrary, a negative NPC option earns the participant more information rents. When the dummy variable of *PositiveNPC* and the interaction is added to the regression on information rents, *EB* is not significant anymore but NPC shows stronger effects. Although the dummy variable *PositiveNPC* is not significant, the interaction of *PositiveNPC* and *NPC* is significant. This shows a differential effect of NPC on information rents. As shown by Figure 8, the relationship between NPC

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[12] The author uses *PositiveNPC* as the dummy variable indicating additionality for the ease of interpretation. The regression results are the same for a dummy variable of *NegativeNPC* and interaction term but with opposite signs.

and information rents could be described by a 2-dimensional coordinate system. As current research mostly discusses the relationship between adoption costs and information rents when costs are positive. This result extends the discussion to practices with negative costs, or in other words, practices with positive private benefits. Although the slope of the curve is flatter with negative NPC, they still seize a huge amount of public funding as information rents because these non-additional participants hold a relative advantage in reverse auctions that they could earn money with very low offers.

2.5 Conclusion

One reason why the PES programs do not work well in terms of low participation or performance is that the programs are too rigid and does not offer many options to participants. This research explores the possibility of improving the PES program by offering more flexible programs. Starting with the hypothesis that more flexible PES program, specified as more options offered to participants to choose from in this paper, can improve the performance of these programs, the author first reviews the literatures on reverse auctions in PES programs, then extends the theoretical model of optimal choice of Latacz-Lohman and Hamsvoort (1997). The theoretical model describes a participant's choice facing multiple options. A lab experiment is programmed under the theoretical model to compare PES program performance with fixed payments and reverse auctions. The author finds complicated trade-offs between flexibility and program performance.

Starting from group level analysis, experiment results show that when more options are offered to participants as more flexible schemes, both fixed payments and revere auctions achieve higher benefits. However, there is a diminishing marginal effect of flexibility because of the fully flexible scheme, offering all 9 options, does not outperform the medium one, offering 3 random options that much. Considering the potential administration and research work required for the social planner, offering a medium flexible scheme may be more cost-effective. On the other hand, although reverse auctions outperform fixed payments in terms of external benefits achieved with the strictest case when there is only one additional option to choose from, reverse auctions do not outperform fixed payments under more flexible schemes. The adoption rates explain the direct reason as more flexible fixed payments induces significantly more participants to adopt a practice while auctions do not do that well. This is because of over-offering from participants during a reverse auction.

A further investigation of the performance of these PES programs and the design of negative cost options enables the research to analyze what is going to happen when there are non-additional choices. Group analysis shows that when more flexible schemes are given to participants, although higher benefits are generated, a lot of the benefits are non-additional, or in other words, a lot of benefits should have been achieved even without extra payment. These non-additional benefits reduce the fiscal efficiency of public funding because the budget was transferred to participants as information rents instead of covering adoption cost for additional practices. Further analysis of participants' choice of option and offer behavior reveals that participants are more likely to choose a non-additional option under more flexible schemes and earns more money as information rents when choosing a non-additional choice.

In terms of the investigation on non-additionality, relaxing the strictness or rigidity of PES programs by offering more options to farmers could increase the adoption rates and benefits achieved. However, this improvement comes with a price of higher information rents and more importantly, more non-additional behaviors, especially when the participants are offered full flexibility to choose any option available. The profit maximization objective of rational farmers in nature will drive participants to choose low cost or non-additional choices. This experiment, as the first one to include non-additional practices in auctions in lab experiments, proves that neglecting non-additional behavior overestimates the performance of PES auctions and more generally, PES programs. Non-additionality should be considered as an important criterion when evaluating these PES programs.

In conclusion, this paper originates from an exploration of improving PES program performance by offering more options to program participants but finds complicated results and trade-offs because of mechanism design, behavioral decisions, and evaluation criteria. Although offering more options can increase the total benefits of PES programs, the social planner also pays farmers more as information rents in addition to the "right" payments to just cover farmer's net private costs. Furthermore, a more flexible program also incurs more non-additional behaviors. From the farmer's perspective, it is consistent with behavioral economics researches that "more options are better," because farmers got to choose the option they prefer and thus earn a higher profit. However, to social planners, it reduces the fiscal efficiency of the PES programs. The extra information rent transferred to the low cost or non-additional farmers may be used to support more projects and increase program participation. When designing PES programs or, further, any government procurement programs, the social planner should be aware of the potential trade-offs between different mechanism (i.e., fixed payments and reverse auctions). This requires (1) a clearly defined program goal which prioritizes a certain evaluation criterion, (2) a relatively flexible schedule that gives options to participants to encourage participants but also maintains fiscal efficiency, (3) caution on non-additional option or behavior which requires further effort in information and research.

Chapter 3

THE EFFECT OF COVER CROP COST-SHARE PAYMENTS IN MARYLAND AND OHIO, CONTROLLING DOUBLE-SELECTION EFFECT AND NON-ADDITIONAL BEHAVIOR

3.1 Introduction

Recognizing the benefits of planting cover crops in “manag(ing) soil fertility, soil quality, water, weeds, pests, disease, or wildlife,” (USDA NASS 2012) government agencies at the federal and the state level are promoting further expansion of cover crops in the United States (Hamilton et al. 2017). Hamilton et al. (2017) show a substantial difference in the current adoption rates by states in 2012, which can be attributed to agronomic, climate, and policy differences. The availability of cost-share programs, which is used as the primary policy instrument, plays a key role in promoting cover crops (Singer et al. 2007) because farmers view the high cost of planting as the primary barrier of using cover crops (Roesch-McNally et al. 2017). The cost-effectiveness of these cost-share programs can be compared in two aspects, the evaluation on the extensive margin as "how many" farmers or farms planted cover crops, and the evaluation on the intensive margin, which refers to "how many acres" per enrolled farm. This research uses a double-selection model, which combines the extensive margin and intensive margin, to estimate and compare the effectiveness of cost-share payments in Maryland and Ohio with survey data.

Cost-share programs for cover crops are available at both the federal level and state level. The federal programs, such as the federal Environment Quality Incentives Program (EQIP) and Conservation Stewardship Program (CSP), are similar across states but the state and local programs vary at different levels. With 30 years' experience in incentivizing cover crops, Maryland is the leading state of cover crop adoption, which may be a result of the regulatory pressure of controlling non-point source pollutions in the Chesapeake Bay area. The Maryland cover crop program, administered by the Maryland Agricultural Water Quality Cost-Share (MACS) Program, paid $18.8 million to 1,443 farmers to planted 395,862 acres of cover crops in the 2017-2018 planting season (MACS 2018 annual report). Farmers can receive annual payments between $45 to $75 per acre depending on the crop species and extra farming practices adopted. There is a five-acre minimum requirement but no acreage caps. Ohio, on the other hand, has lower adoption rates and lower shares of farmland. Although several state and local cost-share programs exist, these local programs mostly aimed at reducing nutrients leaching into specific watersheds and are confined to particular geographical areas. Cost-share payments are also lower, generally between $25 and $35 per acre per year, and usually, come with a cap for the maximum enrollment acreage. For example, the Tiffin River Sediment and Nutrient Reduction Initiative (SNRI) restricts the maximum number of acres a farmer can enroll to be 50 acres, while the Tiffin River and Bear Creek Watershed Improvement Plan (TRBC) only allows enrollment up to 25 acres (Fulton Soil & Water Conservation District).

Several challenges arise in estimating the effects of cost-share payments. First, given the history of promoting cover crops and the potential private benefit generated to farmers, self-adoption of cover crops, which is planting cover crops without any cost-share funding, needs to be controlled as the baseline for comparison. Only those additional farms or acreages planting cover crops should be recognized as the result of the cost-share programs while those nonadditional farms or acreages that would have used the practice without cost-share should not. Several studies address this issue by distinguishing and quantifying additional and non-additional adoptions (Horowitz and Just 2013; Duke et al. 2014; Claassen et al. 2018) for conservation practices. Without controlling the self-adoption counterfactual, research may overestimate the effects of cost-share programs. For example, as Kurkalova et al. (2006) estimates, about 86% of subsidies of conservation tillage went to existing adopters, which reduces the cost-effectiveness of conservation payments. Second, there are two potential selection effects in the adoption process. One arises from the voluntary actions of farmers, and the other one from the selection process of government agencies. The selection processes lead to non-random distribution of farmers and thus inconsistent estimation in regression results. The selection problem is well recognized by researchers in Labor Economics (Lee 1978) and Econometric methods (Lee 1983; Maddala 1986) such as Heckman two-step methods are developed to test and correct this problem. Several Agricultural Economic researches studied the selection bias in conservation payments programs (Cooper and Keim 1996; Wu and Babcock 1998; Lichtenberg and Smith-Ramirez 2011; Bergtold et al. 2012; Ma et al. 2012; Ji et al. 2017). Third, given cross-sectional survey data of farmers, one can only observe farmers' choices with or without cost-share payments but not the counterfactuals. In other words, the observed data only tells part of the story. Researchers used different methods such as switching models (Lichtenberg and Smith-Ramirez 2011, Fleming 2017, Fleming et al. 2018), propensity score matching (Mezzatesta et al. 2013, Claassen et al. 2018, Plastina et al. 2018), or panel data analysis (Lichtenburg et al. 2018) to construct the counterfactuals.

The selection effect in the adoption of conservation practices has been well recognized in the research literature. This research builds upon and extends previous works with new data and new perspectives. Selection originates from different reasons. A simple explanation is that farmers who find cover crops most helpful are more likely to plant cover crops and plant on more farmland. They are also more likely to apply for a cost-share program. A direct comparison of program participants and non-participants will lead to biased estimation because of the ignorance of potential selection issues. Some researchers following Lee (1978) and Maddala (1991) attributes the selection bias as missing data or unobservable information. Some factors, which affect both the adoption decision and the intensity of adoption, are unknown to the researchers. For example, Fuglie and Bosch (1995) constructed a two-stage selection model in the adoption of soil nitrogen testing and corrected the selection bias in the nitrogen application rate. Considering the simultaneous adoption decisions on multiple practices, Wu and Babcock (1998) adopted the model by Lee (1983) to extend one-choice selectivity models to a polychotomous-choice model and controlled for self-selection bias. They argued that farmers who were willing to enroll in one conservation program had lower cost and were more likely to enroll in another program. The data collection process may also introduce selection bias. In the investigation of farmers' acceptance of and responses to cost-share payments for five best management plans (BMPs), Cooper and Keim (1996) recognized the selection bias arose from the sequential nature of survey questions; specifically, because only the non-adopters were asked a dichotomous choice question to accept or to reject an offer and only farmers who accepted the offer gave an acreage response.

A double-selection model extends the single selection model by correcting selection bias arising from the sequential analyses in multiple stages or non-randomness in multiple groups. Khanna (2001) used a double-selectivity model to analyze the sequential decisions of adopting two connected conservation practices, soil testing, and variable rate technology. Ji et al. (2017) used a double-selection model to distinguish factors that lead to long-term versus short-term adoption and partial-versus-complete adoption of soil carbon sequestration technology. They found that cost-sharing programs and households' wealth played a key role in the continued adoption of conservation tillage. Bergtold et al. (2012) addressed the subjective and sequential decisions in adoption, arguing that farmers would have planted cover crops if the perceived net benefit was positive and this perception introduced a potential self-selection bias. They used a three-stage model with double-selectivity to explain the sequential nature of planting cover crops, having positive perceived yield benefits, and the magnitude of perceived yield benefits. Bergtold et al. (2012) also addressed re-education and outreach to increase farmers' perception of the potential yield benefits of cover crops. However, they did not distinguish the sources of information. Recognizing the differential effects of information sources, Dunn et al. (2016) utilized a principal component analysis and summarized two components as "learning from others" and "self-learning." They found substantial self-funding or self-adoption of cover crops and attributed the early adoption to a high willingness of self-learning. However, they found adoption only depended on cost-share at a minimal level.

The selection models are not the only approach to address sequential decisions. Ma et al. (2012) decomposed farmers' participation decision into two steps by a survey question and used a double hurdle model. A farmer would "consider" participation first, before the decision of enrollment, and the decision of acreage later. Ma et al. (2012) found that farmers' willingness to consider participation primarily depended on non-price farm and farmer characteristics but not the cost-share payments. Payments played a much important role in the decision of acreage. Similarly, Chalak et al. (2017) compared a double hurdle model and a Tobit model to find that farming experience, information sources, and irrigation frequency had significant effects on the willingness to adopt. Cooper and Keim (1996) also used a double hurdle model to compare with their selection model, and they got similar results.

Heckman and Vytlacil (2007) summarized the econometric evaluation of social programs and several studies are built on it to evaluate agricultural cost-share programs. Following this general structure, Lichtenberg and Smith-Ramirez (2011) used a switching model to calculate the treatment effects on the treated and untreated farmers of three conservation practices, including cover crops. After correcting the self-selection bias with a multivariate model in the first stage, they found a positive effect of cost-share payments on the extensive margin of adoption decisions but not on the intensive margin of land shares. Fleming (2017) and Fleming et al. (2018) used similar approaches, which constructed two-stage models to correct the selection-bias and estimated treatment effects with switching model. However, Fleming (2017) and Fleming et al. (2018) find different results. Fleming (2017) found that the MACS program had not only a positive direct effect on cover crop acreage but also a positive indirect effect on the enrollment of two other conservation cost-share programs. In addition to a significant direct effect on cover crop acreage and indirect effect of conservation tillage, Fleming et al. (2018) found an adverse indirect effect on farm vegetative cover, which undermined the total effect of cost-share programs. Lichtenberg et al. (2018) utilized adoption data of Maryland farmers in 1997 and 2009 and categorized farmers as new adopters ("joiners"), leavers ("discontinued adopters"), and always participants. By controlling the possible correlation of adoption decision across years, they found that enrolled farmers have 30-36 percentage points higher share of acreage. However, due to limited availability of data, similar panel data or time-series analyses are not widely used.

Nonparametric methods such as propensity score matching (PSM) are also used to evaluate government interventions and program effects. The matching process pairs the treated farmers and the untreated farmers with similar attributes, which enables the calculation of treatment effect as the difference in the outcome of paired farmers. The potential selection bias is thus controlled and corrected because the paired farmers are, theoretically, identical. For example, Plastina et al. (2018) utilized a PSM method with the 2012 Census of Agriculture data and found that farmers who received cost-share payments planted up to 192 more acres of cover crops, which is, on average, an additional 18% of farmland than matched farmers who did not receive any payments. Mezzatesta et al. (2013) used PSM to estimate the additionality of five conservation practices including cover crops. They further decomposed the average treatment effect on the treated (ATT) for the new adopter and voluntary adopters to control for additionality and found a significantly positive effect from the cost-share program and concluded that this effect was also additional. Claassen et al. (2018)

applied a PSM analysis of data from the Agricultural Resource Management Survey and found similar results. Also, they found that the treatment effects vary by practices. Practices that take farmland out of production or have a high up-front-cost, such as the riparian buffers, resulted in high treatment effects, while those practices with high on-farm benefits, such as conservation tillage, resulted in lower effects of cost-share payments. However, they did not realize the specialty of cover crops and did not analyze the cover crops programs. On the one hand, planting cover crops do not take out farmland during the cash crops season and have substantial on-farm benefits. On the other hand, cover crops are also costly because of the seed costs, additional labor and management work required.

This study adapted the generalized double-selection model by Tunali (1986) to estimate the treatment effect of enrollment in cost-share programs for cover crops on both the extensive margin and intensive margin. A three-stage double-selection model with incomplete classification is constructed with the dichotomous adoption decision in the first stage, the dichotomous enrollment outcome in the second stage, and general linear model of the share of farmland in the third stage. The first two-stages not only investigate the factors affecting the adoption decision and enrollment results on the extensive margin but also generate residual variables, which correct the selection effects in the third stage. Thus, the third stage regressions yield consistent estimates of cover crop share function and predict adoption intensity for each observation. The average treatment effects are calculated as the difference between the share of farmland under cover crops for enrolled farmers and their predicted counterfactual share of the farmland without any payments. Farmers are classified into three subgroups according to their adoption decision and enrollment outcome in the 2017-

2018 planting year. “Paid-adopter” refers to the treatment group or those who planted cover crops with a cost-share program. “Self-adopter” refers to those who planted cover crops without any cost-share payments. “Non-adopters” refers to those who did not plant cover crops at all. The survey data used for the estimation are collected from farmers in Maryland and Ohio as part of a larger study (Duke, Johnston, Shober 2018). The survey data provided the share of acreage under cover crops for each farmer, but no information of the counterfactual outcomes, i.e., what the paid-adopters would have done without payments and what the self-adopters would have done with cost-share payments. However, regression on paid-adoption and self-adoption observations can generate out-of-sample predictions, which serve as counterfactual outcomes. The two-states setting also enables the researcher to compare adoption and enrollment outcomes and conduct a cross-state treatment effect.

The empirical analysis provides three main results. First, with self-adoption behavior controlled as the counterfactual, the in-state ATTs are positive and significant for both Maryland and Ohio. This result confirms that cost-share programs increase the share of acreage for farmers enrolled in these programs. The ATT is higher in Maryland compared to Ohio, which can be attributed to higher payments. The in-state average treatment effect on the untreated (ATU) is insignificant for Maryland but significantly positive for Ohio. This result affirms the continuous effort of promoting cover crops for 30 years in Maryland and signals the potential of expanding the cost-share programs in Ohio. Second, a cross-state analysis provides valuable policy insights. The out-of-state ATU are positive and significant for Ohio, and the increment is higher than the in-state ATT and ATU. This again provides evidence that if Ohio expands the cost-share programs similar to the Maryland program, the intensity or the acreage of cover crops will increase for both current paid- and self-adopters. Third, the out-of-state ATT is significantly positive but lower than the within-state ATT for Maryland, which indicates that although there is substantial self-adoption, cost-share programs are still necessary to induce cover crop planting. Lower payments or a payment cap will lead to lower shares of land under cover crops.

3.2 Theoretical framework

Planting cover crops require additional input in return for external benefits and potential private benefits which are uncertain or not well recognized by farmers. Thus, farmers' willingness to adopt cover crops depends on the magnitude of the change in utility. This change can be measured monetarily by the willingness to accept (WTA) payments, which is the minimum payments that the farmer would acquire to adopt this additional practice. With the existence of cost-sharing programs, which are intended to reduce farmers' private net cost and promote adoption, the observed enrollment status offers further information on the determinants of cover crop adoption. Denoting the cover crop adoption decision for farmer i as y_{1i}, where y_{1i} = 1 indicates that the farmer adopted cover crops in 2017 and denoting the status of the cost-share enrollment as y_{2i}, where y_{2i} = 1 indicates that the farmer enrolled in a cost-share program and got payments to plant cover crops in 2017. Note that different from y_{1i}, which is an outcome indicator of a farmer's decision adopting cover crops or not, y_{2i} stands for the outcome of cost-share program enrollment, which is a bilateral decision of the farmer and the government agency. Not all farmers who applied for the cost-share program will be accepted because the government agency has a limited budget and may select applicants with specific rules. Thus, y_{2i} = 1 indicates both that the farmer applied for the cost-share program and got approved by the government agency. The two dichotomous variables, y_1 and y_2, divide the sample of eligible farmers into four subgroups denoted by G_j, $j = 1, \ldots, 4$ as shown in Table 16. The subscript i was suppressed for simplicity for the following part because the analysis is the same for each farmer i.

Group 1 (G_1), or the non-adopters are those who were not enrolled in any cost-share programs and were not planting cover crops at all. Two possible reasons can explain this non-enrollment and non-adoption status. First, these farmers may have strong objections to planting cover crops. Thus, they have the highest WTA, and they are not willing to use cover crops even with the available cost-share payments. Second, these farmers could have adopted cover crops with cost-share but are not eligible for the programs or rejected by the programs. Those farmers locate on the far-right of the WTA spectrum shown in Figure 9 and are the costliest group to incentivize. Group 2 (G_2), or the noncompliance farmers are those who enrolled in a cost-share program but failed to comply with the program requirements of planting cover crops. Various reasons can lead to noncompliance, but the noncompliance rate is low in the existing cost-share programs. Furthermore, reliable information on noncompliance is hard to acquire because there might be a morality challenge in asking farmers if they “failed” or “cheated” on the program, even if accidents or external reasons cause the noncompliance. The survey respondents reported whether they did or did not plant cover crops, but not whether they failed to comply or were just non-adopters. Thus, farmers in G_1 and G_2 are indistinguishable from the survey data used for this research, and they are all identified as non-adopters in Figure 9. This incomplete classification complicates the model specification and will be discussed later.

Group 3 (G_3), or the self-adopters are those who adopted cover crops without any cost-share payments. Numerous reasons can lead to self-adoption. It could be that they were early adopters without knowing these cost-share programs; it could be that they recognized the private benefits of cover crops and did not apply for cost-share out of a sense of environmentalism or altruism; it also could be that they did not qualify for the cost-share programs. Regardless of the reason, the survey sample contains 72 self-adopters (10%) in Maryland and 545 (38%) in Ohio. Those farmers have negative WTAs and locate on the negative quadrant of the WTA spectrum. This research later argues that the self-adoption acreage should be controlled as the base level when calculating the effect of cost-share programs. Group 4 (G_4), or the paid-adopters, are adopters with cost-share programs. This group can be further classified into two sub-groups as the additional-adopters and the nonadditional-adopters shown in Figure 9. The additional-adopters are those who would adopt cover crops with cost-share but would not without payments. The cost-share payments worked as a "trigger" to their decisions, and they are the primary targets of the cost-share programs on the WTA spectrum. The nonadditional-adopter are those who would have planted by themselves but still applied and got approved for payments. These farmers pose a threat to the cost-effectiveness of the cost-share programs because the government agency could have used the payments to induce more additional-adopters. However, these two subgroups are non-distinguishable from the data set and will be treated as one type in the following part.

Last, $share_i$ is a continuous percentage variable between 0 and 1 as the share of a farmer i's farmland planted with cover crops. Thus, the decision to adopt cover crops consists of three dependent variables expressed in reduced form functions as (subscript *i* is hidden for notation simplicity):

$$y_1^* = \boldsymbol{\beta}'_1\boldsymbol{x}_1 + \epsilon_1 \text{ with } y_1 = \begin{cases} 1 & if\ y_1^* > 0 \\ 0 & if\ y_1^* \leq 0 \end{cases} \qquad \text{Adoption decision} \tag{3.1}$$

$$y_2^* = \boldsymbol{\beta}'_2\boldsymbol{x}_2 + \epsilon_2 \text{ with } y_2 = \begin{cases} 1 & if\ y_2^* > 0 \\ 0 & if\ y_2^* \leq 0 \end{cases} \qquad \text{Cost-share enrollment} \tag{3.2}$$

$$share = \boldsymbol{\beta}'_3\boldsymbol{Z} + \sigma_3\epsilon_3 \qquad \text{Share of adoption acreage} \tag{3.3}$$

where y_1^* and y_2^* stands for latent utilities that are unobservable; $\boldsymbol{x}_1$, $\boldsymbol{x}_2$, and $\boldsymbol{Z}$ are vectors of explanatory variables in the three stages; $\boldsymbol{\beta}_m$'s are vectors of unknown coefficients; σ_3 is an unknown scale parameter; and ϵ_m's, the error terms or unobservable components with zero mean and covariance matrix:

$$\Omega_C = var\begin{pmatrix} \epsilon_1 \\ \epsilon_2 \\ \epsilon_3 \end{pmatrix} = \begin{bmatrix} 1 & \rho_{12} & \rho_{13} \\ \rho_{12} & 1 & \rho_{23} \\ \rho_{13} & \rho_{23} & 1 \end{bmatrix}$$

If the adoption decision of equation (3.1), the cost-share enrollment outcome of equation (3.2), and the intensity of adoption as the share of farm acreage indicated by equation (3.3) are independent, there is no correlation among the error terms, and $\rho_{12} = \rho_{13} = \rho_{23} = 0$. Then the three equations can be estimated separately. However, with potential selection effects discussed below, the correlations among the error terms are nonzero and separate estimation results will be biased.

3.2.1 The selection processes

Thinking of the problem from the perspective of experimental economics, the share of acreage planted cover crops, *share,* can be viewed as the experiment outcome if the government agency is recognized as an experimenter. Then, the cost-share programs can be thought of as the treatment and the farmers as the subjects. Those farmers in G_4, who were paid to use cover crops are thus in the treatment group, while those who were not paid are in the control group. To achieve an accurate estimate of the treatment effect of cost-share payments, the experimenter should have a random sample of farmers in the treatment group and the control group. However, the distribution of farmers in the treatment group and the control group may not be random because of the potential selection process. As stated by Tunali (1986): "Although commonly viewed as a missing data or censoring problem, what is at the heart of selectivity is not the lack of observations on the outcome variable but the fact that inclusion in the subsample (sample selection) may be nonrandom." As stated above, a sample selection issue may exist because (1) the farmers who have a low or negative WTA may choose to participate in the program as a voluntary action, and (2) the government agency may select farms with specific attributes into the treated group. Thus, the distribution of farms in the treatment group and the control group is not random. The bivariate structure of the qualitative outcome (y_1 and y_2) leads to two levels of selection which can be tested and corrected in the quantitative estimation of *share* following the general model by Tunali (1986).

Further, as pointed out by Tunali (1986), there are multiple ways to handle multiple selection criteria, and there is a distinction between sequential and simultaneous decisions. Researchers used sequential modeling when there is an apparent order in the selection rules, for example, the adoption of a package of conservation technologies (Khanna 2001), adoption decision followed by a subjective perception of potential benefits (Bergtold et al. 2012), or adoption decision followed by partial versus complete adoption (Ji et al. 2017). Simultaneous selection models are also widely used to detect and correct selection bias for two (Cooper and Keim, 1996; Fleming et al., 2018) or multiple practices (Lichtenberg and Smith-Ramirez, 2011; Fleming, 2017). This paper applies a double-selection model with sequential outcomes shown in Figure 10.

As the previous research shows, the adoption of cover crops has a long history, and there are early adopters before the government decides to promote cover crops by cost-share programs. The first selection process happens when we only observe the enrollment results ($y_2 = 0$ or $y_2 = 1$) for those who were using cover crops ($y_1 = 1$). The enrollment decisions (y_2) are unobservable for group G_1 and G_2, and we were unable to distinguish them. This first-stage selection could be attributed to a self-selection process when some farmers choose to join the program for unobservable reasons. It could be that they are early adopters and are taking advantage of the program by taking the cost-share payments even though they would have planted cover crops by themselves. It could be that they enrolled with other conservation practice programs and were familiar with the administrative process (lower transaction cost of enrollment). In general, the adopters ought to have lower, or even negative, WTA than the non-adopters have.

The second selection process happens when a farmer planted cover crops with cost-share. The second stage is also more complicated because it is a combined selection outcome of both the farmers and the government agency. On one side, some farmers may select to apply or enroll in the cost-share program. On the other side, not all farmers who apply for the program will get approved and the government agencies, with a limited budget, may have specific targets, which prioritize certain program applicants. The program administrator may select farms in certain areas (critical zone in MD) or with specific characteristics. Moreover, these programs are competitive, especially the state or local programs with a limited budget. Those enrolled farms may have higher environmental benefits or fit the program's goal better compared to those applicants rejected. The researcher recognizes the complication in the second selection process. However, due to limited information on the actual cost-share application and approval process, the author uses the enrollment outcome as a combined effect of the two selection processes. Although data reveals the acreage (*share*) of cover crops for both the self-adopters (G_3) and paid-adopters (G_4), the farmers in these two subsamples are not randomly selected. Hence simple linear regression on the share of acreage (*share*) will be biased (inconsistent) without considering the selection effect.

3.3 Empirical model

As analyzed above, the adoption decision (y_1) and enrollment outcome (y_2) divide the sample into four subgroups and there exists a possible selection bias results from the non-random selection of subjects into the treatment group (G_4). One may argue that the treatment effect of cost-share payments could be investigated by using a dichotomous independent variable to control program enrollment. However, this simple estimation requires that the underlying distribution of the unobservable characteristics of the two subgroups to be the same. With potential selection effects discussed above, the subjects in each subgroup are not randomly selected and may exhibit different behaviors. Thus, the simple method may result in inconsistent and inefficient results.

Some researchers utilized two-stage single selection models to correct the selection bias in estimating the treatment effect of cost-share programs (Fleming 2017). Generally, cost-sharing enrollment outcome (y_2) is used as the selection rule in the first stage, and the intensity of cover crop usage (*share*) is estimated with a generalized residual variable (the inverse mills ratio). However, it assumes that unenrolled farmers share the same pattern of attitudes toward cover crops. This may not be true because there are substantial self-adopters in both states. Those who adopted cover crops without cost-share probably have different attitudes or characteristics from those farmers who do not use cover crops at all. To test and correct this potential bias, the researcher adopts a double-selection model to distinguish the non-adopters and self-adopters and further argues that only the self-adopted cover crop acreages should be used as the base when calculating the treatment effects.

3.3.1 Bivariate probit model with partial observation

This paper follows the general structure for models of double-selection with incomplete classification constructed by Tunali (1986). Following the rationale specified above, the adoption decision of equation (3.1) serves as the first selection rule, the cost-share enrollment outcome of equation (3.2) serves as the second selection rule, and the continuous regression of cover crop acreage in the third stage is the primary function of interest. With the main objective to estimate the parameters of equation (3.3) of adoption share, the regression for a subsample having random subjects and complete observations may be written as:

$$E(share|x_3, \mathrm{G}_4) = \boldsymbol{\beta}_3'\boldsymbol{Z} + \sigma_3 E(\epsilon_3|\boldsymbol{Z}, \mathrm{G}_4) \tag{3.4}$$

where G_4 stands for the subsample 4, as a result of the two selection rules. A selection bias exists if $E(\epsilon_3|\boldsymbol{Z}, \mathrm{G}_4) \neq 0$, which leads to inconsistent parameter estimates of $\boldsymbol{\beta}_3$. The enrollment outcome is observed only when a farmer adopted cover crops, or y_2 is observed only when $y_1 = 1$. As explained above, when a farmer responded by no cover crops usage ($y_1 = 0$), we cannot distinguish if the farmer is in G_1 or G_2. Thus, we observe three subgroups instead of four. The probabilities for a farmer to be in one of the three subgroups are now:

$$(P_1 + P_2) = Pr(y_1 = 0) = Pr(y_1^* \leq 0) = F(-C_1) \tag{3.5}$$

$$P_3 = Pr(y_1 = 1, y_2 = 0) = Pr(y_1^* > 0, y_2^* \leq 0) = S(C_2, -C_1; -\rho_{12}) \tag{3.6}$$

$$P_4 = Pr(y_1 = 1, y_2 = 1) = Pr(y_2^* > 0, y_1^* > 0) = S(C_2, C_1; \rho_{12}) \tag{3.7}$$

where P_j, $j = 1, 2, 3, 4$, denotes the probability that a farmer is in group G_j. $F(\cdot)$ denotes the standard univariate normal distribution function. $S(\cdot,\cdot\,;\cdot)$ denotes the standard bivariate normal distribution function and $C_m = \boldsymbol{\beta}'_m \boldsymbol{x}_m$. So equation (3.5) to (3.7) constitutes the qualitative structure of the model.

Further, with the trivariate normal specification, the probability density function for *share* can be calculated. For G_4 ($y_1 = 1$ and $y_2 = 1$):

$$f(share|y_1 = 1, y_2 = 1) = \frac{1}{P_4}\int_{-\boldsymbol{C}_2}^{\infty}\int_{-\boldsymbol{C}_1}^{\infty}\frac{1}{\sigma_3}h(\epsilon_1, \epsilon_2, \frac{y_3 - \beta_3' Z}{\sigma_3})d\epsilon_1 d\epsilon_2 \tag{3.8}$$

where $\underline{h}(\cdot,\cdot\,;\cdot)$ denotes the trivariate normal density for ϵ. The likelihood function is

$$\mathrm{L} = \prod_{G_1+G_2} F(-C_1)\cdot\prod_{G_3} S(C_1, -C_2;\, -\rho_{12})$$

$$\cdot\prod_{G_4}\int_{-\boldsymbol{C}_2}^{\infty}\int_{-\boldsymbol{C}_1}^{\infty}\frac{1}{\sigma_3}h(\epsilon_1, \epsilon_2, \frac{y_3 - \beta_3' Z}{\sigma_3})d\epsilon_1 d\epsilon_2$$

(3.9)

The complexity of these functions makes full information maximum likelihood procedures difficult when the number of parameters to be estimated is large. Considering the complexity of the functions which makes full information maximum likelihood procedures difficult, Tunali (1986) extended the model by Heckman (1976, 1979) and Lee (1976), and offered a two-step procedure. Equation (3.4) can be written as

$$E(share|\epsilon_1 > -C_1,\ \ \epsilon_2 > -C_2) = \beta_3' Z + \sigma_3 E(\epsilon_3|\epsilon_1 > -C_1,\ \ \epsilon_2 > -C_2) \qquad (3.10)$$

Given the trivariate normal assumption, the conditional expectation on the right-hand side is:

$$E(\epsilon_3|\epsilon_1 > -C_1,\ \ \epsilon_2 > -C_2) = \rho_{13}\frac{f(C_1)F(C_2^*)}{P_4} + \rho_{23}\frac{f(C_2)F(C_1^*)}{P_4}$$

$$= \rho_{13}\lambda_1 + \rho_{23}\lambda_2 \qquad (3.11)$$

where $C_1^* = \frac{C_1 - \rho C_2}{(1-\rho^2)^{1/2}}$ and $C_2^* = \frac{C_2 - \rho C_1}{(1-\rho^2)^{1/2}}$, and $\lambda_1 = \frac{f(C_1)F(C_2^*)}{P_4}$, $\lambda_2 = \frac{f(C_2)F(C_1^*)}{P_4}$, the two λ's constitutes the double-selection analogs of the inverse Mill's ratio, which is used in the single selection models. Substitute equation (3.11) into equation (3.4) to get the double-selection bias corrected regression as:

$$share = \boldsymbol{\beta}_3'\boldsymbol{Z} + \rho_{13}\lambda_1 + \rho_{23}\lambda_2 + \sigma_3 \upsilon_3$$

$$= \boldsymbol{\beta}_3'\boldsymbol{Z} + \gamma_1\lambda_1 + \gamma_2\lambda_2 + \sigma_3 \upsilon_3 \qquad (3.12)$$

where $\upsilon_3 = \epsilon_3 - \rho_{13}\lambda_1 - \rho_{23}\lambda_2$ with $E(\upsilon_3|y_1^* > 0, y_2^* > 0) = 0$. The constructed residual variables change the linear intensity model of equation (3.3) to a nonlinear model of equation (3.12), which also poses challenges in estimation. Instead of estimating equation (3.12) directly, the qualitative structure is used to estimate $\lambda 1$ and $\lambda 2$ first, and then estimates are used as instrument variables in equation (3.12) to estimate the other parameters. This is similar to a Heckman 2-step procedure.

The qualitative structure with binary outcomes of y1 and y2 has the likelihood function following Tunali (1986):

$$L^* = \prod_{G_1+G_2}[1 - F(C_1)] \cdot \prod_{G_3} S(C_1, -C_2;\ -\rho_{12}) \cdot \prod_{G_4} S(C_1, C_2;\ -\rho_{12}) \tag{3.13}$$

Maximization of the above likelihood function can give consistent estimates of $\widehat{\boldsymbol{\beta}}_1, \widehat{\boldsymbol{\beta}}_2, \hat{\rho}$. With the estimated value, the instrument variables can be estimated as $\hat{\lambda}_1$and $\hat{\lambda}_2$ for each observation in G_4. Substitute λ_1 and λ_2 with $\hat{\lambda}_1$and $\hat{\lambda}_2$ to get

$$\begin{aligned} share &= \boldsymbol{\beta}_3'\boldsymbol{Z} + \rho_{13}\hat{\lambda}_1 + \rho_{23}\hat{\lambda}_2 + \sigma_3\hat{\upsilon}_3 \\ &= \boldsymbol{\beta}_3'\boldsymbol{Z} + \gamma_1\hat{\lambda}_1 + \gamma_2\hat{\lambda}_2 + \sigma_3\hat{\upsilon}_3 \end{aligned} \tag{3.14}$$

where $\hat{\upsilon}_3 = \upsilon_3 + \rho_{13}(\lambda_1 - \hat{\lambda}_1) + \rho_{23}(\lambda_2 - \hat{\lambda}_2)$.

3.3.2 Identification with information variables

The independent variables $\boldsymbol{x}_1$, $\boldsymbol{x}_2$, and $\boldsymbol{Z}$ from equation (3.1), (3.2) and (3.14) contain many shared variables such as farm acreage, farming experience, and crop species. In a single selection model with Heckman 2-step estimation, the independent variables in the selection equation (or the first step) must contain at least one variable that is not in the result equation (the second step). These additional independent variables, when correctly identified, should (1) provide a reasonable explanation for the existence of selection and (2) satisfy the exclusion condition, which requires that these variables affect the outcome of the first stage but not the outcome of the second stage. The method is similar to an instrument variable (IV) method. For example, suspecting that selection priority is given to farmers closer to a water body, Lichtenberg and Smith-Ramirez (2011), Fleming (2017), Fleming et al. (2018), and Lichtenberg et al. (2018) used the distance of a farm to the nearest water body as an exclusion variable in the selection function. The instrument variables are more complicated in the double-selection model specified in this research because this model here not only uses two selection rules but also sequentially uses the two selection rules. Thus, $\boldsymbol{x}_1$ must contain at least one variable which satisfies the above two requirements and is not in $\boldsymbol{x}_2$. Moreover, $\boldsymbol{x}_2$ must contain at least one variable which satisfies the above two requirements but is not in $\boldsymbol{Z}$. This research uses data on farmers' information sources as the "instrument variables."

This research argues that information and education on using cover crops may affect the adoption and enrollment results on the extensive margin, but not the acreage of cover crops on the intensive margin. The acreage or the quantitative level of usage depends on the farmers' benefit-cost consideration. Once a farmer decided to use cover crops or enroll in a program, the effect of information gradually faded away. A farmer will be more likely to make decisions on the acreage base on his experience or benefit-cost analysis instead of the information. In other words, the marginal effect of information decreases sharply compared to the marginal effect in the qualitative stage. Specific information sources, therefore, can be exclusion variables.

The question on information sources is designed by summarizing open-end responses from a focus group study of farmers before the survey. There are eleven information sources for Maryland and twelve for Ohio including[13] (1) Extension Agent; (2) Seed dealers; (3) Cooperative Extension web services; (4) Soil Conservation District Office; (5) Maryland or Ohio Department of Agriculture; (6) Natural Resources Conservation Service (USDA NRCS); (7) Growers' associations[14]; (8) Farm Bureau; (9) Other farmers – neighbors within five miles; (10) Other farmers – not neighbors; (11) other information sources entered by respondents. Ohio has one additional source: (12) the Ohio no-till council. The survey asked farmers to check any sources from which they have ever received information on cover crops.

The information sources can be grouped into three categories. First, commercial and informal channels, including seed dealers, neighbor farmers within five miles, non-neighbor farmers, and other information entered by respondents. Second, government agencies including Soil Conservation District Office, state Department of Agriculture, and USDA NRCS. The government agencies play an essential role in promoting cover crops and are the primary sources of cost-share payments. The government agencies not only promote the cost-share programs but also cover farmer education and information provision generally. The third category includes non-commercial, non-government organizations including Extension Agent[15], Cooperative Extension web services, Growers' associations, and Farm Bureau. The Ohio No-Till Council is also under this category for Ohio data. They are

[13] An option to manually write down or enter other possible sources are also given.
[14] For example, Maryland Grain Producers or Ohio Corn & Wheat Growers Association
[15] Every land-grant university has an extension service that tries to bring some of the agricultural research results to farmers so that it can actually be used. For example, an extension agent can do nutrient management workshops for farmers in Delaware. So that's another source of potential information for farmers that I would think would be more likely to be educating farmers about cover crops.

civic organizations that do a combination of education for members and policy advocacy or lobbying for farmers.

Information sources differ, and so do their effects on farmers' decisions. Information from the second group, government agencies, will have a higher chance to promote cost-share programs than the other sources from the first and the second group. Thus they may have a stronger effect in affecting the farmers' enrollment outcomes in the second stage. Instrument variables used in the first stage for Maryland include seed dealer, neighbor farmers within five miles, other non-neighbor farmers, and other information sources entered by respondents. Instrument variables used in the first stage for Ohio include the growers' associations and Ohio no-till council. In addition to the information sources used in the first stage, all other information sources are used as instrumental variables for the second stage. One may suspect that the information may be endogenous to the farmers' decision in planting cover crops and enrolling in cost-share programs, i.e., only farmers who receive information from the government agencies may enroll in cost-share programs while other farmers who are unaware of the program will not enroll in the program. The model here assumes the information variables are exogenous given the long history of cover crops in the states, especially in Maryland. The various information sources may not be the optimal instrument variables to use but are the best ones available and assumed to be exogenous.

3.4 Data

Data come from a survey of potential cover crop adopters in Maryland and the northwestern region of Ohio. The potential adopters include primarily farmers who grow corn, soybeans, and small grains as commodity crops. A Freedom of

Information Act (FOIA) request was made to the Farm Service Agency (FSA) office in Maryland and Ohio, in which names and addresses of corn, soybean, and small grain farmers in the targeted counties were acquired[16]. The Maryland FSA list was combined with a list of 1,704 former participants in the MACS program and a list of approximately 1,000 members of the Maryland Grain Producers Board. After removing duplicates, the total number of unique name-address combinations was approximately 5,000. Random sampling was then used to reduce the final list to 3,223 names. The Ohio FSA list contained 7,062 names and addresses in Allen, Fulton, Hancock, Henry, Lucas, Putnam, Seneca, and Wood counties. This was reduced randomly to 5,551 for a final list. The total number of contacts was 8,774. According to the 2012 Census of Agriculture, Maryland had 2,888 corn growers and 2,511 soybeans growers; and there are 4,062 corn growers and 4,675 soybeans growers in the Ohio counties (USDA National Agricultural Statistics Service, 2014). These numbers indicate that the lists cover probably the whole population of potential cover crop users in these two regions.

The survey was conducted using an online-mail mixed-mode based on Dillman et al.'s (2014) method in January through March 2018, when farmers are relatively free of field work. For the first contact, farmers were sent a letter in a hand-addressed envelope explaining the purpose of the survey and inviting them to fill out online at Qualtrics. Approximately five days later, a second letter was sent with the survey web

[16] The original lists include many people who were not potential adopters, including retired and/or former landowners, CRP adopters, non-commodity farmers. Screening questions were used at the beginning of the survey to ensure that respondents were truly part of the intended population.

address and an enclosed prepaid postcard for those who wanted a paper copy of the survey to fill out instead. Paper surveys were sent as requested. Approximately two weeks after that, a reminder postcard was sent. Finally, approximately two weeks after the third mailing, a hard copy of the survey with a prepaid return envelope was sent to everyone who had not yet responded. A unique access code is assigned to each farmer in our sample to keep track of responses.

Farmers were asked their adoption history of cover crops, the acreage of adoption, and cost-share enrollment in 2017. Screening questions are used at the beginning of the survey asking if the respondents (1) make production decisions on any farmland, (2) make production decisions on any cropland for commodity crops, and (3) own any cropland where commodity crops are grown. A further quality control step checks data by the eligibility requirements of the cost-sharing programs. The Maryland cover crops program, the MACS, requires a minimum of five acres of cover crops such that farms smaller than five acres in MD are deemed ineligible and dropped of analysis. MACS provides cost-share of cover crop follows the harvest of corn, sorghum, soybeans, vegetables, or tobacco. Respondents who choose wheat as the only crop on their farm are also dropped out of analysis for the Maryland model. One Ohio program (TMACOG program – Portage & Toussaint watersheds) has a cap of 100 acres per producer, but applications of over 100 acres are also considered. The maximum number of acres a producer can enroll is 50 acres for the Tiffin River Sediment and Nutrient Reduction Initiative (SNRI), 25 acres for the Tiffin River and Bear Creek Watershed Improvement Plan (TRBC), and 400 acres for the Great Lakes Restoration Initiative Nutrient Reduction Program (GLRI – NRP). The acreage of cover crops for farmers enrolled in these programs are checked to ensure that data are consistent with these programs.

The screening questions leads to five types of respondents: (1) landowner-farmer with commodity crops, (2) nonlandowner-farmer with commodity crops, (3) farmer with no commodity crops, (4) landowner but not operator, and (5) unknown type as a result of missing data on the screening question. Type (5) respondents are dropped out of the analysis because these respondents are currently neither a farmer nor a farm landowner. They are not the primary research subjects to the study. These respondents also mostly give incomplete survey responses. Type (4) respondents, the landowners but not farming operators, are also left out of the following analysis for similar reasons. The non-operator landowners mostly rent out their farmlands and do not make major farming decisions. Farmers who rent lands for farming are categorized into type (2) or type (3). These respondents are not the primary research subjects for this research and mostly submitted incomplete responses. After removing type (4) and type (5) respondents and incomplete answers, the subsample includes 699 (21.69% of the finalist) farmers from Maryland and 1417 (25.53% of the finalist) from Ohio.

Table 17 summarizes cover crop adoption, acreage, and cost-share enrollment for farmers in the final sample used in the following analysis. Comparing the number of farmers that fall into each group, we can observe that Maryland has more adopters, about 76%, with the majority planted cover crops with a cost-share program. Ohio, on the other side, has fewer adopters, about 50.1% but the majority are self-adopters. Maryland also has higher acreage and larger share of farmland under cover crops compared with Ohio. Table 17 also offers a first look at the effects of cost-share programs. A policymaker or a government agency may conclude that the effect of offering cost-share payments is about 65.57% in Maryland and 49.66% in Ohio. This conclusion exaggerates the effects of the programs because it overlooks the potential self-adoptions of cover crops. As the data shows, even without any cost-share payments, some farmers still plants cover crops with a significant share of farmland (52.21% in Maryland and 32.36% in Ohio). Further, a naïve economist may recognize the self-adoption as the base for comparison and calculate the effects as the difference in the share of acreage. That is 13.36% in Maryland (65.57% - 52.21% = 13.36%) and 17.3% in Ohio (49.66% - 32.36% = 17.3%). This percentage may not be accurate because of the selection effect described in this chapter. This work here attempts to correct the potential selection effect and give a better estimation of the treatment effect.

Table 18 summarizes variables from the survey data collected on the farms (acreage, crop species, tillage, irrigation) and farmer characteristics (farming experience, gender). Considering the possibility of mixing multiple tillage types and crops, the researcher asked farmers to check all tillage types (conventional tillage, conservation tillage, no-till, and other) and crop species (corn, soybeans, wheat, other crops) planted in 2017.

3.5 Results

To construct a counterfactual for the treated and untreated observations, the researcher estimated the three-stages double-selection model four times separately for (1) Paid-adoption in Maryland; (2) Self-adoption in Maryland; (3) Paid-adoption in Ohio; and (4) Self-adoption in Ohio. The dependent variables of the first stage are the same, a dummy variable, y_1, that equals one if the farmer planted cover crops in 2017.

The second stage dependent variable y_2 is originally defined to be one if the farmer enrolled in a cost-share program in 2017 and 0 if not. The variable $y_2 = 0$ indicates self-adoption. Thus, estimates of the binary self-adoption model in the second stage are the opposites of estimates of the paid-adoption model. Because the primary interested of this research lies in predicting the share of acreage and calculating treatment effects using the continuous regression in the third stage, the following part briefly discusses the sequential bivariate probit model of cover crop adoption and cost-share enrollment.

The sequential bivariate models were estimated using the Heckprob syntax in Stata. Estimation results and predicted values are used to calculate the double-selection bias correction variables of $\hat{\lambda}_1$and $\hat{\lambda}_2$ following Tunali (1986). The following part presents and discusses the sequential bivariate regression results in the first two stages and four linear regressions in the third stage.

3.5.1 Cover crop adoption and cost-share enrollment in Maryland

Table 19 shows estimation results and the average marginal effects of variables on the probability of adopting cover crops and enrollment in cost-share programs in Maryland. The correlation between the error terms of the first stage (y_1, adoption cover crops or not) and the second stage (y_2, enrollment in a cost-share program or not) is estimated as $\rho_{12} = 0.99$ and is significantly different from 0. This result serves as evidence of the first selection. The researcher attributes this first selection to the self-selection of farmers' adoption of cover crops. On the one hand, those farmers who enrolled in the cost-share programs ($y_2 = 1$) are not randomly selected from the overall population but instead are selected from farmers who are currently using cover crops. On the other hand, those farmers who did not use cover crops ($y_1 = 0$) may not even consider applying for cost-share programs. They are probably farmers who have a strong objection in using cover crops and locate at the far right of the WTA spectrum.

Cover crop adoption varies by farmer and farm characteristics. A farmer with more farming experience is less likely to adopt and enroll in a cost-share program. The first stage probit model shows that the estimated average marginal effect of one extra year of farming experience is -0.36% for using cover crops and -0.43% for enrolling in a cost-share program. Female farmers and principal operators of the farm are less likely to report using cover crops by around 10%. However, gender and the role of the principal operator are not significant for cost-share enrollment. Irrigation rate plays a huge role in the decision to plant cover crops, and first stage regression results show that farms who irrigated 34-66% of their land are 14% more likely to plant cover crops. However, irrigation levels do not affect the results of cost-share enrollment.

Corn farmers are 15% more likely to use cover crops and 12% more likely to enroll in cost-share programs. Farmers with wheat and other crops are 8% and 6% more likely to plant cover crops, but these two species do not affect cost-sharing enrollment. One explanation is that the MACS program specified cover crop cost-share for corn, soybeans, sorghum, tobacco or vegetables but not wheat.

Farmers' experience with conservation programs plays a vital role and results show a continuous effect of these programs. Farmers who enrolled with the Conservation Reserve Program (CRP) in 2016 are 8% more likely to plant cover crops and 10% more likely to enroll in the cost-share program. Previous enrollment in cover crop programs has a significant marginal effect, 12% for planting cover crops and

29% for enrolling in cover crop program again in 2017. Failure to comply with cost-share requirements or not receiving payments has an adverse effect, which is not a surprise. Those farmers who in 2016 or earlier signed up for a cover crop program but not complete the contract nor get paid is 6% less likely to plant cover crop again. However, there is no significant effect on program enrollment. It could be that those who had a noncompliance record are not interested in applying again.

The information variables show no significant effects on both cover crop adoption and cost-share enrollment. This is not surprising and can be explained by the history of cost-share programs in Maryland. Maryland has extensive programs in promoting cover crops for over 30 years, and almost all farmers have learned about cover crops from multiple sources. The marginal effect of education and information has possibly faded away to zero, and the lack of information is not the primary obstacle for cover crop adoption in Maryland. Those farmers who have a strong objection to cover crops probably will not change their mind because of the provision of information. On the other hand, those farmers who are acquainted with cover crops will not rely on information provision either.

3.5.2 Cover crop adoption and cost-share enrollment in Ohio

Similar to the Maryland model, there is a significant selection effect in the first stage of the Ohio model (ρ_{12} = -.86). However, the planting of cover crops and cost-share enrollment show a different pattern in Ohio. 0 displays the estimation results and average marginal effects of variables on the probability of planting cover crops and cost-share enrollment in Ohio.

Farm and farmer characteristics play a different role compared to results in Maryland. Neither farming experience nor the role as a principal operator matters for planting cover crops or program enrollment. Female farmers are 25% less likely to enroll in a cost-share program but no significant difference in the probability of planting cover crops. Those completing a paper survey have a 5% less chance of planting cover crops.

Farming activities also make a difference in Ohio. Farmers who choose no-till are 8% more likely to plant cover crops, and those who use conventional tillage are 7% less likely to enroll in a cost-share program. Crop species also show different effects compared to Maryland. Planting corn has no effects, but farmers who have wheat in their production are 29% more likely to plant a cover crop. However, these farmers with some wheat are also 27% less likely to enroll in a cost-share program, which means the majority of them are funding cover crop planting by themselves. The results on other crops are similar but with smaller marginal effects. Farmers with crops other than corn, soybean, and wheat are 15% more likely to plant cover crops but 8% less likely to enroll in cost-share programs. Farmers with lower irrigation levels at 1-33% are 31% more likely to enroll in a program.

Prior experience also played a decisive role in planting cover crops. Surprisingly, failure in cover crops program in 2016 does not show any adverse effects in enrollment or planting. On the contrary, failure with cover crop programs in 2016 has a positively 14% marginal effects in planting cover crops. Without further information, it is hard to tell the reason why this happened in Ohio. A possible explanation is that participation in a cost-share program, even without payments, offers farmers in Ohio a first chance to try or test planting cover crops. Moreover, farmers tended to realize the potential private benefit from this attempt and kept planting cover crops in the following year. Other programs like the USDA commodity programs (5%) or crop insurance and cover crops program in 2016 (16%) have a positive effect in planting but not enrollment. Participation in CRP (11%) and other conservation programs (8%) have a positive effect on enrollment.

Education and information play an essential role in Ohio. This is probably because of a stronger initial marginal effect of information and education efforts in promoting cover crops in Ohio. As Bergtold et al. (2012) show, education and information play a key role in farmers' perception of positive yield benefits as a result of cover crops. Thus, information about the potential benefits of cover crops from trustworthy sources may change farmers' attitudes and introduce new adopters. The effects of information also vary by the source. Information from NRCS (8%), grower association (12%), Ohio no-till council (8%), other farmers (6%), and other manually entered information sources all contributed to the planting of cover crops. On the cost-share enrollment results, farmers who get information from seed dealers and the Ohio Department of Agriculture, show less chance to enrolling in a program (-11% and -9% respectively). This may look counter-intuitive because researches consistently show that additional information should always have a positive effect on enrollment, especially information from the department of agriculture. A possible explanation here is that, in the second stage, the counter fact of adopting cover crops with cost-share ($y_2 = 1$) is self-adoption ($y_2 = 1$). So, this negative marginal result can also be interpreted as a positive effect on self-adoption, i.e., farmers who receive information from seed dealers, and the state department of agriculture is 11% and 9% more likely to plant cover crops without any cost-share. The primary goal of seed dealers is making a profit and sell more seeds regardless of cost-share enrollment. The Ohio Department of Agriculture is promoting cover crops on a larger scale than promoting the cost-share programs. On the other side, the soil conservation office and the NRCS have positive effects on cost-share enrollment. This is probably because these two offices are the primary government agencies in charge of cost-share programs in Ohio.

3.5.3 Generalized linear model on the share of acreage

Superscripts and subscripts can distinguish the four linear regressions in the third stage in addition to equation (3.14) as:

$$share^{P}_{i,\mathrm{MD}} = \beta^{P\prime}_{3,\mathrm{MD}}\,\boldsymbol{Z}_{i,\mathrm{MD}} + \gamma^{P}_{1,\mathrm{MD}}\,\hat{\lambda}^{P}_{1i,\mathrm{MD}} + \gamma^{P}_{2,\mathrm{MD}}\,\hat{\lambda}^{P}_{2i,\mathrm{MD}} + \sigma^{P}_{3i,\mathrm{MD}} \quad \text{for } i \in \mathrm{G}_4,\ \mathrm{MD} \tag{3.15}$$

$$share^{S}_{i,\mathrm{MD}} = \beta^{S\prime}_{3,\mathrm{MD}}\,\boldsymbol{Z}_{i,\mathrm{MD}} + \gamma^{S}_{1,\mathrm{MD}}\,\hat{\lambda}^{S}_{1i,\mathrm{MD}} + \gamma^{S}_{2,\mathrm{MD}}\,\hat{\lambda}^{S}_{2i,\mathrm{MD}} + \sigma^{S}_{3i,\mathrm{MD}} \quad \text{for } i \in \mathrm{G}_3,\ \mathrm{MD} \tag{3.16}$$

$$share^{P}_{i,\mathrm{OH}} = \beta^{P\prime}_{3,\mathrm{OH}}\,\boldsymbol{Z}_{i,\mathrm{OH}} + \gamma^{P}_{1,\mathrm{OH}}\,\hat{\lambda}^{P}_{1i,\mathrm{OH}} + \gamma^{P}_{2,\mathrm{OH}}\,\hat{\lambda}^{P}_{2i,\mathrm{OH}} + \sigma^{P}_{3i,\mathrm{OH}} \quad \text{for } i \in \mathrm{G}_3,\ \mathrm{OH} \tag{3.17}$$

$$share^{S}_{i,\mathrm{OH}} = \beta^{S\prime}_{3,\mathrm{OH}}\,\boldsymbol{Z}_{i,\mathrm{OH}} + \gamma^{S}_{1,\mathrm{OH}}\,\hat{\lambda}^{S}_{1i,\mathrm{OH}} + \gamma^{P}_{2,\mathrm{OH}}\,\hat{\lambda}^{S}_{2i,\mathrm{OH}} + \sigma^{S}_{3i,\mathrm{OH}} \quad \text{for } i \in \mathrm{G}_3,\ \mathrm{OH} \tag{3.18}$$

where superscript *P* stands for paid-adoption and *S* stands for self-adoption. Subscript MD stands for Maryland model, and OH stands for Ohio model. The author started with a naïve linear regression in the third stage, which results in predicted shares of acreage below 0% or above 100% for some observations. Because the dependent variable *share* is bounded on both ends (between 0 and 1), a linear-response model is not suitable. To ensure that the predicted values fall between zero and one, the author utilized a generalized linear model (GLM) with a logit link and the binomial family (Cameron and Trivedi 2005). This allows for the dependent variables to have a logit distribution rather than a simply normal distribution, and for the logit function of the response variable (the link function) to vary linearly with the independent variables rather than assuming that the dependent variable itself must vary linearly.

Table 21 shows the average marginal effects of GLM model of equation (3.15) to (3.18), which are regressions on the intensity of cover crop usage measured by the share of acreage under cover crops to the total acreage of the farmland. Robust standard errors are calculated and reported in case the distribution family is misspecified. Recall the analysis and justification from the theoretical framework, the two groups of farmers, those in group 4 (G_4) as paid-adopters and those in group 3 (G_3) as self-adopters, may have a different pattern in using cover crops. This assumption is supported by regression results shown below. The share of acreage is estimated separately for the paid-adopters and self-adopters in Maryland and Ohio. The double-selection bias correction variables, $\hat{\lambda}_1$and $\hat{\lambda}_2$, are constructed with estimation results from the first two stages following Tunali (1986). The parameters $\hat{\lambda}_1$and $\hat{\lambda}_2$ are not significant in the Maryland models, and joint test of the two variables is not significant either. This indicates that although there is a selection effect in the first stage, there is no selection effect in the intensity of cover crop usage in Maryland. Only $\hat{\lambda}_2$ is significant in the Ohio paid-adoption model, however, $\hat{\lambda}_1$and $\hat{\lambda}_2$ are jointly significant in both Ohio models. Thus, there is a significant selection effect in both the first stage and the second stage in the Ohio model.

As suspected, the two subgroups of cover crop adopters in the two states have very different patterns in the intensity of cover crop usage. Some independent variables show not only a different magnitude of average marginal effects but also opposite signs. First, total farm acreage has negative or insignificant effects on the share of cover crops. The acreage share drops by one percentage points for every 100 acreage increase in farm size in the Maryland paid-adopter group and the same marginal effect in the Ohio self-adoption group. These two subgroups have more farmers than each of their opposite sub-group. This is the opposite of Dunn et al. (2016) who found that larger operations usually adopt a larger share of farmland. However, farm size does not show any significant effects for the self-adopters in Maryland and paid-adopters in OH. Second, commodity farmers who get cost-share payments, both renters and owners of farmland plant more cover crops in Maryland (26 percentage points more for owners and 30 percentage points more for renters) than non-commodity farmers. However, the paid farmland owners in Ohio plant 50 percentage points less while the renters do not have any significant difference to non-commodity farmers. The average marginal effects for self-adopters are significantly negative for commodity farmers in Maryland. The estimated effect is over 100% and this is probably a result of the small sample and extreme values in the subgroup. When the author re-estimated a simple OLS model with the same independent variables, no significant effects are found for being a commodity farmer in Maryland.

Third, cover crop adoption intensity varies by crop species and tillage choices. Note that the survey questions on crop species and tillage choices are multiple choices questions. Thus, the marginal effects of these variables should be interpreted as having a crop species or tillage type versus not having. For example, the paid-adopters who planted corn in Maryland have 14 percentage points less share compared to those who did not plant corn. Soybeans farmers and farmers with other crops planted 12 and 7 percentage points, and wheat farmers do not show any significant variance. This is a result of policy selection because wheat farmers are not eligible for the Maryland cost-share program. Crop species does not affect the adoption intensity for Ohio paid-adopters. However, corn farmers and soybean farmers who enrolled with cost-share in Ohio plant 10 and 25 percentage points fewer cover crops respectively. Tillage type does not show any variance in the Maryland model. However, farmers with conventional tillage use cover crops on 12 percentage points less farmland for both subgroups in Ohio. Farmers who used conservation tillage also have 5 percentage points less share of land with cover crops.

Experience with conservation programs also shows different effects in Maryland and Ohio. Farmers enrolled with USDA commodity programs or crop insurance in the Maryland paid-adoption group tends to have a 5 percentage higher share, but no significant effects found in the self-adoption group, neither in Ohio. However, experience with cover crop programs shows a significant and positive effect for the self-adopters in Ohio even it was a negative experience. To be more specific, farmers who enrolled in a cost-share program before 2016 but not in 2017 have 10 percentage points more land with cover crops, and those who enrolled but somehow not received any payments in 2016 or earlier also have 8 percentage points higher share. This could also be explained by the decreasing marginal effects of the cost-share programs and the relative scarcity of cost-share experience in Ohio. In states that are progressive with cost-share programs, such as Maryland, farmers probably have more experience with one of the cost-share programs. The familiarity with cost-share programs reduces the effect of previous experience. However, in states with relatively fewer efforts, such as Ohio, these programs may change the mind of some farmers and help them understand the potential private benefits of cover crops such that they are willing to adopt a larger share of their land even without cost-share.

Income levels show less variation while irrigation levels show more variance in different groups. Paid farmers with lower irrigation levels turn out to have less share of farmland with cover crops in both Maryland and Ohio. The average marginal effects of no irrigation and irrigation under 33% in Ohio is higher than 100%, which is also possibly a result of extreme values and small sample. OLS regression shows no significant estimates of these two variables. However, self-adopters in Maryland have a higher share of land with lower irrigation levels. All three dummy variables of *IRRIGATENONE, IRRIGATE13, IRRIGATE23* have significantly positive effects on the share of farmland. This is possible because these self-adopters have a strong preference in conserving their farmlands and would use less irrigation. However, these variables are not significant in Ohio.

3.5.4 Treatment Effects

The upper panel of Table 22 first shows the average predicted share of acreage for farmers in G_3 and G_4 from the two states. The predictions are conditional on observed variables and unobserved factors reflected in $\hat{\lambda}_1$ and $\hat{\lambda}_2$. Three conditional expectations are predicted for each farmer in G_3 and G_4: (a) share of acreage if without any cost-share, estimated by regression results of equation (3.16) if a Maryland farmer and equation (3.18) if an Ohio farmer; (b) Share of acreage if enrolled in the Maryland cost-share programs, estimated by equation (3.15); and (c) Share of acreage if enrolled in the Ohio cost-share programs, estimated by equation (3.17). For the self-adopters in G_3, estimates of (b) and (c) stand for the counterfactual outcomes. For paid-adopters in G_4, estimates of (a) represent the counterfactual of no enrollment. For Maryland paid-adopters, estimates of (c) represent the counterfactual of enrolling in an Ohio cost-share program. While for Ohio paid-adopters, estimates of (c) represent the counterfactual of enrolling in a Maryland cost-share program. Average value of prediction (a), (b), and (c) for each observation is calculated and reported in the first three rolls of Table 22 respectively.

The treatment effects are calculated as the difference in predicted values. As the author argues previously, with selection effects controlled, the share of acreage as self-adoption should be controlled as the counterfactual of no-treatment. The benefits of this control lie in the following two points. First, controlling self-adoption as the base of comparison leaves the non-adopters out of the treatment effect calculation. The non-adopters have a much higher rate of WTA, and the cost-share programs will not change their decision on cover crops with the current rate of payments. The intensity of adoption is 0 for them and including the non-adopters in the control group will lead to an underestimate of the average intensity. The researcher believes that a correct estimation of the treatment effect on the intensity of adoption should only include the current adopters but not non-adopters. Second, recognizing the history of promoting cover crops as the status quo and the discussion on non-additional behavior in the current researches, this research estimates self-adoption intensity as the base. This controls non-additional behaviors because the self-adoption rate is what the farmers would have done without the cost-share program. The lower panel of Table 22 shows the average treatment effects of the group.

A within-sample prediction, which is calculated by fitting data for farmer $i \in G_4$ in Maryland generates the predicted share of acreage as a result of the cost-share program. Average share across all $i \in G_4$ in Maryland equals 65.48%. The counterfactual, as the share of acreage, if they did not enroll in a cost-share program is estimated as an out-of-sample prediction. This counterfactual of the treated farmers is calculated by fitting data for farmer $i \in G_4$ in MD to equation (3.16). The average predicted value is 42.64%. The difference between the two predicted values explained above is interpreted as the treatment effect on the treated group, and the average treatment effect of the cost-share programs on the treated farmers (ATT) is 21.85%. Further, Table 22 refers to this ATT as in-state ATT. This means that the paid-farmers, on average, increased cover crops planting by 21.85% of their operating farmland. Similarly, as the control group, a within-sample prediction gets the self-adopters' average share of 51.53%. Fitting data for farmer $i \in G_3$ in Maryland into equation (3.15) would get the counterfactual outcome of the self-adopters if they enrolled in the Maryland program. The average predicted value is 65.25%, and the average difference between the two predicted values is 13.72%. This is referred to as the average treatment effect of the untreated (ATU), and it means that if the self-adopters in Maryland can enroll in the cost-share programs, they will, on average, plant cover crops on additional 13.72% of their farmland. By the same means, the ATT in Ohio is 19.03%, and the ATU is only 0.85% and not significant. The lower ATT and insignificant ATU can be attributed to the low payment rate and acreage cap in the Ohio programs.

Comparing the ATT from Maryland (21.85%) and Ohio (19.03%) estimated here with the naïve results from Table 17 (13.36% in Maryland and 17.3% in Ohio), the author observes higher treatment effects from the double selection model. This is a result of calculating the treatment effects in two different ways. The naïve results summarized from Table 17 is simply a difference of observed action from two groups of farmers. This simple difference overlooks the differences in almost all other aspects of the farmers in the two groups. The double selection model used in this work, however, not only controls all available characteristics of the farmers and farms but also predicts a counterfactual or "what if" outcome that is otherwise unobservable. The treatment effect is calculated as the difference between what they have done and what would have done if they are in the opposite group.

As stated before, another empirical contribution of this research is to compare the effects of local cost-share programs in Maryland and Ohio. The Maryland program has a higher payment rate and no acreage cap. So, policymakers in Ohio may wonder what the predicted acres would be if they changed the Ohio cost-share program to one similar to the Maryland program. Fitting data of farmer i, $i \in G_4$ in OH, into equation (3.15), we get an average share of 59.14%. Compared with the self-adoption rate, the average out-of-state ATU is 27.74%. This indicates that if an Ohio farmer, who currently enrolled in the Ohio programs can get a higher per-acre cost-share payment without a payment cap on acreage, they would plant cover crops on additional 27.74% of their farmland. This increment is higher than the within-state ATT of 19.03% and can be attributed to the increase of cost-share payments. Fitting Ohio self-adopter data into equation (3.15), the average out-of-state ATU is similar, at 26.63%. Compared with the insignificant in-state ATU of 0.85%, this estimated treatment effect is much stronger.

On the other side, in case that policymakers in Maryland want to know the potential results of reducing cost-share payments, one may fit the model from Maryland farmers' data into the Ohio model. The predicted out-of-state ATU is 11.03%, which is lower than the in-state ATT of 21.85% for the enrolled farmers and no insignificant effect found for the self-adoption farmers. This indicates that farmers who currently enrolled in the Maryland program may reduce their land under cover crop by approximately 10.82% when the cost-share payments they receive decreases.

3.5.5 Alternative modeling results

Three reasons motivated the use of a double-selection model instead of a single selection model, as used by Fleming (2017). First, data from this survey provide information on both cover crop adoption and cost-share enrollment status. Thus, the researcher can classify farmers into three subgroups, instead of two subgroups (enrolled farmers versus non-enrolled farmers) as in a single selection model. The additional data ensure the possibility of a more sophisticated model. Second, the more sophisticated three-stage double-selection model can predict the intensity of cover crop usage more accurately and thus generate a better estimation of the treatment effects. The two variables, $\hat{\lambda}_1$and $\hat{\lambda}_2$, generated from the first two stages, not only corrects the potential selection effects in the third stage, but also controls unobservable factors that contributes to the adoption decisions in the first stage and the enrollment status in the second stage. Third, the model further distinguished the non-enrolled farmers as self-adopters and non-adopter. As stated above, the researcher believes that there is a non-negligible difference in the non-adopters and self-adopters. Self-adoption of cover crops should be controlled as the baseline for comparison to exclude non-additional behaviors. To investigate whether the double-selection model can provide better predictions, the author estimated two additional models, a single selection model and a GLM model without any correction of selection bias.

Table 23 shows average treatment effects from two-stage single selection models and GLM models. The single selection models run a probit regression in the first stage and a GLM model in the second stage with an inverse-mills ratio as a correction variable to potential selection bias, which is derived from the first stage. The first stage uses either paid-adoption (variable y_2) or self-adoption for observations in the sample as the selection criteria. The rationale behind the paid-adoption model is that farmers who enrolled in the cost-share programs are not randomly selected from the population and the survey data only reveals the intensity of cover crop usage only for enrolled farmers. Similarly, the self-adoption model suspects a potential selection bias because these farmers adopted cover crops as voluntary actions. Information variables serve as exclusion variables which are used as regressors in the first stage but not in the second stage. The second stage in these two models runs a GLM regression the share of farmland under cover crops. With two single selection models for the two sample of farmers in Maryland and Ohio, the author re-estimated four share functions as equation (3.15) to equation (3.16) and constructed ATT and ATU with the same method as those shown in Table 22. Following the same structure, simple GLM models on farmland share without correcting selection bias are also estimated on paid-adoption and self-adoption farmers.

Comparing results from Table 22 and Table 23, the author finds that the estimated in-state ATTs in the Maryland model (21.84%) and the Ohio model (19.03%) from the double-selection model are consistently higher compared to those from the single selection models (18.97% in Maryland and 17.44% in Ohio) and simple GLM models (19.33% in Maryland and 16.97% in Ohio). So, without considering any selection (using a simple GLM model), or only considering the program enrollment selection (using a single selection model), the alternative models underestimate the cost-share programs on the treated farmers. This is because of the unobservable factors or variables that contribute to both the decision of planting (or not planting) cover crops and the adoption intensity. Even though the two alternative models followed similar grouping of farmers and constructed counterfactual predictions in the same means, there are still unobservable factors which contribute to a higher intensity of cover crops usage from the program participants. These factors are not controlled in the simple GLM model or the single selection model. This also explains the result that the in-state ATU is similarly higher in Maryland from the double-selection model, although the difference is smaller than the difference in the ATT.

However, the estimated in-state and out-of-state ATU show opposite results. Recall that ATU stands for average treatment effect on the untreated farmers which are self-adopters. The treatment effects are calculated as the difference between the self-adoption share of farmland, which is a within-sample prediction, and the predicted share if they are enrolled in a cost-share program. The latter predicted values are out-of-sample predictions. The Maryland out-of-state ATU, which is estimated with the Ohio paid-adoption equation is lower than the two alternative models. The Ohio in-state ATU, which is also predicted with the Ohio paid-adoption equation, show lower ATU, too. However, the Ohio out-of-sample ATU, which is estimated with the Maryland paid-adoption equation, similar to the Maryland in-state ATU, is higher than the simple GLM and the single selection model results. Thus, the ATUs results show that the Maryland double selection model tends to give a higher estimated share than the GLM and single selection model while the Ohio one tends to give a lower estimation.

3.6 Conclusion

This paper estimated the effect of cost-share payments on the intensity of cover crops usage in 2017 using survey data of Maryland and Ohio farmers. The share of farmland planted with cover crops is used as the measurement of production intensity. The dichotomous adoption status and cost-share enrollment outcome classify farmers into subgroups. The treatment group includes paid-adopters who planted cover crops with cost-share payments. Different from previous research, this paper only includes self-adopters in the control group but no non-adopters. This setting assumes a significant difference in cover crops usage between the non-adopters and self-adopters. The self-adoption rate predicts what the treated farmers would have done without the cost-share payments, which are recognized as non-additional shares. Thus, the treatment effects calculated are additional shares of farmlands that results from the cost-share treatment. The non-adopter and noncompliance farmers are not distinguishable from the survey data and are combined into a single group as the non-adopters. This incomplete three-group instead of four-group design is fitted with a double-selection model to generate a consistent estimation of the farmland share equation.

The three-stage double-selection model with incomplete classification is used to correct the selection bias resulting from the non-random distribution of farmers in the treatment and the control group. The first selection rule addresses the self-adoption of cover crops and early adopters. The second selection rule addresses potential selection process in the cost-share program enrollment outcome, which is a combined result of both the farmers' self-selection in enrollment and selection of government agencies. The selection model is built on the assumption that education and information drive farmers to adopt cover crops, but the share of farmland or the intensity of adoption depends on the farmers' consideration of private benefits and costs, instead of information. Thus, information sources are used as instrumental variables in the first and second stage.

Significant selection effects were detected between the first stage and the second stage, which supports the arguments that a selection process exists in the enrollment outcome because of farmers' voluntary actions of planting cover crops. Although the double-selection bias is tested only to be significant in the Ohio model, the correction variables constructed with the residuals from the first two stages also control unobservable factors in the third stage. The major results show that the cost-share program in Maryland is estimated to increase cover crops coverage, on average, by 21.85 percentage points of farmland for the enrolled farmers, while the Ohio programs have a smaller effect of 19.03 percentage point, possibly due to the lower payment levels and a payment cap. Note that the baseline payments in Maryland start from $45, which almost double the baseline payments of $25 to $35 in Ohio programs. However, the treatment effect in Maryland does not double. This can be evidence of decreasing marginal effect in payments. However, this does not mean that Ohio cannot benefit from a higher payment.

Because the double-selection models are constructed separately for Maryland and Ohio, this work also conducted a cross-state prediction on how the adoption share change if Maryland or Ohio switch to a different cost-share program. Predictions show a 27.74 percentage points increment in farmland shares if Ohio can adopt a Maryland program, which majorly pays about $20 more without a cap. Thus, the enrolled farmers may plant cover crops on additional 8.71 percentage points of their farmlands. However, this increment is less than the increments in the per-acre payments, and it also shows decreasing marginal effects. While the Maryland farmers who enrolled in the current MACS program may decrease their share of land from 65.48% to 54.87% if Maryland switches to an Ohio program, which pays only $25 to $35 as the baseline and restricts the maximum acreage of enrollment. This reduction in farmland share shows the necessity of maintaining the current payment level.

The treatment effects on the untreated farmers or the self-adopters are much lower. The in-state ATU shows a 13.72 percentage points increase in Maryland but no significant increase in Ohio. This means that with substantial self-adoption in place, the government programs should target non-adopters and avoid offering cost-share to the existing adopters, especially in Ohio. Because even these self-adopters in Ohio get additional payments for their cover crop usage, they would maintain the current farmlands and would not increase the share of farmlands. For Maryland self-adopters, possibly due to the higher payments, they would increase their shares but with a smaller increment. However, they would not plant any additional cover crops if the payments are lower. This is supported by the insignificant out-of-state ATU in Maryland. If Ohio can not only offer more payments to self-adopters but also higher payments, Ohio self-adopters would actually plant more cover crops, by about 26.63 percentage point. Although this increment is slightly lower than the treated farmers, it is much higher than the in-state ATU, which is insignificant. This indicates that the self-adopters in Ohio may not plant cover crops on extra farmland with the current low payment of $25 to $35, but will plant more given the MACS program, which pays more per acre without a payment cap.

Although focusing on making predictions, this research also draws insights on factors affecting farmers' decisions in adopting cover crops, cost-share enrollments and the intensity of adoption. A key highlight from the comparison the differential effects of information in the two states shows that although information and education of cover crops are praised in the research literature as a low-cost approach in promoting cover crops, the actual effects may fade away, especially in states with a long history of promoting cover crops, such as Maryland. Some information variables show significantly positive effects in Ohio, but almost no effects in Maryland. This conforms with the author's assumption that although the initial information can change the farmers' mind or attitudes toward cover crops at an earlier stage, the effects may fade away. Because farmers may try the practice at an earlier stage, but after trial and error, farmers who find the cost of adoption exceeding the private benefits will probably stop planting cover crops.

A limitation of this research is the lack of information on geographical data. As previous research shows, the cost-share enrollment outcome depends on the location of the farm because the government may give more priority to farms closer to a watershed. Future research may incorporate geographical data, and future, climate data into the analysis.

TABLES

Table 1. Parameterization

Parameter	Value	Explanation
I	4	The number of rings in a city
J	5	The number of landowners or participants in a group
T	4	The number of periods in a life cycle or an experiment round
α_0	200	Multiply coefficient used in the revenue equation
α_1	10 for *Type1* 14 for *Type2* 6 for *Type3* 3 for *Type4* -60 for *Type5*	Multiply coefficient used in the cost equation
α_2	100 for *Type1* 110 for *Type2* 118 for *Type3* 121 for *Type4* 190 for *Type5*	Multiply coefficient used in the cost equation
β	1.6	Power coefficient used in the revenue equation
λ_1	20	Multiply coefficient used in the land value equation, the multiplier for the number of bricks on the location calculated
λ_2	10	Multiply coefficient used in the land value equation, the multiplier for the number of bricks on the location one ring inward
λ_3	5	Multiply coefficient used in the land value equation, the multiplier for the number of bricks on the location one ring outward
λ_4	0	Multiply coefficient used in the land value equation in the independent sessions, the multiplier for the number of bricks on the locations on the left or right
	5	Multiply coefficient used in the land value equation in the interdependent sessions, the multiplier for the number of bricks on the locations on the left or right
τ_{LVT}	0.9	Tax rate on land value under LVT
τ_{UPT}	0.2	Tax rate on land value and improvement value under UPT

Table 2. Optimal choice sets and Resulting Earnings and Compactness

	Type1 (Blue)	*Type2* (Red)	*Type3* (Green)	*Type4* (Purple)	*Type5* (Yellow)
Optimal Choice set					
Under LVT	{1,1,2,2}	{1,1,2,3}	{1,1,2,3}	{1,2,1,3}	{1,2,3,4}
Under UPT	{1,2,3,4}	{1,2,3,4}	{1,2,3,4}	{1,2,3,4}	{1,2,3,4}
Independent Treatments					
LVT earnings	384.10	334.59	310.59	308.41	299.53
LVT earnings treatment effect	+128.17	+29.19	-18.81	-33.09	-40.83
Road Compactness (*Comp*)					
Under optimal LVT choice	18	17	17	17	12.5
Under optimal UPT choice	12.5	12.5	12.5	12.5	12.5
Fixed payment at end of round	20.1	159.4	183.4	195.5	168.7

Note: Original work by authors to design a parameterized model. Because of intertemporal design of the model, the optimal choice for a landowner is a set of ordered locations.

Table 3. Structure of Data and Experiment Sessions

Session #	Treatment	# of "Cities" of 5	Total number of participants	"Paid" rounds	Periods	Individual decisions on bricks	Voting periods
1	Order1, TypeDistA	2	10	16	64	320	32
2	Order1, TypeDistB	2	10	16	64	320	30
3	Order2, TypeDistC	2	10	16	64	320	30
4	Order2, TypeDistA	2	10	16	64	320	36
5	Order3, TypeDistB	2	10	16	64	320	30
6	Order3, TypeDistC	2	10	16	64	320	30
7	Order4, TypeDistA	2	10	16	64	320	31
8	Order5, TypeDistB	2	10	16	64	320	36
9	Order6, TypeDistC	2	10	16	64	320	30
	Sum	18	90	144	576	2,880	285

Source: Original work by authors. Note: Odd sessions had positive-framed information treatments, while even sessions had negative framing. Voting periods for a city vary from 6 to 24 based on the city's choice. The order indicates, in effect, whether UPT or LVT was played first across two treatments (round 1, 3, 5, 7). Order 1 was UPT first, always, while Order 2 was LVT first, always. Order 3 had the following first tax institutions: UPT, LVT, UPT, LVT. Order 4 is LVT, UPT, LVT, UPT. Order 5 is UPT, UPT, LVT, LVT. Order 6 is LVT, LVT, UPT, UPT.

Table 4. Experiment Data on Tax Revenue, Group Earnings, and Compactness

		Predicted		Observed by *LVTPeriods*				
		UPT	LVT	0 UPT (number of rounds)	1	2	3	4 LVT (number of rounds))
City Tax Revenue	*TypeDistA*	1708.4	2050.5	1742.2*** N=37	1868.4 N=3	-	-	2044.1 N=8
	TypeDistB	1708.4	2091.0	1760.8 N=10	-	-	-	2078.8*** N=38
	TypeDistC	1708.4	2103.1	1760.5 N=6	-	-	-	2096.0** N=42
Group Earnings	*TypeDistA*	845.5	910.2	833.3** N=37	709.7 N=3	-	-	868.8 N=8
	TypeDistB	909.6	1143.3	880.3* N=10	-	-	-	1115.9*** N=38
	TypeDistC	999.4	1394.3	954.2 N=6	-	-	-	1360.4*** N=42
CityComp	*TypeDistA*	13.5	16.5	12.7*** N=37	12.7 N=3	-	-	16.2** N=8
	TypeDistB	13.5	17.4	12.9** N=10	-	-	-	17.3* N=38
	TypeDistC	13.5	17.6	12.8*** N=6	-	-	-	17.5 N=42
			Rounds	53	3	0	0	88

Source: Original data collection by authors in nine experiment sessions. Notes: All data at the city level for five participants. Stars reflect t-statistics testing for differences between predicted values and values observed in UPT sessions (*LVTPeriods*=0) and LVT sessions (*LVTPeriods*=4); *** p<0.01, ** p<0.05, * p<0.1. The observed data also include the number of rounds (N) in each realization of *LVTPeriods*. *CityComp* = the average of compactness of five roads in the city.

Table 5. Robust OLS Regression Explaining *GroupEarnings* and *CityComp*

Variables	*GroupEarnings*	*CityComp*
LVTStart	-11.89	-0.08
	(9.49)	(0.08)
LVTPeriods	48.85***	1.02***
	(6.54)	(0.04)
Vote	111.47**	0.53*
	(51.31)	(0.28)
PosInfo1	-24.63	-0.08
	(38.17)	(0.26)
PosInfo2	-15.70	0.14
	(52.44)	(0.35)
NegInfo1	-18.70	-0.15
	(34.81)	(0.33)
NegInfo2	-20.52	0.06
	(53.20)	(0.36)
TypeDistB	-122.52***	-0.02
	(42.59)	(0.25)
TypeDistA	-275.65***	-0.66***
	(51.29)	(0.22)
*TypeDistB*Vote*	-130.95**	-0.43
	(50.52)	(0.34)
*TypeDistA*Vote*	-107.67	-0.06
	(66.23)	(0.34)
*TypeDistB*PosInfo1*	-3.16	0.24
	(48.82)	(0.29)
*TypeDistB*PosInfo2*	-13.75	0.07
	(27.33)	(0.26)
*TypeDistB*NegInfo1*	-13.64	0.47
	(35.72)	(0.36)
*TypeDistB*NegInfo2*	-13.06	0.26
	(28.43)	(0.24)
*TypeDistA*PosInfo1*	42.62	-0.13
	(40.85)	(0.32)
*TypeDistA*PosInfo2*	8.35	-0.31
	(35.95)	(0.29)
*TypeDistA*NegInfo1*	58.64	-0.07
	(35.44)	(0.31)
*TypeDistA*NegInfo2*	2.97	-0.31
	(41.91)	(0.22)
Round	12.40	-0.02
	(12.07)	(0.08)
Constant	1,015.13***	13.06***
	(42.88)	(0.21)
Observations	144	144
R-squared	0.92	0.92

Source: Original data collection by authors. Notes: Model cannot use fixed effects because of design treatments. *TypeDistC*, no voting, no information, and a UPT-period start are the reserved categories. *** p<0.01, ** p<0.05, * p<0.1; Robust standard errors in parentheses.

Table 6. Average Votes for LVT by Treatment

	Vote with no information (t-stat)	*PosInfo1* (t-stat)	*PosInfo2* (t-stat)	*NegInfo1* (t-stat)	*NegInfo2* (t-stat)
TypeDistA (predicted 2)	1.88 N=34 (-0.70)	1.67 N=21 (-2.32)**	1.90 N=21 (-1.00)	1.60 N=10 (-1.81)*	2.00 N=13 (0.00)
TypeDistB (predicted 3)	2.75 N=36 (-1.60)	3.50 N=10 (2.24)*	3.50 N=10 (2.24)*	3.60 N=20 (3.94)***	3.70 N=20 (4.27)***
TypeDistC (predicted 4)	4.13 N=30 (0.94)	4.70 N=20 (6.66)***	4.45 N=20 (3.33)***	4.00 N=10 (0.00)	4.30 N=10 (1.96)*
Voting Periods	100	51	51	40	43

Note: 6 out of the 8 rounds include a voting mechanism that endogenously determines the tax plan used in following periods. Each of the 6 rounds could have 1 to 4 period(s). The average group votes for LVT are calculated by type distribution and information treatment. The number of periods with votes is listed underneath the average group votes (N). There is a maximum of 4 voting periods in any round, but there may be fewer. For this reason, the number of voting periods is not the same across treatments. *** p<0.01, ** p<0.05, * p<0.1.

Table 7. Regression Explaining Group Votes for LVT in any Period with Voting

Variables	*LVTVotes* Model 1	*LVTVotes* Model 2
LVTStart	0.858***	0.817***
	(0.109)	(0.101)
TypeDistB	1.089***	0.637***
	(0.123)	(0.194)
TypeDistC	2.042***	1.807***
	(0.125)	(0.218)
PosInfo1	0.718***	0.191
	(0.196)	(0.237)
PosInfo2	1.033***	0.718**
	(0.333)	(0.341)
NegInfo1	0.606***	0.202
	(0.231)	(0.304)
NegInfo2	1.108***	0.757*
	(0.344)	(0.418)
Round	-0.178**	-0.168**
	(0.0818)	(0.0833)
TypeDistBxPosInfo1		0.784***
		(0.296)
TypeDistBxPosInfo2		0.492*
		(0.286)
TypeDistBxNegInfo1		0.822***
		(0.309)
TypeDistBxNegInfo2		0.704**
		(0.318)
TypeDistCxPosInfo1		0.763***
		(0.254)
TypeDistCxPosInfo2		0.323
		(0.263)
TypeDistCxNegInfo1		0.103
		(0.340)
TypeDistCxNegInfo2		0.0837
		(0.332)
Constant	1.990***	2.212***
	(0.298)	(0.326)
Observations	285	285
R-squared	0.734	0.754

Source: Original data collection by authors. Notes: Model cannot use fixed effects because of design treatments. Model corrects for heteroskedasticity. *TypeDistA*, no voting, no information, and a UPT-period start are the reserved categories. Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table 8. Average Marginal Effects from Logistic Regression to Explain Individual LVT Support and Earnings-Rational Voting

Variables	All types	Type1	Type2	Type3	Type4	Type5
				VoteForLVT		
PosInfo1		0.14** (0.06)	0.26*** (0.08)	0.22*** (0.08)	-0.01 (0.06)	0.02 (0.11)
PosInfo2		0.19*** (0.06)	0.29*** (0.09)	0.27** (0.13)	0.17 (0.12)	NA
NegInfo1		0.16** (0.06)	0.20** (0.09)	0.07 (0.08)	0.15* (0.07)	NA
NegInfo2		0.17** (0.07)	0.28*** (0.10)	0.39*** (0.12)	0.20* (0.11)	NA
LVTStart	0.21*** (0.02)					
Round	-0.03** (0.02)					
Period	-0.00 (0.00)					
				ExpectedVote		
PosInfo1		0.00 (0.03)	0.03 (0.07)	-0.24*** (0.08)	0.00 (0.05)	-0.02 (0.07)
PosInfo2		0.03 (0.04)	-0.04 (0.11)	-0.30** (0.13)	-0.16 (0.11)	NA
NegInfo1		0.02 (0.03)	-0.02 (0.08)	-0.11 (0.09)	-0.19 (0.08)	NA
NegInfo2		-0.03 (0.07)	-0.06 (0.12)	-0.47*** (0.12)	-0.25* (0.13)	NA
LVTStart	-0.03 (0.02)					
Round	0.04** (0.02)					
Period	-0.05*** (0.01)					

Source: Original data collection and analysis by authors. Notes: Marginal effects are derived from logit models (not reported) where no information and a UPT-period start are the reserve categories. The logit models had controls on treatments, types, and interactions with types. Because *Type5* had no variation in the dependent variables for *PosInfo2*, *NegInfo1*, and *NegInfo2*, there are no marginal effects for these three treatments and the reported N=1,381 rather than N=1,425. STATA reports delta method standard errors in parentheses, testing whether the treatment effect is significant. *** p<0.01, ** p<0.05, * p<0.1

Table 9. PES program evaluation criteria

Criteria	Description	Studies
Benefit achieved	Vary by context, could be abatement, environmental benefits, and water quality improvements	Boxall et al. 2013 Fooks et al. 2015 Iftekhar and Tisdell 2014
Participation	Measured by the likelihood of participating under a mechanism, or the number of participants or projects enrolled	Arnold et al. 2013 Boxall et al. 2013 Fooks et al. 2016 Palm-Forster et al. 2016 Whichmann et al. 2016
Information rents (bid, or seller profit)	Payments received minus the actual adoption costs	Arnold et al. 2013 Boxall et al. 2013 Cason et al. 2003 Cason and Gandadharan 2005 Duke et al. 2017 Iftekhar and Tisdell 2014 Messer et al. 2017 Schilizzi and Latacz-Lohmann 2007, 2012 Whichmann et al. 2016
Policy effectiveness	Percentage of benefits (pollution abatement) realized relative to the maximum possible amount	Cason et al. 2003 Cason and Gandadharan 2005 Schilizzi and Latacz-Lohmann 2012
Economic efficiency	Cost per unit of benefit achieved. Calculated as participants or adoption costs divided by benefits	Palm-Forster et al. 2016 Schilizzi and Latacz-Lohmann 2007, 2012
Budgetary cost-effectiveness	Payments per unit of benefit achieved (or benefits achieved per dollar spent)	Boxall et al. 2013 Palm-Forster et al. 2016 Schilizzi and Latacz-Lohmann 2007, 2012
Percentage of Optimal Cost-Effectiveness Realized	Benefits per dollar spent as a percentage of benefit per dollar spent in the maximal benefits (optimal) case	Cason et al. 2003 Cason and Gandadharan 2005 Conte 2017

Note: The last column listed some related studies that used the criteria shown in the first column. Not all studies are listed due to limited table space.

Table 10. Research questions and hypotheses

Research question	Hypothesis	Conclusion
Are auctions more cost-effective under all levels of flexibility relative to fixed payments?	Auctions are more cost-effective under all levels of flexibility relative to fixed payments	Rejected. Auctions are not more cost-effective, in terms of external benefits acquired, when there is only one option.
Do auctions generate fewer information rents under all levels of flexibility relative to fixed payments?	Auctions generate fewer information rents under all levels of flexibility relative to fixed payments	Rejected. Auctions result in fewer information rents on average when more options are offered to farmers.
Does treatment with higher levels of flexibility improves cost-effectiveness and reduces information rents for both auctions and fixed payments?	Treatment with higher levels of flexibility improves cost-effectiveness and reduces information rents for both auctions and fixed payments?	Rejected. Giving more options can always increase the external benefits acquired by also increase the total information rents generated.
Does flexibility affect auctions and fixed payments similarly?	Flexibility affect auctions and fixed payments similarly	Rejected. Rejected. (Details explained by treatment effects in the results part)
Do larger budgets result in higher information rents?	Larger budgets lead to higher information rent	Accepted.
Do higher levels of flexibility lead to higher non-additionality?	Flexibility leads to higher non-additionality	Accepted. Participants are more likely to choose a non-additional option under more flexible schemes
Does the treatment of flexibility change participants' offer behaviors?	Participants over-offer more (higher information rents) under more flexible cases	Accepted. Participants' offers are higher under a 3-paid case but not under a 9-paid case.
Does the level of budget change participants' offer behaviors?	Participants over-offer more (higher information rents) under a larger budget	Accepted. Participants make higher offers when observing a higher budget.
Does the value of EB change participants' offer behaviors?	Participants over-offer more (higher information rents) for higher EB options	Accepted. Participants make higher offers for higher EB options.

Table 11. Treatment design

Payment mechanism	Contract flexibility		
	One-paid option (least flexible)	Three-paid options	Nine-paid options (most flexible)
No payment (NP)	N/A	N/A	N/A
Fixed payments (FP)	FP1	FP3	FP9
Discriminatory Price Auction (DPA)	DAP1	DPA3	DPA9

Table 12. Adoption scenarios

	Self-adopt	Pay-to-adopt
Additional choice	A	C
Non-additional choice	B	D

Note: Self-adopt defined as those who adopt an option without any payment (participants can choose any option regardless of eligibility for payment); Pay-to-adopt defined as those who adopt an option and get paid; Additional choice defined as choices with positive NPC; Non-additional choice defined as choices with negative NPC.

Table 13. Group level data analysis

Independent Variable	Dependent Variable			
	Total EB (1)	Total additional EB (2)	Information Rent (3)	Social welfare (4)
DPA	-50.57 (58.94)	293.77*** (80.47)	37.64 (31.10)	-148.51*** (40.66)
3-paid	402.17*** (44.65)	427.66*** (60.97)	200.96*** (23.56)	205.51*** (30.80)
9-paid	417.11*** (57.38)	216.05*** (78.33)	260.79*** (30.28)	267.50*** (39.58)
*DPA*3-paid*	-427.38*** (47.50)	-436.45*** (64.85)	-190.73*** (25.06)	-195.29*** (32.77)
*DPA*9-paid*	-435.18*** (46.52)	-332.32*** (63.50)	-207.18*** (24.54)	-255.99*** (32.08)
Budget	.14 (.16)	.37* (.22)	.80*** (.08)	.42*** (.11)
Budget^2	.0001* (.0000)	.0001 (.0001)	-.0002*** (.0000)	-.0002*** (.0000)
*DPA*Budget*	.25*** (.05)	-.05 (.07)	.12*** (.03)	.17*** (.04)
Non-additional option	-27.02 (44.42)	-396.50*** (60.64)	320.97*** (23.44)	218.13*** (30.64)
Experience	−4.42 (1.85)	-10.02*** (2.53)	2.07** (.97)	-.68 (1.28)

Note: One, two, and three asterisks indicate 10%, 5%, and 1% significance for a two-tailed hypothesis based on a t-distribution, respectively. Standard errors reported in parentheses.

Table 14. Individual Behavior—Option Choice

	Pooled	NP	FP	DPA
NPC	-.0012*** (.0001)	-.0025** (.0001)	-.0012*** (.0003)	-.0011*** (.0002)
EB	.0006*** (.0000)	.0017*** (.0004)	.0006*** (.0001)	.0006*** (.0000)
Paid (eligibility)	.2937*** (.0367)		.2407*** (.0515)	.2931*** (.0533)
Non-additional	.0341*** (.01058)	.3555** (.1743)	-.0009 (.0134)	-.0011*** (.0194)
*Non-additional*3-paid*	.0031 (0.0141)		.0078 (.0166)	.0613** (.0279)
*Non-additional*9-paid*	.0571* (.0160)		.0912*** (.0302)	.0751*** (.0279)
Option10	.1982*** (.0175)	.4156*** (.1158)	.1990*** (.0343)	.1105*** (.0152)
Number of observations	24,240	1,320	11,880	11,040

Note: Results are based on conditional logit models in which the dependent variable is an indicator variable denoting whether or not the option was selected. The unit of observation is an option available to a participant in a single decision round. One, two, and three asterisks indicate 10%, 5%, and 1% significance for a two-tailed hypothesis based on a t-distribution, respectively. Standard errors reported in parentheses. Regressions with variance clustered at the subject level were run. The estimation results do not vary.

Table 15. Individual Behavior—Offer and Information Rent

	Offer (5)	Offer (6)	Information rent (7)	Information rent (8)
Selected option NPC	.5811*** (.0640)	.6319 (.3274)	-.5969*** (.0706)	-1.3619*** (.2133)
Selected option EB	.4602*** (.0962)	.3181*** (.1086)	.2341*** (.0692)	.0852 (.0802)
3-paid	24.5375*** (9.1049)	25.2409*** (9.0875)	18.3699** (8.2122)	21.6073*** (8.1393)
9-paid	9.8935 (9.2196)	7.8657 (9.2177)	18.6698** (9.5200)	20.3067** (9.3984)
PositiveNPC		-17.0994 (14.0320)		2.2899 (9.3694)
*PositiveNPC*NPC*		.56805* (.3289)		-.8498*** (.2150)
Budget	0.985*** (.0133)	.0982*** (.0132)	.1374*** (.0127)	.1316*** (.0126)
Experience	1.8313 (1.1570)	1.5589 (1.1579)	1.4604* (.8504)	1.1821 (.8501)
Constant	-57.3509** (27.2798)	-18.4453 (32.7873)	-44.5979** (21.1584)	-19.5757 (25.1609)
Number of observations	982	982	671	671
Number of groups	132	132	132	132

Note: Results are based on individual fixed-effect models. The individual fixed-effect are tested to be significant. One, two, and three asterisks indicate 10%, 5%, and 1% significance for a two-tailed hypothesis based on a t-distribution, respectively. Standard errors reported in parentheses. The number of observations is much lower for information rents because not all offers are accepted. Offers rejected will not generate any information rent. And only positive information rents are considered in the regression. Negative information rents are considered as mistakes by participants are not used in the regression.

Table 16. Subgroups and possible outcomes of the adoption and cost-share enrollment

		Cost-share program enrollment (y_2)	
		No cost-share ($y_2 = 0$)	Enrolled with cost-share ($y_2 = 1$)
Cover crops adoption (y_1)	No adoption ($y_1 = 0$)	Non-adopter (G_1; $y_1 = 0$, $y_2 = 0$)	Noncompliance (G_2; $y_1 = 0$, $y_2 = 1$)
	Adoption ($y_1 = 1$)	Self-adopter (G_3; $y_1 = 1$, $y_2 = 0$)	Paid-adopter (G_4; $y_1 = 1$, $y_2 = 1$)

Note: Original work by the author.

Table 17. Cover crop adoption, cost-share enrollment, adoption acreage, and share of farm acreage

State	Number of observations			Average adoption acreage		Average adoption percentage	
	No adoption	Adoption with cost-share	Adoption without cost-share	Adoption with cost-share	Adoption without cost-share	Adoption with cost-share	Adoption without cost-share
Maryland (N=699)	171 (24.46%)	456 (65.24%)	72 (10.30%)	398.03	136.16	65.57%	52.21%
Ohio (N=1,417)	707 (49.89%)	165 (11.64%)	545 (38.46%)	333.34	115.56	49.66%	32.36%

Note: Original data collected from survey results and summarized by the author. After cleaning data and checking observation eligibility as described in the paper, there are 699 available observations from Maryland and 1,417 observations from Ohio.

Table 18. Description and descriptive statistics of dependent variables

Dependent variable	Description	Maryland		Ohio	
		Mean	S.D.	Mean	S.D.
Acreage	Acres of operating cropland	540.59	731.18	476.99	1,049.72
POP_2017	Principle operator in 2017	0.91	0.29	0.89	0.32
FarmYears	Years of farming as an adult	35.76	15.48	36.11	14.90
Female	Female farmer	0.07	0.26	0.04	0.21
Paper	Data acquired from paper booklets	0.54	0.50	0.56	0.50
Owner_ComCropF	Commodity crop farmer, land owner	0.91	0.28	0.94	0.23
NOwner_ComCropF	Commodity crop farmer, not land owner	0.07	0.25	0.05	0.21
Tillage type					
Till_Conser	Conservation tillage	0.39	0.49	0.49	0.50
Till_No	No till	0.85	0.35	0.64	0.48
Till_Conven	Conventional tillage	0.28	0.45	0.53	0.50
Cash crop species produced in 2017					
Crop_Corn		0.80	0.40	0.85	0.36
Crop_Soybeans		0.88	0.32	0.97	0.18
Crop_Wheat		0.45	0.50	0.55	0.50
Crop_OtherCrop		0.36	0.48	0.19	0.40
Cost-share program in 2017					
CSP_2017	Conservation Stewardship Program (CSP) from USDA	0.01	0.11	0.03	0.16
EQIP_2017	Environmental Quality Incentives Program (EQIP) from USDA	0.01	0.09	0.05	0.22
StateCC_2017	State or local programs	0.65	0.48	0.08	0.27
Other_2017	Others	0.01	0.11	0.01	0.09
Government program in 2016 or earlier					
SignedGCC_2016	Signed up for a government program but not complete the contract nor got paid	0.25	0.43	0.08	0.27
PaidGCC_2016	Received a government payment to plant cover crops	0.83	0.37	0.23	0.42
USDACOMorINS_2016	USDA commodity programs or crop insurance	0.55	0.50	0.67	0.47
CRP_2016	USDA CRP (Conservation Reserve Program)	0.34	0.47	0.36	0.48
StateCC_2016	State or local programs	0.77	0.42	0.08	0.27
OtherUSDACC_2016	Other USDA conservation programs (EQIP, CSP)	0.17	0.38	0.16	0.36
OtherUSDANCC_2016	Other USDA conservation programs for practices other than cover crops (e.g., EQIP, CSP, CREP, WRP4)	0.34	0.47	0.16	0.36
OtherLocal_2016	Other state or local farm/conservation programs	0.09	0.29	0.04	0.19
Information source					

CCInfor_ExtentionAgent	Extension agent	0.51	0.50	0.43	0.50
CCInfor_SeedDealers	Seed dealers	0.28	0.45	0.41	0.49
CCInfor_CooperativeWeb	Cooperative extension web services	0.22	0.42	0.16	0.37
CCInfor_SoilConservation	Soil Conservation District Office	0.75	0.43	0.47	0.50
CCInfor_StateDA	State Department of Agriculture	0.70	0.46	0.24	0.43
CCInfor_NRCS	Natural Resources Conservation Service (USDA NRCS)	0.44	0.50	0.22	0.41
CCInfor_GrowerAss	Growers' associations	0.06	0.24	0.04	0.20
CCInfor_FarmBureau	Farm Bureau	0.19	0.39	0.17	0.38
CCInfor_Neighbors5	Other farmers – neighbors within five miles of your farm	0.36	0.48	0.40	0.49
CCInfor_OtherFarm	Other farmers – not neighbors	0.21	0.41	0.23	0.42
CCInfor_OtherEnter	Other sources entered by farmer	0.04	0.20	0.04	0.20
CCInfor_OHNoTill	Ohio No-Till Council	-	-	0.12	0.33
GCFI (USDA's gross cash farm income level)					
GCFIUnder1	Less than $1,000	0.02	0.13	0.02	0.15
GCFI1to149	Between $1,000 and $149,999	0.49	0.50	0.61	0.49
GCFI150to350	Between $150,000 and $349,999	0.19	0.39	0.18	0.39
GCFI350to999	Between $350,000 and $999,999	0.20	0.40	0.14	0.35
GCFI1Mto5M	Between $1,000,000 and $4,999,999	0.10	0.30	0.04	0.19
GCFIOver5M	Greater than $5,000,000	0.01	0.08	0.00	0.05
Irrigation (Percentage of farmland irrigated)					
IRRIGATENONE	None	0.71	0.45	0.96	0.19
IRRIGATE13	1-33%	0.11	0.32	0.02	0.14
IRRIGATE23	34-66%	0.10	0.29	0.00	0.05
IRRIGATE100	67-100%	0.08	0.27	0.01	0.12

Note: Original data collected from survey results and summarized by the author.

Table 19. Bivariate sequential estimation results and average marginal effects of cover crops adoption and cost-sharing enrollment in Maryland

Variables	1st stage CC_2017		2nd stage PaidCC_2017	
	Estimation results	Average marginal effect	Estimation results	Average marginal effect
Acreage	-0.00 (0.00)	-0.00 (0.00)	0.00 (0.00)	0.00 (0.00)
FarmYears	-0.01 (0.00)***	-0.0036 (0.00)***	-0.02 (0.00)***	-0.0043 (0.00)***
POP_2017	-0.42 (0.21)**	-0.10 (0.05)**	-0.28 (0.20)	-0.08 (0.06)
Female	-0.45 (0.21)**	-0.11 (0.05)**	-0.29 (0.22)	-0.08 (0.06)
Paper	0.14 (0.13)	0.03 (0.03)	0.11 (0.12)	0.03 (0.03)
Owner_ComCropF	0.68 (0.46)	0.16 (0.11)	0.49 (0.38)	0.13 (0.11)
NOwner_ComCropF	0.43 (0.50)	0.10 (0.12)	0.08 (0.42)	0.02 (0.12)
Till_Conser	0.20 (0.16)	0.05 (0.04)	0.02 (0.13)	0.00 (0.04)
Till_No	-0.11 (0.18)	-0.03 (0.04)	0.02 (0.17)	0.00 (0.05)
Till_Conven	-0.10 (0.14)	-0.02 (0.03)	-0.03 (0.13)	-0.01 (0.04)
Crop_Corn	0.62 (0.15)***	0.15 (0.03)***	0.43 (0.16)***	0.12 (0.04)***
Crop_Soybeans	0.12 (0.20)	0.03 (0.05)	0.07 (0.19)	0.02 (0.05)
Crop_Wheat	0.33 (0.15)**	0.08 (0.04)**	0.13 (0.12)	0.03 (0.03)
Crop_OtherCrop	0.26 (0.15)*	0.06 (0.04)*	0.03 (0.13)	0.01 (0.04)
SignedGCC_2016	-0.26 (0.14)*	-0.06 (0.03)*	-0.18 (0.14)	-0.05 (0.04)
USDACOMorINS_2016	-0.01 (0.13)	-0.00 (0.03)	0.08 (0.12)	0.02 (0.03)
CRP_2016	0.32 (0.14)**	0.08 (0.03)**	0.35 (0.13)***	0.10 (0.04)***
CCP_2016	0.52 (0.15)***	0.12 (0.04)***	1.05 (0.15)***	0.29 (0.04)***
Conser_2016	0.14 (0.13)	0.03 (0.03)	0.14 (0.13)	0.04 (0.03)
CCInfor_SeedDealers	-0.03 (0.10)	-0.01 (0.03)		
CCInfor_SoilConservation	0.02 (0.14)	0.01 (0.03)	0.05 (0.14)	0.01 (0.04)
CCInfor_StateDA	0.05 (0.15)	0.01 (0.04)	-0.05 (0.14)	-0.01 (0.04)
CCInfor_NRCS	0.07 (0.14)	0.02 (0.03)	0.04 (0.13)	0.01 (0.04)
CCInfor_ExtentionAgent	0.17 (0.13)	0.04 (0.03)	0.06 (0.12)	0.02 (0.03)
CCInfor_CooperativeWeb	-0.08 (0.17)	-0.02 (0.04)	-0.04 (0.15)	-0.01 (0.04)
CCInfor_GrowerAss	0.06 (0.35)	0.02 (0.08)	0.13 (0.26)	0.04 (0.07)
CCInfor_FarmBureau	0.10 (0.19)	0.02 (0.05)	0.10 (0.15)	0.03 (0.04)
CCInfor_Neighbors5	0.06 (0.11)	0.01 (0.03)		
CCInfor_OtherFarm	-0.06 (0.13)	-0.01 (0.03)		
CCInfor_OtherEnter	0.11 (0.35)	0.03 (0.08)		
GCFIUnder1	0.17 (0.59)	0.04 (0.14)	-0.29 (0.45)	-0.08 (0.13)
GCFI1to149	-0.62 (0.43)	-0.15 (0.10)	-0.19 (0.30)	-0.05 (0.08)
GCFI150to350	-0.40 (0.42)	-0.09 (0.10)	0.07 (0.30)	0.02 (0.08)
GCFI350to999	-0.08 (0.36)	-0.02 (0.09)	0.14 (0.27)	0.04 (0.07)
IRRIGATENONE	0.13 (0.29)	0.03 (0.07)	-0.21 (0.26)	-0.06 (0.07)
IRRIGATE13	0.05 (0.32)	0.01 (0.08)	-0.22 (0.30)	-0.06 (0.08)
IRRIGATE23	0.60 (0.35)*	0.14 (0.08)*	0.23 (0.30)	0.06 (0.08)
Constant	-0.13 (0.73)	-	-0.65 (0.56)	-
Observations	681		517	

Note: Rho = 0.99 in the sequential bivariate probit model (Wald test of indep. eqns. (rho = 0): chi2(1) =

1301.61, Prob > chi2 = 0.0000). Robust standard errors in parentheses. Asterisks ***, **, and * denote significance at 1%, 5%, and 10% levels, respectively. The number of observations for the enrollment outcome (517) is less than the number of observations for adoption (681) because only observations for current users of cover crops are used in the second step.

Table 20. Bivariate sequential estimation results and average marginal effects of Cover crops adoption and cost-sharing enrollment in Ohio

Variables	1st stage CC_2017		2nd stage PaidCC_2017	
	Estimation results	Average marginal effect	Estimation results	Average marginal effect
Acreage	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)
FarmYears	0.00 (0.00)	0.00 (0.00)	-0.00 (0.00)	-0.00 (0.00)
POP_2017	0.10 (0.13)	0.03 (0.04)	-0.28 (0.20)	-0.09 (0.07)
Female	-0.03 (0.21)	-0.01 (0.06)	-0.76 (0.40)*	-0.25 (0.13)**
Paper	-0.16 (0.08)**	-0.05 (0.02)**	0.03 (0.12)	0.01 (0.04)
Owner_ComCropF	0.42 (0.46)	0.13 (0.14)	-0.70 (0.49)	-0.23 (0.16)
NOwner_ComCropF	0.73 (0.50)	0.22 (0.15)	-0.48 (0.54)	-0.16 (0.18)
Till_Conser	0.05 (0.08)	0.01 (0.02)	-0.18 (0.11)	-0.06 (0.04)
Till_No	0.27 (0.08)***	0.08 (0.02)***	-0.15 (0.13)	-0.05 (0.04)
Till_Conven	-0.07 (0.08)	-0.02 (0.02)	-0.20 (0.11)*	-0.07 (0.04)*
Crop_Corn	-0.07 (0.11)	-0.02 (0.03)	0.07 (0.16)	0.02 (0.05)
Crop_Soybeans	0.10 (0.22)	0.03 (0.07)	-0.13 (0.35)	-0.04 (0.11)
Crop_Wheat	0.99 (0.08)***	0.29 (0.02)***	-0.83 (0.14)***	-0.27 (0.05)***
Crop_OtherCrop	0.52 (0.10)***	0.15 (0.03)***	-0.25 (0.12)**	-0.08 (0.04)**
SignedGCC_2016	0.46 (0.17)***	0.14 (0.05)***	-0.17 (0.19)	-0.06 (0.06)
USDACOMorINS_2016	0.16 (0.08)*	0.05 (0.02)*	-0.09 (0.12)	-0.03 (0.04)
CRP_2016	-0.00 (0.08)	-0.00 (0.02)	0.34 (0.13)***	0.11 (0.04)***
CCP_2016	0.55 (0.12)***	0.16 (0.03)***	0.24 (0.20)	0.08 (0.06)
Conser_2016	-0.15 (0.11)	-0.04 (0.03)	0.25 (0.13)**	0.08 (0.04)*
CCInfor_SeedDealers	0.13 (0.08)	0.04 (0.02)	-0.33 (0.11)***	-0.11 (0.03)***
CCInfor_SoilConservation	0.09 (0.09)	0.03 (0.03)	0.32 (0.15)**	0.11 (0.05)**
CCInfor_StateDA	-0.00 (0.10)	-0.00 (0.03)	-0.28 (0.13)**	-0.09 (0.04)**
CCInfor_NRCS	0.28 (0.11)**	0.08 (0.03)***	0.33 (0.18)*	0.11 (0.06)*
CCInfor_ExtentionAgent	-0.01 (0.09)	-0.00 (0.03)	0.01 (0.11)	0.00 (0.04)
CCInfor_CooperativeWeb	-0.03 (0.12)	-0.01 (0.03)	-0.04 (0.14)	-0.01 (0.04)
CCInfor_GrowerAss	0.40 (0.19)**	0.12 (0.06)**		
CCInfor_FarmBureau	0.06 (0.11)	0.02 (0.03)	-0.14 (0.14)	-0.05 (0.04)
CCInfor_OHNoTill	0.27 (0.15)*	0.08 (0.04)*		
CCInfor_Neighbors5	0.07 (0.08)	0.02 (0.03)	-0.13 (0.11)	-0.04 (0.03)
CCInfor_OtherFarm	0.19 (0.10)*	0.06 (0.03)*	-0.10 (0.12)	-0.03 (0.04)
CCInfor_OtherEnter	0.35 (0.19)*	0.10 (0.06)*	-0.42 (0.24)*	-0.14 (0.08)*
GCFIUnder1	-0.34 (0.35)	-0.10 (0.10)	-3.74 (1.68)**	-1.22 (0.51)**
GCFI1to149	-0.34 (0.27)	-0.10 (0.08)	0.11 (0.25)	0.03 (0.08)
GCFI150to350	-0.39 (0.26)	-0.11 (0.08)	0.08 (0.24)	0.03 (0.08)
GCFI350to999	-0.36 (0.25)	-0.11 (0.07)	0.30 (0.24)	0.10 (0.08)
IRRIGATENONE	-0.31 (0.29)	-0.09 (0.08)	0.45 (0.43)	0.15 (0.14)
IRRIGATE13	-0.06 (0.38)	-0.02 (0.11)	0.95 (0.57)*	0.31 (0.18)*
Constant	-1.28 (0.66)*	-	1.23 (0.84)	-
Observations	1,356		691	

Note: Rho= -.8639794 in the sequential bivariate probit model (Wald test of indep. eqns. (rho = 0): chi2(1) =

3.97, Prob > chi2 = 0.0463). Asterisks ***, **, and * denote significance at 1%, 5%, and 10% levels, respectively. The number of observations for the enrollment outcome (691) is less than the number of observations for adoption (1,356) because only observations for current users of cover crops are used in the second step.

Table 21. Average marginal effects of GLM regression on the share of acreage in Maryland and Ohio

Variables	MD paid-adoption Equation (3.15)	MD self-adoption Equation (3.16)	OH paid-adoption Equation (3.17)	OH self-adoption Equation (3.18)
Acreage100	-0.01***(0.00)	0.01(0.00)	-0.01(0.00)	-0.01*(0.00)
FarmYears	-0.00(0.00)	0.00(0.00)	-0.00(0.00)	0.00(0.00)
POP_2017	-0.02(0.06)	-0.02(0.10)	0.05(0.09)	0.03(0.04)
Female	0.03(0.07)	0.20*(0.11)	0.15*(0.09)	0.01(0.06)
Paper	-0.01(0.03)	-0.17***(0.06)	0.00(0.05)	-0.06***(0.02)
Owner_ComCropF	0.26**(0.11)	-2.21***(0.27)	-0.50***(0.19)	-0.13(0.33)
NOwner_ComCropF	0.30**(0.12)	-2.07***(0.27)	-0.34(0.22)	-0.09(0.33)
Till_Conser	-0.01(0.03)	-0.02(0.05)	-0.03(0.05)	-0.05**(0.02)
Till_No	-0.01(0.04)	0.11(0.09)	0.05(0.06)	0.00(0.03)
Till_Conven	-0.05(0.03)	-0.02(0.04)	-0.12**(0.05)	-0.12***(0.02)
Crop_Corn	-0.14*(0.08)	-0.42***(0.16)	-0.09(0.09)	-0.10***(0.04)
Crop_Soybeans	-0.12**(0.06)	-0.29***(0.06)	-0.09(0.24)	-0.25**(0.11)
Crop_Wheat	-0.00(0.03)	-0.15**(0.08)	0.09(0.09)	-0.03(0.05)
Crop_OtherCrop	-0.07***(0.03)	-0.17***(0.05)	0.01(0.05)	0.03(0.03)
SignedGCC_2016	-0.02(0.03)	-0.00(0.08)	0.07(0.08)	0.08**(0.04)
USDACOMorINS_2016	0.05*(0.03)	0.08(0.05)	0.06(0.06)	0.02(0.02)
CRP_2016	0.04(0.04)	-0.01(0.09)	-0.07(0.05)	-0.01(0.03)
CCP_2016	0.10(0.12)	-0.01(0.20)	-0.04(0.08)	0.10***(0.03)
Conser_2016	0.01(0.03)	-0.05(0.08)	-0.04(0.05)	0.01(0.03)
GCFIUnder1	-0.10(0.20)	0.13(0.14)		-0.10(0.14)
GCFI1to149	-0.02(0.08)	0.07(0.15)	0.01(0.14)	-0.08(0.10)
GCFI150to350	-0.12*(0.06)	0.07(0.10)	-0.06(0.11)	-0.17*(0.09)
GCFI350to999	-0.07(0.05)	0.13(0.10)	-0.03(0.10)	-0.14*(0.08)
IRRIGATENONE	-0.19***(0.05)	0.59***(0.12)	-2.75***(0.27)	0.11(0.11)
IRRIGATE13	-0.11*(0.06)	0.55***(0.16)	-2.84***(0.31)	0.07(0.17)
IRRIGATE23	-0.07(0.06)	0.62***(0.11)		
$\hat{\lambda}_1$	0.05(0.19)	0.01(0.26)	0.10(0.10)	0.09(0.07)
$\hat{\lambda}_2$	0.07(0.18)	-0.02(0.26)	-0.23**(0.10)	-0.05(0.07)
Observations	446	71	162	528

Note: In the OH self-adoption regression, although the two self-selection variables mills1 and mills2 are not significant individually, a joint test shows that the two variables are jointly significant. The selection bias correction variables are not significant individually or jointly in the MD models. The number of observations varies by the number of farmers assigned in each group. Standard errors in parentheses. Asterisks ***, **, and * denote significance at 1%, 5%, and 10% levels, respectively.

Table 22. Average estimated share of acreage in cover crops and the treatment effects of cost-share programs with the double-selection model

	MD		OH	
	Paid-adoption (G4, N=379[17])	Self-adoption (G3, N=71)	Paid-adoption (G4, N=162)	Self-adoption (G3, N=523)
Without cost-share	42.64%	51.53%	31.20%	32.12%
With MD cost-share	65.48%	65.25%	59.14%	59.14%
With OH cost-share	54.87%	43.00%	50.23%	32.94%
In-state ATT	21.85%***	-	19.03%***	-
In-state ATU	-	13.72%***	-	0.85%
Out-of-state ATU	11.03%***	-8.54%	27.74%***	26.63%***

Note: The ATT and ATU are calculated for each observation first then averaged level is reported in the table above. The average ATT and ATUs are tested with a t-test and asterisks ***, **, and * denote significance at 1%, 5%, and 10% levels, respectively.

Table 23. Average treatment effects of cost-share programs with a single selection mode and a simple GLM model

	Single selection model				Simple GLM			
	MD		OH		MD		OH	
	Paid-adoption (G4, N=446)	Self-adoption (G3, N=71)	Paid-adoption (G4, N=163)	Self-adoption (G3, N=539)	Paid-adoption (G4, N=446)	Self-adoption (G3, N=71)	Paid-adoption (G4, N=163)	Self-adoption (G3, N=539)
Within-state ATT	18.97%***	-	17.44%***	-	19.33%***	-	16.97%***	-
Within-state ATU	-	13.18%***	-	4.76%***	-	13.35%***	-	12.44%***
Out-of-state	16.79%***	2.72%	26.28%***	26.58%***	20.98%***	7.76%***	25.80%***	26.47%***

Note: The ATT and ATU are calculated for each observation first then averaged level is reported in the table above. The average ATT and ATUs are tested with a t-test and asterisks ***, **, and * denote significance at 1%, 5%, and 10% levels, respectively.

[17] Note on missing prediction

FIGURES

Figure 1. Stylized City with Concentric Growth Rings

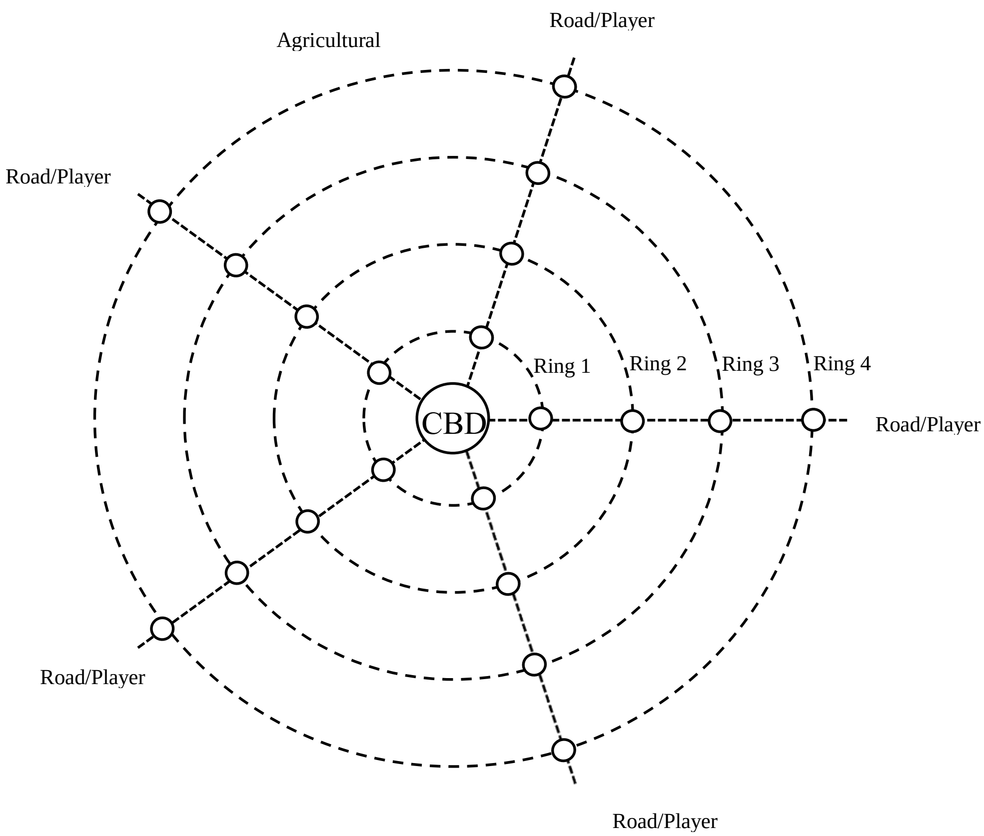


Figure 2. Government and farmers' decision tree

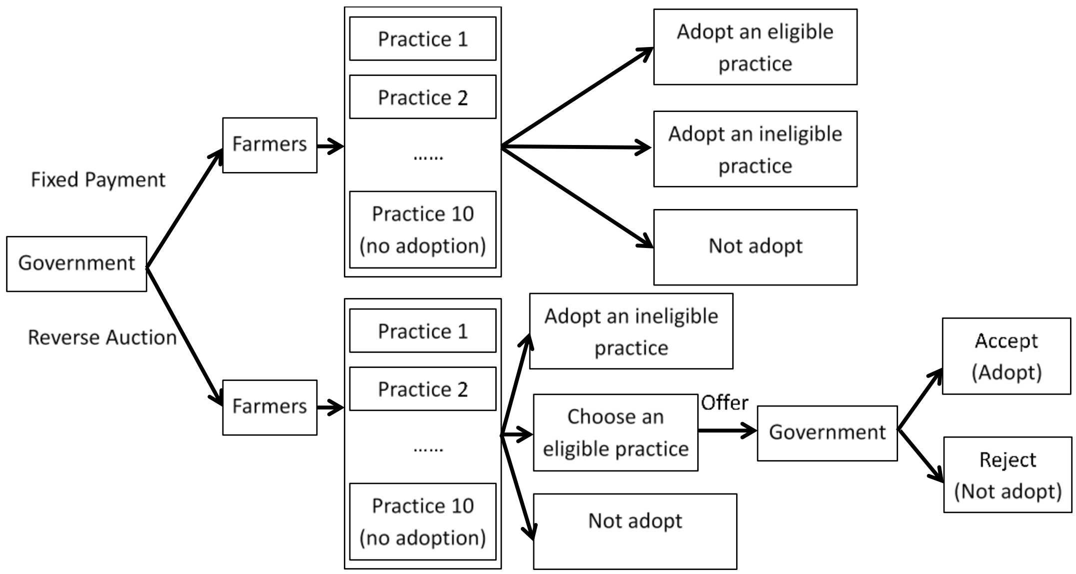
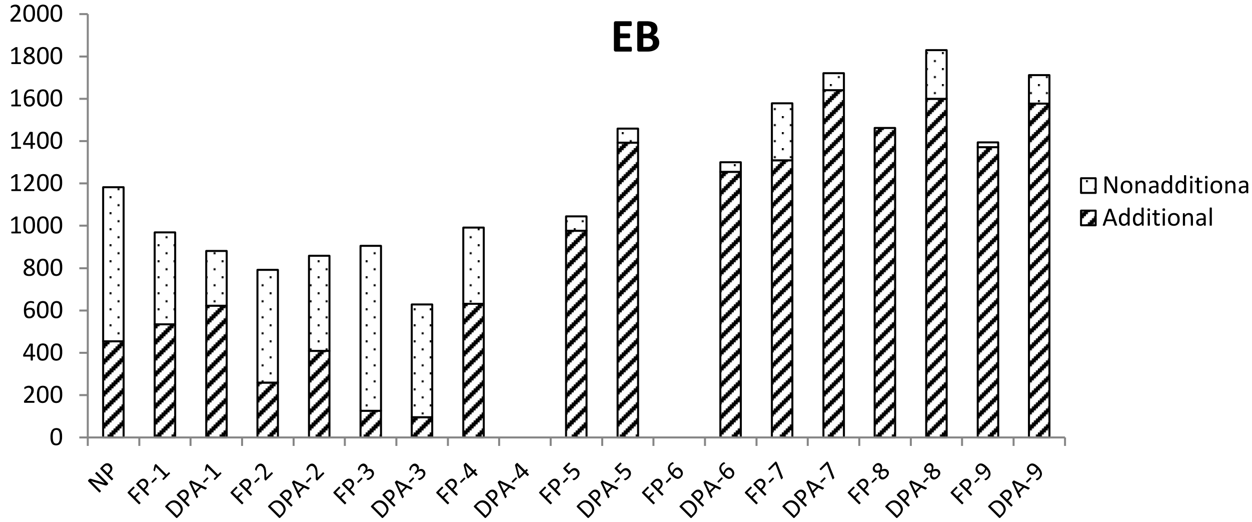


Figure 3. Average group EB by options under 1-paid option treatment



Note: The eligible option is randomly selected among option 1 to option 9. Option 4 is never selected during DPA while option 6 is never selected during FP.

Figure 4. Average group EB by treatments

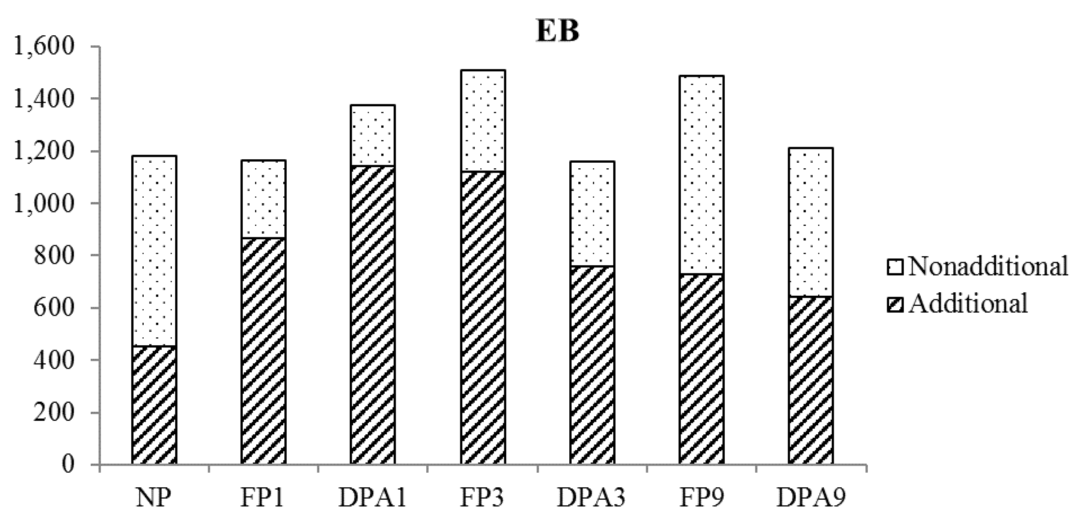
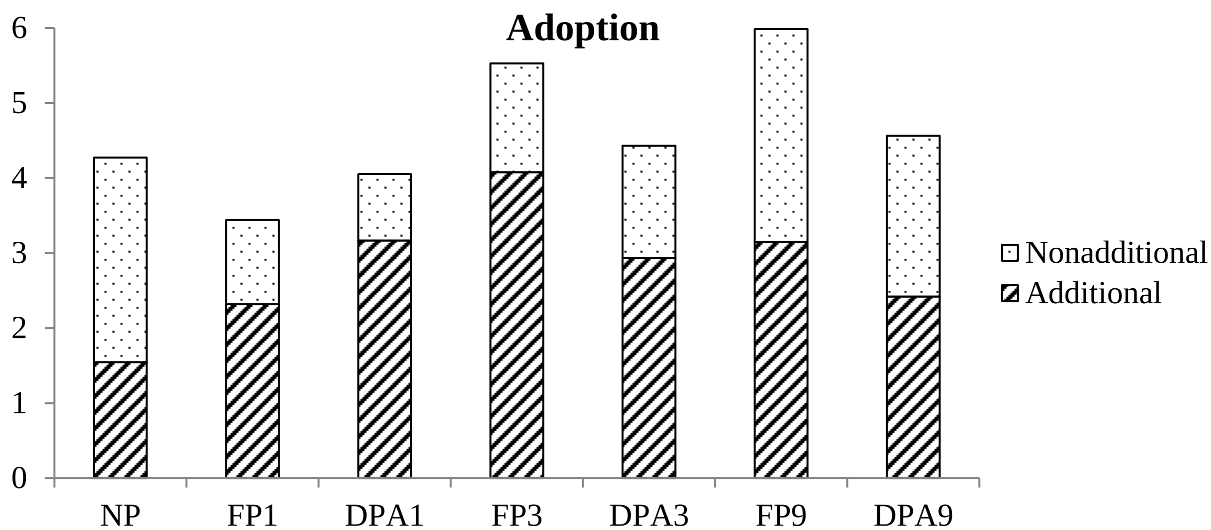


Figure 5. Average adoption by a group



Note: The number of adoptions means the number of participants (out of 6 participants in a group) that adopted a certain practice. The statistics shown in this figure show similar results as figure 4 by design because more adoption leads to higher EB in spite of the heterogeneous EB.

Figure 6. Budget leftover, information rent and adoption cost by treatments

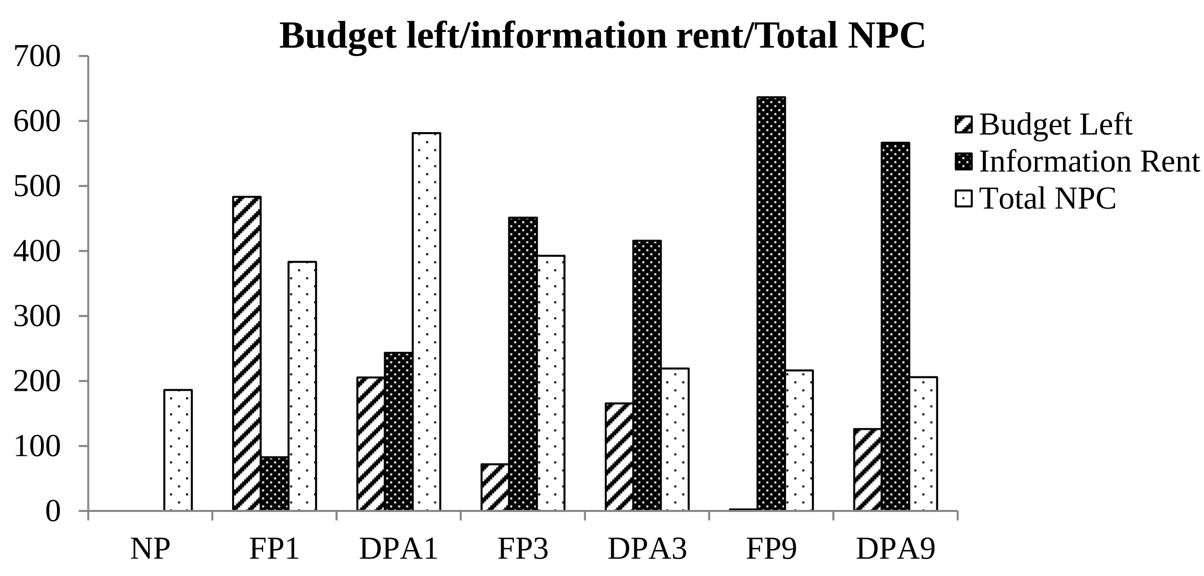


Figure 7. Average Social Welfare by treatments

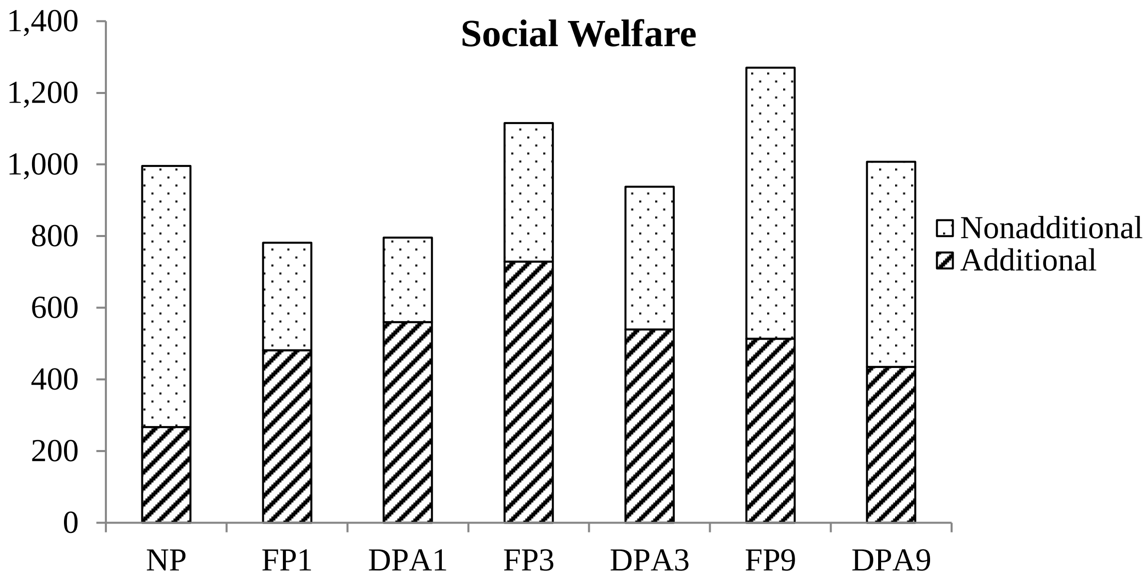


Figure 8. NPC and information rent

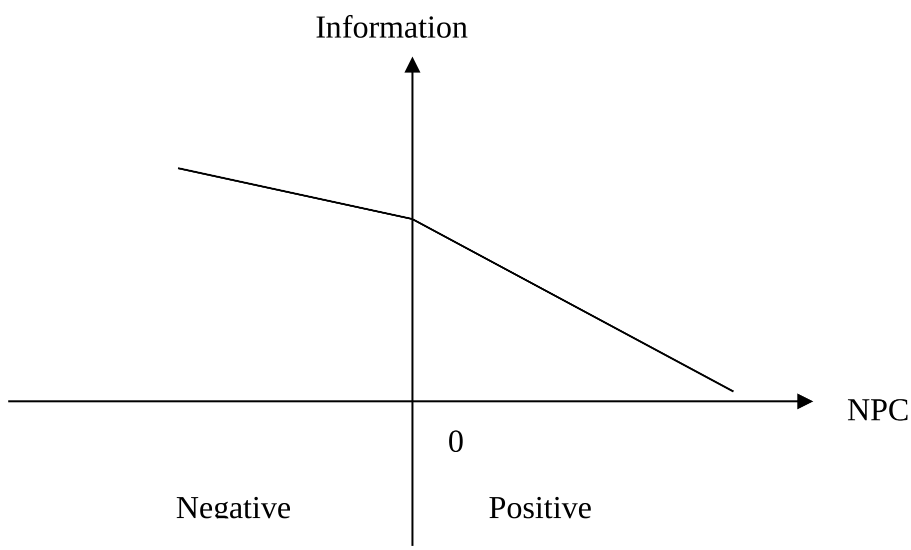
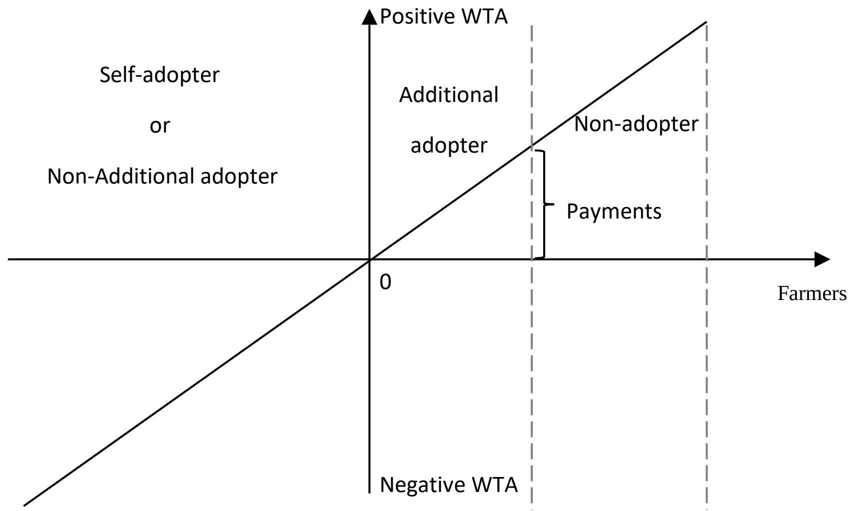
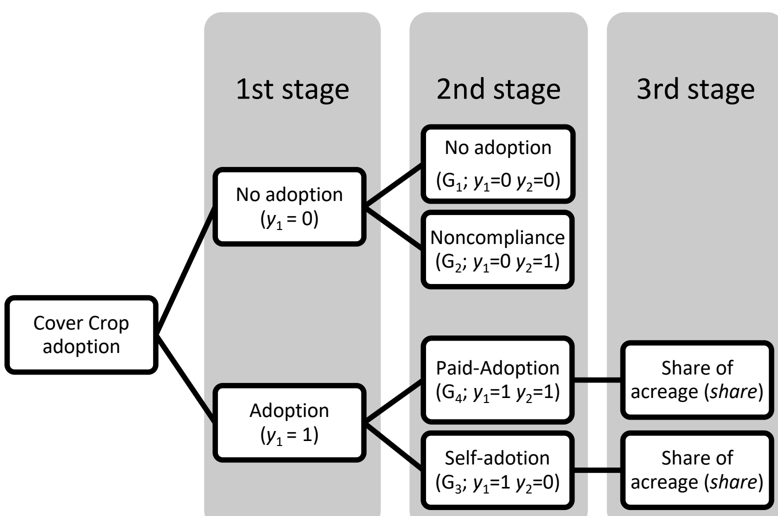


Figure 9. The WTA spectrum and subgroups of farmers



Note: Original work by the author. Farmers are ordered by WTA from negative to positive and from small to large on the spectrum. This WTA line is drawn hypothetically to demonstrate the subgroups of the sample. The WTA curve need not be linear.

Figure 10. Grouping and sequential outcome for a double-selection model



Note: Original work by the author.

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Appendix

IRB APPROVAL FOR CHAPTER 1

DATE: May 14, 2018

TO: Joshua Duke
FROM: University of Delaware IRB

STUDY TITLE: [947547-3] Lincoln Institute Fellowship: Experiments in Land Taxation

SUBMISSION TYPE: Amendment/Modification
ACTION: APPROVED
APPROVAL DATE: May 14, 2018
EXPIRATION DATE: August 28, 2018
REVIEW TYPE: Expedited Review
REVIEW CATEGORY: Expedited review category # (7)

Thank you for your submission of Amendment/Modification materials for this research study. The University of Delaware IRB has APPROVED your submission. This approval is based on an appropriate risk/benefit ratio and a study design wherein the risks have been minimized. All research must be conducted in accordance with this approved submission.

This submission has received Expedited Review based on the applicable federal regulation.

Please remember that informed consent is a process beginning with a description of the study and insurance of participant understanding followed by a signed consent form. Informed consent must continue throughout the study via a dialogue between the researcher and research participant. Federal regulations require each participant receive a copy of the signed consent document.

Please note that any revision to previously approved materials must be approved by this office prior to initiation. Please use the appropriate revision forms for this procedure.

All SERIOUS and UNEXPECTED adverse events must be reported to this office. Please use the appropriate adverse event forms for this procedure. All sponsor reporting requirements should also be followed.

Please report all NON-COMPLIANCE issues or COMPLAINTS regarding this study to this office.

Please note that all research records must be retained for a minimum of three years.

Based on the risks, this project requires Continuing Review by this office on an annual basis.

Please use the appropriate renewal forms for this procedure.

If you have any questions, please contact Nicole Farnese-McFarlane at (302) 831-1119 or nicolefm@udel.edu. Please include your study title and reference number in all correspondence with this office.

IRB APPROVAL FOR CHAPTER 2 AND CHAPTER 3

DATE: May 4, 2017

TO: Joshua Duke, Ph.D
FROM: University of Delaware IRB

STUDY TITLE: [1059765-1] Determining Willingness to Tradeoff Contract Attributes Focus Groups via Economic Experiments

SUBMISSION TYPE: New Project

ACTION: APPROVED
APPROVAL DATE: May 4, 2017
EXPIRATION DATE: May 3, 2018
REVIEW TYPE: Expedited Review
REVIEW CATEGORY: Expedited review category # (7)

Thank you for your submission of New Project materials for this research study. The University of Delaware IRB has APPROVED your submission. This approval is based on an appropriate risk/benefit ratio and a study design wherein the risks have been minimized. All research must be conducted in accordance with this approved submission.

This submission has received Expedited Review based on the applicable federal regulation.

Please remember that informed consent is a process beginning with a description of the study and insurance of participant understanding followed by a signed consent form. Informed consent must continue throughout the study via a dialogue between the researcher and research participant. Federal regulations require each participant receive a copy of the signed consent document.

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Please report all NON-COMPLIANCE issues or COMPLAINTS regarding this study to this office.

Please note that all research records must be retained for a minimum of three years.