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Private Information and the Allocation of Land Use Subsidies in Malawi†

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Efficient targeting of public programs is difficult when the cost or benefit to potential recipients is private information. This study illustrates the potential of self-selection to improve allocational outcomes in the context of a program that subsidizes tree planting in Malawi. Landholders who received a tree planting contract as a result of bidding in an auction kept significantly more trees alive over a three year period than did landholders who received the contract through a lottery. The gains from targeting on private information through the auction represent a 30 percent cost savings per surviving tree for the implementing organization. (*JEL* D04, D44, D82, O13, Q24, Q28)

Health, environment, and poverty alleviation programs are often designed to target transfers toward recipients who maximize the net benefits of the program. If costs or benefits are private information, or if there are incentives for strategic behavior, then targeting can be improved through the use of mechanisms that induce self-selection into the program (Nichols and Zeckhauser 1982; Coady, Grosh, and Hoddinott 2004). Self-selection has most frequently been implemented through market segmentation for subsidized food or health products or through below-market wages in public employment settings (Besley and Coate 1992; Alderman and Lindert 1998), however, questions around the design of targeting tools for environmental land use programs have gained policy prominence with increasing attention to climate change mitigation and biodiversity conservation (Babcock et al. 1997; Ferraro 2008; Mason and Plantinga 2011).

For self-selection to improve efficiency, recipients must possess private information about their costs or benefits under the program, and must respond to an

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allocation mechanism that reveals some of that private information. Both of these conditions may not be satisfied in practice. Yet, in spite of the implications for policy design, few field studies have directly measured how targeting public funds through self-selection affects program outcomes.

This study provides a straightforward empirical test of the role of private information in the allocation of subsidies for afforestation to landholders in Malawi.[1] Subsidies are in the form of a contract that provides landholders with a fixed number of seedlings and pays per surviving tree at regular intervals over a period of three years. If landholders' willingness to accept (WTA) the afforestation contract is private information, then an allocation mechanism that induces self-selection can, in theory, improve allocational efficiency.[2] A uniform price, sealed bid procurement auction, in which each landholder can win at most a single contract, has a dominant strategy of truthful bidding, and offers the advantage of endogenously determining a market clearing price.[3] The auction allocates subsidy contracts to landholders with the lowest bids, and exhausts the total available budget (Vickrey 1961, 1976). For the auction to efficiently target contracts, landholders must reveal their private information through their bids, something that inexperienced laboratory subjects often fail to do (Kagel 1995; List 2003).[4] To empirically identify self-selection outcomes, the field experiment assigns eligible landholders either to a uniform price, sealed bid auction or to a lottery treatment that randomly allocates afforestation contracts.[5] The auction clearing price is used to set the subsidy level in both treatments.

Results show that self-selection through the auction improves allocational efficiency, measured by tree survival outcomes, relative to random selection in the lottery treatment group. At the end of the three year contract, the average tree survival in the auction treatment is 10.5 percentage points or around one-third of a standard deviation higher than under the lottery treatment. Because of fixed per-contract costs, the difference in survival rates translates, for the implementing organization, into a 30 percent difference in cost per surviving tree in spite of the piece rate nature of the subsidy. The survival results are highly correlated with other measures of landholder effort under the contract, consistent with tree survival outcomes that are determined by landholder actions rather than underlying endowments, such as land quality.

[1] Afforestation is the conversion of marginalized or agricultural land to forest. It is recognized as a carbon sequestration strategy by the Intergovernmental Panel on Climate Change (IPCC 2007) and provides local benefits including erosion reduction (World Bank 2011).

[2] Paying farmers per surviving tree at regular intervals avoids adverse selection concerns associated with targeting on WTA alone. Contract details and assumptions about the relationship between WTA and tree survival are further described in Sections I and II. For land use subsidies, such as avoided deforestation, where adverse selection concerns are first order, targeting on WTA may result in little behavior change (e.g., Sheriff 2009).

[3] A number of authors have proposed the use of auctions as a conservation targeting tool (Latacz-Lohmann and Van der Hamsvoort 1997; Ferraro 2008), and this study provides the first rigorous evidence on conservation auction performance from a field experiment. Previous research relies on standard program evaluation techniques or simulations to investigate the effects of targeting conservation investments based on deforestation risk or on opportunity cost (Alix-Garcia, De Janvry, and Sadoulet 2008; Jack, Leimona, and Ferraro 2009; Sheriff 2009).

[4] Note that bidders need not follow the dominant strategy exactly for the auction to allocate to those with the lowest willingness to accept. As long as bids preserve the ranking of participants, those with the lowest WTA will receive a contract.

[5] The lottery treatment was set up as a take-it or leave-it posted offer. Over 99 percent of landholders accepted the contract offer, so the lottery treatment was implemented on an unselected sample of landholders. Further detail on the implementation of both treatments is provided in Section II.

Simulations of alternative targeting approaches show that the auction does better at allocating contracts to households with high rates of tree survival than does targeting based on easily observed characteristics. However, as the household-level information available to the project designer increases, outcomes from targeting on observables improve relative to targeting on private information through auction bids. Furthermore, targeting on private information does best relative to targeting on observables in the short run. As more time elapses, the private information revealed by landholders at the time of the auction becomes less relevant for tree survival outcomes.

Like many field experiments and empirical case studies, the generalizability of the results may be limited. The study exploits a limited source of variation—landholder selection around a single contract price—and is therefore silent on the response of selection or survival to variation in contract price. In spite of these limitations, the study makes three main contributions. First, it demonstrates that landholders have private information relevant to their performance under a contract that offers incentives for afforestation. Second, it shows that an auction can reveal some of that information and target contracts to landholders with high tree survival outcomes, which offers a measure of allocational efficiency in this setting. Third, it provides evidence that the gains from self-selection relative to targeting on observables decrease as the information available to the project designer increases.

These contributions build on the existing empirical literature on the targeting of public spending using self-selection, little of which has compared alternative targeting approaches or directly measured allocational efficiency. A recent study by Alatas et al. (2012) compares two approaches to allocating cash transfers, one of which leverages community-level information about poverty. They find substantial differences in the composition of selected households, but are not able to measure allocational efficiency. A related literature examines targeting subsidized health products on the basis of willingness to pay, and finds mixed results on whether willingness to pay is correlated with allocational outcomes, measured by use and health impacts (Ashraf, Berry, and Shapiro 2010; Cohen and Dupas 2010; Berry, Fischer, and Guiteras 2012).[6]

The next section provides a simple conceptual framework for the field experiment. Section II describes the experiment and data collection. Section III presents the main results from the contract allocation and tree survival outcomes. Section IV shows additional results including simulation of alternative targeting rules and an analysis of how costs and benefits vary with the targeting approach. Section V concludes.

I. Conceptual Framework

A simple conceptual framework provides intuition for landholder choices in the experiment. A project designer has a fixed budget to allocate toward subsidies for

[6] Berry, Fischer, and Guiteras (2012) is the only one of these studies that tests two different approaches to allocation. They compare willingness to pay for water filters in Ghana using a Becker-DeGroot-Marschak bidding mechanism and a take-it or leave-it price. They find modest but significant differences between the mechanisms, with higher willingness to pay elicited through the take-it or leave-it price. Related to the use of field experiments to explore the effects of self-selection, Chassang, Padró i Miquel, and Snowberg (2012) discuss the importance of unobserved agent type in the context of randomized controlled experiments in which individual effort is unobservable but affects returns to the treatment.

afforestation. The subsidies are in the form of a contract that pays a piece rate p per surviving tree. Under the contract, landholders are required to set aside a fixed amount of land to plant T trees. The amount of land allocated to the contract and the number of trees planted are not choice variables for the landholder, so the fixed costs F of the contract are assumed to be exogenous. Both fixed and variable costs $v(t)$ may vary across landholders, and trees survive only if both fixed and variable investments are incurred. The total cost to the landholder of planting and caring for t trees is therefore $F + v(t)$, with $t \leq T$.

Assuming $v'(t) > 0$, $v''(t) > 0$, a landholder's minimum willingness to accept (WTA) the afforestation contract is defined by a choice of the piece rate p^* and level of variable investment $v(t^*)$ defined by the tangency of the profit maximizing marginal variable cost curve and the total cost curve:

$$WTA = p^* t^*$$

$$s.t. \quad F + v(t^*) = p^* t^*$$

$$v'(t^*) = p^*.$$

Without imposing restrictions on the functional form of $v(t)$, it is sufficient that F and $v(t)$ are positively correlated for tree survival to be decreasing in willingness to accept at any given $\tilde{p}$.[7]

The project designer wishes to allocate contracts to minimize the cost per surviving tree. Without information about the distribution of landholder costs of afforestation, the project designer will be unable to set an efficient price to clear the market. An appropriately designed auction will, in theory, give landholders an incentive to reveal their willingness to accept an afforestation subsidy and will endogenously determine a clearing price based on bids and the available budget.[8]

As shown in the standard results for a sealed bid, second-price auction with single-unit demand and private values, rational bidders who do not collude have a dominant strategy to bid their value (Vickrey 1961; Krishna 2002).[9] If bids contain private information about the costs of contract implementation, then allocation on WTA will improve tree survival outcomes relative to a no-targeting alternative. If, on the other hand, bidders do not have private information about their implementation costs, or do not reveal costs through their bids, then alternative targeting approaches may do as well or better.

[7] The relationship between fixed and variable costs is an empirical one. However, given that both fixed and variable costs depend on labor, a positive correlation is likely.

[8] The contract, as implemented, pays farmers for the number of surviving trees at each of four periods spread over three years. Even if selection is based on short-run willingness to accept, it will still lead to higher tree survival outcomes if $v(t)$ is decreasing over time, while the contract piece rate $\tilde{p}$ is constant.

[9] In this case, the uniform price is set by the first rejected offer, making it strategically equivalent to a single unit second price auction (Vickrey 1961, 1976). The clearing price could also be determined by a target number of contracts rather than a budget constraint.

II. Context and Implementation

The study introduced random assignment at the allocation stage of a program to subsidize afforestation on private land in Ntchisi District in Central Malawi, implemented by the World Agroforestry Centre (ICRAF).[10] The program was modeled on the growing number of payments for environmental services and carbon offset programs in Malawi and elsewhere around the world.[11] These types of land use subsidy programs are a substantial government expenditure in both developed and developing countries, totaling billions of dollars annually in direct payments to landholders in exchange for environmentally beneficial investments on private land (Pattanayak, Wunder, and Ferraro 2010).

In the study setting, afforestation on private land produces both private benefits, including soil fertility, and timber income, and public benefits, including carbon sequestration, reduced erosion, and biodiversity. The private benefits accumulate slowly, and as a result, few farmers invest in afforestation without some form of encouragement. Implementation of the experiment and the data corresponding to each implementation phase are as follows.

Baseline Survey and Randomization.—A census of 472 households in 23 villages collected household-level information on labor, land, and household characteristics and preferences.[12] Households reporting less than one acre of private land in the baseline survey were ineligible for contracting and were excluded from the randomization. Treatment groups were assigned using a simple random draw.

Invitations to participate in the allocation mechanisms were delivered to households one week in advance by field staff who were unaware of the details of the study. The invitation provided no specific information about the contract or the allocation process to minimize the opportunity for collusion. Of the 472 invited households, 433 (91.7 percent) participated in the auction or lottery treatments. Differences between participants and nonparticipants are shown in Table A1 in the Appendix. Participants are more likely to be male, live closer to their fields, experience fewer months of food shortage, belong to a labor-sharing group, are more trusting of outsiders, and have higher discount rates. This nonrandom selection into the project occurred before treatment assignments were revealed, and is therefore orthogonal to treatment.

Table 1 shows treatment balance for participating households. Comparing variable means across groups using a t-test shows only one variable that differs at $p < 0.10$. Auction participants are slightly more likely to report prior contact with the implementing organization ($p = 0.064$). A joint F-test on these variables indicates no

[10] In the study setting, private land refers to land that is owned and managed by a single household under customary tenure arrangements.

[11] Ajayi, Jack, and Leimona (2012) discuss the challenges of auction implementation in developing countries and compare the project described here with results from a conservation auction in Indonesia, but do not discuss the experiment in detail.

[12] The implementing organization selected the villages based on their participation in previous projects and the capacity of the government extension staff to assist with the study. The initial selection of villages is therefore not random, though treatment assignment at the household level is. The baseline survey originally included 27 villages, 4 of which were later dropped from the study for budgetary reasons (one from each of the four extension planning areas included in the study). Power calculations and the budget available for data collection and contract payments determined the sample size at each stage of the project.

Table 1—Participant Characteristics by Treatment Group

	Treatment 1 Lottery		Treatment 2 Auction		Difference p-value
	(1)		(2)		(3)
Household size	4.439	(2.103)	4.636	(2.083)	0.329
Education (1–5)a	2.283	(0.839)	2.154	(0.844)	0.111
Laborsharing group (0/1)	0.400	(0.491)	0.465	(0.500)	0.174
Casual labor income (0/1)	0.663	(0.474)	0.662	(0.474)	0.980
Use family labor only (0/1)	0.761	(0.428)	0.759	(0.429)	0.957
Stated labor constraint (0/1)	0.044	(0.205)	0.044	(0.205)	0.998
Minutes gathering firewood	121.385	(84.716)	111.412	(95.735)	0.254
Total landholding (acres)	4.940	(3.540)	4.970	(3.012)	0.925
Number of fields	1.410	(0.719)	1.373	(0.606)	0.562
Total number of crops	3.137	(1.221)	3.035	(1.106)	0.365
Minutes from home to field	20.648	(18.794)	23.931	(24.994)	0.126
Cash crops (0/1)	0.810	(0.393)	0.829	(0.377)	0.605
Stated land constraint (0/1)	0.024	(0.155)	0.048	(0.215)	0.190
Past borrowing (0/1)	0.312	(0.465)	0.307	(0.462)	0.908
Age of the participant	37.804	(16.037)	38.770	(15.905)	0.530
Female participant (0/1)	0.449	(0.499)	0.522	(0.501)	0.129
Months of food shortage	4.190	(2.116)	4.211	(2.229)	0.923
Prior tree planting (0/1)	0.488	(0.501)	0.500	(0.501)	0.801
Prior contact with NGO (0/1)	0.244	(0.430)	0.325	(0.469)	0.064
Risk preferences (1–3)b	2.146	(0.933)	2.123	(0.921)	0.792
Time preferences (1–6)b	3.223	(2.175)	3.344	(2.189)	0.565
Mistrusts outsiders (1–3)	1.912	(0.766)	1.810	(0.753)	0.163
Willingness to try tech(1–3)	1.344	(1.343)	1.230	(1.314)	0.371
Asset index	11.107	(3.498)	11.346	(3.102)	0.451
Observations	205		228		
Joint F-stat for village indicators					0.13

Notes: Means are reported for each of the treatment groups with standard deviations in brackets. p-value adjusted for standard errors clustered at the village level: The range for categorical variables is provided in parentheses. (0/1) indicates a dummy variable, equal to one if the response is yes. Time preferences and risk preferences are elicited using survey questions. Higher risk preference corresponds to more risk seeking. Higher time preference corresponds to a lower discount rate.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

statistical difference between the treatment groups ($F(24, 429) = 1.12$). Table 1 also highlights some relevant features of the implementation setting. Casual labor markets are active but seasonal; around 40 percent of households participate in labor-sharing and around two-thirds seek casual labor for income. The average household reports five acres of land on which they plant just over three different types of crop. Around half of the respondents had some trees growing on their land at the time of the baseline survey.

The Contract.—The afforestation contract required that landholders plant 50 seedlings provided by the implementing organization on half an acre of private land. Seedlings were distributed at the start of the rainy season, three months after contracts were allocated. The implementing organization provided trainings on tree planting and care to all recipients. Landholders were paid based on the contract price and the number of surviving trees.[13]

[13] In practice, the incentives are in the form of a piece rate per surviving tree. Based on piloting outcomes, bids were solicited for the full contract amount.

Contract Allocation.—Participants arrived to the allocation site, registered, and were separated into their preassigned treatment groups, which were implemented simultaneously to avoid information spillovers. The two treatments used different enumerators, who followed scripts that provided the same information except for the description of the allocation mechanism. In both treatments, the contract was fully explained to individuals before they were asked to make any decisions. Auction participants heard a thorough explanation of the auction rules and the bidding incentives and were given several examples.[14] They were specifically told that the best strategy was to bid the lowest price that would make them willing to accept the contract. The enumerator explained that the budget available to purchase tree planting contracts was limited, but the size of the budget was not announced.[15] Sealed bids were collected and ranked on the spot to determine the auction clearing price based on the available budget and the first rejected bid. Participants in the lottery treatment group were told the take-it or leave-it price, based on the auction clearing price, but were not informed about the source of the price.[16] The lottery was conducted among participants who accepted the take-it or leave-it price, by drawing participant identification numbers from a box in front of the group.

Monitoring and Payments.—Tree survival outcomes were measured six months into the contract, and again after one, two, and three years. During each monitoring visit, representatives from the government, the implementing organization, and local officials visited the field where the trees were planted and counted surviving trees, based on predetermined criteria.[17] Approximately 3 percent of the observations reflect nonmonotonicities in tree survival and, therefore, errors or discrepancies in monitors' assessments of tree survival status. In the main results, the monitoring data is analyzed in its original state. Linear imputations of the nonmonotonic observations provide a robustness check. The lead monitor also conducted an assessment of field maintenance including land care practices, such as weeding and constructing fire breaks, that were covered in the trainings. The assessment resulted in a monitoring score that ranges from one to five, with five being the highest score.

III. Main Results

A. *Contract Allocation Outcomes*

Figure 1 shows the cumulative distribution function of bids in the auction, with a mean of approximately 60,000 Malawi Kwacha (MWK) and a median of MWK

[14] Examples illustrated the mechanism but used bidding scenarios that were unrelated to the contract, and were too low to anchor participant values.

[15] Note that in a procurement setting, either a quantity constraint or a budget constraint can be used to set supply. In this case, due to actual budget limitations, a budget constraint was used. Therefore, the budget constraint must bind for the auction to be incentive compatible. Preliminary qualitative work indicated that the budget would bind and participants were explicitly told that the budget was insufficient to cover all bids.

[16] The difference in the timing between the treatments was facilitated by serving refreshments at different points during the process. All participants knew that they would receive refreshments as part of their show up fee.

[17] Landholders received notice approximately one week in advance of the monitoring. The monitoring team was not aware of the treatment assignment of the individual.

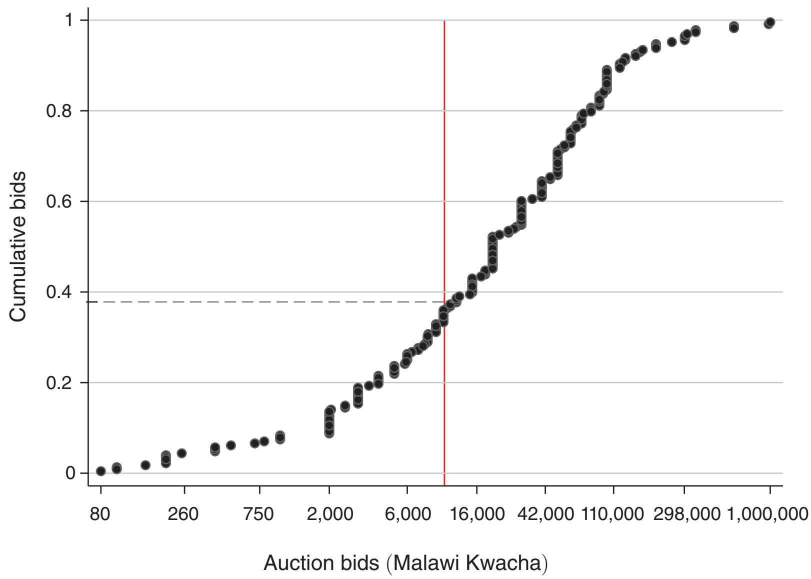

Figure 1. Individual Bids for Tree Planting Contracts and Allocation by Treatment

Notes: Cumulative distribution of auction bids, with the *x*-axis distributed in logs. The vertical line shows the clearing price. The horizontal line shows the corresponding percent acceptance at this price in the auction treatment.

20,000 (bid curve is plotted on a logarithm scale). The auction had a fixed budget of 1.05 million MWK, resulting in a clearing price of MWK 12,000 based on the first rejected offer and the available budget.[18] As a result, 85 individuals (37.3 percent of the market) received contracts, a selection determined both by the number of bidders in the auction and the budget available for contract payments.

The auction clearing price, shown by the vertical line in Figure 1, was offered to 205 individuals in the lottery treatment group, all but one of whom accepted the contract.[19] The lottery selected 91 individuals for contracting, resulting in a representative sample of landholders for comparison to the self-selected sample under the auction.

Auction.—To investigate observable determinants of self-selection under the auction, Table 2 reports the estimates from a linear regression of bids from landholder i in village v on observable characteristics $(\mathbf{x}_i)$, with standard errors clustered at the village level:

$$bid_{iv} = \beta \mathbf{x}_i + d_v + \varepsilon_{iv}. \tag{1}$$

[18] \$1 USD = 140 MWK. As a point of reference, Malawi's per capita gross national income (GNI) was USD 290 or MWK 40,600 in 2008 (World Bank 2008). The contract paid up to MWK 6,000 or about 15 percent of per capita GNI in the first year and up to MWK 3,000 or about 7 percent of per capita GNI in each of the subsequent years.

[19] This divergence in contract supply at the clearing price is inconsistent with standard theoretcial predictions and is substantially larger than what could be explained by imbalanced randomization. A full analysis of the allocation outcomes is beyond the scope of this paper, and is a topic for further research.

Table 2—Baseline Predictors of Bids and Contract Allocation

	log (bid)		Bid rank	Contracted (0/1) Auction	Contracted (0/1) Lottery
	(1)	(2)	(3)	(4)	(5)
Household size	−0.032	−0.080	−0.977	0.031	0.082**
	(0.059)	(0.053)	(2.250)	(0.051)	(0.036)
Education (1–5)	0.254*	0.253***	8.797	−0.023	0.200
	(0.140)	(0.086)	(5.152)	(0.128)	(0.130)
Laborsharing group (0/1)	−0.283	0.057	−11.253	0.340	0.212
	(0.342)	(0.337)	(11.141)	(0.221)	(0.231)
Casual labor income (0/1)	−0.508	−0.509	−16.926	0.397	−0.079
	(0.349)	(0.354)	(11.948)	(0.252)	(0.213)
Use family labor only (0/1)	0.245	0.151	9.543	−0.085	−0.174
	(0.337)	(0.357)	(12.338)	(0.262)	(0.279)
Stated labor constraint (0/1)	1.128**	0.617	36.915	−0.842*	−0.239
	(0.527)	(0.364)	(22.027)	(0.480)	(0.532)
Minutes gathering firewood	−0.004***	−0.002	−0.127***	0.002***	0.001
	(0.001)	(0.001)	(0.034)	(0.001)	(0.001)
Total landholding (acres)	0.040	0.028	1.212	−0.010	−0.038*
	(0.047)	(0.045)	(1.646)	(0.035)	(0.022)
Number of fields	−0.307	0.063	−11.725	0.220	0.193
	(0.322)	(0.228)	(11.469)	(0.228)	(0.180)
Total number of crops	−0.047	0.004	−3.132	0.087	−0.095
	(0.156)	(0.134)	(4.507)	(0.099)	(0.099)
Minutes from home to field	−0.004	−0.001	−0.234	0.005	0.001
	(0.004)	(0.004)	(0.166)	(0.003)	(0.005)
Cash crops (0/1)	0.135	0.004	9.345	−0.198	0.670**
	(0.307)	(0.313)	(11.691)	(0.233)	(0.320)
Stated land constraint (0/1)	−0.895*	−0.371	−27.980*	0.269	0.071
	(0.494)	(0.613)	(14.440)	(0.329)	(0.418)
Past borrowing (0/1)	0.341	0.229	10.670	0.043	0.053
	(0.292)	(0.312)	(10.268)	(0.196)	(0.190)
Age of the participant	0.001	0.001	0.025	0.006	0.009*
	(0.008)	(0.008)	(0.305)	(0.006)	(0.005)
Female participant (0/1)	−0.467	−0.249	−15.770	0.332	−0.220
	(0.306)	(0.252)	(10.668)	(0.205)	(0.191)
Months of food shortage	−0.029	0.001	−2.259	0.057	0.002
	(0.069)	(0.068)	(2.583)	(0.051)	(0.047)
Prior tree planting (0/1)	0.111	0.284	4.710	0.003	0.130
	(0.272)	(0.267)	(9.715)	(0.210)	(0.192)
Prior contact with NGO (0/1)	−0.492	−0.621*	−16.128	0.259	0.284
	(0.319)	(0.308)	(10.890)	(0.223)	(0.249)
Risk preferences (1–3)	0.036	−0.071	2.283	−0.137	−0.071
	(0.131)	(0.130)	(4.824)	(0.085)	(0.071)
Time preferences (1–6)	−0.167***	−0.089	−5.283**	0.127***	0.007
	(0.055)	(0.053)	(1.876)	(0.030)	(0.041)
Mistrusts outsiders (1–3)	0.193	0.225	7.276	−0.147	0.035
	(0.206)	(0.223)	(6.261)	(0.127)	(0.138)
Willingness to try tech (1–3)	−0.084	−0.074	−2.853	0.032	−0.030
	(0.080)	(0.067)	(3.127)	(0.065)	(0.063)
Asset index	−0.050	−0.037	−1.361	0.041	−0.043
	(0.056)	(0.062)	(1.986)	(0.043)	(0.037)
Constant	11.484***	9.105***	175.202***	−2.661***	−1.166
	(1.016)	(1.345)	(37.695)	(0.842)	(1.004)
Village FE		Yes			
Observations	227	227	227	228	205
Adjusted R^2	0.207	0.468	0.202	0.134	0.083

Notes: Regressions of bids on baseline survey variables. Columns 1 and 2 are OLS regressions with ln(bid) as the dependent variable. Column 3 is an OLS regression of bid rank on explanatory variables. Columns 4 and 5 are probit regressions with the binary contract allocation outcome as the dependent variable. Columns 1 and 2 omit a single high outlier bid from the regression. Standard errors are clustered at the village level in 1, 2, and 3. See Table 1 for a description of the regressors.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

The first and second columns of Table 2 use the natural log of the auction bid as the outcome variable. The joint significance of village indicator variables (d_v) (column 2) implies that some factors that affect bids, and therefore auction selection, are correlated within village. The model in column 3 uses bid rank (lowest to highest) as the outcome variable. A simple ranking of bids preserves the ordinal sorting of the auction while eliminating outliers that may be due to bid shading or miscalculation. Results for bid rank (column 3) are qualitatively similar to the results with ln(bid) as the outcome (columns 1 and 2). Column 4 shows the selection into the contract for the auction treatment, using a probit model.

Several explanatory variables are consistently signed and sometimes significant across regression models. Intuitively, the positive coefficients on education level and reporting a binding labor constraint are consistent with a higher opportunity cost of implementing the contract. Households that spend more time gathering firewood place lower bids, perhaps in anticipation of using planted trees for fuel. The payoffs from the trees occur in the future, after costs are incurred, so the negative coefficient on the time preference variable, which corresponds to a lower discount rate, is also intuitive. The negative coefficient on the stated land constraint is harder to interpret, particularly in light of customary tenure rules and abundant land in the study area.

Lottery.—Given that contracts were allocated randomly in the lottery treatment group, household characteristics should not predict contracting outcomes, and jointly they do not. However, the lack of stratification and the small sample size results in some differences in household characteristics in a probit regression of lottery outcomes on household observables (Table 2, column 5). Household size and the sale of cash crops are slightly higher among contract recipients, while land size is slightly lower. The implications of this imbalance will be revisited in Section IIIB.

B. *Contract Implementation Outcomes*

Selection on auction bids only matters to the project designer to the extent that it improves tree survival outcomes. As shown in the conceptual framework, this depends on the presence of private information, on the revelation of some of that information in bids, and on the nature of implementation costs for the landholder. The primary outcome for evaluating targeting is the number of surviving trees, out of 50, at each of 4 monitoring rounds over three years. Average tree survival decreases over time, though the auction treatment group remains stochastically dominant, as illustrated by the CDFs of survival outcomes by monitoring round (Figure 2). Under the assumption of independent observations, I first test the difference in tree survival outcomes nonparametrically. Specifically, a Wilcoxon rank-sum test pools and ranks the survival data and compares the sum of the rankings for each treatment. Figure 2 reports the two-sided p-value associated with the test for each of the four monitoring rounds. The difference is greatest six months into the contract (p-value $= 0.004$), decreases at one year (p-value $= 0.220$), then increases again through the final monitoring round (p-value $= 0.055$).

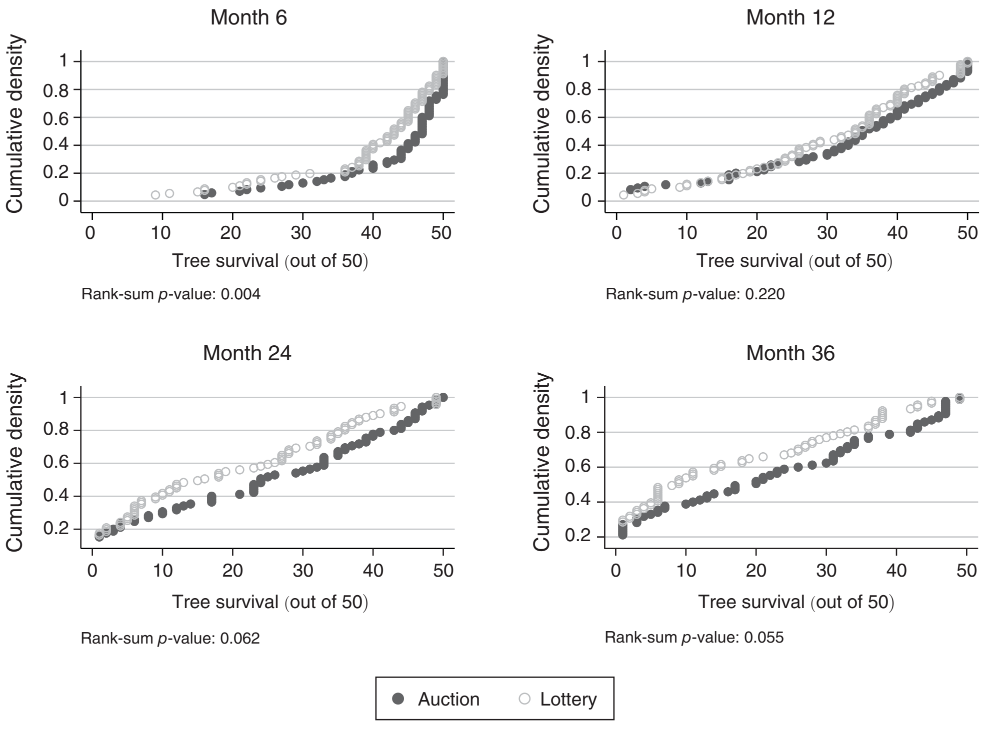

Figure 2. Tree Survival Outcomes by Treatment Group

Notes: CDFs for tree survival outcomes for the auction and lottery allocation treatments at each monitoring and payment interval. Rank-sum *p*-values are from a two-sided Wilcoxon rank-sum test. The maximum possible tree survival was 50 trees.

A disadvantage of the nonparametric tests is that each round is analyzed separately. A linear regression analyzes average treatment effects across all four rounds, with clustering at the village level to allow for arbitrary correlation across time and within village. Monitoring round intercepts are included in all regressions to correct for common time shocks across villages. Table 3, column 1 presents the results of a linear regression of the number of surviving trees on the auction treatment and round indicators (r_t), using ordinary least squares with standard errors clustered at the village level:

$$trees_{ivt} = \beta T_i + r_t + \varepsilon_{ivt}. \tag{2}$$

On average, across the four rounds of monitoring, approximately 3.85, or around 15 percent, more trees survive under contracts allocated through the auction than contracts allocated through the lottery treatment.[20] The total number of villages (23)

[20] Village fixed effects are not included because some of the relevant selection under the auction occurs at the village level, as shown in Table 2. Spatial regressions (not shown) also indicate geographic autocorrelation of tree survival outcomes at the household level. Both social and biophysical factors may contribute to the spatial autocorrelation.

Table 3—Tree Survival and Other Contract Outcomes

	Survival (out of 50)		Score (1–5)	Exit
	OLS	Tobit	Ordered logit	OLS
	(1)	(2)	(3)	(4)
Auction	3.858*	4.754*	0.428**	−0.015
	(2.138)	(2.669)	(0.218)	(0.040)
Round 2	−9.239***	−10.932***	−0.962***	0.017*
	(0.978)	(1.019)	(0.160)	(0.009)
Round 3	−18.920***	−21.972***	−1.323***	0.119***
	(1.299)	(1.349)	(0.245)	(0.037)
Round 4	−22.795***	−26.924***	−1.890***	0.205***
	(1.351)	(1.761)	(0.311)	(0.045)
Lottery treatment mean	25.92	25.92	2.55	0.13
R^2 measure	0.269	0.040	0.044	0.066

Notes: $N = 704$. Standard errors are clustered at the village level in all models. The outcome variable in columns 1 and 2 is tree survival, out of a maximum of 50 trees. Column 1 estimates the model using OLS and column 2 uses a Tobit regression censored above at 50 and below at 0. Column 3 shows the odds ratio for an ordered logit regression for the monitoring score (1–5). Column 4 uses a linear probability model for contract exits.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

may be insufficient for asymptotic inference with clustered standard errors, so I use a wild bootstrap correction to address the small number of clusters (Cameron, Gelbach, and Miller 2008).[21] The correction slightly increases the standard errors shown in panel A, column 1, and lowers the p-value on the treatment effect to $p = 0.098$. As a robustness check, the 3 percent of the monitoring observations that indicate nonmonotonic tree survival outcomes are corrected with linear imputations of tree survival trends. With the corrected data, the magnitude of the auction treatment effect is increased slightly to 3.97 trees and the significance level is unchanged ($p < 0.10$).

Round specific treatment effects, obtained by interacting the auction treatment with monitoring round indicators, are plotted in Figure 3. The figure illustrates the downward trend in tree survival in both treatment groups. The positive treatment effect of contract allocation through the auction increases slightly over time, as does the size of the standard errors. By the fourth monitoring round, three years after the trees were planted, the auction leads to 5.24 (standard error 2.34) or 33 percent more surviving trees than the lottery treatment. From the implementing organization's perspective, tree survival outcomes at the end of the contract are most relevant, though the total amount paid out in afforestation subsidies depends on performance throughout the contract.

The linear specification in column 1 of Table 3 does not reflect the implicit censoring of the outcome variable at 50 surviving trees, which is the maximum allowed under the contract. Approximately 11 percent of landholders kept all 50 trees alive

[21] The bootstrap correction holds regressors constant and resamples residuals, randomly switching the sign of the residual at the cluster level, and imposing a null of $\beta = 0$.

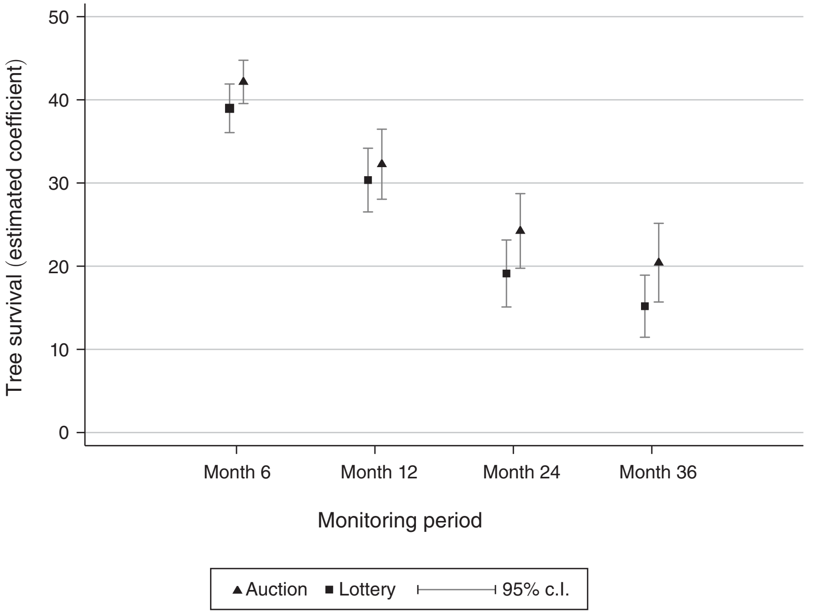

Figure 3. Tree Survival Outcomes over Time

Notes: Estimated marginal effects by monitoring period and treatment. Standard errors are clustered at the village level.

for at least 6 months of contract implementation. At the other end of the distribution, around 12 percent of tree survival observations are zeros. A tobit model adjusts the distribution to address predicted values that fall outside the possible range when round controls are included in the OLS specification. The tobit regression results, with censoring at a maximum of 50 and a minimum of zero trees, are shown in column 2 of Table 3, and are of slightly higher magnitude than the linear regression results (column 1), with a mean treatment effect of 4.75 (s.e. 2.67) surviving trees in contracts allocated through the auction. The wild bootstrap corrected p-value for the treatment effect estimated with the tobit model is $p = 0.087$.

The score assigned by the monitor is an additional proxy for landholder effort, not subject to random shocks that may affect tree survival. Tree survival is positively correlated with the monitoring score for the 3 years of data $(\hat{\rho} = 0.81)$. The ordered logit regressions reported in column 3 of Table 3 use the monitoring score (1–5) as the dependent variable and a latent variable model for each of the 4 cutoffs associated with the monitoring score. The score regression reports the odds ratio for the auction treatment indicator and shows a strong treatment effect. An individual in the auction is 0.43 times more likely to receive a 1-unit higher score on the monitoring than is an individual in the lottery treatment group. The positive coefficient on the auction treatment variable is consistent with greater investment of variable inputs, including labor, contributing to the higher tree survival outcomes reported in column 1.

Over the 3 years of the contract, around 24 percent of contracted landholders chose not to continue their contracts. Throughout the analyses, these are coded as zeros, both for surviving trees and for the monitoring score. A binary exit variable equals one for the period when the landholder dropped out and for all subsequent periods. Results for the auction treatment effect on exits are estimated with a linear probability model, and shown in column 4 of Table 3. Landholders exit the contract at the same rate in both treatments. Results are therefore not driven by differences on the extensive margin of contract participation.

Landholder Observables and Tree Survival Outcomes.—The relationship between tree survival outcomes, observable household characteristics, and auction bids may further explain the role of private information in the auction selection. Table 4 separates the estimation of tree survival outcomes by treatment group to isolate the direct relationship between household observable characteristics and tree survival from the effects of selection under the auction. Linear regressions of tree survival on observables include monitoring round indicators and cluster standard errors at the village level.

Table 4 shows two results. First, some household characteristics are correlated with tree survival outcomes in the randomly selected households contracted through the lottery treatment (column 1). Specifically, larger households keep fewer trees alive, while households with access to credit (past borrowing) and more assets keep more trees alive. Together, these coefficients suggest that liquidity constraints may correspond to lower tree survival. On the other hand, households that rely on family labor have higher tree survival outcomes, which may indicate supervision challenges with hired workers. Distance from home to field, production of cash crops, and a stated land constraint are negatively signed, consistent with a higher opportunity cost of land driving down tree survival. Finally, those who distrust outsiders have lower tree survival, perhaps because they do not expect to be paid, as do those more willing to try new technologies. The interpretation of these coefficients is speculative, but the overall variance in tree survival explained by household characteristics is reasonable ($R^2 = 0.527$).[22]

Second, higher tree survival outcomes under the auction treatment are driven by selection that is not fully explained by the relationship between observables and outcomes in the unselected sample (lottery treatment). Comparing columns 1 and 2 in Table 4 shows that most characteristics significantly related to tree survival under the representative lottery group (column 1) have a less precisely estimated coefficient of the same sign in the self-selected auction group (column 2), suggesting that selection under the auction reduces some of the relevant variation in characteristics. Cash crop production, past borrowing, and willingness to try new technologies are three exceptions where the relationship with tree survival is significant among the randomly selected lottery group, but of the opposite sign (insignificant) among self-selected auction participants. On the other hand, the auction does not select for the same characteristics (Table 2) that directly predict tree survival in the unselected

[22] I can also use these results to check whether the slight imbalance in lottery outcomes described in Section IIIA is likely to have affected tree survival outcomes. Both household size and production of cash crops are associated with a greater likelihood of receiving a contract through the lottery and worse tree survival outcome. However, both variables are balanced across treatments among contract holders, making it unlikely to have played a substantial role in the divergent tree survival outcomes.

Table 4—Determinants of Tree Survival by Treatment Group

	Lottery (1)	Auction (2)
Household size	−1.008*	−0.151
	(0.582)	(0.868)
Education (1–5)	−0.528	−1.131
	(1.481)	(1.437)
Laborsharing group (0/1)	0.991	3.715
	(2.309)	(3.600)
Casual labor income (0/1)	−2.074	2.382
	(2.332)	(4.561)
Use family labor only (0/1)	5.451*	2.931
	(3.024)	(3.591)
Stated labor constraint (0/1)	5.450	16.507**
	(5.744)	(7.868)
Minutes gathering firewood	0.030	−0.004
	(0.018)	(0.011)
Total landholding (acres)	0.084	0.258
	(0.598)	(0.634)
Number of fields	−1.792	−5.693**
	(2.002)	(2.666)
Total number of crops	1.257	−0.415
	(1.380)	(1.482)
Minutes from home to field	−0.096*	−0.025
	(0.048)	(0.061)
Cash crops (0/1)	−11.149**	0.411
	(5.285)	(4.534)
Stated land constraint (0/1)	−12.676**	−15.550*
	(5.193)	(7.713)
Past borrowing (0/1)	4.394*	−4.563
	(2.458)	(4.053)
Age of the participant	0.067	0.183
	(0.070)	(0.122)
Female participant (0/1)	1.546	2.778
	(2.851)	(2.684)
Months of food shortage	0.405	0.020
	(0.909)	(1.168)
Prior tree planting (0/1)	2.661	−0.623
	(2.644)	(3.432)
Prior contact with NGO (0/1)	−2.734	2.094
	(3.164)	(2.907)
Risk preferences (1–3)	−1.822	−1.269
	(1.411)	(1.528)
Time preferences (1–6)	−0.969	−0.114
	(0.635)	(0.599)
Mistrusts outsiders (1–3)	−3.312*	−1.804
	(1.743)	(2.541)
Willingness to try tech (1–3)	−2.575**	0.018
	(0.998)	(1.137)
Asset index	1.646***	0.701
	(0.484)	(0.777)
R^2	0.527	0.385
Observations	364	340

Notes: Linear regression of tree survival outcomes across four monitoring rounds on household explanatory variables (described in Table 1). Regressions include monitoring round indicators. Standard errors are clustered at the village level.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

sample (column 1, Table 4). This suggests that unobserved characteristics also played a substantial role in selection under the auction.

A direct correlation between auction bids and tree survival would provide further evidence for private information revealed through the auction. However, among the limited number of bidders who received contracts, this relationship is weak, perhaps because of the limited variation in winning bids. The standard deviation in bids below the clearing price is 2 percent of the standard deviation in bids above the clearing price. To further investigate the relationship between observable characteristics, bids, and tree survival, I use a regression of bids on household characteristics and village indicators to predict bids for the lottery treatment group. This results in a wider range of (predicted) bids with corresponding tree survival outcomes. A regression of tree survival on predicted and actual bids for all contracted landholders shows a negative relationship between bids and tree survival, consistent with selection on private information. The statistical significance of the relationship is sensitive to the specification. If the natural log of bids and tree survival are used for the regression, it is insignificant at conventional levels ($\beta = -0.919$, s.e. $= 0.588$, $p = 0.133$). The same analysis, using rankings for both bids and tree survival, is significant at $p < 0.10$ ($\beta = 0.15$, s.e. $= 0.08$).

IV. Additional Results and Discussion

The higher rate of tree survival under the auction confirm that self-selection improved allocational efficiency for the available budget. This result is sufficient to establish the presence of private information about contract costs and the ability of an auction to reveal some of that information. However, the result raises additional questions. First, comparing the auction allocation to a random allocation poses a relatively low hurdle for the auction to demonstrate its selection abilities. Using household characteristics, can targeting on observables do as well or better than the auction? Second, what are the costs and benefits of the program, and how do these vary with how contracts are targeted?

A. *Targeting on Observables*

Targeting public spending on observable characteristics is a common approach to identifying recipients for public programs. If observable characteristics proxy well for opportunity cost, targeting on observables may do as well as self-selection. However, gathering and correctly weighting the household observable characteristics that determine tree survival may be as difficult for a project designer as eliciting willingness to accept through an auction. I explore the potential for targeting on observables in this context by simulating alternative targeting rules that vary the amount of household-level information available to the project designer.

I observe both household characteristics and tree survival outcomes for the 89 landholders who were contracted at random under the lottery. I therefore focus on those observations to simulate alternative targeting approaches that rely on household observables. In doing so, I ignore both the households that self-selected under the auction, whose characteristics and survival outcomes are not representative of the larger population, and the households who did not receive contracts, and for whom I do not observe tree survival outcomes.[23] Since I wish to compare targeting on observables with selection under the auction, which included 228 bidders, I use the representative sample of 89 contracted landholders to simulate, using repeated random sampling, larger samples of 228 households for whom I observe both household characteristics and tree survival outcomes. I rank the resulting samples according to each targeting rule and identify average tree survival in the top 37 percent (the number selected under the auction). I repeat this exercise 10,000 times for each targeting rule.

I explore four approaches to targeting, increasing the informational intensity with each rule. First, the simplest targeting approach relies on a single observable household characteristic. In this exercise, I use five variables from the baseline survey that proxy for characteristics identified as priorities for targeting by stakeholders in Malawi: total acres owned, household size, participation in the labor market, past experience with the implementing organization, and past tree planting.[24] Second, targeting on multiple characteristics requires additional household data but may improve outcomes. Assuming no information about how to weight these characteristics, I combine the five variables of interest using equal weights by normalizing land size and household size to one, and summing these with the remaining three binary characteristics. Third, information about the relationship between observable characteristics and tree survival outcomes allows for more sophisticated targeting. I regress tree survival outcomes on the five priority characteristics to obtain regression weights which are used to construct a weighted index. Finally, a larger set of household characteristics may further enhance targeting. The fourth targeting approach uses regression weights for all household covariates (the weighting regression is shown in Table 4, column 1). Note that the last two targeting rules require difficult to obtain information and an assumption that the relationship between household characteristics and tree survival holds over time and out of sample.

Figure 4 compares the relative performance of targeting on observable characteristics with targeting on willingness to accept through the auction. The top panel shows simulated outcomes after six months, and the bottom panel shows outcomes at the end of the three year contract. Generally, the simulations confirm that incorporating additional information into the targeting rule improves allocational efficiency, as measured by tree survival. Targeting on observables also performs better, relative to the auction selection, as more time elapses. For example, targeting with regression weights on the five priority characteristics is much closer to the auction's selection after three years than after six months. This suggests that bidders' have better private information about short-term costs than long-term costs. Even in the short term, if enough is known about household characteristics and how they relate to tree survival outcomes, targeting on observables can lead to a better selection than the auction. However, gathering the

[23] An alternative approach would involve predicting tree survival for the entire auction sample using observable characteristics from the baseline survey, and apply the targeting rules to the auction sample. This approach ignores the error term in the predicting equation, which explains almost half of the total variation in tree survival outcomes (Table 4), so I employ a methodology that takes advantage of the representative sample of landholder for whom both characteristics and tree survival are observed.

[24] These characteristics were determined through interviews with stakeholders, including government officials. The approach is similar to what is used by Jack, Leimona, and Ferraro (2008) with data from Indonesia.

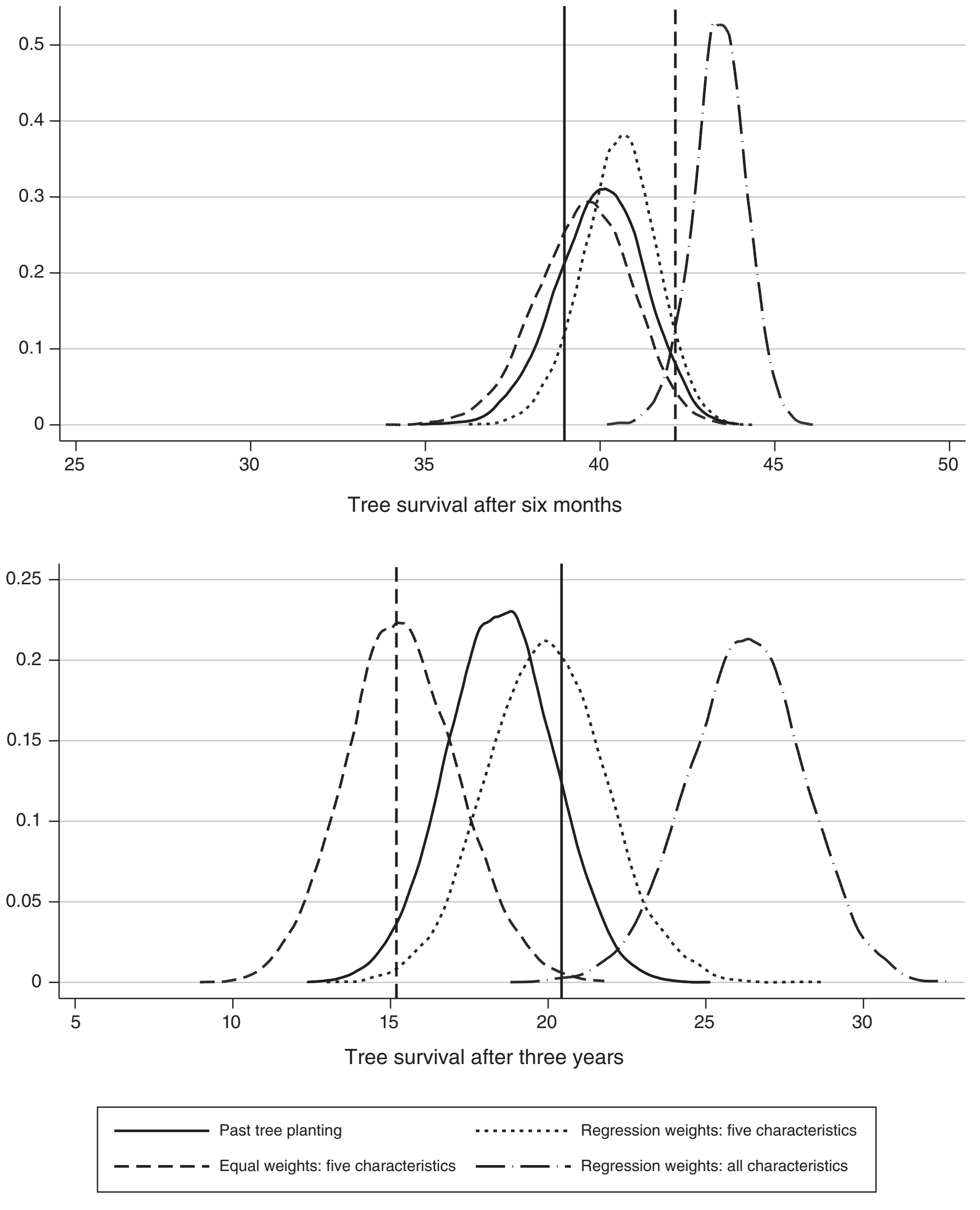

Figure 4. Simulated Targeting on Observables

Notes: Density plots of mean tree survival rates at six months (top) and three years (bottom) from simulations of alternative targeting rules using observable household characteristics. The dashed vertical line shows the mean tree survival rate from the lottery selection. The solid vertical line shows the mean tree survival rate from the auction.

amount of information needed to implement a targeting rule that relies on regression weights for a full set of household characteristics has a number of disadvantages: it may be more costly than running an auction; it requires enough elapsed time to measure the relationship between characteristics and outcomes; and it requires strong assumptions about the stability of this relationship over time and across landholder populations.

B. *Program Costs and Benefits*

Without detailed ex post measures of the impacts of the contract on consumption and other livelihood measures, a full welfare analysis is not feasible. I first examine the costs and benefits of the program for the implementing organization, then turn to a more tentative assessment of landholder costs. Costs to the project designer include both fixed per contract costs and variable costs per surviving tree. The variable costs per surviving tree are the same for each of the mechanisms, since the program pays only for surviving trees. The fixed costs of the program include enrolling participants, training them on the afforestation contract, providing inputs, and monitoring tree survival. Fixed costs are US$125 per contract, including field staff time but not including research-specific costs such as survey data collection. Including all fixed and variable costs, the cost per tree at the end of the three year contract was US$2.51 lower—US$8.44 per tree—for contracts allocated under the auction.[25] These calculations are sensitive to the scale of the project but indicate that the tree survival differences carry cost implications for the project designer in spite of the piece rate nature of the incentive.[26]

The estimates of the costs per contract can be compared with projections of the carbon sequestration benefits from the trees. Using species-specific allometric equations to transform biomass into carbon storage, Kachamba (2008) projects that each tree had sequestered only 0.013 tons of carbon after 3 years. Using a social cost of carbon of US$21, this implies sequestration benefits of US$0.26 per tree at the end of the contract. The value increases over time as the tree grows. After 20 to 25 years, it is projected to surpass the per tree costs, regardless of how contracts are allocated, with a sequestration value per tree of US$19.70. If carbon sequestration is the only social benefit generated by the program, then there are more cost effective ways to sequester carbon.

Both costs and benefits to the landholders are more difficult to quantify. Benefits may include soil fertility, erosion mitigation, wind blocks, firewood, and poles from construction. Local imperfections in land, labor, and credit markets imply that market prices for inputs and outputs do not reflect the true costs to the landholder. Appendix Table 2 uses market prices for inputs and outputs to estimate total costs associated with the contract as a benchmark against which to compare auction bids. An analysis of this type, using market rates and approximating input costs, is a standard approach to setting posted price offers in developing country payment for environmental services programs.

Land markets are highly imperfect in the study setting, with few incidents of land rental reported in the baseline data. A household that takes land out of production in order to implement the contract may lose crop income, which is valued well above potential income from the contract. Most baseline survey respondents

[25] These calculations assume that both an auction and a lottery are conducted for all eligible landholders as a group, and include transport costs for the participants. For a posted price offer, it would be possible to implement the mechanism by going door to door, however, the costs of such an approach would not necessarily be lower than transporting participants to a central site and completing the exercise in a single day.

[26] Evidence on the price elasticity of survival outcomes would offer further guidance to the organization on the tradeoff between higher fixed costs associated with more contracts and a lower piece rate incentive.

report participation in the casual labor market (around two-thirds of respondents), which implies that informal market wage rates may reasonably approximate the opportunity cost of labor.[27] At agricultural labor market prices, the opportunity cost of the contract for a household with surplus land is approximately MWK 19,600. The median auction bid was MWK 20,000. Because the presence of land or labor constraints are difficult to observe at the household level, a further analysis of how costs varied with the targeting approach or how they relate to tree survival outcomes is beyond the scope of the available data.

V. Conclusion

This study offers empirical evidence on the importance of private information for targeting public funds for environmental land use activities in Malawi. A straightforward field experiment directly compares outcomes when participants self-select into the program with a no-targeting alternative. The program offered landholders a piece rate subsidy for planting trees, which is paid out conditional on tree survival at four intervals over three years. Tree survival was significantly higher among farmers who revealed a low willingness to accept through an incentive-compatible auction than among landholders who were randomly assigned a contract. The treatment effects were sustained over the three years of the contract, which suggests that cross sectional heterogeneity may be more important for determining tree survival than are unanticipated intertemporal shocks. However, the counterfactual of no-targeting provides only a weak test of the efficiency gains from the auction. Targeting on observables is a common alternative approach, and simulations using the tree survival data and landholder characteristics show that, with enough household-level data, targeting on observables may also allocate contracts to landholders with high survival outcomes. Whether the information is private and revealed through self-selection or observable and collected through surveys and pilot implementation outcomes, difficult to observe household-specific information is important for efficient targeting.

The paper extends previous research on targeting on willingness to pay for health products (e.g., Ashraf, Berry, and Shapiro 2010; Cohen and Dupas 2010; Berry, Fischer, and Guiteras 2012) to a new context, and suggests that willingness to accept may, in the case of environmental subsidies, be a relevant characteristic for self-selection. However, the results focus on a narrow source of variation—selection around a single contract price, which was determined by the budget available for contracting, the number of participants in the auction, and the distribution of bids. Changing any of these factors would change the clearing price, affecting both selection and the incentive conveyed by the contract price. Thus, considerations of optimal subsidies must also account for the price elasticity of tree survival, which requires further empirical research.

[27] Note that tree planting under the contract coincides with the planting of other crops and also the peak period of labor demand.

Appendix

Table A1—Household Characteristics by Participation Status

	Nonparticipants		Participants		Difference p-value
	(1)	(2)	(3)	(4)	(5)
Household size	4.231	(2.590)	4.543	(2.092)	0.383
Education (1–5)	2.181	(0.854)	2.215	(0.843)	0.812
Laborsharing group (0/1)	0.289	(0.460)	0.434	(0.496)	0.084
Casual labor income (0/1)	0.789	(0.413)	0.663	(0.473)	0.111
Use family labor only (0/1)	0.744	(0.442)	0.760	(0.428)	0.821
Stated labor constraint (0/1)	0.077	(0.270)	0.044	(0.205)	0.350
Minutes gathering firewood	124.641	(105.256)	116.134	(90.719)	0.580
Total landholding (acres)	5.545	(5.993)	4.956	(3.269)	0.387
Number of fields	1.237	(0.490)	1.390	(0.662)	0.163
Total number of crops	3.051	(1.099)	3.083	(1.162)	0.869
Minutes from home to field	29.330	(32.380)	22.377	(22.310)	0.075
Cash crops (0/1)	0.868	(0.343)	0.820	(0.385)	0.452
Stated land constraint (0/1)	0.051	(0.223)	0.037	(0.189)	0.655
Past borrowing (0/1)	0.282	(0.456)	0.309	(0.463)	0.723
Age of the participant	35.581	(10.895)	38.313	(15.956)	0.767
Female participant (0/1)	0.718	(0.456)	0.487	(0.500)	0.006
Months of food shortage	4.821	(2.723)	4.201	(2.174)	0.096
Prior tree planting (0/1)	0.553	(0.504)	0.494	(0.501)	0.491
Prior contact with NGO (0/1)	0.289	(0.460)	0.286	(0.453)	0.968
Risk preferences (1–3)	2.171	(0.923)	2.135	(0.928)	0.821
Time preferences (1–6)	4.315	(1.969)	3.287	(2.181)	0.005
Mistrusts outsiders (1–3)	2.104	(0.852)	1.858	(0.760)	0.056
Willingness to try tech (1–3)	1.188	(1.295)	1.284	(1.328)	0.664
Asset index	10.641	(3.616)	11.233	(3.293)	0.287
Observations	39		433		

Notes: Means are reported from baseline survey data, for participants and nonparticipants, with standard deviations in brackets. Difference in means: See Table 1 for description of regressors.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Table A2—Estimated Contract Costs

	Labor costs		Input costs	Land costs	
	Field preparation (annual)	Weeding and harvest (annual, variable)	Inputs (annual)	Production income (annual)	Net
Panel A. Alternative land uses					
Local maize	8,400	22,800	2,400	141,210	107,610
Soya	10,200	15,600	300	445,050	418,950
	(one-time)	(annual, variable)		(annual)	
Panel B. Tree planting contract					
Productive land	4,000	14,400	0	12,000	−6,400
Idle land	5,200	14,400	0	12,000	−7,600

Notes: All data are from Ntchisi District Agriculture and Development Officers and the 2007 Crop Estimate Report for Ntchisi District, Malawi. Labor costs are calculated at average market wage rate for day casual labor. Crop income calculated using local market rates from 2008 and average yields from 2007. All cost estimates are based on inputs and outputs for half an acre for the two leading alternative land uses, local maize and soya bean, and for tree production. Interviews with District Agriculture and Development officers provided information on labor requirements for the contract and alternative land uses.

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