Uncertainty, self-selection and the design of subsidies: Evidence from Zambia*

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Abstract

Many technology adoption decisions are made under uncertainty about the costs or benefits of adoption. As shocks to the net benefits are realized, agents may prefer to abandon a technology that appeared profitable at the time of take up. Thus, uncertainty breaks the link between the decision to take up and to follow through with a technology. We use a field experiment to study the performance of two common incentive tools to promote adoption in a context characterized by uncertainty about the costs of follow through. Farmers in rural Zambia are offered input subsidies for tree seedlings and performance rewards for tree survival, both of which are effective at increasing the total number of trees. The incentive treatments identify a structural model of intertemporal decision making under uncertainty. Estimation results indicate that the farmers experience idiosyncratic shocks to net profits after take up, which increase participation but lower average per farmer tree survival. While input subsidies have a relatively small (positive) effect on social welfare, the level of performance reward that maximizes welfare is high, due to its combined effect on (a) the social value of increased tree survival, (b) direct farmer profits conditional on performance and (c) the farmer's unconditional option value of postponing the decision to follow through.

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1 Introduction

Incentives are often used to increase technology adoption in the face of market failures, ranging from missing credit markets (Tarozzi et al. 2011; Miller and Mobarak 2011) to positive externalities (Miguel and Kremer 2004; Davis et al. 2013). Whether the incentive is conditioned on taking up (subsidy on purchase price) or on following through with the technology (a performance reward) should not affect outcomes, as long as take-up and implementation decisions are made under the same information sets, i.e. the adopter faces no uncertainty about the benefits and costs associated with follow through. If, on the other hand, the link between the take up and follow through decisions is broken by uncertainty in the costs or benefits of implementation, the marginal taker may differ from the marginal implementer, and a subsidy conditioned on take up is not equivalent to a performance reward.[1] As uncertainty increases, the decision to take up the technology becomes less predictive of follow through, lowering the cost effectiveness of adoption incentives, particularly when performance monitoring is costly. At the same time, a performance reward provides a positive transfer in the option to postpone the follow through decision until shocks to net benefits are realized. Hence, from a social planner's perspective, the type of instrument chosen may play a role in total net benefits from adoption incentives and not only in the distribution of benefits between society and the individual adopter.

We use a field experiment to examine the link between farmer decisions to take up and to follow through with an agricultural technology that generates positive environmental externalities. Specifically, we introduce random variation in input subsidies and performance rewards in the context of a tree-planting program in Zambia. The program encourages the adoption of trees that both fix nitrogen (providing private fertilizer benefits) and sequester carbon (generating a positive climate change mitigation externality). The use of adoption incentives in this context is justified by both the positive environmental externality and the absence of credit markets to support long-run agricultural investments.[2] Our reduced form results show that farmers are responsive to both types of instruments, but on different decision margins: input subsidies increase take up and performance rewards increase tree survival conditional on take up. The incentives that farmers face at the time of take up do not affect selection into the contract in terms of the likelihood of follow through, and a

[1]Subsidies for take up and performance rewards may also deliver different outcomes if follow through has an intensive margin. This is true for durable goods such as bed nets, light bulbs, or stoves (Dupas 2013; Ashraf et al. 2010; Cohen and Dupas 2010; Berry et al. 2012). However, to the extent that take up and implementation decisions are made at different points in time, uncertainty about benefits and costs associated with follow through may drive an additional wedge between subsidies and performance rewards in the case of durable goods.

[2]Incentives for land use practices that generate positive externalities are common in both developed and developing countries, where they are referred to as agri-environmental or payments for environmental services schemes. A number of quasi-experimental studies evaluate the impact of land use incentives in developing countries (Robalino and Pfaff 2013; Uchida et al. 2009; Alix-Garcia et al. 2012; Arriagada et al. 2012), most of which rely on matching techniques. Other papers examine the issues and challenges facing incentive design (for example, Chambers and Quiggin 1996; Wu and Babcock 1996; Mason and Plantinga 2013). Few papers use exogenous variation to empirically test contract design (Pattanayak et al. 2010).

large share of farmers take up but produce no trees. In other words, farmers appear to have limited ability to predict their costs of implementation at the time of take up.[3]

The reduced form results motivate a model of technology adoption under uncertainty.[4] The model allows for some share of the costs associated with implementation to be observed at the time the take up decision is made, and the remaining share to be observed at the time of the follow through decision. If the share of unobserved costs is large, farmers' take up decisions may be poorly correlated with follow through. While subsidies may be effective for increasing take up, subsequent shocks will lower average follow through. In this case, a performance reward has the advantage of paying out conditional on tree survival outcomes. The reward may also increase take up beyond just its effect on expected payoffs by increasing the option value of the contract; farmers can abandon the technology without penalty in states of the world where scarce resources, such as labor, are more profitably allocated to other productive activities.

The intertemporal decision model is identified using the variation created by our experiment. First, note that the heterogeneity in the cost of follow through that is always observed by the farmer creates heterogeneity in expected benefits across farmers at the time of take up. Although some of this heterogeneity may be linked to observable characteristics of the farmer, the model allows for a random component of observed costs, which is identified through the variation in the subsidy for take up. Second, the remaining component of the implementation costs is only observed by the farmer at the time the follow through decision is made. This component is also treated as random (unobserved to the econometrician) and its distribution is identified from farmers' response to the random variation in the rewards for performance.

We estimate that the heterogeneity in contract payoffs known the to the farmer at the time of take up is substantial, as are the shocks to private profit that arrive after take up. Using our structural estimates, we implement a variety of simulations to examine program performance and welfare. First, we assess the optimal combination of subsidies and rewards in our research setting. Within the range of the experimental variation, tree survival and welfare are both maximized when subsidies and rewards are at their highest. Allowing the experimental parameters to vary across a wider range reveals that welfare peaks with rewards around 4 times the maximum value offered in the experiment.[5] The feasible range of the input subsidy is small relative to the reward. Second, we vary the magnitude of the uncertainty at the time of the take up decision. Participation is increasing

[3] The lack of self-selection in our setting contrasts with Jack (2013), who demonstrates efficiency gains from targeting on private information about the opportunity cost of afforestation in a payments for environmental services program in Malawi. Unlike in this earlier work, we find no evidence of self-selection in our setting, which may be due to differences in how contracts design or allocation, or in the magnitude of uncertainty in implementation costs.

[4] The model assumes that liquidity constraints are not correlated with the net benefits of the technology, which would confound willingness to pay for the technology with ability to pay (see, for example, Jayachandran (2013) for evidence that liquidity constraints drive PES take up). We rule out liquidity constraints in our empirical setting through our study design, which allows us to focus on valuation of the technology.

[5] The out of sample simulations carry a strong *ceteris paribus* assumption, which may not be justified for very large reward values.

and tree survival is decreasing in uncertainty. These two effects offset each other, resulting in an expected number of trees per invited farmer that does not vary substantially with uncertainty. In spite of the higher social cost per tree at higher levels of uncertainty, overall welfare is also relatively constant as uncertainty increases because of the increasing option value that is transferred to the farmer. Greater uncertainty therefore transfers value from the public sphere (cost effectiveness) to the private sphere (option value). For a sub-sample of farmers, the entire expected profit from the contract hinges on the option to postpone the decision to follow through. Third, we examine the role of monitoring costs in the relative ranking of these policy instruments. When monitoring performance is costly, welfare increases discontinuously when rewards are equal to zero. Input subsidies also have a detrimental impact on welfare by increasing the number of participants who require costly monitoring.

The use of incentives to increase technology adoption has been widely studied by development economists, with most applications focused on subsidizing take up. The emphasis on the initial take up margin is often justified by prohibitively high costs associated with measuring performance (e.g., Dupas et al. (2013)).[6] Consequently, much of the recent literature examines the allocational efficiency of subsidies in the face of unobservable heterogeneity (Cohen and Dupas 2010; Ashraf et al. 2010; Berry et al. 2012). To the extent that the literature on adoption incentives has considered uncertainty, the emphasis has been on learning about the benefits of the technology or costs of follow through, where subsidizing early adopters may improve subsequent self-targeting (Dupas 2013; Oster and Thornton 2012).[7] Our interest is in uncertainty associated with stochastic shocks to the costs or benefits of the technology that arrive after the initial take up decision.[8]

The literature on investment under uncertainty differentiates between technological uncertainty and input price uncertainty, the former of which can be learned by undertaking the project and the latter of which is external to the project but is revealed over time (Pindyck 1993; Dixit and Pindyck 1994). Most relevant to the problem we study are the papers that consider land use decisions under uncertainty, including conservation set-asides (Schatzki 2003; Isik and Yang 2004), deforestation (Albers 1996) and land development (Quigg 1993). In these papers, uncertainty increases the option

[6]Incentives targeted at follow through more closely resemble conditional cash transfers, which condition payments to eligible households on a threshold level of behavior such as school attendance or health clinic visits (e.g., Gertler and Gruber 2002; Schultz 2004). Examples of the use of incentives to increase follow through or utilization include medication adherence (Giuffrida and Torgerson 1997) and gym attendance (Charness and Gneezy 2009).

[7]A much larger literature examines learning as part of the technology adoption process, with many applications in agriculture (see Foster and Rosenzweig (2010) for a summary).

[8]A recent paper on monsoon forecasting in India demonstrates that uncertainty associated with seasonal rainfall variation has a large impact on agricultural investment decisions and productivity (Rosenzweig and Udry 2013). While aggregate shocks such as weather or prices are a substantial source of uncertainty for agricultural households, Carter (1997) shows that much of the variance in yields over time is idiosyncratic to the household rather than common to the village in rural Burkina Faso. Returns to technology adoption may vary both across farmers and over time, as shown by Suri (2011). More generally, the role of risk and uncertainty in agricultural technology adoption is the focus of a large literature (see Feder et al. (1985); Foster and Rosenzweig (2010) for reviews), little of which focuses on implications for the design of adoption incentives.

value of delayed investment. Farmers in our study, on the other hand, maintain the option value of the contract by participating in the program and making continued costly investments, which parallels investment projects with cost uncertainty (Pindyck 1993), education decisions (Stange 2012) and retirement choices (Stock and Wise 1990).[9]

Methodologically, our econometric framework is an example of sequential identification of subjective and objective opportunity cost components in a dynamic discrete choice model (Heckman and Navarro 2007, 2005). As described in Heckman and Navarro (2007), we can account for selection into treatment (in our case take up of the tree planting program as well as non-corner solution tree survival outcomes) when identifying the distribution of the unobserved opportunity cost determinants. We do so by introducing two layers of random variation in economic incentives, one of which produces a probability of participation equal to one for a sub-population and a second of which produces interior solution in tree planting outcomes with probability one in the limit. The use of experimental variation in treatments at two different points in time offers an alternative to a panel data structure (Einav et al. 2013), since statistically independent samples are exposed to each of the different treatment combinations. To our knowledge, this is the first paper to introduce experimental variation in order to satisfy the exclusion restrictions needed for sequential identification.[10]

The rest of the paper proceeds as follows. We turn next to a description of the empirical context and experimental design. In Section 3, we show the reduced form treatment effects, which motivate our model of intertemporal decision making under uncertainty. We present the model and its identification in Section 4 and show estimation results and welfare simulations in Section 5. Section 6 presents alternative explanations, including learning and procrastination, and Section 7 concludes.

2 Context and experimental design

Nearly three-quarters of the rural population in Zambia is engaged in agriculture (CSO 2012), and the potential for afforestation and reforestation is high. One estimate suggests over 73,000 square kilometers of land eligible for carbon offsets in Zambia (Zomer et al. 2008). Under agroforestry-based land management, this could result in the sequestration of over 7 million tons of carbon dioxide equivalent per year (Kokwe 2012). At a conservative price of USD 5 per ton, the total potential revenue from carbon offsets is upward of USD 30 million annually. The project we study

[9]The option value is akin to the "quasi-option" value described by Arrow and Fischer (1974): the value of the information revealed by delaying investment, or – in our case – ongoing costly investment to maintain the option to follow through. In this sense, the contract in our study is a put option (Pindyck 1993). Like Stange (2012), we calculate the option value as the difference between a binding participation decision and one with free exit.

[10]Combining field experiments with structural modeling is an increasingly popular approach to extending the generalizability of experimental findings and testing among alternative models of behavior (see, for example, Todd and Wolpin 2006; DellaVigna et al. 2012; Duflo et al. 2012; Alatas et al. 2013). Our approach resembles a selective trial as described by Chassang et al. (2012) in that the participation decision reveals private information about farmer type.

encourages the adoption of agroforestry trees (*Faidherbia albida*) by small holder farmers engaged in contract cotton farming. The study was implemented in coordination with Dunavant Cotton Ltd., a large cotton growing company with over 60,000 outgrower farmers in Zambia, and with an NGO, Shared Value Africa. The project, based in Chipata, Zambia, targeted approximately 1,300 Dunavant farmers. The project is part of the NGO partner's portfolio of carbon market development projects in Zambia, and the private sector partner's ongoing investment in long run yield improvements for its farmers. We describe the technology, the study sample, the experimental design and implementation in the remainder of the section.

2.1 The technology

Faidherbia albida is an agroforestry species endemic to Zambia that fixes nitrogen in its roots and leaves, providing farmers with private benefits over the long term, including better soil fertility resulting in higher maize yields. Optimal spacing of *Faidherbia* is around 100 trees per hectare, or at intervals of 10 meters. The relatively wide spacing, together with the fact that the tree sheds its leaves at the onset of the cropping season, means that incorporation of *Faidherbia* on a plot does not displace other crop production (Akinnifesi et al. 2010). Agronomic studies suggest significant yield gains from planting *Faidherbia* relative to production without fertilizer. However, these private benefits take 7-10 years to reach their full value, and may be insufficient to justify the up-front investment costs in the absence of more short-run incentives. Indeed, a back of the envelope estimate suggests that the adoption of *Faidherbia* is not a privately profitable investment for most farmers, with average benefits of USD 2.02 and average costs of USD 19.[11] Negative net benefits, on average, are consistent with low adoption rates at baseline: less than 10 percent of the study households reported any *Faidherbia* on their land.[12]

Although the tree offers numerous potential public benefits, including erosion control and wind breaks, in our analysis we focus on the carbon sequestration benefits. Sequestration flows are estimated from allometric equations based on Brown (1997) and adapted to the growth curves for *Faidherbia*. Appendix figure A.1 shows both the annual and cumulative CO_2 take up per tree. Over

[11] Agronomic studies suggest yield gains from planting *Faidherbia* relative to production without fertilizer, with estimates of increases of 100 to 400 percent (Saka et al. 1994; Barnes and Fagg 2003), corresponding to income gains of up to USD 400 per hectare per year. For our back of the envelope calculation, we take mid-range values from the literature and assume that yields on half a hectare of land increase by 525 kilograms, starting 10 years after trees are planted. At current maize prices, this equals additional income of 136.50 USD per year from years 10 to 20. At a discount rate of 0.67, which is based on survey data and the literature, the present value of this investment is USD 2.02. While this discount rate is high, it ignores risk and uncertainty and is in line with observed interest rates and elicited individual discount rates in rural developing country settings (Conning and Udry 2007; Cardenas and Carpenter 2008). It corresponds to a discount factor of 0.6, which we employ in the structural model. With respect to the private costs, surveys implemented in conjunction with our study indicate that farmers invest around 38 hours planting and caring for the trees. With an agricultural wage rate of around USD 2.5 per day, these time investments result in around USD 19 of labor costs incurred in the first year.

[12] Informal land tenure in the project area presents an additional barrier to adoption. By focusing on landholders engaged in contract farming arrangements, the project targets households with relatively greater tenure security.

the course of 30 years, a tree sequesters around 4 tons of carbon dioxide equivalent. Discounting the annual sequestration at 15 percent leads to a present value of around 0.48 tons. At a marginal social cost of carbon of 21 USD/ton (Greenstone et al. 2011), the present value of sequestration associated with 35 trees is around USD 353.

2.2 Experimental design and randomization

The study design relied on Dunavant's outgrower infrastructure, which is organized around sheds, each of which serves several dozen farmer groups. The farmer groups consist of 10-15 farmers and a lead farmer, who is trained by Dunavant each year and in turn trains his or her own farmers on a variety of agricultural practices.

The study design varied three major margins of a contract offered to Dunavant farmers for adoption of *Faidherbia albida*. The base parameter of the contract is a fixed number of seedlings (50, or enough to cover half a hectare) to be planted and managed by the farmer and his or her household. In keeping with the existing bonus schemes that study farmers face, and to facilitate empirical identification (see Section 4), the program offered a threshold incentive conditional on performance (tree survival) after one year. Farmers were paid conditional on keeping 70 percent (35) of the trees alive through the first dry season (for 1 year).[13] The costs associated with planting and caring for the trees are highest during the first year, after which the labor costs associated with the trees decreases substantially. This cost structure justifies the use of short run incentives.

Figure 1 summarizes the experimental design. First, the size of the input subsidy (A) was varied at the farmer group level in increments of 4,000 ZMK from a subsidy of 12,000 ZMK (fully subsidized) to zero. At zero subsidy, farmers paid 12,000 ZMK (approximately USD 2.60) for inputs, which is the cost recovery price for the implementing organization. Groups were randomly assigned to one of four input subsidy treatments using the min max T approach (Bruhn and McKenzie 2009), balanced on Dunavant shed, farmer group size and day of the training. The resulting treatment groups are described in Section 3.2.

Second, the size of the performance reward (R) was varied at the individual level, in increments of 1,000 ZMK, ranging from zero to 150,000 ZMK or approximately 30 USD.[14] Variation in the reward was introduced using a random draw at the time of the take up decision. One-fifth of all draws were for zero ZMK with the remaining four-fifths distributed uniformly over the range.

[13]Numerous other incentive schemes would be feasible in this setting. In addition to its advantages in our empirical design, the threshold incentive was was easy to enforce, consistent with other related contracts in the setting, and easy to explain to the farmers. The threshold was chosen based on *Faidherbia* survival rates in other programs in Eastern Zambia. Specifically, for a sample of around 3000 farmers tracked by another NGO, which offered no performance incentive, a 70 percent survival rate was achieved by around 20 percent of farmers.

[14]At the time of the study, the exchange rate was just under 5000 ZMK = 1 USD. In piloting, the distribution of payments extended to 200,000 but was scaled back prior to implementation. The scratch cards with values between 150,000 and 200,000 were removed from the prepared cards by hand, but six of them were missed. For the main analysis, we top-code payments at 150,000.

Third, the timing of the payment draw was varied at the individual level to occur either before or after the farmer's take up decision. The timing treatment was pre-determined based on the registration number. Thus, in the surprise reward treatment, farmers were unaware of the existence of the conditional payments when they made their decision to take up the program. This timing variation to separate selection effects from effort effects is similar in spirit to Karlan and Zinman (2009). We do not control for or measure beliefs about potential financial benefits from joining in the surprise reward treatment.

2.3 Data and implementation

The field experiment was implemented between November 2011 and December 2012 in six Dunavant sheds with 125 farmer groups. The study included the following stages: farmer training, program enrollment, baseline survey, end line survey, tree monitoring, reward payment. We describe each of these stages in turn.

Farmer training

Lead farmers attended a training on *Faidherbia* at their Dunavant shed, which provided them with the information and materials necessary to train their own farmer groups.[15] The group trainings were scheduled by the project team for approximately four weeks after the lead farmer training and were attended by study enumerators. Either the household head or an immediate family member participated in the group training, as per Dunavant's usual practices. At the group training, farmers were provided with instructions on planting and caring for the trees, were given information about the private fertilizer benefits and public environmental benefits of the trees, and were informed about the eligibility requirements for the program.[16] All farmers who attended the group training received a show up fee of ZMK 12,000 and lunch.[17]

Program take up

After the lead farmer completed the technical training, study enumerators explained the details of the contract, including the cost of the inputs (12,000 ZMK minus the subsidy), which was announced to the group as a whole. To implement the individual-level randomization of the rewards and allow

[15] As described above, this is standard practice. At the lead farmer training, study enumerators collected basic information from the lead farmers for input into the randomization. Thus, any selection into the *Faidherbia* training by lead farmers is orthogonal to treatment. In addition to training their own farmers, lead farmers were responsible for constructing a nursery in their community, and were paid for the labor and materials. Backup nurseries were also constructed at the Dunavant sheds.

[16] Eligibility required that land must have been un-forested for 20 years, must be owned by the farmer, and must not be under flood irrigation. Study enumerators confirmed that all lead farmers covered the necessary training material and summarized the main points at the end of the training to ensure that all farmers had accurate information.

[17] The show up fee is equal to approximately a day's agricultural wages. Note that the show up fee was sufficient to cover the cost of inputs at even the lowest subsidy level.

participants to make their take up decision in private, farmers were called upon individually by study enumerators, who used the registration list to determine whether the farmer was in the surprise reward treatment. If not, then the farmer was told that could earn a reward for 70 percent tree survival after one year. The farmer then drew a scratch off card from a bucket, which revealed the individual reward value, after which the take up decision was recorded. The surprise treatment proceeded similarly but the take up decision was recorded before the farmer was informed that conditional payments were available. Conditional on agreeing to join the program, the farmer was told about the rewards and given the opportunity to draw a scratch card. After the value was revealed, farmers could change their minds about participation. None did.

Baseline survey and other data collection

Following the take up decision, all farmers, regardless of take up, were given a baseline survey that lasted for approximately one hour. Of the 1317 farmers for whom a take up decision was recorded, 1292 are in the baseline survey sample. After the survey, participating farmers signed the contract, paid the input cost and collected their seedlings.[18]

Two additional data collection instruments were administered between the baseline and endline surveys. First, one-fifth of the farmers were randomly sampled for ongoing visits to collect information on activities and inputs associated with the contract and with other crops. These visits, which we refer to as effort monitoring, consisted of a very short survey (~20 minutes), during which a project monitor asked the farmer about agricultural activities since the last visit.[19] We control for the effort monitoring subsample in our regressions but do not analyze its effects on program outcomes. Second, all farmers were visited at the end of the planting season for a very brief survey on seedling pick up, transplanting and care, primarily for the purpose of obtaining qualitative information about the timing of contract implementation.

End line survey, tree survival monitoring and reward payments

One year after program enrollment, we collected endline tree survival and household data, and delivered performance rewards. These visits were organized as follows. All farmers in the study sample were given an endline survey, regardless of their decision to participate in the contract. Of the 1292 farmers in the baseline survey, 1232 (>95 percent) are also in the end line survey. Farmers with contracts were visited one week later for field monitoring, during which the farmer and a study enumerator examined each tree, and recorded whether it was sick, healthy or dead. All surviving trees counted toward the performance threshold. In case of disputes, a third party was called in

[18] In a small number of cases, the lead farmer's nursery had not generated enough seedlings to support the group. In those cases, seedlings from the backup nursery were transported to the site.

[19] No information was provided to the farmers about their performance and monitors were instructed not to prompt specific activities or answer technical questions.

from the village. Additional data to proxy for quality of care, such as tree height and diameter and evidence of weeding and watering, were also recorded by the enumerator.

Finally, within a couple of days of the monitoring visit, the reward payment team visited each farmer group and paid out rewards to farmers with 35 or more surviving trees. Keeping the payments separate from the monitoring was intended to improve monitors' objectivity.[20] Of the farmers eligible for monitoring, 9 could not be found for monitoring. We assume zero tree survival for these individuals in our main analyses.

3 Summary statistics and reduced form results

Before moving to the structural model, we summarize the data and present reduced form results. We begin with characteristics of the sample, then empirically test the validity of the experimental design, and present the reduced form effects of the treatments on contract take up and performance. We focus, in the reduced form analysis, on evidence for selection on type and discuss the implications for unobserved heterogeneity, which we explicitly estimate in Section 4.

3.1 Sample characteristics

Appendix table A.1 provides summary statistics from the baseline survey. Around 70 percent of participants are heads of household and around 13 percent of households are female headed. Respondents have, on average, just over 5 years of education and live in households with just over 5 members. Respondents' answers to a time preference question reveals an ordinal ranking of their discount rate, where 5 represents the lowest value placed on the future.[21] Non-agricultural assets represent an unweighted sum of durables owned by the household. Households have around 3 hectares of land spread across just under 3 fields, which are an average of around 20 minutes away from their dwelling. Around 10 percent of households state that soil fertility is one of the major challenges that their household faces. Households have worked with the partner organization for an average of over 4 years and over 40 percent interact regularly with their lead farmer. Very few are affiliated with other local organizations that promote *Faidherbia albida* (CFU and COMACO). Almost 70 percent of respondents report familiarity with the technology but only around 10 percent had adopted prior to the program. Finally, the survey asked farmers to recall risks to the trees

[20]As a check for collusion between the monitors and farmers, we test whether individual monitors are associated with a higher probability that a farmer passes the tree survival threshold. No single monitor indicator is significantly correlated with reaching the threshold, nor are the monitor indicators jointly predictive. This implies that either all monitors were cheating to the same degree or that no monitors were cheating. Given differences in career concerns with the study implementers (some had higher paid jobs as survey supervisors when not engaged in monitoring), cheating by all monitors is unlikely.

[21]The survey tool used to elicitate the discount rate is very coarse. A rank of 3 corresponds to a discount rate in the interval $0.26 < r < 1.00$. The survey responses are most informative as a relative ranking, so we do not use them directly in our structural estimation. They do, however, inform our choice of 0.6 for the discount factor in the structural estimation.

described in the training. Out of four risks described in the training (lack of water, termites, weeding, animals), respondents remembered around 1.7.

3.2 Balance and attrition

We begin by testing the randomization outcomes by comparing observable characteristics across treatment groups. Columns 1, 3 and 5 of Appendix table A.1 report the means and standard deviations for the zero input subsidy, zero reward and known reward timing treatment, respectively. Columns 2, 4 and 6 show the coefficients and standard errors from a linear regression of the covariate on input subsidy, reward level, and the surprise reward treatment, respectively. We observe some imbalance in the assignment of the input subsidy treatment with slightly larger households with more non-agricultural assets receiving lower input subsidy assignments on average. We also observe that older respondents with larger households and better self-reported soil fertility are marginally more likely to be assigned to the surprise reward treatment. The table tests balance for 17 variables and consists of 51 separate regressions. Five coefficients significant at $p < 0.10$ is therefore expected.[22]

Non-random attrition will also undermine the reduced form identification strategy.[23] We examine attrition at each stage of data collection in Appendix table A.2. Column 1 reports means and standard deviations of the treatment variables for all 1317 individuals who made a take up decision. Columns 2, 3 and 4 show coefficients and standard errors for univariate linear regressions of each treatment variable on a dummy for participation in the baseline survey, end line survey and in the tree monitoring, respectively. Participation rates in the baseline are high, with over 98 percent of individuals included. For the end line, 94 percent of all study farmers and over 95 percent of baseline respondents were located. We see some evidence that farmers who received lower input subsidies were marginally less likely ($p < 0.10$) to participate in the surveys. Otherwise, attrition is balanced across treatments. For the tree monitoring, over 95 percent of the 1092 households that took up the program were located.

Finally, spillovers across treatments pose a threat to the experimental design. Because the input subsidy treatment was assigned at the group level, spillovers are relatively unlikely. During regular effort monitoring visits, no farmers asked about difference in input costs. The value of the reward, on the other hand, varied at the individual level within-group. By revealing the reward treatment one by one immediately before soliciting the take up decision, we mitigate the potential that take up is affected by rewards received by others.[24] We observe some suggestive evidence that

[22] We include the variables shown in Appendix table A.1 as controls in our reduced form regressions. Two variables, respondent age and the ordinal measure of time preference, are not included because of of missing observations. Results are similar if they are included and interacted with a dummy for non-missing values. Without these variables, the sample size drops from 1292 to 1288 when controls are included in the regressions.

[23] Selection into treatment is also a threat to the experiment's internal validity. By design, this is unlikely: group level input subsidy treatments were revealed only after individuals arrived for training, and individual-level reward treatments were assigned in a one-on-one interaction with study enumerators.

[24] This is confirmed empirically by regressing the probability of take up on the average random reward draw that

being in a farmer group with an average higher reward draw has a positive spillover on own survival probabilities, though the magnitude of the effect is small relative to the direct effect of the reward.[25]

3.3 Participation

We measure the participation response to the exogenous variation in the contract parameters by estimating the linear regression

$$y_{ig} = \alpha + \beta A_{ig} + \delta R_i + \phi X_i + u_{ig} \tag{1}$$

where y_{ig} is the binary participation decision for farmer i in group g, A_{ig} is the input subsidy applied to group g, R_i is the reward offered to farmer i conditional on keeping 70 percent of his trees alive for one year and X_i is a vector of individual level controls. Errors are clustered at the farmer group level. All specifications also control for the random sample of farmers assigned to the effort monitoring visits and the surprise reward treatment.

We estimate equation 1, with y_{ig} equal to 1 if the farmer takes up the program and zero otherwise, and include an interaction term for A_{ig} and R_i in some specifications. Table 1 shows that participation is increasing in the input subsidy (column 1), with take up rates close to 100 percent when inputs are fully subsidized. Relative to farmers offered the contract at a full subsidy, subsidies of 8,000, 4,000 and zero ZMK decrease participation by 11, 22 and 26 percent respectively. Take up is also (marginally) increasing in the reward (column 2), with a one thousand ZMK increase leading to 0.06 percent higher take up. Finally, the effect of the reward differs across input subsidy conditions (column 4) with larger effects at lower subsidies. While participation is responsive to the exogenous variation in contract payoffs, heterogeneity across farmers also plays an important role: 10 percent or less of the variation in take up observed in the data is explained by the variation in subsidies and rewards.

preceded a farmer's own draw. The outcomes of preceding draws have no effect on the probability of take up.

[25] Randomization of rewards at the individual level also gives rise to concerns about reselling seedlings to those with higher rewards or transplanting young trees just before monitoring. We use several pieces of information to investigate these concerns. First, we would expect that take up would reflect the potential to re-sell if farmers were aware of the individual-level variation in rewards. In other words, farmers that were aware of the arbitrage opportunities generated by the variation in the reward treatment should be more likely to take up, and increasingly so as the size of the subsidy falls (i.e. would respond less to the subsidy). However, a regression of take up on the interaction of the surprise reward treatment and the level of the input subsidy shows no significant interaction or clear pattern of coefficients. Second, to investigate the potential for transplanting, we take advantage of planting data collected for all farmers shortly after the end of the rainy season. We construct a measure of the difference between the planting and the monitoring tree counts, which is positive for around 100 farmers. The positive value indicates either very delayed planting or transplanting. Restricting attention to those with a positive value, the coefficient from regressing this measure of extra trees on the size of the reward is insignificant, and becomes negative (and insignificant) when group fixed effects are included. Third, we examine the within-group spillovers associated with the effect of the reward on tree survival outcomes. To the extent that transfers of any kind are happening within group, we expect a steeper slope on the reward within-group than on average. We observe a slightly smaller and statistically indistinguishable coefficient on the reward when group fixed effects are included, relative to the coefficient without fixed effects.

3.4 Incentive effect of reward payments

Figure 2 shows the histogram of tree survival outcomes. The distribution of outcomes when the reward is zero (left panel) provides a counterfactual description of tree survival outcomes in the absence of reward payments. Farmers who did not take up are included with a tree survival outcome of zero. Relative to tree production in the absence of a reward, positive rewards shift mass away from zero and to the right of the threshold. Given the threshold structure of the contract, tree production outcomes not equal to the performance threshold when rewards are positive are driven by private payoffs from the trees.

We re-estimate equation (1), with tree survival outcomes on the left-hand side. Panel A of Table 2 includes only farmers who did not select on the reward, namely those in the surprise reward treatment. Panel B includes all farmers, with tree survival outcomes set equal to zero for farmers who did not take up the program. In Panel B we interact a dummy for a non-missing reward value with the reward in '000 ZMK to avoid dropping farmers in the surprise reward treatment who did not take up the program. Columns 1 and 2 of Panel A show that the average number of surviving trees increases by 0.07 (s.e. 0.02) for each thousand ZMK increase in the reward, or by about 7.3 (s.e. 1.8) trees when rewards go from zero to the average positive reward of around 77,000 ZMK. The probability of having zero trees is decreasing in the reward by 0.1 percent (s.e. 0.0004) for every thousand ZMK increase in the reward (column 3). The probability of keeping at least the threshold number of trees alive is increasing in the reward (columns 5 and 6), yet the number of trees produced, conditional on being above the threshold is not responsive to the reward (columns 9 and 10), consistent with production above the threshold being driven by private benefits. The results are statistically similar, though slightly smaller in magnitude, for the full sample shown in Panel B, which includes the selection effect of the reward for farmers who made their participation decision after drawing R.

3.5 Selection effects

Our design separately identifies the selection induced by the subsidy on inputs and the performance reward. In our context, selection is defined by differences in private payoffs from the contract among those who choose to participate under different exogenous contract incentives. Specifically, as the level of subsidy or reward decreases, the expected private payoff needed to induce participation increases. With little or no uncertainty about payoffs, higher expected private payoff individuals will keep more trees alive, i.e. we will observe a selection effect in farmers' performance.

Conditional on take up, Equation (1) with tree survival as the dependent variable estimates the selection effect of A_{ig} on tree survival.[26] Figure 3 plots the marginal effects, controlling for the

[26] This assumes that the amount paid for inputs does not directly enter into the farmer's optimal number of trees, which is not valid in the presence of behavioral biases. The sunk cost fallacy has been studied in multiple developing country field experiments (Ashraf et al. 2010; Cohen and Dupas 2010; Berry et al. 2012), none of which has found

rewards, effort monitoring condition, surprise reward treatment and individual covariates. Conditional on take up, tree survival is constant across all four input subsidy conditions, suggesting little effect of A on the composition of expected private payoffs. The intention to treat coefficients are included for comparison, and combine the participation effect of A with the selection effect. Given the lack of self-selection, results are similar to those shown in Table 1.

Analysis of the effect of selection on the reward on tree survival is less straightforward, since R enters into both the farmer's participation decision and tree survival outcome. To isolate the selection effect of the reward, we interact a treatment indicator for the surprise reward treatment with the level of the reward:

$$trees_{ig} = \alpha + \beta A_{ig} + \delta R_i + \gamma surprise_i + \lambda R_i \times surprise_i + \phi X_i + u_{ig} \tag{2}$$

where $\delta + \lambda$ identifies the incentive effect of the reward on tree survival, conditional on take up. Specifically, recall that farmers in the surprise reward treatment decided to join the program before learning about the reward, so R has only an incentive effect on tree survival. For farmers who drew R before they decided whether or not to participate, the effect of R on tree survival is made up of both the selection and incentive effects. Therefore, the difference between the known and the surprise reward treatment (λ) isolates the selection effect of R, conditional on take up. We would expect λ to be negative if higher rewards attract lower net benefit types.

Figure 4 shows marginal effects of tree survival on the reward timing treatment interacted flexibly with four reward bins, conditional on take up. The lines are overlapping and statistically indistinguishable at all reward levels. The figure therefore indicates that a higher reward does not induce self-selection that translates into meaningful tree survival differences.

4 Model, identification and estimation

The reduced form analysis indicates that the participation decision is a poor predictor of tree survival. This finding is consistent with a large share of the variation in payoffs from the contract coming from variation in the realized shocks to effort cost. The high average participation is also consistent with a large option value from the contract in the absence of penalties for non-compliance. We now turn to a model of intertemporal decision making under uncertainty in which farmers respond to a threshold contract for tree production. The structure imposed by the model allows us to explicitly estimate the distribution of both the contract payoffs that are known to the farmer ex ante and net benefits that are realized ex post. The magnitude of the uncertainty faced by the farmer at the time of the participation decision determines the amount of selection we observe as well as the role that option value – the ability to exit the contract penalty-free at any point after

evidence of its empirical relevance. In our setting, a sunk cost effect and a selection effect work in the same direction and so can both be ruled out in the case of a null result.

take up – plays in driving take up. In this section, we set up the model, describe its identification from the exogenous variation in contract parameters, and explain the estimation procedure.

4.1 Farmer net benefits

General profit function Consider the decision of a farmer to plant and care for trees for their private benefits. Assume a farmer has a general quadratic profit function given by

$$\Pi(N) = \sum_{t=5}^{\bar{T}} \left[\frac{1}{(1+r)^t} \left(\alpha_0 N - \alpha_1 N^2 \right) \right] - \gamma_0 N - \gamma_1 N^2 - \gamma_2 \times \mathbf{1}(N > 0) \tag{3}$$

where α_0 and α_1 are private benefit parameters, r is the annual discount factor, $\bar{T}$ is the maximum number of years a tree lives, γ_0 and γ_1 are cost of implementation parameters that are proportional to the number of trees and γ_2 is a fixed cost of implementation.

Equation (3) can be rewritten as

$$\Pi(N) = (\tau\alpha_0 - \gamma_0)\, N - (\tau\alpha_1 + \gamma_1)\, N_1^2 - \gamma_2 \times \mathbf{1}(N > 0)$$

where $\tau = \sum_{t=5}^{\bar{T}} \left[\frac{1}{(1+r)^t} \right]$. This new expression for the profit makes clear that, with $\alpha_0 > 0$, $\alpha_1 > 0$, $\gamma_0 > 0$, $\gamma_1 > 0$, $\gamma_2 > 0$, and $\tau\alpha_0 - \gamma_0 \geq 0$, equation (3) essentially describes decreasing marginal value to the number of trees planted and cared for. This is consistent with our empirical observation that a number of farmers plant a strictly positive number of trees that is below 50 (the number of seedlings they receive) whenever the reward for tree survival is equal to zero.

Once all implementation costs and benefits are revealed, a farmer chooses the number of trees to plant and care for that maximizes (3). The optimal number of trees planted and cared for by farmers is characterized by

$$N^* = \begin{cases} \frac{\tau\alpha_0 - \gamma_0}{2(\tau\alpha_1 + \gamma_1)} & \text{if } \frac{(\tau\alpha_0 - \gamma_0)^2}{4(\tau\alpha_1 + \gamma_1)} - \gamma_2 > 0 \\ 0 & \text{if } \frac{(\tau\alpha_0 - \gamma_0)^2}{4(\tau\alpha_1 + \gamma_1)} - \gamma_2 \leq 0 \end{cases}$$

Note that in principle, all parameters in (3), $\alpha_0, \alpha_1, \tau, \gamma_0, \gamma_1,$ and γ_2, may vary across farmers. Our experimental variation, however, does not allow us to separately identify all potential sources of heterogeneity across farmers. We therefore turn to a quasi-profit function that allows us to use our experimental variation to characterize farmer heterogeneity along two important dimensions of the farmer's profit maximization problem, $\max_N \Pi(N)$: the interior solution and the profit level evaluated at the optimal number of trees.

Quasi-profit function The same interior and corner solutions conditions delivered by (3) are generated by the following quasi-profit function indexed by two parameters.

$$\Pi_Q(N) = N - \frac{(\tau\alpha_1 + \gamma_1)}{(\tau\alpha_0 - \gamma_0)} N^2 - \left[\gamma_2 - \left(1 - \frac{1}{(\tau\alpha_0 - \gamma_0)} \frac{(\tau\alpha_0 - \gamma_0)^2}{4(\tau\alpha_1 + \gamma_1)}\right)\right] \times \mathbf{1}(N > 0) \tag{4}$$

This alternative function allows for heterogeneity across farmers in the interior solution, $\frac{\tau\alpha_0 - \gamma_0}{2(\tau\alpha_1 + \gamma_1)}$, as well as the scale of the profit associated with the interior solution, $\left[\gamma_2 - \left(1 - \frac{1}{(\tau\alpha_0 - \gamma_0)} \frac{(\tau\alpha_0 - \gamma_0)^2}{4(\tau\alpha_1 + \gamma_1)}\right)\right]$. For simplicity, Equation (4) can be parameterized as

$$\Pi_Q(N|T_i, F_i) = N - \frac{1}{2T_i} N^2 - F_i \times \mathbf{1}(N > 0) \tag{5}$$

where $T_i > 0$ is the interior solution to the optimization problem and $F_i \in (-\infty, \infty)$ is the scale and fixed cost parameter that determines the corner solution. The solution to the optimization problem defined by $\max_N \Pi_Q(N)$ is given by

$$N^* = \begin{cases} T_i & \text{if } T_i - F_i > 0 \\ 0 & \text{if } T_i - F_i \leq 0 \end{cases}$$

The advantage of (5) over (3) is that all random parameters are identified out of the variation induced by our experiment. The quasi-profit function (5) and profit function (3) produce the same solution (N^*) and take the same value when evaluated at the optimal solution. I.e.,

$$\Pi(N^*) = \Pi_Q(N^*|T_i, F_i) = \begin{cases} \frac{1}{2}T_i - F_i & \text{for } N^* > 0 \\ 0 & \text{for } N^* = 0 \end{cases}$$

Hence, (5) can be used to evaluate welfare under the more general profit function (3).

From here on, we assume that in the absence of an external incentive or reward for tree survival, farmers maximize

$$\Pi_Q(N|T_i, F_i) = \left(N - \frac{1}{2T_i} N^2\right) - \mathbf{1}(N > 0) \times F_i$$

with respect to the number of trees, N.

Introducing a reward for survival

The farmer's objective function when faced with a reward for tree survival is assumed to be

$$\Pi_R(N|F_i, T_i, R_i) = \left(N - \frac{1}{2T_i} N^2\right) + \mathbf{1}(N \geq \bar{N}) R_i - \mathbf{1}(N > 0) F_i \tag{6}$$

where R_i denotes the reward for reaching the goal of $\bar{N}$ trees. A farmer that faces a reward of R_i will maximize her objective function (6) subject to the choice of trees N. Denote by N_i^{**} the number of trees that maximizes (6),

$$N_i^{**}(T_i, F_i, R_i) = \arg\max_N \left(N - \frac{1}{2T_i}N^2\right) + \mathbf{1}(N \geq \bar{N})R_i - \mathbf{1}(N > 0)F_i \tag{7}$$

4.2 Dynamics, timing of information, and program participation decision

In order to analyze the effects of uncertainty about net benefits on the performance of the incentive program, we assume the farmer makes program-related decisions in two periods. In an early period, denoted $t = 0$, the farmer decides whether or not to pay the input cost in order to participate in the tree planting program. At this point in time, the farmer has partial information about her net benefits from the contract. At a later point in time, denoted $t = 1$, the farmer makes the decision of how many trees to plant and care for described by equation (7). At this later point in time, the farmer-specific determinants of the net benefit function (T_i, F_i) are fully known to the farmer.

More specifically, we separate the scale of the net benefit into two components such that $F_i = F_{0i} + F_{1i}$. Therefore, we now have three farmer-specific determinants of the profit function: T_i, F_{0i}, and F_{1i}. We assume that out of these three determinants, only F_{0i} is known to the farmer at the time the participation decisions is made ($t = 0$), while F_{1i} and T_i are revealed to the farmer at $t = 1$. Finally, we assume that the farmer knows the the distribution of F_{1i} and T_i conditional on F_{0i} at the time she makes her decision to participate.

Given this timing for the information, a farmer decides to participate in the program if the present value of the expected profit is greater than subsidized program cost:

$$\delta \mathbb{E}\left[\max_N \Pi_1(N|T_i, F_{0i}, F_{1i}, T_i, R_i)\middle| F_{0i}\right] - c + A_i \geq 0 \tag{8}$$

where c is the cost of the seedlings, A_i is the randomly determined subsidy, and δ is the discount factor.[27] The expectation in (8) is taken over the distribution of (F_{1i}, T_i) conditional on F_{0i}.

4.3 Identification and estimation

Identification of the structural model consists of uniquely identifying the joint distribution of unobservables T_i, F_{0i} and F_{1i}. Identification relies on

a) the observed variation in N_i^{**} and the participation decision across different treatments

[27] Like Stange (2012), we note that in the context of stochastic dynamic structural models the discount factor may not be separately identified from the scale parameter of future period shocks. Given that F_{1i} and T_i are the only parameters that are affected by the discount factor, a higher δ causes these $t = 1$ parameters to play a larger role in the participation decision.

b) random variation in R_i

c) random variation in A_i

d) a distributional assumption for F_i and T_i conditional on F_{0i}.

The identification of the joint distribution of F_i and T_i as well as the marginal distribution of T_i can be non-parametrically identified in the subset of the support such that $\bar{N} < T_i < 50$ and $F_i > \frac{1}{2}T_i$. In order to do this, we use the the subset of the sample for which

$$\lim_{a \to \mathcal{A}_1} \Pr(\mathbb{E}\left[\max_N \Pi_1(N|T_i, F_{0i}, F_{1i}, T_i, R_i) \middle| F_{0i} \right] \geq c - a) = 1,$$

such that there is no selection on participation at the time the farmer decides to plant trees. Within this set, we can use the variation in R to identify the joint distribution of F_i, T_i as well as the marginal distribution of T_i.

First, consider the probability of planting $N^{**} = n > \bar{N}$ trees when $R = r$. These probabilities can be written as

$$\Pr(N^{**} = n; R = r) = \Pr\left(F_i < r + \frac{1}{2}n \middle| T_i = n \right) \Pr\left(T_i = n\right) \tag{9}$$

Because the left hand side of (9) is empirically observable, changes in r trace out the joint distribution of F_i and T_i.

Using a limit set argument, the marginal distribution of T_i is also identified. Rewrite the joint probability of observing $N^{**} = n$ trees when the reward is r as

$$\Pr(N^{**} = n; R = r) = \Pr\left(T_i = n | \, F_i - r < \frac{1}{2}n \right) \Pr\left(F_i - r < \frac{1}{2}n \right)$$

There is a limit set such that

$$\lim_{r \to \mathcal{R}_1} \Pr\left(F_i - r < \frac{1}{2}n \right) = 1$$

Hence, $\lim_{r \to \mathcal{R}_1} \Pr\left(T_i = n | \, F_i - r < \frac{1}{2}n\right) \Pr\left(F_i - r < \frac{1}{2}n\right) = \Pr(T_i = n)$.

Note that non-parametric identification of the marginal distribution of T_i and joint distribution of F_i and T_i occurs only in the subset of the support such that $\bar{N} < T_i < 50$. Hence, additional parametric assumptions are required to fully characterize these distributions. In the empirical estimation we assume that T_i and F_i are independent and that T_i is distributed either uniform$(0, b)$ or log-normal(μ_T, σ_T), while F_i is normally distributed. These assumptions can be relaxed to account for distributions that vary across individuals according to observable characteristics, which allows for some correlation between T_i and F_i.

The decision to participate provides independent identification of the distribution of F_i through the variation in R and A. This allows us to capture the role of information and uncertainty in the participation decision and to determine the share of private profit that comes from the option to costlessly exit at $t = 1$. In order to separately identify the known and unknown components of F_i at $t = 0$, we add the following assumptions

(i) $F_i = F_{0i} + F_{1i}$

(ii) $F_{0i} \sim n\left(\mu_F, \sigma^2_{F_0}\right)$, $F_{1i} \sim n\left(0, \sigma^2_{F_1}\right)$, $F_{1i} \perp F_{0i}$, and

(iii) farmers know F_{0i} but do not know either F_{1i} or T_i at the time they make their decision to participate.

Identification of the distribution of F_{0i} is obtained from the decision to participate, which is characterized by the inequality

$$\delta \mathbb{E}\left[\max_N \Pi_1(N|T_i, F_{0i}, F_{1i}, T_i, R_i)\middle| F_{0i}\right] \geq c - A_i \tag{10}$$

The left side of (10) is a known function of the random variable F_{0i}. Denote this function $h(F_{0i}; r_i)$, so we can rewrite (10) as

$$h(F_{0i}; R_i = r_i) \geq c - a_j \tag{11}$$

The right side of (11) can take one of four known values, $c - a_j$ for $j = 1, ..., 4$, and is randomly determined by the research design. The left hand side of (11) is known up to F_{0i} and varies across individuals according to the known cost determinant, r_i. Provided that $h(F_{0i}; r_i)$ is invertible,[28] we can identify the distribution of F_{0i}, from the random variation in a_j and r_i

$$\Pr\left(F_{0i} \leq h^{-1}(c - a_j, r_i)|\mu_F, \sigma^2_F, G_T\right) = \Pr(Part_i|A_i = a_j, R_i = r_i)$$

where μ_F, σ^2_F, G_T (the distribution of T) can be treated as known since they are identified from tree survival as described above.

Our ability to obtain an alternative identification for F_i using the participation decision allows us to relax the assumption of mean consistency between two periods at which individuals make choices. We use this feature of our research design to explore whether $\mu_{F0} \neq \mu_{F1}$. A non-zero mean for F_{1i} is consistent with common shocks to the scale of the net profit, with learning, or with commonly held incorrect beliefs at $t = 0$. In the current draft, we assume $\mu_{F1} = 0$.[29]

Estimation

Our identification strategy allows us to estimate several versions of the model described above. The first set of estimations assumes that all farmers face the same distribution of F_{i0}, F_{i1} , and T_i. A

[28] It can be shown that there exists some $\bar{f}$ such that $h(F_{0i}; r_i)$ is strictly monotonically decreasing on $(-\infty, \bar{f})$.

[29] Preliminary runs of the model suggest $\mu_{F1} \neq 0$. Future drafts will include this more flexible parameterization. For now, we discuss the implications of common shocks and learning in Section 6.

second set allows these distributions to vary with observable characteristics of the farmer.[30]

For the estimation of the first version of the model, we further adopt a parametric assumption on T_i. Because the tree survival outcomes are fairly evenly distributed, we begin with the assumption that $T_i \sim$ uniform$(0, b]$. Alternatively, we assume that $T_i \sim$ log-normal(μ_T, σ_T) to test for robustness of distributional assumption. We also assume that F_{1i} and F_{0i} are both normally and independently distributed with $F_{1i} \sim n(0, \sigma_{F_1})$ and $F_{0i} \sim n(\mu_F, \sigma^2_{F_0})$, and $F_{0i}, F_{1i} \perp T_i$. The second version of the model is not included in the current draft.[31]

We estimate the model using simulated maximum likelihood. The log-likelihood function is over observations of the number of planted trees, $N = 0, ..., 50$, and the participation decision, $P = 0, 1$. Because there are no trees planted whenever the individual chooses not to participate, the support of this bivariate vector is given by the 52 (P, N) pairs: $(0,0), (1,0), (1,1), (1,2), ..., (1,50)$.

$$l(\xi; DP, \tilde{N}) = \sum_{i=1}^{M} \left\{ (1-DP_i)\ln(1-\pi_{P,i}) + DP_i \ln(\pi_{P,i}) + DP_i \sum_{j=0}^{50} \mathbf{1}(N_i = j) \ln \Pr(N = j) \right\}$$

where $\xi = (\mu_F, \sigma^2_{F_0}, \sigma^2_{F_1}, b)$.

We use numerical methods to minimize the negative of the log-likelihood. For each likelihood evaluation, we use 500 draws of (T_i, F_{0i}, F_{1i}). Also within each likelihood evaluation and for each draw of (T_i, F_{0i}, F_{1i}), the expectation in the right hand side of (10) is numerically computed using 100 draws of (T_i, F_{1i}) conditional on the draw of F_{0i}.[32]

5 Structural Estimates and Welfare Results

In this section, we describe the structural estimates and carry out a number of welfare simulations.

[30] We show estimates from the first set below and will include the set with heterogeneous, observable characteristics in future drafts.

[31] In a second version of the model, we assume that $F_{0i} \sim n(\mu_{Fi}, \sigma_{F_0})$, $F_{1i} \sim n(0, \sigma_{F_1})$, and either $T_i \sim$ uniform(b_i) or $T_i \sim$ log-normal(μ_{Ti}, σ_T), where $\mu_{Fi} = X_{Fi}\beta_F$, $b_i = X_{Ti}\beta_T$, and $\mu_{Ti} = X_{Ti}\beta_T$. This allows $F_i = F_{0i} + F_{1i}$ and T_i to be correlated through observable characteristics of the farmer. Hence, in the second version of the model the parameter set becomes $\xi = (\beta_F, \sigma^2_{F_0}, \sigma^2_{F_1}, \beta_T)$. The inclusion of observable characteristics allows us to both have a more flexible distribution of farmer types and quantify the importance of observable and unobservable characteristics in characterizing heterogeneity in private benefit determinants that are known to the farmer at all times.

[32] Simulated methods often result in stepwise objective functions which work poorly with gradient-based numerical optimization algorithms. To facilitate the numerical optimization, we "smooth" the objective function by computing the multilogit formula for each decision over participation and the number of trees. We assume a relatively small variance parameter of the logistic error term: 0.5. However, we experiment with different values for this parameter. We find that smoothing does not significantly affect the point estimates and does improve substantially the curvature of our objective function.

5.1 Structural Estimates and Model Fit

Table 3 shows the set of estimation results described in Section 4.3 that vary the distributional assumptions for T. The sample includes the 1314 farmers who made a take up decision.[33] Panel A shows estimates with $T_i \sim \text{uniform}(0, b]$. This model generates estimates for b of 58.531 (s.e. 1.877) trees. The mean of the scale parameter, μ_F is 39.423 (s.e. 5.153), with a standard deviation on the known component of F (σ_{F0}) of 283.730 (s.e. 5.208) and a standard deviation on the unknown component of F (σ_{F1}) of 163.308 (s.e. 1.802).[34]

In Panel B, T is distributed log-normal. The estimated μ_T is 2.932 (s.e. 0.026) and the estimated σ_T is 1.165 (0.019). These coefficients imply that T has a mean of 36.952, a median of 18.758 and a standard deviation of 62.720. With the log-normal distribution on T, μ_F is 32.493 (s.e. 0.536), σ_{F0} is 176.377 (s.e. 0.377) and σ_{F1} is 94.287 (s.e. 0.156). The mean of T as well as μ_F are similar under both distributional assumptions. The standard deviations for F (σ_{F0} and σ_{F1}) are smaller in Panel B, likely to compensate for the more disperse nature of T when distributed log-normal with the given parameters.

We examine model fit by comparing the reduced form treatment effects using the simulated and the observed data, restricting the sample to be the same for both. Panel A of Table 4 shows results with the observed data, and Panels B and C the results with the simulated data under an assumption of $T_i \sim \text{uniform}(0, b]$ and $T_i \sim \text{log-normal}(\mu_T, \sigma_T)$, respectively. Columns 1-4 estimate the effect of a thousand ZMK increase in the input subsidy on take up and tree survival outcomes. Columns 5-8 repeat the regressions with the reward (in '000 ZMK) on the righthand side. Columns 2-4 and 6-8 are conditional on take up. The effect of the input subsidy on take up (column 1) and the effect of the reward on all outcomes (columns 5-8) show similar coefficients for the observed and simulated datasets. The model appears to be worse at predicting the effect of the input subsidy on tree survival (columns 2-4). Namely, the model predicts some self-selection on the input subsidy, such that higher subsidies result in lower tree survival among participating farmers, which we do not observe in the data.[35]

[33] Of the 1317 farmers who made take up decisions, three are missing reward values and are not included in this estimation sample. The sample further differs from that used in the reduced form analysis, where the inclusions of controls from the baseline survey lowered the sample size to 1288.

[34] Recall that farmers in the surprise reward treatment made a take up decision before learning their reward. We model this aspect of the design by allowing the surprise reward treatment to have an independent effect on the participation decision. When T is distributed uniform, we estimate that the surprise reward treatment affects the scale of the expected profit at -55.669 (se. 2.896). We also allow the regular visits to collect data on program implementation that were administered to one-fifth of farmers to independently affect the tree survival decision (but not the participation decision). The estimated effect is -140.033 (s.e. 6.294). Both of these coefficients accompany Panel A of Table 3 and are in thousand ZMK. Results are similar when T is distributed log-normal.

[35] These selection effects are likely driven by the amount of structure imposed on heterogeneity in types. They appear to dissipate once individual heterogeneity in the means of F and T are incorporated, as will be presented in future drafts.

5.2 Program outcomes and welfare

We use estimates from the structural model to simulate program outcomes (participation and tree survival) and welfare under different scenarios. For these analyses, we use the results from the model with a uniform distribution of T. Results for alternate models are qualitatively similar.

Program participation and tree survival

We begin with the effect of the two main experimental parameters, the input subsidy and the performance reward, on participation (take up) and tree survival as the subsidy and reward are allowed to vary over a wider range than observed in our experimental setting. Notice that the feasible range for variation in the input subsidy – bounded by a full subsidy and the market price – is much smaller than the range for variation in the performance reward. Panels A and B of Figure 5 shows the simulated effect on the percent of farmers who take up and who comply with the contract (keep 70 percent of the trees alive for one year), as the reward is varied (Panel A) and the input subsidy is varied (Panel B). Participation and compliance are monotonically increasing in the size of the reward for all levels of input subsidy, with participation approaching 100 percent as the reward becomes large or the subsidy covers the full cost of program participation.

Panels C and D of Figure 5 show the effect of the reward and the input subsidy on tree survival, where the black lines are unconditional and the red lines are conditional on participation. Like in Panels A and B, the effect of the input subsidy is decreasing in the reward and the effect of the reward on tree survival is monotonically increasing over the simulated range, though it flattens considerably around 600,000 ZMK. The overlapping black lines in Panel C indicate that the participation effect of the input subsidy is offset by the selection for farmers with lower tree survival outcomes, on average. Recall that these selection effects are not detected in the experimental data.

For the range of rewards (0 to 150,000 ZMK) and input subsidies (0 to 12,000 ZMK) offered in the experiment, Figure 5 makes clear that there is not an interior optimal combination of subsidies and rewards. Specifically, tree survival is highest at the maximum reward value, and while conditional on participation, tree survival is higher when input subsidies are small (zero), this selection effect is offset by higher rates of participation. The upper bound on the reward shown in these figures is large, equal to about USD 200, or around one-third of annual income for farmers in the sample.

Welfare set up

Next, we consider the social benefits of the program by accounting for private and public costs and benefits in a single welfare function. Denote α as the social value of carbon, η as the marginal cost of public funds, and m as the monitoring cost. The average welfare per farmer conditional on reward R and subsidy A is thus

$$W(A,R) = \mathbb{E}_{F_{0i}}\left[\Pr(Part_i|F_{0i};A,R)\left(B(F_{0i};A,R) - C(F_{0i};A,R)\right)\right] \tag{12}$$

where benefits are equal to the sum of gross public gain and discounted expected private profit,

$$B(F_{0i};A,R) = \alpha \times \mathbb{E}_{T_i,F_{1i}|F_{0i}}\left[N^{**}(T_i,F_{0i},F_{1i};R)\right] + \delta\mathbb{E}_{T_i,F_{1i}|F_{0i}}\left[\Pi_R\left(N^{**}\left(T_i,F_{0i},F_{1i};R\right)\right)\right],$$

costs are equal to the sum of the reward expenditures and implementation costs

$$C(F_{0i};A,R) = R \times \Pr\left(N^{**}\left(T_i,F_{0i},F_{1i};R\right) \geq \bar{N}\middle| F_{0i};R\right) \times (\eta) + \left[A + m \times \mathbf{1}(R > 0)\right] \times (\eta - 1)$$

and f(F_{0i}) is the marginal distribution of F_{0i}.

Note that a cost of public funds is incurred for R (ηR) conditional on compliance (i.e. $N^{**} > \bar{N}$), while for the subsidy $((\eta-1)A)$, it is incurred for all participants. This makes R an attractive policy instrument relative to A in the presence of uncertainty, provided that monitoring costs are small. Monitoring costs, m, make R less attractive because monitoring costs must be incurred for all participating farmers. At the same time, R will increase the participation rate both through its effect on expected payoffs and through its option value. The option value is increasing in the uncertainty adopters face. Hence, R further increases the private component of welfare in the presence of uncertainty.

Using our model estimates, we disaggregate the average private profit and total welfare associated with the contract, as well as the heterogeneity in private profit, as the contract parameters are varied. We also examine how the results change with the level of uncertainty and with the cost of monitoring performance. Unless otherwise noted, we assume that the social value of carbon is 21 USD / ton and use the carbon sequestration estimates associated with the *Faidherbia* growth curves shown in Appendix figure A.1, discounted at 15 percent for a value of 48,000 ZMK per tree. We set the cost of monitoring at 35,000 ZMK per farmer, based on administrative cost estimates, the value of η at 1.2 and the farmer's discount factor at 0.6.[36]

Welfare results

Figure 6 shows average per farmer social welfare and private profits. Welfare, which includes both the social value of the trees and the private profits to the farmer, is increasing in the reward up to a maximum just below 600,000 ZMK after which it falls (Panel A). This maximum is driven in part by the flattening off of tree survival outcomes at around 600,000 ZMK as shown in Figure 5. For low rewards (dashed line), welfare is increasing in the subsidy, because of its effect on participation,

[36]See Footnotes 11 and 21 for discussion of this parameter value. In future drafts, we will test that the results are robust to the choice of discount parameter.

but when rewards are high (solid) line, welfare does not vary with the subsidy (Panel B). Recall that when the subsidy is 12,000 ZMK, participation is 100 percent, so a higher subsidy is simply a transfer, which lowers welfare because of $\eta > 1$. Panels C and D focus on the private profit to the farmer in thousand ZMK, and shows profits both with (black lines) and without the costless exit at $t = 1$ (red line, labeled static). The vertical difference between these lines therefore reflects the option value that the contract provides to the farmer. Panel C shows clearly that private profit is increasing in the size of the reward and the value of the input subsidy. The option value, though hard to observe directly from the figure, increases to a reward level of around 200,000 ZMK, after which it begins to fall. As the reward becomes large, the likelihood of compliance under all shock realizations increases, so the value of costless exit diminishes. This is more clearly visible in Panel D. Together, the figure shows convexity in the private profit and the concavity in social welfare as a function of the reward, which indicates that the share of the social value that is in private profit is increasing in the reward.

The average private profit masks a substantial amount of heterogeneity in both expected profits and the option value. We calculate the discounted expected profit using normal kernel density smoothing. Note that resulting negative expected profits are an artifact of the kernel smoothing normality assumption. Results are shown in Panel A of Figure 7, for an input subsidy of zero ZMK at two values of the reward. Most farmers have low expected profits. When the reward is increased from zero (lower bound of the experimental variation) to 150,000 ZMK (upper bound of the experimental variation), the mass at the mode of around 12,000 ZMK is shifted rightward across the right tail of the distribution. This change in the distribution of expected profit when the reward increases is consistent with the role that the reward plays in overcoming scale costs associated with the contract. Panel B of the figure shows the share of the expected profit that comes from option value, where each point represents a farmer's simulated profit level. At very low levels of expected profit, the entire contract value depends on the option to costlessly exit after uncertainty is revealed. Note that the individuals for whom the option value is 100 percent of expected profits, meaning entire value of the contract depends on the ability to postpone the follow through decision, fall at the low end of expected profits, and are therefore most likely to have take up and follow through decisions that change due to shocks or a change in contract incentives.

To investigate the effect of uncertainty on program outcomes and welfare, we repeat the graphs in Figures 5 and 6, varying the amount of uncertainty (σ_{F1}) faced by the farmer at the time of the participation decision. Figure 8 shows the results with σ_{F1} on the x-axis. At low levels of uncertainty, compliance among participants is very high and the subsidy has a strong selection effect (Panel A). Both of these results are eroded by uncertainty. Specifically, participation is increasing in uncertainty for any positive input cost, while compliance is falling. The participation and compliance results offset each other in expectation (Panel B): more individuals join at higher levels of uncertainty but produce fewer trees on average (black lines). Note that this is context-specific, but a result driven by our data rather than specific parameter choices. Welfare is also largely unchanged as uncertainty increases (Panel C). However, at higher levels of uncertainty, the social cost per tree is higher, since it takes more farmers to produce the same number of trees. These social losses are offset by the option value transferred to the farmer, which is increasing in uncertainty as shown in Panel D. In this context, the effect of increasing uncertainty is to transfer value from the public to private profit.

We vary the assumed costs of monitoring tree survival in Figure 9 to examine how the welfare gains from rewarding outcomes is affected by monitoring cost. Panel A shows the welfare over the full range of simulated rewards. Welfare is lower at higher levels of monitoring costs, but still positive throughout. Panel B shows the range of rewards observed in the experiment. The dots correspond to welfare at zero rewards, so when no monitoring is necessary. These are substantially above the welfare levels achieved at low reward levels with modest or high monitoring costs. In the absence of rewards, subsidizing inputs is preferable. However, at higher monitoring costs, the participation effect of the subsidy comes at the cost of monitoring the extra participants, causing the welfare gains of the input subsidy to fall in the monitoring cost.

6 Alternative explanations

In this section, we discuss three potential explanations that are consistent our empirical results, but are not explicitly captured by our model: common shocks, procrastination and learning.

6.1 Common and idiosyncratic shocks

By interpreting the ex post variance in profits across farmers as the sum of the variances of the known and unknown components of the private profit, the current version of our model allows for idiosyncratic but not common shocks. In all likelihood, farmers face both common and idiosyncratic shocks and to the extent that the former are important, it will generate downwards bias in the variance parameter that describes the unobserved shocks.[37] We consider two sources of information about the relative importance of common and idiosyncratic shocks: household self-reports from the baseline and endline data and the existing literature on agricultural productivity in rural Sub-Saharan Africa.

[37] The assumption that ex post variance in profits is the sum of the variances of the known and unknown components of the profits is equivalent to assuming that shocks are uncorrelated across farmers. A positive correlation in the shocks across farmers will result in a lower ex post variance. Hence, the model will assume that the residual variance (the leftover variance once the variance in known shocks is subtracted) corresponds to the variance of the unknown shocks. This restriction will cause an underestimation of the variance in the unknown component of the shocks. As described in Section 4.3, we can extend our current estimation to allow for a non-zero μ_{F1}. Though not shown, preliminary results indicate that μ_{F1} is positive, consistent with negative shocks to the mean profitability of the contract after take up. This result is consistent with either a common shock, or with commonly held incorrect beliefs or learning. We cannot distinguish among these interpretations of μ_{F1}, but allowing for both learning and common shocks in the estimation addresses the current concern about bias in the variance parameters.

First, the two most often discussed common shocks are weather and prices. In our setting, the primary source of price risk is from cotton, which experienced a very negative shock in the year of study. At baseline, 97 percent of respondents who forecasted a minimum cotton price for the coming year that exceeded the realized price. Close to 80 percent of the households in our study grew cotton in the contract year, and were therefore affected by the price shock. However, the share of a household's agricultural land under cotton ranges from zero to 100 percent, with a mean of 34 percent and a standard deviation of 26 percent. Exposure to cotton price risk therefore varies substantially across households, resulting in a sizable farmer-specific component to this shock.

Weather represents the other likely source of common risk exposure. Unlike cotton prices, rainfall was fairly typical during the year of the contract, according to local agronomists. When asked about the greatest challenges facing their households, two-thirds of respondents indicated health problems, which are likely to arrive as idiosyncratic shocks, while only 2 percent indicated weather or rainfall. Further evidence of unexpected shocks are reported at endline, with almost 50 percent of households reporting lost cattle or livestock to death or theft during the past year and 10 percent of households reporting the death or marriage of a working age member.

Second, a growing literature documents large negative effects of household illness on agricultural productivity in the region of study (Fink and Masiye 2012; Baranov et al. 2013) and on consumption more broadly (for example, Gertler and Gruber 2002). These findings are consistent with a larger literature that documents, in most cases, a disproportionate share of income risk from idiosyncratic factors in rural developing country settings (summarized in Dercon 2002). Thus, while common shocks are potentially an important source of uncertainty in agriculture, our focus on idiosyncratic shocks appears reasonable in this context.

6.2 Procrastination

An alternative explanation for the observed pattern of high take up and low compliance is procrastination or hyperbolic time preferences. Sustained effort choices are frequently associated with time inconsistent behavior, in which the individuals initially takes up, intending to follow through. But when the time comes to act, costs loom larger (or benefits smaller) than anticipated at the time of initial adoption. Mahajan and Tarozzi (2011) document time inconsistent technology adoption for insecticide treated bednets in India, with low rates of net re-treatment.

We examine evidence for procrastination or hyperbolic time preferences, by constructing two measures of procrastination from the survey data. The first of these relies on a series of baseline survey questions about the respondent's tendency to spend money quickly or delay purchases or actions. These questions are combined into a binary measure of procrastination, which is likely to miss naive procrastinators.[38] As part of the endline survey, a series of questions about procrasti-

[38]Specifically, the baseline questions prompted (1) If I get money, I tend to spend it too quickly, (2) I often change my mind and do not follow through with my original intention and (3) I tend to postpone activities until later. All

nation in other activities, including paying school fees, purchasing agricultural inputs and milling maize were added to the survey. These are combined into a second binary measure, which is more likely to capture naive procrastinators.

We begin by examining whether these measures of procrastination are correlated with contract take up or tree survival conditional on take up, controlling for other characteristics. They are not. We next investigate the insight that farmers prone to procrastination may be differentially sensitive to a contract structure that requires them to pay more upfront for inputs if the potential rewards arrive only after a year of effort. We regress take up on an interaction of each of the two procrastination measures and the input subsidy level. For the self-described procrastinators, there is a weakly greater likelihood of take up at higher subsidy levels. However, the interaction is insignificant with the measure more likely to capture naive procrastinators. These results are summarized in Appendix table A.3, and suggest a relatively minor role for procrastination in driving take up or contract compliance outcomes.

6.3 Learning

We interpret the model as one of intertemporal shocks to opportunity cost. If, instead, farmers are simply unaware of the costs and benefits of the trees ex ante, then learn about the technology during the first year of implementation, we may misattribute heterogeneous learning to the arrival of an idiosyncratic shock.[39] While we cannot explicitly rule out heterogeneous learning as an explanation for our empirical findings, we can explore the extent to which knowledge of the technology changes over the course of the year-long contract.

Our most reliable measure of knowledge about the technology is the number of risks to tree survival that the farmer is able to list. Out of four major risks to the trees (see Section 2.3), farmers list an average of 1.64 risks at baseline and 1.75 at endline one year later. Farmers who took up the contract show a larger increase in knowledge than those who did not take up. This is driven both by actual increases in knowledge by some adopters and high rates of forgetting by non-adopters. Conditional on take up, tree survival is also higher among those who had planted *Faidherbia* at baseline and presumably had less to learn about the technology. These correlations should not be interpreted as causal, but do suggest that some of the information that arrives after take up may be in the form of learning about the technology.

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responses were on a four point Likert item from strongly disagree to strongly agree. A binary measure was constructed to equal one if the respondent agreed or strongly agreed with any of the statements.

[39] To the extent that learning follows a common pattern or incorrect beliefs are commonly held at the time of the participation decision, this would be captured in a non-zero mean of F_1. As described in Section 4.3 and footnote 37, preliminary estimates of μ_{F1} are consistent with average incorrect beliefs about the cost of the technology.

7 Conclusion

We present the findings from a field experiment that investigates the determinants of adoption of a technology that generates positive externalities. In Zambia, farmers decide whether to adopt a nitrogen fixing tree under considerable uncertainty about the costs of implementation. The study introduces exogenous variation into the initial subsidy for inputs and the size of a reward conditioned on tree survival. We observe a weak correspondence between the initial decision to take up and the the decision to follow through, which does not systematically vary across subsidy conditions. At the same time, farmers are responsive to the size of the reward in their decision to keep trees alive. Results are consistent with the arrival of shocks to the cost of implementation after the farmer has decided whether to participate in the project.

The experimental variation is used to identify a structural model of intertemporal decision making under uncertainty, which explains our field results and quantifies the uncertainty that the farmers face at the time of take up. The model highlights several key tradeoffs associated with incentives for technology adoption under uncertainty. First, uncertainty increases take up but decreases the average follow through of participants. Performance rewards can offset some of the negative effect of uncertainty on performance but also attract more participants, all of whom require potentially costly monitoring. Second, while uncertainty increases the investment needed to produce an additional tree, it makes up for these costs by providing an unconditional option value to the farmer, who can postpone the decision to produce trees until after cost shocks are realized. This option value is large, and for the subset of farmer who are sensitive to the feasible variation in the input subsidy, it constitutes 100 percent of the expected profit from the program. In this context, uncertainty has the effect of shifting the welfare benefits of the program from the public to the private domain. Third, when it is costly to monitor outcomes, welfare is higher when rewards are set to zero and inputs are subsidized. However, for positive reward levels, input subsidies have the effect of increasing participation and therefore the number of individuals subject to costly monitoring.

It is important to note that rewards have a much larger potential effect on welfare both because they can be increased far beyond the feasible range for input subsidies – participation plateaus at 100 percent if the subsidy exceeds the price of the input (farmers are paid to join). For inexpensive technologies, this may correspond to a relatively small incentive. Like all empirical case studies, our data are specific to our setting. However, the combination of the experimental data with a structural model allows us to escape some of these limitations through out of sample simulations. Future drafts of the paper will both extend these simulations and explore a richer modeling framework to account for additional farmer-level heterogeneity.

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<table>
<tr><td colspan="2" rowspan="2"></td><td colspan="4">Group level: Input subsidy (A)
(Cost recovery price = 12,000 ZMK)</td></tr>
<tr><td>A = 12,000
(full subsidy)</td><td>A = 8,000</td><td>A = 4,000</td><td>A = 0
(full price)</td></tr>
<tr><td rowspan="2">Individual level:
Take up timing</td><td>Draw reward
then elicit take up</td><td colspan="4" rowspan="2">Individual level:
Randomly vary R (continuous) between 0 and 150,000 ZMK</td></tr>
<tr><td>Elicit take up
then draw reward
(surprise reward
treatment)</td></tr>
</table>

Figure 1: Experimental design

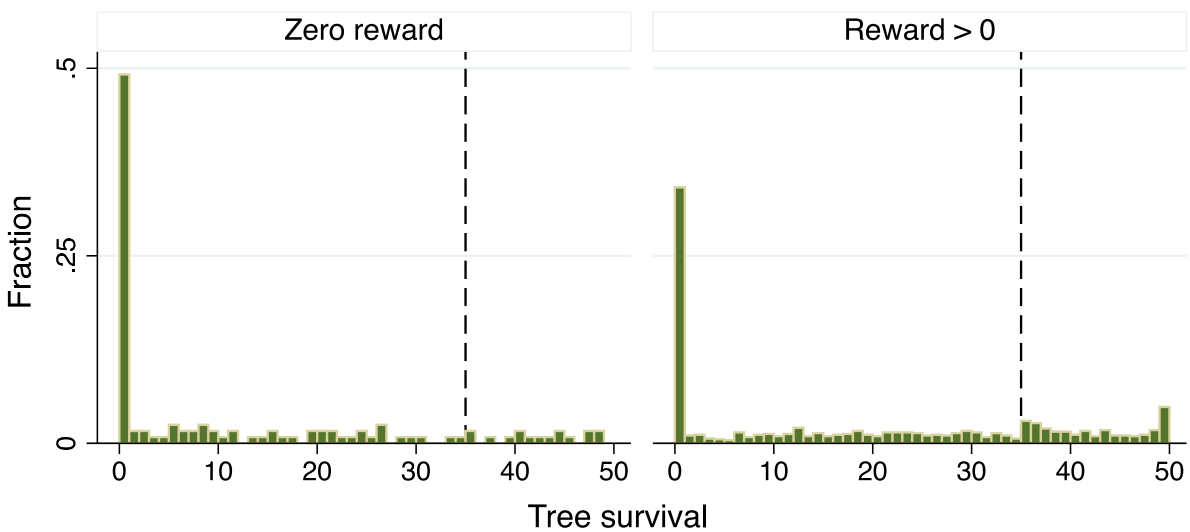


Figure 2: Histogram of tree survival at zero and positive reward levels

Notes: Histogram of tree survival outcomes, by zero (N=122) and positive reward (N=970) levels. Tree survival outcomes are conditional on take up. The vertical dashed line shows the performance threshold for the reward.

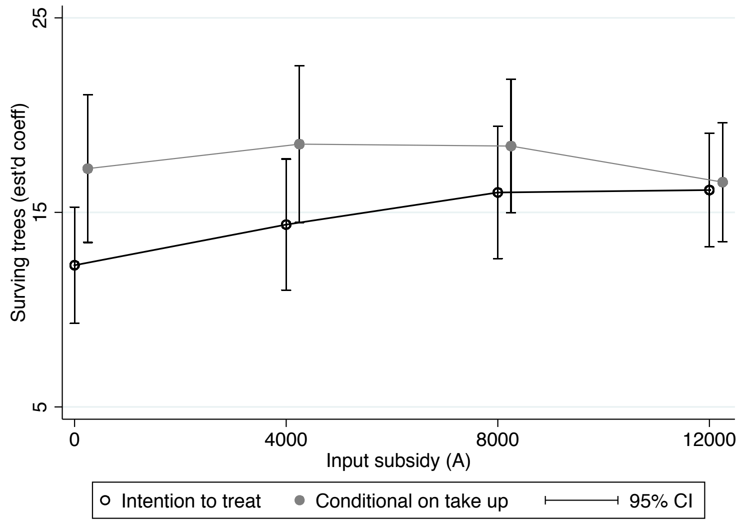


Figure 3: Effect of self-selection on tree survival, by input subsidy treatment

Notes: Coefficients and clustered standard errors from an OLS regression of tree survival on input subsidy categories (ZMK), controlling for treatment and individual covariates. Circles show estimated coefficients for all farmers, replacing outcomes with zeros for non-participants. The gray lines shows estimated coefficients conditional on take up, and represents the test for selection on input subsidy level.

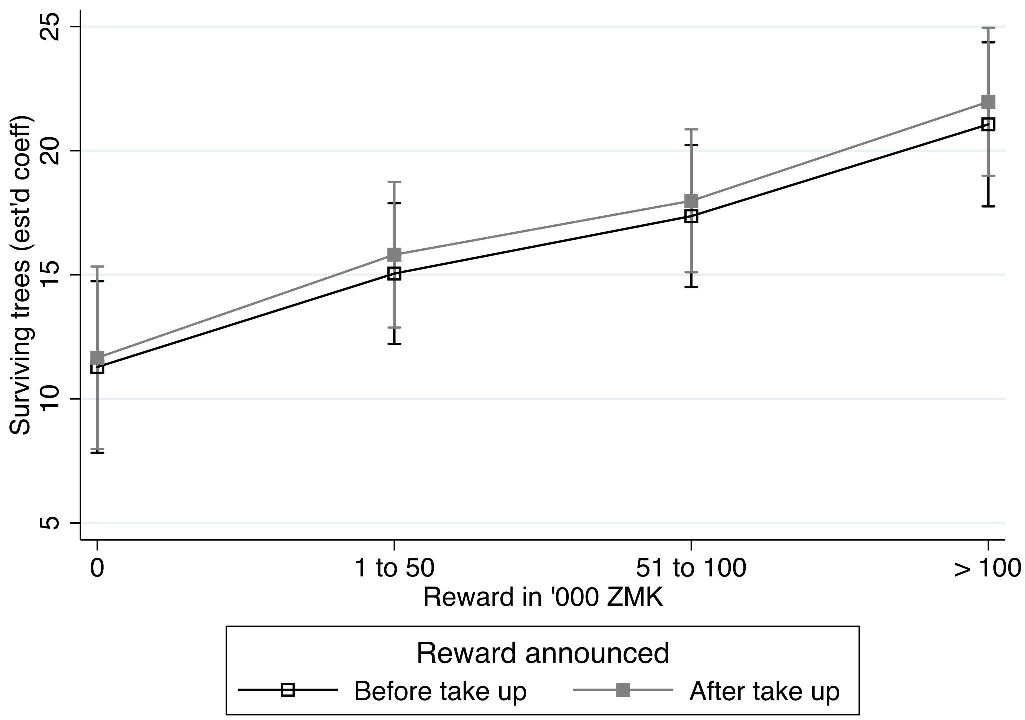
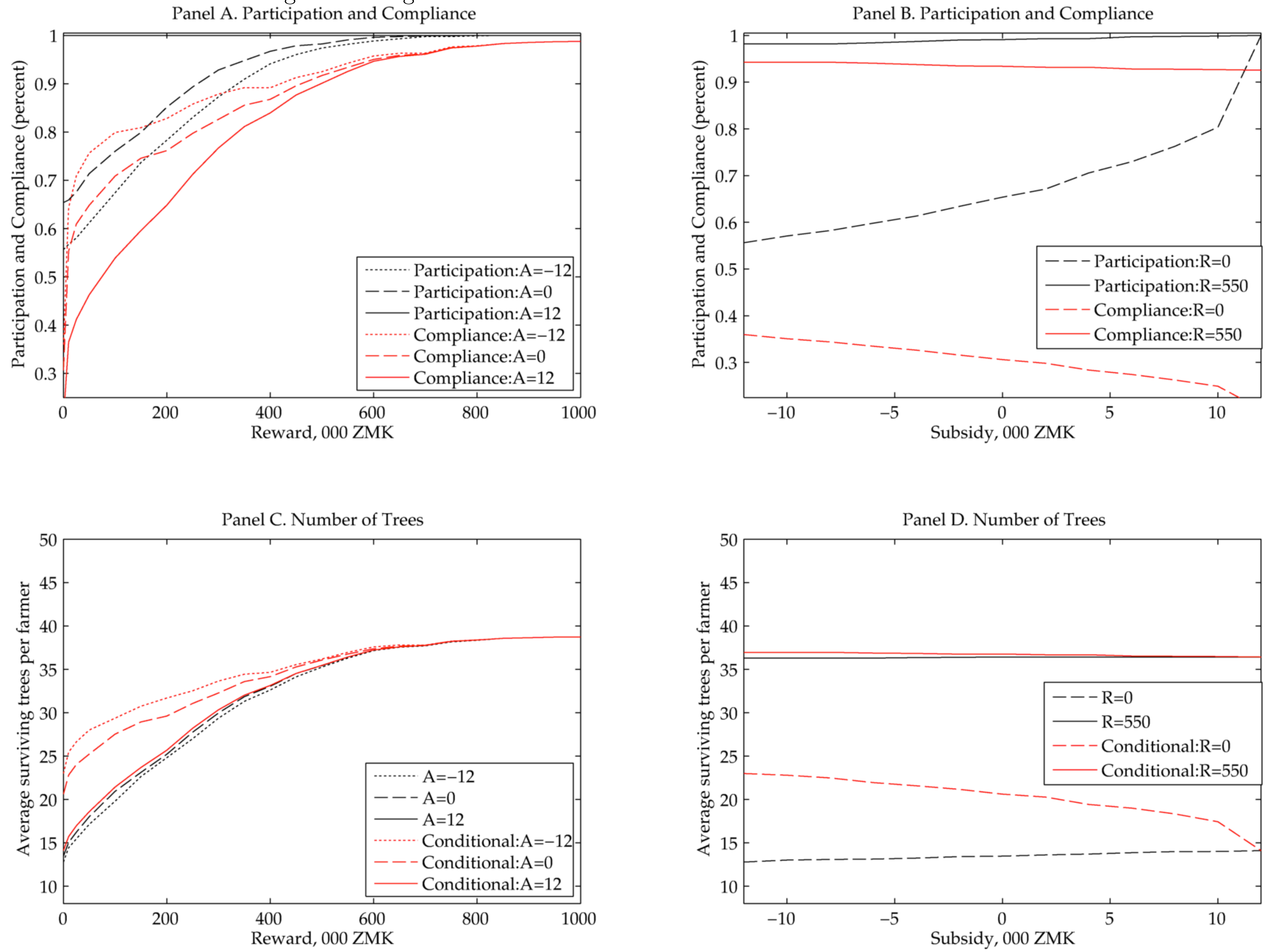


Figure 4: Effect of self-selection on survival, by reward timing treatment

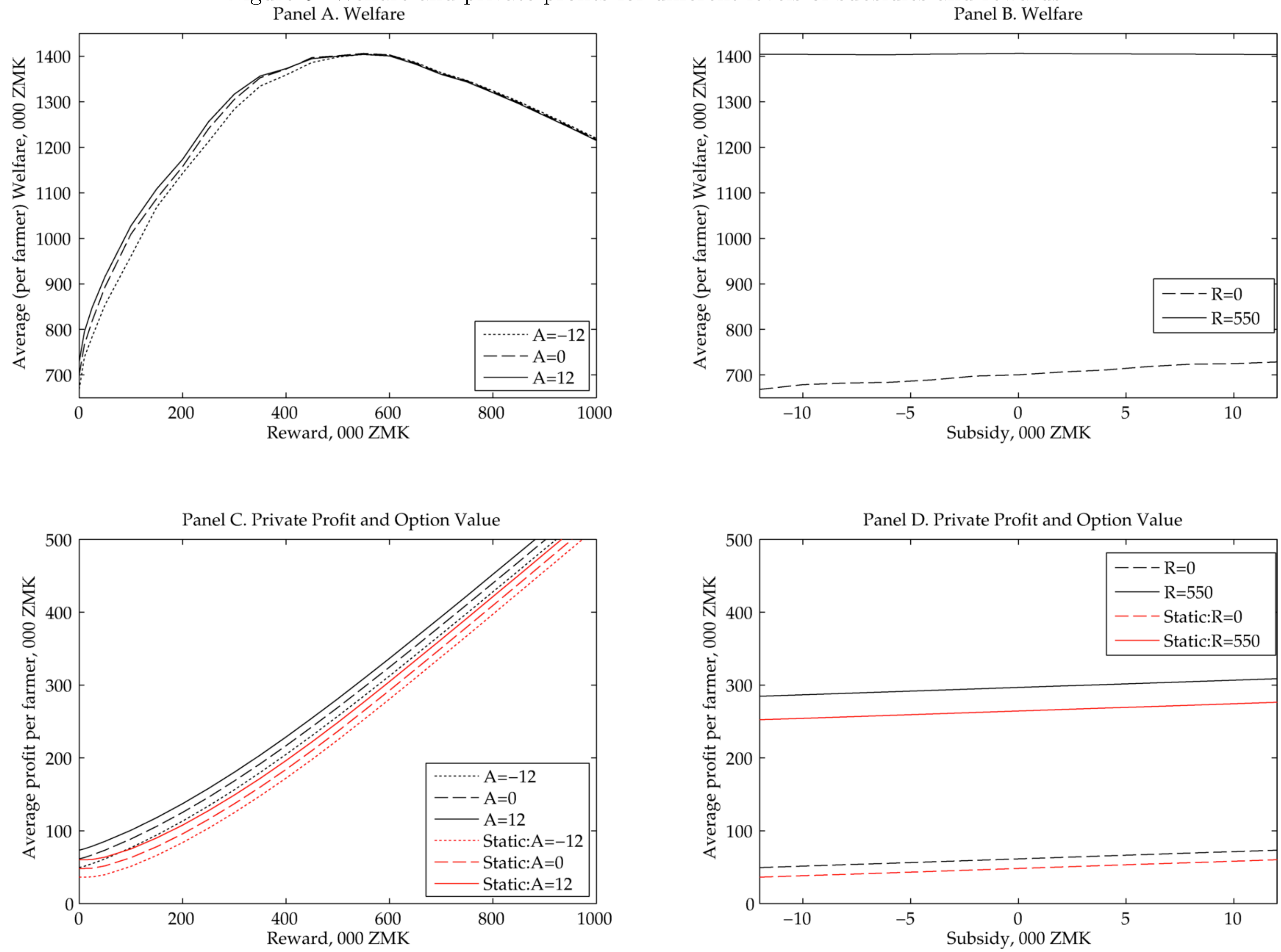
Notes: Figure plots estimated coefficients from an OLS regression tree survival on reward categories interacted with the surprise reward treatment, conditional on take up and controlling for individual covariates. The black line plots coefficients for farmers who were informed of their reward before take up. The gray line plots coefficients for farmers unaware of the reward at the time of take up (surprise reward treatment). Error bars show the 95 percent confidence interval.

Figure 5: Program outcomes for different levels of subsidies and rewards



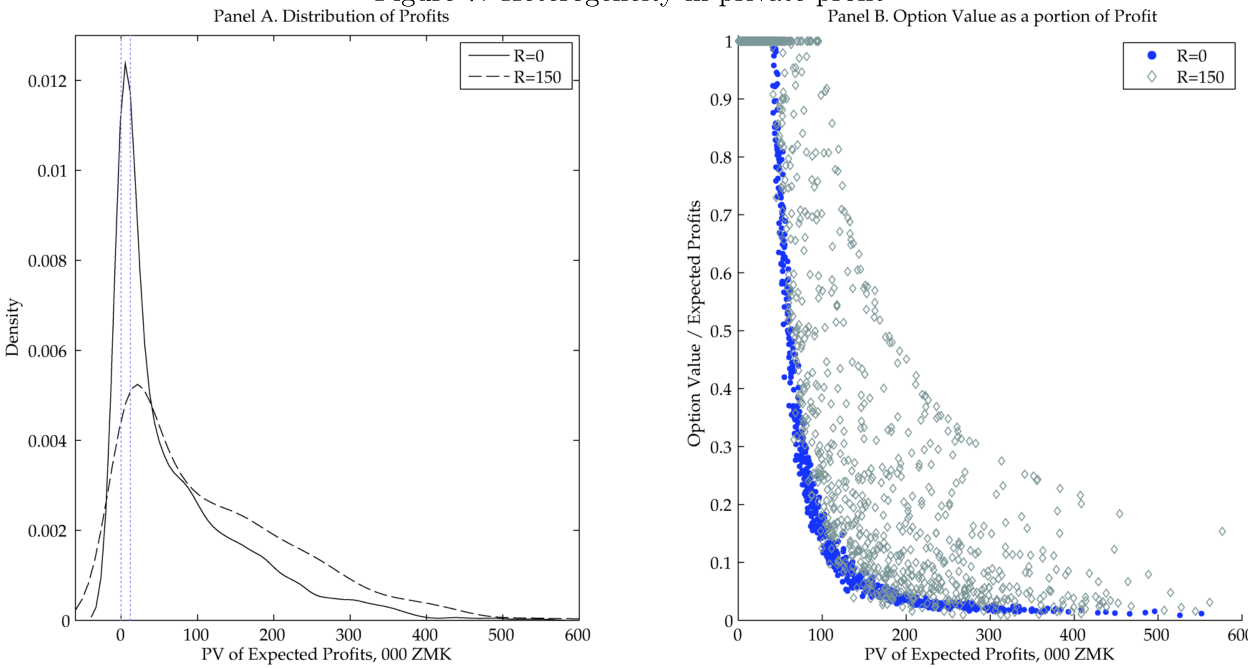
Notes: Panels A and B show the simulated percentage of participation (take up) and compliance (achieving 70% tree survival after one year). Panels C and D show the average per farmer number of trees surviving after one year. Lines labeled conditional show the average per farmer number of trees surviving conditional on taking up. Panels A and C are shown as functions of performance reward, while Panels B and D are shown as functions of input subsidy. Participation, compliance, and tree survival results simulated using Panel A of Table 3, i.e. with uniformly distributed T.

Figure 6: Welfare and private profits for different levels of subsidies and rewards



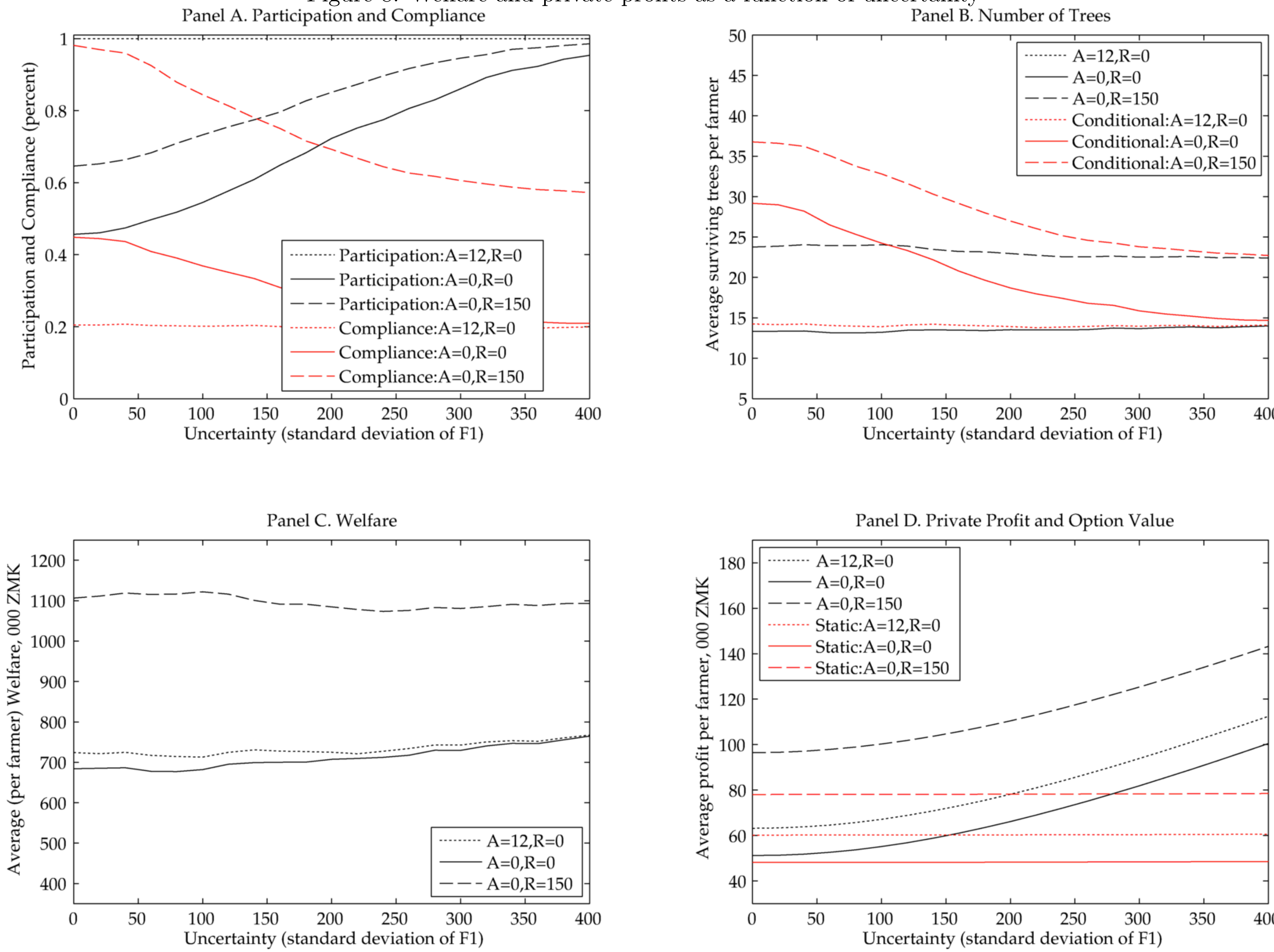
Notes: Panels A and B show the simulated average per farmer welfare in '000 ZMK. Panels C and D show the average per farmer private profit in '000 ZMK, where the lines labeled static describe a contract with commitment to comply. Option value is the vertical distance between the a given line and its static counterpart. Panels A and C are shown as functions of performance reward, while Panels B and D are shown as functions of input subsidy. Welfare and private profit results simulated using Panel A of Table 3, i.e. with uniformly distributed T.

Figure 7: Heterogeneity in private profit



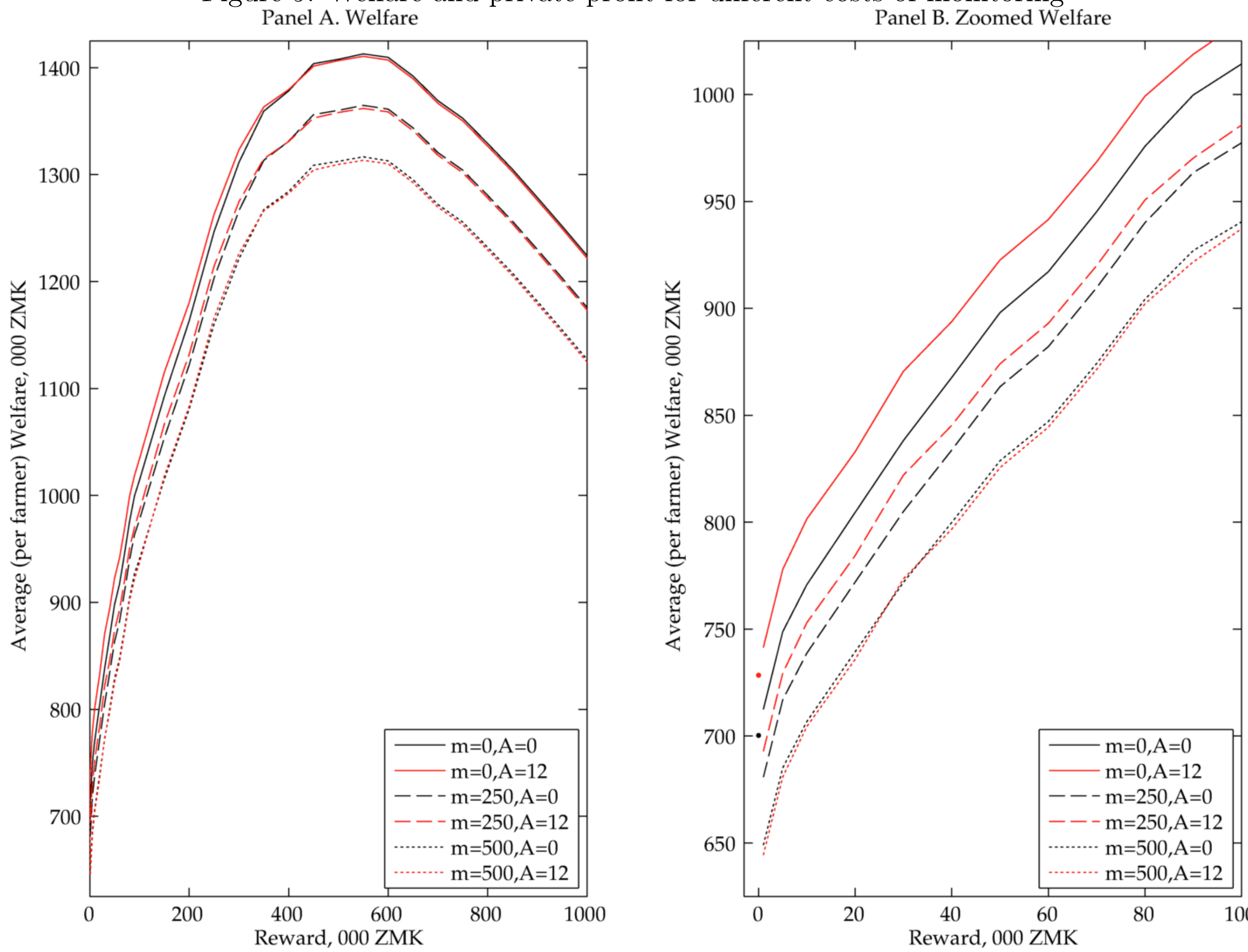
Notes: Panel A shows the distribution of discounted expected profits calculated using normal kernel smoothing using optimal bandwidth for normally distributed data (between 20 and 50 for the simulated data). Vertical lines at 0 and 12 cover the range of (undiscounted) subsidy values used in the experiment. Panel B shows the share of expected profit that comes from the option value of the contract as a function of profit. Each point represents an agent's simulated profit level. Reward given in '000 ZMK. The discount factor is 0.6. Profits simulated using Panel A of Table 3, i.e. with uniformly distributed T. Both panels contain only expected profit up to 600,000 ZMK, all larger values are not shown. Negative values in Panel A are an artifact of the kernel smoothing.

Figure 8: Welfare and private profits as a function of uncertainty



Notes: The horizontal axis varies the uncertainty faced by agents when deciding to participate (i.e. the standard deviation of F1, which is in '000 ZMK). Panel A shows simulated percentage of participation and compliance. Panel B shows the average per farmer number of trees surviving after one year. Commitment gives the average per farmer number of trees surviving conditional on taking up. Panel C shows the simulated average per farmer welfare in '000 ZMK. Panel D shows average per farmer private profit in '000 ZMK, where the lines labeled static describe a contract with commitment to follow through. Option value is the vertical distance between the a given line and its static counterpart. All results simulated using Panel A of Table 3, i.e. with uniformly distributed T.

Figure 9: Welfare and private profit for different costs of monitoring



Notes: Both panels give welfare in '000 ZMK as a function of performance reward. Panel A shows the reward from 0 to 1,000,000 ZMK, whereas Panel B zooms in on the interval from 0 to 100,000 ZMK and rescales the welfare axis. There is a discontinuity in welfare due to monitoring costs at a reward level of 0, as indicated by the points in Panel B. All results simulated using Panel A of Table 3, i.e. with uniformly distributed T.

Table 1: Effect of treatments on contract take up

Dependent variable is program take up	(1)	(2)	(3)	(4)
Input subsidy (A) = 4000	0.0632	0.0177	-0.0381	
	[0.0703]	[0.0759]	[0.1250]	
Input subsidy (A) = 8000	0.1600**	0.1525**	0.2500**	
	[0.0645]	[0.0685]	[0.1044]	
Input subsidy (A) = 12000	0.2629***	0.2476***	0.3704***	
	[0.0554]	[0.0575]	[0.0846]	
Reward ('000 ZMK)		0.0007**	0.0014**	0.0005*
		[0.0003]	[0.0007]	[0.0003]
A=4000 X Reward			0.0007	
			[0.0010]	
A=8000 X Reward			-0.0014*	
			[0.0008]	
A=12000 X Reward			-0.0018**	
			[0.0007]	
Group Fixed Effects	No	No	No	Yes
Individual Controls	Yes	Yes	Yes	Yes
# obs	1288	611	611	611
Dependent variable mean	0.839	0.849	0.849	0.849

Notes: OLS regressions of the binary take up outcome on program treatments, with standard errors clustered at the group level. Column 1 includes all baseline respondents. Columns 2-4 exclude participants in the surprise reward treatment. * $p<0.10$ ** $p<0.05$ *** $p<0.01$.

Table 2: Effect of rewards on tree survival

	# trees		1.(Zero trees)		1.(trees ≥ threshold)		# trees > 35	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Conditional on take up, surprise reward treatment only								
Reward in '000 ZMK	0.0688***		-0.0018***		0.0012***		-0.0018	
	[0.0146]		[0.0004]		[0.0004]		[0.0077]	
Reward > 0		6.9389***		-0.1967***		0.1064**		-0.2882
		[1.9702]		[0.0600]		[0.0498]		[1.1632]
Dep var mean, R = 0	11.3182		0.5303		0.1515		43.0000	
N	561		561		561		126	
Panel B. All farmers, ITT								
Reward in '000 ZMK	0.0633***		-0.0015***		0.0012***		-0.0014	
	[0.0105]		[0.0003]		[0.0003]		[0.0062]	
Reward > 0		6.5004***		-0.1420***		0.1210***		0.1025
		[1.3462]		[0.0430]		[0.0334]		[1.2854]
Dep var mean, R = 0	10.1791		0.5373		0.1194		43.2857	
N	1288		1288		1288		242	

Notes: OLS regressions of tree survival outcomes on reward conditions. Standard errors in brackets are clustered at the group level. Panel A shows results for self-selected farmers (known reward treatment), conditional on take up. Panel B shows all farmers, with tree survival outcomes equal to zero for non-participants. All regressions include controls for input cost treatment, monitoring condition, along with the full set of individual controls shown in Table A.1. Dependent variables are the continuous number of surviving trees (1 and 2), an indicator for zero trees (3 and 4), an indicator for tree survival at or above the threshold (5 and 6), and the continuous number of surviving trees conditional on being above the threshold (7 and 8). * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$.

Table 3: Structural parameter estimates

Parameters in T			Parameters in F		
b	mu_T	sd_T	mu_F	sd_F0	sd_F1
Panel A. Uniform Distribution					
58.531			39.423	283.730	163.308
(1.877)			(5.153)	(5.208)	(1.802)
Panel B. Log-Normal Distribution					
	2.932	1.165	32.493	176.377	94.287
	(0.026)	(0.019)	(0.536)	(0.377)	(0.156)

Notes: Parameters fitted by simulated maximum likelihood, with smoothing (lambda is 0.5) and tolerance (1e-5). In Panel A, T is distributed uniformly on the interval (0,b). In Panel B, T is distributed log-normal with parameters mu_T and sd_T. T distributed log normal with the given parameters has mean 36.952, median 18.758, mode 4.833, and standard deviation 62.720. The estimated models include two parameters not shown in the table: the first is the effect of the surprise reward treatment on take up (F_surp), and the second is the effect of regular data collection visits on the tree survival decision (F_moni). Standard errors are given in parentheses and are the square root of the diagonal entries of the inverse Hessian from the likelihood.

Table 4: Comparison of structural and reduced form estimates

	(1) Take up	(2) # trees	(3) 1.(trees ≥ threshold)	(4) 1.(zero trees)	(5) Take up	(6) # trees	(7) 1.(trees ≥ threshold)	(8) 1.(zero trees)
Panel A. Observed Data								
Input Subsidy in 000 ZMK	0.022***	-0.075	-0.004	-0.003				
	(0.002)	(0.117)	(0.003)	(0.003)				
Reward in 000 ZMK					0.002***	0.065***	0.001***	-0.001***
					(0.000)	(0.011)	(0.000)	(0.000)
Observations	1314	1092	1092	1092	1314	1092	1092	1092
Panel B. Simulated Data, T ~ Uniform								
Input Subsidy in 000 ZMK	0.025***	-0.696***	-0.014***	0.018***				
	(0.002)	(0.129)	(0.003)	(0.003)				
Reward in 000 ZMK					0.000	0.068***	0.003***	-0.001***
					(0.000)	(0.012)	(0.000)	(0.000)
Observations	1314	1063	1063	1063	1314	1063	1063	1063
Panel C. Simulated Data, T ~ Log-normal								
Input Subsidy in 000 ZMK	0.030***	-0.663***	-0.013***	0.020***				
	(0.002)	(0.132)	(0.003)	(0.003)				
Reward in 000 ZMK					0.001***	0.104***	0.004***	-0.001***
					(0.000)	(0.012)	(0.000)	(0.000)
Observations	1314	1008	1008	1008	1314	1008	1008	1008

Notes: OLS regressions on input subsidy (columns 1-4) and performance reward (columns 5-8) using observed data (Panel A) and simulated data (Panels B and C). Columns 1 and 5 show coefficients and standard errors for an indicator variable for particpation. Columns 2 and 6 show coefficients and standard errors for the number of surviving trees. Columns 3 and 7 (4 and 8) show coefficients and standard errors for indicator variables representing 35 or more (exactly zero) surviving trees. *** p<0.01, ** p<0.05, * p<0.1.

Appendices

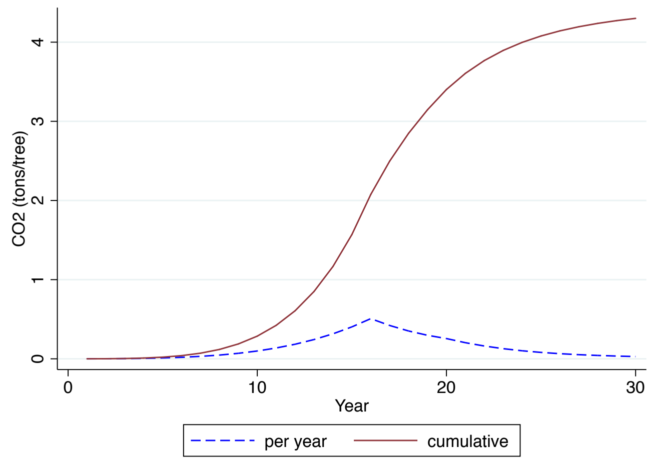


Figure A.1: Carbon sequestration associated with *Faidherbia*

Notes: Annual and cumulative CO_2 sequestration per tree over a 30 year period. Sequestration rates are based on allometric equations adapted for the biomass and growth patterns of *Faidherbia albida*.

Table A.1: Balance

	A=0 Mean [SD] (1)	A > 0 (2)	R=0 Mean [SD] (3)	Reward > 0 (4)	Surprise=0 Mean [SD] (5)	Surprise reward (6)	N (7)
Respondent is head of household	0.735	0.001	0.694	0.0000	0.702	0.038	1292
	[0.442]	[0.003]	[0.463]	[0.0003]	[0.458]	[0.025]	
Age, respondent	37.872	-0.074	37.439	0.0071	37.25	1.284*	1266
	[13.716]	[0.100]	[12.808]	[0.0073]	[13.684]	[0.709]	
Female headed household	0.135	0.001	0.149	0.0001	0.119	0.009	1292
	[0.343]	[0.003]	[0.358]	[0.0002]	[0.324]	[0.017]	
Years of education, respondent	5.342	-0.039	5.284	0.0033	5.363	-0.009	1292
	[3.276]	[0.028]	[3.133]	[0.0022]	[3.331]	[0.177]	
Household size	5.465	-0.036**	5.328	0.0003	5.246	0.208*	1292
	[2.142]	[0.015]	[2.390]	[0.0013]	[2.217]	[0.114]	
Ordinal discount rate (1 - 5)	2.454	-0.001	2.538	0.0004	2.43	0.106	1262
	[1.627]	[0.013]	[1.714]	[0.0010]	[1.612]	[0.095]	
Non-agricultural assets	9.806	-0.101**	9.343	-0.0013	9.08	0.314	1292
	[5.789]	[0.041]	[5.625]	[0.0034]	[5.506]	[0.287]	
Years working with Dunavant	4.228	-0.033	3.776	-0.0015	3.865	0.069	1292
	[3.748]	[0.035]	[3.404]	[0.0022]	[3.496]	[0.214]	
Total landholdings (hectares)	3.02	-0.022	2.881	0.0000	2.873	0.056	1290
	[2.248]	[0.023]	[2.188]	[0.0014]	[2.253]	[0.115]	
Number of fields	2.874	0.001	2.866	0.000	2.886	-0.054	1292
	[1.065]	[0.009]	[1.194]	[0.0008]	[1.122]	[0.070]	
Average distance from home to plots	20.532	-0.084	19.416	-0.0202	18.397	0.91	1292
	[24.195]	[0.236]	[22.436]	[0.0122]	[20.625]	[1.068]	
Poor soil fertility	0.108	-0.001	0.104	0.0000	0.106	-0.029*	1292
	[0.310]	[0.002]	[0.307]	[0.0002]	[0.308]	[0.017]	
Regular interaction with lead farmer	0.415	0.004	0.448	-0.0006**	0.412	0.007	1290
	[0.493]	[0.004]	[0.499]	[0.0003]	[0.493]	[0.029]	
Affiliated with CFU or COMACO	0.037	0.002	0.037	0.000	0.042	0.003	1292
	[0.189]	[0.002]	[0.190]	[0.0001]	[0.202]	[0.012]	
Prior knowledge of Faidherbia	0.68	-0.002	0.664	0.000	0.64	0.027	1292
	[0.467]	[0.004]	[0.474]	[0.0003]	[0.480]	[0.025]	
Prior planting of Faidherbia	0.111	-0.001	0.09	0.0001	0.088	0.014	1292
	[0.314]	[0.002]	[0.287]	[0.0002]	[0.283]	[0.014]	
Knowledge of risks to tree survival	1.72	-0.005	1.701	0.0000	1.648	-0.013	1292
	[0.905]	[0.006]	[0.785]	[0.0005]	[0.816]	[0.046]	
N	325	967	134	1041	614	678	

Notes: Means are reported for the base group in columns 1, 3 and 5. Coefficients and standard deviations from a regression of the household variable on treatment are reported in other columns. * p < 0.10 ** p < 0.05 *** p < 0.01.

Table A.2: Attrition across data collection phases

	Takeup Mean [SD] (1)	Baseline (2)	Endline (3)	Tree monitoring (4)
Input cost	6.1564	0.0029*	0.0000	0.0000
	[4.5399]	[0.0016]	[0.0020]	[0.0007]
Reward ('000 ZMK)	69.3347	0.0001	0.0000	0.0000
	[48.4713]	[0.0001]	[0.0001]	[0.0000]
Surprise reward treatment	0.5239	0.0097	0.0124	0.0025
	[0.4996]	[0.0088]	[0.0124]	[0.0061]
N, outcome = 1	1317	1292	1232	1083

Notes: Attrition across data collection rounds by treatment. Column 1 shows means and standard deviations for each treatment. Each cell in columns 2 - 4 shows the coefficient from a regression of an indicator being present at the data collection stage regressed on each treatment with standard errors clustered at the farmer group level. Column 4 is conditional on participating in the program (N=1092). For observations missing the reward variable (surprise reward treatment, no take up), a missing variable dummy for the reward is added to the regression. Reported coefficients are among non-missing reward values. * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$.

Table A.3: Procrastination

Dependent variable:	Takeup	Survival	Survival	Takeup	Takeup
	(1)	(2)	(3)	(4)	(5)
	Panel A: Self-described procrastinator				
Binary Procrastination Measure	-0.0080	-0.8574	-1.3361	-0.0273	-0.0016
	[0.0208]	[1.1563]	[1.1410]	[0.0440]	[0.0651]
Input subsidy in '000 ZMK	0.0221***	-0.0513	-0.0184	0.0206***	0.0233***
	[0.0044]	[0.2004]	[0.1952]	[0.0047]	[0.0058]
Reward in '000 ZMK			0.0669***		0.0007**
			[0.0114]		[0.0003]
Procrastination x Input subsidy				0.0031	-0.0028
				[0.0047]	[0.0069]
Constant	0.4507***	9.0534***	4.3205	0.4622***	0.3980***
	[0.0783]	[3.1850]	[3.1716]	[0.0804]	[0.1088]
N	1275	1071	1071	1275	603
	Panel B: Reports procrastination on other activities				
Binary Procrastination Measure	-0.0302	0.0622	0.1404	-0.0938	-0.0521
	[0.0278]	[1.2728]	[1.2767]	[0.0629]	[0.0767]
Input subsidy in '000 ZMK	0.0231***	-0.1045	-0.0745	0.0197***	0.0205***
	[0.0044]	[0.2021]	[0.1958]	[0.0043]	[0.0050]
Reward in '000 ZMK			0.0686***		0.0006*
			[0.0119]		[0.0003]
Procrastination x Input subsidy				0.0102	0.0076
				[0.0066]	[0.0080]
Constant	0.4589***	8.8880***	3.8534	0.4766***	0.4333***
	[0.0801]	[3.1334]	[3.1569]	[0.0806]	[0.1014]
N	1223	1030	1030	1223	576

Notes: OLS regressions of take up and survival on indicators of procrastination. Standard errors clustered at the group level are in brackets. Columns 2 and 3 condition on take up. Column 5 conditions on knowing the reward before take up (excludes the surprise reward treatment). See text for a description of the procrastination measures used in the regressions. * $p<0.10$ ** $p<0.05$ *** $p<0.01$.