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Allocation in environmental markets: A field experiment with tree planting contracts*

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Abstract

The use of market based instruments in developing countries is rapidly expanding, particularly in environmental policy. Several different market mechanisms theoretically lead to efficient allocation of investments in environmental quality without ex post trading, though their empirical performance in these contexts remains untested. This study provides the first evidence from a developing country field experiment to directly compare alternative allocation mechanisms: a uniform-price, sealed bid procurement auction and a posted offer market. The experiment was conducted in Malawi for the allocation of tree planting contracts. Results reveal highly divergent outcomes for the two strategically equivalent mechanisms. The auction set the clearing price for both mechanisms and enrolled the 38 percent of the auction treatment group that bid below the price. In the posted offer treatment group, 99.5 percent of participants accepted the contract at that price. Compliance results show significantly more trees surviving per contract allocated under the auction. Results point to a violation of procedure invariance and show a tradeoff between quantity and quality across the two mechanisms. The auction achieves a better selection of high compliance landholders, but potentially at greater cost than the posted offer market.

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1 Introduction

Consider the following allocation problems in environmental policy: sulfur dioxide emissions allowances in the United States, waste discharge permits along a river in Wisconsin, and payment contracts for groundwater recharge through revegetation in Australia. In the first, allowance trading following the initial allocation resolves multiple design challenges and makes the initial allocation irrelevant, at least in theory (Montgomery 1972). Through trading, the market arrives at a least-cost allocation and an equilibrium price, and provides an opportunity for market participants to learn about the market. In the second case, trading is impeded by transaction costs (Hahn 1989), and in the third case, trading is prohibited because investments must be long term to yield environmental benefits (Whitten et al. 2003). In these latter two situations, the initial allocation affects the aggregate costs of achieving policy objectives (Stavins 1995, Ferraro 2008), and policy-makers must choose among one-shot mechanisms to determine the initial allocation and the clearing price.[1]

Strategically equivalent mechanisms will, in theory, lead to similar allocations at a given price, though empirical studies show that market outcomes may be sensitive to the design of market institutions (Lusk et al. 2004, Lucking-Reiley 1999). This sensitivity is exacerbated in new markets involving inexperienced participants and unfamiliar goods and services (e.g., List & Shogren 1999) and in one-shot markets where learning opportunities are limited (Fehr & Tyran 2005). Complex mechanisms that perform well in theory or in repeated markets may not achieve efficient outcomes if participants do not understand the payoffs (Nalebuff & Bulow 1993). Both the academic and practitioner literatures recognize this tradeoff between efficiency and simplicity in the choice of market mechanism (FAO 2007, Kagel & Levin 2009).

The tradeoffs between alternative mechanisms are difficult to evaluate a priori. In most settings, a single mechanism is used at any given point in time and isolating the effect of alternative designs is confounded by other changing variables. Consequently, existing evidence has primarily been collected in laboratory experiments (Kagel 1995, Cason, Friedman & Milam 2003) or on the internet, where multiple market formats are often used for the same good at the same point in time (Lucking-Reiley 1999, Lee & Malmendier 2007).

This study provides the first evidence from a developing country field experiment to directly compare two strategically equivalent allocation mechanisms. Random assignment to a uniform-price, sealed bid procurement auction or a posted offer market facilitated compari-

[1] Efficient design of one-shot mechanisms is by no means limited to environmental markets. Many procurement settings face the very challenges described here, and economics experiments frequently compare outcomes in one-shot and repeated markets.

son of allocation and contract compliance results.[2] The experiment was conducted in Malawi for the allocation of tree planting contracts on private land. In contrast with theoretical predictions, results revealed a dramatic difference in the performance of the mechanisms. The auction determined the clearing price in both mechanisms and enrolled the 38 percent of auction bids below that price. In the posted offer treatment group 99.5 percent of participants accepted the contract at the clearing price. According to the willingness to accept values revealed in the experiment, enrolling 99.5 percent of the market would cost 10 times as much under the auction as under the posted offer market. However, at the auction clearing price, the auction out-performed the posted offer mechanism in terms of ex post contract outcomes. Compliance, measured by the number of surviving trees, was 9.5 percent or over a third of a standard deviation higher among landholders who received the contract through the auction, though all contracts paid only for surviving trees.

The divergence in mechanism performance suggests a violation of one of the assumptions underlying the theoretical proposition of equivalence of the sealed-bid, uniform price auction and the posted offer market. Three necessary assumptions are examined in turn. The first two, independent private value and no collusion, are upheld by a combination of the main results and supplementary data. The third assumption, individual rationality, cannot be supported. A survey of hypothetical valuations replicates the divergent experimental outcomes. The evidence suggests a violation of procedure invariance in the pattern of significantly lower willingness to accept under the posted offer market, which mirrors the divergence found in contingent valuation studies. Without procedure invariance, the same price will not clear different markets making outcomes sensitive to the choice of mechanism.

The choice of allocation mechanisms in the study highlights the tradeoffs facing a policy maker. An auction and a posted offer market both select for the lowest cost sellers, yet the posted offer market relies on a price determined ex ante while the auction reveals the clearing price. Though this property makes the auction more efficient in the face of uncertainty around the correct price, responding to a fixed price presents a simpler valuation task to a market participant than does identifying the dominant bidding strategy in an auction. Many of the policy design challenges described are exacerbated in developing countries, where the use of market based instruments is increasing in spite of a lack of evidence about the performance of alternative mechanisms in these contexts. Higher transaction costs in

[2] The uniform price was set by the first rejected offer, making it strategically equivalent to a single unit second price auction, in which truthful bidding is incentive compatible (Vickrey 1961). A fixed budget determined total demand.

developing countries limit trading opportunities and make one-shot outcomes more important (Blackman & Harrington 2000). Poorly functioning ancillary markets for inputs and outputs makes it more difficult to estimate opportunity costs and set prices ex ante. Finally, market participants in developing countries may disproportionately benefit from the learning opportunities associated with repeated markets. My empirical findings suggest a different interpretation of the efficiency-simplicity tradeoff between the two mechanisms, under which quantity in the initial allocation is traded of with quality in the implementation.

Potential payoffs from targeted evaluations of alternative allocation mechanisms are high, particularly in developing countries where the use of market based policy approaches is increasing but evidence is scarce (Blackman & Harrington 2000). Developing country government programs have spent over a billion dollars in recent years on incentives for land use changes that improve environmental outcomes (Ortiz & Kellenberg 2002, Uchida et al. 2005, Munoz-Pina et al. 2008). In addition, the voluntary market for carbon offsets traded over 700 million dollars worth of emissions reductions in 2008, a third of which came from land use projects (Conte & Kotchen 2009). Such investments are likely to increase as binding climate policies come into force. For example, pending climate legislation in the United States allows up to a billion tons of carbon offsets from international sources, which will offer incentives for activities that reduce emissions or sequester atmospheric carbon, such as reforestation and afforestation projects.

Though the data from the field experiment cannot be used to speculate about the sensitivity of compliance outcomes to contract prices, the results at the clearing price suggest that when the selection pressures are strong, such as when supply substantially exceeds demand, the auction is better able to identify high compliance individuals, though doing so substantially reduces the initial quantity of contracts supplied at the clearing price. Simulation of alternative allocation rules used by a social planner consistently falls short of the the contract implementation outcomes of the auction, which suggests that the auction may successfully sort on unobservables.

The next section offers a simple model of opportunity cost and a theoretical proposition of equivalent performance of the auction and posted offer market. Section 3 describes the experiment and data collection. Results from the allocation mechanisms and the contract compliance are presented in Section 4, which includes an examination of the spatial structure of the outcomes. Section 5 brings the main results and ancillary data to bear on the three assumptions underlying the proposition of equivalent performance. Section 6 discusses implications for the design of future policy and simulates the allocation outcomes for contract allocation by a social planner. Section 7 concludes.

2 Model

Tree planting on private land imposes costs that are borne by the landholder. To the extent that the same trees provide positive externalities, subsidies to encourage tree planting may be socially desirable. Whether the benefits of such payments exceed the costs depends in part on aggregate willingness to accept a tree planting contract at a given price. The contract studied here pays individuals on a per-surviving tree basis and imposes no financial penalties for default. It is assumed (and empirically validated) that individuals do not value the contract for its option value alone, due in part to the fixed costs associated with land use changes. Individual opportunity cost is a function of land, labor and other inputs diverted toward tree planting. A simplified household model that accommodates missing markets through both tradeable ($j \in L$) and nontradeable ($j \in U$) inputs and outputs (e.g., de Janvry et al. 1991, Taylor & Adelman 2003) helps formalize the sources of opportunity cost to the landholder and provides a theoretical benchmark for the study. In the absence of the contract, a household is assumed to choose consumption c and production q to maximize its utility of consumption:

$$\max_{c,q} U(\mathbf{c}, \mathbf{z})$$

$$\text{s.t.} \quad \begin{array}{ll} \sum_{j \in L} p_j c_j \leq \sum_{j \in L} p_j (q_j + T_j) & \text{cash income constraint} \\ q_j + T_j \geq c_j, \; j \in U & \text{equilibrium for nontradables} \\ p_j = \overline{p}_j, \; j \in L & \text{exogenous market price} \\ G(\mathbf{q}, \mathbf{z}) = 0 & \text{production technology} \end{array}$$

Following de Janvry et al. (1991), $\mathbf{z}$ is a vector of household characteristics and preferences and the household faces constraints associated with both tradable (L) and non-tradable (U) inputs and outputs. The cash income constraint requires that expenditures are less than or equal to income from production q_j and the endowment T_j of tradeable good $j \in L$. The equilibrium for nontradables constrains consumption c_j by the household production q_j and endowment of nontradeable $j \in U$. For traded goods, market prices are defined by $\overline{p}_j$. Endogenous prices for nontradables can be defined from the shadow prices for tradables (λ) and nontradables (μ) as $p_j = \mu/\lambda$, which allows both to enter the Langrangian as a single expression. The production technology is a function of the input and output vectors $\mathbf{q}$ and household characteristics $\mathbf{z}$ and imposes a shadow price of ϕ on production.

$$L = U(\mathbf{c}, \mathbf{z}) + \lambda[\sum \overline{p}_j(q_j + T_j - c_j) + S] + \phi G(\mathbf{q}, \mathbf{z})$$

The first order conditions for the Lagrangian produce a system of output supply and input demand equations $q_j = q_j(\mathbf{p}, \mathbf{z})$, the latter of which enter negatively. To further differentiate inputs from outputs, define the demand for inputs of labor (l), land (k) and other inputs (f) such as fertilizer used in the production of output j as q_{js} where $s \in \{l, k, f\}$. Maximized production profits are therefore:

$$\Pi^* = \sum_j [p_j q_j^*(l, k, f, z) - \sum_s p_{js} q_{js}^*] + p_t q_t^* - \sum_s p_{ts} q_{ts}^*,$$

where q_t^* is the equilibrium production of trees that require q_{ts}^* inputs. If inputs or outputs are not traded on the market then p_j and p_{js} refer to the shadow price, which will differ from the market price.

The contract requires allocation of a fixed amount of additional land and labor to tree production at q_t' and offers a new price for trees produced, p_t'. Previous tree planting is not rewarded under the contract. The change in quantity of tree production enters the landholder's re-optimation process as an additional constraint, while the change in the price on trees enters via the budget constraint. After reallocation of inputs and outputs to accommodate the new maximized utility, production profits shift to Π'.[3] The household prefers the new allocation if:

$$\Pi' - \Pi^* = \sum_j [p_j(q_j' - q_j^*) - \sum_s p_{js}(q_{js}' - q_{js}^*)] + (p_t' - p_t)q_t' - \sum_s p_{ts}(q_{ts}' - q_{ts}^*) \geq 0.$$

Rearranging terms solves for the price incentive that makes a household willing to accept the contract:

$$\alpha = p_t' \geq \frac{\sum_j [p_j(q_j' - q_j^*) - \sum_s p_{js}(q_{js}' - q_{js}^*)] - \sum_s p_{ts}(q_{ts}' - q_{ts}^*)}{q_t'} + p_t$$

Households' minimum willingness to accept values (α) for the contract are equal to their opportunity cost and vary in the shadow prices of land, labor, other inputs, and household characteristics (via both the production technology and utility maximization).[4] The model can be used to generate comparative static predictions for willingness to accept.

[3] Since consumption and production are not separable if markets are missing, this reallocation represents a simplification of the household adjustment to the change in prices for trees. De Janvry et al. (1991) show the full predictions of the model for the different scenarios of market completeness.

[4] The effects of the contract relative to the local economy are small so market prices should not be affected.

In keeping with previous work on household decision-making in contexts with missing markets, the model is written deterministically, and therefore predicts that all households willing to accept a contract that requires q_t trees produced at price p_t' should fully comply with the contract. Non-compliance with the contract can come from two sources. First, agricultural households in developing countries face many sources of uncertainty including weather, crop prices and illness. A more realistic modeling approach treats parameters as stochastic and maximizes expected utility.[5] Realized opportunity cost may exceed expected opportunity cost due to ex post shocks or incorrectly formed expectations, leading to less than full compliance with the contract, particularly for inputs that are variable rather than fixed. Second, individuals may incorrectly calculate opportunity cost, due to limited cognitive ability or a misunderstanding of the valuation task. It is assumed that, conditional upon receiving a contract, compliance is increasing in the surplus over the true willingness to accept or opportunity cost, α, that the landholder receives ($\partial c/\partial(P-\alpha) > 0$). Greater surplus helps buffer both ex post shocks and calculation errors.

Proposition. *Under assumptions of (1) private and independent values, (2) no collusion and (3) purely rational individuals, a sealed bid, uniform price (generalized Vickrey) auction will result in the same allocation as a posted offer market that uses the auction clearing price as the offer price.*

Proof. As shown in the standard results for a sealed bid second price auction with single unit demand and independent private values, rational bidders who do not collude have a dominant strategy to bid their true value α (Vickrey 1961, Krishna 2002).[6] The auction therefore leads to a ranking of bids by willingness to accept and all bidders with valuations below the clearing price (set by the first rejected bid), $\alpha < P$, receive a single object in a procurement setting.

In a one-shot posted offer market, individuals have a dominant strategy to accept the price offer if it is greater than their true value, $P > \alpha$. If indifferent individuals reject the offer, then the allocation, defined by who receives a contract, is the same as for the uniform price auction at a given price.■

Indicate the auction market and the posted offer market by the subscript $M = \{A, PO\}$. Let the allocation at price P be described by the supply, $y_M(P)$, and compliance be the

[5] Note that risk aversion could result in a certainty equivalent that either raises or lowers willingness to accept. If the riskiness of income under alternative land uses exceeds the riskiness of income under the contract, then the contract actually reduces uncertainty and lowers willingness to accept.

[6] Risk aversion does not affect bidding incentives in a Vickrey auction (Krishna 2002).

aggregate tree survival outcomes, conditional on the allocation, $c|y_M(P)$. In the language of the household model, all individuals with valuations $\alpha < P$ should receive a tree planting contract whether a uniform price sealed bid auction or a posted offer market is used to allocate the contracts. Therefore, both the number and type of households allocated a contract should be equivalent, as should the compliance with the contract. The proposition gives rise to allocation and compliance predictions under the auction and posted price mechanisms:

$$y_A(P) = y_{PO}(P)$$
$$c|y_A(P) = c|y_{PO}(P)$$

Experimental outcomes that contradict these predictions indicate a failure of one of the assumptions to the proposition: 1) independent private values, 2) no collusion or 3) individual rationality.

3 Experimental context and design

In cooperation with an international organization, I implemented a study designed to test the equivalence of a uniform price, sealed bid auction and a posted offer market in Ntchisi District in Malawi, which is a relatively flat and arid part of the country, heavily dependent on rainfed agriculture. Households in the District own an average of 7.25 acres and grow primarily maize, soya, tobacco and potatoes. Tree planting on private land produces both private benefits, including soil fertility and timber income, and public benefits, including carbon sequestration. The temporal delays associated with the private benefits and the externalities associated with the public benefits result in little private investment in tree planting. International development organizations are implementing projects in Malawi around land use payments for avoided deforestation, carbon sequestration, and watershed services (Chiotha & Kayambazinthu 2009). The research was coordinated with ongoing activities implemented by The World Agroforestry Centre (ICRAF) and the Malawi government, and employed an experimental design at the allocation stage of a program to subsidize tree planting on private land.

While the small size of the intervention limits actual carbon sequestration impacts, the questions of program design are similar to those faced by potential carbon offset projects and agri-environmental programs at much larger scales. For example, in designing its payments for hydrological services scheme, Mexico's government considered the use of a procurement auction to elicit opportunity costs associated with forest conservation, however, officials rejected the approach as "too innovative" (Munoz-Pina et al. 2008). A posted offer price was eventually offered, based on estimates of the opportunity cost of foregone deforestation. Mexico allocated an annual budget of 18 million dollars to payments, but more households signed up for the program at the offered price than could be paid under the budget, which may indicate that the price was too high. Other programs in developing countries have similarly favored posted offer markets for their simplicity (FAO 2007), potentially at the expense of the efficiency gains that developed country programs have achieved through the use of auctions (Latacz-Lohmann & Van der Hamsvoort 1997, Latacz-Lohmann & Schilizzi 2005).

The data presented in this paper include the baseline household survey, the allocation of tree planting contracts and the first of four contract monitoring rounds, six months after the initial allocation. The tree planting contract requires that land holders set aside half an acre of land and plant 50 seedlings provided by the implementing organization. At each of the four monitoring periods over three years, landholders are paid a fixed price for each surviving tree. The costs to the landholder associated with the contract are further examined in Section 4.3.

3.1 Study sample

Data on covariates relevant to the household model presented in Section 2 were collected in a baseline survey of 472 households in June of 2008. Every household in 23 villages spread over four extension planning areas responded to an extensive household questionnaire. The implementing organization selected the villages based on their previous involvement in ICRAF activities and the capacity of the government extension staff to assist with the study. The initial selection of villages for participation in the study is therefore not random, though treatment assignment within the sample is random. The baseline survey originally included 27 villages, four of which were later dropped from the study for budgetary reasons (one from each of the four extension planning areas included in the study). The survey collected household information on labor, land and household characteristics and preferences. A simple random draw assigned eligible households to the auction or the posted offer treatment group. Households with less than one acre of private land were deemed ineligible to receive a contract and excluded from the randomization.

Table 1 shows that the randomization successfully allocated individuals evenly into the two treatment groups for a set of variables relevant to household willingness to accept the tree planting contract. These variables are used as the standard set of regressors throughout the analysis.

[Table 1]

The only variable that differs at the $p < 0.10$ level is response to a question measuring trust in outsiders. A slightly greater number of individuals in the posted offer group indicate that outsiders are less trustworthy than members of their own community. All other variables are statistically equivalent between treatment groups. A joint F-test on these variables indicates no statistical difference between the treatment groups ($F(24, 430) = 1.16$).

The summary statistics (Table 1) show that the average household contains around 4.5 individuals and the household head has some primary school education (category = 2). Around 40 percent of households participate in labor-sharing and around two-thirds seek casual labor for income. The average household reports food shortages for over four months of the year and has about five acres of land. Households plant just over three different types of crop and around half had some trees growing on their land at the time of the baseline survey. On average, fields are more than 20 minutes' walk from the home and many report borrowing money, though access to formal credit is very low (around 4 percent).

3.2 Contract allocation

Invitations to participate in the allocation mechanisms were delivered by government extension agents one week in advance, and participants were told the date of the event, informed about the show up fee and told they would be given a chance to make some decisions about whether their household wanted to be involved in a tree planting project. The invitation provided no specific detail about the contract or the allocation process. The invitations balanced concerns about collusion with a need for enough information to encourage participation by the relevant household decision-maker. Households were pre-assigned to one of the two treatment groups, though the project staff involved were not aware of the assignment at the time of invitation delivery.

Of the 472 invited households, 433 (91.7 percent) participated in the allocation of contracts. Participants were significantly more likely to be part of a labor sharing group, less likely to seek casual labor as a source of income, and have significantly more fields than non-participants. Compared with non-participants, participants are also less frequently female, are significantly less patient, and have more trust in outsiders. Participation was significantly higher among households assigned to the auction treatment group than the posted offer group ($p < 0.05$), a difference that is attributed to chance since invitations to the two groups were identical and invitations did not differ by treatment group.[7]

Both treatments were implemented at the same time, to avoid information spillovers between treatment groups. During registration, which occurred in the same place for both treatment groups, the treatment assignment determined which of two locations the participants were sent to, and the color-coded participation materials that they received. The two treatments had different enumerators, but the scripts revealed exactly the same information except for the description of the mechanism through which the contracts would be allocated. In both mechanisms, 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 received several examples.[8] 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 was limited, but the size of the budget was not announced.

Posted offer participants were given a take it or leave it price, but were not informed about the source of the price. Decisions in both the auction and the posted offer market were submitted via sealed bid. In both treatments, individuals used color coded slips with pre-printed identification numbers to record their decisions, which were then collected by assistants based on approximate seating location.

3.3 Monitoring and payments

A project representative visited all individuals who received a contract one week after the allocation for contract signing. In-home visits gave individuals a chance to decline the contract if reflection after the allocation stage led them to change their minds about the contract terms. Between September and February, project staff and government officials conducted three trainings for contract holders to ensure that heterogeneities in information about tree planting and care did not drive differences in compliance outcomes. All contract holders received 50 seedlings in December 2008, just before the planting season. Contract compliance was measured six months after seedlings delivery, with payments based on the number of trees surviving at the time of monitoring.

[7] Conditional on participation, the treatment groups remain balanced on the set of household variables shown in Table 1 ($F(24, 429) = 1.12$). No variable differs signficantly between groups at $p < 0.05$.

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

A team of representatives from the government, the implementing organization and local officials conducted the monitoring. The landholder accompanied the monitoring team to the field where the trees were planted and conducted a count of surviving trees, based on pre-determined criteria. Landholders received notice approximately one week in advance of the monitoring, so some may have taken precautions to improve the field before the visit, but the advanced notice was unlikely to affect survival rates. The landholder and the monitoring team reached consensus on the survival status of each tree before moving on to the next tree. At the end of the session, the landholder signed the monitoring ledger to verify agreement on the survival outcomes. The lead monitor also conducted an assessment of the maintenance of the field including checks for several land care practices that had been part of the trainings. These included constructing firebreaks around the edge of the field, pits to catch water around each tree, weeding and distributing mulch for soil fertility. The monitor assigned a score based on these criteria without knowing the treatment group to which the individual belonged. Payments were delivered to the individual's home one week after the conclusion of monitoring and all aspects of the monitoring results remained private.

3.4 Attrition

Overall, pure attrition rates are very low. Four individuals out of the 176 who received contracts did not plant the trees. Two of the cases of pure attrition occurred in each of the treatment groups. Exit interviews were conducted on all individuals who dropped the contract. One individual from the posted offer group did not sign the contract after receiving it through the lottery, saying that he would be traveling too much in the coming months to implement the contract. Another individual, also from the posted offer group, moved away after receiving the contract. Two other individuals, from the auction group, also dropped the contract and both cited unanticipated shocks as a reason for not continuing with the contract.[9] A fifth individual is dropped from the analysis because of malicious interference by a neighbor.[10] In addition, two individuals who participated in the allocation process had died by the time of the first monitoring period, though neither had received a contract. In

[9] One individual refers to illness in the family that reduced the labor supply and the other individual claims that the seedlings were stolen. The latter could have arguably been prevented with greater effort (watchfulness).

[10] The individual, from the auction treatment group, planted the trees but they were uprooted by a neighbor who claimed that the land belonged to her. The village chief ruled in favor of the contract recipient, but at that point the trees had already died. This observation represents the sole case of an involuntarily ended contract, and is dropped from the analysis. The individual received a payment in the first monitoring period equal to the average payout in his village.

the analysis of contract performance, the emphasis is on marginal compliance, or choice of effort, rather than idiosyncratic factors that lead to the described attrition on the margin. Robustness checks on compliance outcomes that preserve the original treatment assignment (intention to treat) include individuals who did not plant the trees.

4 Results

The primary results from the field experiment include the number and type of individuals willing to accept the contract at the clearing price under each of the allocation mechanisms and the contract compliance for all individuals receiving a contract. Allocation outcomes are examined first, followed by data on contract compliance, which includes an investigation of spatial determinants of outcomes. Market prices for inputs and outputs under alternative land uses and under the contract calibrate the experimental results.

Table 2 summarizes allocation and compliance results. Eighty-five individuals (37.4 percent of the market) received contracts through the auction, which revealed a clearing price of Malawi Kwacha (MWK) 12,000 based on the first rejected offer and the available budget.[11] The clearing price was offered to individuals in the posted offer, 99.5 percent of whom accepted the contract. Thus, over twice as many individuals in the posted offer treatment group as in the auction treatment group revealed a willingness to accept below the auction clearing price. The budget available to purchase tree planting contracts was approximately split between the two treatments, which forced the implementing organization to use a lottery to allocate contracts among those who accepted the price in the posted offer treatment. The lottery was conducted on the spot. Ninety-one individuals received contracts under the lottery conducted after the posted offer announcement, resulting in a random allocation of contracts in the posted offer market. A probit regression model of the probability of receiving a contract in each of the mechanisms estimates the overall significance of selection under each of the mechanisms, conditional on the baseline survey measures reported in Table 1.

$$\Pr(contract_i = 1|\mathbf{x}_i) = \Phi(\mathbf{x}_i'\beta),$$

where Φ is the cumulative distribution function of the standard normal random variable and $\mathbf{x} = (l,\ k,\ z)$ is a vector of labor, land and household characteristics from the baseline survey. The likelihood ratio chi-square for the model is statistically significant for the auction

[11] 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).

treatment but not for the posted offer treatment, consistent with the randomness of the lottery. The final row of Table 2 shows that compliance under the auction, in terms of number of surviving trees, was higher than under the posted offer mechanism, implying that the selection effectively identified individuals most able to comply with the contract.

[Table 2]

The remainder of the Section performs further analysis of these summary results, which point to divergent performance by the two strategically equivalent allocation mechanisms and a failure of the theoretical proposition. Section 5 examines alternative explanations for the outcomes.

4.1 Contract allocation outcomes

A comparison of contract allocation outcomes under the two mechanisms provides the first test of the proposition of equal performance. Figure 1 shows the bidding pattern in the auction as a cumulative distribution function, with a positively skewed distribution, a mean of approximately MWK 60,000 and a median of MWK 20,000 (bid curve is scaled in logs). The clearing price is shown by the vertical line at MWK 12,000 on the graph, which crosses the bid distribution just above 0.38 on the cumulative distribution function. Only a single price point is observed in the distribution of willingness to accept for the posted offer treatment group, which coincides with the 99.5th percentile on the vertical axis. The clearing price of MWK 12,000 therefore represents an upper bound on the willingness to accept of 99.5 percent of the posted offer treatment group.

[Figure 1]

4.1.1 Determinants of allocation: Auction

One of the advantages of the auction from the policy maker's perspective is the information revealed through the bids. Bids based on willingness to accept, α, should lead to sorting on the characteristics driving landholder opportunity cost, as measured by a series of baseline survey questions. Table 3 reports the results of a linear regression of logarithm bids on these characteristics ($\mathbf{x} = (l_i, k_i, z_i)$).

$$\ln(bid_i) = \mathbf{x}_i'\beta + d_v + \varepsilon_i$$

Columns 1-3 of Table 3 divide the explanatory variables ($\mathbf{x}_i$) into the major components of opportunity cost: labor (column 1), land (column 2), household characteristics (column 3). A test for heteroskedasticity (Breusch-Pagan / Cook-Weisberg) rejects the null of constant variance, so robust standard errors are reported. Adding village indicator variables, d_v (column 5), increases the explanatory power of the regression, by de-meaning the data at the village level.

[Table 3]

Regressing auction bids on these baseline survey measures shows that predictive ability of these characteristics is moderate, explaining at most 32.4 percent of the variation in logarithm bids (column 5), in part because of the large number of binary or categorical explanatory variables. Village indicator variables (column 5) eliminate the significance of several individual regressors that vary more between villages than within villages. A high intraclass correlation in log bids at the village level ($\rho = 0.315$) falls when bids are conditioned on $\mathbf{x}_i$ ($\rho = 0.194$). The regression is log-linear so coefficients on independent variables can be viewed as percent changes in the dependent variable.

The final column of Table 3 presents the predicted comparative static results according to the household model, taking into consideration both the direct effect on production constraints and the effect on the value of outside options. Though formal derivations are not shown, most predictions can be understood intuitively. For example, more education increases the value of the outside option and increases willingness to accept by about 25 percent. Engaging in casual labor indicates a non-binding labor constraint and households that seek casual labor bid about 50 percent lower than households that do not seek off farm labor. Variation in household-specific shadow prices for trees (p_t) is measured by the minutes that a household spends gathering firewood and prior tree planting. The signs on the coefficients of most variables are in line with the predictions, which is suggestive of sorting under the auction.

As a robustness check, bid ranks are used as the outcome variable, since a simple ranking of bids preserves the ordinal sorting of the auction while eliminating outliers due to bid shading or miscalculation. Replacing the outcome variable in Table 3 with bid rank yields very similar qualitative results, including the direction and significance (though obviously different coefficients) for regression coefficients and the regression model overall.

4.1.2 Determinants of allocation: Posted offer

Table 2, above, shows that the overall insignificance of household level variables for predicting allocation under the posted offer is consistent with the randomness of the lottery used to determine allocation conditional on accepting the price offer. Household variables are jointly statistically indistinguishable between those who did and did not receive a contract ($F(24, 201) = 0.99$), though the lack of stratification and the small sample size results in some significant differences using a two sample t-test for equal means. Mean education level for both those who did and did not receive a contract under the posted offer market lottery is "some primary school" though those who received contracts had slightly higher average attainment ($p = 0.086$). Those who receive a contract have an average of 1.51 fields versus 1.33 fields among those who did not receive a contract ($p = 0.089$). Finally, 90 percent of those who received a contract sell cash crops compared to only 74 percent of those who did not receive a contract ($p = 0.003$). The theoretical impact of these jointly insignificant selection effects in the lottery are mixed. Higher levels of education and more cash crop sales are associated with higher willingness to accept, while a greater number of fields is associated with lower willingness to accept. These variables will be further studied in the section on contract compliance to ascertain whether they directly affect landholder compliance with the contract.

4.2 Contract implementation outcomes

The proposition of equal performance predicts equivalent compliance outcomes for the auction and the posted offer market, conditional on allocation. Given the observed differences in allocation outcomes, the prediction of equivalent compliance no longer holds. However, comparison of compliance outcomes across the mechanisms offers insights into nature of the divergent allocation. The primary outcome for measuring contract compliance is the number of healthy trees, out of 50, at the time of the first monitoring mission, supplemented by the score assigned by the monitor. The number of healthy trees is highly correlated with the monitoring score (0.77). Though the score is a subjective qualitative measure, it is potentially a better indicator of the effort that the landholder is exerting in implementing the contract, because it is not subject to the same shocks that may affect tree survival and are outside of the landholder's control. Nineteen individuals were in full compliance with the contract, with all 50 trees classified as healthy. Tree survival rates are left skewed for both treatment groups, with a mean of around 40 (81 percent survival), a minimum of 3 and a maximum that is truncated at 50. Figure 2 shows the kernel density estimates (Epanechnikov kernel) and underlying histogram for each survival outcome, by treatment group.

[Figure 2]

Both the mean and the distribution of survival rates are significantly higher for individuals who received the contract through the auction. A two sample t-test for equal means shows a difference in means of 3.68 trees ($p = 0.011$). Comparing distributions in a two-sample Kolmogorov-Smirnov test for equality of distribution also shows a significant difference between the treatment groups ($p = 0.024$).

4.2.1 Individual-level contract implementation

Two outcome measures associated with contract performance, the number of surviving trees and the monitoring score, are used to assess compliance in a regression framework, reported in Table 4.

[Table 4]

Panel A presents the results of an linear regression of the number of surviving trees on treatment indicators and village indicator variables, using ordinary least squares with robust standard errors to correct for the heteroskedasticity in the data. The estimates show approximately 3.7 more trees surviving for contracts allocated through the auction than contracts allocated through the posted offer market. Some of the variation in survival outcomes is correlated at the village level and de-meaning the data to address the intracluster correlation, by adding village indicator variables, lowers the magnitude of the effect to 2.6 more trees surviving in the auction treatment group.

The linear specification in Panel A does not capture the count nature of the outcome variable, nor does it reflect the implicit censoring of the outcome variable at 50 surviving trees, which is the number handed out by the program and therefore the maximum that any individual could have kept alive, regardless of effort levels. Since approximately 11 percent of landholders kept all 50 trees alive, the data suggest that some of these participants exerted effort sufficient to keep more than 50 trees alive. A negative binomial regression accommodates the nonlinearity of count data and the overdispersion in the data better than a Poisson distribution (Cameron & Trivedi 1998). A censored negative binomial regression adjusts the distribution to account for the fact that the maximum outcome is censored at 50 surviving trees.

The censored negative binomial regression is estimated by maximizing the likelihood function

$$L(\boldsymbol{\theta}) = \sum_{i=1}^{N}\{d_i \ln f(y_i|\mathbf{x}_i,\boldsymbol{\theta}) + (1-d_i)\ln(1-\sum_{j=0}^{c-1} f(j|\mathbf{x}_i,\boldsymbol{\theta})\},$$

where d_i is equal to one if the observation is uncensored and to zero if the observation is censored, and c is equal to 50. The uncensored density function $f(y_i|\mathbf{x}_i,\boldsymbol{\theta})$ is equal to the the negative binomial distribution, and the censored density equals $(1-F(c-1|\mathbf{x},\boldsymbol{\theta}))$ when $y = c$.

The censored negative binomial regression coefficients are shown in Panel B of Table 4 and show a significant treatment effect. The coefficients represent the difference in the logs of expected counts of the response variable for a one unit change in the predictor variable, given the other predictor variables in the model are held constant. The marginal effects for the treatment indicator show similar results to the linear regression, with 3.68 more trees surviving for individuals who received a contract through the auction.[12]

The ordered logit regressions reported in Panel C use the monitoring score as the dependent variable and a latent variable model for each of the four cutoffs associated with the monitoring score. The score regressions (Panel C) show a stronger treatment effect than the survival outcomes. The marginal effects associated with the coefficients show that the effect is non-linear and receiving the contract through the auction increases the probability of receiving the highest monitoring score by 9 percent and decreases the probability of having the lowest score by 4 percent.

To detect the effect of the attrition described in Section 3.4, I re-run these regressions to include the four individuals who did not implement the contract. Coding the number of surviving trees and the monitoring score for these individuals as zeroes slightly lowers the coefficients on the regression of outcome on treatment in each of the three specifications, and increases the standard errors slightly. All three treatment effects retain their significance levels.

Pooling the treatment groups combines the direct effect of regressors on compliance outcomes with the effects of selection in the auction. Restricting the regression of tree survival outcomes on baseline survey variables to the posted offer group uses the random assignment under the lottery to isolate the effect of regressors on outcomes that is independent of the

[12] As a robustness check, the analysis is run without the censoring, and as a Poisson model. The marginal effects and the significance are robust to these alternative specifications. Inverting the outcome measure to regress the number of dead trees on explanatory variables, with censoring at zero, also produces equivalent results.

selection effects in the auction. Table 5 shows estimates of the direct relationship between contract compliance and labor, land and household characteristics for the posted offer group. Assuming compliance is increasing in the surplus over willingness to accept provided by the contract price ($\partial c/\partial(P-\alpha) > 0$), the predicted relationships between the outcome variable and the regressors is shown in the rightmost column. A linear model and a negative binomial model are reported in Table 5, with the latter accounting for the count nature of the outcome variable.

[Table 5]

Most of the coefficients are signed as expected, and some, such as level of education and months of food shortage, both significantly predict bids in Table 3 and tree survival outcomes in Table 5. Section 4.1.2 described some selection in the posted offer market lottery outcomes for individuals with higher levels of education, more fields and cash crop production. Education and cash crops are both significantly related to lower tree survival, suggesting that the selection may have lowered compliance relative to a lottery outcome balanced on those two variables. Comparing mean education levels and cash crop sales for posted offer contract recipients with those who received a contract through the auction shows no significant difference across treatments, and indicates that the observed contract compliance differences are unlikely to be explained by these two variables.[13]

4.2.2 Spatial effects on contract implementation

Though treatment groups were balanced at the village level, the village in which a contract recipient lives is predictive of both selection under the auction and contract compliance, which could be due to a number of factors. First, villages may proxy for other unobservable determinants of willingness to accept or opportunity cost, such as social capital, which should affect auction selection. Second, some villages may be located in microclimates or soil conditions more suitable for tree cultivation, which will affect auction selection if individuals are aware of these factors. Third, information flows more easily within villages and contract holders within a village may have learned from each other about how to keep the trees healthy, which occurs after contract allocation and is therefore less likely to affect auction selection.

[13] The variable measuring previous experience with the implementing organization is also balanced among contract holders across the two treatments. Thus, differential contract enforcement based on past interactions should not affect compliance results.

The random allocation under the posted offer mechanism allows for a separation of the effects of village location on contract compliance that function via selection from effects that are due to ex post learning or social influences. The coefficients in Table 6 report regressions of selection (contract allocation) and contract compliance (tree survival) on village indicator variables, separated by treatment group.

[Table 6]

Column 3 replaces the village level fixed effects with two different explanatory variables that describe how others in the village received their contracts. A count of the number of contract holders in the village who received the contract as a result of selection under the auction as well as a count of those who received the contract as a result of random allocation both enter on the righthand side of the estimation and are normalized for the village population. In the posted offer market (Panel B), village indicators do not significantly predict contract allocation (Column 1) but do predict contract compliance (Column 2), suggesting that spatial location is important for survival even if allocation is random.

As a simple correlation, the relationship between the number of individuals with contracts in the village and compliance outcomes appears positive and highly significant ($p = 0.006$). Breaking the effect apart by treatment shows that the effect is heterogeneous. An increase in the number of individuals selected for under the auction does not improve performance for the auction treatment group (Panel A, column 3), but does have a significant positive effect for the posted offer treatment (Panel B, column 3). An exogenous increase in the number of contracts in the village as a result of the lottery has small positive effect on compliance outcomes in the auction treatment group (Panel A, Column 3), and an insignificant and negatively signed effect for the posted offer group (Panel B, Column 3). Overall, the greatest benefits from an additional contract awarded within a village accrue to someone who received the contract through the lottery, and only if the additional contract in the village was selected for under the auction. These results offer some evidence for peer effects in contract implementation.

Village location proxies for spatial location but does not necessarily pick up other types of spatial spillovers. Spatial regressions are used to further examine the importance of spatial autocorrelation in determining contract compliance. The spatial models take two forms that differ in their assumptions about the underlying structure of the spatial effects (Ward & Gleditsch 2008). Both use a spatial weighting matrix $\mathbf{W}$ that assigns weights to other observations as a function of distance, and assigns connectivity based either on a threshold distance value or a fixed number of neighbors. The chosen spatial weighting matrices are based on spatial autocorrelation assumptions designed to mimic the average effects of the village clusters, and either assign connectivity to 18 neighbors, which is approximately the average number of participants per village, or to the neighbors within 2 kilometers, which is the average total land area per village. The first, a spatial lag model assumes that higher contract compliance by individual i's neighbor directly affects i's own compliance and estimates

$$y_i = \mathbf{x}_i'\beta + \rho\mathbf{w}_i'y_i + \epsilon_i,$$

where $\mathbf{w}_i'y_i$ are the weights assigned to the outcomes of i's neighbors. A spatial error model instead assumes that the underlying error structure is spatially autocorrelated due, for example, to omitted variables that are spatially autocorrelated and affect outcomes directly. The model estimates

$$y_i = \mathbf{x}_i'\beta + \lambda\mathbf{w}_i'\xi_i + \epsilon_i,$$

where $\mathbf{w}_i'\xi_i$ is the spatial component of the error, weighted for proximity, and λ is the extent of correlation in errors given the spatial weighting matrix.

The maximum likelihood estimation results for the spatial lag (Panel A) and spatial error models (Panel B) are shown in Table 7.

[Table 7]

The likelihood ratio test for spatial dependence is reported for all regressions but is significant only for the spatial lag model with a 2 kilometer weighting matrix. Both the coefficient on lagged outcomes and the coefficient on the spatial component of the errors are significant, while the coefficients for the fixed number of neighbors are not. This suggests that physical distance matters more than a certain number of people to learn from, but does not rule out social spillovers since the lagged spatial structure to the data appears more appropriate than the spatial errors structure. The causes and implications of these spatial effects deserve further investigation.

4.3 Calibration of allocation and compliance results

Outside data can help calibrate the highly divergent performance of the auction and posted offer markets, both in terms of the willingness to accept values revealed at the allocation stage and the relative compliance outcomes. The household model presented in Section 2 assumes that both market prices and shadow prices affect the opportunity cost of the tree planting contract at the household level. Consequently, market prices for inputs and outputs do not accurately reflect the true costs associated with implementation of the contract. However, market prices do offer an estimate of magnitudes and the relative importance of different constraints. To calibrate the willingness to accept values revealed by the auction, market prices are used as a rough proxy for labor, land and other input costs, shown in Panel A of Table 8. The contract requires that landholders set aside half an acre of land for tree planting, so I base all cost estimates on inputs and outputs for half an acre for the two leading alternative land uses, local maize and soya bean, and for tree planting. Interviews with District Agriculture and Development officers provided information on labor requirements for land clearing, field preparation and planting, and weeding and harvest. All figures in this section can be compared with 2008 per capita gross national income of MWK 40,600 for Malawi (Bank 2008).

As shown in Panel A of Table 8, land clearing is a one-time cost associated with conversion of idle land to production, while field preparation and planting is an annual fixed labor cost that is slightly higher for the tree planting contract than for maize or soya bean production. The contract demands slightly less labor for weeding and harvest than do alternative land uses, though these costs are variable. Total labor input costs stay approximately the same if land is converted from maize or soya (MWK 8,600 to 10,400) to tree production (MWK 8,800). Annual input costs of MKW 800 for local maize or MWK 100 for soya production fall to zero under conversion to the tree planting contract, which requires no input expenditure on seeds or fertilizer. Foregone income from crop production represents the largest cost associated with reallocating half an acre of land to the tree planing contract, which pays up to MWK 6,000 in the first year and half that amount in subsequent years.

A household that trades at market prices for labor, land and other inputs loses between MWK 37,670 and 143,650 under the contract. However, poorly functioning input markets makes the contract profitable for some households that operate on one or more of these margins. For example, a cash constrained household that faces missing labor and land markets cannot exchange labor and land for other inputs such as fertilizer. In the notation of the household model, $\frac{\partial \Pi^*}{\partial f} = p_j \frac{\partial q_j^*}{\partial f} - p_{jf} \frac{\partial q_{jf}^*}{\partial f} > 0$ and $\frac{\partial \Pi^*}{\partial l}, \frac{\partial \Pi^*}{\partial k} = 0$ for households with non-binding labor and land constraints. Since the tree planting contract requires reallocation of land and labor but no other inputs (Table 8), $\Pi' - \Pi^*$ costs only the value of foregone leisure time if other inputs are the only limiting factor of production in the absence of the contract. The same comparative static expression for $\frac{\partial \Pi^*}{\partial s}$ holds for all inputs $s \in \{l, k, f\}$.

Taken one at a time, these constraints offer a calibration of the willingness to accept values revealed in the contract. As described, a household constrained only by inputs should have submitted a very low bid, equal to the value of foregone leisure. A household constrained only by labor must bear the cost of the additional labor under the contract, but need not reduce crop production if labor markets are active. However, because of missing labor markets, the contract also requires reallocation of labor from crop production. Missing markets raise the opportunity cost of labor above the pure labor cost of MWK 10,000, which is just below the auction clearing price and around the 35th percentile of bids. A land constrained household forfeits income from crop production but bears no increase in labor or other input cost. For these households, the estimates in Panel A of Table 8 predict bids between MWK 41,070 and 142,350, which fall between the 63rd and 92nd percentile of observed bids. These values cover much of the relevant range of the observed bid curve, and indicate that bids were not out of line with estimates based on market prices and the predictions of the household model. Of course, these estimates use market prices while simultaneously assuming missing markets, so the values are, at best, an approximation of true opportunity cost.[14]

Observed patterns of production adjustments among households that receive a contract offer suggestive evidence on the actual constraints facing households that received a tree planting contract under either the auction or the posted offer market. Of the 171 households that implemented a contract, 47 demonstrated a land constraint by reallocating productive land to the contract without bringing other new land into production or holding other idle land. Another 38 have other idle land but still reallocated productive land and did not bring other land into production, which suggests a labor constraint. The remaining 89 households are assumed to face an income constraint to production. Panel B of Table 8 summarizes these speculative reallocation outcomes.

[Table 8]

I construct a categorical variable to capture relative constraint costs using the information in Table 8. Among the contract holders, a categorical variable reflecting the production constraint faced by each household ranks the cost of constraints in ascending order from cash inputs (=1) to land (=3). For the 82 contract holders who received a contract through

[14] Market prices may provide a lower bound on opportunity cost, if the household chooses not to participate in the market because shadow prices are higher than market prices. Missing markets may also be due to high transaction costs (Key et al. 2000), or to a simple lack of input accessibility in some places, which would not necessarily imply that market prices are a lower bound.

the auction, the constraint variable is weakly positively correlated with bid ($\rho = 0.06$), consistent with a higher opportunity cost for those facing more costly constraints. For all contract holders, the constraint variable is weakly negatively correlated with the number of surviving trees ($\rho = -0.10$), consistent with better implementation from those receiving a higher surplus under the contract payment. On the whole, the calibration of bid prices and household constraints offers some support for the household model's description of landholder decision making and confirms the range of auction bids with market prices for inputs and outputs. Approximately 85 contract holders, or half of the total, appear to have decreased crop production as a result of the contract and may therefore be undercompensated for their opportunity cost.

5 Discussion of mechanism performance

The allocation and compliance results clearly demonstrate that the proposition of equal performance does not hold in this context. Taken alone, the allocation results indicate that a posted offer mechanism generates a higher quantity of supply than does a uniform price auction. However, the compliance outcomes complicate the story and show that the auction successfully selects for high compliance individuals. At the clearing price, the auction is the more efficient mechanism per contract.

These results are by no means the first to show differences in performance across strategically equivalent mechanisms, though the precise institutions studied here have not been subject to previous experimental comparison.[15] In a laboratory setting, Lusk, Fedlkamp and Schroeder (2004) compare four incentive compatible mechanisms and find significant differences in both first-round and later market outcomes. They also review other experimental studies that find divergence among theoretically equivalent institutions. Examples from outside of the laboratory are less common, though the internet has become a popular setting for empirical comparisons of market institutions (e.g., Lucking-Reiley 1999). Lee and Malmendier (2007) use buying behavior on the internet site eBay to provide a comparison of mechanisms similar to those studied here. They find that bidders in second-price auctions pay considerably above posted-offer prices and attribute the results to irrational behavior, including competitive arousal. The subject pools, stakes and the context for these studies

[15] Several studies have compared variations on posted offers and auctions, including the early series of experiments to compare repeated posted offer and double auction institutions by Ketcham, Smith and Williams (1984).

differ considerably from the setting I study and no experimental evidence exists to date on the performance of theoretically equivalent mechanisms in developing countries.

In the experimental results, the failure of the proposition of equal performance implies a violation of one of the necessary assumptions. Each of the three assumptions is examined below: 1) independent private value, 2) no collusion, and 3) individual rationality.

5.1 Assumption 1: Independent private value

The proposition of equal performance assumes that valuations are private and independent, which implies that the value of the contract is known to the to the individual and that the values or information held by others does not affect how much her own valuation.[16] Individual valuations do not fit this assumption if contracts have some common value element or other source of interdependence. If, for example, the labor inputs for the contract are the same for all households but bidders do not know the labor requirements with certainty, then heterogeneous shadow prices for inputs will lead to a mixed common and private value for the contract.[17] A pure common value is the extreme version of this, in which true willingness to accept, α, is the same for all households. In a mixed private and common value auction, more information reduces the weight that individuals place on the common value component of the valuation, and improves the efficiency of the outcomes (Milgrom & Weber 1982, Krishna 2002). Winning in these circumstances is usually taken as bad news about the true value of the object relative to the winning bid. In a single unit procurement setting, this leads to upward bid shading, though Hernando-Veciana (2004) shows that in multi-unit auctions with single unit demand, a risk of losing a contract at a favorable price can also lead to downward bid shading. Thus, the direction of bid shading from a failure of private independent values is potentially ambiguous.[18]

By announcing a price for the contract, the posted offer market provides more information than the sealed bid auction. However, this only improves allocation if the price conveys

[16] Note that private values can be uncertain and still meet the assumption. For example, in an art auction with no resale market, a bidder may be uncertain as to how much she will enjoy having the sculpture in her house, but the values and information held by others do not affect her own uncertain private value.

[17] Survey questions were administered in the baseline survey to evaluate individual perceptions of the value structure of a tree planting contract. Fewer than one-quarter of participants agreed that the effort to plant trees is the same for all families.

[18] For winner's curse to generate substantial upward bid shading, it has to dominate the loser's curse effect, which is increasing in the number of units being purchased. After the auction, a subsample of respondents indicated that, on average, they expected around 50 percent of participants to receive contracts. These expectations balance winner's curse and loser's curse bid shading.

relevant information about the contract value. Empirical evidence on the benefits of price information in posted offer markets is relatively scarce. Holt and Sherman (1990) examine a market with both product price and quality chosen by the seller. They find that information asymmetries reduce efficiency and that information about the price but not the quality does not improve outcomes.[19] In the posted offer market, a simple Bayesian updating model predicts updating from α to $\alpha' = \rho P + (1-\rho)\alpha$, with $\rho = \frac{\sigma^2}{1+\sigma^2}$ so that the posterior valuation (α') converges to the price P as uncertainty increases.

My experimental results reveal nearly full acceptance of the price in the posted offer market, which implies that posted offer participants place nearly full weight on the price offer when updating prior value signals as revealed by the bids.[20] Uninformative prior value signals indicate a pure common value to the contract. However, the auction would not be expected to sort individuals according to their willingness to accept in a pure common value setting, since the true α would be the same for all bidders. The significantly higher compliance outcomes in the auction treatment group are inconsistent with a pure common value to the contract, which is required for the allocation outcome to result from a failure of Assumption 1. An assumption of independent private values appears more consistent with the data than other standard assumptions about value structures.

5.2 Assumption 2: No collusion

The proposition of equal performance assumes that bidders do not collude to affect prices in the auction. In the face of collusive behavior, an auction no longer offers the benefit of correctly determining a clearing price for the population. Second price auctions are susceptible to collusion, though sealed-bids and one-shot bidding reduce collusion (Robinson 1985). Real world examples of collusion range from the internet (Ariely et al. 2005) to large public tenders (Baldwin et al. 1997, Cramton & Schwartz 2000). In the literature on conservation auctions, efforts at collusion are observed in experiments that allow for communication among participants (Cummings et al. 2004) though greater numbers of bidders and less feedback between rounds successfully limits collusion (Cason, Gangadharan & Duke 2003).[21]

[19] Uncertain private value will not generate the observed updating results in the posted offer mechanism. Unless values are common or otherwise affiliated, the price offer does not contain information about the contract's value to any particular individual without imposing additional assumptions about individual beliefs.

[20] Here, I assume that all other assumptions, including individual rationality, hold so that participants are assumed to bid according to their dominant strategy.

[21] Another form of strategic behavior in the form of bid-shading for rent extraction has been observed in conservation auctions in the United States and Australia (Stoneham et al. 2003, Kirwan et al. 2005, Claassen

Collusive behavior in a posted offer market is only likely in the face of repetition, where demand withholding in early periods can affect later prices, which is not a concern in the one-shot setting studied here.

Collusive behavior in the auction will increase bids above the dominant strategy, leading to a higher quantity of supply in the posted offer market ($y_{PO}(P)$) than in the procurement auction ($y_A(P)$). Though all allocated contracts are accompanied by rents in both the auction and the posted offer market, the hypothesis of collusive behavior leads to greater rents to those who receive the contract through the auction. If the ex post value of the contract is stochastic, these greater rents may allow for higher compliance under the auction, consistent with the prediction that compliance is increasing in the difference between the price and minimum willingness to accept ($\partial c/\partial(P-\alpha)$).

Allocation and compliance results are not in conflict with the predictions from a hypothesis that bidders in the auction colluded to drive up prices. However, a great deal of care was taken during the invitation process to minimize the risk of collusion and to ensure that it could be detected if it did occur. No information about the specific nature of the contracts or bidding was revealed in advance. The government staff who delivered the invitations to the villages did not have enough detail about the contract requirements to provide an anchor from which collusion could take place. Though village is an important determinant of bid, results on the relationship between village and contract compliance indicate that this selection can be attributed to unobservable characteristics that vary at the village level. Random spot checks during the registration process also suggested that people were not aware of the specifics of the activities.

Concern about collusion is not limited to communication in advance of the auction. Though individuals were encouraged not to talk during the auction process, it would have been possible for them to see each others' bids. Transmitting this information beyond the individuals seated immediately next to the bidder would have been difficult. Bids were collected in order of seating to detect this type of collusion. Analyzing the correlation of bids of those seated near each other during the auction further rules out collusion that took place at the time of allocation. The bids of nearest neighbors are marginally correlated, which is reasonable if similar individuals chose to sit near each other, though this correlation dissipates quickly with distance. Figure 3 shows autocorrelations between bids for each

et al. 2008). Generally, the shading observed in these settings can be attributed to the presence of a scoring rule that considers the ecological benefits of a land parcel, though it may also be due to efforts at rent extraction due to a misinterpretation of bidding incentives (e.g., Laury 2002). Shading under the described auction rules should be limited to bid shading due to common value.

pairwise comparison of seated neighbors (leftmost bar is nearest neighbor).

[Figure 3]

Supplementary data upholds Assumption 2 and confirms that collusion did not cause the divergent mechanism performance.

5.3 Assumption 3: Individual rationality

The proposition of equal performance assumes fully rational behavior by market participants. Bounded rationality may affect behavior under the auction, the posted offer market, or both. The imprecision of the predictions associated with a failure of Assumption 3 make it difficult to reject as a description of the empirical results. However, support for the first two assumptions points to failure of rational behavior.

The greater complexity of the auction compared to the posted offer market raises the possibility that irrational or naïve bidding drove the divergence in the mechanisms. Substantial experimental evidence on behavior in second-price auctions indicates that naïve bidders frequently deviate from dominant bidding strategies (Kagel & Levin 1993, Kagel 1995). Bidding errors should, however, lead to inferior sorting in the auction and lower compliance among auction participants, which contradicts my empirical evidence, but does not necessarily imply that bids revealed true willingness to accept. Cognitive measures collected during the follow up survey also do not suggest any relationship between cognitive ability and bidding behavior.[22]

Bounded rationality may also lead to mechanism outcomes that are sensitive to how values are elicited, a phenomenon documented in the literature on preference reversals and contingent valuation studies. Procedure invariance requires that strategically equivalent elicitation methods reveal the same preferences, and is a standard assumption in economics. However, Tversky et al. (1990) provide evidence that violations of procedure invariance explain many instances of preference reversals. Their experiment compares pricing and choice decisions for lotteries and shows greater attention to payoffs in pricing the lotteries and a greater attention to probabilities in the choice decision, a result that has been replicated by many others (Seidl 2002). The literature on non-market valuation also cites numerous violations of procedure invariance including differences between open-ended and dichotomous

[22] A series of reverse digit spans tests, in which the respondent was read a series of numbers and asked to recite them back in reverse order after a brief pause. The test measures both active memory and ability to abstractly manipulate numbers.

choice elicitation procedures (Harrison 2006). The comparison between an auction and a posted offer market has parallels with the elicitation mechanisms studied in these two literatures: the auction resembles an open ended pricing decision while the posted offer market resembles a dichotomous choice decision. The direction of valuation divergence described in both preference reversal studies and the valuation literature is consistent with higher willingness to accept under the auction than under the posted offer market. These literatures also offer numerous explanations of why these differences are observed, without clear consensus on which provides the "true" valuation.

A survey of hypothetical contract valuations replicates the divergent valuation patterns observed at the allocation stage of the study. Collected nine months after the original experiment, the hypothetical valuation responses summarized in Table 9 reveal within-subject acceptance of posted offer prices dramatically below hypothetical bids for the contract.[23]

[Table 9]

The average hypothetical bid was close to 11,000 MWK, which is just below the actual contract price of MWK 12,000, and may suggest some strategic behavior in the survey responses. The hypothetical amount is significantly lower than the mean bid of MWK 58,128 from the auction. Later in the survey, individuals were asked to name the lowest price that would still make them feel that the contract was good for them. Both the mean and the variance of responses to this phrasing of the open ended valuation question decrease relative to the hypothetical bidding format, with the mean falling to around MWK 8,800. These open ended bidding style questions are complemented by two dichotomous choice, posted offer style valuation questions, which offered very low contract values. Over three-quarters (77.9 percent) of respondents said they would accept the contract were it offered to them for 1,500 MWK, though only 15 percent named a price of 1,500 or lower in the hypothetical bid question and only 8 percent bid below that price in the original auction. These values also allow a comparison of prices, holding quantities fixed across the mechanisms. The 78th centile of hypothetical bids is MWK 12,000, showing nearly an order of magnitude difference due to elicitation method. Even more surprisingly, 76.6 percent of participants said that they would accept the contract for no pay.

[23] Note that the hypothetical valuation survey also provides an additional check on the independent private value assuption. If common values contributed to the divergence in allocation outcomes across the mechanisms, then the divergence should be reduced with experience with the contract. At the time of the valuation survey, respondents had learned the true cost of contract implementation.

Though a direct comparison of hypothetical bids to bids in the original experiment would suggest inflated bids in the latter, such a comparison is not valid. First, the survey valuations are hypothetical and second, participants have significantly more information about the true costs of contract implementation at the time of the survey. In spite of the caveats about hypothetical valuations, the within-subject replication of the divergence observed in the allocation experiment is striking and may suggest a persistent behavioral phenomenon in this context.

5.4 Alternative explanations

In addition to the hypotheses explored, the proposition of equal performance also requires that individuals do not accept the contract purely for its option value, which is borne out in the result that instances of pure attrition or defection are extremely low.[24] A positive option value does not predict divergent mechanism outcomes since bids should unravel and all positive posted offer prices should be accepted. However, it is worth noting that the structure of many allocation or selection problems relies on the performance of selected agents, making ex post incentives and uncertainty important at the selection stage. Laffont and Tirole (1987) and McAfee and McMillan (1986) examine this problem, and both suggest a tradeoff between ex ante selection of the lowest cost offer and ex post performance. Like most environmental payment program, the contracts studied here do not stipulate penalties or fines for non-compliance (Choe & Fraser 1999, Chomitz et al. 1999). Had the compliance results shown substantial default at the start of the implementation period, the contract incentives would be a greater concern as a source of high acceptance rates under the posted offer market. Other potential alternative explanations that would affect willingness to accept, such as a temporary income effect due to the show up fee, do not explain divergence between the mechanisms.

6 Implications of the findings

Results from the field experiment can be used to investigate the implications of different allocation mechanisms for environmental markets. First, outcomes are simulated for allocation of contracts by a social planner, based on criteria identified by relevant stakeholders in

[24]This assumption is analogous to the assumption of no budget constraint, which is needed for the bidding strategies in both markets to hold (Krishna 2002).

Malawi. Second, cost-benefit implications are reviewed, including estimates of the carbon sequestration provided by the program. Finally, the findings are discussed in light of climate policy.

6.1 Social planner

Allocation of contracts based on observable characteristics of eligible landholders offers an alternative approach to contract allocation, and is often used to identify recipients for publicly funded programs. Consultation with relevant stakeholders in Malawi suggested criteria that a social planner would use to select households into a tree planting program. Informal interviews with the implementing organization and with government officials generated consensus that priority characteristics include: land size, available labor and information about trees. It was also suggested that some individuals have a greater preference for trees than others. I simulated a social planner's selection based on five variables selected from the baseline survey to proxy for these characteristics: total acres owned, household size, participation in the labor market, past experience with the implementing organization and past tree planting.[25]

The random assignment resulting from the lottery conducted to allocate contracts in the posted offer market provided a representative sample of contract holders. These observations allowed me to use the baseline survey data and tree survival outcomes to simulate how contract implementation varies with selection on different household characteristics. The 89 contract holders from the posted offer treatment were used to generate random samples of 205 individuals (the number of participants in the treatment). The relevant selection criteria led to a ranking of households based on their desirability and the contract implemention outcomes for the top 89 provided a measure of mean performance for the selection. Figure 4 shows the results of 10,000 repetitions of the simulated selection outcomes for each of 7 selection rules.

In the first 5 rules, the social planner selects for the households with the largest landholdings, the greatest number of family members, participation in the labor market, past interaction with the NGO and past tree planting. In the sixth rule, each of these variables is normalized to one and summed into a linear index of the characteristics. In the seventh selection rule, a linear regression of the number of surving trees on the five household variables for the 89 randomly selected posted offer contract recipients provides weighting coefficients,

[25] The approach is similar to what is used by Jack et al. (2008) for data from Indonesia.

which are then used to create a weighted index for selection.

[Figure 4]

As shown in Figure 4, the distribution of the simulated mean tree survival outcomes falls well below the auction selection mean for all selection rules. The three top performing selection rules are past tree planting, the linear index of household characteristics and the regression weighted index. All perform better than the random posted offer selection but worse than the auction selection. The mean of the distribution of simulated means is 40.52 for past tree planting selection, 40.58 for the linear selection index, and 40.26 for the regression weighted selection index. Auction selected individuals keep, on average, 42.51 trees alive. Though the sample sizes used to conduct the simulation are on the small side, they do constitute a random sample so the outcomes suggest that the auction selected on unobservable or difficult to measure characteristics that would not be available to a social planner.

Even if a social planner could use observable characteristics of the landholder to determine an allocation that led to high rates of compliance, the planner would still face the challenge of setting a price for the contract. As suggested in Sections 2 and 4.3, opportunity cost may be difficult to approximate from market prices if key input and output markets are missing. One approach to setting prices where opportunity cost is difficult to predict is to conduct an auction with a representative sub-sample of the population to set market prices (Athey et al. 2002). However, the posted offer responses in both the incentive compatible allocation experiment and to the hypothetical survey questions suggest that such an approach may not work in this context. If the mechanism used to elicit values has a large impact on willingness to accept, then the same price will not clear different markets. The observed excessively high acceptance rates that resulted in lower performance means that posting a price may not lead to separation of households by opportunity cost around that price.

6.2 Program evaluation

The benefits side of a cost-benefit analysis of the project is complicated by fact that biological carbon sequestration is not permanent. For example, sequestration benefits are reversed if the tree is used for firewood, so the species of tree planted under the project was selected for its use as timber, which improves its lifetime carbon profile but does not resolve concerns about long-term sequestration. Leaving aside important questions of how to value biological carbon sinks and the extent of their substitutability with emissions reductions (van Kooten 2008), a rough program evaluation considers a simplified approximation of benefits. Whether the prices elicited by the auction justify payments for tree planting, from an efficiency standpoint, can be evaluated by comparing projections of carbon sequestered under the program with the total payments to contract holders.[26] Table 10 shows the projected carbon sequestration value per tree, based on estimates of above- and below-ground biomass as the trees grow. The payments for the contract are around USD 1.70 per tree and provide an estimate of the cost per ton of carbon, if the tree survives up to the age specified in the year column. Carbon prices associated with reforestation and afforestation projects under the Clean Development mechanism were around USD 16 in 2008 and around USD 6.4 on the voluntary markets (Capoor & Ambrosi 2009), so the cost per ton of carbon sequestered enters the range of current carbon prices between the 15th and 25th year of tree growth.

[Table 10]

Cost effectiveness comparisons based on compliance outcomes between the two allocation mechanisms are irrelevant since the payments are delivered only for surviving trees. However, the payments under the contract are only a component of the overall program costs, which contain a fixed component for each allocated contract. The transaction costs associated with the delivery of each contract make the auction the more cost effective of the mechanisms, holding implementation costs and contract prices constant, though it may also be the more expensive of the two mechanisms to implement and almost certainly results in higher prices for a given level of supply.

Evaluation of the relative performance of the auction and posted offer mechanisms is limited by the lack of experimental results on the price elasticity of compliance. Results from the valuation survey allow me to bound the change in compliance that would compensate for the cost savings from offering a lower price in a posted offer market. At an offer price of MWK 1,500, which was accepted by over two-thirds of respondents in the hypothetical valuation, the budget used in the auction could purchase 656 contracts or eight times as many as were purchased through the auction. To result in the same total number of surviving trees as observed under the auction, the average contract recipient would only need to keep 5.3 trees alive, which is 12 percent of the number kept alive by the average auction participant. Since the contracts pay on a per surviving tree basis, total payments under an MWK 1,500

[26]This estimate of benefits includes neither benefits nor costs accrued by the individual landholder, nor does it include project implementation costs in the price of the contract payments.

posted offer approach would be USD 741.28 versus USD 5925.90 under the auction, with the same number of surviving trees. These relative cost effectiveness numbers also imply that the carbon prices shown in Table 10 are eight times as high as the carbon prices associated with a MWK 1,500 contract. All of these estimates are very simplistic and ignore the effect of increasing the number of contracts on transaction costs, which would lower the relative advantage of a low posted price offer with high acceptance rates.

6.3 Environmental policy

Concerns about verifiability and the environmental integrity of biological carbon offsets, such as those obtained through afforestation and reforestation projects, have arisen in reviews of the Clean Development Mechanism of the Kyoto Protocol (Wara & Victor 2008) and discussions of pending United States climate legislation (e.g., Olander 2008). The difficulty in monitoring and enforcing projects that fail to deliver promised carbon reductions has led to discounting of biological carbon credits and limits on the total quantity of offsets in recent climate legislation. While auctions and other approaches to allocation have received a fair amount of attention in discussions of the initial distribution of emissions allowances, the role of allocation in offset projects has received little attention. The divergent performance of the auction and the posted offer mechanisms in terms of the contract compliance demonstrates the role of selection in addressing these concerns.

To scale the magnitude of compliance differences to a policy relevant scale, consider pending domestic legislation (House bill HR2454), which allows up to one billion tons of carbon dioxide-equivalent from international offsets. If five percent of that total amount (50 million tons) were purchased through afforestation projects that used individual level contracts allocated under a posted offer market, then, extrapolating from my results on compliance after six months, total tons of carbon dioxide sequestered after 25 years would be 3.75 million tons less than if auctions were used to select landholders into the offset projects. Though this rough estimate relies on extreme assumptions of equal prices under each of the allocation mechanisms and equal long and short run compliance rates, it does suggest that the selection effects are non-negligible. On the other hand, a posted offer approach will clearly generate higher supplies at lower prices, potentially making it more attractive as an approach for maximizing offset supplies, particularly if concerns about long run compliance can be overcome through innovative contract design and lowered monitoring costs.

Overall, the importance of an allocation mechanism that selects for high-compliance individuals is sensitive to the intersection of supply and demand. As demand grows relative to supply, more of the market receives a contract, selection becomes less important and the relatively higher supply offered by a posted offer market becomes more desirable. Levels of offset supplies are, of course, sensitive to price and projections of international supply suggest that supply will exceed demand at domestic trading prices, making selection at both the project and sub-project level potentially important.

7 Conclusion

The design of allocation mechanisms for environmental policy has received a fair amount of academic attention (e.g., Muller & Mestelman 1998, Goeree et al. 2009), however, this is the first field experiment to examine the one-shot allocation problem in environmental markets. Results show that both initial contract allocation and subsequent compliance outcomes are sensitive to the choice of mechanism. Explanations for the observed higher supply quantity under the relatively simple posted offer mechanism and the better quality selection under the auction are not consistent with the theoretical prediction of equal performance under an independent private values framework. The results point to a violation of procedure invariance, similar to the observed differences in preferences over risky choices elicited in the studies on preference reversals (o Tversky et al. 1990, Seidl 2002)r the divergence in valuations elicited by dichotomous choice and open ended questions in contingent valuation studies (. Balistreri et al. 2001)

The auction and the posted offer market trade off quantity and quality, which is a somewhat different tradeoff than the more frequently cited choice between efficiency and simplicity in market institutions. Policy implications from my findings depend to some extent on the policy objective, though the overarching message is that instrument choice matters. If the goal is to maximize the number of trees grown for the least amount of money, then naming a very low price in a posted offer market is likely to out-perform an auction. What is gained in cost effectiveness may be, counterintuitively, lost in efficiency. The posted offer market has little selection effect and so such an approach will not necessarily identify high compliance landholders, defined as those with the lowest opportunity cost or willingness to accept. Thus, if allocational efficiency is important, as may be the case in carbon offset markets, then an auction is preferred.

The results are inconclusive from a welfare standpoint, which depends to some extent on whether the auction clearing price actually equalized willingness to accept and contract price on the margin. The posted offer supply response suggests that the clearing price from the auction was high, while estimates of the cost of reallocating land and labor for the contract suggest that the price was low. In combination with the hypothetical valuation responses, evidence from the posted offer market may indicate that individuals in the study context will accept prices that do not compensate for opportunity cost, potentially due to uncertainty or a scarcity of income generating activities.

Future research can test the robustness of the experimental results and help to pinpoint the cause of the violation of procedure invariance. For example, adding a third treatment that requires posted offer participants to assess and write down their willingness to accept before observing the posted price offer would test whether the divergence is due to the greater cognitive effort exerted during the auction that allows for better selection. Varying the price in the posted offer market would offer evidence beyond the hypothetical responses to the effect of price on acceptance and compliance. Testing the generalizability of these findings beyond environmental policy settings may also lend insights into the behavioral patterns that characterize the results.

With the expansion of market mechanisms as a tool for environmental policy in developing countries, the performance of standard market formats in new settings and with new goods and services deserves further research. Better understanding of the tradeoffs faced by alternative allocation and pricing mechanisms is particularly important in contexts where imperfect input and output markets make setting prices difficult because of the prevalence of household-specific shadow prices, and also where one-shot mechanisms do not allow for learning and price adjustment. Carbon offset projects in developing countries are an important example of new market settings that are likely to be highly sensitive to the allocation mechanism used to select landholders into a project. The financial viability and the environmental integrity of international offset projects will be affected by the mechanisms used to determine the initial allocation of contracts for sequestration activities.

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Table 1: Household characteristics by treatment group

	Treatment 1 Auction			Treatment 2 Posted offer	
Labor (l)					
HH size	4.607	(2.100)		4.421	(2.169)
Education (1-5)[a]	2.161	(0.840)		2.265	(0.834)
Laborsharing group (0/1)	0.452	(0.498)		0.391	(0.485)
Casual labor income (0/1)	0.668	(10.392)		0.678	(10.282)
Use family labor only (0/1)	0.752	(0.436)		0.765	(0.425)
Stated labor constraint (0/1)	0.045	(0.218)		0.048	(0.212)
Minutes gathering firewood	110.938	(95.967)		123.044	(87.218)
Land (k)					
Total landholding (acres)	4.998	(2.987)		4.984	(4.034)
Number of fields	1.361	(0.591)		1.396	(0.698)
Total number of crops	3.045	(1.105)		3.117	(1.198)
Minutes from home to field	23.838	(24.532)		22.018	(22.036)
Cash crops (0/1)	0.830	(0.373)		0.817	(0.379)
Stated land constraint (0/1)	0.045	(0.202)		0.030	(0.167)
Household characteristics (z)					
Past borrowing (0/1)	0.310	(0.467)		0.304	(0.455)
Age of the participant	38.770	(16.381)		37.772	(16.773)
Female participant (0/1)	0.533	(0.498)		0.478	(0.500)
Months of food shortage	4.252	(2.271)		4.252	(2.184)
Prior tree planting (0/1)	0.502	(0.498)		0.496	(0.500)
Prior contact with NGO (0/1)	0.320	(0.467)		0.252	(0.440)
Risk preferences (1-3)[b]	2.108	(0.933)		2.168	(0.940)
Time preferences (1-6)[b]	3.443	(2.209)		3.297	(2.154)
Mistrusts outsiders (1-3)	1.817	(0.747)	*	1.943	(0.789)
Willingness to try tech (1-3)	1.234	(1.322)		1.320	(1.335)
Asset index	11.289	(3.173)		11.074	(3.473)
N	242			230	
Joint F-statistic for village indicators			0.02		

Notes: Means are reported for each of the treatment groups with standard deviations in parentheses. Difference in means: * p<0.10, ** p<0.05, *** p<0.001. [a]The range for categorical variables is provided in parentheses. (0/1) indicates a binary dummy variable, coded 1 if the response is yes. [b]Time preferences and risk preferences are elicited using survey questions. Higher risk preference corresponds to lower risk aversion. Higher time preference corresponds to a lower discount rate.

Table 2: Summary allocation and compliance outcomes for each treatment group

	Treatment 1 Auction	Treatment 2 Posted offer
Number of participants	227	203
Willingness to accept < clearing price (% of treatment)	37.4	99.5
Received contract (% of treatment)[a]	37.4	43.4
Mean contract outcome (% compliance)[b]	85.02	77.66
Probit regression of 0/1 contracting outcome on baseline variables[c]		
Likelihood ratio chi-square	38.76**	22.96

Notes: [a]A lottery was used to resolve the oversubscription in the posted offer market. [b]Percent compliance is measured as the percent of the maximum of 50 trees surviving after six months. [c]Probit regression of binary contract outcome variable on survey measures from Table 1. P-value for the likelihood ration chi-quare statistic is indicated * $p<0.10$, ** $p<0.05$, *** $p<0.001$.

Table 3: Linear regression of logarithm bids on household characteristics

	Labor (l)		Land (k)		Other (z)		All		All		
	(1)		(2)		(3)		(4)		(5)		$d\alpha / dx$
HH size	-0.034	(0.053)					-0.039	(0.064)	-0.089	(0.055)	-
Education (1-5)[a]	0.240*	(0.142)					0.240	(0.151)	0.247**	(0.123)	+
Laborsharing group (0/1)	-0.185	(0.244)					-0.253	(0.255)	0.090	(0.255)	-
Casual labor income (0/1)	-0.541*	(0.275)					-0.498*	(0.285)	-0.467	(0.291)	-
Use family labor only (0/1)	0.480	(0.329)					0.274	(0.336)	0.202	(0.330)	+
Stated labor constraint (0/1)	0.981**	(0.383)					1.141**	(0.493)	0.588	(0.401)	+
Minutes gathering firewood	-0.003**	(0.001)					-0.004***	(0.001)	-0.002	(0.001)	-
Total landholding (acres)			0.026	(0.048)			0.038	(0.048)	0.020	(0.043)	-
Number of fields			-0.390*	(0.204)			-0.334	(0.218)	0.039	(0.211)	-
Total number of crops			-0.043	(0.159)			-0.073	(0.150)	-0.019	(0.132)	-
Minutes from home to field			-0.006	(0.004)			-0.004	(0.004)	-0.001	(0.004)	+
Cash crops (0/1)			0.073	(0.350)			0.177	(0.348)	0.081	(0.318)	+
Stated land constraint (0/1)			-1.188*	(0.702)			-0.988	(0.637)	-0.518	(0.713)	+
Past borrowing (0/1)					0.344	(0.259)	0.420	(0.269)	0.327	(0.264)	-
Age of the participant					0.011	(0.008)	0.000	(0.008)	0.001	(0.009)	+
Female participant (0/1)					-0.444*	(0.252)	-0.491*	(0.252)	-0.295	(0.241)	-
Months of food shortage					-0.126**	(0.055)	-0.019	(0.071)	0.014	(0.059)	+
Prior tree planting (0/1)					-0.121	(0.278)	0.060	(0.268)	0.208	(0.242)	-
Prior contact with NGO (0/1)					-0.504*	(0.277)	-0.505*	(0.275)	-0.611**	(0.268)	-
Risk preferences (1-3)[b]					0.010	(0.133)	0.059	(0.129)	-0.041	(0.129)	+
Time preferences (1-6)[b]					-0.171***	(0.058)	-0.174***	(0.057)	-0.089	(0.055)	-
Mistrusts outsiders (1-3)					0.078	(0.151)	0.176	(0.157)	0.189	(0.155)	+
Willingness to try tech (1-3)					-0.069	(0.095)	-0.104	(0.094)	-0.099	(0.084)	-
Asset index					-0.025	(0.048)	-0.040	(0.055)	-0.020	(0.052)	-
Constant	9.823***	(0.514)	10.445***	(0.475)	10.993***	(0.794)	11.541***	(0.939)	9.004***	(1.173)	
Joint F-statistic for village indicators									4.29***		
Number of observations	228		228		228		228		228		
Adjusted R-squared value	0.055		0.008		0.070		0.118		0.324		

Notes: OLS regressions of logarithm bid on survey regressors. Robust standard errors are in parentheses. * p<0.10, ** p<0.05, *** p<0.001. $d\alpha / dx$ describes the expected comparative static effect of each regressor on willingness to accept. [a] See Table 1 for description of regressors.

Table 4: Regressions of contracting outcomes on treatment and village fixed effects

A. Survival (OLS)	(1)	(2)	(3)
Auction	3.681**		2.646*
	(1.424)		(1.370)
Village F-stat[a]		4.13***	4.45***
Constant	38.831***	46.125***	44.141***
	(1.105)	(1.127)	(1.789)
Adjusted R-sq	0.037	0.254	0.255
B. Survival (NB)	**(1)**	**(2)**	**(3)**
Auction	0.085**		0.061*
	(0.038)		(0.036)
Marginal effect	3.68		3.67
Village chi-square		97.66***	98.68***
Constant	3.634***	3.819***	3.767***
	(0.030)	(0.023)	(0.041)
C. Score (ordered logit)[b]	**(1)**	**(2)**	**(3)**
Auction	0.829***		0.878***
	(0.294)		(0.328)
Village chi2		39.37***	38.78**
Adjusted R-sq	0.047	0.096	0.129

Notes: N=171. Panel A reports an ordinary least squares (OLS) regressions of survival (number of trees) and monitoring score (out of 5) on treatment. Panel B reports a censored negative binomial regression of survival on treatment with an upper limit at 50. Panel C reports an ordered logit model with monitoring scores ranging from 1 to 5. Robust standard errors are in parentheses (Panels A and B), bootstrapped VCE in Panel C; * p<0.10, ** p<0.05, *** p<0.001. [a]Village F-stat reports the joint F-statistic and significance of village indicator variables. [b]The ordered logit estimates the constant at each cutoff in the outcome variable, not shown.

Table 5: Linear and censored regressions of tree survival on baseline survey measures for posted offer treatment

	Tree survival				
	OLS		Negative binomial		*dc / dx*
Labor (l)					
HH size	0.002	(0.899)	0.001	(0.022)	+
Education (1-5)[a]	-2.149	(1.674)	-0.065*	(0.036)	-
Laborsharing group (D)	-0.937	(3.714)	-0.044	(0.080)	+
Casual labor income (D)	-0.040	(3.373)	-0.024	(0.077)	+
Use family labor only (D)	2.441	(4.295)	0.046	(0.101)	-
Stated labor constraint (D)	1.163	(5.513)	-0.047	(0.127)	-
Minutes gathering firewood	0.005	(0.015)	0.000	(0.000)	+
Land (k)					
Total landholding (acres)	-0.354	(0.670)	-0.012	(0.016)	+
Number of fields	2.256	(2.571)	0.062	(0.056)	+
Total number of crops	-0.142	(1.246)	-0.005	(0.026)	+
Minutes from home to field	-0.038	(0.075)	-0.001	(0.002)	-
Cash crops (D)	-8.322	(5.936)	-0.218	(0.133)	-
Stated land constraint (D)	-0.109	(8.074)	0.051	(0.168)	-
Household characteristics (z)					
Past borrowing (D)	5.992*	(3.250)	0.172**	(0.077)	+
Age of the participant	0.136	(0.098)	0.004*	(0.002)	-
Female participant (D)	-2.797	(3.262)	-0.108	(0.077)	+
Months of food shortage	0.925	(0.940)	0.032	(0.023)	+
Prior tree planting (D)	4.907*	(2.681)	0.156***	(0.060)	+
Prior contact with NGO (D)	-0.998	(4.318)	-0.020	(0.091)	+
Risk preferences (1-3)	-3.054**	(1.427)	-0.094***	(0.033)	-
Time preferences (1-6)	-1.036	(0.763)	-0.032*	(0.018)	+
Mistrusts outsiders (1-3)	0.231	(1.780)	0.013	(0.043)	-
Willingness to try tech (1-3)	-1.866*	(1.104)	-0.061**	(0.025)	+
Asset index	0.836	(0.632)	0.022	(0.014)	+
Constant	41.686**	(16.440)	3.722***	(0.380)	
Village F-stat[b]	2.29**		50.57***		
N	89		89		
Adjusted R-sq	0.161				

Regressions of tree survival on survey regressors for posted offer treatment only. Robust standard errors are in parentheses. * p<0.10, ** p<0.05, *** p<0.001. dc / dx describes the expected comparative static effect of each regressor on compliance. [a]See Table 1 for a description of regressors. [b]Village F-stat reports the joint F-statistic and significance of village indicator variables.

Table 6: Village effects on contract allocation and compliance, by treatment

A. Auction treatment			
	Contract allocation (probit)	Tree survival (OLS)	Tree survival (OLS)
	(1)	(2)	(3)
Village F-stat	46.06***	3.89***	
Contracts in village[a]			
received via auction			3.326
			(6.900)
received via lottery			19.558*
			(10.908)
Constant	0.674	45.167***	37.220***
	(0.482)	(1.312)	(3.171)
N	204	82	82
R-square	0.199	0.406	0.043

B. Posted offer treatment			
	Contract allocation (probit)	Tree survival (OLS)	Tree survival (OLS)
	(1)	(2)	(3)
Village F-stat	16.13	20.40***	
Contracts in village			
received via auction			17.445**
			(7.668)
received via lottery			-9.945
			(13.280)
Constant	-0.566	49.000***	37.524***
	(0.503)	(0.815)	(3.981)
N	204	89	89
R-square	0.061	0.333	0.057

Notes: Probit and OLS regressions of outcomes on village variables, by treatment. Robust standard errors are in parentheses. * p<0.10, ** p<0.05, *** p<0.001. [a]The number of contracts in the village, by treatment group, are normalized for village size.

Table 7: Spatial autocorrelation models of contract compliance outcomes

A. Spatial lag model		
	Spatial weights (W)	
	2 km	18 neighbors
Auction	3.387 **	3.561**
	(1.413)	(1.420)
Lag survival	0.391*	0.225
	(0.210)	(0.189)
Constant	23.139***	29.799***
	(8.578)	(7.758)
Spatial lag dependence[a]	2.932*	1.657

B. Spatial error model		
	Spatial weights (W)	
	2 km	18 neighbors
Auction	3.226**	3.468**
	(1.406)	(1.418)
Constant	39.059***	39.017***
	(1.337)	(1.127)
λ (spatial errors)	0.384*	0.211
	(0.214)	(0.194)
Spatial error dependence[a]	2.449	1.318

Notes: Maximum likelihood estimates of tree survival using spatial weighting matrix constructed from specified neighbors or distance thresholds. N=171 for all regressions. Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.001. [a] Likelihood ratio test statistic.

Table 8: Contract costs estimated at market prices (Panel A) and household constraints (Panel B)

A. Estimated input and output values (Malawi Kwacha) for crop production and the tree planting contract						
	Labor costs			Input costs	Land costs	
	Land clearing (one-time)[a]	Field preparation and planting (annual, fixed)[a]	Weeding and harvest (annual, variable)[a]	Inputs (annual)	Production income (annual)[b]	Net
Alternative land uses						
Local maize	0	2800	7600	800	47070	35870
Soya	0	3400	5200	100	148350	139650
Tree planting contract						
Year 1, productive land	0	4000	4800	0	6000	-2800
Year 1, idle land	1200	4000	4800	0	6000	-4000
Year 2	0	0	4800	0	3000	-1800

B. Observed land reallocation patterns and implied household constraints						
Prior land use	Total[c]	Brought new land into production	Have other idle land	Land constrained	Labor constrained	Input constrained
Maize	53	13	23	25	14	14
Soya	29	8	11	17	4	8
Tobacco	7	1	3	3	3	1
Other crop	8	5	3	2	1	5
Fallow	77	28	58	-	16	61
Total	174	55	98	47	38	89

Notes, Panel A: All data are from Ntchisi District Agriculture and Development Officers and the 2007 Crop Estimate Report for Ntchisi District, Malawi. [a] Labor costs calculated at average market wage rate for day casual labor. [b] Crop income calculated using local market rates from 2008 and average yields from 2007.

Panel B: Data from follow up household survey and monitoring. Households that did not bring new land into production, do not have other idle land and did not use fallow land are identified as land constrained. Households that have other idle land but did not bring new land into production or use fallow land are identified as labor constrained. Remaining households are identified as labor constrained. [c]Total sums to >171 because some land allocated toward the contract was previously used to produce more than one crop.

Table 9: Hypothetical contract valuation

	N[a]	Mean	Std. dev	Min	Max
How much would you bid if asked to bid for contract now?	414	11080.19	21830.26	50	350000
How much would you bid if this were being offered by your neighbor?	411	11585.77	11398.34	250	150000
What is the lowest price that would make you feel this contract is good for you?	410	8828.23	9076.14	200	90000
Would you accept the contract for MWK 1,500?	421	0.779	0.415	0	1
Would you accept the contract for no pay?	419	0.766	0.424	0	1

Notes: Responses by allocation experiment participants to a survey of hypothetical valuations conducted nine months after allocation. Values are in Malawi Kwacha (MWK). [a]Response rates to the questions vary because of some respondents' unwillingness or inability to answer these hypothetical questions. The total number of respondents interviewed in the follow up survey was 424 out of 433 participants in the allocation activities.

Table 10: Carbon sequestration projections and carbon price per tree

Year	Above ground biomass	Below ground biomass	Total tons/tree	Carbon cost ($/ton)[a]
5	0.010	0.003	0.013	136.00
10	0.010	0.003	0.013	136.00
15	0.060	0.016	0.076	22.52
20	0.160	0.041	0.201	8.48
25	0.510	0.127	0.637	2.67
30	0.640	0.160	0.800	2.13
35	0.650	0.161	0.811	2.10
40	0.470	0.117	0.587	2.90

Notes: Biomass estimates by Kachamba, D. (2008) Report on carbon storage potential of M'bawa trees in Central Malawi. [a] Carbon price based on the total tons of carbon per tree multiplied by the total contract payment of ~USD 1.70/tree.

Figure 1: Individual bids for tree planting contracts and allocation by treatment

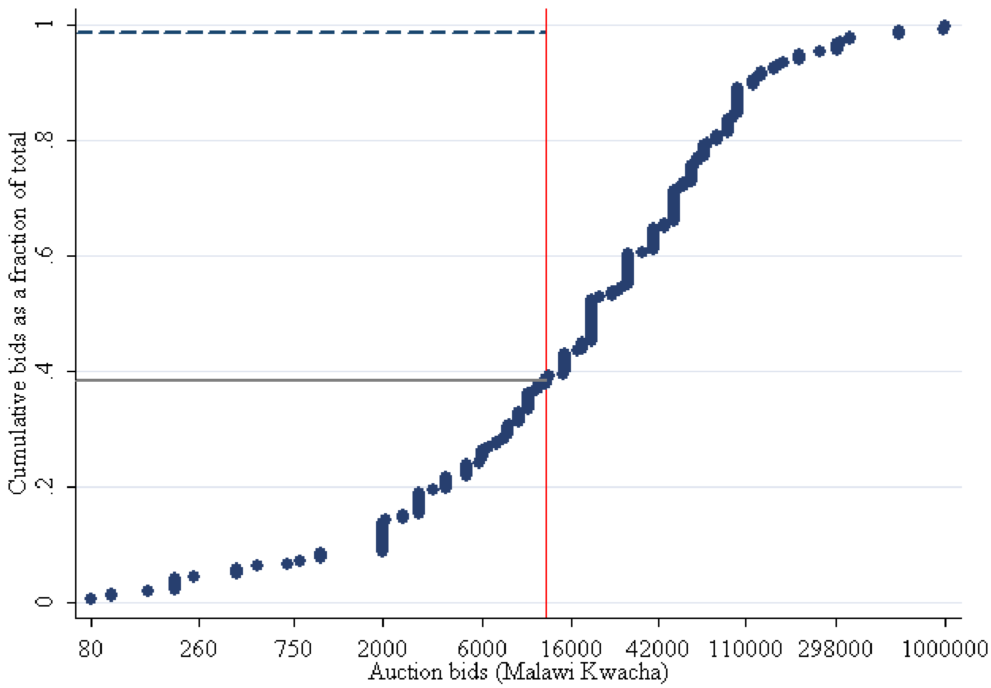


Figure 2: Tree survival outcomes by treatment group

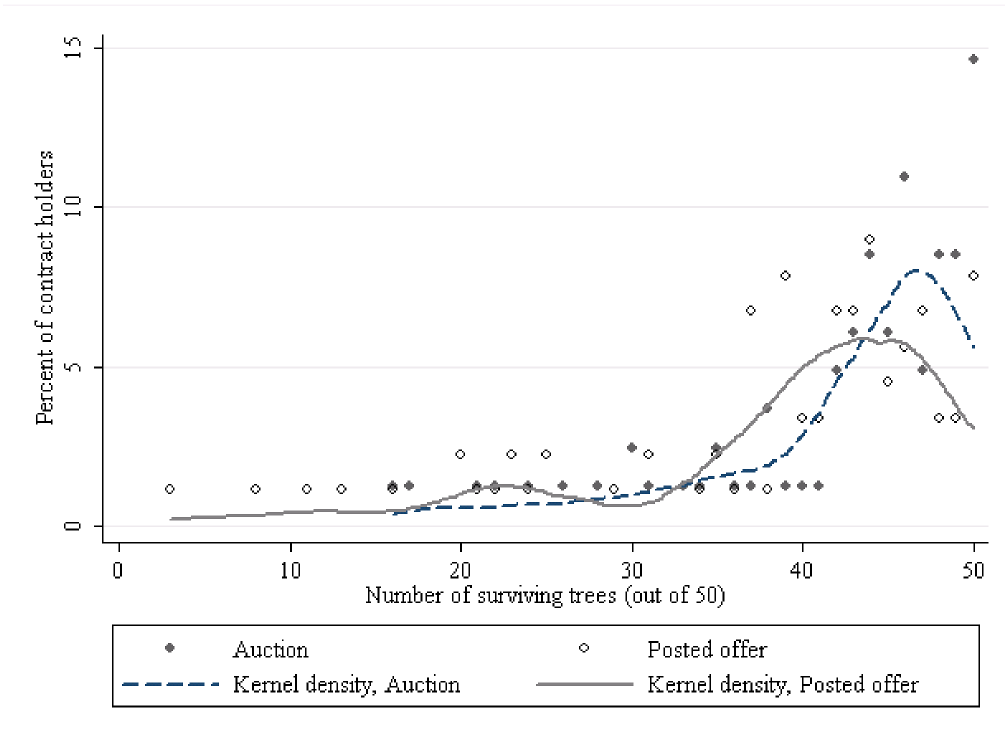


Figure 3: Correlation of bids based on auction seating location

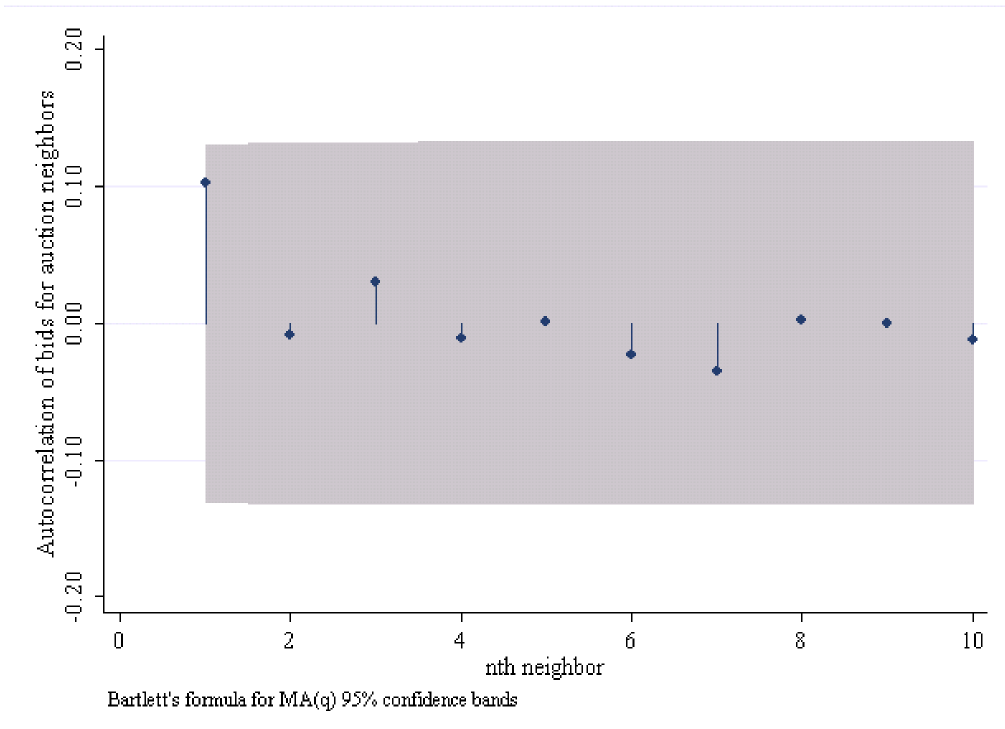
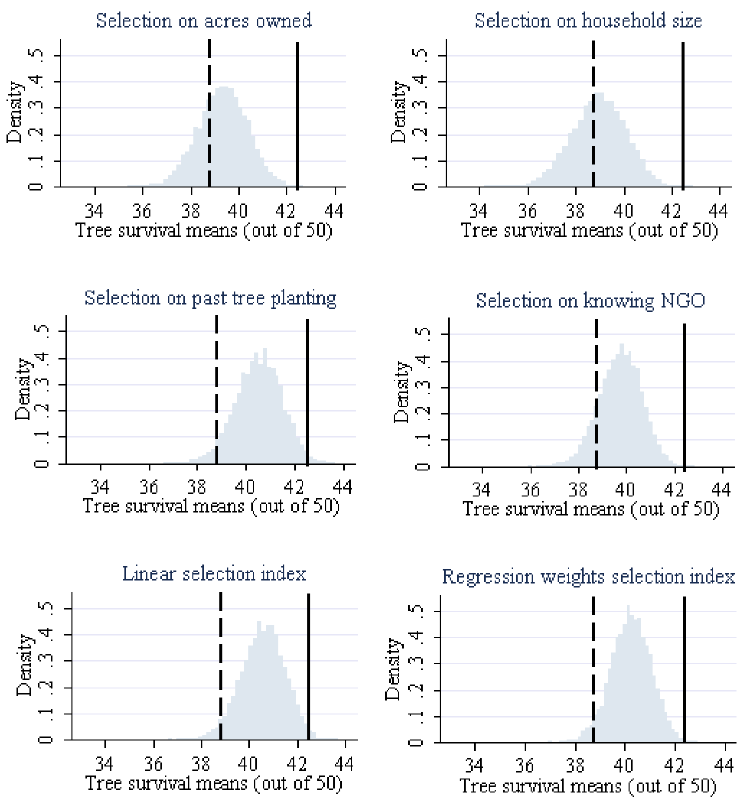


Figure 4: Simulated contract outcomes for selection on observables by a social planner



Notes: Histograms of mean survival rates from simulations of social planner selections using randomly drawn samples from the posted offer contract recipients. Each histogram describes simulated outcomes from a different selection criterion. The dashed line shows the mean tree survival rate from the posted offer selection. The solid line shows the mean tree survival rate from the auction selection.