Technology adoption under uncertainty: Take up and subsequent investment in Zambia*

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Abstract

Technology adoption often requires multiple investments over time, with costs and benefits that are unknown at the outset. As new information emerges, agents may prefer to abandon a technology that was profitable in expectation. We use a field experiment with two stages of randomization to vary the payoffs individuals consider when taking-up and following-through with a new technology: a tree species that provides fertilizer benefits to adopting farmers. Farmers' responses to the experimental variation identify the share of information about costs and benefits that is unknown at the time of take-up. Results indicate that farmers experience idiosyncratic shocks to net payoffs after take-up, which increase take-up but lower average tree survival. Simulations highlight that subsidizing take-up negatively affects the composition of adopters only when the level of uncertainty is low. Thus, high uncertainty provides an additional explanation for why technologies may go unused or abandoned even take-up is costly.

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1 Introduction

Many technology or product adoption decisions consist of at least two parts that occur at different points in time: an initial *take-up* decision, which is usually costly, and one or more subsequent investment or *follow-through* decisions that are necessary to fully realize the value of the technology. Examples of follow-through decisions that occur after take-up include planting or cultivating hybrid crop varieties, cooking with a clean stove in place of an older stove, taking prescribed medicine, and using a gym membership. At the time of take-up, both benefits of the technology and costs associated with the follow-through decision may be unknown to the potential adopter. New information arrives between the take-up and follow-through decisions in the form of learning about the technology (Foster and Rosenzweig 1995; Bandiera and Rasul 2006; Conley and Udry 2010; Beaman et al. 2014) or in the form of transient shocks to the opportunity cost of follow-through. If the new information is bad news about the profitability of the technology, then adopters may opt to abandon the technology (i.e., forgo the follow-through investments). Adopters know that they can reoptimize once new information is available and are likely to account for this at the time of take-up. Thus, the take-up decision can be interpreted as the purchase of an option to follow-through. Hereforth, we generally refer to *unknown* profit components as those pieces of information that are unknown to the potential adopter at the time of take-up but become known before the follow-through decision is made. Uncertainty, in our context, corresponds to the (strictly positive) variance of these unknown profit components and we generally assume that the distribution of these components is known by the potential adopter. Analogously, we refer to *known* profit components as information that the adopter has at all points in time.

The dynamics of multi-staged adoption decisions complicate the use of economic incentives to correct sub-optimal rates of adoption. Most often, incentives are applied in the form of subsidies on the take-up price of a technology. Note that in a rational and static framework (i.e., with no uncertainty), and if follow-through consists of a single binary choice (i.e., with no intensive use margin to the technology), everyone who takes-up at any positive price finds it in her best interest to also follow through.[1] Thus, subsidies that succeed at increasing take-up will be equally successful at increasing follow-through. The presence of

[1]Note that many follow-through decisions, including those in our empirical application, often have an intensive margin (Ashraf et al. 2010; Cohen and Dupas 2010; Fischer et al. 2014). If the follow-through decision has an intensive margin, individuals who take-up with a subsidy compared to no subsidy may follow-through with lower or higher intensity depending on whether the individually optimal follow-through intensity is positively or negatively correlated with the total profit from the technology. We further discuss the implications of an intensive margin of follow-through when we introduce our empirical framework.

uncertainty in the profitability of the technology introduces a complex relationship between the take-up price and the likelihood that those who take-up also follow-through. We refer to this relationship as the *screening effect* of the take-up price that exists even in the absence of an intensive margin on follow-through. First, the presence of at least some uncertainty in the profitability of the technology generates a positive screening effect of the take-up price. A positive screening effect means that a higher price induces selection of those who are more likely to follow-through or, conversely, a higher subsidy lowers follow-through conditional on take-up. Second, high levels of uncertainty induce high take-up rates and lower the screening effects of the take-up price. Thus, whenever there is uncertainty in the net benefits of follow-through, subsidies for take-up may be accompanied by less than perfect rates of follow-through and the extent to which the take-up price screens for high follow-through types depends on the magnitude of the uncertainty.

With the exception of several papers on the effect of learning about the technology on future take-up decisions (but not follow-through decisions), the existing literature has paid little attention to the relationship between uncertainty and the effectiveness of subsidies for technology adoption.[2] This paper begins to fill that gap by asking how uncertainty affects both take-up and follow-through outcomes for subsidized technologies, both in theory and in the context of a specific technology. We develop theoretical and analytical frameworks that can be applied in the following very general circumstances: (a) the decision to take-up the technology is separated in time from subsequent costly follow-through investments that are necessary for the full realization of the benefits, and (b) a failure to follow-through is not penalized (there is limited liability). We apply our analytic framework to farmer decisions to adopt agroforestry trees in Zambia. This technology requires both a one-time take-up decision (purchasing seedlings) and follow-through decisions (planting and caring for the trees) that occur in the months after take-up and are necessary for tree survival, but can be neglected without penalty. In this context, uncertainty at take-up emerges from potential shocks to the opportunity cost of follow-through, including shocks that affect the trees directly (e.g., pests, drought) but also those that affect the value of competing activities (e.g., illness of a household member, crop prices) and therefore the opportunity cost of time. Because the benefits of the trees emerge several years after planting, it is likely the case that, in our context, no new information on benefits emerges before the follow-through decision is made. Therefore, the relevant uncertainty in our context concerns the opportunity cost of

[2]The dynamic effects of subsidies on subsequent demand for the same technology is investigated by Carter et al. (2014), Dupas (2014) and Fischer et al. (2014), all of which find support for learning.

follow-through, but not the benefits. Nevertheless, the analytic framework we propose allows for the estimation of the magnitude of all sources of uncertainty that are dissipated before follow-through decisions are made. Our study proceeds in four steps: (1) a conceptual model of technology adoption under uncertainty; (2) a field experiment with variation in adoption payoffs at two points in time; (3) a structural model that builds on (1) and (2) and allows us to distinguish between uncertainty and alternative explanations for our empirical results; and (4) simulations that vary the magnitude of uncertainty and shed light on its importance relative to other drivers of selection.

In the first step, we develop intuition with a model of inter-temporal adoption under uncertainty in the presence of subsidies, where individuals make binary take-up and follow-through decisions at two different points in time. Although restrictive, assuming follow-through is binary in our conceptual model facilitates the exposition of the different components of the problem. Between the points in time at which the take-up and follow-through decisions take place, new information about the opportunity cost of follow-through is acquired. The theoretical model generates clear predictions about the relationship between uncertainty and adoption outcomes. First, a mean-preserving increase in uncertainty makes take-up more attractive provided that abandoning the technology at a later stage is costless. This is because, regardless of how costly following-through turns out to be, profit is always bounded below at zero by the option to abandon the technology. Thus, uncertainty can only increase the upside of the take-up decision.[3] Second, the existence of some uncertainty induces a positive screening effect of the take-up price, and thus subsidies reduce follow-through conditional on take-up. Third, this screening effect fades away with large amounts of uncertainty. Intuitively, if adopters know very little about their net cost of follow-through when they take-up, then a higher take-up price will no longer be effective at screening out those who are less likely to follow-through. Our basic conceptual framework borrows heavily from the literature on investment under uncertainty (Pindyck 1993; Dixit and Pindyck 1994) and, like Fafchamps (1993), shows that choices that appear to lead to losses (like purchasing a technology that is soon to be abandoned) can be rational ex-ante if their purpose is to preserve flexibility.[4]

The three main take-aways of our basic model hold when we extend our framework to allow for an intensive margin of follow-through (e.g., the number of trees cultivated in our

[3]This is true even in the presence of insurance, as the costless exit is still present in this case, and thus the contract becomes a substitute for insurance. See Giné and Yang (2009) for an example of how an uninsured credit contract may be more attractive than an insured one in the presence of limited liability.

[4]Other applications of dynamic decision making under uncertainty in the development and environmental literature include Bryan et al. (2014); Magnan et al. (2011); Arrow and Fisher (1974).

empirical application, or the frequency of usage in the case of other technologies and products). However, an intensive margin to the follow-through decision offers another explanation for the existence of positive or negative screening effects from higher prices at take-up, even in a setting with no uncertainty. This is because profits from the technology can differ both in the optimal scale or intensity of usage and in the magnitude of the net benefits. Implicitly, testing for a positive correlation between these two sources of heterogeneity has been the objective of much of the existing literature on the screening effects of price, which has focused on the adoption of technologies with an intensive margin of use or follow-through (e.g., Ashraf et al. 2010; Cohen and Dupas 2010). However, unlike the screening effect of price associated with uncertainty, which will always lead to higher follow-through at higher take-up prices, the screening effect of price on the intensive margin of follow-through could go in either direction. If, for example, adopters differ in both the fixed cost and marginal cost of adoption and these costs are negatively correlated, adopters who take-up at higher prices end up performing worse along the intensive margin (a negative screening effect). Suri (2011) documents this structure of profit heterogeneity in the case of hybrid crop varieties in Kenya. We account for these different dimensions of heterogeneity in the empirical design of our study to ensure that we do not attribute to uncertainty findings that could be driven by alternative drivers of selection.[5]

In our second step, we implement a field experiment involving the adoption of a technology that requires multiple investment decisions to generate empirical evidence on whether uncertainty in the profits from follow-through affect adoption outcomes. Farmers choose whether to adopt an agroforestry tree species that generates private soil fertility benefits over the long term, but carries short-run costs.[6] We observe whether the 1,314 farmers in the study take up a 50-tree seedling package at the start of the agricultural cycle. The follow-through decision consists of the number of seedlings that the farmer chooses to plant and care for (which we together refer to as tree cultivation) and, thus, has an intensive margin. This decision is separated in time from the take-up decision since it occurs over the course of the subsequent year.

We introduce exogenous variation into the adoption decision at two different points in

[5]The existing literature has also explored several other reasons screening effects may vary across contexts, including liquidity constraints and psychological channels such as sunk cost effects or procrastination (e.g., Mahajan and Tarozzi 2011; Fischer et al. 2014). Our experimental design mitigates concerns that liquidity constraints confound selection effects. Because we observe no screening effect of the take-up price on follow-through, we can rule out several psychological channels (see Section 4.2). We examine the empirical relevance of procrastination in Section 7 using survey measures of propensity to procrastinate, and find little evidence supporting this alternative explanation in our context.

[6]Agroforestry is, in its own right, an important technology for study. Zomer et al. (2014) estimate that 40 percent of agricultural land incorporates trees, a number that has been increasing over the last decade. We provide greater detail on the private productivity benefits and public environmental benefits of the species we study in Section 3.

time. First, we vary the take-up cost by subsidizing the purchase of the 50 seedlings. Farmers' take-up response to this randomly assigned incentive helps characterize the heterogeneity in expected net costs of follow-through across farmers (the known component of profits).[7] Second, we vary the payoff to follow-through by varying the size of a reward for the survival of at least 35 trees one year after take-up. The tree cultivation choices farmers make in response to the reward help us characterize the distribution of follow-through costs after potential shocks have been realized. Under the assumption that shocks are independent across farmers, the difference in the variance of net costs between the two points in time can be attributed to uncertainty.[8] Note that we do not artificially vary the allocation of shocks across our study population, since we are interested in identifying the underlying distribution of shocks that farmers naturally face. Instead, the reward creates exogenous variation in the incentive to follow-through, while the subsidy generates variation in the incentive to take-up. Together, these two sources of variation allow us to estimate the joint distribution of known and unknown components of follow-through costs. In other words, the experimental variation we induce allows us to estimate the magnitude of uncertainty relative to other forms of farmer heterogeneity.[9] This is among the first papers to introduce multiple dimensions to the experimental design to distinguish between adoption decisions and returns to investment (see Karlan and Zinman (2009) and extensions of their design by Ashraf et al. (2010); Cohen and Dupas (2010) and others), and the first to use this research design to explore time-varying returns to investment.

Our empirical results suggest that, in our context, there is a substantial amount of uncertainty in follow-through costs and that uncertainty is largely responsible for both low follow through rates conditional on take-up and a lack of significant screening effects of take-up prices. The reduced form responses to our treatments are consistent with our theoretical predictions about behavior in the face of substantial uncertainty: while farmers respond to economic incentives – they take-up at higher rates under higher subsidies and follow-through at higher rates under higher rewards – the randomized take-up subsidies are not predictive of the follow-through outcomes.[10] In addition, a large share of farmers who paid a positive

[7]Liquidity constraints could also affect the decision to take-up. To minimize the importance of cash-on-hand, farmers receive a show-up fee sufficient to cover take-up costs. We also test for self-selection using random variation in the timing of the reward announcement. We discuss these tests for liquidity constraints and other confounds in Section 4.

[8]The cross-farmer independence assumption rules out common shocks. In a model variant, discussed in Section 5, we relax the independence assumption by allowing for an unexpected common shock to all farmers.

[9]A similar source of variation has been used to identify the distribution of health shocks in the insurance literature: the performance reward varies the distribution of net benefits from follow-through much in the way that varying the terms of an insurance contract has a state-contingent effect on the distribution of payoffs (see Einav et al. (2013); Bryan et al. (2014); Karlan et al. (2014), among others).

[10]The lack of self-selection in our setting stands in contrast with Jack (2013), who provides evidence that farmers self-select

price end up abandoning the technology altogether.[11]

As our theoretical results highlight, the lack of an observed screening effect can also stem from a negative screening effect of take-up prices along the intensive margin counteracting the positive screening effect that emerges under moderate levels of uncertainty. Thus, in our third step, we turn to our structural model to estimate the magnitude of uncertainty revealed through farmers' choices and to shed further light on the the extent to which uncertainty is responsible for our empirical findings relative to other sources of heterogeneity. Our structural model accomplishes this by allowing for heterogeneity in the privately optimal number of trees as well as in the net cost of follow-through, and allows these two known (to the farmer) components of private profit to be freely correlated.[12] We find that in our setting, heterogeneity along the intensive margin of tree survival operates in the opposite direction as the extensive margin heterogeneity (similar to Suri 2011), thus working against the positive screening effect of prices generated by uncertainty. In addition, our estimates find a large variance in the unknown component of costs, i.e. a large amount of uncertainty. To illustrate the significance of this uncertain component of costs, we make the following calculation: the flexibility associated with waiting for more information leads to 26 percent higher take-up than if farmers had to pre-commit to growing a certain number of trees prior to take-up, and 68 percent of farmers end up growing a different number of trees than they would have under pre-commitment.

Finally, in our fourth step, given that multiple factors contribute to the lack of a significant screening effect of the take-up price, we simulate outcomes under different levels of uncertainty to better understand its importance in explaining our results. We find that, given the large amount of uncertainty revealed by farmers' choices, reducing it would result in a positive and significant screening effect of prices. More precisely, reducing the variance of shocks by 50 percent (holding everything else constant) would increase tree survival among those who take-up under full price by 17 to 20 percent relative to those who take up under a price of zero.

From a policy standpoint, uncertainty lowers adoption outcomes per dollar of subsidy

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based on future costs into a tree planting incentive contract in Malawi. She studies a different context and different contract design. In addition, the pattern of selection effects over time in her study is consistent with a multi-year extension of our conceptual framework, which would predict stronger selection as the number of farmers who continue to cultivate trees shrinks.

[11]We rule out a number of alternative motives for this behavior. First, we rule out re-sale of trees by exploiting cross-group variation in incentives to re-sell (see Appendix Table A.4.1). Second, we test whether a desire to please the experimenter (social desirability bias) drives our results by allowing for a common "boost" to the attractiveness of take-up in our structural model, and find it has little effect on our estimates (Section 6).

[12]This is akin to correlated random coefficient (CRC) models, where returns to the technology are allowed to differ across potential adopters and therefore influence their decision to adopt (Heckman et al. 2010).

invested, but increases the expected private profits to the adopter because the downside risk from taking up is bounded at zero. The simulations also highlight a tradeoff associated with take-up subsidies under uncertainty: the negative screening effect of subsidizing the take-up price may be offset by higher rates of take-up, such that a larger number of farmers produce a smaller number of trees each. The policy implications of this tradeoff depend on whether the benefits of the technology come from high levels of follow-through by a small number of adopters, or from a lower level of follow-through spread across many adopters. The simulations also point to the benefits of incentivizing follow-through directly. Stronger contracts that force adopters to follow-through once they take-up address the problem of high take-up coupled with low follow-through, but they do so at a clear cost to the adopter. While this paper does not provide a policy prescription on the structure of economic incentives in the presence of uncertainty, it highlights the importance of further research exploring more innovative solutions to encourage both take-up and follow-through in the presence of uncertainty.

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) when identifying the distribution of the unobserved opportunity cost determinants. We do so by introducing two layers of random variation in economic incentives, one that produces a probability of take-up equal to one for a randomly selected sub-population and a second that produces an interior solution in tree cultivation 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 (used for example, in 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.

The paper proceeds as follows. We begin with a simple theoretical model to generate intuition. Section 3 describes the empirical context and experimental design, and Section 4 shows reduced form results. We present the empirical model and its identification in Section 5 and show estimation results and simulations in Section 6. Section 7 discusses interpretation and Section 8 concludes.

2 A simple model of intertemporal technology adoption

In this section, we analyze take-up and follow-through decisions when both decisions are binary. This provides insight into the role of uncertainty in self-selection. Consider a two period model, where each agent chooses whether to purchase (take-up) a single unit of a technology in the first period (time 0), and whether to follow-through with implementation of the technology in the second period (time 1). The only cost incurred at take-up is $c - A$, where c is the price of the technology and A is an exogenous subsidy. At time 0, individuals have some information about the costs and benefits of following-through, but not all. We assume the privately known and unknown components of the net (of benefits) cost of following through, F_0 and F_1 respectively, are additively separable. F_0 is known at all times and F_1 is revealed to the agent at time 1. Thus, the private benefit of following through is given by $R - (F_0 + F_1)$, where R is an exogenous reward conditional on following through. Note that because the term$F_0 + F_1$ represents the cost net of benefits, it can be positive or negative. Assume that c, A and R are constant across agents, while F_0 varies according to some cdf $G_0(f_0)$. Assume further that

(i) F_0 and F_1 are independent, and

(ii) $\mathbb{E}_{t=0}(F_1) = \mathbb{E}_{t=1}(F_1) = 0$

The independence assumption, (i), is essentially a restatement of the information structure: there is no information left in F_0 that would help the individual better predict the realization of F_1. In other words, F_0 represents the agent's best guess at $t = 0$ about her specific net cost of following through, and F_1 represents any new information that emerges after the take-up decision is made. Assumption (ii) means that agents have rational expectations.

Following backward induction, the agent decides to follow-through at $t = 1$ if $R - F_0 - F_1 > 0$. If this inequality does not hold, the agent decides not to follow-through, which yields a payoff of zero at $t = 1$. At $t = 0$, the agent decides to take-up by purchasing the technology if

$$c - A - \delta \mathbb{E}_{F_1} \max(R - F_0 - F_1, 0) < 0 \tag{1}$$

where δ is the one-period discount factor and the expectation in (1) is taken with respect to the density of F_1.

To simplify the exposition, assume that the distribution of F_1 is such that $F_1 \in \{f_L, f_H\}$, with $f_L < f_H$ and $\Pr(F_1 = f_L) = p_L$.[13] With $p_L = \frac{1}{2}$, this simple distributional assumption

[13] We adopt this distributional assumption on the shock to obtain closed form expressions that are simple and intuitive.

on the shocks allows us to represent a mean-preserving increase in uncertainty as a symmetric widening of the distance between f_L and f_H.[14] With this assumption, we can classify individuals into three types: those who always follow-through, regardless of the realization of F_1 (*always follow-through types*), those who follow-through only if the low net cost shock is realized (*contingent follow-through types*), and those who never follow-through (*never follow-through types*). These three types of agents can be characterized by whether their value of F_0 is below $R - f_H$, between $R - f_H$ and $R - f_L$, and above $R - f_L$, respectively. Figure 1 graphically shows the proportions for each type of agent using areas under a symbolic bell-shaped distribution for F_0, separated by gray dashed lines. Figure 1 also illustrates two thresholds (along the support of F_0) for take-up in black dashed lines. The first take-up threshold (labeled $R - \mathbb{E}(F_1) - \frac{c-A}{\delta}$) is only binding if it falls to the left of the threshold that defines always adopters ($R - f_H$). When this first take-up threshold binds, only always follow-through types take-up. The second take-up threshold (labeled $R - f_L - \frac{(c-A)}{\delta\, p_L}$) is perhaps more interesting. When binding, all always follow-through types take-up, but only a share of contingent follow-through types take-up (those to the left of the threshold). We use this figure to explain intuitively the results outlined by each of our propositions below. The formal proofs of these propositions can be found in Appendix A.1.

However, the results from our conceptual model are robust to different distributional assumptions. More specifically, results from Propositions 1-3 below also hold under continuous distributional forms for the shock including the uniform distribution and the normal distribution. Proposition 4 below is a direct consequence from investment under uncertainty theory, and holds under any distributional assumption (Dixit and Pindyck 1993).

[14] This model simplifies our empirical setting in two key ways: first, it assumes a binary follow-through decision and second, it assumes a discrete distribution on F_1. As we show when we present our empirical model, the propositions derived from this model are not an artifact of the distributional assumption on F_1 nor of the binary decision that characterizes follow-through in this simple model.

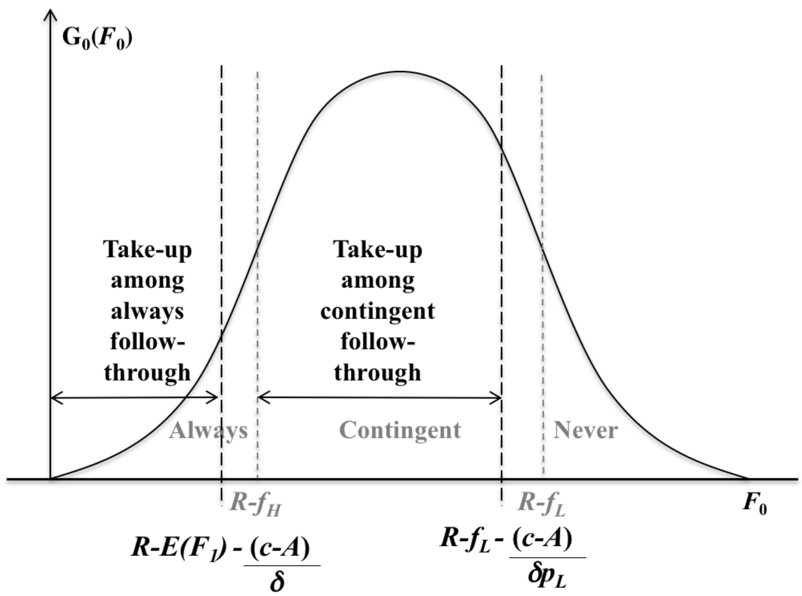

Figure 1: Take-up and follow-through thresholds as a function of agent type

Notes: The figure shows the shares of always follow-through, contingent follow-through and never follow-through types over a symbolic probability density function of F_0. The grey thresholds ($R - f_H$ and $R - f_L$) correspond to the follow-through thresholds, while the black thresholds correspond to the take-up thresholds.

Proposition 1 *Follow-through conditional on take-up weakly increases as a function of take-up cost, i.e. there is a screening effect of the take-up cost.*

To see this, note that as take-up cost increases (represented by $c - A$ in Figure 1), the second take-up threshold (right black dashed line) moves to the left, bringing down the overall share of contingent follow-through types among the set of individuals who take-up. Since contingent types follow-through with probability less than one (p_L), this in turn increases the share of individuals who follow-through among those who take-up.[15]

Proposition 2 *An increase in uncertainty reduces follow-through conditional on take-up*

This can also be appreciated from Figure 1: a widening of the distance between f_L and f_H causes the share of contingent follow-through types to increase (as the two grey dashed lines move further apart). Note that as uncertainty increases, the position of the second take-up threshold does not change relative to the threshold that determines the upper bound for contingent follow-through types. Thus, this group becomes a larger share of those who take up, reducing average follow-through.

[15] If the take-up cost, $c - A$, increases enough that the first take-up threshold is binding, follow-through conditional on take-up reaches 100 percent and is constant for further increases in the take-up cost.

Corollary 2.1 *Under no uncertainty, everyone who takes-up follows-through.*

This is easy to see from Figure 1: under no uncertainty (where $f_L = f_H$) there would be only always follow-through types and never follow-through types. Thus, under no uncertainty, there is also no screening effect of the take-up cost.

Proposition 3 *The screening effect of the take-up cost shown in Proposition 1 dissipates under large amounts of uncertainty.*

To see this, consider the takeaways of Propositions 1 and 2 simultaneously. The share of contingent adopters that are excluded by an increase in the take-up cost becomes a smaller proportion of all those who take-up when uncertainty increases.

Proposition 4 *The option value associated with take-up is increasing in uncertainty, which results in higher take-up at all take-up cost levels.*

This is shown formally in Appendix A.1 along with the formal definition of option value in our context. Intuitively, the option value is the value of reoptimizing once new information (the realization of F_1) emerges. As the distance between f_H and f_L increases, the payoff at $t = 1$ conditional on a low cost shock (f_L) increases for contingent follow-through types. This is because agents can choose not to follow through if the payoff of doing so is negative; thus the payoff at $t = 1$ is bounded at zero even when f_H, the high cost, becomes very large. Meanwhile, an increasing distance between f_L and f_H means that f_L becomes lower and lower, increasing the expected payoff of follow-through conditional on f_L being realized. Thus, the expected value of the contract at $t = 0$ is strictly increasing in uncertainty, and this increase emerges solely because of the possibility of reoptimizing (i.e. choosing not to follow-through when the cost is high). A higher expected value of the contract results in higher take-up.

A note on risk neutrality. We assume linear utility – or risk neutrality – throughout the paper, including the empirical analysis. Assuming some degree of risk aversion does not change our results qualitatively, although it lowers the value placed on extreme positive profitability shocks at the time of take-up. This makes the expected value of the contract at take-up less responsive to increases in uncertainty. That said, the risk neutrality assumption is relatively innocuous and carries important advantages given our empirical context. Although risk aversion is an important component of intertemporal decisions with costs or benefits that represent substantial shares of household income, our specific technology adoption decision causes relatively small changes to income. In addition, our framework (both theoretical and empirical) models decisions as a function of the net-benefits associated with adoption relative to the best alternative use of household resources. Thus, a positive shock to the opportunity cost of adoption could correspond to an increase or a decrease in overall household income. For example, an increase in profitability of a competing economic activity and a labor shortage due to health could both represent an increase in the opportunity cost of adoption, but would have opposite effects on total income and thus on marginal utility of income. Incorporating risk aversion into our theoretical model would require us to make modeling assumptions about the nature of the opportunity cost of adoption. Hence, assuming risk neutrality allows us to leave the source of the opportunity cost unspecified, which makes our framework generalizable to any source of uncertainty regardless of its impact on overall income.

Transitory shocks and learning. So far, we have left open the question of whether F_1 should be interpreted as a persistent or a transitory shock, and our framework is consistent with both interpretations. However, the distinction matters for future take-up decisions. If the F_1 component of the returns to the technology has a persistent component, future take-up decisions will occur under a lower level of uncertainty since individuals will have learned its realization. If F_1 is completely transitory, future take-up decisions will look similar to the first take-up decision since individuals will receive a new draw of the shock next time. We cannot completely disentangle these two interpretations of the model in our context, though we use survey data to provide suggestive evidence on the extent of learning or persistence (see Section 7).

3 Context and experimental design

We bring the propositions from our conceptual model to a multi-stage technology adoption problem, characterized by uncertainty in the costs and benefits of following through with the technology. To test the relevance of the framework, a technology adoption decision requires two key ingredients: first, adoption of the technology must involve investment decisions at two or more different points in time, and second, there is no penalty associated with abandoning the technology.[16]. We study the adoption of agroforestry trees, which require an initial investment in planting inputs and subsequent investment in tree care. The technology

[16]Our results also apply if the penalty is small and independent of the unknown component of net-costs.

can also be abandoned with no penalty, and thus meets the minimal criteria needed for our adoption framework. In the context of an ongoing project to encourage the adoption of agroforestry trees (*Faidherbia albida*), we introduce exogenous variation in the payoffs to farmers at the time of their take-up and follow-through decisions. We use the experimental variation to uncover the existing levels of heterogeneity and uncertainty in the population of farmers, which we model as random parameters. This section describes the context and the experimental design in detail.

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 1,314 farmers growing cotton under contract with Dunavant, alongside other subsistence crops. The project is part of the NGO partner's portfolio of carbon market development projects in Zambia.

3.1 The technology

Faidherbia albida is an agroforestry species endemic to Zambia that fixes nitrogen, a limiting nutrient in agricultural production, in its roots and leaves. 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 planting *Faidherbia* does not displace other crop production (Akinnifesi et al. 2010). Agronomic studies suggest significant yield gains from *Faidherbia*.[17] However, these private benefits take 7-10 years to reach their full value, and may be insufficient to justify the front-loaded investment costs, particularly if farmers have high discount rates. In the first year after trees are planted on the field, the farmer has to invest time to weed, water and protect the trees from pests and other threats (Glenn 2012). We refer to these investments collectively as the cultivation decision. Survey data indicate that farmers in our study devoted around 2.4 hours per tree, on average, to cultivation activities in the first year.[18] Once a seedling survives the first dry season, it requires little additional care and the costs of cultivation diminish substantially. Our study ends after one year, which implies that the new information farmers acquire over the course of the study includes the opportunity

[17] Estimates of yield increases range from 100 to 400 percent, relative to production without fertilizer (Saka et al. 1994; Barnes and Fagg 2003), with the biggest gains seen when the species is intercropped with maize (Shitumbanuma 2012). This is within the estimated range of yield impacts associated with chemical fertilizer application in Zambia. High costs of fertilizer and challenges of access make average returns to fertilizer zero or negative (Xu et al. 2009).

[18] Other than planting inputs (seeds, planting sleeves and nursery materials or seedlings), the trees require only time. For comparison, Glenn (2012) measures substantially higher labor costs in the first year, though the costs she calculates include activities that the farmer would likely have done on the field even absent the planting of *Faidherbia albida*.

cost of cultivation, but not long-run fertilizer benefits since these will not have started by the time the study ends. Note that the opportunity cost of the first year investments may vary substantially across farmers and may have many components that are hard for the researcher to measure directly. Consequently, one major objective of this paper is to estimate the heterogeneity in opportunity costs via a revealed preference approach. Nevertheless, we conducted a rough cost-benefit calculation which suggests that the average private benefit, discounted at appropriate rates, is less than the average private costs.[19] Consistent with this back of the envelope calculation we observe low adoption rates at baseline: less than 10 percent of the study households reported any *Faidherbia* on their land. Note that our rough calculation assumes perfect information about benefits and free sourcing of inputs. Thus, lack of information and the absence of an existing market for *Faidherbia* seedlings may also contribute to the low baseline adoption rates. Informal land tenure presents an additional barrier to adoption. By focusing on landholders engaged in contract farming arrangements, the project targets households with relatively secure tenure.

Importantly, the net present value of tree planting becomes positive once you add the carbon sequestration benefits of *Faidherbia,* justifying the use of subsidies. A recent case study of alternative land-based carbon sequestration activities on small farms found a positive social net present value associated with planting *Faidherbia*, while the NPV for most other activities was negative (Palmer and Silber 2012). We also conducted our own calculation based on allometric equations from Brown (1997), adapted to the growth curves for *Faidherbia*. We estimate that over 30 years, a tree sequesters around 4 tons of carbon dioxide equivalent. Discounting the annual sequestration at 15 percent, and assuming long run tree survival rates consistent with what we observe in the short run, leads to a present value of around 0.48 tons per tree. A social cost of carbon of USD 37 per ton implies USD 17.76 worth of external benefits per tree, which more than covers the private cost of adoption.[20] Additional local externalities, not accounted for in this NPV calculation, include erosion control and wind breaks. The combination of private and public benefits has led to renewed

[19]For this 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 for 50 trees or around USD 0.04 per tree. While this discount rate is high, it 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 2.4 hours per tree in activities related toplanting and tree care. At an agricultural wage rate of around USD 2.5 per day, these time investments correspond to around USD 0.75 of labor costs per tree in the first year.

[20]If instead of the social cost of carbon, we consider the value of a carbon credit at USD 5 per ton, the NPV of the carbon sequestration still exceeds the private cost of adoption (USD 2.4 vs. USD 0.75 per tree).

interest in agroforestry and afforestation in developing countries in recent years. For example, Zomer et al. (2014) estimate that around 40 percent of the world's agricultural land and over 1 million square kilometers in sub-Saharan Africa is under agroforestry.

3.2 Experimental design and data collection

The field experiment was implemented between November 2011 and December 2012 with 125 farmer groups and 1,314 farmers. Implementation of the study relied on Dunavant's outgrower infrastructure, which is organized around sheds, each of which serves several dozen farmer groups. Each farmer group consists 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. Implementation was concentrated at two points in the agricultural season. Appendix Figure A.4.1 shows the implementation timeline, relative to other agricultural activities. First, farmer training, program enrollment, and a baseline survey all occurred at the beginning of the planting season. As the figure shows, this is also the time that farmers make decisions on other crops and technologies. Second, the endline survey, tree survival monitoring and reward payment occurred at the end of our study period, one year after program enrollment. In addition, we performed mid-year tree monitoring for a subsample of farmers and a brief survey at the end of the planting season.

At the training, farmers were provided with instructions on planting and caring for the trees, information about the private fertilizer benefits and public environmental benefits of the trees, and details on eligibility for the program.[21] All farmers who attended the training received a show up fee of 12,000 ZMK and lunch. Farmers were told the money received, which was equivalent to about a day's agricultural wages, was compensation for their time and was theirs to keep. This design feature was intended to reduce the effect of immediate liquidity constraints on take-up. Since it may also have generated experimenter demand effects, we allow for a homogenous positive demand shock in the take-up decision when we estimate the structural model (see Sections 5.3 and 6.1).

Enrollment occurred at the end of the training and consisted of farmers' take-up decision. Study enumerators explained the details of the enrollment choice: a take it or leave it offer of 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. The study design varied two major margins of the farmer's decision to adopt *Faidherbia albida*. First, the size of the take-up subsidy (A)

[21] 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.

varied between 0, 4,000, 8,000, and 12,000 ZMK. At zero subsidy, farmers paid 12,000 ZMK (approximately USD 2.60 or around a day's agricultural wages) for inputs, which is the cost recovery price for the implementing organization, but falls below farmers' full cost of accessing seeds or seedlings outside of the program. Groups were randomly assigned to one of four take-up subsidy treatments with equal probability using the min max T approach (Bruhn and McKenzie 2009), balanced on Dunavant shed, farmer group size and day of the training. The subsidized price of the inputs was announced to all farmers in the group at the end of training, before the take-up decision was made.

Second, the program offered a threshold payment conditional on follow-through (tree survival) after one year. The payment varied randomly at the individual level. Farmers received the reward if they kept 70 percent (35) of the trees alive through the first dry season (for 1 year).[22] The threshold reward, as opposed to a per-tree incentive, allows us to draw a sharper distinction between private incentives and external incentives to cultivate the trees, which aids identification of the structural model. To implement the individual-level randomization of the rewards and allow participants to make their take-up decision in private, the study enumerators called the farmers aside one by one and described the threshold nature of the reward. 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 size of the threshold performance reward (R) was varied in increments of 1,000 ZMK, ranging from zero to 150,000 ZMK or approximately 30 USD.[23] 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. The frequency of treatment outcomes are shown in Appendix Figure A.4.2.

We introduced an additional source of variation that provides another test for screening or self-selection based on contract characteristics at take-up, less likely to be affected by cash-on-hand liquidity constraints: the timing of the reward draw was varied at the individual level to occur either before or after the farmer's take-up decision, with 52.5% assigned to the surprise reward treatment. When the reward is known before take-up, it affects both the type of farmer who takes-up and also the decision to follow-through; when it is not known

[22]In addition to its advantages in our empirical design, the threshold reward was easy to enforce, consistent with other related contracts offered by Dunavant, 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.

[23]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.

at take-up, it affects only follow-through. Varying when the reward was revealed allows us to isolate its effect on self-selection, similar to Karlan and Zinman (2009).[24] Specifically, conditional on take-up, we can test whether the effect of the reward value on follow-through outcomes depends on whether the reward value was factored in to the take-up decision (see Appendix Table A.4.2).

Following the take-up decision, all farmers were given a baseline survey that lasted for approximately one hour. After the survey, participating farmers signed a contract indicating their agreement with the program terms, paid the take-up cost and collected their seedlings. To preempt an effect of seedling choices on tree survival, farmers were not allowed to pick their seedlings.

One year after the training, all farmers in the study sample were given an endline survey. Approximately one week after the endline survey, farmers with contracts were visited for field monitoring, during which the farmer and a study enumerator examined each tree, and recorded whether it was sick, healthy or dead. Monitors also recorded indicators of activities likely to affect survival outcomes: weeding, watering, constructing fire breaks, and field burning (which, in contrast to the other three, threatens tree survival). All surviving trees counted toward the tree survival threshold. Within a couple of days of the monitoring visit, farmers with 35 or more surviving trees received their reward payment. Keeping the payments separate from the monitoring was intended to improve monitors' objectivity.[25]

In addition to the baseline and endline surveys, one-fifth of the farmers were randomly sampled for ongoing data collection on activities and inputs associated with the trees and with other crops. Farmers selected for this effort monitoring received a very short survey (around 20 minutes) every two weeks, during which a project monitor asked the farmer about agricultural activities, including those related to the trees, since the last visit. No information was provided to the farmers about their performance and monitors were instructed not to prompt specific activities or answer technical questions. We control for the effort monitoring subsample in our analysis. The resulting data yield two important facts about the timing of farmer investments. First, planting activities began immediately after the training for some farmers, while other farmers chose to delay tree planting until other crops were planted and the rainfall patterns were clearly established. Second, tree care activities spanned the

[24] We do not manipulate or measure beliefs about potential financial benefits from joining in the surprise reward treatment, and cannot therefore assume that farmers in the surprise reward treatment assumed $R = 0$ at the time of take-up.

[25] 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. Some monitors had higher paid jobs as survey supervisors when not engaged in monitoring. Thus, if cheating was occurring, we would expect the opportunity cost of doing to be different across monitors. However, no single monitor indicator is significantly correlated with reaching the threshold, nor are the monitor indicators jointly predictive.

entire agricultural season and tapered off before the tree survival monitoring one year after training, consistent with the need for ongoing investments on the part of the farmer and front-loaded cultivation costs.

A note on the timing of farmers' decisions and information. Because the program offered rewards and measured outcomes for one year, the experiment manipulates costs and benefits during the first year only. The rationale for short-run incentives is that the opportunity costs associated with planting and caring for the trees are highest during the first year when the trees are vulnerable. The follow-through decision we use for the bulk of our analysis, the survival of the tree, is more accurately described as the cumulative outcome from numerous follow-through decisions made over the course of the year after take-up. New information may reveal itself starting immediately after the take-up decision is made, or at different points in time, as family members fall ill, crops fail, or input and output prices change.[26] When new information arrives that affects the opportunity cost of caring for the trees, farmers may reoptimize on the number of trees they continue to cultivate (if any). Our empirical model imposes a simplified version of the timing; we assume there are only two decision periods (take-up and follow-through) as opposed to many. This simplified timing assumption corresponds well to the empirical setting if the bulk of the information arrives shortly after take-up or with a series of shocks that are highly correlated.[27]

4 Summary statistics and reduced form results

Appendix Table A.4.3 shows baseline summary statistics by treatment category and the largest pairwise normalized difference within each treatment. Around 74 percent of participants are heads of household and 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. 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

[26] The take-up decision is made at the beginning of the planting season, as shown in Appendix Figure A.4.1. This is the natural timing of take-up decisions for other crops and technologies. Therefore, our design allows for an amount of time between take-up and follow-through that is similar to many other agricultural technologies.

[27] The presence of multiple follow-through decisions in response to a series of independent shocks would lead to tree survival rates that are similar to those under a single shock with very large variance (higher than the sum of the variances of the underlying shocks). This follows because investments towards survival at each point are necessary to guarantee survival at a later point. Thus, successful outcomes are rare and stem from a series of fortunate events. Technologies and products with opportunities for subsequent investment that are not interlinked (like bednets or medication) will not have a blown-up variance of this sort.

state that soil fertility is one of the major challenges that their household faces. Households have worked with Dunavant Cotton for an average of over 4 years and over 40 percent interact regularly with their lead farmer. Almost 70 percent of respondents report familiarity with the technology but only around 10 percent had adopted prior to the program, likely due in part to the absence of a market for *Faidherbia albida* seeds or seedlings.

We test for balance in the randomization outcomes by calculating normalized differences for each treatment (subsidy and reward) category pair, and report the largest difference across subsidy categories in column 5 and across reward categories in column 9. None of the differences exceed the rule of thumb threshold of 0.25 (Imbens and Wooldridge 2009). We also examine whether non-random attrition at any stage of data collection affects internal validity (Appendix Table A.4.4).[28] The baseline survey covered over 98 percent of trained farmers, while the endline survey included over 95 percent of baseline respondents. We see some evidence that farmers who received lower take-up subsidies were marginally less likely ($p < 0.10$) to participate in the surveys. Otherwise, survey attrition is balanced across treatments. For the tree survival monitoring, over 95 percent of the 1,092 households that took up the program were located.[29]

Finally, spillovers across treatments pose a threat to the experimental design. Because the take-up subsidy was assigned at the group level, spillovers are relatively unlikely. The value of the threshold reward, on the other hand, varied at the individual level. By revealing the reward value privately to each farmer before the take-up decision, we mitigate the likelihood that take-up is affected by rewards received by others. However relative reward values may still affect follow-through since farmers can share information after they leave the training. We test for spillovers associated with the threshold reward and observe little evidence that they affected outcomes.[30]

[28] Selection into treatment is also a threat to the experiment's internal validity. By design, this is unlikely: group level participation 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.

[29] Of the farmers eligible for monitoring, we were unable to locate 9 of them and thus assume zero tree survival in the analysis.

[30] Specifically, we test whether spillovers across reward levels affected take-up by regressing the probability of take-up on the average reward drawn preceding a farmer's own draw, and find no effect. We also might be concerned that randomization at the individual level would affect follow-through, through re-sale of seedlings to those with higher rewards or transplanting trees from outside of the program. First, we test for within-group spillovers associated with the effect of the reward on tree survival outcomes (see Appendix Table A.4.1). 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. To further look for evidence of transplanting, we take advantage of a brief survey conducted on all farmers who took up after most planting was complete. The number of trees counted in this survey is below the number observed during tree monitoring at the end of the project for around 100 farmers, indicating 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 shows no significant relationship.

4.1 Reduced form evidence of uncertainty

This section describes our reduced form results. First, we examine how the incentive offered by the threshold reward affects tree survival outcomes. Second, we look for reduced form evidence consistent with the presence of uncertainty. Third, we address alternative explanations for our findings including liquidity constraints and behavioral decision-making.

Table 1 displays means and standard deviations for several program outcomes: take-up, follow-through (tree survival ≥ 35), zero surviving trees and the number of trees conditional on positive survival rates. These statistics are broken down by treatment and show clear patterns of responses to the incentives offered in the experiment. We also estimate a linear relationship between the administrative outcomes and the treatments, shown in Table 2. Panel A shows results for all relevant farmers and Panel B shows results conditional on take-up. In Panel A, specifications that include the reward are restricted to the sub-treatment that drew reward values prior to making a take up decision (the "no surprise group") since farmers in the surprise treatment group did not draw a reward value if they did not take up.

We use the means and standard deviations presented in Table 1 and the linear regression results in Table 2 to examine the predictions of our conceptual model. First, notice that take-up rates are increasing across values of the take-up subsidy (Table 1), with a thousand ZMK increase in the value of the subsidy leading to a 2 percentage point increase in the probability of take-up (column 1, Table 2). Take-up rates are high, on average, even in the zero subsidy condition, where over 70 percent of farmers take-up. Without further evidence, this could be due to high known net-benefit from follow-through on average, and/or to high expected values driven by option value (see Proposition 4).[31]

Second, we observe that follow-through rates vary considerably within treatment and are low, on average, with only 25 percent of farmers reaching the 35-tree threshold (column 2, Table 1). This holds even in the zero subsidy condition, ruling out that the high take-up was due to farmers being certain about high payoffs associated with cultivating a large number of trees. Instead, low follow-through conditional on take-up is consistent with large amounts of uncertainty according to Proposition 2.[32] At the same time, follow-through is responsive to the value of the reward (column 4, Table 2), which indicates that farmers had at least some control over tree survival outcomes.

[31] Recall that even among those that receive no subsidy, costs of accessing seedlings are still lower than they would be outside of the study. Section 5 offers one approach to measuring how much of the high take-up can be explained by aggregate shocks, which could include experimenter demand effects.

[32] Behavioral explanations such as over-optimism or procrastination might also be consistent with high take-up and low-follow through, even at positive take-up prices. We discuss behavioral explanations consistent with the reduced form results, as well as the interpretation of the type of new information, in Section 7.

Table 1: Summary statistics

	(1) Take-up	(2) 35-tree threshold	(3) # trees | # trees> 0	(4) Zero trees
		Panel A: full sample		
mean	0.83	0.25	27.42	0.36
sd	0.38	0.44	14.31	0.48
		Panel B: by take up subsidy treatment		
A = 0				
mean	0.71	0.26	27.60	0.37
sd	0.46	0.44	14.31	0.48
A = 4000				
mean	0.76	0.29	28.86	0.36
sd	0.43	0.45	13.67	0.48
A = 8000				
mean	0.86	0.27	29.30	0.38
sd	0.35	0.44	14.19	0.49
A = 12000				
mean	0.97	0.22	24.93	0.33
sd	0.17	0.41	14.52	0.47
		Panel C: by reward treatment		
R = 0				
mean	0.90	0.13	22.00	0.49
sd	0.31	0.34	14.70	0.50
R = (0,70000]				
mean	0.90	0.21	25.45	0.40
sd	0.30	0.41	14.62	0.49
R = (70000,150000]				
mean	0.93	0.32	29.53	0.30
sd	0.25	0.47	13.67	0.46

Notes: Means and standard deviations of take-up (column 1) and follow-through (columns 2-4) outcomes, by experimental treatment. Column 1 includes all farmers (N=1314). Columns 2-4 are conditional on take-up (N=1092). Column 2 reports the number of farmers who reached the performance reward threshold.

Table 2: Effects of treatments on take-up and follow-through

	Take up		Follow-through		# trees $\mid$ trees > 0		1.(zero trees)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel A. Unconditional on take-up				
Take-up subsidy	0.022***		0.002		-0.229		-0.016***	
	(0.005)		(0.004)		(0.200)		(0.005)	
Reward		0.001*		0.001***		0.047***		-0.001***
		(0.000)		(0.000)		(0.017)		(0.000)
Knew reward at take-up		x		x		x		x
Dep. Var. Mean	0.83	0.84	0.21	0.21	27.42	27.11	0.47	0.47
N	1314	624	1314	624	701	333	1314	624
				Panel B. Conditional on take-up				
Take-up subsidy			-0.004		-0.229		-0.003	
			(0.004)		(0.200)		(0.005)	
Reward				0.001***		0.044***		-0.001***
				(0.000)		(0.013)		(0.000)
Dep. Var. Mean			0.25	0.25	27.42	27.42	0.36	0.36
N			1092	1092	701	701	1092	1092

Notes: OLS regressions of outcomes on treatment variables. The reward and take-up subsidy are both measured in thousand ZMK. Columns 1 and 2 show effects on take-up, columns 3 and 4 on follow-through at the 35 tree threshold, columns 5 and 6 on the number of surviving trees conditional on positive tree survival, and columns 7 and 8 on the probability of zero surviving trees. Panel A includes farmers who do not take-up and assumes they have zero surviving trees. Even columns restrict the sample to farmers in the "no surprise group" (knew the reward before take-up) so that reward values are non-missing for all observations. Panel B is conditional on take-up.

Third, a large number of farmers abandon the technology altogether (have a survival of zero trees), even conditional on taking up with zero subsidy (37 percent, column 4, Table 1). This rules out the no-uncertainty scenario according to Corollary 2.1 of our conceptual model. It is also not the case that zero-tree farmers are unresponsive to monetary incentives, since the likelihood of zero surviving trees is decreasing in the value of the reward (column 8, Table 2).

We also see no reduced form effect of the subsidy treatment on the likelihood of reaching the 35-tree threshold or of abandoning the technology , conditional on take-up (Panel B, Table 2). This is consistent with Proposition 3, which states that the screening effect of subsidies will be diminished by high levels of uncertainty in the net benefits of follow-through. The variation in the timing of the reward announcement offers a separate test for farmers' ability to self-select at take-up based on information about their net-costs of follow-through. Conditional on take-up, the effect of the reward on follow-through does not depend on whether the farmer factored it into the take-up decision or not (see Appendix Table A.4.2).

Finally, we note one additional observation that is consistent with the relatively small amount of information about follow-through costs that is available to farmers at take-up. Observable characteristics of the farmer are very poor predictors of follow-through: the R-squared from a regression of tree-survival on observables is 0.042. In comparison, the experimental treatments deliver an R-squared of 0.077. It is also worth noting that observables are also poor predictors of take-up, which means that even the known component of follow-through costs is hard to measure directly.[33] The low explanatory power of observables further motivates our structural model, which uses a revealed preference approach to estimate the heterogeneity across farmers at both take-up and follow-through.[34]

4.2 Additional results and confounds

In this section, several potential confounding factors. First, we examine alternative measures of effort, then we investigate the role of liquidity constraints in self-selection and finally we test for psychological effects in take-up and follow-through decisions, including time inconsistency.

As the measure of follow-through that we use in our analysis is based on tree survival, it is important to corroborate that other more direct measures of farmers' investments are also

[33] See Appendix Table A.4.5 for a complete version of these results.

[34] Unobserved heterogeneity is shown to be an important determinant of decision making in contexts ranging from labor markets and human capital (Heckman 2001) to trade (Eaton et al. 2011).

responsive to our treatments. Enumerators recorded signs of weeding, fire breaks, watering and burning during field monitoring visits at the end of the project. All of these costly activities are likely to affect tree survival, the first three positively and the last negatively. A linear probability regression on the threshold reward shows a positive effect on weeding, fire breaks and watering, with p-values of 0.059, 0.129, and 0.041 respectively. The coefficient on field burning, which threatens tree survival, is negative and statistically insignificant. Complete results can be found in Appendix Table A.4.6.

Our research design allows us to address concerns that liquidity constraints play an important role in take-up decision and farmers' expected payoffs at the time of take-up in two ways. First, as noted above, all farmers received a training show up fee sufficient to cover the cost of take-up in even the lowest subsidy treatment. Thus, cash-on-hand is unlikely to interfere with take-up. Second, the variation in the timing of the reward for follow-through provides a separate test for the screening effects of known payoffs at take-up, which does not depend on immediate concerns about liquidity. Importantly, the reward is paid after a year, and thus the response to it at the time of take-up should not be contaminated by immediate liquidity constraints. As noted above, we see no difference in the response of follow-through to the value of the reward based on the timing of the reward announcement (see Appendix Table A.4.2).

Our design also allows us to investigate selected psychological effects associated with the initial price paid for the technology. First, sunk cost effects would cause higher follow-through among adopters who pay more for take-up, because adopters would consider their expenditure at take-up when making their follow-through decision (Ashraf et al. 2010; Berry et al. 2012; Cohen and Dupas 2010). Since we do not find an effect of subsidy on follow-through decisions, we can rule out sunk cost effects. Second, farmers could extract information about the quality of the technology from the NGO's decision to subsidize (Rao 2005). If higher subsidies accompany better technologies, then farmers in a higher subsidy condition might have a higher follow-through (tree survival). Again, the absence of an effect of subsidies on follow-through decisions makes this effect unlikely. Third, if paying farmers to take up the technology crowds out their intrinsic motivation for the technology, we might see higher subsidies leading to lower follow-through (Deci 1971; Benabou and Tirole 2003). Because we observe no effect of the exogenous variation in take-up subsidies on follow-through, we are able to rule out all three of these explanations. Note that were we to observe a screening effect of the take-up price, we would not be able to distinguish it from these other channels using our design.

An alternative explanation that is inconsistent with our modeling framework is procrastination or hyperbolic time preferences. Sustained effort choices are frequently associated with time inconsistent behavior, in which the individual 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 take-up decision. We examine evidence for procrastination or hyperbolic time preferences by constructing two measures of procrastination from the survey data (see Appendix Table A.4.7), which differentiate to some degree between naive and sophisticated procrastinators.[35] 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. For the self-aware (sophisticated) 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 suggest at most a minor role for procrastination in driving take-up or follow-through outcomes.

5 Model, identification and estimation

The reduced form results in Section 4 provide evidence that is consistent with uncertainty in the opportunity cost of follow-through. However, they do not rule out that, in addition to uncertainty, other sources of heterogeneity in costs may explain the lack of screening effect of the take-up cost. For instance, a zero or even negative correlation between take-up prices and follow-through rates could emerge if there is a negative correlation between the privately optimal number of trees and the total profit farmers derive from them.[36] In addition, the reduced form results do not offer any insight into the magnitude of the uncertainty that farmers face since both the known and unknown components of the opportunity cost of follow-through appear to be uncorrelated with observables, and thus unobservable to the

[35] Our first measure 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 may miss naive procrastinators. As part of the endline survey, a series of questions about procrastination 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.

[36] A correlation (positive or negative) between the optimal scale and the level of profit can emerge from the joint distribution of the primitive parameters that govern a profit function (e.g. marginal costs, fixed costs, marginal benefits, etc.). For instance, Suri (2011) finds that low adoption rates of hybrid maize among farmers who seem to have high returns from adoption can be traced to a positive correlation between fixed costs and marginal benefits from adoption using a random coefficients model.

researcher in the absence of a revealed preference approach. To address these remaining questions, we adapt our simple theoretical model described in Section 2 to our empirical setting and explicitly estimate the distribution of random parameters governing a quasi-profit function (a "reduced form" profit function).

5.1 Farmer net benefits

General profit function. We begin with a general characterization of a farmer profits at time $t = 1$ as a function of the number of trees she decides to plant and care for:

$$\tilde{\Pi}(N) = \left[\sum_{t=7}^{\bar{T}} \frac{1}{(1+r)^t}\left(\alpha_0 N - \alpha_1 N^2\right)\right] - \gamma_0 N - \gamma_1 N^2 - \gamma_2 \times \mathbf{1}(N > 0) \qquad (2)$$

where N is the number of trees, r is the annual discount rate, α_0 and α_1 govern the yearly flow of benefits from trees (which begin seven years after the tree is planted), and γ_0, γ_1 and γ_2 govern variable and fixed costs of planting and caring for the trees, all of which occur in the first year. Equation (2) describes a convex function in the number of trees cultivated provided that all parameters are positive and $\tau\alpha_0 - \gamma_0 \geq 0$, where $\tau = \sum_{t=7}^{\bar{T}} \frac{1}{(1+r)^t}$.

The solution to the profit maximization problem defined by (2) is:

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

The existence of interior and corner solutions to this function is consistent with two empirical observations: many farmers choose to cultivate zero trees and a number of farmers find it optimal to cultivate between zero and 50 trees (the number of seedlings they receive) in the absence of an external incentive.[37]

The differences in tree choices across farmers could emerge from heterogeneity in some or all parameters in (2). Our experimental variation, however, does not allow us to separately identify heterogeneity along all of these dimensions. We therefore turn to a quasi-profit function that uses our experimental variation to characterize farmer heterogeneity along two important dimensions of the farmer's profit maximization problem: the interior solution and the profit level evaluated at the optimal number of trees.

[37]See Appendix Figure A.3.1.

Quasi-profit function and farmer's decision at $t = 1$. We now define the quasi-profit function, $\Pi(N)$, as

$$\Pi(N) = N - \frac{1}{2T_i}N^2 - F_i \times \mathbf{1}(N > 0) + \mathbf{1}(N \geq \bar{N})R_i \tag{3}$$

where $\bar{N} = 35$ is the threshold that triggers the reward, and R_i is the reward. The same interior and corner solutions conditions delivered by (2) are generated by (3) whenever $R_i = 0$. The quasi-profit function in (3) allows for heterogeneity across farmers in the two "reduced form" parameters (in the structural sense), T_i and F_i. T_i corresponds to the interior solution, and F_i is a scaling or residual parameter that ensures maximum profits in the quasi-profit function coincide with profits in the generic quadratic function. A free correlation structure is key to their interpretation as reduced form parameters, since each is a function of several common structural parameters.[38] Even in the absence of uncertainty, a negative or zero correlation between them could generate the type of selection patterns we observe in the data: zero correlation between the take-up cost and reaching the tree survival threshold.

The advantage of the quasi-profit function (3) over (2) is that the joint distribution of its two random parameters is identified out of the variation induced by our experiment: the last term in (3) corresponds to the exogenous reward, which we vary randomly across farmers. The reduced form nature of (3) means that we do not need to specify which structural parameters in (2) are driving the variation in choices. And yet, since (2) and (3) share the same value at the optimal solution, we can still use (3) to evaluate farmers' profits under the more general profit function (2).

Tree survival as a farmer decision. Throughout our estimation, we assume that tree survival is deterministic conditional on farmers' costly effort. As we explain in greater detail in Appendix 3, this assumption is also consistent with a model where survival is probabilistic and the probability of survival is a convex continuous function of effort, e, up to $\tilde{e}$, where it attains one. Farmers would respond to such probability profile by investing the minimum effort that guarantees survival, $\tilde{e}$, in all trees they choose to cultivate.[39] Empirically, the small bunching of tree survival at 35 (the reward threshold) we observe in the data is consistent with either deterministic survival or this restrictive form of probabilistic survival (see Appendix

[38]Especifically, $T = \frac{\tau\alpha_0 - \gamma_0}{2(\tau\alpha_1 + \gamma_1)}$ and $F = \gamma_2 - \left(1 - \frac{1}{(\tau\alpha_0 - \gamma_0)}\frac{(\tau\alpha_0 - \gamma_0)^2}{4(\tau\alpha_1 + \gamma_1)}\right)$.

[39]Except, perhaps, one of them, as is explained in Appendix 3.

Figure A.3.1).

5.2 Dynamics and take-up decision

As in the conceptual model, we assume the farmer makes adoption-related decisions in two periods: $t = 0, 1$. The random parameter F_i, which largely determines the magnitude of optimized profits, is divided into two additive components: F_{0i} and F_{1i}, where F_{0i} is known at all periods and F_{1i} is known at $t = 1$ but not at $t = 0$ (i.e. the unknown component of profit).[40] In addition, we assume that T_i is known to the farmer at all times. This amounts to assuming that there is uncertainty about the net costs of tree cultivation, but not about the optimal scale of the technology. The advantage of this particular structure of information is that it allows us to nest a model without uncertainty (i.e., $\text{Var}(F_{1i}) = 0$) that could also deliver no screening effects (or even negative screening effects) within our more general model. Moreover, because F_i is the predominant determinant of the maximum profit attainable, take-up decisions will be mainly affected by uncertainty in F_i rather than T_i; and thus assuming T_i is unknown at the time of take-up would not have an important impact in our results.

At $t = 0$, the farmer decides whether or not to pay to take-up the technology. At this point in time, the farmer has partial information about her net benefits from the contract. Assuming the farmer knows the distribution of F_{1i} conditional on F_{0i} and T_i at $t = 0$, the farmer chooses to take-up if

$$\delta \mathbb{E}_{F_{1i}|F_{0i},T_i}\left[\max_N \Pi(N|T_i, F_{0i}, F_{1i}, R_i)\right] - c + A_i \geq 0 \tag{4}$$

where c is the cost of the seedlings, A_i is the randomly determined subsidy, and $\delta = \frac{1}{1+r}$ and is assumed equal to 0.6.[41] Note that this representation of the net present value of farmer's profits from trees maintains the risk neutrality assumption from Section 2.[42]

[40] See the last paragraph of Section 3.2 for a discussion on our two-period assumption.

[41] Like Stange (2012) and others, we note that in the context of stochastic dynamic structural models the discount factor is not separately identified from the scale parameter of future period shocks. We used survey data on time preferences to inform our choice of 0.6, which is in line with observed interest rates in our setting and elicited individual discount rates in other rural developing country settings (Conning and Udry 2007; Cardenas and Carpenter 2008).

[42] As discussed in Section 2, this assumption is innocuous to the extent that the changes in income produced by our program are small relative to total income. The highest reward from our program is roughly 3.5 percent of average annual income.

5.3 Identification and estimation

Identification of the structural model consists of uniquely identifying the joint distribution of unobservables T_i, F_{0i} and F_{1i}. In addition to the above described assumptions on the timing of information, we maintain assumptions (i) and (ii) on the components of F_i from Section 2, and add the following assumptions:

(iii) No common shocks: $F_{1i} \perp F_{1j} \; \forall i \neq j$

(iv) Normality: $F_{0i} \sim n\left(\mu_F, \sigma^2_{F_0}\right)$, $F_{1i} \sim n\left(0, \sigma^2_{F_1}\right)$

(v) Joint normality: $(F_i, \ln T_i) \sim n(\mu, \Sigma)$

Below we explain the role that each of these assumptions plays in identification. In what follows, we denote the randomized values of A_i and R_i as a_i and r_i to emphasize their role as known (to the farmer and researcher) and exogenous.

With no assumptions other than profit-maximizing behavior on behalf of the farmer and a quadratic profit function that allows for corner solutions, the joint distribution of F_i and T_i can be non-parametrically identified in the subset of the support such that $\bar{N} < T_i < 50$. To see this, consider the follow-through decision of the subset of the sample for which

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

such that there is no self-selection on take-up. Within this subset of the sample, we can use the variation in r_i to identify the joint distribution of (F_i, T_i). For this group, the probability of cultivating $N^* = n > \bar{N}$ trees when $R = r_i$ can be written as

$$\Pr(N^* = n; R = r_i) = \Pr\left(F_i < r_i + \frac{1}{2}n \middle| T_i = n\right) \Pr\left(T_i = n\right). \tag{5}$$

Because the left hand side of (5) is empirically observable, increments in r_i, holding n constant, trace out the conditional distribution of F_i given T_i. The same expression can then be used to recover the marginal distribution of T_i by varying n and dividing by the conditional distribution of F_i. Since non-parametric identification of the joint distribution of F_i and T_i occurs only in the subset of the support such that $\bar{N} < T_i < 50$, additional parametric assumptions are required to fully characterize these distributions. We therefore adopt assumption (v) for the estimation.

We use farmers' take-up decisions in combination with assumptions (i)-(iv) in order to separately identify the distributions of F_{0i} and F_{1i}, once the joint distribution of F_i and T_i has been identified. Under these assumptions, the decision to take-up in response to r_i and a_i provides independent identification of the distribution of the known component of F_i, F_{0i}. More formally, identification of the distribution of F_{0i} is obtained from the decision to take-up, which is characterized by the inequality in (4). The left side of (4) is a known function of the random variable F_{0i}: note that parameters $\mu_F, \sigma_F^2,\ \mu_T,\ \sigma_T^2$, and $\rho_{T,F}$ can be treated as known since they are identified from tree survival as described above. Denote this function $h(F_{0i}; r_i)$, so we can rewrite (4) as

$$h(F_{0i}; r_i) \geq c - a_i. \tag{6}$$

The right side of (6) can take one of four known values, $a_i \in \{0, 4000, 8000, 12000\}$. The left hand side of (6) is known up to F_{0i} and varies across individuals in response to the known cost determinant, r_i. Provided that $h(F_{0i}; r_i)$ is invertible,[43] we can identify the distribution of F_{0i}, from the random variation in a_i and r_i:

$$\Pr\left(F_{0i} \leq h^{-1}(c - a_i, r_i) | \mu_F, \sigma_F^2, \mu_T, \sigma_T^2, \rho_{T,F}\right) = \Pr(TakeUp_i | a_i, r_i). \tag{7}$$

Common shocks and mean shift model. Assumption (iii) plays an important role for identification, as it implies that the variance of F_i across farmers is the sum of the variances of its two components: $\sigma_{F_0}^2 + \sigma_{F_1}^2$.[44] The variance of shocks is thus partially identified from subtracting the variance estimate of F_{0i}, identified from (7), from the variance of F_i, identified from tree choices. Shocks that are common across farmers do not translate into variance in tree choices, and would lead to an underestimate of $\sigma_{F_{1i}}^2$. In our context, much of the uncertainty farmers face appears to be from idiosyncratic shocks. According to our survey, two-thirds of respondents list health problems as their household's greatest hardship, almost 50 percent of households report losing cattle or livestock to death or theft during the past year, and 10 percent of households report the death or marriage of a working age member.[45] However, given that farmers are also likely affected by common shocks such as rainfall patterns and commodity prices (both of which are likely to have an idiosyncratic component), we estimate a variant of our model that allows for a specific type of common

[43]It can be shown that there exists some $\bar{f}$ s.t. $h(F_{0i}; r_i)$ is strictly monotonically decreasing on $(-\infty, \bar{f})$.
[44]Assumption (iii) is necessary for maximum likelihood estimation.
[45]See Section 7 for further discussion of the relevance of common and idiosyncratic shocks in our setting.

shock: one that is completely unforeseen at the time of take-up and is common across all farmers. This model variant can be estimated by relaxing assumption (ii), i.e. allowing for subjective and objective expectations about the mean of the shock to differ. Thus we refer to this model as the *mean shift model*. The mean shift – or difference between the subjective mean of the shock distribution at take-up and its objective mean – is identified because the random variation in r_i and a_i allows us to identify μ_F in (7) from the take-up decisions independently of the estimate from tree choices. In addition to allowing us to incorporate a type of common shock, the mean shift also captures any over-optimism or experimenter demand effects that are common across farmers. These behaviors will share the same structure as the unforeseen common shock: the subjective mean will differ from the objective mean of the opportunity cost distribution.

Estimation. We estimate the model using simulated maximum likelihood. The log-likelihood function is over observations of the number of trees, $N = 0, ..., 50$, and the participation decision, $DP = 0, 1$. The sample includes the 1,314 farmers who made a take-up decision. Because the farmer cultivates no trees whenever she chooses not to participate, the support of this bivariate vector is given by the 52 (DP, N) pairs: $(0, 0), (1, 0), (1, 1), (1, 2), ..., (1, 50)$.

$$\begin{aligned} l(\xi; DP, N) &= \\ &\sum_{i=1}^{M} \left\{(1-DP_i)\ln(1-\pi_{P,i}) + DP_i \ln(\pi_{P,i}) + DP_i \sum_{j=0}^{50} \mathbf{1}(N_i = j) \ln \Pr(N = j)\right\} \end{aligned} \tag{8}$$

where $\xi = (\mu_F, \sigma^2_{F_0}, \sigma^2_{F_1}, \mu_T, \sigma^2_T, \rho_{T,F})$.

We use numerical methods to minimize the negative of the simulated log-likelihood. For each likelihood evaluation, we use 1,500 draws of (T_i, F_{0i}, F_{1i}). Within each likelihood evaluation and for each draw of (T_i, F_{0i}, F_{1i}), the expectation on the right hand side of equation (4) is numerically computed using 100 draws of (T_i, F_{1i}) conditional on the draw of F_{0i}.[46] Standard errors for the estimated parameters are obtained as the inverse of the inner product of the simulated scores.[47]

[46]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. A further discussion of the estimation algorithm can be found in Appendix A.2.

[47]See Appendix A.2 for a more detailed description of our standard error calculation.

6 Structural estimates and simulation results

In this section, we describe the structural estimates and carry out simulations to assess the importance of uncertainty in farmer's decisions relative to other forms of heterogeneity.

6.1 Structural estimates and model fit

Table 3 shows the point estimates for the main parameters described in Section 5.3.[48] Panel A shows the estimates of our baseline model, which assumes that farmers' expectations about F are correct and consistent over time. Panel B shows the results of allowing for an unexpected common shock to all farmers at $t = 1$ (a mean shift). Because point estimates are somewhat hard to interpret (e.g. the μ_T and σ_T parameters do not correspond to the mean and standard deviation of the log-normally distributed parameter T), we convert the estimated parameters into more easily interpretable outcomes using simulation.[49] The estimated joint distribution of T and F shown in Panel A is such that the mean ex-post privately optimal number of trees is 8.46 (s.d. 14.64), with about 59 percent of farmers choosing to cultivate no trees.[50]

This joint distribution also implies that the average ex-post private profit from the optimal number of trees is 108.39 thousand ZMK (about 23.50USD). However, ex-post private profits vary widely across farmers: the s.d. is 185.47 thousand. Importantly, the model estimates that about 39 percent of the variance in ex-post profits is due to new information that emerges after the take-up decision is made. To put the importance of the flexibility associated with waiting for new information into perspective, our model results imply that, relative to a scenario in which the farmer had to pre-commit to a particular number of trees prior to take-up, 26 percent more farmers are willing to take-up and 68 percent of farmers end up growing a different number of trees when the number can be adjusted based on new information.

The variance of shocks to information about costs is partially identified out of the difference in the variances of expected profits at take-up ($t = 0$) and ex-post profits at the time

[48] There are two remaining parameters that are omitted from Table 3 for the sake of brevity, but discussed in detail in Appendix A.2: these are the surprise reward parameter, α_S, which allows the group of farmers who did not know of the possibility of a reward at take-up to differ in their expected benefits from the contract; and the unannounced monitoring treatment parameter, α_m, which allows for those monitored to experience increased or decreased motivation to care for the trees.

[49] The point estimates in Table 3 can be used to simulate farmer's draws of F and T. These draws are then used to compute optimal tree cultivation decisions that account for interior and corner solutions, which are then be plugged back into the profit function to compute maximized profit. The statistics presented here correspond to means and variances from 10,000 simulated draws.

[50] These statistics assume that farmers can plant a maximum of 50 trees. Although we allow for the distribution of T to be unbounded, we present statistics of the bounded distribution because we fit the econometric model using only this range of outcomes. According to our estimates, about 6 percent of the farmers would choose a private optimum of 50 or more trees.

Table 3: Structural parameter estimates

Parameters in T			Parameters in F					
μ_T	σ_T	ϱ	μ_F	σ_{F0}	σ_{F1}	α_s	α_m	μ_{Fs}
Panel A. No Mean Shift								
3.539	1.401	0.818	107.58	307.87	211.42	-91.79	-238.40	-
(0.057)	(0.066)	(0.066)	(11.822)	(93.278)	(49.953)	(16.222)	(73.887)	-
Panel B. Allowing for Mean Shift								
3.579	1.392	0.835	74.48	290.06	193.05	-54.42	-229.53	53.29
(0.071)	(0.075)	(0.073)	(15.47)	(84.622)	(45.427)	(20.47)	(74.444)	(26.761)

Notes: Parameters fitted by simulated maximum likelihood using 1500 draws of the random vector (F_{0i}, F_{1i}, T_i), with smoothing (lambda is 0.5) and tolerance (1e-15). The baseline model (Panel A) restricts the mean of F_i to be the same in both time periods. The mean shift model (Panel B) allows the mean of F_i to differ between the two periods, and the parameter F_shift to capture this difference. The log-likelihood value for the baseline model is 11142.064, while it is 11138.996 for the mean shift model.

the follow-through decision is made ($t = 1$) and partially identified out of its non-linear effect on the mean level of the expected profits at take-up.[51] In the absence of common shocks, the variance of the unobserved component of shocks will be correctly identified. The presence of common shocks generates a tug-of-war between these two sources of identification: the expected value of the profits pulls the variance of shocks parameter, σ_{F1}, up while the ex-post variance in profits, which does not reflect the variance of common shocks, pulls σ_{F1} down. Our mean shift model helps us explore the importance of the bias in σ_{F1} generated by the presence of common shocks, by allowing the mean level of profits to be different at $t = 0$ and $t = 1$.

The corresponding results from the mean shift model are presented in Panel B of Table 3. The estimated difference in means between the two periods, the mean shift, is given by parameter μ_{F_S}. A non-zero value for this estimated parameter has two plausible interpretations. First, it can represent a single common shock whose possibility was unknown at the time of take-up and affected all farmers equally. Second, it can pick up a common update in the value for the technology that occurred after the take-up decision was made. The latter interpretation is a useful test for the presence of experimenter demand effects on take-up: the perceived obligation of potential adopters to take-up in the presence of the experimenter. Our estimate of the value for μ_{F_S} is positive but small and not significantly different from zero at standard confidence levels. The positive value is consistent with either the presence of a common shock of the specific type described above or an experimenter demand effect. Note,

[51] Recall that a higher variance in shocks results in a larger option value for the contract.

however, that in either case, it is small compared to the standard deviation of the shocks, σ_{F_1}. And, importantly, allowing for this effect induces a small change in the variance of the shocks: σ_{F1}, falls moderately from 211.42 to 193.05, which is consistent with a positive bias in our baseline model stemming from the presence of common shocks.[52] This suggests that uncertainty in the form of idiosyncratic shocks is important and that experimenter demand effects are not driving our results.

We now turn to the interpretation of the parameters that govern the distribution of known (at the time of take-up) sources of heterogeneity across farmers. We estimate a high positive correlation between F and $\ln T$, $\rho_{T,F} = 0.81$ ($\rho_{T,F} = 0.83$ in the mean shift model).[53] Because F and T are reduced form parameters (see Section 5.1), the positive correlation between them could stem from two sources: a positive correlation between farmers' fixed costs and farmers' interior solution to the maximization problem (a component of the reduced form random parameter, F, is the fixed cost of the generic quadratic profit function), or a high mean and high variance in the linear term of net marginal returns (the term $\tau\alpha_0 - \gamma_0$ in the reduced form expression for T and F).[54]

Because F enters negatively in the profit function, the positive correlation between T and F translates into a negative correlation between the privately optimal number of trees and the level of profits. This negative correlation generates low follow-through rates (as in low numbers of trees cultivated) among farmers whose expected value from the contract is high and thus take-up under a high price. In other words, the static heterogeneity identified by the model induces a negative screening effect of high prices at take-up. Note that in the reduced form results we find no statistically significant screening effect of take-up prices on follow-through. Given our results so far, this close-to-zero screening effect is the result of both large variance in the unobserved component of follow-through costs, which according to our model results in a positive but small screening effect, and a negative screening effect stemming from static heterogeneity. However, without further analysis, it is hard to assess the importance of uncertainty in driving the statistically insignificant screening effect. After all, it could be that given the other forms of heterogeneity in our model, the level of uncertainty exerts little influence on self-selection in our particular context. In order to better understand the role

[52] Unfortunately, we cannot calculate the share of the variance attributed to common shocks using model estimates since we are not explicitly modeling random common shocks with a known distribution at the time of take-up.

[53] F_1 is assumed to be orthogonal to F_0 and T. Thus, the correlation between F and T stems solely from the correlation between F_0 (the known component of F) and T.

[54] We verified this is the case for plausible distributions of the deep structural parameters via simulation. The term $\tau\alpha_0 - \gamma_0$ enters linearly in T and non-linearly in F. Thus, if this term is high in average and has high variance, it may generate a negative relationship between T and F.

of uncertainty on self-selection in our outcomes, Section 6.2 conducts several simulations of take-up and follow-through outcomes where we hold constant the parameters that define the static heterogeneity in our data and vary the level of uncertainty.

Before turning to model simulations, we assess the fit of our model. Our model does well in predicting some simple observations in the data. For example, our baseline model predicts that 1,104 farmers (1,112 under the mean shift model), out of a total of 1,314, will take-up; our data show 1,092 participants. Our model also predicts that 173 out of the 963 farmers that faced a strictly positive take-up cost (i.e. a subsidy less than 12,000 ZMK) will choose to cultivate zero trees (180 in the mean shift model), while the observed number of farmers in this category is 112. That is, the dynamics in our model replicate an observation that seems at odds with rationality in a static framework: some farmers who purchase the trees choose not to cultivate them. In this sense, our result parallels the result by Fafchamps (1993) in that individuals make costly choices to maximize future flexibility.

Next, we further examine the model fit by comparing the reduced form treatment effects using simulated outcomes from the estimated model and the observed data. Most of the magnitudes and signs between the treatments and outcomes are well matched by our model estimates. Panel A of Table 4 shows results with the observed data, while Panels B and C show the corresponding simulations using the estimates from Table 3. Columns 1-4 estimate the effect of a thousand ZMK increase in the subsidy on take-up and follow-through outcomes. Columns 5-8 repeat the regressions with the reward (in thousand ZMK) on the righthand side. The effect of the subsidy and the reward on take-up (columns 1 and 5) and whether the farmer reached the 35-tree threshold (columns 2 and 4, conditional on take-up) are similar across the observed and simulated data. The effect of the reward on zero tree survival is also very similar in the observed and simulated data.

There are, however, some discrepancies between what our model predicts and the observed data. Columns 3 and 7 show the effect of the take-up subsidy and reward on the number of surviving trees, excluding zeros (which are shown in columns 4 and 8). Interestingly, the sign of the coefficient on the subsidy is different between the observed and simulated data, though standard errors are relatively large. The effect of the reward on the number of trees (column 7) is larger in the simulated data, consistent with the effect on reaching the 35-tree threshold. Finally, we see evidence that the take-up subsidy selects for farmers more likely to keep zero trees alive (column 4) in the simulated but not in the observed data, indicating some screening effect of take-up prices on abandoning the technology altogether in the simulated data only. Using simulations, we explore whether some

Table 4: Comparison of structural and reduced form estimates

	(1) Take-up	(2) Follow-through	(3) # trees | # trees>0	(4) 1.(zero trees)		(5) Take-up	(6) Follow-through	(7) # trees | # trees>0	(8) 1.(zero trees)
					Panel A. Observed Data				
Take-up subsidy	0.022***	-0.004	-0.229	-0.003	Reward	0.001*	0.001***	0.044***	-0.001***
	(0.005)	(0.004)	(0.200)	(0.005)		(0.000)	(0.000)	(0.013)	(0.000)
Observations	1,314	1,092	701	1,092		624	1,092	701	1,092
R-squared	0.071	0.002	0.005	0.001		0.006	0.018	0.022	0.019
					Panel B. No Mean Shift				
Take-up subsidy	0.020***	-0.003	0.044	0.010***	Reward	0.001*	0.003***	0.092***	-0.001***
	(0.002)	(0.003)	(0.130)	(0.003)		(0.000)	(0.000)	(0.012)	(0.000)
Observations	1,314	1,126	631	1,126		624	1,126	631	1,126
R-squared	0.064	0.001	0.000	0.008		0.005	0.105	0.083	0.011
					Panel C. Allowing for Mean Shift				
Take-up subsidy	0.020***	-0.003	-0.002	0.008**	Reward	0.001*	0.003***	0.094***	-0.001***
	(0.002)	(0.003)	(0.124)	(0.003)		(0.000)	(0.000)	(0.012)	(0.000)
Observations	1,314	1,120	605	1,120		624	1,120	605	1,120
R-squared	0.062	0.001	0.000	0.006		0.006	0.107	0.089	0.013

Notes: This table shows coefficients from regressions of each of four indicator variables (take-up, follow-through (threshold), tree survival larger than zero, and no tree survival) on each of our randomized treatments (take-up subsidy and threshold reward). Panel A shows these regression outcomes for the true data. Panel B and C show the fit of the structural model by simulating all four outcomes using the model estimates and examining the how much the linear relationships between outcomes and treatments resemble those in Panel A. Panel B uses baseline model estimates (Panel A of Table 2), while Panel C uses estimates from the mean shift model (Panel B of Table 2). Column 5 is estimated for farmers who learned about the reward before making their take-up decision only.

of the discrepancies between our estimated model and the data stem from the assumption of deterministic survival of the trees conditional on effort by introducing a stochastic component to tree survival outcomes, taking parameter estimates as given. We find little to no improvement when stochasticity is introduced into the tree survival outcomes (see Appendix A.3).

6.2 The effect of uncertainty on farmer profit and program outcomes

To better understand the importance of uncertainty relative to other multi-dimensional heterogeneity in our results, we use estimates from the structural model to simulate farmer profits and program outcomes (take-up and tree survival) under different levels of uncertainty. For these analyses, we use the results from the model with the mean shift parameter, such that both the mean and the standard deviation of F_i may vary over time (Panel B in Table 3). In the simulated results below, we set the mean shift for $t = 1$, μ_{F_s}, equal to zero, so that we can equate the expected benefits with the true average discounted benefits from the program.[55] Results from using our baseline model estimates are qualitatively similar. The simulations deliver five main results, the first three of which echo the findings from the conceptual model: (1) take-up is increasing in uncertainty, (2) higher take-up prices screen for high follow-through rates in the presence of uncertainty, and (3) the screening effect of price dissipates at high levels of uncertainty. Two additional policy-relevant results emerge from the simulations: (4) subsidizing take-up affects total trees both through its effect on take-up and on follow-through via any screening effect, and the former may dominate, and (5) rewarding follow-through directly may be more effective than subsidizing take-up.

Farmer profits

We begin by examining the effect of uncertainty on the average per-farmer expected private profit (right hand side of inequality 1) implied by the empirical model. In order to do so, we simulate the value of the expected profit for each farmer at different values of σ_{F1}. The relationship between the mean expected profit and σ_{F1} corresponds to the solid black curves in Figure 2. Panel A shows the relationship for a reward of zero and Panel B for the largest reward offered (150 thousand ZMK). Both are shown at a full subsidy, so that take-up is 100 percent (i.e. there is no self-selection based on take-up price).

[55] This treatment of the mean shift parameter is consistent with the common-shock interpretation of this parameter.

(1) Expected farmer profits are increasing in uncertainty. This result is analogous to the theoretical result discussed in Proposition 4: the option value from the contract increases with uncertainty and thus drives a positive relationship between the expected profit and uncertainty. The option value, as defined in Appendix A.1, is shown by the dashed lines in Figure 2 for different values of the reward. The option value is always non-negative, and is also the only component of the expected private profit that varies with uncertainty. Recall that this result stems from the asymmetric response of the expected profit to positive and negative shocks: if the realization of the random component of the profit drives it below zero, the farmer will respond by not cultivating any trees at all (effectively bounding the profit realizations at zero). This optimizing behavior turns the high variance of the shocks into an asset of the contract, which in turn results in higher take-up.

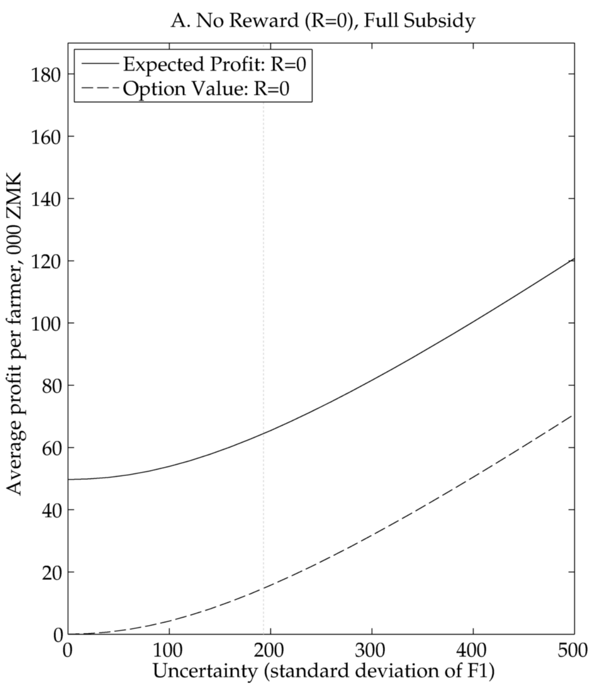

Figure 2: Farmer expected profit as a function of uncertainty

Notes: This figure shows a simulation of farmers' mean expected profit as we vary the level of uncertainty (the standard deviation of F_1). For the simulations, we use the estimated parameters from Panel B of 3, except for σ_{F_1}, which we vary along the horizontal axis. The mean per-farmer profit is shown in a solid black line for low (Panel A, $R = 0$) and high (Panel B, $R = 150$) reward values. Profits, the subsidy (A) and the reward (R) are expressed in thousand ZMK. The dashed lines show the mean option value for the two different reward levels. We define the option value as the value of re-optimizing the number of trees to cultivate after F_1 is realized relative to the value of a static choice.

The positive relationship between expected private profit and uncertainty has implications for take-up decisions: under higher uncertainty, more farmers are ex ante attracted to the contract, even though its ex post expected value is unchanged. A high enough level of uncertainty may result in an expected profit that exceeds even an unsubsidized take-up cost. Hence, the ability of the take-up cost to “tease out” those who will be more likely to reach the tree survival threshold is dampened by high levels of uncertainty. This can be observed more directly when looking at take-up and follow-through outcomes as a function of uncertainty, which we turn to next.

Program outcomes

Figure 3 plots average take-up at low and high subsidies (dashed and solid lines) and low (Panel A) and high (Panel B) rewards as a function of uncertainty (σ_{F1}).

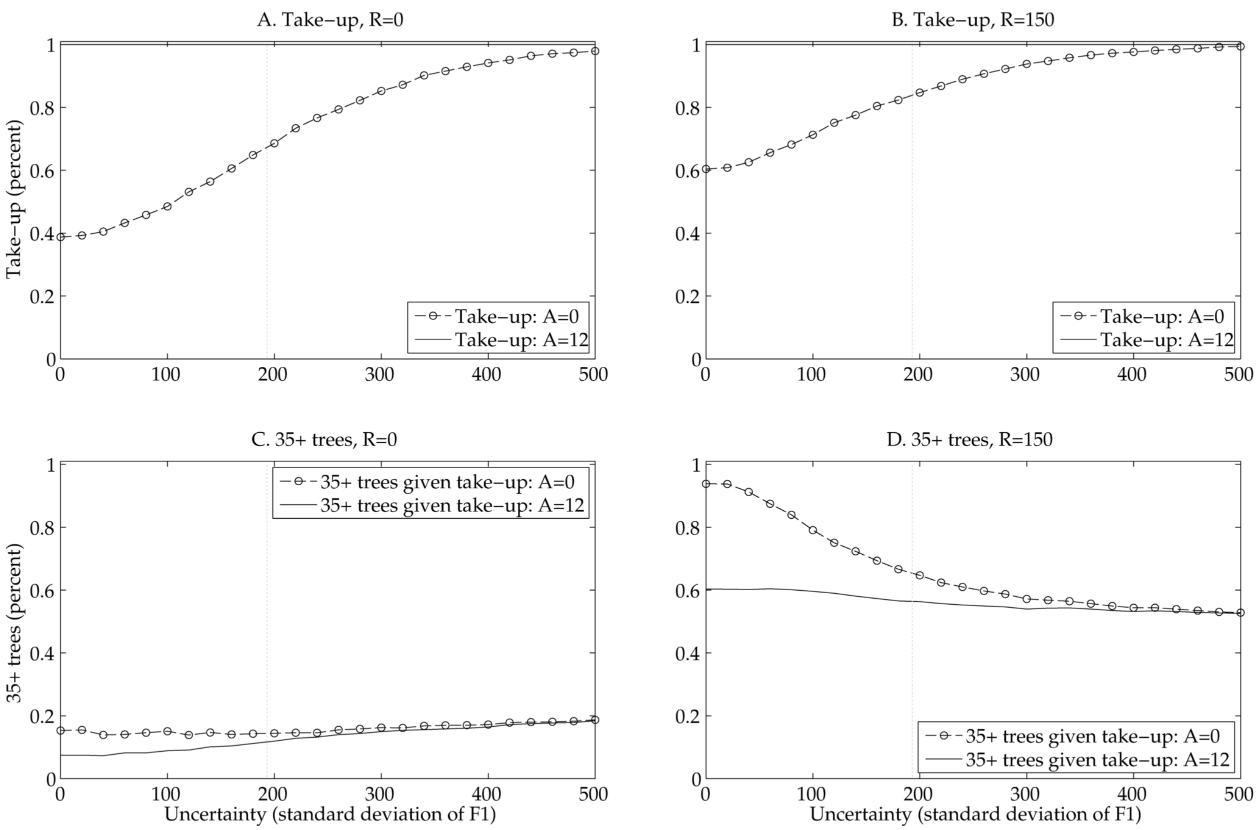

Figure 3: Take-up and threshold outcomes as a function of uncertainty

Notes: This figure shows a simulation of farmers' average take-up and 35-tree reward threshold outcome as we vary the level of uncertainty (the standard deviation of F_1). For the simulations, we use the estimated parameters from Panel B of Table 3, except for σ_{F_1}, which we vary along the horizontal axis. Take-up and threshold (trees $\geq$ 35) outcomes are shown for different combinations of the reward value and take-up subsidy, both of which are shown in thousand ZMK.

Panels C and D show the share of individuals who reach the 35-tree threshold conditional on take-up for the same combinations of subsidies and rewards. Figure 4 shows the effects of uncertainty on the average number of trees for different values of the subsidy (A) and reward (R). Panels A and B show the average tree survival, unconditional on take-up (non-participants have zero surviving trees). Panels C and D show average tree survival conditional on take up. Because take-up is 100 percent with a full subsidy (A=12), the solid lines in Panels C and D coincide with the solid lines in the Panels A and B, respectively.

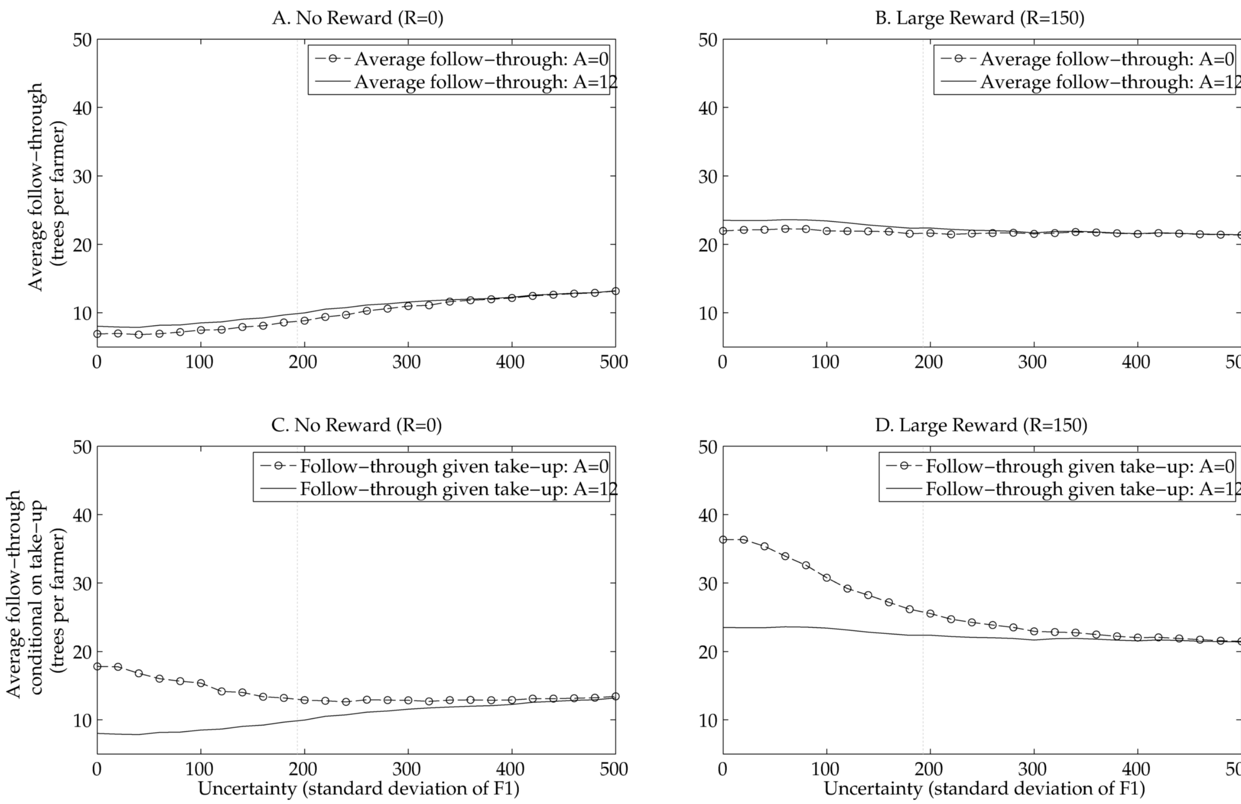

Figure 4: Tree survival as a function of uncertainty

Notes: This figure shows a simulation of farmers' mean number of surviving trees as we vary the level of uncertainty (the standard deviation of F_1). For the simulations, we use the estimated parameters from Panel B of Table 3, except for σ_{F_1}, which we vary along the horizontal axis. The mean number of surviving trees is shown for different combinations of the external threshold reward and the subsidy for take-up, both of which are shown in thousand ZMK. The top panels show per-farmer tree survival for all farmers (those who didn't take up have zero trees); the bottom panels show tree survival conditional on take-up.

(2) When uncertainty is low, a higher take-up price increases tree survival conditional on take-up. This result can be seen from the difference between the two lines in Panels C and D of figures 3 and 4 at low levels of uncertainty, and is what we refer to as the screening effect of price. This difference shows that follow-through rates are higher among those who face a high take-up price compared to those who face a low take-up price and is analogous to Proposition 1 in our conceptual model (Section 2).

(3) The screening effect of the take-up price falls substantially with high levels of uncertainty. This result can be seen from tracking the difference between the two lines in Panels C and D from low to high levels of uncertainty. For the level of uncertainty identified from our data ($\sigma_{F1} = 195$), the gain in tree survival from charging more at take-up is modest – less than 5 trees – and it continues falling as uncertainty increases. The reduction in the screening effect at high levels of uncertainty is analogous to Proposition 3 in our conceptual model. Importantly, our simulations suggest that even in the presence of static heterogeneity, uncertainty remains an important driver of the existence or absence of screening effect. To give a sense of its importance, note that halving the standard deviation of the shock relative to the value we estimate from the data would result in 15% higher follow through (Figure 3, Panel D).[56]

(4) The effect of a high subsidy on take-up may dominate its effects on screening. This result is most clearly seen by comparing unconditional tree survival (Panels A and B of Figure 4) with tree survival conditional on take-up, which excludes the take-up effect (Panels C and D). The boost associated with the selection effect observed at low levels of uncertainty in Panels C and D is more than offset by the take-up effect: many more farmers take-up when subsidies are high (see Panels A and B in Figure 3). The two counteracting effects lead to similar average tree survival across subsidy levels, unconditional on take-up (Panels A and B). Hence, for technologies whose benefits kick in with total follow-through (whether or not follow-through is spread about few or many adopters), subsidies may increase the benefits of adoption. Note, however, that uncertainty also lowers the take-up advantage of high subsidies, as take-up increases with uncertainty for all subsidy levels.

(5) When uncertainty is high, a reward conditional on follow-through is more effective at inducing tree survival than a subsidy. This result can be appreciated

[56]The reduction in tree survival in the low subsidy group (with strong self-selection) relative to the full subsidy group (without self-selection) is driven by a reduction in follow-through conditional on take-up for the highly self-seclected group (see Proposition 2). Note that without self-selection (i.e. if take-up is fully subsidized) it appears that follow-through increases slightly with the variance of shocks whenever the reward is low (solid line in C of Figure 3). We call this effect *the corner solution effect,* as it takes place when the sign of the condition for cultivating more than zero trees is to the left of the mean of F, i.e. for very low R. This effect emerges when we relax the assumption of binary shocks and we assume a distribution of shocks that is continuous and symmetric around the mean; and, as we can see from the negative slope of the low subsidy group response, is dominated by the self-selection effect.

when comparing the effect of moving from a lower R to a higher R as compared to moving from a low A to a high A. Even with the optimal (in our setting) combination of a low take-up subsidy and high threshold reward, uncertainty can bring down the number of farmers that reach the 35-tree threshold.

7 Discussion and interpretation

Our model and results are consistent with several interpretations of intertemporal adoption decisions and the nature of the uncertainty that farmers face. Our preferred interpretation is one of idiosyncratic and common shocks to the opportunity cost of follow-through with the technology, with idiosyncratic shocks playing a relatively more important role. We also describe suggestive evidence on whether the new information acquired after take-up is learned (i.e. persistent information) or transient.

7.1 Common vs. idiosyncratic shocks

In our surveys and in other developing country micro data, farmers frequently refer to shocks as a primary determinant of agricultural outcomes. The first version of our model treats the ex post variance in profits across farmers as the sum of the variances of the known and unknown components of the private profit, and therefore allows for idiosyncratic but not common shocks. This restriction on the distribution of the information revealed after take-up is relaxed in our second version of the model, where we allow for a specific type of common shock: an unexpected homogenous mean shift on the distribution of private profit after take-up. The data indicate that the mean shift, which could be interpreted as an unexpected common transitory shock or as homogenous learning (see below), is consistent with a common increase in costs after take-up. The estimated mean shift is small in magnitude relative to the variance (see Table 3). We interpret this estimate as evidence that this type of common shock is present but of limited importance.

The variation in our data does not allow us to identify other types of common shocks: namely, shocks whose distribution is known at take-up, or that are correlated across subsets of farmers (as opposed to all). Therefore, to assess the validity of our identifying assumptions, we consider two additional sources of information about the importance of common and idiosyncratic shocks: household self-reports from our surveys and the existing literature on agricultural productivity in rural Sub-Saharan Africa.

First, two of the most frequently 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. To the extent that the negative shock to cotton prices was very unlikely from the farmer's stand point, the mean shifter model is likely to capture its effects on farmers' decisions and to reduce the bias in our estimates of the variance of idiosyncratic shocks. Note, however, that to the extent that there are common shocks (such as crop prices and rainfall) whose variance is known and thus are incorporated in the take-up decision, our estimates for the variance of idiosyncratic shocks will continue to be biased upwards even in the mean shifter model.

According to our baseline data, idiosyncratic shocks are an important concern for farmers. When asked about the greatest hardships faced by their households, respondents describe idiosyncratic shocks. Two-thirds of respondents list health problems as their greatest hardship. At endline, almost 50 percent of households report losing cattle or livestock to death or theft during the past year, which is a substantial income shock, and 10 percent of households reporting the death or marriage of a working age member, which affects the opportunity cost of labor.

Second, a growing literature documents large negative effects of household illness on labor supply and agricultural productivity in the region of study (e.g., Fink and Masiye 2012) 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, we conclude that while common shocks are potentially an important source of uncertainty in agriculture, the fact that our model focuses on idiosyncratic shocks appears reasonable in this context.

7.2 Learning

Our theoretical and empirical models are similar, from an ex ante perspective, if the information that arrives between take-up and follow-through is a persistent (learning) or transient shock to opportunity cost, provided that both types of new information are independent across farmers. More specifically, our framework is equivalent to a standard learning model under an assumption of risk neutrality. In addition, to the extent that learning shocks are common across all farmers and unexpected, the mean shift model could account for learning. Interpreted this way, the positive mean shift estimate is consistent with farmers systematically underestimating the costs of follow-through at the time of take-up. The small estimated magnitude of the mean shift parameter would then imply little systematic updating across all farmers. Learning shocks that are independent across farmers and whose distribution is known by the farmers ex ante would show up in the variance of F_1. Thus, we cannot distinguish between persistent and transitory shocks to the opportunity costs of cultivating trees.

The potential for learning about the value of the technology during the first year of tree cultivation is limited given that tree survival benefits are not realized until several years after the end of the study, and planting and caring for trees resembles activities that farmers undertake regularly. Thus, if learning occurs, it is likely related to the opportunity costs of cultivating the trees, rather than the benefits. We use survey data to explore the extent to which baseline knowledge of the technology affects the results. Specifically, we expect that farmers who had more baseline knowledge have less to learn and are therefore less likely to end up with zero surviving trees if they take-up under a positive price.

We construct three baseline knowledge measures: (1) whether the farmer had any *Faidherbia albida* on their land at baseline, (2) whether anyone in the farmer group had *Faidherbia albida* on their land at baseline, and (3) whether the farmer could name at least one risk to tree survival at baseline. Appendix Table A.4.8 shows the results from a linear regression of the likelihood of having zero surviving trees on a knowledge variable, an indicator for paying a positive price and their interaction, conditional on take-up. Learning would predict a negative and significant interaction. We observe a negative and statistically insignificant interaction for the first two knowledge variables, and a positive and significant interaction for the third. In other words, we find no clear evidence that learning explains the pattern of paying to take-up and keeping zero trees alive, which can be explained by transient shocks that arrive after take-up.

8 Conclusion

This paper shows that uncertainty can play an important role in the adoption of technologies that require costly investments over time. We provide a broad framework for adoption decisions that allows for both time-invariant and time-varying heterogeneity as well as multiple dimensions of time-invariant heterogeneity across potential adopters. This framework applies to many adoption decisions in agriculture, development, environment, and health (for example, the adoption and adherence to medical treatments). In our conceptual model, we show that uncertainty in the opportunity cost of adoption can increase take-up rates at the cost of reducing average follow-through rates. High levels of uncertainty at the time of take-up provides an additional explanation to what has been discussed in the technology adoption literature for why charging higher prices may be ineffective at selecting for adopters likely to follow-through.

Findings from our field experiment show reduced form evidence that is consistent with our conceptual model, in the context of agricultural technology adoption. In Zambia, farmers decide whether to take-up a nitrogen fixing tree under considerable uncertainty about the benefits and costs of following through to keep the trees alive. 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 structural model also helps distinguish between alternative explanations for the absence of screening effects of prices. We find that, in our setup, a negative correlation between the intensive margin of adoption and the scale of profits counteracts the screening effect of the take-up price under uncertainty, which is already weak given the high levels of uncertainty that we observe. Simulations to calibrate the importance of uncertainty indicate that reducing the uncertainty in our setting by half would increase the number of farmers that reach tree survival threshold by 15 percent.

Our study is an example of how experimental variation can be used to identify dynamic structural models. The use of experimental variation in treatments at two different points in time offers an alternative to a panel data structure, since statistically independent samples are exposed to 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. One caveat of our basic identification strategy is that it relies on shocks being independent across farmers. Therefore, a variant of our model allows for a uniform common shock to farmers, provided that it is completely unanticipated (i.e. the subjective probability assigned by farmers is zero).

The combination of the experimental data with a structural model allows us to look beyond our study setting and simulate adoption outcomes under different levels of uncertainty. The simulations point to the potential for incentives placed directly on follow-through, conditional on take-up (the threshold reward in our experiment) to perform better than take-up subsidies in the presence of uncertainty. We stop short of drawing strong policy conclusions from these results, because our study was not designed to test optimal policy design to maximize follow-through or social welfare. While we can compare the cost effectiveness of subsidies applied to a fixed amount of take-up or to a follow-through threshold, there are a number of concerns about using our results to make statements about optimal policy design, including the specificity of the reward design that we implement.[57] Instead, we use the paper to make progress on identifying the role of new information in adoption dynamics, which has implications for future work to investigate optimal contract design in the presence of uncertainty at the follow through stage.

From a policy standpoint, uncertainty has the effect of lowering adoption outcomes per dollar of subsidy invested, while increasing the expected private profits to the adopter, because the downside risk of take-up is bounded at zero. To the extent that subsidies rely on public funds, an increase in uncertainty represents an ex ante transfer from the public to the private domain, driven entirely by the adopter's ability to re-optimize follow-through once new information becomes available. While stronger contracts that force adopters to follow-through once they take-up a subsidized technology would address the problem of high take-up coupled with low follow-through, they would do so at a clear cost to the adopter. Future research to explore more innovative solutions to encouraging both take-up and follow-through in the presence of uncertainty offers a promising direction for both environmental and development policies. For example, cheaper monitoring solutions that facilitate rewards for follow-through outcomes can have positive effects on both take-up and follow-through, as shown in our setting.

[57] A number of other features of our setting make us cautious about using our results to discuss optimal policy. For example, the fact that we cannot estimate marginal costs separately from marginal benefits limits our exploration of alternative contract structures. In addition, our assumption of risk neutrality would be less appropriate if we were to compare across contracts that impose different levels of risk. Finally, the optimal contract depends on whether the new information that arrives after take-up is a transient shock to opportunity cost or more permanent shock that affects subsequent take-up decisions. We are unable to fully distinguish between these interpretations.

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