

# Understanding heterogeneity: Risk and learning in the adoption of agricultural technologies

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## Abstract

Adoption rates of apparently high return agricultural technologies are puzzlingly low in many developing countries. While recent work has suggested that these average returns are consistent with substantial heterogeneity, relatively little is known about the mechanisms through which realized returns affect adoption decisions. In this paper, I derive testable predictions for models in which this relationship is driven by precautionary savings or by learning—mechanisms with distinct policy implications. These are tested using unique data on the Cocoa Abrabopa Association, which provided a package of fertilizer and other inputs on credit to cocoa farmers in Ghana. Exploiting the expansion of the program to identify impacts, I find high but heterogeneous returns. Low experienced returns among adopters are associated with low program retention rates, and patterns of returns and adoption are consistent with learning rather than precaution. This learning mechanism is also seen in the relationship between measures of subjective expectations and output realizations. Taking into account subjective uncertainty in the learning process, structural estimates suggest that farmers' choices are consistent with reasonable levels of risk aversion.

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# 1 Introduction

What constrains investment in agricultural technologies? This question is important—and stubbornly persistent—in development economics. Not only does agriculture continue to represent the primary source of income for many of the world’s poor, but low adoption rates of agricultural technologies, such as fertilizers and improved seed varieties, have accompanied the stagnation of agricultural productivity in Africa in particular (World Bank 2008).

There is little dispute that there exist agricultural technologies with high expected returns in many Sub-Saharan contexts. This view is supported by a growing body of evidence. Notably, Esther Duflo, Michael Kremer and Jonathan Robinson report experimental evidence of a mean seasonal return of 36 percent to fertilizer use among maize farmers in the Busia District of Kenya (Duflo, Kremer, and Robinson 2008). And yet rates of fertilizer use are low: fewer than 24 percent of farmers in Duflo and coauthors’ study had used fertilizer in the preceding year. Even where apparently high-return technologies do get adopted, many farmers abandon them. In a distinct sample of Kenyan maize farmers, Tavneet Suri documents that 30 percent of farmers switch into and out of the use of hybrid seeds in a given year (Suri 2011). In Ethiopia, Stefan Dercon and Luc Christiansen find that, while only 22 percent of farmers use fertilizer in a given year, a further 14 percent of farmers in the final round of their survey were not using fertilizer in spite of having done so in previous survey rounds (Dercon and Christiaensen 2011). Low rates of adoption and lack of sustained use of such technologies, combined with apparent high returns, therefore present a puzzle.

Several mechanisms have been put forward to explain observed patterns of agricultural technology adoption. Processes of social learning have been much studied (Bandiera and Rasul 2006, Conley and Udry 2001, Foster and Rosenzweig 1995, Munshi 2004). If social learning is sufficiently important, low-adoption equilibria may persist in spite of potentially high returns. Alternative theories include credit and supply-side constraints (Moser and Barrett 2006, Zerfu and Larson 2010). In Kenya, Duflo and co-authors find evidence consistent with the view that time inconsistency in farmers’ preferences causes inefficient adoption decisions (Duflo, Kremer, and Robinson 2009).

While they do address important elements of observed adoption patterns, these theories are generally not well equipped to explain why adoption is not sustained. In the most common form of learning model, for example, farmers only adopt technologies when they know how to use them effectively, and

this knowledge, once acquired, is never lost (Foster and Rosenzweig 1995, Jovanovic and Nyarko 1996). Likewise, instability in the supply of inputs alone seems an ad hoc explanation, and one incapable of explaining the widespread failure of farmers to persistently adopt profitable technologies even in cases where farmers have accessed them in the past. One exception is Dercon and Christiaensen (Dercon and Christiaensen 2011), who argue that year-to-year variation in the ability of households to bear risks associated with high-return technologies may explain instability in their use. Even so, if the typical farmer experiences such high returns as have been reported in the literature—a premise that we revisit in this paper—one would expect such technologies, once established, to pay for themselves. Indeed, these stylized facts lead Duflo to assert that “prima facie, neither limited liability nor risk aversion seem capable of explaining such a low level of fertilizer use” (Duflo 2006).

In this paper, I study the relationship between farmers’ experiences with a new technology and their decisions to continue its use. I distinguish two potential mechanisms relating ex-post heterogeneity in returns to subsequent adoption decisions: learning and precautionary savings. These two mechanisms have distinct policy implications. In the case of precautionary savings, the provision of insurance would promote efficient increases in adoption rates. In the case of learning, information is likely to be underprovided, but it is also possible that persistent heterogeneity makes adoption inefficient for some farmers.

We test between these mechanisms using a unique panel dataset that exploits the growth of a large scale non-profit initiative, the Cocoa Abrabopa Association, which alleviated credit constraints to the adoption of the *hi-tech* package of inputs (a specific combination of fertilizer, insecticide, and fungicide) among cocoa farmers in Ghana. We use the timing of the roll-out of this program to identify the effects of hi-tech adoption, and to test competing theories of the role of impact heterogeneity in determining subsequent adoption decisions. We supplement this analysis with survey measures of subjective probability distributions for potential outcomes both with and without the hi-tech package. Data on subjective beliefs allow us to show directly that expected returns respond to output realizations, and that once subjective uncertainty is taken into account, farmers’ decisions are consistent with levels of risk aversion similar to those found in laboratory experiments in developing countries (Harrison and Rustrom 2009).

Following the work of Heckman and coauthors, there is growing interest in models with “essential heterogeneity” (Heckman, Urzua, and Vytlačil 2006). In these authors’ framework, essential heterogeneity is defined by the

empirical relationship between ex-ante decisions of selection into treatment and the ex-post distribution of returns. In applications where agents learn about their (possibly heterogeneous) returns to a treatment over time, both the agents' information set and their economic constraints may be affected by the outcomes of experimentation. Long-term dynamics of adoption may then be governed by the way in which agents respond to experimentation—a possibility that requires us to distinguish the mechanisms linking ex-post heterogeneity to subsequent adoption decisions. Central in this analysis will be the degree of persistence in the process governing potential outcomes.

The role of treatment effect heterogeneity in explaining low adoption rates in African agriculture is understudied in the literature, with papers by Tavneet Suri (2011) and Duflo, Kremer and Robinson (2008) providing two notable exceptions. Using an observational, panel dataset of Kenyan maize farmers to estimate average returns for discrete subgroups, Suri finds a non-monotonic relationship between the adoption rates and expected returns of these subgroups in her data. Duflo, Kremer and Robinson (2008) use treatment interactions and quantile treatment effects to characterize heterogeneity in their experimental data. They find that net returns to fertilizer adoption are negative for 13.5 percent of farmers, in spite of the estimated 36 percent seasonal return.<sup>1</sup> These studies are silent on two issues: whether heterogeneity in the ex-post distribution of returns is driven by persistent differences across farmers or transient sources of risk, and whether the association between realized returns is driven by learning or by precautionary savings motives.

We take as our starting point that the literature is inconclusive on three questions: (1) How heterogeneous are the rates of return to agricultural technologies, such as fertilizer? (2) Does heterogeneity in returns affect the sustained adoption of such technologies, beyond farmers' initial experimentation? And (3) is this relationship between heterogeneity in realized returns and sustained technology adoption caused by prudence in response to transient shocks, or learning about persistent differences in the suitability of a technology across farms and farmers? Evidence presented in this paper suggests that heterogeneity in the returns to fertilizer use is substantial, it affects continued adoption, and it does so primarily through a process of learning.

The remainder of this paper proceed as follows. Section 2 provides a

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<sup>1</sup>As pointed out by Foster and Rosenzweig (2010), these “net” returns do not account for labor costs. If labor or other complementary inputs also increased in response to fertilizer use, then they will estimate the true net returns, and underestimate the fraction of farmers for whom net returns are negative.

theoretical basis for the empirical work, by drawing out the distinct implications of mechanisms of prudence and learning for the relationship between ex post returns and subsequent adoption decisions. Section 3 describes the specific intervention studied, the data and the quasi-experimental setting that will be used to estimate impacts. Section 4 presents our estimates of the average treatment effect, demonstrating robustness to a range of identifying assumptions, and providing evidence of heterogeneity across farmers. In Section 5, we test mechanisms of the relationship between realized gains from adoption and decisions to continue adoption, using data on membership decisions and survey measures of expected returns. Section 6 estimates farmer risk aversion using subjective expectations and observed choices. Section 7 concludes.

## 2 Theoretical framework

When the effects of agricultural technologies are heterogeneous, at least two theoretical mechanisms relate the ex post distribution of returns to experimenting farmers' decisions to continue adoption. On the one hand, such farmers may update their beliefs about the distribution of outcomes that they face based on these realizations.<sup>2</sup> Farmers experiencing low returns in a given period may become more pessimistic and decide to abandon the technology. On the other hand, even if the distribution of output under available technologies is known perfectly by all farmers, we may still see a relationship between ex post realizations of returns and decisions to continue adoption. As shown below, farmers who exhibit prudence—that is, who build a buffer stock in anticipation of the adoption of risky technologies (Kimball 1990)—may be deterred by low realized returns to adoption simply because it affects their ability to insure themselves against adverse events in the future. In this section we propose a stylized model of prudence and learning mechanisms, which can be used to distinguish empirically between the two.

These two mechanisms linking output realizations and adoption decisions have very different policy implications. When prudence drives the relationship between realized returns and subsequent choices, policies that insure farmers against downside risks can induce higher levels of adoption and consequent welfare gains. By contrast, in the learning case, and particularly

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<sup>2</sup>This may be true regardless of whether returns are homogeneous across farmers, but unknown in the population, or when the returns are farmer-specific. We return to this distinction below.

when returns are homogeneous but unknown, there may be a role for policy in resolving uncertainty by providing information about returns. Where social learning is possible, experimentation—a public good—is undersupplied.<sup>3</sup> If returns are both heterogeneous and unknown, policies designed to encourage all farmers to adopt a given technology may be inefficient. Instead, a second-order question becomes central to policy: are the returns themselves amenable to intervention?

These points are readily understood in a stylized model of a farmer’s decisions over consumption and technology adoption. Anticipating our empirical application, we consider the farmer’s decision between traditional, ‘low-tech’ technology,  $L$ , and a new, ‘high-tech’ technology,  $H$ , where the latter is characterized by a higher mean and higher variance. To highlight the contrast between precaution and learning mechanisms, we consider the implications each channel separately.

## 2.1 Output realizations and adoption decisions under precautionary savings

Consider first the case in which the distribution of outcomes under each technology is known by the farmer, so that prudence governs the relationship between realized income and adoption decisions.

At the beginning of the first period, the farmer is endowed with assets  $A$ , and receives a draw of income,  $y_{1H}$  from technology  $H$ . Given a concave utility function  $u$ , the farmer’s decision problem is to choose both a consumption level,  $c$ , and a second-period technology,  $w \in \{H, L\}$ , in order to maximize expected lifetime utility

$$V(c, w) = u(c) + E u(A + y_{1H} - c + y_{2w}), \quad (1)$$

where the expectation is taken with respect to the (possibly subjective) probability distribution for the chosen technology of second-period income. In the absence of learning, initial assets and first-period income affect decisions only through their sum (Deaton 1992).

Whether a high realization of income  $y_{1,H}$  induces farmers to continue using the new technology depends on the curvature of the farmer’s utility function and the distribution of potential outcomes  $y_{2,H}, y_{2,L}$ , as characterized in Proposition 1.

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<sup>3</sup>This undersupply is exacerbated when farmers engage in strategic delay (Bardhan and Udry 1999, Bandiera and Rasul 2006). In the simple model that follows, however, we focus only on learning by doing, so that such strategic considerations do not apply.

**Proposition 1.** *Income realizations and technology choice under precautionary savings.*

*In the farmer's maximization problem in equation (1) an increase in first-period income  $y_{1H}$  increases the expected utility of the high-tech technology  $H$  relative to the traditional technology  $L$  if and only if, for a given initial income and assets and second-period technology choice optimal consumption under  $H$ ,  $c_H^*$ , is lower than optimal consumption under  $L$ ,  $c_L^*$ .*

*Proof.* We are interested in the sign of  $\partial(V_H^*, V_L^*)/\partial y_{1H}$ , where  $V_w^*$  represents the value of the value function  $V$ , with consumption chosen optimally for a given technology choice  $w = H, L$ .

Note that, for a given technology choice,  $w$ , the first order condition from the choice of  $c$  is given by

$$u'(c) - E_w [u'(A + y_{1H} - c + y_{2w})] = 0 \quad (2)$$

which implicitly defines the  $c_w^*$  as a function of  $A + y_{1H}$ .

To understand what happens when  $y_{1H}$  increases, we differentiate the value function for a given prospect,  $w$ , to obtain

$$\frac{\partial V_w^*}{\partial y_{1H}} = E_w [u'(A + y_{1H} - c_w^* + y_{2w})] = u'(c_w^*), \quad (3)$$

where the first equality makes use of the envelope theorem and the fact that  $c_w^*$  is chosen optimally, and the second equality is obtained by substituting in the first order condition. Consequently we have

$$\frac{\partial(V_H^* - V_L^*)}{\partial y_{1H}} > 0 \iff u'(c_H^*) > u'(c_L^*), \quad (4)$$

which is equivalent to the condition that  $c_H^* < c_L^*$ , by the concavity of  $u$ .  $\square$

Whether this condition will hold depends on features of the utility function and on the properties of the distribution for the counterfactual outcomes  $y_{2H}, y_{2L}$ . A few conditions can be stated in general (Kimball 1990). First, the condition in equation (4) will only hold if the farmer's utility function exhibits *prudence*—that is, if the third derivative of the utility function is positive.<sup>4</sup> Second, if the distribution of  $y_H$  first order stochastically dominates  $y_L$ , then equation (4) will not hold. Third, if the distribution of  $y_H$  is a mean-preserving spread of the distribution under  $y_L$ , then condition (4) will hold. Intuitively, then, an increase in first-period income will have a greater

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<sup>4</sup>Specifically, Kimball (1990) proposes  $u'''/u''$  as a measure of absolute prudence.

effect on the likelihood of choosing a higher-mean, higher-variance technology,  $H$ , when farmers exhibit higher prudence, and where the increase in variance caused by moving from the distribution  $L$  to  $H$  is large relative to the increase in expected value.

The prudence mechanism draws no distinction between the effect of (liquid) assets,  $A$  and first-period income as determinants of the choice between second-period technologies. Consider an extension of the example above, in which in period 0 the farmer first realizes an income  $y_{0L}$  drawn from the traditional technology, before deciding a period 0 consumption level,  $c_0$  such that the assets at the beginning of period 1 are given by  $A = y_{0L} - c_0$ . Starting from an interior solution in which the constraint  $c_0 \leq y_{0L}$ , the marginal propensity to consume out of an increase in the realization of  $y_{0L}$  will be less than one. In this case, even past income realized under the traditional technology can be conducive to the adoption of the new technology.

## 2.2 Learning returns by experimentation, in the absence of precautionary savings

This result contrasts with the implications of a Bayesian learning mechanism. Under this alternative mechanism, farmers update beliefs about returns to a new technology on the basis of output realizations. To make this contrast as stark as possible, suppose that there is no savings technology, so that consumption and income in each period are equal. Consider again the decision of a farmer who has just experimented with technology  $H$  for the first time. This farmer holds a (subjective) prior distribution for high- and low-technology options, with cumulative distribution functions  $F_{H,0}, F_{L,0}$ , respectively, at the end of period 0. For simplicity, assume the farmer is myopic, in the sense that she decides whether to use technology  $H$  by comparing subjective expected utilities for the coming year with each technology (i.e., she does not value the knowledge generated by experimentation when making her adoption decision). Thus she will choose technology  $H$  in period 1 if

$$\tilde{V}_{H,0} \equiv \int u(y) dF_{H,0} dy > \int u(y) dF_{L,0} dy \equiv \tilde{V}_{L,0}. \quad (5)$$

Farmers who choose to adopt when technology  $H$  becomes available receive an output realization,  $y_{1H}$  that they can use to update their priors. This realization affects not only the expected value of  $y_H$ , but also the precision (defined as the inverse of the variance) of this subjective probability distribution.



When subjective probability distributions are normally distributed, closed forms allow sharp predictions to be drawn. This follows from two features of Bayesian-normal learning (see Chamley 2004 for an extensive discussion). First, the posterior precision is given by  $\rho_1 = \rho_0 + \rho_e$ , where  $\rho_1$  is the precision of the posterior belief,  $\rho_0$  is the precision of the prior, and  $\rho_e$  is the precision of the Gaussian signal. Importantly—and only for the normal distribution—the posterior precision does not depend on the actual information contained in the realized draw  $y_{1H}$ , which may be high or low, but only on its variance. Second, the posterior expectation,  $\mu_1$ , for Bayesian-normal updating is a weighted average of the signal  $y_{1H}$  and the prior  $\mu_{0H}$ , with weights proportional to the precision of each term:  $\mu_1 = \rho_0/(\rho_0 + \rho_e)\mu_{0H} + \rho_e/(\rho_0 + \rho_e)y_{1H}$ .

In the Bayesian-normal case, the posterior distribution for a farmer who receive a high signal  $y'_{1H}$  first order stochastically dominates the posterior distribution for a farmer who receives a low signal  $y''_{1H}$ , with  $y'_{1H} > y''_{1H}$ . Since risk-averse (subjective) expected utility maximizing agents will always choose a stochastically dominant distribution over a stochastically dominated distribution, it follows that—holding prior beliefs and returns to technology  $L$  constant—an increase in the output realization  $y_{1H}$  from which a farmer learns can only increase her probability of deciding to adopt. If this probability is increasing in the subjected expected utility differential, then the probability of adoption is strictly monotonically increasing in  $y_{1H}$ :

$$\frac{\partial \tilde{V}_{H,1} - \tilde{V}_{L,1}}{\partial y_{H,1}} > 0. \quad (6)$$

where  $\tilde{V}_{H,1}, \tilde{V}_{L,1}$  represent subjective expected utilities for technologies  $H$  and  $L$  under information available at period 1.

In general, the variance of the posterior belief will not be independent of the signal, and in some cases variance may actually *increase* after experimentation. The latter is true, for example, when agents' types are binary, and agents have prior beliefs that the probability of being a 'good' type is close to either zero or one. Even in this case, however, it remains true that the posterior distribution for agents receiving high signals first-order stochastically dominates the posterior distribution for agents receiving low signals.

In the absence of a precautionary savings mechanism, a learning mechanism gives no reason for past realizations of the traditional technology to be positively associated with the adoption of  $H$  in subsequent periods. On the contrary, if the distribution of  $y_L$  is also an object of learning, then high realizations of  $y_{0L}$ , defined as a draw from technology  $L$  in period 0, actu-

ally *reduce* the expected gain from choosing  $w = H$  over  $w = L$  in period 2. Thus we have:

$$\frac{\partial \tilde{V}_{H,1} - \tilde{V}_{L,1}}{\partial y_{L,0}} \leq 0, \quad (7)$$

with the inequality holding strictly if farmers learn from the realization  $y_{L,0}$  about the distribution of outcomes under the traditional technology.

This observation provides the basis of an empirical test. While both learning and precautionary mechanisms link income from experimentation with technology  $H$  to the decision to sustain its adoption, in the most plausible cases (that is, when agents exhibit prudence) they have opposite implications for the association between adoption of new technologies and previous yields under traditional technologies. We take this test to the data in Section 5.

### 3 Context and data

In 2006, the Cocoa Abrabopa Association (CAA), a not-for-profit subsidiary of Wience Ghana Ltd, began a program of distributing inputs on seasonal credit to cocoa farmers in Ghana. With the support of the Ghana Cocoa Board, CAA provided farmers with access to two acres' worth of a package of fertilizer, pesticides, and fungicides. This specific bundle of inputs, known as the *hi-tech* package, had been promoted by the Cocoa Research Institute of Ghana since 2001, though problems of poor repayment rates had limited distribution.<sup>5</sup> CAA provided these inputs to groups of between 8 and 15 farmers on a joint liability basis, with dynamic incentives: groups that failed to repay in full would be suspended for a minimum of one year, while those that repaid successfully would be given four acres' worth of inputs in the following year, subject to approval of a CAA field officer. In addition to these physical inputs, farmers in the first year of membership were advised on their proper application by a CAA promoter, and some business training would be provided by Technoserve Ghana. Judged by its expanding membership rolls, the program has been wildly successful: from 1,440 farmers in 2006, CAA expanded to a membership of 18,000 farmers by 2009 (Cocoa Abrabopa Association 2009).

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<sup>5</sup>Although the use of these broad categories of inputs is not new to Ghanaian cocoa farmers, the particular configuration was. Evidence from other contexts (Duflo, Kremer, and Robinson 2008) shows that economic returns can be highly sensitive to precise quantities and combinations of inputs used.

To identify the impact of CAA membership on farmer incomes, we took advantage of the fact that much of CAA’s expansion during this time was at an extensive margin: it involved expansion into new villages. CAA’s expansion operates on an annual cycle, as follows. Promoters first arrive in a new village in January of a given year, and by February farmers make their decisions to opt into the program (or not), forming groups accordingly. Inputs begin to arrive in May, but the harvest does not take place until October, with repayment of inputs due by December of that year.

We conducted surveys in September of 2008, 2009, and 2010—prior to the harvest for that calendar year, but after decisions about CAA membership had been made. In each wave of the survey, we conducted a representative sample of two types of villages: those that had been reached by CAA for the first time in the most recent completed harvest, and those that had been reached by CAA for the first time in the current year, i.e., the growing season that was ongoing at the time of our visit.<sup>6</sup> The former had experienced one full season since the arrival of Abrabopa and had made their membership decisions for the following season, but had not yet harvested any cocoa in the second year of exposure by the time of our visit. The latter had made membership decisions for the season in progress at the time of our survey, but had not yet experienced the results of those decisions.

In each type of village, we conducted representative samples of two populations of farmers: those who joined in the first year of its availability in their village, whom we call *initial adopters*, and those who opted not to join CAA in the same year, whom we refer to as *initial non-adopters*. The resulting sample used in estimation is given in Table 1.

As will be described in detail in the next section, we use a cross-sectional difference-in-differences approach to estimate the average effect of the first wave of Abrabopa membership on initial adopters. To do so, we pool data from both survey rounds. We then define two key variables. First, we define a dummy variable,  $z_{vt}$ , to indicate exposure to Abrabopa in village  $v$  and year  $t$ . Given the sampling strategy in Table 1, any village in our sample with  $z_{vt} = 0$  must have  $z_{v,t+1} = 1$ . Second, we define  $m_{ivt}$  as an indicator of the adoption decision of farmer  $i$  in village  $v$  and year  $t$ . To denote initial adopters, who adopt in the first year of exposure, we drop the time subscript

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<sup>6</sup>In the 2009 and 2010 surveys, we also revisited farmers from the previous survey who had by then been exposed to the program for two or more seasons. To focus on a comparable set of “early adopters”, and in light of the possible cumulative effects of sustained fertilizer use, we do not make use of these observations to estimate returns to adoption. See Opoku, Dzene, Caria, Teal, and Zeitlin (2009) for further details of the survey.

Table 1: Estimating sample, by survey round and membership classification

Season of first visit	Initial adoption decision	Observations, by survey round		
		2007	2008	2009
2007	Adopt	81	[41]	[39]
	Do not adopt	34	[36]	[31]
2008	Adopt	93	80	[54]
	Do not adopt	42	34	[51]
2009	Adopt	0	96	87
	Do not adopt	0	36	31

Note: survey round refers to the most recent completed harvest at time of each survey. Observations in brackets represent villages with two or more years of exposure at time of survey; these are not used to estimate average treatment effects among cohort of initial adopters.

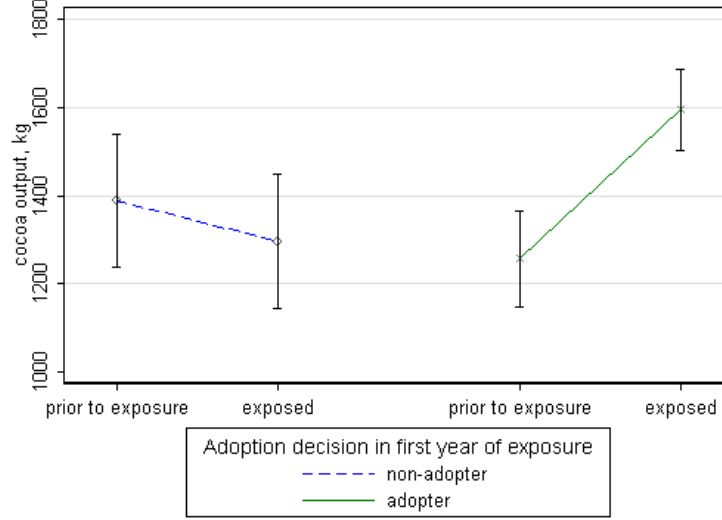
and define  $m_{iv} = \mathbf{1}\{m_{ivt} = 1, z_{vt} = 1, z_{vs} = 0\}$  for some  $t$  and for all  $s < t$ . Initial adopters choose to adopt in the first year of availability in their village, but not necessarily in subsequent years, as we shall see. In the sample used to estimate impacts on farmer production, we restrict attention to the year of exposure or the year immediately prior. Treatment  $w_{ivt}$  occurs in this sample when the individual is an initial adopter and Abrabopa is present in the village—that is, if  $w_{ivt} = m_{iv}z_{vt}$ .

The basis for our difference-in-differences identification strategy is illustrated in Figure 1, which shows our four categories of farmers. The first ( $z_{ivt} = 1, m_{iv} = 1$ ) are those who receive treatment (i.e., they have adopted the hi-tech package). The second ( $z_{ivt} = 1, m_{iv} = 0$ ) were offered the treatment but did not adopt. The third ( $z_{ivt} = 0, m_{iv} = 1$ ) are farmers who will adopt in the first year that Abrabopa reaches their village, but whose villages have not completed a season under treatment at the time of the survey round under consideration. And fourth are those farmers who will choose not to join in the first year of exposure, and whose villages have not yet been exposed to Abrabopa ( $z_{ivt} = 0, m_{iv} = 0$ ). We estimate impacts on output levels because the program provided inputs for a fixed quantity of land, irrespective of farmers’ cultivated area.

Given this setup, we can read off a difference-in-differences estimate of program impacts directly from Figure 1. We estimate the average treatment effect on the treated as

$$\begin{aligned} \tau_{ATT} = & \text{E}[y_{ivt}|m_{iv} = 1, z_{vt} = 1] - \text{E}[y_{ivt}|m_{iv} = 0, z_{vt} = 1] \\ & - (\text{E}[y_{ivt}|m_{iv} = 1, z_{vt} = 0] - \text{E}[y_{ivt}|m_{iv} = 0, z_{vt} = 0]). \end{aligned} \quad (8)$$

Figure 1: Average cocoa output, by treatment status in year of study



The first two terms of equation (8) give the differences in output between those who were early adopters and those who were not, in villages exposed to Abrabopa. This within-village difference nets out one potential form of selection bias, arising from any common, village-level productivity variables that may be correlated with the timing of Abrabopa exposure. Still, this may be a biased estimate of the treatment effect if relatively productive farmers were more likely to become members. By subtracting the second two terms of equation (8), the pre-treatment difference between early adopters and non-adopters in villages not yet exposed to Abrabopa, we can account for this second form of selection bias, arising from within-village selection. In the pooled cross-section without any controls, the average treatment effect of Abrabopa adoption in equation (8) can be read off from Figure 1 as 430 kg.<sup>7</sup> From the figure, neither form of selection bias appears very strong: in villages not yet experiencing output under treatment, those who go on to become early adopters are, if anything, worse off than non-adopters. More-

<sup>7</sup>Mean output levels for adopters are 1258 kg and 1596 kg, before and after exposure respectively. For non-members the corresponding output levels are 1389 and 1296 kg. This estimate differs from the estimate in column (1) of Table 3 only because the Table includes indicator variables for survey round.

over, non-adopters in villages under treatment are, if anything, worse off than non-adopters in villages not yet treated.

Sampled farmers also provided information on a range of socio-economic characteristics and agricultural practices. These data are summarized in Table 2, where we present summary statistics by initial adoption decision and Abrabopa exposure during most recent harvest prior to survey. Table 2 includes the *prima facie* evidence of Abrabopa’s impacts that underlies Figure 1: initial adopters’ output exceeds that of non-adopters in exposed villages in both survey waves, while early adopters’ output levels in the season before their village is exposed to Abrabopa are higher than non-adopters. Rates of fertilizer use are low (less than 50 percent) among farmers not yet reached by Abrabopa. Farms in this sector are typically small, ranging between three and four hectares, with initial adopters of the hi-tech package tending to have slightly smaller farms on average. Education levels are low, with fewer than 10 percent of farmers having attended post-primary or higher education, and approximately 20 percent of farmers in the sample are female.

We also use the difference-in-differences approach outlined above to estimate treatment effects across a range of characteristics.<sup>8</sup> Since household characteristics are not expected to be impacted by treatment, this provides a test of whether differences in observed variables confound our quasi-experimental design. We find statistically significant treatment effects only for output and fertilizer use. The absence of treatment effects on labor inputs is striking, since optimal labor choices could in principle respond to treatment. As compared with the distribution of labor inputs in the sample, estimated treatment effects on labor are relatively small, and are estimated with reasonable precision. We will return to the implications of this for evaluating returns to adoption in the following section.

Finally, the last two rounds of survey data collected direct measures of farmers’ beliefs about the distribution of their own potential outcomes, with and without the hi-tech inputs, in the following season. To do so, we followed an approach pioneered by Manski and coauthors (Dominitz and Manski 1997, Manski (Manski 2004)), and applied more recently to questions of development by Attanasio and Kaufmann (2009) and Delevande et al. (2009). Farmers were asked and yields attainable under each technology choice. Then placed probability weights on four equally-spaced intervals

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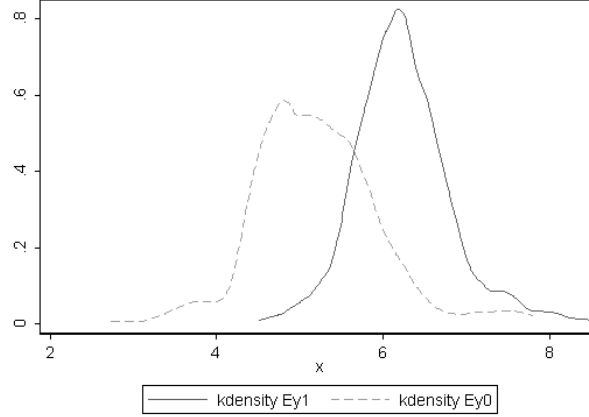
<sup>8</sup>Specifically, for each characteristic  $x_{it}$  of farmer  $i$  in wave  $t$ , we estimate  $x_{it} = \beta_{0t} + \beta_m m_{iv} + \beta_z z_{vt} + \tau m_{iv} z_{vt} + \varepsilon$ , where  $\beta_{0t}$  are wave-specific intercepts,  $m_{iv}$  identifies initial adopters,  $z_{vt}$  denotes village-level Abrabopa exposure, and treatment is defined by the interaction effect  $\tau$ .

Table 2: Survey characteristics, by treatment status

	non-adopter, before	non-adopter, after	adopter, before	adopter, after	$\tau$
cocoa output, kg	1388.88 ( 1260.80)	1296.11 ( 1518.03)	1257.66 ( 1504.78)	1596.03 ( 1443.13)	437.12** ( 214.63)
land devoted to cocoa, ha	4.01 ( 3.59)	4.12 ( 3.88)	3.39 ( 2.49)	3.44 ( 2.49)	-0.07 ( 0.66)
age	49.30 ( 12.68)	50.04 ( 13.18)	47.51 ( 13.54)	49.48 ( 12.99)	1.47 ( 1.46)
female	0.25 ( 0.44)	0.23 ( 0.42)	0.19 ( 0.39)	0.18 ( 0.39)	0.01 ( 0.06)
HH size, working-age adults	3.03 ( 1.75)	2.97 ( 1.85)	3.03 ( 1.63)	3.11 ( 1.57)	0.15 ( 0.26)
Junior Secondary School or higher	0.08 ( 0.28)	0.10 ( 0.30)	0.08 ( 0.27)	0.08 ( 0.27)	-0.02 ( 0.03)
used any fertilizer	0.31 ( 0.47)	0.43 ( 0.50)	0.39 ( 0.49)	0.99 ( 0.11)	0.48*** ( 0.08)
fertilizer qty (50 kg bags)	4.58 ( 9.13)	6.09 ( 10.67)	3.63 ( 5.78)	10.18 ( 10.55)	4.92*** ( 1.34)
HH labor days	93.68 ( 137.34)	97.32 ( 103.40)	81.26 ( 99.21)	112.04 ( 135.62)	25.82 ( 20.76)
paid labor days	64.63 ( 89.30)	89.61 ( 190.95)	66.40 ( 100.62)	66.33 ( 121.45)	-24.59 ( 17.92)
Nnobia (labor-sharing) days	16.80 ( 44.85)	13.05 ( 27.36)	32.96 ( 100.80)	30.88 ( 58.67)	1.35 ( 10.15)

Notes: Table reports means and standard deviations by treatment status in columns (1)–(4). Column (5) presents estimated effects of treatment on each characteristics, with heteroskedasticity-robust standard errors, clustered by village, in parentheses. Symbols \*\*, \*\*\*, \*\*\* in column (5) denote statistical significance of estimated treatment effect at the 10, 5, and 1 percent level respectively.

Figure 2: Distribution of expected log yields, with and without *hi-tech* inputs



between these upper and lower bounds. We fit log-normal subjective probability distribution to each farmer's reports for  $w = 0, 1$  by minimizing sum of squares

$$\min_{\mu_{iw}, \sigma_{iw}^2} \sum_{k=1}^4 [F_{iwk} - F(y_{iwk}; \mu_{iw}, \sigma_{iw}^2)]^2. \quad (9)$$

These data are illustrated in Figure 2, which plots kernel densities for expected (log) output under each counterfactual outcome. Farmers on average believe the returns to be positive from adoption, but there is substantial variation in these expectations.

## 4 Average and heterogeneous treatment effects

### 4.1 Average treatment effect on the treated

To make clear the identifying assumptions underlying our estimates of average returns, consider the following model for potential outcomes of gross output under two counterfactual scenarios - with and without the *hi-tech* inputs ( $w = 1, 0$  respectively):

$$y_{0ivt} = \mu_0 + \eta_i + \lambda_{vt} + \varepsilon_{0ivt} \quad (10)$$

$$y_{1ivt} = \mu_1 + \eta_i + \lambda_{vt} + \varepsilon_{1ivt} \quad (11)$$



for farmer  $i$  in village  $v$  and year  $t$ . For the time being, we ignore the role of observed, farmer-specific covariates. The  $\eta_i$  give farmer-specific, time-invariant unobserved characteristics, while the  $\lambda_{vt}$  capture village-year unobserved shocks to productivity; we will be concerned about the potential correlation of both with treatment status.<sup>9</sup> Without further loss of generality, we assume that  $E[\eta_i] = E[\lambda_{vt}] = E[\varepsilon_{wivt}] = 0$ , so that the difference  $\tau_{ATE} = \mu_1 - \mu_0$  gives the average treatment effect in this population. The quantity  $\tau_{ATT} = \mu_1 - \mu_0 + E[\varepsilon_{1ivt} - \varepsilon_{0ivt} | w_{ivt} = 1]$  gives the average treatment effect on the treated (ATT). We focus on identification of the ATT in this section, since this is identifiable under a more plausible set of assumptions.

Observed outcomes are given by the switching regression,  $y_{ivt} = y_{0ivt} + (y_{1ivt} - y_{0ivt})w_{ivt}$ . Substituting in equations (10) and (11) yields

$$y_{ivt} = \mu_0 + (\mu_1 - \mu_0 + \varepsilon_{1ivt} - \varepsilon_{0ivt})w_{ivt} + \mu_0 + \eta_i + \lambda_{vt} + \varepsilon_{0ivt} \quad (12)$$

Examination of equation 12 clarifies the nature of the selection problem that must be addressed in estimating the ATT. A naive regression of  $y_{ivt}$  on  $w_{ivt}$  returns a consistent estimate of  $\tau_{ATT}$  only if  $E[\eta_i + \lambda_{vt} + \varepsilon_{0ivt} | w_{ivt}] = E[\eta_i + \lambda_{vt} + \varepsilon_{0ivt}] = 0$  (Heckman 1997). This reflects the weaker identifying assumptions required for the ATT than the average treatment effect in the population as a whole; the former requires only that, conditional on covariates, average outcomes among the untreated are a consistent estimate for the average outcomes that treated would have obtained, in the absence of the program. Thus consistent estimates of  $\tau_{ATT}$  are possible even if individual-specific returns to adoption ( $\varepsilon_{1ivt} - \varepsilon_{0ivt}$ ) are correlated with adoption choices,  $w_{0ivt}$ . But the assumption required for identification of the ATT from a naive regression of output on treatment alone will fail if adoption is correlated with either village-level differences in productivity or with the idiosyncratic productivity of farmers. We take up these problems below.

We first formalize the process by which membership is determined. Recall that we define treatment,  $w_{ivt}$ , as the effect of the first year of use of the *hi-tech* package, in light of the potential for accumulation of impacts over years. In order to estimate treatment effects, we restrict attention to

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<sup>9</sup>For counterfactual states,  $w = 0, 1$ , we can decompose the state-specific error term  $\varepsilon_{wivt}$  into two parts,  $\varepsilon_{wivt} = \alpha_{wiv} + u_{wivt}$ , where the first component represents a time-invariant, individual-specific return. We will refer to this as “essential heterogeneity”; it is the object of learning by the farmer. The second component,  $u_{wivt}$  is time-varying and captures a source of objective risk. Both can impact future technology decisions: the former affects subjective perceived returns, while the latter affects farmers’ liquidity, buffer stocks, etc.

village-years,  $vt$ , in which either (a) Abrabopa has never had any members in that village before, and Abrabopa will have its first members in village  $v$  in year  $t + 1$ ; or (b) Abrabopa has its first ever members in village  $v$  in year  $t$ . Consequently we examine first-year impacts only on the subpopulation of ‘initial adopters’ who join Abrabopa in the first year that it is available in their village.

In this set of village-years, use of Abrabopa’s hi-tech inputs is the product of two factors: firstly, that Abrabopa visits the individual’s village,  $v$ , in year  $t$ , and secondly, that the individual joins in that year. Let  $z_{vt}$  be a dummy variable indicating the presence of Abrabopa in village  $v$  in year  $t$ , and let  $m_{iv}$  be a dummy variable indicating that individual  $i$  in village  $v$  is the ‘type’ who joins Abrabopa in the first year in which it is available in their village. Thus in these villages,  $w_{ivt} = z_{vt}m_{iv}$ .

Our identification strategy rests on two key features of our data. The first of these is the ability to observe the *future* membership decisions of individuals in villages that have not yet been visited by Abrabopa at the time of the output realization  $y_{ivt}$ .<sup>10</sup> The second of these is the ability to observe the productive outcomes for a representative sample of those who do *not* join Abrabopa in any given village-year.

We use the first of these features to address potential correlation between the individual-specific unobservables and treatment status, arising through individual selection into Abrabopa. The second allows us to address the potential correlation between village-level characteristics and treatment status, arising through the non-random roll-out of Abrabopa coverage.

To do so, we begin by assuming that the process by which farmers are selected into membership is constant over time, with respect to unobserved characteristics that differentiate them from village-mean productivity:

$$E[\eta_i + \varepsilon_{0ivt} | m_{iv} = 1, z_{vt} = 1] = E[\eta_i + \varepsilon_{0ivt} | m_{iv} = 1]. \quad (13)$$

This assumption implies that those who join Abrabopa when it first reaches their village in year  $t$  are the best farmers in those villages, then farmers who join Abrabopa upon its arrival in their villages in year  $t + 1$  are also the best farmers in those villages. Using the subset of farmers who are observed both before and after adoption, we can relax this assumption. In a fixed effects estimate, we require only that the time-varying component of the error term,  $\varepsilon_{0ivt}$ , is uncorrelated with  $z_{vt}$ . This will be used to provide a test of the assumption in 13 below.

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<sup>10</sup>For a similar use of future adoption decisions to address selection problems in a pipeline evaluation, see, e.g., Field (2005).

If we were also willing to assume that the roll-out of Abrabopa availability was as good as random with regard to village productivity levels, so that  $E[\lambda_{vt}|z_{vt}] = 0$ , then a comparison of current and future program members would suffice to identify the ATT. Clearly, effective randomness of roll-out would be a strong assumption. We are able to avoid making such an assumption by using data on non-members in program villages. In essence, we can use mean outcomes of initial non-adopters to estimate the village-specific effect. Our estimates under this identification strategy are then a form of difference-in-difference estimates: we compare within-village differences between those who join Abrabopa and those who do not, in villages that have just been reached by Abrabopa in year  $t$  and those that will only be reached by Abrabopa in year  $t + 1$ .

This strategy requires an auxiliary assumption that there are no externalities from the presence of the program on non-members—akin to the standard *stable unit treatment value* (SUTVA) assumption. Formally, we require that

$$E[\eta_i + \varepsilon_{0ivt}|m_{iv} = 0, z_{vt} = 1] = E[\eta_i + \varepsilon_{0ivt}|m_{iv} = 0, z_{vt} = 0] \quad (14)$$

This implies that the idiosyncratic component of the outcome observed for the untreated is uncorrelated with village-level exposure, *after* conditioning on the village-year effect,  $\lambda_{vt}$ . Again, we are able to relax this assumption with a farmer-fixed-effect estimate on a subset of our data; we find no significant differences in estimates in this case. Since we are interested here only in impacts in the first year of the program’s presence in a village, we believe this to be a plausible assumption. Neighboring farmers will not have had an opportunity to observe program impacts and make any corresponding adjustments in their own production.<sup>11</sup>

Given assumptions (13) and (14), we can operationalize our difference-in-differences estimator in two ways. Most directly, we regress output on dummy variables for adoption in the first year of exposure,  $m_{iv}$ , current exposure to Abrabopa,  $z_{vt}$ , and their interaction,  $w_{ivt} = m_{iv}z_{vt}$ . Under assumptions (13) and (14), a regression of the form

$$y_{ivt} = \beta_0 + \beta_z z_{vt} + \beta_m m_{iv} + \beta_w m_{iv} z_{vt} + \varepsilon_{ivt} \quad (15)$$

consistently estimates the ATT as the coefficient  $\beta_w$ . An alternative is to use a village-year fixed estimator for equation (15), in which case the village-level exposure variable,  $z_{vt}$ , is not identified, but the treatment effect for

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<sup>11</sup>Conley and Udry (2010) demonstrate that farmers do adapt their technology choices in response to ‘news’ about their neighbors’ levels of production. As in their estimation strategy, we rely on farmers’ inability to react until after output realizations have occurred

early adopters remains identified by the interaction of indicators for the early adoption decision of the individual and village-level exposure.

Results of these estimates are presented in Table 3. Column (1) provides estimates of the basic difference-in-difference specification of equation (15). The estimated average treatment effect experienced by Abrabopa members can be read off from the first row as 437kg—an estimate approximately 1.9 times the output required to repay the cost of the input package (230 kg). In column (2), we further include a vector of potentially confounding farm and farmer characteristics.<sup>12</sup> Columns (3) and (4) include village and village-year indicator variables, respectively. The estimated treatment effect is substantively unchanged by these additional robustness measures. Finally, column (4) includes farmer fixed effects, identifying the treatment effect off of changes in membership observed for a subset of 195 farmers who were interviewed both before and after treatment.

We cannot reject the hypothesis that the estimates of column (1) are consistently estimated in any of these specifications. Estimated coefficients on exposure at the village and individual levels (variables  $z_{vt}$  and  $m_{iv}$ , respectively) confirm the lack of evidence for selection bias by village or individual. The estimated treatment effect of 391 kg is substantial: it represents a 30% increase in the pre-treatment output of those who go on to adopt.

A similar identification strategy can be used to test whether participation in Abrabopa increases use of complementary inputs. If such impacts were to be found, then these should be taken into account in calculating returns to participation and technology use. For any input,  $x_{ivt}$ , used by farmer  $i$  in village  $v$  and year  $t$ , the effect of Abrabopa membership on input use can be implemented by simply replacing cocoa output by  $x_{ivt}$  in the specification of equation (8). This was done in Table 2 for three types of labor input: household labor, hired labor, and *nnoboa* labor, a traditional labor-sharing arrangement. In each case, point estimates are economically small, and we are not able to reject the hypothesis that these complementary input levels remain constant under participation in the program. In the absence of any

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<sup>12</sup>We do not control for productive inputs, on the grounds that changes in labor and non-labor inputs mediate the causal effect of program membership on production (Foster and Rosenzweig 2010). More generally, controlling for post-treatment variables may introduce biases into estimates of causal effects (Heckman and Navarro-Lozano 2004, Pearl 2009). We include controls for farmer gender, an indicator variable for whether the farmer has attained junior secondary or higher education, and household size, as well as (quadratic functions of) farmer age and cultivated farm size. Given that it typically takes three years for cocoa trees to reach bearing age, we consider it reasonable to take our measure of cultivated farm size, which explicitly excludes trees too young to bear cocoa, as exogenous in this context.

Table 3: Estimates of average impact of CAA membership on members in the first year

	(1)	(2)	(3)	(4)	(5)
	OLS	Controls	Village FE	V×t FE	Ind. FE
current CAA member, $w_{ivt}$	437.1* (214.63)	450.0** (168.62)	482.8** (175.49)	461.5*** (164.18)	391.6** (143.27)
villaged exposed to CAA, $z_{vt}$	-54.15 (278.08)	-201.6 (190.15)	-274.4 (163.92)		-138.6 (122.52)
early adopter, $m_{iv}$	-130.9 (200.30)	-35.75 (185.66)	-116.7 (158.42)	-90.87 (145.02)	
Individual controls	No	Yes	Yes	Yes	Yes
Wave indicators	Yes	Yes	Yes	No	Yes
N	606	577	577	577	577
Controls: p-value		0.00	0.00	0.00	0.02

Dependent variable is cocoa output, in kg. Robust standard errors in parentheses, clustered at village-level. Estimates in columns (2)–(4) contain controls for farmer gender, log age, and post-primary education, and for log farm size and log tree age. Resulting  $p$  value from  $F$  test for joint significance of control variables presented.

such indirect costs, an impact of 391 kg corresponds to an economic return of 70 percent on the value of the inputs received.

## 4.2 Treatment effect heterogeneity

Estimated returns to the CAA technological package are high among adopters, and yet 32 percent of first-year members in the sample were no longer members one year later. This epitomizes a broader puzzle in agricultural technology adoption: if returns to adoption of this technology are indeed so high, and if credit constraints are not binding,<sup>13</sup> then why do so many members drop out?

Learning about returns provides one explanation. If there is persistent heterogeneity across farms or farmers—such that individuals can learn from current yields about their idiosyncratic, future returns from returns vary substantially across individuals—then incomplete adoption could be an outcome of rational choices. The average treatment effect on the treated, estimated in the previous section, is not informative about the fraction of farmers expecting positive returns - what Heckman (2010) calls the “voting criterion”. In this section, we show that high average treatment effects mask substantial heterogeneity in the ex-post distribution of impacts.

To test for such heterogeneity, we augment the village-year fixed effects empirical specification of Table 3 by interacting the indicator for current membership,  $w_{ivt}$  with a vector of observed individual characteristics,  $x_{ivt}$ .<sup>14</sup> These include farm size, tree ages, and the share of hybrid trees on the farm, and lagged (pre-adoption) output, as well as farmer characteristics: indicators for female farmers and farmers with post-primary (JSS) or higher education, and the log of household size.

These results are presented in column (1) of Table 4. Here we observe a negative association between lagged output and experienced returns. This is consistent with what one might expect for an intervention that includes insecticide and fungicide. It is not surprising for this to have its strongest effects among those farmers whose prior practices led to low output levels.

A potential concern with the results in column (1) is that they might be biased by nonlinearities in the relationship between characteristics  $x$  and yields. Since treatment effects are identified by the interaction of an early adopter indicator,  $m_{iv}$ , with village-level exposure, such nonlinearities would

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<sup>13</sup>This is true by construction in the present context, since the program provided the input package on credit.

<sup>14</sup>To facilitate interpretation, continuous variables are standardized prior to interaction, using means and variances among early adopters.

Table 4: Heterogeneity along observable dimensions

	(1)	(2)
current CAA member, $w_{ivt}$	176.5 (181.20)	235.2 (148.47)
early adopter, $m_{iv}$	60.91 (102.13)	128.4 (109.10)
$w_{ivt} \times$ female	-208.8 (128.28)	-248.5* (125.81)
$w_{ivt} \times$ JSS	-135.9 (238.67)	6.882 (199.39)
$w_{ivt} \times \ln(\text{adults hh members})$	76.83 (92.20)	79.55 (90.31)
$w_{ivt} \times \ln(\text{land devoted to cocoa, ha})$	-4.093 (106.64)	173.3* (95.18)
$w_{ivt} \times \ln \text{ mean tree age}$	56.20 (39.28)	99.87** (43.78)
$w_{ivt} \times$ fraction hybrid trees	161.4 (157.47)	193.3 (149.45)
$w_{ivt} \times$ L.cocoa output, kg	-413.5* (213.11)	-680.8*** (179.97)
Direct effects of characteristics $x$	Yes	Yes
N	570	570
Treatment interactions: p-value	0.07	0.00
Adopter interactions: p-value		0.00

Notes: dependent variable is cocoa output, in kgs. Individual controls for interacted characteristics included in all specifications. All columns include village-year fixed effects. Heteroskedasticity-robust standard errors reported, clustered at village level.

be a source of bias if members have different levels of  $x$  than non-members. Under our identification strategy, this can be addressed by allowing the time-invariant membership indicator,  $m_{iv}$ , to interact with characteristics  $x$  as well. This is done in column (2). Addressing this potential source of bias reveals interesting dimensions of heterogeneity: women experience substantially lower returns, while larger farms and those with older trees experience higher returns. The p-value on a Wald test of the hypothesis that coefficients on the interaction of characteristics and membership ( $m_{iv} \times x_{ivt}$ ) is statistically significant at the one percent level, confirming the importance of these controls.

Observed heterogeneity appears to reflect both agronomic and social or economic factors. Higher returns for older trees is evidently an agronomic property of the input package. The relatively high treatment effect experienced by larger farmers may result from an association between the responsiveness of land to inputs and farm size. It may also reflect returns to scale—particularly if farmers use their group’s spraying ‘knapsack’ on other plots—or large farmers’ ability to shift complementary inputs in response to the application of Abrabopa plots. The low treatment effect experienced by female farmers remains an intriguing question.

To provide a full characterization of the economic implications of the heterogeneity in returns, we use a quantile treatment effects approach to show that the heterogeneity in returns we do observe has economic implications.

Differences in quantiles of the distribution of outcomes under treatment and control can be interpreted as quantiles of the treatment effect only under the assumption of perfect positive dependence. In this case, the treatment does not change the ranking of outcomes, so that the first quantile of the distribution without treatment,  $Y_0$ , represents the counterfactual for individuals in the first quantile of the distribution with treatment,  $Y_1$ . With treatment and control groups of different sizes, we compare impacts across quantiles of the outcome distribution, rather than directly matching individuals.

To do so, we continue with the identification strategy of Section 4.1. In particular, we estimate quantile treatment effects using a model that includes a dummy variable for presence of Abrabopa in the village in the year under study,  $z_{vt}$ , as well as a dummy identifying those individuals who join Abrabopa in the first year of exposure in their village,  $m_{iv}$ . Treatment is denoted by the interaction of early adopters with village-level exposure:  $w_{ivt} = m_{iv}z_{vt}$ . We now explicitly adopt a random coefficients framework to estimate the regression model,

$$y_{ivt} = \beta_{i0} + \beta_{iz}z_{vt} + \beta_{im}m_{iv} + \beta_{iw}z_{vt}m_{iv} + e_{ivt}. \quad (16)$$

In the potential outcomes framework of equations (10) and (11),  $\beta_{iw} = \mu_1 - \mu_0 + \varepsilon_{1ivt} - \varepsilon_{0ivt}$ . Under the additional assumption of perfect positive dependence, quantile treatment effects on the treated are identified by the coefficient  $\beta_{iw}$  on this interaction term at the corresponding quantiles of the outcome distribution.<sup>15</sup>

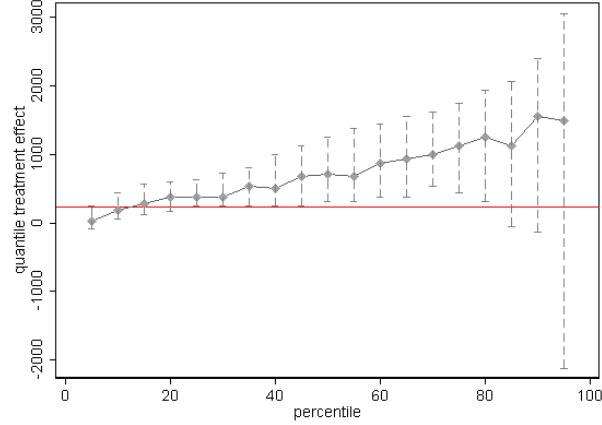
Results are presented in Figure 3. The figure presents estimates for vigintiles (20-quantiles) of the outcome distribution among the treated. For ease of interpretation, we include a horizontal line at an impact of 230.8 kg, the sales volume required to repay the Abrabopa loan in the years studied. In the absence of any other costs of complementary inputs, individuals make a profit if and only if they experience a treatment effect in excess of this point.

Confidence intervals are estimated by a block bootstrap, with resampling conducted at the village-year level to account for potential non-independence in outcomes within these sampling units. Although we can reject the null hypothesis of a treatment effect of zero for all but tails of the distribution, we fail to reject the hypothesis of a treatment effect above the break-even point for individuals at or below the 25<sup>th</sup> percentile and above the 80<sup>th</sup> percentile. Estimates at the upper end of the distribution are relatively imprecise, due to a small number of high-output farmers in our sample.

<sup>15</sup>For quantile  $q$ , this is the treatment effect on treated individual  $i$  such that  $i = \arg \inf_{y_{i1}} F_1(y_1 | w_{ivt} = 1) > q$ .



Figure 3: Quantile treatment effects



Notes: Figure illustrates estimated quantile treatment effects and associated bootstrap (90%) confidence intervals. Non-parametric, block bootstrap confidence interval based on 400 repetitions, re-sampling at village level. Horizontal line at 230.8 kg indicates cocoa output required to repay direct cost of inputs.

Figure 3 suggests that, in this context, an exclusive emphasis on mean impacts misses important features of the distribution. In economic terms, there appears to be substantial heterogeneity in returns to program participation; in general, returns appear to be higher for more productive farmers. Crucially, we fail to reject the hypothesis of zero returns, net of repayment costs, for 7 of the 20 vigintiles of the treated population.

We address the questions of whether this observed heterogeneity in returns can help to explain program retention, and through what mechanism, in the following section.

## 5 Heterogeneity and continued adoption

In this section, we demonstrate a relationship between realized output gains experienced by initial adopters and their decisions to remain within the program. To distinguish learning and precaution mechanisms for this effect, we undertake two exercises. First, we show that the pattern of relationships between output histories and adoption decisions is consistent with the learning model, and contradicts the model of precautionary savings, as outlined

in Section 2. Second, we make use of measures of subjective perceptions of returns, and show that these respond to realized output gains in the way predicted by the learning model.

We begin by constructing a proxy for realized treatment effects at the individual level, and show that this individual measure correlates with retention, even after conditioning on possible confounding factors. Of course, since each farmer can make one and only one membership decision in a given period, the treatment effect that she experiences is never directly observable. This is the “fundamental problem of causal inference” (Holland 1986). Typically, strong assumptions are required for measures of individual- and time-specific treatment effects to be constructed.

Our aim here is more modest. We construct a proxy for the measure that is correlated with the realized treatment effect, and more importantly with the signal of the treatment return to adoption as experienced by the farmer. Given such a signal,  $\tilde{\tau}$ , we test its relationship with the subsequent decision to renew membership with a binary choice model of the form

$$w_{iv,t+1}^* = \phi_0 + \phi_\tau \tilde{\tau}_{ivt} + \phi_x x_{ivt} + u_{iv,t+1} \quad (17)$$

where membership in period  $t + 1$  is chosen if  $w_{ivt}^* \geq 0$ , and  $x_{ivt}$  represents a vector of controls for potential confounding factors, as described below. Assuming normality of the error term,  $u_{iv,t+1}$ , the parameters of equation (17) can be estimated as a probit.

As a proxy for individuals’ signal of returns, we use pre-post changes in log cocoa output as experienced by initial adopters. For this population we define  $\tilde{\tau}_{ivt} = y_{1ivt} - y_{0iv,t-1}$ , where this notation reflects the fact that log output in periods  $t$  and  $t - 1$  represent draws from the distributions with and without technology, respectively. Following the notation of potential outcome equations (10) and (11), this will correspond to the true treatment effect on log output,  $\tau_{ivt}$ , only if  $\lambda_{vt} = \lambda_{v,t-1}$  and  $\varepsilon_{0ivt} = \varepsilon_{0iv,t-1}$ . This would require both that any village-level components of production are constant over time, and that individuals’ idiosyncratic output in the absence of Abrabopa is the same in the pre-exposure period,  $t - 1$ , as it is in the first period of exposure,  $t$ .

Neither of these assumptions will hold in practice: there will be village-level characteristics (such as rainfall) that vary across years, as well as shocks to potential output in the absence of fertilizer. Consequently the proxy  $\tilde{\tau}_{ivt}$  will be composed of the true treatment effect for individual  $i$  in period  $t$ ,  $\tau_{ivt} = y_{1ivt} - y_{0ivt}$ , plus two terms that reflect the failure of these assump-

tions:

$$\tilde{\tau}_{ivt} = \tau_{ivt} + \underbrace{(\lambda_{vt} - \lambda_{v,t-1})}_{\hat{\lambda}_{vt}} + \underbrace{(\varepsilon_{0ivt} - \varepsilon_{0iv,t-1})}_{\hat{\varepsilon}_{0ivt}}. \quad (18)$$

The correlation between this proxy and future membership decisions,  $w_{iv,t+1}$ , will reflect the effect of realized returns on individual decisions, as well as any correlation between the terms  $\hat{\lambda}_{vt}$  and  $\hat{\varepsilon}_{0ivt}$  and other determinants of membership. Thus measurement error in the proxy  $\hat{y}_{ivt}$  creates a potential source of bias beyond the threat of omitted factors correlated with both the true, idiosyncratic treatment effect,  $\tau_{ivt}$ , and the sustained adoption decision. We take up these considerations below.

In Appendix Figure A.1, we show that values of this proxy across quantiles of the output distribution closely correspond to estimated quantile treatment effects. Given the evidence to support this measure,  $\tilde{\tau}_{ivt}$ , of individual treatment effects as experienced by the farmer, we turn to estimation of equation (17). Probit coefficients are presented in Table 5, where the dependent variable in all specifications is the farmer’s membership decision in the subsequent year of exposure, which we observe through survey data and verify with administrative records.

In columns (1) and (2), the sample of farmers is restricted to initial adopters immediately following their we demonstrate a positive relationship between a farmer’s gain in output over the two-year period ending in their first year of membership and their membership decision in the second year. This effect is substantial, implying that a one standard deviation increase in this measure of the treatment effect is associated with a 13 percent increase in the likelihood of remaining in the program.

Column (2) of Table 5 tests between the precautionary savings and learning mechanisms as explanations for the relationship between realized heterogeneity and subsequent adoption. In this specification, current and past output levels are allowed to enter independently in the individual’s choice to sustain membership. Under the precautionary savings mechanism, both should enter the adoption decisions with the same sign, but this is not the case. The negative sign on lagged (pre-adoption) output is consistent with either the possibility of known heterogeneity in yields under the old technology, or the view that farmers update their beliefs about the traditional technology based on recent experiences as well.

In column (3), we substantiate this hypothesized mechanism by showing that farmers’ subjective expected returns to the adoption of the new technology are positively associated with their experienced gains from experimentation. This further corroborates the hypothesis of learning about

Table 5: Individual output gains, program retention, and expected returns

	(1) New	(2) New	(3) $E[\ln y_1 - \ln y_0]$
$\Delta \ln \text{cocoa}$	0.479*** (0.18)		0.0777* (0.04)
$\ln(\text{cocoa output, kg})$		0.426* (0.24)	
$L.\ln \text{cocoa}$		-0.547*** (0.17)	
Farmer characteristics	Yes	Yes	Yes
Obs	229	229	156

Notes: Columns (1)–(2) report probit coefficients, with dependent variable  $\mathbf{1}[\text{continued adoption}]$ . Column (3) reports OLS regression coefficients, where the dependent variable is the farmer’s subjective expected return at time  $t$ . Standard errors clustered at village level in all specifications. Output variables rescaled by dividing by 1,000 kg prior to estimation. Farmer controls include gender, education, HH size, farm size, tree age and type.

returns by providing direct evidence of the mechanism.

From these results, we conclude that the data are consistent with a model in which individuals experience heterogeneous returns to participation in the Abrabopa program, and they condition their decisions to remain within the program on these experienced returns.<sup>16</sup> The association between measures of returns and future membership decisions does not appear to be driven by the ‘supply’ of the program—although repayment failures do cause expulsion in accordance with the program’s bylaws. Nor is this association driven by current income alone: those who experience greater gains upon joining the program are more likely to remain, even at a given level of current production.

## 6 Implied risk preferences: a structural approach

Average returns to some agricultural technologies have been estimated to be so large that adoption decisions appear difficult to reconcile with ‘rea-

<sup>16</sup>Sustained membership in Abrabopa is not equivalent to sustained use of the technology it provides. Examination of whether treatment heterogeneity correlates with continued use of the hi-tech package is not possible in this context, since we observe only whether broad categories of inputs such as fertilizer were used, but not their exact make or proportions on a given plot; moreover, we only have these production data for 29 of the first-year members in our sample who subsequently drop out of Abrabopa. Among these 29 farmers, 14 report using fertilizer in some form and quantity.

sonable' estimates of farmers' aversion to risk (Duflo 2006). This has led some researchers to test for behavioral anomalies that may lead to inefficient choices, and evidence does suggest that these may be part of the picture (Duflo, Kremer, and Robinson 2009).

The evidence presented here has suggested two ways to reconcile empirical estimates of returns with observed adoption rates. First, the recognition that farmers are often learning about returns suggests that subjective uncertainty may deter some farmers from adoption, even when the variance of ex post estimates of returns does not justify low adoption rates. Second, when there is persistent heterogeneity in returns, the returns for some farmers may not justify adoption even when production uncertainty is limited.

Data on subjective perceptions of the distribution of potential outcomes under each counterfactual state—adoption or disadoption—allow us to test whether, once subjective uncertainty and perceived heterogeneity are taken into account, adoption decisions are consistent with reasonable attitudes toward risk. We do so in this section by estimating the parameters of a structural model of choice.

## 6.1 Model specification

We address three substantive issues in the specification and identification of a structural model of technology choice. In particular we specify a utility function used to evaluate risky alternatives; a stochastic decision rule to translate expected values into choices; and a way of addressing with potential constraints to adoption that may be correlated with beliefs.

We estimate a CRRA instantaneous utility function of the form

$$u(m) = \frac{m^{1-\rho}}{1-\rho}, \quad (19)$$

where  $\rho$  is the coefficient of relative risk aversion, and  $m$  is net income from cocoa production in a given state of the world.<sup>17</sup> While more general specifications for the utility function are of course possible, the CRRA specification has advantages not only of parsimony, but also of comparability to laboratory experimental evidence of risk preferences in other developing-country contexts (Andersen, Harrison, Lau, and Rustrom 2009, Harrison,

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<sup>17</sup>We assume that the only relevant pecuniary and non-pecuniary costs of adoption are the direct costs of the *hi-tech* input package. This is supported by the results presented in Table 2, where we find no impact of adoption on labor inputs.

Humphrey, and Verschoor 2010).<sup>18</sup> Individual  $i$ 's expected value for technology choice  $w \in \{H, L\}$  is then given by

$$\tilde{V}_{i,w} = \int_0^\infty u(m) f_{i,w}(m) dm \quad (20)$$

where  $m$  is net cocoa income, and  $f_{i,w}(m)$  is individual  $i$ 's subjective probability density function for net cocoa income under adoption decision  $w$ .<sup>19</sup>

Given values  $\tilde{V}_{i,H}, \tilde{V}_{i,L}$  associated with uncertain prospects with and without technology adoption, a deterministic theory would suggest that the technology is adopted whenever  $\tilde{V}_{i,H} > \tilde{V}_{i,L}$ . To allow for the possibility that unobserved factors shift individuals' choices, we embed these values in a stochastic choice rule. Standard choices here include probit decision rules of the form

$$w_i^* = \theta(\tilde{V}_{i,H} - \tilde{V}_{i,L}) + \varepsilon, \quad (21)$$

where  $\varepsilon$  is normally distributed with mean zero and variance of unity, and where the technology is adopted when  $w^* > 0$ . Here, the parameter  $\theta$  measures the determinism of choice: as  $\theta$  goes to zero, choice is effectively random, while for high values of  $\theta$  the prospect with highest expected value is chosen with near certainty.

However, as Wilcox (Wilcox 2008, Wilcox 2011) points out, standard stochastic choice models such as probit or logit have an unfortunate property for the estimation of parameterized utility functions. In particular, Wilcox shows that, if one distribution is a mean-preserving spread of the other, a probit decision rule does *not* have the property that the probability of choosing the 'safe' distribution will be monotonic increasing in the degree of risk aversion. This arises because in spite of the cardinality of the utility function, a change in  $\rho$  changes its scale, while the variance of the stochastic component of choice,  $\varepsilon$ , remains constant. Increases in  $\rho$  can decrease the likelihood of choosing stochastically dominant alternatives because choices

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<sup>18</sup>Note that we are assuming here that a CRRA function is used to evaluate the expected value of uncertain income prospects. This is equivalent to a utility function defined over consumption only if all income is consumed in each period, and there is no background wealth. These appear to be reasonable starting points for estimation, not only because the population under study is relatively poor, but also because the estimates of Table 5 fail to reject a model in which precautionary savings does not play a role in technology adoption: pre- and post-adoption output levels have opposite signs and equal magnitudes as determinants of the choice to sustain adoption by initial adopters.

<sup>19</sup>In ongoing work, joint with Mans Söderbom, we use a similar structural approach to test for departures from expected utility maximization. This lies beyond the scope of the present paper.

become more random. Given the aim of estimating  $\rho$ , this is potentially a serious concern. To address this problem, Wilcox (2011) proposes a normalization, which he calls ‘contextual utility’. Wilcox rescales the parameter  $\theta$  by dividing it by the difference between the utility of the highest possible outcome in the state space and that of the lowest possible outcome in the state space. We test the sensitivity of our results to this rescaling, using the maximum and the minimum of the state space specified by each respondent.

We allow for the possibility that observed choices may be determined by factors other than preferences by allowing a vector of observed characteristics,  $x$ , to shift the choice index as follows:

$$w_i^* = \theta(\tilde{V}_{i,H} - \tilde{V}_{i,L}) + \psi x + \varepsilon. \quad (22)$$

In the estimates that follow, we include as shift factors (log) current output, farm size, and age, as well as indicators for gender, Junior Secondary School (JSS) or higher education, and survey round.

## 6.2 Estimation results

Results of these structural estimates are presented in Table 6. Estimates of the coefficient of relative risk aversion,  $\rho$ , are in line with those found in artefactual laboratory experimental studies in other contexts. For example, Andersen and coauthors (2009) estimate a CRRA parameter of approximately 0.75 using an adult Danish subject pool; our estimates generally fall within their 95% confidence interval. In a developing country context, Harrison and coauthors (2010) estimate a CRRA parameter of 0.54 on a pooled sample of subjects from Ethiopia, India, and Uganda. Moreover, estimates of the parameter  $\rho$  are remarkably stable: they are substantively unaffected either by the stochastic choice specification or by allowing covariates to shift the choice probability independently of preferences.

Turning to covariates, likelihood ratio tests reject the restricted model without covariates in all choice specifications at the one percent level. Higher output levels remain correlated with adoption decisions, which may reflect repayment requirements, as well as precautionary motives. Conditional on perceived returns, larger farmers are no more likely to sustain adoption. We find evidence that older and female farmers are significantly less likely to adopt, conditional on beliefs.

Table 6: Estimated parameters of structural choice model

	Logit (1)	Logit (2)	Probit (3)	Probit (4)	Contextual (5)	Contextual (6)
$\rho$	0.70 (0.05)	0.66 (0.11)	0.70 (0.05)	0.65 (0.11)	0.57 (0.26)	0.73 (0.18)
$\theta$	0.18 (0.01)	0.13 (0.01)	0.11 (0.00)	0.08 (0.00)	0.45 (0.07)	0.32 (0.05)
$\psi$ : constant	0.27 (0.10)	1.99 (1.17)	0.17 (0.06)	1.30 (0.71)	0.22 (0.08)	1.98 (1.16)
$\psi$ : current output	.	0.24 (0.05)	.	0.14 (0.03)	.	0.25 (0.05)
$\psi$ : ln farm size	.	-0.03 (0.13)	.	-0.02 (0.08)	.	-0.02 (0.13)
$\psi$ : ln age	.	-0.59 (0.30)	.	-0.37 (0.18)	.	-0.60 (0.29)
$\psi$ : female	.	-0.47 (0.21)	.	-0.29 (0.13)	.	-0.46 (0.21)
$\psi$ : JSS	.	-0.31 (0.29)	.	-0.19 (0.18)	.	-0.32 (0.29)
$\psi$ : wave 3	.	0.16 (0.17)	.	0.10 (0.10)	.	0.17 (0.17)
N	661	661	661	661	661	661
log likelihood	-447	-425	-447	-426	-449	-426

Notes: dependent variable is an indicator of decision to adopt in subsequent season. Standard errors in parentheses. Logit choice model used in columns (1) and (2); probit choice model used in columns (3) and (4); contextual utility rescaling (with logit errors) used in columns (5) and (6).



## 7 Conclusions

In many developing countries, the persistent adoption of agricultural technologies with high average returns is believed to be one of the principal policy challenges. Experimental and observational studies documenting these high average returns have led to a puzzle: why, if returns are so high, do farmers not adopt—and sustain—the use of agricultural technologies such as fertilizer or hybrid seeds?

We have presented evidence consistent with the view that high average returns mask substantial, persistent heterogeneity in realized returns. Farmers at the low end of the distribution of cocoa production exhibit particularly low returns - so much so that we are unable to reject a zero economic return for the bottom quarter of the distribution. We have shown evidence to support the view that this heterogeneity matters, economically speaking: farmers experiencing signals of low returns are less likely to continue use of the technology.

This *ex post* heterogeneity likely reflects a combination of persistent heterogeneity and time-varying riskiness of returns. Both may have consequences for decisions to sustain or to disadopt a given technology. In this paper we provide evidence that a learning mechanism—as opposed to a precautionary savings motive—drives the observed relationship between realized returns and subsequent adoption decisions.

The distinction between the effect of transient and persistent heterogeneity in returns seems a valuable area for future work. If persistent heterogeneity is quantitatively important, policymakers will need to be cautious in advocating widespread adoption of such technologies. Even when average returns are high, many farmers may stand to lose.

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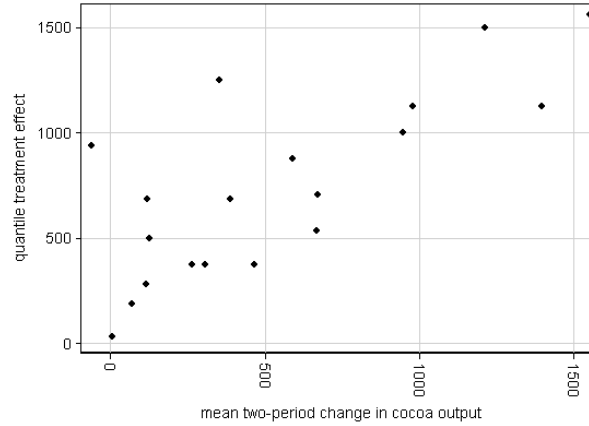
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## Appendix A Validating individual proxies for treatment effect

We validate the individual-specific proxy for experienced treatment effects,  $\tilde{\tau}_{ivt}$ , by comparing the quantile treatment effects estimated in Figure 3 to the mean value of  $\tilde{\tau}_{ivt}$  for individuals in a neighborhood of the same vigintile, as was done with retention rates above. For example, we compare the quantile treatment effect at the 5<sup>th</sup> percentile of the outcome distribution among the treated,  $F_1(y_{ivt})$ , to the mean value of the individual measure  $\tilde{\tau}_{ivt}$  between the 2.5<sup>th</sup> and the 7.5<sup>th</sup> percentiles of the treated population.

If the perfect positive dependence assumption holds—so that quantile treatment effects can be interpreted as the average response at that quantile—and if our proxy is a relatively precise estimate of the individual treatment effect, then the two measures should correlate closely.

Figure A.1: Validating alternative measures of impact: quantile treatment effects versus mean pre-post changes by quantile



Notes: Horizontal axis is defined change in cocoa output among treated farmers. The local average of this measure within a neighborhood of a given vigintile (20-quantile) is plotted against the corresponding quantile treatment effect.

Figure (A.1) shows the relationship between these outcome measures for vigintiles of the outcome distribution among the treated. The measures of these two measures of impact are indeed closely related; they have a correlation coefficient of 0.76. Since these measures of treatment effects

are arrived at under non-overlapping identifying assumptions, their evident similarity supports the assumptions required to interpret each as an estimate of the treatment effect.