Essays on Development Economics

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Abstract

This dissertation studies agricultural technology adoption, child labor and development. Although adoption of fertilizers has been high in South Asia, farmers may fail to use it efficiently. Besides higher costs incurred by households engaged in agriculture, inefficient use of fertilizers may also have negative consequences for the environment. The first chapter of this dissertation uses a field experiment in Bangladesh to study whether providing farmers access to a simple rule-of-thumb tool (leaf color chart) to manage the timing of fertilizer applications can improve efficiency of fertilizer use and lead to productivity gains. The second chapter explores whether characteristics of agricultural trainers, who introduced the leaf color charts to the farmers in the treatment group, play an important role in the adoption and use of leaf color charts by farmers. The final chapter of this dissertation studies the impact of a large public workfare program targeting rural households in India on children. In particular, we study the impact of time use by the youngest and oldest children in a household as adult time use changes in response to new work opportunities.

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To Abbu and Ammu

Introduction

The first chapter of this dissertation examines whether providing farmers access to a simple rule-of-thumb tool to manage use of fertilizers can lead to productivity gains. The green revolution led to significant improvements in rice yields in South Asia, through the adoption of high-yielding varieties and the increase of inputs including fertilizers. Although adoption of fertilizers has been high, farmers may still fail to use it efficiently. Besides higher costs incurred by the farmers, inefficient use of fertilizers may also have negative consequences for the environment. In a field experiment in Bangladesh, I provide treatment farmers with a simple rule-of-thumb tool (leaf color chart) to improve the timing of fertilizer applications for urea, a popular nitrogen fertilizer. I find that treatment group farmers reduce urea use by 8% and improve yields by 7% on average, suggesting there is significant scope to improve urea management. Without access to the chart, farmers apply urea too early in the season, during a period when it is likely to be wasted. Gains in yield are driven by increases at the top of the yield distribution, but the reduction is urea use is more evenly distributed implying that farmers at all levels of urea use can save urea on average without sacrificing yields. Cost-effectiveness estimates suggest that each $1 spent on this intervention produces a return of $9 through a combination of savings of urea and higher revenue. Even without an increase in yields, the cost will be recovered after two seasons of use.

The second chapter explores whether characteristics of agricultural trainers determine the adoption and use of leaf color charts. Since the 1960s, governments and international development agencies have invested considerable resources in agriculture extension services in developing countries. Despite limitations and poor performance of agriculture extension in many countries, extension workers remain a major channel through which information about new technology is disseminated to farmers. There is limited evidence on the mechanisms that can make extension services work and little understanding of the characteristics of the trainers that can make extension successful. In the field experiment described in the first chapter, treatment farmers obtained training through agriculture extension trainers. I examine variation in trainer quality and explore whether the measured quality is related to observable characteristics of the trainer. Overall, I find no evidence that characteristics such as age, gender and experience of trainers determine trainer quality. There is some evidence that trainer performance may be negatively correlated with being a university graduate or an extension worker.

The third chapter of this dissertation, which is joint work with Anitha Sivasankaran, examines the impact of the National Rural Employment Guarantee Act (NREGA), a large rural public workfare program in India, on intra-household allocation of time and labor supply. Particularly, we focus on the impact of the NREGA on schooling and employment outcomes for children. We use several rounds of nationally representative cross-sectional data and panel data for three states from the National Sample Survey (NSS) in India. The NREGA offers 100 days of guaranteed work to adults from rural households with the intention to help households smooth consumption during lean agricultural seasons. Providing employment opportunities to households can affect intra-household allocation of time and resources by changing income and bargaining power. We use the phased roll out of NREGA to different districts and measure the difference-in-difference between districts that received the program early relative to those that received it later. In our analysis we look at the impact on children when adults take-up NREGA work. On one hand, additional income in the household can increase resources spent on children's education and reduce child labor. However, if wages in the economy increase or adults take-up new jobs, child labor could increase. Our results show an increase in time spent on education for younger children and an increase in time spent working outside the household for older children.

Chapter 1

Can a Rule-of-Thumb Tool Improve Fertilizer Management? Experimental Evidence from Bangladesh

1.1 Introduction

Since the green revolution in the 1970s, farmers in South Asia have achieved considerable improvements in rice yields by adopting high-yielding varieties, expanding irrigation and increasing their use of inputs, including fertilizers. However, productivity improvements in agriculture appear to have slowed, raising concerns that the gains from such changes have been nearly exhausted (Mottaleb, Mohanty and Nelson, 2014; Pingali, Hossain and Gerpacio, 1997; Pingali and Shah, 2001). As improvements in agricultural productivity raise living standards and reduce poverty, it is important to understand whether there is scope for further improvements by changing the management of existing technology and inputs. The literature on technology adoption suggests that farmers may fail to adopt optimal practices for a variety of reasons including information and resource constraints or behavioral factors such as limited attention (Hanna, Mullainathan and Schwartzstein, 2014; Jack, 2013; Marenya and Barrett, 2007). If such barriers have prevented farmers from fully exploiting the potential gains from green revolution technologies, it may be possible to improve productivity by addressing these barriers. Using data from a field experiment in Bangladesh, this paper explores how farmers manage the use of urea, a nitrogen fertilizer that has been widely-used since the green revolution. I provide farmers with a simple rule-of-thumb tool that reduces the decision costs involved in optimally using the fertilizer. The results indicate that access to the tool helps farmers reduce wastage of urea and some farmers also gain higher yields. While policy has historically encouraged increasing the use of fertilizers, these results demonstrate that improving the timing of fertilizer applications can increase the efficiency of fertilizer use overall and improve yields for some farmers.

In Bangladesh, agricultural lands are intensively cultivated and there are high levels of use of chemical fertilizers. Among the various fertilizers, the use of urea is most widespread. Urea, a source of nitrogen that is needed for plant growth, is used almost universally by rice farmers and it takes a share of over 65% of total fertilizers used in the country (Jahiruddin, Islam and Miah, 2009; Kafiluddin and Islam, 2008). Despite significant experience in using the fertilizer, farmers may fail to use urea efficiently. For any agricultural input, farmers have to learn about the right quantity, the correct timing and the proper method of application and the optimal application may also depend on other inputs, plot quality and environmental conditions. For urea in particular, the timing of the applications is very important in addition to quantity, which makes it easier to make mistakes. Unlike other fertilizers, urea needs to be applied several times during a season as it does not remain in the soil long due to it volatility (Choudhury and Kennedy, 2004, 2005; Koenig et al., 2007). The timing of each of the applications is important as urea applied at the wrong time can have little or no effect on yields. It is possible for the unused urea to leach from agricultural soils to surface or ground water, although the extent of environment pollution is not well studied for Bangladesh (Gilbert et al., 2006).[1] Returns to urea are high when the crop can

[1] An FAO report states that scientists overall agree that level of urea use is still not high enough to cause significant pollution, partly due to the heavy seasonal rainfall that flushes the residues away. However, there are no organizations that systematically monitor this. The same report also states that nitrate toxicity in drinking water is increasingly observed and that there has also been a build-up of nitrous oxides in the atmosphere because of unscientific use of fertilizers (FAO, 2011).

immediately take-up a lot of nitrogen so that wastage is reduced. This is the time when there is a shortage of nitrogen in the crop as can be identified from light green leaves. Crops that have sufficient nitrogen have dark green leaves. A leaf color chart (LCC) is a simple tool that indicates whether urea is needed by the crop. It is a plastic, ruler-shaped strip containing four panels that range in color from yellowish green to dark green, which can be used to determine if the crop has sufficient nitrogen, by matching the leaf color to the chart. By using an LCC farmers can identify precisely when the crop needs nitrogen and time urea applications accordingly (Alam et al., 2005; Buresh, 2010; Witt et al., 2005). Thus, it can help improve decisions on both quantity and timing.

Through a randomized control trial, I provided farmers in the treatment group with an LCC as well as basic training on how to use the chart. Treatment farmers were invited to attend a training session in their village at the beginning of the *Boro* (dry) season of 2013, followed by a short informal refresher training a few weeks later.[2] During the training sessions, treatment farmers were instructed to compare the color of the rice crop leaves with the LCC before applying urea and encouraged to apply the fertilizer only when the crop was deficient in nitrogen. The intervention, particularly the refresher training sessions, focused on rule-of-thumb training that provided very simple rules on when to check leaf colors and when to apply the fertilizer.[3] The training may also address constraints such as lack of information on timing and help farmers pay attention to the importance of leaf colors.

Prior to the intervention, I conducted a baseline survey that collected data on urea used and yields obtained in the *Boro* season of 2012. I conducted a detailed endline survey at the end of the season after the intervention, to determine any changes in urea use and yields caused by access to LCCs. During the 2013 season, several short midline surveys were also

[2]Field staff were instructed to time the refresher training session to the period when most farmers start applying urea.

[3]There is evidence in the literature that rule-of-thumb training can be much more effective than a more-complex training program (Drexler, Fischer and Schoar, 2014).

conducted to explore time use by farmers.[4] Data were also collected on the dates of urea applications and quantities applied on each date to understand any changes in the timing of fertilizer use.

The LCC farmers save urea and improve yields on average, suggesting that productivity gains can be obtained with just improvements to management of urea. For the analysis, I estimate the effect of gaining access to an LCC on urea application patterns, total urea use and yields. I first identify specific changes in farmer behavior in applying urea. I observe that on average farmers in the treatment group are more likely to delay the first application of urea until 21 days after planting instead of applying earlier in the season when returns to this fertilizer are low.[5] Treatment farmers reduce quantity of urea applied in the low-return period by 0.031 kilograms per decimal per application, although there is no significant difference in the quantity of urea applied in the high-return period. I find some evidence that farmers apply urea more frequently in the high-return period, although the coefficient is small and significant only at the 10% level. Treatment farmers are also marginally more likely to visit their fields more often.

I estimate that farmers in the treatment group reduce overall urea use by 0.079 kilograms per decimal[6], which is a decrease of about 8% compared to baseline levels and that they improve yields by about 1.76 kilograms per decimal, which is approximately an increase of 6.8%.[7] These results establish that substantial inefficiencies exist in the way farmers typically apply urea fertilizer; despite using more urea on average, they fail to obtain higher yields. The results suggest that standard notions of underuse and overuse of fertilizers need to be redefined, as quantity is not the only dimension of fertilizer use that predicts yields but timing also needs to be considered. The savings in urea for the treatment group is likely to

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[4]Some midline surveys were conducted on a sub-sample of farmers.

[5]Extension workers recommend that urea should be applied 3 times during the period between 21 days after planting date until a month before harvest.

[6]1 acre = 100 decimals

[7]The results for reduction is urea are consistent for all specifications, while the results for yields become imprecise for one specification.

be caused by a reduction in urea application in the unproductive period. Within the correct urea application period, I find no significant difference in the quantity of urea applied between the two groups, which implies that treatment farmers may improve the timing of urea application within the this period and increase the quantities of nitrogen that the crops can effectively absorb, which in turn generates the increase in yield for the treatment farmers who gain higher yield. Although it is not possible to observe this directly with the available data, the findings that treatment farmers apply urea more frequently in the high-return period and that they visit their fields more often, together provide suggestive evidence that this is the case.

The results show substantial average gains by farmers, however, it is important to understand what happens to farmers at various points across the distributions of urea and yield. There is substantial variation in quantities of urea used by farmers at baseline so the treatment effects may vary by baseline behavior. Estimates from quantile regressions show farmers at all levels of the distribution reduce urea without sacrificing yields. The results for farmers at the lowest quantiles of urea use, suggest that savings of urea are possible without harming yields even when very little urea is used. The treatment coefficients on yields are not precise for the quantile regressions. However, the highest quantiles have the largest coefficients suggesting that treatment effects are largest for farmers who had higher yields at baseline. These results also suggest that the average increase in yields, observed earlier, is driven by a relatively small sub-group of treatment farmers, while decrease in urea is more uniform across different types of farmers. I also conduct a cost-effectiveness analysis, and find that the intervention is highly cost-effective and every $1 spent on the intervention generated a return of $9 for the mean farmer, through a combined effect of savings in urea and higher revenue. Without an increase in yields, costs would still be recovered in two seasons through the savings in urea.

An LCC is an effective tool as it provides simple rules and gives understandable signals on whether or not leaves are healthy in terms of nitrogen sufficiency. The intervention provided information and directed attention to the importance of leaf colors for urea application. The availability of signals may also make it less risky for farmers to experiment and modify urea applications. The intervention also provided simple rules on when to apply urea. All of these factors can improve management of urea. The findings also show that in Bangladesh and in countries using similar technologies, such as India, there is still significant scope for productivity gains by improving management of inputs within existing technology and resources.

The paper is organized as follows. Section 1.2 provides background on the cultivation of rice in Bangladesh, discusses the challenges of using urea efficiently and how leaf color charts can help. Section 1.3 describes the the experimental design, data and the empirical strategy. Section 2.5 presents the results, including changes in urea application patterns and treatment effects on urea use and yields. Section 1.5 presents results from quantile regressions and examines whether there is any evidence for heterogenous treatment effects by time preferences and cognition of the primary farmer and baseline level of household income. Section 1.6 discusses cost-effectiveness of the intervention and Section 2.6 concludes.

1.2 Context

1.2.1 Rice Farming and Urea Use in Bangladesh

In Bangladesh, agriculture remains one of the most important sectors, characterized by a large number of small farmers. The agricultural sector contributes 21% to the GDP and employs about 50% of the labor force (BBS, 2009). Rice is the staple food of the population of about 160 million and provides over 70% of direct calorie intake in the country (Alam et al., 2011). About 13 million agricultural households are involved in rice cultivation. Since the green revolution, use of high yielding varieties of rice have become widespread particularly in the *Boro* (dry) season. Rice crop yield has grown from 0.76 tons per acre in 1970 to 1.9 tons per acre in 2012. The increase occurred mainly due to the use of high-yielding varieties that require higher levels of fertilizers and a considerable increase in irrigation (Alam et al., 2011; Anam, 2014; BBS, 2012).

The use of urea fertilizers has been common since the green revolution. Traditionally, urea has been heavily subsidized. The price of urea in the country is fixed by the government and is general much lower than world prices, although the price was increased in 2011. Although urea (nitrogen) fertilizers have been used most widely, use of non-urea fertilizers also increased after subsidies were introduced in 2004. In 2008, urea had a share of over 65% of all fertilizers used in the country. Overall, the use of fertilizers have increased by 400 percent in the last 30 years (Alam et al., 2011; Anam, 2014; BBS, 2012; Kafiluddin and Islam, 2008).

Although the increase in yields have been high, a rapidly growing population puts pressure to continue to improve yields as self-sufficiency in rice production is a politically important goal. Despite the large gains in productivity and intensive use of inputs, a gap remains between potential yield and actual yield achieved by farmers, known as the yield gap (Alam, 2010; Begum and D'Haese, 2010; Ganesh-Kumar, Prasad and Pullabhotla, 2012).[8] A high yield gap implies that there is still scope for improvement through better input management. A persistent yield gap suggests that despite decades of experience, there are shortcomings in learning by farmers and potential mistakes in management of inputs that persist.

1.2.2 Importance of Timing for Urea

Urea is particularly challenging to use in comparison to other fertilizers, as the timing of the applications matter and can be difficult for farmers to learn. Farmers need to account for differences in urea needs across plots and seasons and in addition time the application of urea well. Farmers apply all non-urea fertilizers once just before planting, although some farmers also apply urea once at that time.[9,10] Typically, urea is applied in two or three

[8] Potential yield is defined as the yield obtained in demonstration/test plots by agricultural specialists using existing technology.

[9] Planting refers to transplanting the seedling from a nursery to the main plot.

[10] In focus group discussions, most farmers stated that urea should be applied two to three weeks after planting, although some farmers mentioned that they apply urea at planting for a feeling of safety to protect

separate applications, starting a few weeks after planting and ending at the start of the flowering stage, about a month before harvest (approximately over a period of 40 days).[11]. If some non-urea fertilizers in the field are unused by the crop, it is retained by the soil and improves the quantity of nutrients available for crops in the next season. In contrast, much of the urea applied can be wasted as it is volatile and can leave the soil fairly quickly (Choudhury and Kennedy, 2004, 2005; Koenig et al., 2007).[12]

Due to this potential for quick loss, urea is typically applied in several applications instead of once, as described above, but it may not be sufficient to minimize wastage. Farmers may obtain sub-optimal yield even if they are using high levels of the fertilizer if they use urea very inefficiently. Depending on the rate of loss, if urea is applied at a time when the crop does not require much nitrogen, it will not contribute towards yield. Similarly, if farmers fail to apply urea at the time when the crop in deficient in nitrogen, they will obtain lower yield. Overall, returns to urea are likely to be higher if it is applied when leaves have insufficient nitrogen and returns to urea may be very low if it is applied when the crop has sufficient nitrogen.

1.2.3 Leaf Color Charts

The Leaf Color Chart (LCC) is a simple tool that allows farmers to understand whether urea is needed by the crop at any point in time during the urea application period.[13] It is a plastic, ruler-shaped strip containing four panels that range in color from yellowish green (nitrogen deficient) to dark green (nitrogen sufficient). As discussed above, rice farmers

against yield loss.

[11] A stylized timeline is shown in Appendix Figure A.1

[12] After a urea application, the nitrogen introduced in the soil constantly cycles among its various forms, including ammonia, nitrate and ammonium, and much of the nitrogen can be lost from conversion of ammonia and nitrate to nitrogen gas, as well leaching downwards and run-off away from the roots. The rate of loss depends on soil pH, temperature, moisture and other soil properties and there vary across plots and over seasons.

[13] The standardized LCCs used in this study were obtained from the International Rice Research Institute (IRRI).

usually apply urea in several split applications during a season. With an LCC, before any application, farmers can compare the color of the paddy leaf to the chart to determine if nitrogen is needed. This allows for efficient urea application that is timed to a period when uptake by crops will be high (Alam et al., 2005; Buresh, 2010; Witt et al., 2005). The instructions that accompany an LCC also tell farmers to first check 21 days after planting to determine if they should start applying urea, as the first three weeks are considered a period of higher wastage.[14]

The literature on LCCs in agricultural journals usually finds an increase in returns either through substantial reduction in use of nitrogen fertilizers without any reduction in yields, or through substantial reduction in nitrogen fertilizers as well as improvements in yields (Alam et al., 2005, 2006; Balasubramanian et al., 2000; Islam, Bagchi and Hossain, 2007; Singh et al., 2002). However, many of the studies are from demonstration plots which were closely supervised by agricultural workers. If farmers are given LCCs and basic training, it is not clear if they would choose to adopt and use LCCs and also whether they would be able to use them effectively. LCCs will only change urea use or yields if farmers are unable to learn how to time urea application well on their own, which they may have learned to do through experience.

1.3 Experimental Design, Data & Empirical Strategy

1.3.1 Study Area

I conducted this study in partnership with the Center for Development Innovation and Practices (CDIP), a non-government organization in Bangladesh.[15] The study was implemented in 105 villages under 20 CDIP branches spread across 21 sub-districts in the 8

[14]Conversations with agriculture specialists in Bangladesh revealed that although the crop may respond to any urea applied early in the season, the returns are lower in that period, which is why they recommend starting urea application three weeks after planting. The first urea application is timed with early tillering (seminal roots and upto five leaves develop), which is usually around 21 days during the *Boro* season due to colder temperatures (Alam et al., 2005).

[15]CDIP is primarily a micro-finance institution that also has education programs.

districts of Brahmanbaria, Chandpur, Comilla, Gazipur, Lakhipur, Munshiganj, Naranganj and Noakhali. A map of Bangladesh identifying the districts is shown in the Appendix in Figure A.3. Appendix Table A.1 presents some summary statistics for the districts. Among the districts, Narayanganj is less agricultural as it is close to the capital, Dhaka, and has a higher concentration of industries. However, the villages from Narayanganj included in this study have a high prevalence of agricultural activity. All locations are rural without the presence of a major market.

1.3.2 Data & Intervention

I conducted a baseline survey in September-October 2012, for 1440 farmers. I collected data at the plot level on all crops grown in the past year by season. The survey focused on the *Boro* season of 2012, and included information for the season on all prices and all inputs including fertilizers. A short survey was conducted with an additional 605 farmers in December 2012.[16] CDIP staff conducted the baseline surveys in their program locations, after I provided training.

Treatment farmers were invited to attend a training session in their village in January 2013. The training session was organized by local CDIP staff and led by an extension worker or agriculture officer invited from the Department of Agricultural Extension (DAE). During the session, each farmer received a leaf color chart and instructions on how to use the chart. CDIP staff conducted home visits for farmers who did not attend the training, to provide the LCC and instructions. The training sessions were generally held just before or around the time of planting. CDIP staff also conducted a more informal refresher training (individually with farmers or in small groups) a few weeks after the main training (before the time urea is generally applied). Figure A.2 in the Appendix shows a timeline for the study.

CDIP staff conducted four short midline surveys electronically on about 67% of the

[16]Due to delays in receiving funding for the project, I could not be conduct the longer baseline survey for all farmers, since the intervention had to be completed by January 2013. New farmers were added to the study by including additional CDIP branches and by following the same guidelines in selecting farmers.

sample.[17] These surveys focused on time use by farmers. A midline survey focusing on the timing of urea applications was conducted on all farmers. An endline survey was conducted for all farmers after harvest from June to August 2013. I implemented the endline survey through an independent survey company, that had not been involved in the interventions or previous data collection to reduce the probability of bias. The survey was similar to the long-form baseline survey, and collected detailed plot-level information for all farmers in the study. We were able to track 97.5% of the households and about 75.7% were still involved in rice cultivation.[18]

1.3.3 Randomization

CDIP selected 20 of their branch offices to participate in the study and I selected approximately 100 farmers from villages covered by each branch. Within each branch, approximately, one-third of the sample was drawn from CDIP micro-finance clients and the remaining two-thirds were drawn from farmers residing in villages with a CDIP school. Further details on sampling are discussed in the Appendix A.1.

I randomly assigned farmers into either a treatment or a control group, from a list of participants that included basic information about the farmer and the household.[19] To assign the farmers, I stratified the sample by CDIP branch and by type of sub-sample (CDIP microfinance clients and farmers from villages with CDIP schools) in the branch, and then randomized at the individual level.[20] Since I randomized at the individual level, each village in the study has both treatment and control group farmers, although the proportion varies. This design increased statistical power compared to the alternative of randomizing at the

[17] Sample size was limited by funding constraints. I selected the locations randomly after excluding some areas with expected staff shortages in that time period.

[18] Overall, 91.3% were still involved in agriculture.

[19] Random assignment was conducted after the baseline survey was completed, but not before all the baseline data had been entered.

[20] The choice of stratification was determined by preferences stated by CDIP to have an equal number of treatment and control group farmers in each branch, and in each type of sample within the branch.

village level, and as I discuss in section 1.4.1, cross-overs do not appear to be a concern in this setting.

Table 1.1 shows summary statistics and checks for balance across the treatment and control groups at baseline. Columns (1) and (2) show summary statistics for the control and treatment groups. On average, farmers in the control group are 45 years old, have 5.9 years of schooling, cultivate rice on 2.37 plots in the *Boro* season, and have a monthly non-agricultural household income of Tk 10,330 (USD 132). The average plot area is 29 decimals, and 1.01 kilograms of urea are applied per decimal and yield of 26.22 kilograms per decimal are obtained. Column (3) shows estimates from regressions of each baseline variable on a treatment dummy and strata fixed effects. There are no significant differences between the two groups for average age, years of schooling, number of plots farmed, non-agricultural income of the household, total plot area cultivated, urea use, yield, revenue or costs. A joint test reveals that the coefficients are not jointly significant.

Since some of the midline surveys were conducted on a sub-sample and there was also some attrition at endline, I also conduct randomization checks for the midline and endline samples as shown in Appendix Table A.3.[21] There are no differences at baseline for the midline sample. For the endline sample (farmers remaining in rice cultivation), revenue and costs are marginally lower (significant at 10% level) but the estimates have similar magnitudes as estimates for the baseline sample. The coefficients are not jointly significant. Treatment farmers were invited to the training in January around the time of planting and did not know about their treatment status before then. Farmers make decisions on rice cultivation before planting, as seedlings are grown separately prior to that date so they can be transplanted to the plots at planting. Therefore, decisions on whether to cultivate rice or what varieties to cultivate will not be related to treatment.

[21] I selected the locations for the midline surveys randomly after excluding some areas with expected staff shortages in that time period.

Table 1.1

Baseline Characteristics

	(1)	(2)	(3)
	Summary Statistics		Randomization Checks
	Control Group	Treatment Group	Treatment
Farmer & Household Characteristics:			
Age (years)	45.02	45.78	0.663
	(12.73)	(12.40)	(0.546)
	[994]	[1001]	[1995]
Schooling (years)	5.86	5.72	-0.136
	(4.38)	(4.28)	(0.189)
	[948]	[970]	[1918]
Number of Plots	2.37	2.36	-0.015
	(1.18)	(1.18)	(0.046)
	[1008]	[1017]	[2025]
Non-agricultural income (Tk)	10329.70	9657.928	-674.164
	(10759.79)	(10392.05)	(455.634)
	[936]	[940]	[1876]
Total Plot Area (decimals)	65.30	67.09	1.215
	(43.42)	(43.62)	(1.763)
	[1008]	[1017]	[2025]
Number of Household Assets	4.28	4.34	0.042
	(2.23)	(2.17)	(0.106)
	[708]	[714]	[1422]
Plot Level Variables):			
Plot Area (decimals)	28.87	30.18	1.125
	(20.72)	(22.97)	(0.740)
	[2252]	[2260]	[4512]
Urea (kg/decimal)	1.01	1.01	-0.001
	(0.69)	(0.62)	(0.025)
	[2253]	[2263]	[4516]
Yield (kg/decimal)	26.22	25.25	-1.093
	(19.71)	(15.81)	(0.764)
	[2253]	[2263]	[4516]
Revenue (kg/decimal)	361.86	342.71	-21.641
	(278.02)	(205.08)	(13.198)
	[1682]	[1702]	[3384]
Total Cost (Tk/decimal)	245.92	233.87	-14.236
	(230.93)	(159.76)	(8.884)
	[1684]	[1704]	[3388]
Profit (Tk/decimal)	115.99	109.03	-7.455
	(292.69)	(209.38)	(12.658)
	[1682]	[1702]	[3384]
Joint Test (chi-squared)			2.51
p-value			(0.1130)

Notes: For columns (1) & (2), standard deviations are shown in parentheses and sample sizes are shown in square brackets. Column (3) reports the coefficients for regressions of each dependent variable on *Treatment* and strata fixed effects. Robust standard errors for regressions with individual/household level variables and standard errors clustered at household level for regressions with plot level variables are shown in parentheses. Sample sizes are shown in square brackets. The joint test used a chi-squared test to estimate whether the coefficients are jointly significant. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

1.3.4 Empirical Stratgey

I estimate the intent-to-treat effect of getting access to an LCC. I estimate a simple difference specification (Equation 1.1) for outcomes for which data are not available at baseline. This specification is used to estimate changes in urea application patterns using data in the midline surveys.

$$y_{ph} = \alpha_0 + \alpha_1 Treatment_h + \rho X_h + \delta Z_{ph} + \gamma_s + \epsilon_{ph} \tag{1.1}$$

y_{ph} is a urea application pattern in plot p by household h. $Treatment_h$ takes a value of 1 for households in the treatment group and is 0 otherwise and X_h includes controls for household and individual specific characteristics including age and years of education completed by the farmer interviewed (primary farmer in household), total plot area cultivated by household, non-agricultural household income. Z_h includes plot level variables such as variety of rice. γ_s controls for strata fixed effects and ϵ_{pht} is the error term. Standard errors are clustered at the household level. The coefficient α_1 estimates the difference between the treatment and control groups during the season.

For outcomes such as urea use and yields, for which data are available at baseline and endline, I estimate treatment effects using a difference-in-difference estimator (Equation 2).

$$\begin{aligned} y_{pht} &= \beta_0 + \beta_1 Treatment_h + \beta_2 Post_t + \beta_3 Treatment_h * Post_{ht} \\ &\quad + \rho X_{ht} + \delta Z_{pht} + \gamma_s + \epsilon_{pht} \end{aligned} \tag{1.2}$$

y_{pht} the outcome in plot p for household h at time t. $Post_t$ is 1 for the observations from the endline survey and 0 if it is from the baseline. Other variables are the same as above. Standard errors are clustered at the household level. Since assignment to receive an LCC was random, β_3 estimates the causal effect of gaining access to an LCC.

1.4 Results

In this section I present the main findings of this study. I first show estimates of take-up of leaf color charts in section 1.4.1. In section 1.4.2, I describe the observed behavior of farmers in applying urea, in the absence of leaf color charts, and discuss expected changes due to the intervention, followed by section 1.4.3, where I estimate whether we observe any of these changes after treatment. In section 1.4.4, I present the treatment effects on urea and yields as well as treatment effects on revenue, costs and profits for a sub-sample.

1.4.1 Take-up

Table 1.2 shows several estimates for the take-up of leaf color charts. During the endline survey, farmers were asked whether they received an LCC, whether they attended the main training, whether they used the LCC during the season and were also asked to show their LCC (if they said they had received one). The estimates in the table show that the treatment group farmers were much more likely to receive the LCC, attend training, use the LCC and could show the LCC to enumerators. Estimates with and without controls for individual and household characteristics are similar. The probability of stating that they received an LCC is 68.4 percentage points higher for the treatment groups farmers compared to the control group farmers. About 75% of the treatment group state they received a LCC. 7.8% of the control group also state they received an LCC, most likely through government extension workers.[22] The primary farmer in the household is the person interviewed at the endline survey and only 59% attended the DAE training session. Qualitative interviews with some of the farmers later on, revealed that in many of these cases, the primary farmer was away from the village or working in an additional occupation during the training and a family member attended instead as his representative, as CDIP records indicate almost full attendance, however, the representative often failed to explain how the LCC works to the

[22] Although CDIP staff were instructed not to allow anyone other than farmers who were invited to attend the training, in a few cases other farmers came. I find from CDIP records and qualitative work that the control group farmers who have an LCC, usually received it from the DAE representative outside the training or in a few cases if they attended the training.

Table 1.2

Take-up & Stated use of LCCs

	(1) Received LCC	(2) Attended Training	(3) Used LCC	(4) Could Show LCC
Panel A: Without Controls				
Treatment	0.684***	0.531***	0.491***	0.581***
	(0.018)	(0.020)	(0.020)	(0.019)
Mean of Control Group	0.0788	0.0604	0.0604	0.0723
Observations	1,526	1,526	1,526	1,526
Panel B: Including Controls				
Treatment	0.682***	0.529***	0.489***	0.579***
	(0.018)	(0.020)	(0.020)	(0.019)
Age (years)	0.000	0.001	0.001	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Schooling (years)	-0.006***	-0.006**	-0.005**	-0.004*
	(0.002)	(0.003)	(0.003)	(0.003)
Total plot area	0.000	0.000	0.001**	0.000*
	(0.000)	(0.000)	(0.000)	(0.000)
Income (Non-agri)	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Mean of Control Group	0.0788	0.0604	0.0604	0.0723
Observations	1,526	1,526	1,526	1,526

Notes: The dependent variables are dummy variables that respectively take on values of 1 if farmers state receiving a leaf color chart, attending the training, using the chart and if they can show the chart to the enumerator, and 0 otherwise. Robust standard errors are shown in parentheses. All regressions include strata fixed effects. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

farmer. 56% of the treatment farmers stated they used the LCC compared to 5.5% of the control group farmers. Therefore, there were some cross-overs but it was very limited.

1.4.2 LCC Instructions & Expected Changes

Figure 1.1 shows four histograms that illustrate how farmers in the control group apply urea. The first chart shows the distribution of the number of days between planting and first urea application. About 13% of farmers apply urea at planting or before planting. Most farmers apply urea 15 days after planting, and less than 20% wait until 21 days. Therefore, most farmers apply urea early, in a period where returns may be lower. Most farmers apply urea twice and almost 40% apply urea three times as traditionally recommended. The third chart shows the distribution of urea per application and the average is 0.52 kilograms per decimal. The tail of this distribution are driven by farmers who apply only once. The last histogram shows the distribution of the number of days between flowering and last urea application. The last application can be timed close to flowering and the large duration in driven by people applying fewer than three times. There are no returns to urea after flowering (where the variable is negative), and very few farmers make the mistake of applying urea then.

In this study, I provided farmers in treatment group with an LCC and provided instructions on how to use the charts. Farmers were told to focus on a few simple instructions and a translated version of the handout is shown in Appendix Table A.2.[23] Farmers were told to start checking leaf colors in their field with the LCC 21 days after planting to determine if they need to apply urea, which is a later starting date compared to what we observe above. After applying urea on any date, farmers were instructed to check back in 10 days, to determine whether additional urea is needed. If the chart indicated that urea was not needed, farmers were told to check again in 5 days. During each application, they were advised to apply 9 kilograms of urea per 33 decimals of land (0.27 kg/decimal), which is lower than the mean application. The Bangladesh Rice Research Institute estimates that with an LCC most farmers will apply urea four times instead of recommended number of three applications.[24] Farmers were also instructed to stop at flowering, which the data suggest that most farmers already do.

Based on these instructions, there are several possible changes in behavior compared to prevalent practices. Farmers may delay urea application until 21 days after planting, apply urea more frequently and apply smaller quantities of urea per application. Farmers may improve timing of application (within the correct application period) so that they apply when leaves are light and delay application when leaves are dark. The instructions do not directly tell farmers to apply less urea overall or have more applications, but rather allow

[23]These were distributed during the refresher training sessions based on instruction developed by the Bangladesh Rice Research Institute (http://knowledgebank-brri.org/how-to-use-lcc.php), but simplified further.

[24]As stated in an instruction manual available at http://knowledgebank-brri.org/how-to-use-lcc.php.

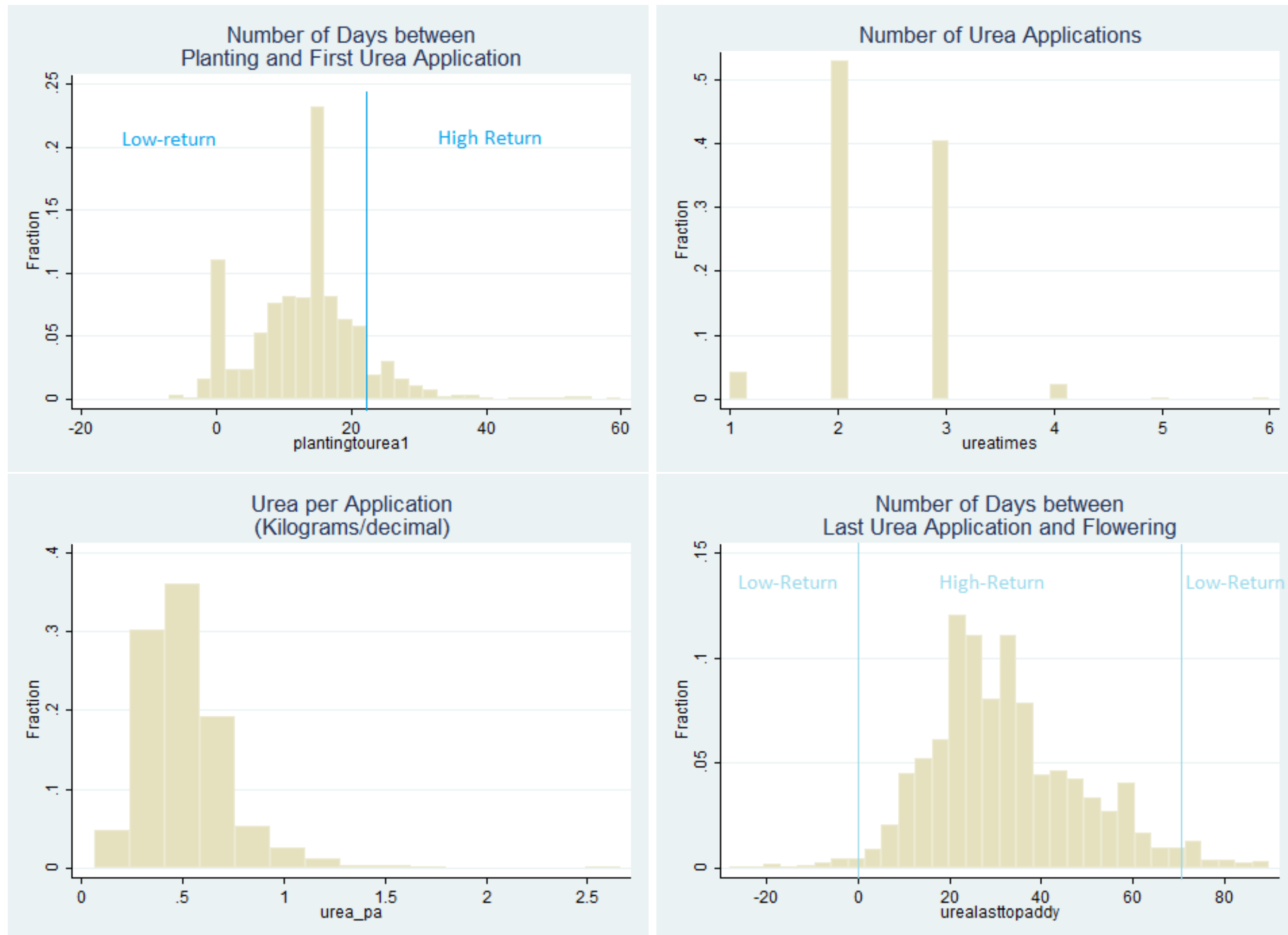

Figure 1.1: *Urea Application Patterns for Control Group*

the leaf colors to indicate if they should apply at any point in time. In addition to estimating overall treatment effects on urea use and yields, I explore if there is any evidence for the first three changes in the next section. It is not possible to directly test the last change in behavior, as we do not know when farmers check leaf colors.

1.4.3 Timing of Urea Applications

In this section, I identify changes in behavior by farmers in the timing of urea applications as discussed above. Specifically I test whether (i) farmers delay urea application until 21 days after planting, (ii) apply urea more frequently and (iii) if they apply smaller quantities of urea per application. In the last round of the midline survey, timed around the end of the urea application period, I collected data at the plot level for all farmers on urea application dates and quantities applied on each date. I use this data to estimate the changes discussed above. I also estimate whether farmers spend more time in their fields, as LCCs may encourage farmers to check leaf colors frequently.

Table 1.3 shows estimates of Equation 1.1 for several outcomes from the midline data with and without individual and household level controls. The dependent variable in column (1) is a dummy variable that takes on a value of 1 if the first urea application in a plot took place on or after 21 days after planting. Panel B presents the results without controls and shows that farmers in the treatment group are much more likely to have waited until 21 days to start urea application compared to the control group. About 11.9% of farmers in the control group wait 21 days, and this increases by 4 percentage points in the treatment group (significant at 1% level). The dependent variable in column (2) is a dummy variable that takes on a value of 1 if the last urea application took place after flowering, the time when farmers should stop applying urea. Farmers in the treatment group are much less likely to apply urea at this period (decline of 0.9 percentage points), although these results come from a very small number of farmers who make this mistake. The mean interval between urea applications overall declines by 0.55 days (significant at 10% level), which is likely due to the delay in start time.

Table 1.3

Changes in Behavior in Using Urea

	Overall Change			Change in Frequency			Change in Quantity		
	(1) Applied 1st Urea After 21 days	(2) Applied Urea After Flowering	(3) Mean Interval between Applications (days)	(4) # Times Urea Applied	(5) # Times Urea Applied High-return Period	(6) # Times Urea Applied Low-return Period	(7) Urea per app. (kg/dec.)	(8) Urea/app. High-return Period (kg/dec.)	(9) Urea/app. Low-Return Period (kg/dec.)
Panel A: Without Controls									
Treatment	0.042***	-0.009***	-0.541*	0.020	0.050*	-0.030	-0.011	-0.007	-0.031***
	(0.014)	(0.004)	(0.293)	(0.028)	(0.029)	(0.026)	(0.009)	(0.014)	(0.012)
Control Mean	0.119	0.0132	20.75	2.419	1.250	1.169	0.508	0.423	0.496
Observations	3,541	3,541	3,107	3,541	3,541	3,541	3,541	3,541	3,541
Panel A: Including Controls									
Treatment	0.040***	-0.009***	-0.551*	0.020	0.047*	-0.027	-0.011	-0.007	-0.030***
	(0.014)	(0.003)	(0.295)	(0.028)	(0.029)	(0.026)	(0.009)	(0.015)	(0.012)
Control Mean	0.119	0.0132	20.75	2.419	1.250	1.169	0.508	0.423	0.496
Observations	3,541	3,541	3,107	3,541	3,541	3,541	3,541	3,541	3,541

Notes: This table shows changes in urea application patterns overall, as well as within periods of high-returns and low-returns to urea. The high-return period is defined as 21 days after planting until 60 days after planting (expected time of flowering). The low return period is defined as any application within 21 days after planting or after 60 days of planting. Control variables include age, schooling, income, total plot area and baseline urea. Standard errors, shown in parentheses, are clustered at household level. All regressions include strata fixed effects. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Columns (4), (5) and (6) show estimates for differences in frequency of urea applications between the treatment and control groups. The dependent variable in column (4) is the total number of times urea is applied while this variables is split up into the number of applications at the period of high-returns and low-returns respectively.[25] There is no significant difference in the frequency of urea applications overall, but the coefficient in positive and significant at the 10% level in the high-return period. The coefficient on treatment for the number of applications at the low-return period is negative but not significant. Columns (7), (8) and (9) show treatment effects on average quantity of urea in each application overall, in the high-return and low-returns periods. The coefficients in columns for urea per application overall and urea per application in the high-return period are negative but not significant. There is a decline in urea per application of 0.03 kilograms per decimal in the low-return period, which is significant at the 1% level. This is a 6% decrease compared to the control group. The results are consistent without controls (Panel A).

Overall, these results show strong evidence that treatment farmers on average delay the starting date of urea applications to a more productive period and reduce urea used in the low-returns period. There is weaker evidence that suggests that the intervention increases the frequency of urea applications in the high-return period. Changes in the overall timeline of urea application (intervals measured in days) are shown in Appendix Table A.4.

In the second and fourth rounds of the midline surveys, a sub-sample of farmers were asked about time spent on various agricultural activities in the last seven days. The results are shown in Table 1.4. I compute Tobit estimates since the variables are highly censored at zero, but report OLS estimates in Appendix Table A.5. The dependent variable in column (1) is the number of days in the last week, the farmer visited his fields. The other dependent variables are total number of minutes spent in the last seven days on fertilizer application, weeding, applying pesticides and other activities in the field. Most of the coefficients are

[25]High-return period in the interval from day 21 after planting until the flowering date, and the low-return period is any time before or after that period.

Table 1.4

Tobit Estimates of Time Use by Farmers (7 day recall)

	(1) #Times in Field	(2) Fertilizer Application (minutes)	(3) Weeding (minutes)	(4) Pesticide Application (minutes)	(5) Other Activities (minutes)
Panel A: Without any Controls					
Treatment	0.154*	7.629	13.948	10.038	6.407
	(0.081)	(10.285)	(18.962)	(14.952)	(9.340)
Control Group Mean	2.700	50.31	57.35	4.471	38.85
Observations	2,066	2,066	2,066	2,066	2,066
Panel B: Including all controls					
Treatment	0.134*	7.949	10.047	9.245	2.200
	(0.079)	(10.186)	(18.639)	(14.903)	(9.130)
Control Group Mean	2.700	50.31	57.35	4.471	38.85
Observations	2,066	2,066	2,066	2,066	2,066

Notes: This table shows Tobit estimates of treatment effects on on time use by farmers using data from Rounds 2 and 4 of the midline surveys. The dependent variables in Columns (2) to (5) are total time spent in minutes in the last seven days on different agricultural activities. Control variables in Panel B include age, schooling, total plot area cultivated and non-agricultural income. Standard errors clustered at the household level are shown in parentheses. All regressions control for survey round and strata FE.
*** p<0.01, ** p<0.05, * p<0.1.

positive but not precise, partly due to insufficient statistical power because these data are from a smaller sample, however, it shows that treatment farmers visit their plots 0.13 times more often (significant at the 10% level).

1.4.4 Treatment Effects on Urea Use and Yield

Table 1.5 shows the ITT effects of gaining an LCC through the intervention on urea used and yields attained by farmers. Columns (1) and (4), shows the treatment effects without any controls. Controls for age and years of education of the farmer, non-agricultural family income, total area cultivated by the farmer and the variety of rice cultivated on the plot, are included in the rest of the regressions. Household fixed effects are also included in columns (3) and (6). The unit of observation is a plot and all regressions are clustered at the

Table 1.5

Full Sample: Treatment Effects on Urea & Yield

Urea & Yield in Kilograms per Decimal

	Urea			Yield		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment*Post	-0.074**	-0.079**	-0.089**	1.823**	1.757**	1.352
	(0.035)	(0.034)	(0.041)	(0.867)	(0.849)	(0.941)
Treatment	-0.001	0.001		-1.103	-1.035	
	(0.025)	(0.025)		(0.772)	(0.759)	
Post	0.059**	0.084***	0.088***	-3.416***	-3.238***	-2.932***
	(0.026)	(0.026)	(0.031)	(0.677)	(0.697)	(0.787)
Controls	No	Yes	Yes	No	Yes	Yes
Household FE	No	No	Yes	No	No	Yes
Mean at Baseline	1.011	1.011	1.011	25.73	25.73	25.73
Observations	8,144	8,144	8,144	8,144	8,144	8,144

Notes: This table shows treatment effects on urea use and yield. Control variables include age, schooling, total plot area cultivated, income, rice variety.
Standard errors clustered at the household level are shown in parentheses. All regressions include strata fixed effects.
100 decimals = 1 acre
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

household level and include strata fixed effects.

I find that, on average, urea use declines while yield increases for the treatment group relative to the control due to the intervention, and that these results are robust across the three specifications discussed above. Column (2), shows that having access to leaf color charts result in a decrease in urea use of 0.079 kilograms per decimal (significant at the 5% level). The coefficient is not significantly different without other control variables (Column (1)) or when household fixed effects are included (Column (3)). This is equivalent to an 8% decrease in urea use on average. Average area cultivated by farmers is about 66 decimals, so farmers in the treatment group save about 5.2 kilograms of urea on average, which is a savings of Taka 104 (USD 1.33). Column (5), shows that getting access to LCCs lead to an increase in yields of 1.757 kilograms per decimal (statistically significant at the 5% level), which is an increase of 6.8% from the mean baseline yield. The mean price of rice is Tk 15 per kilogram, so for average plot holding of 66 decimals, there is a gain of Tk 1739 in revenue (USD 22.3). The effect is not precise with household fixed effects, however, it is possible that standard errors are magnified in this specification due to the structure of the data.[26]

I also estimate the effects on total revenue, costs and profits for the farmers, to understand further the magnitude of the returns. As discussed in the section above, price data of inputs and details on quantities used for non-fertilizer inputs are only available at the baseline for the “long survey" sample of farmers so I estimate two sets of regressions. Columns (1) to (3) of Table 1.6 shows the difference-in-difference estimates for revenue, total cost and profits for farmers for the “long survey" sample. The difference between the treatment and control groups at endline are estimated for all farmers in the study and columns (4) to (6) shows the estimates for revenue, costs and profits.

Panel B shows estimates after controlling for individual, household characteristics and rice variety. For the sample for whom price data are available, revenue increases by Tk 34.4 per decimal (significant at 5% level), total cost is higher by Tk 20 per decimal for the treatment group but it is not significant. Profits are higher by Tk 14 per decimal and is also not statistically significant. Using endline data for all farmers in the sample, revenue is higher by Tk 10 per decimal for the treatment group (significant at 5% level), total cost are positive but not statistically significant.[27] The results in Panel A (without controls) are similar.

Overall, the treatment effects are substantial, particularly in savings of urea. Back of the envelop calculations discussed above show large quantities of savings of urea and higher revenue. This implies inefficiencies exists in the way urea is applied by the average farmer. With better information or signals, that farmers obtain due to this intervention, they are

[26]Estimates from an alternative specification using logs of urea per decimals and logs of yield per decimal is shown in Table A.6. The results are consistent overall, however the estimates for effect of urea have a larger magnitude while that for yield have a smaller magnitude and lose precision. Based on these estimates, urea use decreases by 12% (significant at 1% level) while yields increases at 3.8% but is not significant.

[27]There are some concerns about the quality of the price data in the baseline and endline surveys, and some of the variables are much more noisy compared to other measures that were collected. To address this concern, I collected price data retrospectively at the village level (from local fertilizer stores) in March 2014. Table A.7 in the Appendix estimates the same regressions using price data collected from the villages. The results are consistent and of similar magnitude as the first set of estimates although profits for the long survey sample are no longer significant.

Table 1.6

Revenue, Cost & Profits

All dependent variables in Takas per decimal

	Long Survey Sample			Full Sample		
	(1)	(2)	(3)	(4)	(5)	(6)
	Revenue	Total Cost	Profit	Revenue	Total Cost	Profit
Panel A: Without Controls						
Treatment*Post	35.597**	16.940	18.657			
	(15.810)	(16.973)	(20.061)			
Treatment	-21.416	-13.413	-8.003	9.453**	4.126	5.327
	(13.503)	(9.170)	(12.968)	(4.660)	(10.514)	(11.351)
Post	-30.629**	39.619***	-70.248***			
	(12.724)	(11.114)	(14.136)			
Means (Baseline/control group)	352.3	240.0	112.3	344.0	289.1	54.92
Observations	6,102	6,102	6,102	3,632	3,632	3,632
Panel B: Including Controls						
Treatment*Post	34.412**	15.998	18.414			
	(15.454)	(16.873)	(20.001)			
Treatment	-19.615	-11.429	-8.186	10.035**	5.213	4.950
	(13.164)	(8.982)	(12.894)	(4.626)	(10.672)	(11.636)
Post	-28.206**	42.406***	-70.612***			
	(13.348)	(11.193)	(14.531)			
Means (Baseline/control group)	352.3	240.0	112.3	344.0	289.1	54.92
Observations	6,102	6,102	6,102	3,632	3,632	3,632

Notes: Controls variables include age, schooling, total plot area cultivated, non-agricultural income and rice variety. Standard errors clustered at the household level are shown in parentheses. All regressions include strata fixed effects.
100 decimals = 1 acre
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

now able to both save urea and benefit from higher yields. The results on changes in timing of urea applications in the previous section suggest that the reduction in urea use observed overall comes from a reduction in urea used during the low-returns period.

The change in start date is not sufficient to explain an increase in yield, as applying urea before the third week will not harm the crop. However, an increase in yield can be explained if farmers improve timing of urea application within the period of high returns. There is some evidence that the treatment group farmers visit their fields more often and apply urea more frequently in the high-returns period, although the coefficients are small as discussed in the previous section. These results provide suggestive evidence that treatment farmers may learn to improve the timing of urea use and spend more time on fertilizer application to ensure that returns to urea are higher.

1.5 Who Benefits from the Intervention?

In this section I discuss who benefits from the intervention. I estimate quantile regressions of urea and yield to identify any changes in these distributions due to treatment in section 1.5.1. I also investigate whether there is any evidence for heterogeneous treatment effects by time preferences, cognition or income in section 1.5.2.

1.5.1 Estimates from Quantile Regressions

As an LCC will encourage farmers who underuse to use more urea and farmers who overuse to use less urea, we may expect non-linear responses. To explore how the distributions of urea use and yield change with access to LCCs, I estimate quantile regressions for both. I control for individual, household and plot characteristics and strata fixed effects and cluster errors at the household level. Figure 1.2 shows the results of the quantile regressions, and reports coefficients at 0.1 quantile intervals from 0.1 quantile to 0.9 quantile. The figure shows that the full distribution of urea use shifts downwards for the treatment group. We cannot rule out that the coefficients are significantly different from one another. There is no significant change in the distribution of yield, however, the largest increase occurs at the highest end of the distribution. These results suggest that there is potential to save urea without sacrificing yields at all levels of the distribution. It also shows that the largest treatment effects come from farmers with the highest yields at baseline.

1.5.2 Treatment Effects by Patience, Cognition & Income

Treatment effects for households in the study may vary by characteristics of the primary farmer who makes agricultural decisions or by characteristics of the household. Since the timing of urea applications are important and as the LCC encourages farmers to check their fields regularly, the treatment effects may vary by time preferences or the level of patience of the primary farmer. An LCC is an easy-to-use tool and instructions to use the LCC in this intervention were simplified as much as possible, however, the ability to use the tool correctly may still depend on the cognitive abilities of the primary farmer. Finally, treatment effects may vary by the level of baseline household income if poverty acts as a constraint on whether farmers choose to take-up this tool.

At the endline survey, farmers were asked a series of standard questions to determine their time preferences. For the first set of questions, farmers were asked to choose between (hypothetically) receiving 1000 takas today or one month later, if they stated they would prefer to receive the money today they were asked what they would prefer in a choice between 1000 takas today or 1100 takas one month later. The stakes were increased incrementally and based on these questions I create a variable that measures where farmers switch from stating a preference for today to stating a preference for a larger amount tomorrow, which I use as a proxy for patience. I use a second set of similar questions with higher stakes (starting at 100,000 takas) to compute an additional measure of time preference. At the endline survey, farmers were given a short math quiz and a Raven's test, and scores were computed for each based on the number of correct answers.[28] I use both as measures of cognition. Ideally, these data would have been collected at baseline.

[28] 15 puzzles were chosen from the standard Raven's progressive matrices after piloting in a similar location outside the study area to ensure sufficient variation in responses.

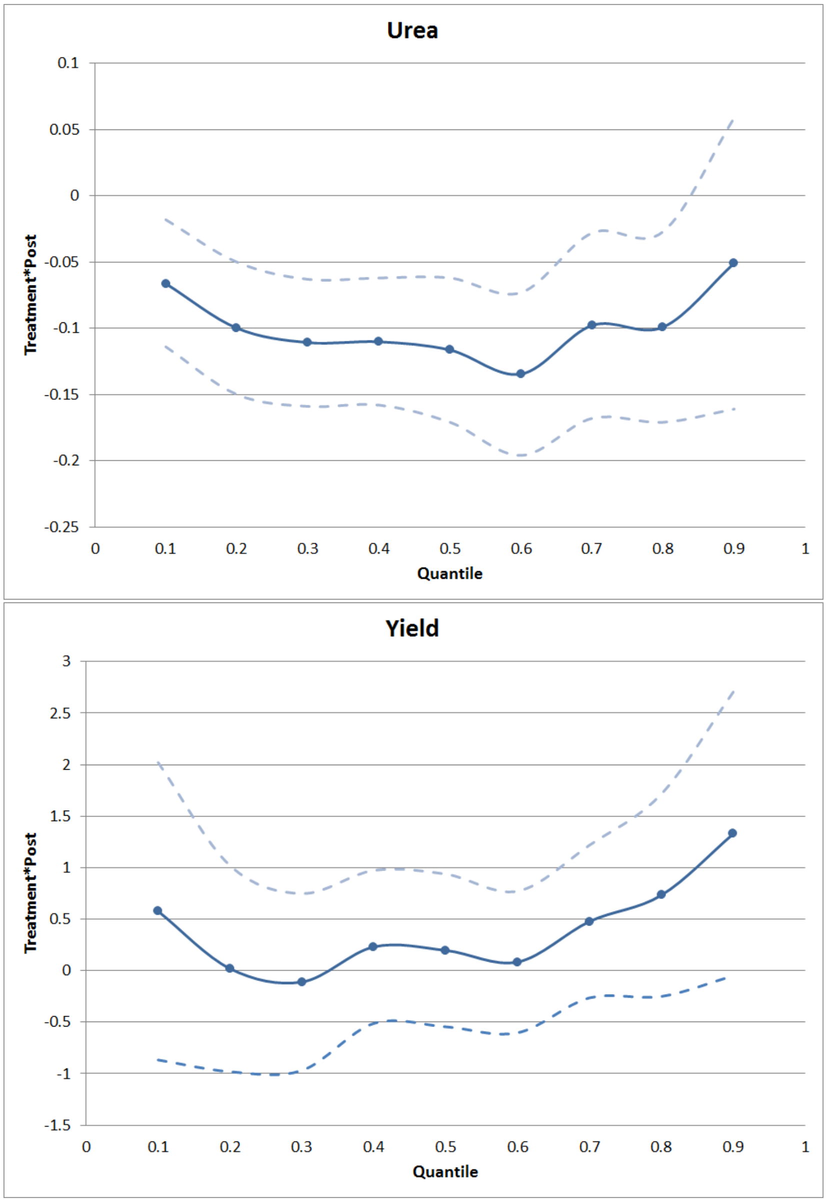

Figure 1.2: *Quantile Regressions*

The figures report estimates from quantile regressions of urea use and yield on $Treatment_h * Post_t$. The regressions also included covariates for $Treatment_h$, $Post_t$, controls for age, schooling, income and total plot area, rice variety and strata fixed effects. Standard errors are clustered at the household level. The quantiles are from 0.1-quantile to 0.9-quantile at 0.1-quantile intervals. 95% confidence intervals are shown. The dotted line shows the estimate of the corresponding OLS coefficient.

However, time preferences or cognition are unlikely to change due to treatment, therefore, I use the endline measures to estimate whether treatment effects differ by measured levels of patience or cognition. I also estimate whether treatment effects vary by baseline levels of non-agricultural household income. To do so, I estimate Equation 3 for each of these measures.

$$\begin{aligned} y_{pht} \;=\; & \beta_0 + \beta_1 Treatment_h + \beta_2 Post_t + \beta_3 Treatment_h * Post_{ht} + \beta_4 C_h \\ & + \beta_5 C_h * Treatment_h + \beta_5 C_h * Post_h + \beta_6 C_h * Treatment * Post_h \\ & + \rho X_{ht} + \delta Z_{pht} + \gamma_s + \epsilon_{pht} \qquad (1.3) \end{aligned}$$

C_h is an individual or household characteristic, such as time preference and cognition of primary farmer or non-agricultural household income. All other variables are the same as before. Table 1.7 shows estimates of β_6 that tests whether treatment effects differ by time preferences, cognition or income. The sample sizes are smaller since these measures were collected at endline and the response rate was lower compared to the other modules in the survey. Overall, I find no differences in treatment effects on urea or yield for any of these measures suggesting that treatment effects are the same across the distribution of farmers for these characteristics. The coefficient showing treatment effect on yield by the low-stakes time preference variable is marginally significant at the 10% level in Panel A, but becomes imprecise when I include controls for age, schooling and total plot area cultivated. The treatment effects for urea do not vary by the level of patience using either measure and there are no differential effects on yields using the second measure for time preferences. There is no heterogeneity in treatment effects by cognition using either math scores or Raven's scores, suggesting that the tool was easy enough for everyone to use.[29] Treatment effects do not differ by baseline non-agricultural income, which suggest that for the farmers in this study resource constraints did not hinder the ability to take up and use the chart. This is

[29] I also find no difference in treatment effects by years of schooling using a similar specification (results not presented).

not surprising, as the LCC was provided free of charge and did not require any significant investments later on.

1.6 Cost-Effectiveness of Intervention

Table 1.8 shows a cost-benefit analysis of the intervention and an estimate of the cost-effectiveness. Each LCC costs US $1.3 including shipping from Philippines and indirect fees. The expenses for the intervention included honorariums for DAE trainers, refreshments during training sessions, transportation costs and direct expenses incurred by CDIP workers to arrange the local training sessions and printing expenses for training materials. After including these expenses, the total cost per LCC is approximately $2.60.

To estimate benefits, I use treatment effects on urea and yield to compute back-of-the-envelope estimates of urea saved and yield gained for the mean farmer. On average farmers, cultivate rice on 66 decimals of land. Using the official price of urea and the average reported price of rice at the endline survey, I estimate that farmers save $1.34 on average from reducing urea use. This amount is larger than the cost of one LCC. I also estimate that the average farmer gains $22.34 additional returns from higher yields. Combining both, the total benefit is $23.68. Overall, the cost-effectiveness of the intervention is 9.10, i.e. every $1 spent on the intervention generated a return of $9.10. The cost-effectiveness is much higher when we consider the fact that the costs are a one-time expense, however, the LCC is durable and can be used by the farmer for many years. Moreover, these estimates show returns during the *Boro* season, but the LCC can also be used during the *Aman* season, although returns may be lower as average yields are lower in *Aman* compared to *Boro* season.

I use estimates for treatment effects on yields rather than treatment effects on revenue and profits, since I do not have data on revenue for all farmers, and costs and profits are imprecisely estimated. The cost-effectiveness estimate for one season will be higher ($11.6) if we use the estimated treatment effect on revenue. Profits are positive but not statistically significant, but using the point estimate for profits, the cost-effectiveness estimate over one season is $6.12. If we assume that there were no changes in profits and that the only

Table 1.7

Treatment Effects by Time Preferences, Cognition and Baseline Household Income

Urea & Yield in Kilograms per Decimal

	(1) Urea	(2) Yield	(3) Urea	(4) Yield	(5) Urea	(6) Yield	(7) Urea	(8) Yield	(9) Urea	(10) Yield
Panel A: Without Controls										
Time Preference (Low Stakes)*Treatment*Post	0.033	0.849*								
	(0.021)	(0.460)								
Time Preference (High Stakes)*Treatment*Post			-0.008	0.228						
			(0.022)	(0.506)						
Math Score*Treatment*Post					-0.013	-0.253				
					(0.030)	(0.806)				
Ravens Score*Treatment*Post							0.055	0.789		
							(0.037)	(1.118)		
Non-agri Income*Treatment*Post									0.002	-0.030
									(0.003)	(0.074)
Mean at Baseline	1.011	25.73	1.011	25.73	1.011	25.73	1.011	25.73	1.011	25.73
Observations	7,080	7,080	7,080	7,080	7,080	7,080	7,080	7,080	7,468	7,468
Panel B: Including Controls										
Time Preference (Low Stakes)*Treatment*Post	0.026	0.706								
	(0.021)	(0.443)								
Time Preference (High Stakes)*Treatment*Post			-0.015	0.077						
			(0.021)	(0.494)						
Math Score*Treatment*Post					-0.010	-0.263				
					(0.030)	(0.799)				
Ravens Score*Treatment*Post							0.051	0.654		
							(0.036)	(1.086)		
Non-agricultural Income*Treatment*Post									0.002	-0.039
									(0.003)	(0.074)
Mean at Baseline	1.011	25.73	1.011	25.73	1.011	25.73	1.011	25.73	1.011	25.73
Observations	7,080	7,080	7,080	7,080	7,080	7,080	7,080	7,080	7,468	7,468

Notes: Controls include age, schooling, total plot area cultivated and rice variety. Regressions in columns (1)-(6) also control for non-agricultural income in Panel B. Coefficients not shown for the variables Treatment, Post, Treatment*Post, the specific characteristic variable in each column as well as the interactions of the variable with the Treatment and Post variables. Standard errors clustered at the household level are shown in parentheses. All regressions include strata fixed effects. Time preference variables range from 0 (most patient) to 6 (least patient). Math scores and Raven's score measure the number of correct answers and range from 0 to 7 and 0 to 8 respectively. Non-agricultural income is the reported month non-agricultural income in 1000 Takas per month as reported at the baseline survey. 100 decimals = 1 acre. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 1.8

Cost-Benefit Analysis of Program

Costs:	
Cost of 1000 LCCs[1]	$1,100
Costs of Training & Distribution[2]	$1,500
Total cost of intervention	$2,600
Direct Cost per LCC	$1.10
Total Cost per LCC	$2.60
Benefits:	
Savings in Urea for Mean Farmer	$1.34
(0.079 kg/dec. urea saved *66 decimals of land*Tk 20/kg of urea*$0.012/Tk)	
Increase in Returns for Mean Farmer	$22.34
(1.76 kg/dec yield gain.*66 decimals of land*Tk 15/kg of rice*$0.012/Tk)	
Total Benefit per farmer per season	$23.68
Cost-Effectiveness (Benefits/Costs):	9.10

Notes: [1]Includes cost of importing 1000 LCC from the Philippines, including shipping ($1000) and bank and agent fees ($100).
[2]Includes honorarium for DAE trainers, refreshments during training, transport of LCCs, additional training costs for CDIP staff and printing.
I use the DD estimates of treatment effects of urea and yield from Table 1.5.
The average land area cultivated for rice is 66 decimals, price of urea is Tk 20/kg (official price) and mean reported price of rice is Tk 15/kg.
I use an exchange rate of 1 USD = Tk 78 to convert returns to dollars.

treatment effect came from savings in urea of $1.34, even in that scenario the program is cost-effective if the LCC is used for two seasons.

1.7 Conclusion

This paper explores whether there is potential for productivity gains through better management of chemical fertilizers. While it is challenging to learn how to reduce wastage of urea, farmers can learn to do so by paying attention to the timing of urea fertilizers and getting cues from the color of the rice leaves to determine whether the crop is getting sufficient nitrogen. In this study, through a field experiment, I provide rice farmers in the treatment group with an LCC, a simple tool applying rule-of-thumb learning, that helps with the management of urea fertilizers. I find that farmers save urea by 8% on average when they gain access to a leaf color chart, and in addition they may also benefit from an increase in yield, which suggest a failure to learn how to effectively apply urea without help from the chart, although farmers in the country have had decades of experience in using urea. In particular, I find that farmers make the error of applying urea too early in the season, when the returns are lower and they are likely to correct this error once they have access to an LCC. I also find that there is scope to save urea by farmers at all levels of the distribution and that the largest gains in yield come from farmers who were performing relatively better at baseline.

An LCC may be effective in improving urea management due to several features, most important of which is the ability to produce clear signals on nitrogen sufficiency and provide simple rules to follow, which reduce the complexity of learning the urea application process. A leaf color chart reduces both the cost and the risk associated with experimenting with urea and also focuses attention on a key dimension of input. The literature on learning presents several reasons why farmers fail to adopt improved agricultural practices. Lack of information, poverty and resource constraints, and risk preferences can all lead to poor adoption or sub-optimal use of inputs (Jack, 2013; Marenya and Barrett, 2007; Liu, 2013). Leaf color charts can help farmers in the presence of many of these barriers. The LCC intervention provides basic information on timing and the significance of leaf colors and when they use an LCC the farmers get understandable signals in real time on how they are performing. Farmers now know that if leaves are dark, it means that the crop is healthy and has sufficient nitrogen. If they make a change in how they apply urea, the LCC shows them clearly whether the crop is being harmed, instead of having to wait until harvest, so farmers may be more willing to experiment.

The literature shows that behavioral constraints may limit how much farmers learn from experiments. Since there are many input dimensions, farmers with limited attention may fail to notice important aspects of the production process (Hanna, Mullainathan and Schwartzstein, 2014). If farmers fail to notice leaf colors or understand the relationship between urea applications and leaf colors, then an LCC focuses their attention to this important dimension of the cultivation cycle. Alternatively, an LCC may be effective due to its application of rule-of-thumb learning. The literature demonstrates the potential effectiveness of using simple rules to promote learning. Drexler, Fischer and Schoar (2014) conduct a field experiment with micro-entrepreneurs to promote financial literacy, and find that a simplified rule-of-thumb training is much more effective than a more-complex training program.

This paper's key contribution to the literature lies in demonstrating that measures of overuse or underuse of chemical fertilizers is insufficient in understanding whether farmers use fertilizers efficiently. Returns to fertilizers also vary by timing and attention should be paid to this dimension. The findings in this paper have several implications for policy. There is significant scope to improve productivity by improving the management of urea. This result holds even for farmers who perform well at baseline. LCCs are very cost-effective, and therefore disseminating LCCs to farmers in the region can lead to large gains. Policymakers and researchers should also explore other inputs that have the potential to be mismanaged. Although considerable resources are devoted towards agriculture extension, it is often reported to be insufficient. In this study, I utilized the existing network of a micro-finance organization without significant experience in agriculture to distribute the

LCCs. Although extension workers were invited to conduct the primary training, CDIP workers were effective in reaching farmers and providing training that emphasized the simplicity of the rules. Therefore, for rule-thumb technology, there is significant scope to speed up awareness and dissemination by making use of other networks to complement traditional agriculture extension.

Chapter 2

Trainer Quality and Adoption of Leaf Color Charts

2.1 Introduction

Improving agricultural productivity in developing countries has been considered essential to reduce poverty and promote growth (deGraft Johnson et al., 2014; Gollin, 2002). To improve productivity, it is necessary for farmers to learn to adopt new innovations and therefore, it is important to understand the barriers to technology adoption. There is considerable evidence that constraints faced by farmers such as lack of information, poverty and resource constraints, poor infrastructure leading to high costs of inputs (Jack, 2013; Marenya and Barrett, 2007; Suri, 2009) as well as behavioral constraints such as time inconsistency or limited attention (Duflo, Kremer and Robinson, 2011; Hanna, Mullainathan and Schwartzstein, 2014) may limit learning and adoption of improved agricultural practices. However, in addition to the circumstances faced by farmers, the way in which information on new technology is disseminated may also determine adoption and improvements in productivity (Anderson and Feder, 2007).

Agriculture extension has been a key channel through which governments in developing countries have aimed to promote the diffusion of new technology. There were approximately

500,000 agricultural extension personnel worldwide in 2005, with 95 per cent of them working in public agricultural extension systems (Anderson and Feder, 2007). Despite considerable investments in extension services by both government and international organizations, agriculture extension has not been very successful in many countries (Feder et al., 2010). Poor infrastructure, insufficient political support, lack of financial sustainability of some extension models have been major reasons for low success rates of public extension programs (Anderson and Feder, 2007; Rivera, K. M. and Crowder, 2001). However, there is very limited empirical evidence of what mechanisms can make extension effective (Kondylis, Mueller and Zhu, 2014). There is also little understanding of the role of extension workers. Even if farmers overcame the constraints above, the decision to adopt a new technology and the ability to gain from adoption may depend on how much farmers trust the trainers and how effectively the trainer transmits information. Although poor technical knowledge of extension workers, insufficient training, lack of communication skills and other shortcomings are often mentioned, but the importance of each of these are not well known.

In this paper, I explore whether characteristics of agricultural trainers determine the adoption and use of leaf color charts (LCC), a new tool that that can help farmers improve the timing of fertilizer applications for urea, a widely-used nitrogen fertilizer. In a field experiment in Bangladesh, in partnership with a local micro-finance institute, I distributed LCCs to farmers who were randomly assigned to the treatment group. Treatment group farmers received leaf color charts in a training session in their village, where they received the chart and obtained training from workers from the Department of Agricultural Extension (DAE). As described in Chapter 1, I find that 56% of the treatment group farmers state that they used the chart, and that on average they have high gains as they reduce urea use by 8% and improve yields by 7% on average after receiving access to the chart. Performance measures for extension workers are rarely collected, and this setting provides an opportunity to estimate proxies for performance and quality of trainers. In this chapter, I explore whether trainers are a significant predictor of adoption of and gains from leaf color charts for farmers in the treatment group. I also examine whether trainer quality is related to any observable trainer characteristics.

The intervention and training took place in January 2013, and using records from the training sessions which included the name and contact information of the trainer, I collected data on the background of the trainers through phone surveys in February 2014. The data include information on the age, gender, years of experience, highest degree and type of trainer (extension workers, senior DAE trainers or others). There were 58 trainers in total, although response rates varied by the question. These variables may be related to the ability of the trainer to effectively communicate information about LCCs to farmers or influence the perception of the trainer by the farmers, both of which can determine the farmer's decision to adopt the leaf color chart as well as the effectiveness with which he is able to use the chart.

To explore the importance of trainers, I estimate predicted values of trainer fixed effects for several measures of take-up and outcomes for farmers. It is not possible to directly observe take-up in this context; however, I use two measures from the endline data in 2013, collected after the crop was harvested at the end of the season after the intervention. Farmers were asked if they used a leaf color chart during the season. At the same time, farmers were also asked to show the LCC to the enumerator conducting the survey (if they stated that they had a leaf color chart). I use both as proxies for take-up. As measures of gains from treatment, I compute the mean changes in urea use and yields for each household. Based on the analysis in Chapter 1, farmers who adopt effectively are likely to reduce use of urea and may have higher yields. I estimate regressions for each of these outcomes on dummy variables for individual trainers and compute the predicted values of trainer fixed effects. I use the variation in predicted trainer fixed effects as a measure of the importance of fixed trainer quality, which I show using histograms of the predicted values. The results indicate that there is some variation in the predicted values of the trainer fixed effects.

I also estimate whether trainer quality is related to any observable trainer characteristic. To do so, I estimate regressions of the predicted values of trainer fixed effects on various trainer characteristics. Overall, there is no evidence that characteristics such as age, gender and experience of trainers determine trainer quality. There is some evidence that trainer performance may be negatively correlated with being a university graduate or an extension worker. Due to limitations of the data, I cannot rule out the possibility that unobservable characteristics of villages that determine both adoption rates by treatment farmers as well as they type of trainer they receive drive the results.

The rest of the paper is organized as follows. Section 2.2 discusses the role of agricultural extension and the motivation for this analysis. Sections 2.2 and 2.4 describe the context, data and the estimation strategy. Section 2.5 presents the results and section 2.6 concludes.

2.2 Background and Motivation

Improving agricultural productivity has been an important component of development strategies to improve income and well-being in developing countries, particularly due to the large number of people often engaged in agriculture (Aker, 2011; Owens, Hoddinott and Kinsey, 2003). Agriculture extension acts as a mechanism through which information on new technologies, improved farming practices and better management can be delivered from agricultural researchers and experts to farmers to improve farm productivity (Birkhaeuser, Evenson and Feder, 1991). Extension workers can help reduce the difference between potential and actual yields in farms by providing information to farmers on new technology as well as helping farmers become better managers (Anderson and Feder, 2007). As a result, governments and international organizations have invested in agricultural extension since the 1960s. There were approximately 500,000 agricultural extension personnel worldwide in 2005, with 95 per cent of them working in public agricultural extension systems (Anderson and Feder, 2007).

Although governments and international development agencies have invested considerable resources in the past five decades in agriculture extension services in developing countries, the performance of agriculture extension is considered to be disappointing overall in many places (Feder et al., 2010). Despite decades of experience with various extension programs and new technologies, adoption rates of new technology and yields remain relatively low in many developing countries (Aker, 2011). On the other hand, in countries such as India, the classic training and visit model of agriculture extension which has overall failed, played a key role in bringing in the green revolution (Sharma, 2002).[1] In a Food and Agriculture Organization of the United Nations (FAO) review in 2001, Rivera, Qamar, and Crowder state that many extension services across the world were in disarray or barely functioning at all (Rivera, K. M. and Crowder, 2001). Insufficient resources and very low ratios of extension workers to agricultural households may mean that overall performance of extension has been poor because the services are not reaching most farmers. It is therefore important to understand whether extension services are effective when they reach the targeted communities.

Reviews of the literature such asEvenson (1997) andAnderson and Feder (2007) find mixed results on the impact of extension on farm performance. There are cases of very high returns on extension investment and other cases of negligible returns (Anderson and Feder, 2007; Bindlish and Evenson, 1997; Davis, 2008; Gautam, 2000). The observed impact of extension depends on the way in which extension services are provided, as well as the circumstances of the farmers who receive extension service (Anderson and Feder, 2007). Overall, it is often difficult to measure the causal effect of extension due to limitation of the data or the design and many earlier studies suffer from problems of selection bias of both communities and farmers which may lead to positive findings (Owens, Hoddinott and Kinsey, 2003). Recent research has focused both on understanding constraints to technology adoption and learning by farmers, such as poor infrastructure leading to high costs (Suri, 2009), poorly developed input delivery systems (Shiferaw, Kebede and You, 2008), time inconsistency (Duflo, Kremer and Robinson, 2011) or limited attention (Hanna, Mullainathan and Schwartzstein, 2014). Recent work has also looked at improving adoption by farmers by modifying and providing alternatives to traditional extension (BenYishay and Mobarak, 2014; Kondylis, Mueller and Zhu, 2014). Within tradition extension, the characteristics of

[1] In the Training and Visit model, an extension worker provides information on new technologies to a contact farmer in the village, who in turn can disseminate the information to other farmers (Kondylis, Mueller and Zhu, 2014).

extension workers that may determine effectiveness of extension work is less well studied.

Even if farmers did not face the constraints discussed above, adoption can be influenced by the quality of extension workers. Extension workers are responsible for educating farmers about new technology such as improved varieties, cropping techniques, optimal input use, prices and market conditions, more efficient methods of production management, storage, nutrition, and others subjects (Anderson and Feder, 2007). Therefore, extension workers need to be knowledgeable on a wide range of topics and as new technology and methods are developed, they need to keep updating their knowledge. Besides having the knowledge on all relevant topics, to teach farmers effectively, extension workers also have to communicate the information to farmers. In addition, they require the ability to spot and diagnose problems and have economic-management and risk-management skills to help farmers use resources efficiently (Anderson and Feder, 2007).

Therefore, the effectiveness of extension workers may depend on their quality which may be related to their education background and technical training, experience in working with farmers, communication skills, cognitive ability and problem-solving skills and many other observable and unobservable characteristics. The knowledge of the workers will also depend on access to training. In a study on the perception of agriculture extension staff in Bangladesh, insufficient training facilities for extension workers, lack of periodic training, and lack of performance appraisals were among the serious concerns raised by the workers who were surveyed (Reynar and Bruening, 1995). Performance of extension workers are not commonly measured, therefore, there are limited studies that look at predictors of work performance by extension workers. Some studies with agriculture extension workers have found strong correlations between work performance and quality of work life (Jamilah et al., 2010) and measures of leadership competency (Khalil et al., 2008). Besides the actual quality of trainer, a farmer's decision on whether or not to adopt the new technology that the extension worker is introducing may depend on the farmer's perception of their ability, as found in another context of health-care workers in Bangladesh. In the study, perceived quality of the healthcare worker by respondents was strongly related to the adoption of a family planning method (Koenig, Hossain and Whittaker, 1997). Due to cultural norms, age and gender of trainer may influence farmers' perceptions of and trust in the trainer. Incentives faced by the trainer may also influences his or her effectiveness.

In this paper, I study whether trainer quality plays a role in the adoption of a new tool called a leaf color chart (LCC) and whether quality is related to observable characteristics of the trainers who introduced the LCC to the farmers. Since farmers in the study were geographically dispersed across 21 sub-districts in the country, each of which has its own agriculture extension office, there is variation in the background characteristics of the trainers. Other than age and gender, I also examine whether adoption is correlated with experience of the trainers. With experience, trainers may be able to improve their communication skills, diagnose problems more effectively and gain more practical knowledge. On the other hand, younger trainers, or trainers who completed their education more recently may have more updated knowledge of best agricultural practices due to limited re-training of extension workers. One of the shortcomings of the classic training and visit extension system is that extension workers may transmit poor quality information to the farmers (Kondylis, Mueller and Zhu, 2014). The information loss may be due to lack of technical skills or poor knowledge of extension workers (Hoque and Usami, 2007). I investigate whether there is any correlation between trainer performance and the highest degree of the trainer, to examine if the level of education of the trainer is related to adoption. In a field experiment,Kondylis, Mueller and Zhu (2014) find that training farmers centrally can have better outcomes for adoption of some technologies compared to the traditional model of training by an extension worker. I examine whether there are any similar patterns in this context and compare differences in performance between extension workers compared to other trainers (usually higher ranked extension personnel).

2.3 Context and Data

The data used in this paper come from a field experiment in Bangladesh with 2045 rice farmers from 2012–2013 to evaluate whether receiving access to a simple rule-of-thumb tool called a leaf color chart (LCC), can improve the timing of urea, a popular chemical fertilizer. The role of LCCs in improving efficiency of urea use is described in detail in the previous chapter. As estimated in Chapter 1, I find that 56% of the treatment group farmers state that they used the chart, and that on average they have high gains as they reduce urea use by 8% and improve yields by 7% on average after receiving access to the chart.[2] In this chapter, using additional data collected in 2014 on the background of trainers who had led the primary training during the intervention to distribute LCCs, I examine whether trainer quality is determined by observable characteristics of the trainers.

The main study was conducted in partnership with the Center for Development Innovation and Practices (CDIP), a non-government organization in Bangladesh. I implemented the study in 105 villages under 20 CDIP branches spread across 21 sub-districts in the 8 districts of Brahmanbaria, Chandpur, Comilla, Gazipur, Lakhipur, Munshiganj, Naranganj and Noakhali. Through a randomized control trial, I provided farmers in the treatment group with an LCC as well as basic training on how to use the chart. Treatment farmers were invited to attend a training session in their village in January 2013 at the beginning of the *Boro* (dry) season. The training session was organized by local CDIP staff and usually led by an extension worker or agriculture officer invited from the Department of Agricultural Extension (DAE).[3] During the session, each farmer received an LCC and instructions on how to use the chart. Typically, the DAE representative introduced the LCC and provided instructions and demonstrations on how to use the LCC as a tool to make decisions on the quantity and timing of urea applications. In a few villages, the main training was led by CDIP staff (after receiving training from DAE representatives) as the DAE personnel were unable to go to the village on the dates selected. CDIP workers conducted home visits for households that did not attend the training, to provide the LCC and instructions.[4]

[2] I also find that farmers apply urea too early in the season, during a period when it is likely to be wasted, and they improve this behavior once they receive access to LCCs.

[3] I sent requests separately to each DAE office at the district level. The deputy directors from the district-level offices forwarded the request and authorization to each office at the sub-district level from which trainers were assigned. CDIP coordinated with the sub-district offices to arrange the training sessions.

[4] Conducted for approximately 5% of the sample according to CDIP administrative records, while either

The training sessions were generally held just before or around the time of planting.[5] The training provided simple rules on when to check leaf colors with the LCC and when to apply the fertilizer. However, while standardized instructions were available, there is likely to be variation in the way the training sessions were conducted in practice.

Prior to the intervention, I conducted a baseline survey that collected data on urea used and yields obtained in the *Boro* season of 2012. I conducted a detailed endline survey at the end of the season in 2013 after the intervention, to determine any changes in urea use and yields caused by access to LCCs. The records from the training sessions included information on the name, designation and mobile number of the trainer in each village. Using these records, I subsequently collected data on the background of the DAE trainers who participated in the study through phone surveys.

In the 105 villages included in the study, the primary training sessions were conducted by 58 trainers. While most of the trainers were DAE representatives, 3 were study/CDIP staff who were trained by the DAE personnel and filled in for them in 10 villages.[6] At the time of the training, records were kept of each trainer's name and designation. After the study ended, surveys were conducted over the phone in February 2014 to collect information on the education background of the trainers. Table 2.1 shows the summary statistics for the trainers. The mean age of the trainers is 46 years and 10% of the trainers are female. The average years of work experience is 21 years.[7] The trainers have three types of highest educations degrees. 80% have a diploma in agriculture, which is less than a bachelor's degree but consists of coursework completed after high school. 11% have a bachelor's degree and 9% have a master's degree. The next panel shows the designation of the trainer.

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the primary farmer or a representative from all remaining households attended the training. However, at the endline survey only 75% of the treatment farmers state that they received an LCC, and 59% of the treatment (primary) farmers state that they attended the training session.

[5] CDIP staff also conducted a more informal refresher training (individually with farmers or in small groups) a few weeks after the main training (before the time urea is generally applied).

[6] There were time constraints as the primary training had to be completed before planting, therefore, after being trained by a DAE representative in a separate session and observing some training sessions in the villages, 3 study/CDIP staff led the training in 10 villages on dates in which the DAE representatives had conflicts.

[7] This variable was coded as the number of years since the highest degree.

Table 2.1

Summary Statistics for Primary Trainers

	Mean	Standard Deviation	Observations
Basic Information:			
Age (years)	45.46	9.72	52
Proportion Female	0.10	0.31	58
Experience(Years)	20.65	10.40	54
Highest Degree:			
Proportion with Diploma	0.80	0.41	54
Proportion with Bachelors	0.11	0.32	54
Proportion with Masters	0.09	0.29	54
DAE Designation:			
Proportion DAE Extension Worker	0.76	0.43	58
Proportion DAE Senior Officers	0.19	0.40	58
Proportion non-DAE Trainers	0.05	0.22	58

Notes: There were 58 trainers who led the primary LCC training session, 3 of whom were CDIP Trainers while the rest were from DAE. The majority of DAE trainers held the designation Sub-Assistant Agriculture Office (formerly known as block supervisors) and are classified as DAE Extension Workers. All other DAE trainers are grouped together into DAE Senior Officers.

76% are Sub-Assistant officers (formerly called block supervisors) who we typically think of as extension workers who work directly with farmers and oversee extension activities in their designated villages. The remaining DAE trainers have various designations, some of whom are officers from the sub-district level offices. They are pooled together as Senior Officers and consist of 19% of the trainers. 5% are trainers from CDIP. Trainers were also asked about their previous educational performances including rank or GPA in their highest degree and well as rank or GPA for standardized national-level exams from Grade 10 and Grade 12. However, response rates for many of these are very poor, which may be a problem of recall; therefore, these variables were excluded from the analysis.

2.4 Empirical Strategy

To understand the important for trainers in this intervention, I estimate individual trainer fixed effects. First, I estimate Equation 2.1 for treatment group farmers to estimate the magnitudes of the individual trainer fixed effects for several outcomes, including measures for take-up of LCCs, as well as change in urea and yields. The sample is restricted to treatment group farmers since the intervention did not provide the control groups farmers access to the trainers.

$$y_h = \alpha_0 + \mu_i + \alpha_1 X_h + \epsilon_h \tag{2.1}$$

y_h is stated use of LCC, ability to show LCC at endline, change in mean urea use or change in mean yields for treated households. X_h includes controls for household characteristics including age and years of education completed by the farmer interviewed (primary farmer in household), total plot area cultivated by household, non-agricultural household income. μ_i controls for trainer fixed effects.

Next, I compute the predicted trainer fixed effects for each of the outcomes above for the 58 trainers, collapse the dataset to the trainer level and then estimate Equation 2.2 to explore whether any of the observable trainer characteristics are correlated to the predicted trainer fixed effects.

$$\begin{aligned} TF\_y\_hat\_i &= \beta_0 + \beta_1 Age_i + \beta_2 Female_i + \beta_3 Experience_i \\ &\quad + \beta_4 ExtensionWorker_i + \beta_5 Graduates_i + \epsilon_i \end{aligned} \tag{2.2}$$

$TF\_y\_hat\_i$ is the predicted trainer fixed effect for outcome y for trainer i. Age_i, $Female_i$, $Experience_i$ are respectively the age, female dummy and years of experience of the trainer. $ExtensionWorker_i$ is a dummy variable if the trainer is a village-level extension worker and 0 otherwise (others are mostly more senior trainers from the DAE). $Graduates_i$ is 1 if the trainer has a bachelors or masters degree and is 0 otherwise.

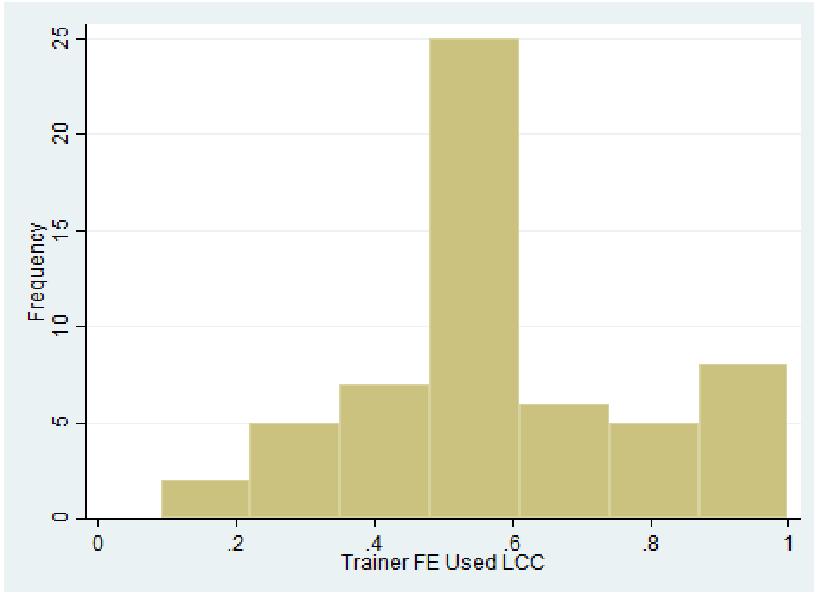

Figure 2.1: *Histogram of Trainer Fixed Effects for Take-up (Used LCC)*

2.5 Results

2.5.1 Trainer Fixed Effects

Figures 2.1 and 2.2 show histograms of predicted values of trainer fixed effects for two measures of take-up of LCCs by farmers. I compute the predicted values for each measure after estimating Equation 2.1. Among the measures of take-up discussed previously in Chapter 1, for this analysis, I use stated use of LCCs by the primary farmer and whether the farmer showed an LCC to the enumerator at the endline survey. The decision to use the LCC and to retain the LCC may depend on the effectiveness of the training. Other measures on whether farmers received the LCC or decided to attend training are more likely to depend on CDIP staff, rather than primary trainers, as CDIP managed the logistics of the intervention, invited treatment group farmers to the training and provided the LCC to farmers who failed to attend the training through home visits.

The histograms show that there is observable variation in predicted trainer fixed effects, although the level of variation differs by outcome. Figure 2.1, shows that over 40% of the

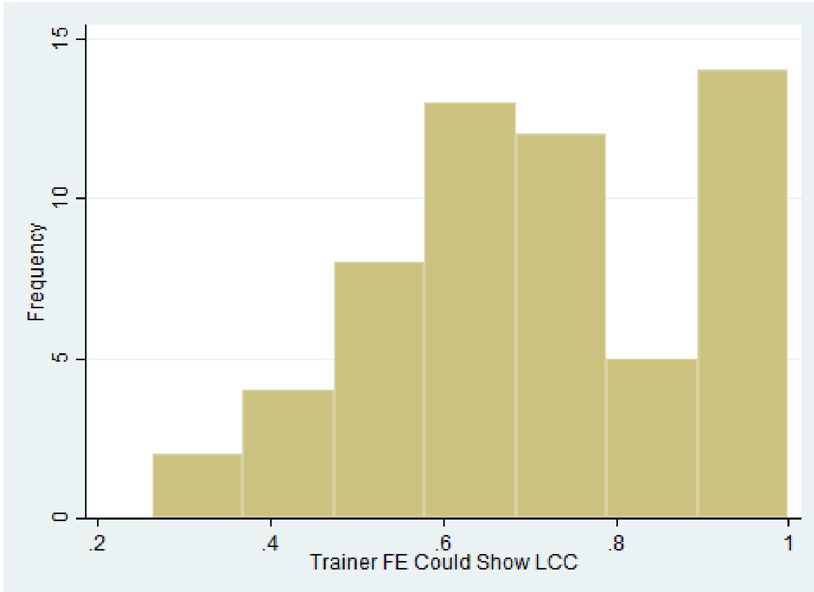

Figure 2.2: *Histogram of Trainer Fixed Effects for Take-up (Could Show LCC)*

trainers are close to the mean rates of stated take-up. Some trainers are predicted to be close to attaining full rates of stated adoption, while a few trainers have farmers who rarely state using the LCC. There is variation in predicted trainer fixed effects for take-up as measured by farmers being able to show the LCC to the enumerator during the endline survey (Figure 2.2), but the predicted values are less concentrated around the mean.

Figures 2.3 and 2.4 show similar histograms of trainer fixed effects for mean change in urea and mean change in yield. There is some observed variation in trainer fixed effects for both these outcomes as well, although there is more variation for change in urea than change in yield.

These results suggest that there are differences in trainer quality, however, overall individual trainers are unlikely to be a large determinant of adoption of LCCs in this intervention as for most of the regressions the coefficients of trainer dummy variables were not jointly significant. Since LCCs are simple, and standardized instructions were available to farmers it is likely that trainers did not play as important a role for the intervention as they may with more complex technologies. Moreover, a refresher training was conducted

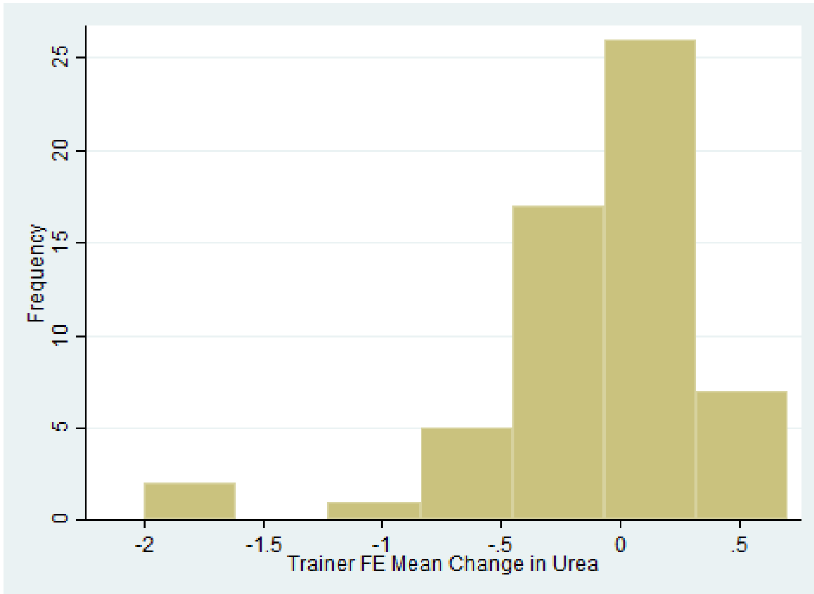

Figure 2.3: *Histogram of Trainer Fixed Effects for Change in Urea Use*

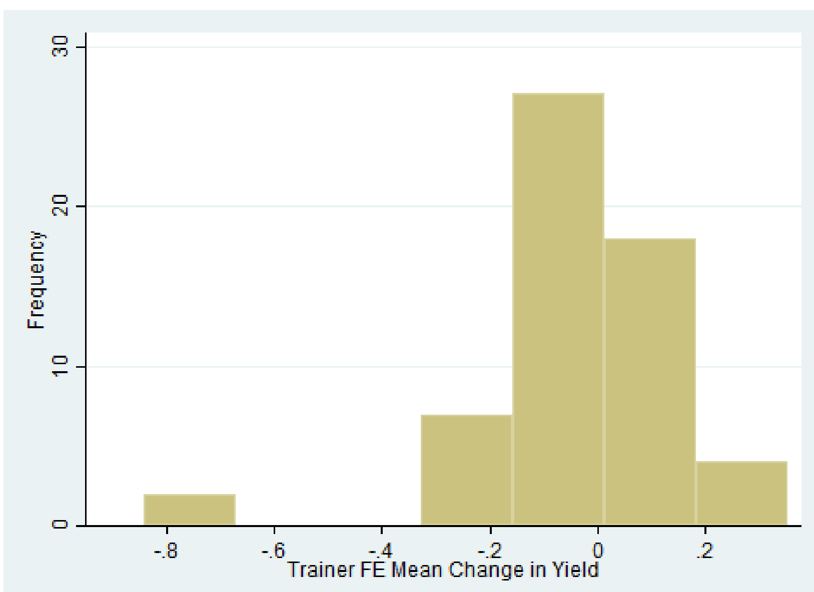

Figure 2.4: *Histogram of Trainer Fixed Effects for Change in Yields*

informally a few weeks after the main training to ensure farmers receive simple and standardized instructions and to answer questions. The refresher training may have further reduced the potential importance of the primary trainers.

2.5.2 Trainer Characteristics

Since there is variation in the predicted trainer fixed effects, I further investigate whether observable trainer characteristics are correlated with the trainer fixed effects, which acts as proxies for trainer quality.

Table 2.2 shows estimates of Equation 2.2 for the two measures of take-up. Controlling for the remaining variables, there is no significant correlation between trainer quality (as determined by the measures of take-up) and trainer age, gender or experience. There is no significant correlation between being an extension worker and the predicted trainer fixed effects for take-up as measured by stated use of LCCs by farmers. However, the results indicate that after controlling for age, gender, experience, and highest degree, farmers trained by extension workers were less likely to be able to show the LCC at endline. Trainers who are university graduates were significantly more likely to perform more poorly compared to non-graduates after controlling for age, gender, experience and designation.[8]

Table 2.3 shows estimates of Equation 2.2 for the two main farmer outcomes; mean change in urea and mean change in yields for treatment farmers. To construct these variables, I first estimate the mean urea used and mean yields obtained for each household at baseline and endline and then compute the difference. There is no significant correlation between trainer quality (as determined by changes in urea and yield) and trainer age, gender or being a university graduate, holding the other variables constant. The coefficient for university graduates for trainer fixed effects based on change in urea use is positive and change in yields is negative (although they are not statistically significant), but the signs are consistent with the results above (lower take-up rates by farmers trained by graduates), as successful

[8] Table B.1 in the Appendix, shows estimates of a regression of University Graduate on the the other trainer characteristics to provide further insight on the characteristics of graduates and the correlations between being a graduate and the other observable characteristics.

Table 2.2

Regressions of Predicted Trainer FE on Trainer Characteristics (Take-up of LCCs)

	(1) Trainer FE Used LCC	(2) Trainer FE Could Show LCC
Extension Worker	0.013	-0.127**
	(0.054)	(0.055)
University Graduates	-0.150**	-0.308***
	(0.059)	(0.060)
Trainer Age (years)	0.001	0.001
	(0.004)	(0.004)
Female Trainer	-0.063	-0.087
	(0.076)	(0.062)
Experience (years)	-0.004	-0.003
	(0.004)	(0.003)
Observations	58	58

Notes: The variable Extension Workers takes a value of 1 if the trainer is a DAE extension worker and 0 otherwise. The majority of trainers held the designation Sub-Assistant Agriculture Office (formerly known as block supervisors) and are classified as DAE Extension Workers. The variable University Graduates is a dummy variable that is 1 if the trainer has a bachelors or masters degree and is 0 otherwise. Robust standard errors are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

adoption by farmers is likely to reduce urea use. The coefficient for experience in column (1) is significant at the 10% level, but the magnitude is small. There is also a positive correlation between being an extension worker and predicted trainer fixed effects for change in urea, and the coefficient is large at 0.155 but only significant at the 10%.

As an alternate specification, I also estimate trainer fixed effects for mean urea and mean yield for households at the endline, instead of changes in urea and yield for the household, and then regress the predicted trainer fixed effects for each on trainer characteristics. Table 2.4 shows the estimates. The results are overall similar to the results above, showing some evidence that extension workers and university graduates trained farmers who had higher urea use at endline. The main difference between the two sets of results is the large positive correlation between female trainers and trainer fixed effects for endline yields. Since baseline and endline yields for the same household are likely to be strongly correlated, the difference between the two results suggest that female trainers may have been assigned to villages with higher baseline yield.

Table 2.3

Regressions of Predicted Trainer FE on Trainer Characteristics (Change in Urea Use and Yield)

	(1) Trainer FE Mean Change in Urea	(2) Trainer FE Mean Change in Yield
Extension Worker	0.155*	0.023
	(0.083)	(0.044)
University Graduates	0.200	-0.121
	(0.121)	(0.081)
Trainer Age	0.007	0.001
	(0.011)	(0.003)
Female Trainer	0.187	0.029
	(0.253)	(0.084)
Experience (years)	0.016*	-0.004
	(0.009)	(0.004)
Observations	58	58

Notes: The variable Extension Workers takes a value of 1 if the trainer is a DAE extension worker and 0 otherwise. The majority of trainers held the designation Sub-Assistant Agriculture Office (formerly known as block supervisors) and are classified as DAE Extension Workers. The variable University Graduates is a dummy variable that is 1 if the trainer has a bachelors or masters degree and is 0 otherwise. Robust standard errors are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 2.4

Regressions of Predicted Trainer FE on Trainer Characteristics (Urea Use and Yield at Endline)

	(1) Trainer FE Mean Endline Urea	(2) Trainer FE Mean Endline Yield
Extension Worker	0.122*	1.663
	(0.071)	(1.260)
University Graduates	0.142*	-0.125
	(0.078)	(1.701)
Trainer Age	-0.003	-0.010
	(0.004)	(0.057)
Female Trainer	-0.021	4.146**
	(0.079)	(1.691)
Experience (years)	0.003	0.029
	(0.004)	(0.057)
Observations	58	58

Notes: The variable Extension Workers takes a value of 1 if the trainer is a DAE extension worker and 0 otherwise. The majority of trainers held the designation Sub-Assistant Agriculture Office (formerly known as block supervisors) and are classified as DAE Extension Workers. The variable University Graduates is a dummy variable that is 1 if the trainer has a bachelors or masters degree and is 0 otherwise. Robust standard errors are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Overall, the results do not find any strong patterns between observed trainer characteristics and trainer quality across all the measures. However, there is weak evidence that adoption of and benefits from LCCs may be lower for the farmers who received training from university graduates and extension workers. A negative correlation between trainer quality and being an extension worker is consistent with observations on the literature on low levels of technical training and poor access to re-training facilities for village extension workers. While the negative correlation between trainer performance and higher educational degrees is puzzling, it is possible that highly educated trainers are be too far removed from the target farmers, and less effective as a result. It is also possible that highly educated trainers have lower incentives to train farmers well, if they have more responsibilities besides training due to their qualifications.

2.6 Conclusion

This paper utilizes a field experiment with rice farmers in Bangladesh, where treatment group farmers received a new tool called a leaf color chart (LCC), to explore whether trainer quality and observable characteristics of trainers are important in the adoption of a new technology. I use data on measures of take-up of the LCC by farmers as well as changes in urea and yield, to create proxies for performance of extension staff who conducted the primary training for the intervention. I examine variation in trainer quality and explore whether the measured quality is related to observable characteristics of the trainer. I find variation in trainer quality, as indicated by variation in predicted values of trainer fixed effects. However, there is no significant correlation between trainer quality and observable characteristics of trainers such as age, gender and experience. There is evidence of a lower adoption rates by farmers who were trained by university graduates compared to those who were trained by workers with diplomas in agriculture. Due to the limitations of the data, I am unable to distinguish between different mechanisms, however, trainers with higher levels of education may be relatively more removed from the target farmers and they may also have lower incentives to train well. There is also weak evidence of lower adoption rates for farmers trained by extension workers compared to extension personnel with higher designations. The negative correlation observed in this chapter is consistent with observations in the literature of poorer levels of technical knowledge of village extension workers. Since assignment of trainers was non-random, unobserved differences between villages that received extension workers as trainers and those that received higher ranked personnel may also drive the results.

Chapter 3

How does Child Labor respond to changes in Adult Work Opportunities? Evidence from NREGA[1]

3.1 Introduction

Workfare programs in many developing countries aim to reduce poverty by functioning as conditional cash transfers. Typically such programs do not directly target children, but have the potential to improve outcomes for children by increasing household income and financial security. However, these programs can also have perverse effects on children by changing the rural economy and time allocation of household members. This paper studies the impact of a large workfare program in India on schooling and employment outcomes for children.

The Mahatma Gandhi National Rural Employment Guarantee Act (NREGA), passed in 2005 in India, has created one of the largest public works programs in the world. NREGA offers 100 days of guaranteed work to rural households with the intention of helping households smooth consumption during lean agricultural seasons. NREGA targets the

[1]Co-authored with Anitha Sivasankaran

household, rather than individual members and NREGA work can only be taken up by adults. While NREGA increases household income and can increase education for children, it also increases wages in the rural economy, thus changing the opportunity cost of schooling for children. Moreover, it can cause other changes both in the rural economy and within the household by changing time allocation decisions of adults and bargaining power of women. Therefore, the impact of NREGA on both children's schooling and labor market decisions is an empirical question.

We use several rounds of nationally representative cross-sectional data from the National Sample Survey (NSS) in India. We exploit the phased roll-out of NREGA to different districts and measure the difference-in-difference between districts that received the program early relative to those that received it later. We find that time spent in public works increases for both adult men and women, which is consistent with findings from other papers (for example, (Imbert and Papp, 2013). Moreover, wages for casual work (non-NREGA casual labor) increase for adult men and women. For children, we show that when NREGA work is introduced to a district, younger children (ages 6 to 9) experience a 3 percent increase in time spent on education and older children (ages 15 to 17) experience an 18 percent increase in time spent working outside the household.

However, with the cross-sectional NSS data we cannot tell whether the impact of NREGA we measure is for adults and children from the same or from different households. Therefore, as a robustness check, we use panel data from three states also collected by the NSS. We look at how time use for children changes during weeks when adults take-up NREGA work. The results from the panel data are consistent with the results from the cross-sectional data, and suggest that the impact of NREGA for adults and children that we observe are likely to be from individuals within the same household. When adult time in public works in a given week increases, time spent by younger children in education increases and time spent by older children working outside the household increases.

The main results support a model where the income effect of NREGA is stronger for younger children for whom the wage change due to NREGA is unlikely to matter. The substitution effect due to the wage increase is stronger for older children and increases the opportunity cost of schooling. However, a simple back of the envelop calculation suggests that the wage elasticity of labor supply for older children is 4.4, which is implausibly high compared to estimates from other settings and suggests other channels for the increase in labor supply by older children.

It may be the case that new jobs which were previously not available for children due to job rationing open up when some of adult labor is used for NREGA. This is consistent with the results from the panel data that show time spent by older children doing outside work increases in weeks that parents work in NREGA. Another mechanism that could explain the magnitude of the wage elasticity could be that adults spend less time working in household enterprises when NREGA jobs open (which we observe in the data), and there may be strong complementarities between adult and child work in household enterprises leading to older children spending more time working outside the household rather than in the household. This is also consistent with the panel data results which show a positive correlation between adult and child time in household enterprise work, and a decrease in time spent by older children in household enterprise work in weeks that adults work in NREGA.

This paper adds to the growing body of literature evaluating the impact of NREGA (Ravi and Engler, 2009; Sharma, 2009; Azam, 2011; Afridi, Mukhopadhyay and Sahoo, 2012; Zimmermann, 2012; Imbert and Papp, 2013) etc. However, we focus on the effects on children, who are non-participants in the program. The closest work to ours is by Afridi, Mukhopadhyay and Sahoo (2012) which finds that greater participation of mothers in NREGA is associated with better educational outcomes for their children by empowering mothers through better labor opportunities for women. However, this is the first paper that studies NREGA's differential effects by age group on children.

This paper also contributes to the literature on promoting education for children and reducing child labor which have been key policy issues in developing countries. Research on conditional cash transfers (CCTs) have shown that CCTs can reduce outside work for children (Schultz, 2004) and domestic work for girls (Skoufias et al., 2001). Studies on unconditional cash transfers have also established that such transfers can delay entry into paid employment for children (Edmonds and Schady, 2009) and have a positive, although smaller, impact on schooling (Baird, McIntosh and Ãzler, 2011). However, when an income increase for the household is not due to a pure transfer, but rather some other economic shock, changes to child labor often depend on changes in adults' activities due to the shock as well as any changes in the local economy.

Finally, this paper contributes to the literature on targeting. Many policies and programs targeting children have focused on women. We find that a workfare program that targets the household rather than specific individuals can have positive effects on children. However, the different effects on older and younger children suggest that careful consideration should be given to potential spillovers when designing programs.

The rest of this paper is structured as follows. Section 3.2 describes the background and details of NREGA. In Section 3.3, we provide a simple conceptual framework to explain the differential effects on education and child labor by age group. Section 3.4 describes the data and the estimation strategy. Sections 3.5 and 3.6 present the main results and robustness checks. We discuss alternative mechanisms in Section 3.7 and conclude with a policy discussion in Section 3.8.

3.2 Background on NREGA

The Mahatma Gandhi National Rural Employment Guarantee Act (NREGA) was enacted in 2005 and it guarantees 100 days of wage employment work per financial year to every rural household in India. Although the law was passed in 2005, the act was not made applicable to all districts at the same time. It was first phased into 200 districts in February 2006. An additional 130 districts were included in April 2007 for the second phase and the remaining 284 districts were included in April 2008[2]. Within each state, the earlier districts were chosen

[2]Information retrieved from NREGA website. Phase in dates and list of districts compiled from http://nrega.nic.in/MNREGA_Dist.pdf and http://nrega.nic.in/circular/Report_to_the_people.pdf respectively.

because they were identified as backwards and least developed (MoRD, 2010).

Once the program is available in a district, each rural household is entitled to 100 days of guaranteed wage employment in a financial year, if adult members in the household are willing to do unskilled manual work under the program. To enroll in the program, a household registers with the Gram Panchayat (village-level self governing body) and is issued a Job Card. Job Card holders can then apply for work to the Gram Panchayat and are entitled to receive work within 15 days of the application. If they do not receive work within that time, households are supposed to receive unemployment insurance, although this aspect of the program is not well implemented. Although the program targets households rather than individuals, it promotes participation of women in wage employment. According to the Act, at least one-third of workers hired under the program must be women (NREGA, 2005).

Since poverty alleviation is the main focus of the NREGA, it is often compared to a cash transfer program (Imbert and Papp, 2013; Kapur, Mukhopadhyay and Subramanian, 2008). Moreover, workers are paid wages at the state-wise specified wage rates for the program, which are usually higher than prevailing agricultural wages. Several papers document an increase in private sector wages for men and women (Imbert and Papp, 2013; Berg et al., 2012) and only for women (Zimmermann, 2012) as a result of the program. Thus the program can be considered to have two effects on the rural economy - it increases income and the wage rate for households.

3.3 Conceptual Framework

In this section we provide a simple conceptual framework to understand the impact of NREGA on a rural household, specifically for child labor. Following Basu, Das and Dutta (2010), we model a unitary household with one adult and one child[3]. We use a unitary model

[3] Intra-household dynamics may change since NREGA provides women with a chance to work which could increase their bargaining power. However, for the purpose of this basic model, we simplify and do not use a collective household model.

of the household since NREGA is targeted at the household rather than at an individual, but we make a distinction between adults and children because children cannot work in NREGA jobs.

The household derives utility from consumption. We assume adult labor is costless. However, child labor is costly and the opportunity cost is the time spent in school. Utility is given by the following quasi-linear utility function:

$$U(c,l) = \phi(c) - \alpha l$$

where c is household consumption, l is the time spent by children working, $\phi'(c) \geq 0$, $\phi''(c) \leq 0$ and α is a positive real number.

This utility function satisfies the *Luxury Axiom*, which is defined as Òa family will send the children to the labor market only if the familys income from non-child-labor sources drops very low". Adults supply a fixed time to the labor market T. We assume the price of the consumption good is 1 and wages for adults and children are w and w^C respectively. The budget constraint is given by

$$c \leq w^C l + wT$$

The household problem is given by:

$$\max_{l} \left\{ \phi(w^C l + wT) - \alpha l \right\}$$

We assume a perfect labor market with one sector (agriculture). Children work in this sector, however, they are less productive and their productivity is a function of their age, a. One unit of child labor is $p(a)$ units of adult labor, where $0 \leq p(a) \leq 1$ and p is an increasing in a. Older children are more productive than younger children and are therefore more substitutable for adult labor. Wages w and w^C are such that $w^C = p(a)w$.

The household problem can be now expressed as

$$\max_{l} \{\phi(p(a)wl + wT) - \alpha l\}$$

This gives us the first order condition

$$p(a)w\phi'(p(a)wl + wT) = \alpha$$

Differentiating implicitly with respect to w and rearranging the terms, we get

$$\frac{dl}{dw} = -\frac{\phi' + w(T + p(a)l)\phi''}{p(a)w^2\phi''} \tag{3.1}$$

Labor supply for children increases with w when the following condition holds:

$$p(a) > -\frac{\phi'}{wl\phi''} - \frac{T}{l} \tag{3.2}$$

Since $p'(a) \geq 0$, this conditional is more likely to hold when age, a, increases. Thus, older children are more likely to respond to an increase in wages by increases their labor supply than younger children.

When NREGA work is introduced into the rural economy, another sector (public sector) opens, but only adults can work in this sector. NREGA wage is set at $\bar{w}$ where $\bar{w}$ is greater than the pre-NREGA wage in the economy. Moreover, days of NREGA work are capped at 100 days. Since public sector wages are higher than agriculture sector wages, adults will shift to the public sector. But they will only work a maximum of 100 days there and spend any additional time working in agriculture. This shifts the labor supply curve in the agriculture sector to the left, as adults spend less time in the sector. This shift in labor supply increases wages in the agricultural sector.

Higher wages and household income from NREGA have two effects on children. While higher household income reduces child labor supply and increases schooling through an income effect, children also respond to higher wages and spend more time working through the substitution effect. Equation 3.2 implies that the substitution effect is more likely to be true for older children since the increase in wages is larger for older children. For younger children the income effect is more likely to dominate. We will test this empirically in the following sections.

3.4 Data & Estimation Strategy

We use four rounds of nationally representative cross-sectional employment data collected by the National Sample Survey Office (NSSO) starting in 2004 and until 2008. The NSSO Employment and Unemployment survey is conducted from July to June in order to capture one full agriculture cycle and is stratified by urban and rural areas of each district. Since the NREGA is only applicable for individuals living in rural areas, we drop the urban population in our analysis. We include all districts from all states in India, excluding Jammu and Kashmir since survey data is missing for some quarters due to conflicts in this area. The NSSO over-samples some types of households and therefore all estimates are computed adjusted using the sampling weights provided by the NSSO.

Our data spans January 2004 to January 2006 to form the pre-program period and July 2007 to June 2008 for the post-program period. To define the pre-program and post-program periods, we obtained data on the NREGA phase-in by district from the NREGA website. We use the individual as our primary unit of analysis. Table 3.1 provides summary statistics for the pre-program period from the 60th round of the NSS data.

Our main outcomes are individual-level measures of time spent on various activities in the last seven days for adults as well as children. The NSSO Employment and Unemployment surveys collect data at the individual level on activities undertaken in the last seven days at the time of the survey by each household member over the age of four. For each day and each activity, the survey records whether the activity was performed at an intensity of 0, 0.5 or 1 day. Using this data we construct variables on number of days spent by each household member in the past week on public works, non-public outside work, work on household enterprise, domestic activities and all other activities. For children, we

Table 3.1

Summary Statistics: NSS 60th Round

Average Number of Children by Age Group:	
Age 6 to 17	1.365
	(1.391)
Age 6 to 9	0.509
	(0.747)
Age 10 to 14	0.582
	(0.827)
Age 15 to 17	0.275
	(0.525)
Individual & Household Characteristics:	
Age	25.797
	(19.333)
Fraction literate	0.530
	(0.499)
Fraction married	0.461
	(0.498)
Fraction widowed	0.046
	(0.209)
Fraction divorced	0.002
	(0.044)
Fraction in scheduled caste tribe	0.742
	(0.437)
Fraction Christian	0.019
	(0.137)
Fraction Muslim	0.105
	(0.307)
Household size	6.201
	(2.963)

separate out number of days spent on educational activities and we only have one category of outside work since children cannot work in public works. The activities are mutually exclusive and the total adds up to 7 days for each individual.

The survey also asks total earnings in the past seven days for individuals who worked in casual labor. Our wage measures use this data to compute average earnings per day worked in non-public casual labor.

Our empirical strategy follows Imbert and Papp (2013) and uses the phased roll-out of the NREGA to different districts and compares changes in districts that received the program earlier to districts that received the program later. The program was introduced to 200 districts in February 2006 as part of the first phase, to 130 districts in April 2007 as part of the second phase and to all remaining districts in April 2008. We compare individuals from districts in the first two phases to individuals from districts that received the program in the final phase.

However, a simple comparison of individuals from districts that received the program in different phases is biased by the fact that districts in the earlier phases are more backward than those in later phases on socio-economic characteristics such as agricultural wages and output which directly affect labor market outcomes. To address this concern we compare changes over time in districts that received the program earlier to those that received it later and include district fixed effects.

We use the following difference-in-difference specification comparing Phase I & II districts to Phase III districts before and after NREGA is rolled out for Phase I & II districts:

$$y_{idt} = \beta_0 + \beta_1 nreg_{dt} + \gamma X_{idt} + \mu_d + \eta_t + \epsilon_{idt} \tag{3.3}$$

where y_{idt} is days spent in education, labor, domestic activities, etc for an individual i in district d at time t. The variable $nreg_{dt}$ is 1 if at the date of the survey, NREGA was available in district d and is 0 otherwise. X_{idt} is a set of individual and household level variables including age, age squared, literacy, religion, social group, and household size. We also include district fixed effects (μ_d) and quarter-year fixed effects (η_t). We re-weight observations using sampling weights and cluster standard errors at the district level. The coefficient β_1 gives the effect of NREGA on days spent in each activity by individual i.

3.5 Results

3.5.1 Changes to the Rural Economy

Table 3.2 shows the changes in time spent in various activities by adult men and women. Once NREGA is rolled into a district, casual public work by men increases by 0.055 days in the last seven days. For women, time spent in casual public work increases by 0.032 days in the last seven days. Both these coefficients are significant at the 1 percent level. Mean days

Table 3.2

Number of Days Spent by Adults on Different Activities in the last 7 days

Includes District Fixed Effects and Year*Quarter Fixed Effects

	(1) Casual Public	(2) Non-Public Outside	(3) HH Enterprise	(4) Domestic	(5) Other
Panel A: Men					
NREG	0.055***	0.087	-0.190***	-0.011	0.059
	(0.012)	(0.060)	(0.063)	(0.014)	(0.037)
Observations	315,371	315,371	315,371	315,371	315,371
Non-NREG mean	0.021	2.283	3.474	0.106	1.116
Panel B: Women					
NREG	0.032***	-0.060*	0.035	-0.051	0.045
	(0.009)	(0.035)	(0.054)	(0.062)	(0.027)
Observations	314,630	314,630	314,630	314,630	314,630
Non-NREG mean	0.010	0.830	1.479	4.136	0.545

Notes: Includes controls for age, age^2, literacy, marital status, household size, religion and social group. Standard errors adjusted for clustering at 570 districts in parentheses. *** p<0.01, ** p<0.05, * p<0.1

spent on casual public work before NREGA is 0.021 days by men and 0.010 days by women, so this a very large increase; time spent in casual public work approximately doubles for men and triples for women.

For men, the increased days spent in casual public work mostly comes from a reduction in time spent working in household enterprises. The number of days spent working in household enterprises by men decreases by 0.190 days in the last seven days, and the coefficient is significant at the 1 percent level. For women, the increased time in casual public work comes from a reduction in time spent on non-public outside work (decrease of 0.060 days in the last seven days, significant at the 10 percent level), and also from a reduction in time spent in domestic work (decrease of 0.051 days in the last 7 days), although this coefficient is not significant at the 10 percent level.

While the percentage increase in days spent in public works is large, the magnitude of

Table 3.3

Log of Daily Casual Wages (Non-Public)

Includes District & Year*Quarter Fixed Effects

	Adults: 18 to 60		
	All	Women	Men
	(1)	(2)	(3)
NREG	0.041***	0.053**	0.035**
	(0.016)	(0.024)	(0.015)
Observations	79,199	22,041	57,158
Non-NREG mean	55.43	39.70	62.20

Notes: Includes controls for age, age^2 household size, literacy, marital status, religion, social group. Standard errors, adjusted for clustering, in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

the change in terms of days spent in a year is small, approximately 2.9 days per year for men and 1.7 days for women. However, this averages over all rural households regardless of participation. Estimate of average days worked by participating households is much higher and according to the official website, in 2010-11 the NREGA provided 2.27 billions person-days of employment to 53 million households (Imbert and Papp, 2013).

Table 3.3 shows changes in log of daily casual wages (from non-NREGA work), once the program comes into the district. Overall, wages increase by 4.1 percent, and the coefficient is significant at the 1 percent level. Disaggregating by gender, we see that wages for women increase by 5.3 percent and wages for men increase by 3.5 percent. Both coefficients are significant at the 5 percent level.

Thus household income increases from both wages earned from NREGA work and from higher wages from non-NREGA work. Moreover, as Appendix Table C.1 shows, changes in total days worked by the household also increases further increasing household income.

3.5.2 Effect on Time Use by Children

The increase in family income can change time spent by children in schooling. Panel A of Table 3.4 shows the effect on time allocation towards education when NREGA is rolled in

Table 3.4

Number of Days Spent by on Education in the last 7 days

All Children: Age 6 to 17 years (Never married)

Includes District Fixed Effects and Year*Quarter Fixed Effects

	(1) All Children	(2) Age 6 to 9	(3) Age 10 to 14	(4) Age 15 to 17
Panel A: All Children				
NREG	0.029	0.184***	-0.015	-0.194**
	(0.049)	(0.069)	(0.059)	(0.099)
Observations	294,484	100,422	127,366	66,696
Non-NREG mean	5.384	5.875	5.741	3.748
Panel B: Boys				
NREG	0.028	0.200**	-0.051	-0.221*
	(0.056)	(0.080)	(0.067)	(0.129)
Observations	294,484	100,422	127,366	66,696
Non-NREG mean	5.601	6.014	6.017	4.063
Panel C: Girls				
NREG	0.040	0.164*	0.031	-0.190
	(0.059)	(0.084)	(0.078)	(0.127)
Observations	137,101	47,698	59,421	29,982
Non-NREG mean	5.132	5.721	5.418	3.341

Notes: Includes controls for age, age^2, literacy, household size, religion and caste. Standard errors adjusted for clustering at 570 districts in parentheses. *** p<0.01, ** p<0.05, * p<0.1

to a district. Column 1, pools children of all age groups together, and we see that there is no significant impact on time spent in education. However, when we disaggregate by age group, we see strong effects in opposite directions for the youngest and oldest age groups. Children between ages 6 to 9 years, spend 0.184 days more in the past week (significant at the 1 percent level), and children aged 15 to 17 spend 0.194 days less, in the past week, in schooling (significant at the 5 percent level). The coefficient is not significant at the 10 percent level for children in the middle age group of 10 to 14 years. Panels B and C of Table 4, shows the results by gender. The results for boys are stronger, although the coefficients for girls are similar in magnitude and direction but less precise.

Table 3.5 shows the effects on labor market and activities other than education by

Table 3.5

Number of Days Spent by Children on Different Activities in the last 7 days

All Children: Age 6 to 17 years (Never married)

Includes District Fixed Effects and Year*Quarter Fixed Effects

	(1) Outside Work	(2) HH Enterprise	(3) Domestic Work	(4) Other
Panel A: Age 15 to 17				
NREG	0.131**	-0.090	0.088	0.065
	(0.061)	(0.064)	(0.074)	(0.052)
Observations	66,696	66,696	66,696	66,696
Non-NREG mean	0.728	0.887	1.083	0.554
Panel B: Age 10 to 14				
NREG	0.007	0.025	0.044	-0.062
	(0.016)	(0.034)	(0.038)	(0.039)
Observations	127,366	127,366	127,366	127,366
Non-NREG mean	0.124	0.196	0.432	0.507
Panel C: Age 6 to 9				
NREG	-0.005*	0.002	0.016	-0.197***
	(0.003)	(0.010)	(0.020)	(0.065)
Observations	100,422	100,422	100,422	100,422
Non-NREG mean	0.004	0.014	0.056	1.052

Notes: Includes controls for age, age^2, literacy, household size, religion and caste. Standard errors adjusted for clustering at 570 districts in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

children, when NREGA is introduced to a district.[4] Panel A shows the effects on children aged 15-17 years. When NREGA in introduced to a district, children in this age group spend 0.131 days more working outside the household in the last seven days. This coefficient is significant at the 5 percent level. This represents an 18 percent increase in time spent working outside the household for children in this age group. The coefficients for the remaining activities are not precise. Overall, the results show that 15-17 year old children spend more time working for a wage, at the expense of time spent in education.

For children in the youngest age group (ages 6-9 years, shown in Panel C), time spent in "other" activities decreases once NREGA comes in. In the past seven days, the youngest

[4]Note that the time spent on the different activities including education adds up to seven days for each child.

children spend 0.197 days less in other activities. This category is coded as anything other than time spent in domestic work, household enterprise work, outside work or education and we interpret it as leisure. The coefficient for outside work for the youngest children is also negative, but as very few children in this age group work, this coefficient should be interpreted cautiously. As before, the coefficients for children in the middle age group are not precise.

Overall, the results in Tables 3.4 and 3.5 show that when NREGA is introduced to a district time spent in education increases for the youngest children and time spent working for a wage outside the household increases for the oldest children. Tables C.2 and C.3 in the Appendix, show results separated by gender. Consistent with previous results for education, we find that the changes in time spent by boys and girls are similar, but that the coefficients for girls are less precise.

3.6 Robustness Checks: Further Evidence from Panel Data

The data used for the previous results are several rounds of cross-sectional data from the NSSO which does not allow us to observe the same household over time. We thus cannot differentiate whether the effects we observe are for adults and children from the same or from different households. In this section we provide evidence from panel data for three states collected by the NSSO.

3.6.1 Data & Estimation Strategy

The NSSO conducted a panel survey with a focus on NREGA spanning the years from 2009 to 2011. At this time, the NREGA had been introduced in all districts. The sample consisted of 912 villages in Andhra Pradesh, Madhya Pradesh and Rajastan. The survey included four rounds of the Employment and Unemployment surveys in the same format at the cross-sectional surveys. Each household was visited four times over two years between July 2009 to June 2011. Table 3.6 provides summary statistics on household composition for children in different age groups in the panel data.

Table 3.6

Summary Statistics: Panel Data Household Composition

Household Composition in Visit 1	
Percentage of Households with Children age 6 to 17	57.8
Percentage of Households with Children age 6 to 9	32.2
Percentage of Households with Children age 10 to 14	35.9
Percentage of Households with Children age 15 to 17	22.7
Percentage of Households with Children in both groups:	
Age 6 to 9 & Age 10 to 14	18.1
Age 10 to 14 & Age 15 to 17	13.7
Age 6 to 9 & Age 15 to 17	6.4
Percentage of Households with Children in all three groups	5.1

Since the panel data was collected after NREGA was available in all districts, we do not have variation in NREGA work availability within the sample. We instead use the panel data to look at the response of time allocation by children within the household when adults take up NREGA work. This allows us look at whether the response by children that we observe in the cross sectional data is likely to be children from the same households or from different households as the ones where adults work in NREGA. We use the following specification with household fixed effects:

$$y_{ht} = \beta_0 + \beta_1 CasualPublicDaysAdults_{ht} + \beta_2 X_{ht} + \gamma_h + \delta_t + \mu_{ht} \quad (3.4)$$

where y_{ht} is the household aggregate of time spent on each activity by children from household h at time t and $CasualPublicDaysAdults_{ht}$ is the total number of days spent by the adults in the household on casual public work in the last seven days. β_1 is the change in time allocation by children in the household when time spent working in NREGA jobs by adults in the household changes.

3.6.2 Results: Changes within Household

Table 3.7 estimates the changes in time allocation by children within a household in weeks when adults spend more time in casual public work. Panel A presents the results for children between the ages of 15 to 17 and shows that one additional day of work in the past seven days by adults in casual public work results in children working outside by 0.038 days more during that period (significant at the 5 percent level). Additional time working outside is reallocated from less time working in household enterprises (a decrease of 0.027 days in the last week, also significant at the 5 percent level).

For the younger children, in age groups 10-14 years and also 6-9 years, additional time spent by adults in casual public work is related to children spending more time in education (Panels B and C). For each additional day spend by adults in casual public work in the last seven days, 10-14 year olds spend 0.018 days extra in school (significant at the 5 percent level) and 6-9 year olds spend 0.013 days extra in school (significant at the 1 percent level) during that time. For the youngest children, as in the cross-sectional data, the extra time in education mostly comes from time otherwise spent in "other" activities, that we interpret as leisure.

Overall, these results are consistent with the results from the cross-sectional data showing increases in time spent in education for younger children and increases in time spent working for older children. Further, since we observe the same household over time in the panel data, these results suggest that the changes seen in educational and outside work for younger and older children likely come from the same households where parents work in NREGA.

The key difference between the two sets of results is the reallocation in time for the oldest children. For both specifications, we observe an increase in time spent working outside. However for the cross-sectional data, time is mainly reallocated from education while in the panel data, the time is reallocated from working in household enterprises and we do not observe a negative effect on education. Since the first specification estimates changes when NREGA is introduced, it is more likely to capture the general equilibrium effects as the economy is adjusting, while the second specifications looks within households after the initial adjustments have taken place.

Table 3.7

Number of Days Spent by Children on Different Activities in the last 7 days during Adult NREG Work (Panel): Household Aggregates

Includes Household Fixed Effects					
	(1) Education	(2) Domestic Work	(3) HH Enterprise	(4) Working Outside	(5) Other
Panel A: All Children: Age 15 to 17					
# Days in Casual Public Work (adults)	0.004	-0.006	-0.027**	0.037**	-0.003
	(0.008)	(0.010)	(0.012)	(0.015)	(0.008)
Mean of dependent variable	4.40	1.23	1.07	0.82	0.45
Observations	15,722	15,722	15,722	15,722	15,722
Panel B: All Children: Age 10 to 14					
# Days in Casual Public Work (adults)	0.018**	-0.007	0.006	-0.002	-0.011
	(0.007)	(0.005)	(0.005)	(0.003)	(0.007)
Mean of dependent variable	8.37	0.78	0.45	0.16	0.48
Observations	25,176	25,176	25,176	25,176	25,176
Panel C: All Children: Age 6 to 9					
# Days in Casual Public Work (adults)	0.013*	-0.001	-0.001	0.001	-0.010
	(0.008)	(0.003)	(0.002)	(0.001)	(0.008)
Mean of dependent variable	8.31	0.15	0.05	0.01	0.83
Observations	22,589	22,589	22,589	22,589	22,589

Notes: Standard errors clustered at the household level in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

3.7 Alternative Mechanisms

The introduction of NREGA results in two opposing effects on child labor and education that vary by age. Our results from the cross-sectional and panel data suggest that the income effect of NREGA dominates for the youngest children for whom wage is unlikely to increase. Considering their age group of 6 to 9, we can even assume that the entire effect is an income effect since children in this age group are very unlikely to work outside for wages. For older children, there is a strong substitution effect from the increase in wage and the substitution effect dominates the income effect.

The estimates in Section 3.5 show an increase in labor supply of 18 percent for older children from a wage increase of 4.1 percent for adults. This suggests a wage elasticity of child labor supply of 4.4. This is likely to be an underestimate since the wage increase for children is likely to be smaller and the effect on older children is a net effect of the income and substitution effect of NREGA. This magnitude is implausible in this context and given findings in other studies (Grootaert and Kanbur, 1995). This suggests that there may be other channels that are important to consider.

3.7.1 Existence of Surplus Labor

Labor markets in rural India are likely to be imperfect, and may be characterized by the presence of surplus labor. In such a scenario, before the availability of NREGA, older children could have wanted to work outside the household but may have unable to find work as the labor market did not clear. When NREGA is introduced into a district, job opportunities open up for children since adults now spend some of their time doing NREGA work. Therefore, older children do not simply respond to higher wages, but are now able to work more outside due to increased job availability. While we cannot test this directly using the data, the large estimates for wage elasticity of child labor supply suggest that this is a possible channel.

3.7.2 Changes in Household Enterprise work

Changes in time allocation by adults can also directly change time allocation for children if work by children in the household is a substitute or complement for work by adults. Table C.1 in the Appendix and Table 3.2 show that both the household and adult men in particular spend less time working in household enterprises (predominantly agricultural activities in our sample) once NREGA enters a district. A reduction in time spent working in household enterprises by adults can free up time for children if household enterprises if there are strong complementarities to adult and child time in household enterprises. If this is the case, older children may now take up jobs outside the household and younger children may spend more time in school. Moreover, if parents are more flexible as employers compared to outsiders, it may also explain the reduction in time spent in schooling by older children, if they now have to skip school more.

While it is difficult to test this more rigorously with the available date, Table 3.8 explores the relationship between time spent by adults in household enterprises and time allocation of children in the panel data sample. We use an estimation similar to Section 3.6 replacing time spent by adults in public works with time spend by adults in household enterprise work. We see that there are strong complementarities between time spent by adults and time spent by children in household enterprises and the results are the strongest for the oldest children. For each additional day spent by adults in household enterprise work in the last seven days, children in the age group 15 to 17 spending 0.065 days more working in the household enterprise (significant at the 1 percent level). This suggests that some of the increase in outside work by older children may come from a shift away from household enterprise work.

3.8 Conclusion

The NREGA is one of the largest public works programs in a developing country that targets adults in rural households and is aimed at reducing poverty and financial secu-

Table 3.8

Number of Days Spent by Children on Different Activities in the last 7 days during Adult HH Enterprise Work (Panel)

	Includes Household Fixed Effects				
	(1) Education	(2) Domestic Work	(3) HH Enterprise	(4) Working Outside	(5) Other
Panel A: All Children: Age 15 to 17					
# Days in HH Enterprise Work (adults)	-0.003	-0.022***	0.074***	-0.036***	-0.014***
	(0.004)	(0.005)	(0.006)	(0.005)	(0.004)
Mean of dependent variable	4.39	1.23	1.07	0.82	0.45
Observations	15,722	15,722	15,722	15,722	15,722
Panel B: All Children: Age 10 to 14					
# Days in HH Enterprise Work (adults)	0.002	-0.004	0.020***	-0.009***	-0.009**
	(0.004)	(0.003)	(0.003)	(0.002)	(0.004)
Mean of dependent variable	8.37	0.78	0.45	0.16	0.48
Observations	25,176	25,176	25,176	25,176	25,176
Panel C: All Children: Age 6 to 9					
# Days in HH Enterprise Work (adults)	0.002	0.002	0.002*	-0.001	-0.003
	(0.004)	(0.002)	(0.001)	(0.001)	(0.004)
Mean of dependent variable	8.31	0.15	0.05	0.01	0.83
Observations	22,589	22,589	22,589	22,589	22,589

Notes: Standard errors adjusted for clustering at the individual level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

rity by improving employment opportunities for the household, particularly when other employment options are scarce. This paper provides evidence on the impact of such a workfare program on children. We find that the effect on children varies by age, with younger children potentially benefitting from the increased household income and spending more time in school, while older children respond by increasing labor supply which may be an unintended consequence of the program.

Various large-scale programs in developing countries target school attendance, particularly for young children, including conditional cash transfers (Behrman, Parker and Todd, 2005; Schultz, 2004; Rawlings and Rubio, 2005), school feeding programs (Afridi, 2010; Bundy et al., 2009; Jomaa, McDonnell and Probart, 2011), female school stipend programs. (Chaudhury and Parajuli, 2010; Raynor and Wesson, 2006). NREGA is not a program that targets education of children directly, and also differs from other programs in that it targets the household rather than any specific member. Although NREGA promotes employment opportunities for women, it is not specifically targeted towards women in the household. However, as our results show, the spillovers to education for young children are potentially large. If the magnitudes for improvements in school attendance by NREGA are similar to that by other programs, it raises the need for further discussion on the need for targeting. Even if the magnitude is smaller, the results for younger children are comforting as it provides evidence that improved financial security for the household results in increased schooling and improved opportunities for young children.

On the other hand, although NREGA work is restricted to adults, we observe perverse effects on education for older children due to the changes it causes in the local economy and time allocation within the households. Our results show that older children spend less time in school, as well as more time in the labor market, at least partly due to the higher wages caused by NREGA. Therefore, to promote schooling for older children further safety nets should be built into NREGA and into any similar programs. Moreover, our results suggests that when evaluating the effects of such programs it is important to take into account possible spillovers.

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Appendix A

Appendix to Chapter 1

Appendix

A.1 Sample Selection

CDIP selected 20 of their branch offices to participate in the study and and I selected approximately 100 farmers from villages covered by each branch. Within each branch, approximately, one-third of the sample was drawn from CDIP micro-finance clients and the remaining two-thirds were drawn from farmers residing in villages with a CDIP school.[1] The second group of farmers may or may not be directly connected with CDIP.[2] For the first sub-sample, I randomly selected four micro-finance groups from the list provided by CDIP for each branch, and then randomly selected 10 rice farmers from each group. For the second sub-sample, two villages were selected by CDIP in each branch. I conducted a census of farmers in those villages and then randomly selected 30 rice farmers from each village.[3] To be included in the study, the farmer had to meet the following criterion: (1)

[1] The total number of farmers and proportion of CDIP clients in the sample varied in some branches due to logistical constraints or in branches with fewer rice producing areas.

[2] Sample drawn this way for logistical purposes, based on preferences stated by CDIP.

[3] The number of villages or micro-credit groups in each branch sometimes varied based on availability of CDIP staff.

agree to participate, (2) have cultivated rice in the 2012 *Boro* season, (3) at the time of the survey expect to cultivate rice in 2013 and (4) cultivate no greater than five plots in the 2012 season. I did not conduct a census for the short survey, but farmers were selected by CDIP based on these criterion above. In all cases, the primary farmer in the household was interviewed, and multiple farmers were never selected from the same household. At the time of the survey, if the enumerator realized that we had earlier received the name of the household head instead of the main agricultural decision maker, then he or she interviewed the primary farmer instead. Therefore, the household can be considered to be the unit of analysis.

A.2 Supplementary Figures

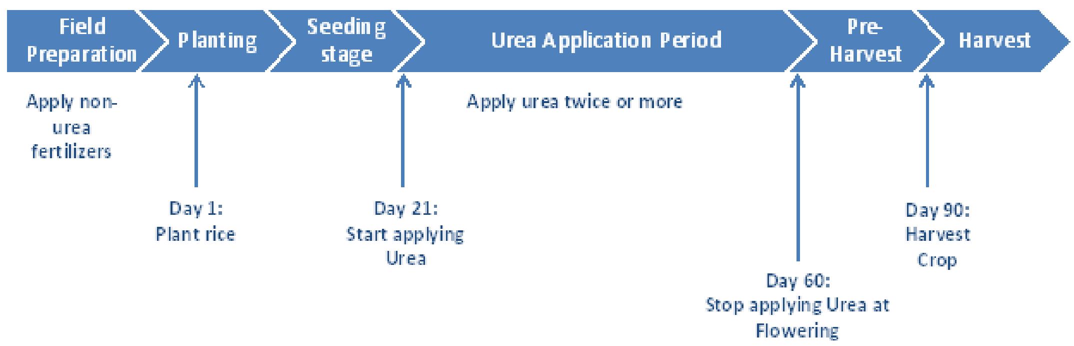

Figure A.1: *Stylized Timeline for Rice Cultivation during Boro Season*

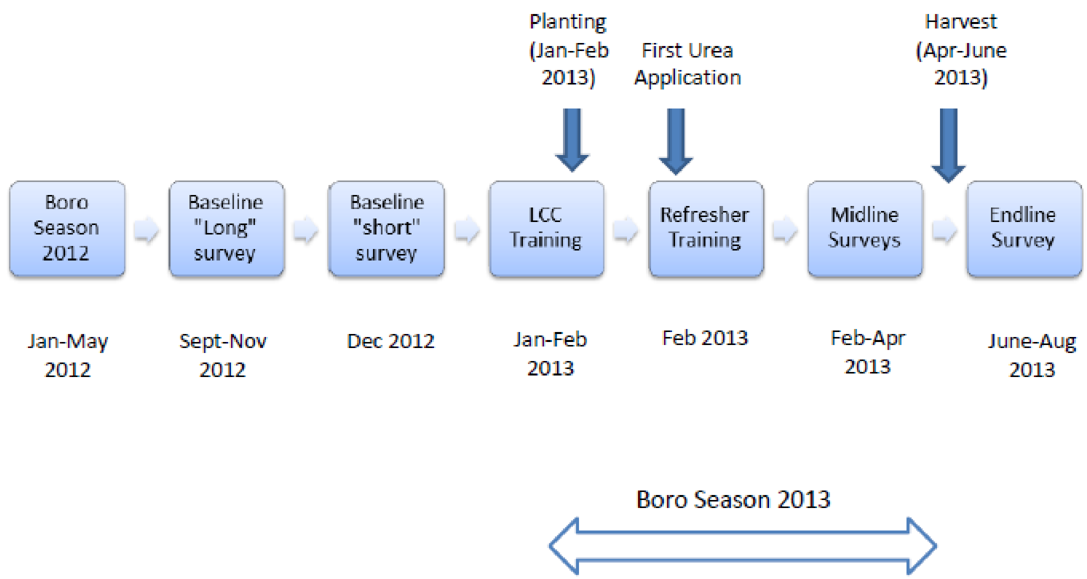

Figure A.2: *Timeline of Study*

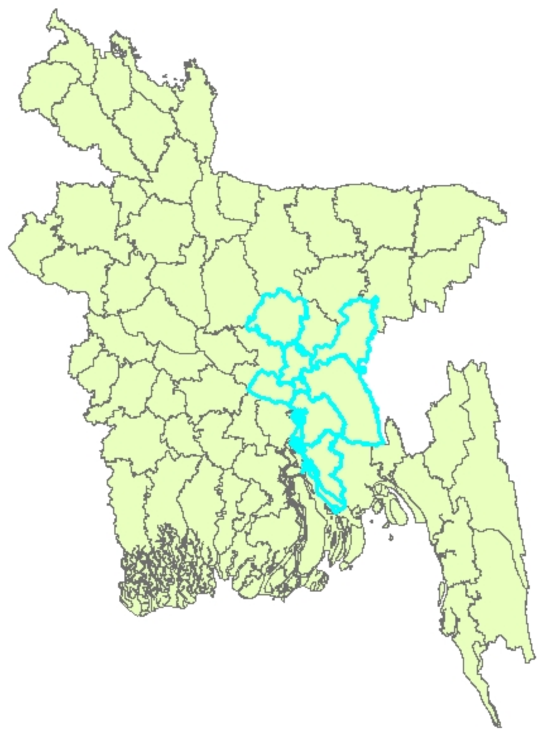

Figure A.3: *Study Areas (Districts) in Bangladesh*

A.3 Supplementary Tables

Table A.1

Descriptive Statistics for Districts in Study Area

District	% Population in Rural Areas	% Population in Agriculture	Average Household Size (Rural)	Urbanization (%)	Literacy Rate (%)
Brahmanbaria	84.21	30.02	5.28	15.79	45.3
Comilla	84.40	30.54	5.10	15.60	53.3
Chandpur	81.97	25.56	4.76	18.03	56.8
Gazipur	69.52	24.02	4.14	30.48	62.5
Lakhipur	84.79	25.10	4.71	15.21	49.4
Munshiganj	87.13	13.29	4.56	12.87	56.1
Narayanganj	66.46	6.30	4.40	33.54	57.1
Noakhali	84.02	19.61	5.20	15.98	51.3
Bangladesh	**76.70**	**23.85**	**4.46**	**23.3**	**51.8**

Note: Source: Bangladesh Bureau of Statistics.
% Urbanization, Literacy rate obtained from Community Reports for each district from the Bangladesh Population & Housing Census 2011. % Population in rural areas computed from total rural population and total population for each district from the same source.
% Population in Agriculture computed from total population and total population in agriculture obtained from Statistical Yearbook of Bangladesh, 2010.
All data obtained online at http://www.sid.gov.bd/

Table A.2

Instructions for Using LCCs

1. Check leaf color with LCC every 10 days, starting 21 days after planing until flowering (If urea is not needed on a day when you check with the LCC, check back again in 5 days).
2. Every time you check leaf color with an LCC, pick out 10 healthy leaf samples (Walk diagonally across the field from one end to the other to pick 10 bunches).
3. For each bunch of leaves, select the topmost fully developed leaf and place it on the LCC to match a color. Compare in the shade of your body.
4. Out of the 10 samples, if 6 or more are light in color (it matched the first two panels of the LCC) then apply 9 kilograms of urea every 33 for decimals of land. Check leaf color with LCC again in 10 days.
5. If urea is not needed on the day you measure (out of the 10 leaf samples, 4 or fewer are light), then check the leaf color again in 5 days with the LCC to see if urea needs to be applied.

Table A.3

Randomization Checks after Attrition

Differences at Baseline for Midline & Endline Samples

	Individual/Household level Variables				**Plot level Variables**						
	(1) Age	(2) Schooling (years)	(3) Non-agri. Inc. (Tk)	(4) Total Plot Area (dec.)	(5) Plot Size (dec.)	(6) Urea (kg/dec)	(7) Yield (kg/dec)	(8) Revenue (Tk/dec)	(9) Total Cost (Tk/dec)	(10) Profit (Tk/dec)	(11) Chi-squared Test
Panel A: Midline (Time Use) Sample											
Treatment	0.006	-0.163	-521.520	-0.327	0.865	-0.010	-0.956	-5.675	-9.178	3.977	0.67
	(0.744)	(0.268)	(661.530)	(2.188)	(0.929)	(0.028)	(0.847)	(10.825)	(10.794)	(13.563)	(0.4138)
Control Mean	45.84	6.077	12934	78.04	45.84	1.069	26.81	362.9	251.8	109.9	
Observations	1,062	1,013	1,016	1,080	2,548	2,488	2,488	2,327	2,346	2,327	
Panel B: Endline Sample											
Treatment	0.361	-0.172	-797.780	1.594	1.237	-0.006	-1.291	-23.644*	-18.369*	-4.293	2.41
	(0.629)	(0.213)	(549.472)	(2.126)	(0.869)	(0.027)	(0.801)	(12.115)	(9.413)	(13.387)	(0.1205)
Control Mean	46.25	5.973	10985	80.51	46.25	1.005	26.23	354.6	241.7	111.4	
Observations	1,524	1,477	1,428	1,549	3,638	3,567	3,566	2,703	2,724	2,703	

Notes: This table shows randomization checks for the midline (time-use) sample and the endline sample after attrition. It reports coefficient of *Treatment* for regressions of each dependent variable on *Treatment* and strata fixed effects for the midline time-use surveys. Robust standard errors for regressions with individual/household level variables and standard errors clustered at household level for regressions with plot level variables are shown in parentheses.
The joint test used a chi-squared test to estimate whether the coefficients are jointly significant.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.4

Changes in Urea Application Intervals during the Season

	(1) # Days from Planting to 1^{st} Application	(2) # Days between 1^{st} and 2^{nd} Applications	(3) # Days between 2^{nd} and 3^{rd} Applications	(4) # Days between 3^{rd} and 4^{th} Applications	(5) # Days between 5^{th} and 6^{th} Applications	(6) # Days from Last Application to Flowering
Panel A: Without any Controls						
Treatment	0.446	-0.609**	0.176	0.745	-0.525	-0.405
	(0.377)	(0.298)	(0.518)	(1.148)	(3.074)	(0.718)
Control Group Mean	13.27	20.72	19.66	17.42	19.40	32.30
Observations	3,541	3,115	1,481	96	13	3,541
Panel A: Including Controls						
Treatment	0.435	-0.598**	0.164	0.489	0.930	-0.346
	(0.372)	(0.298)	(0.527)	(1.030)	(4.699)	(0.711)
Control Group Mean	13.27	20.72	19.66	17.42	19.40	32.30
Observations	3,541	3,115	1,481	96	13	3,541

Notes: This table shows differences in urea application over the season between the treatment and control groups. Control variables include age, schooling, non-agricultural income and total plot area.
Standard errors, shown in parentheses, are clustered at household level. All regressions include strata fixed effects.
*** p<0.01, ** p<0.05, * p<0.1.

Table A.5

OLS Estimates of Time Use by Farmers (7 day recall)

	(1) #Times in Field	(2) Fertilizer Application (minutes)	(3) Weeding (minutes)	(4) Pesticide Application (minutes)	(5) Other Activities (minutes)
Panel A: Without any Controls					
Treatment	0.128*	3.919	6.028	0.825	1.684
	(0.073)	(3.464)	(4.607)	(0.871)	(3.084)
Control Group Mean	2.700	50.31	57.35	4.471	38.85
Observations	2,066	2,066	2,066	2,066	2,066
Panel B: Including all controls					
Treatment	0.112	3.921	5.827	0.786	1.349
	(0.071)	(3.436)	(4.554)	(0.866)	(3.032)
Control Group Mean	2.700	50.31	57.35	4.471	38.85
Observations	2,066	2,066	2,066	2,066	2,066

Notes: This table shows OLS estimates of treatment effects on on time use by farmers using data from Rounds 2 and 4 of the midline surveys. The dependent variables in Columns (2) to (5) are total time spent in minutes in the last seven days on different agricultural activities. Control variables in Panel B include age, schooling, total plot area cultivated and non-agricultural income.
Standard errors clustered at the household level are shown in parentheses. All regressions control for survey round and strata FE.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.6

Full Sample: Treatment Effects on Urea & Yield (Logs)

	Log Urea			Log Yield		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment*Post	-0.113***	-0.120***	-0.126***	0.041	0.038	0.032
	(0.033)	(0.033)	(0.039)	(0.025)	(0.025)	(0.029)
Treatment	0.031	0.034		-0.010	-0.007	
	(0.023)	(0.023)		(0.019)	(0.019)	
Post	0.169***	0.199***	0.198***	-0.054***	-0.042**	-0.040*
	(0.024)	(0.025)	(0.029)	(0.019)	(0.019)	(0.023)
Controls	No	Yes	Yes	No	Yes	Yes
Household FE	No	No	Yes	No	No	Yes
Mean at Baseline	1.011	1.011	1.011	25.73	25.73	25.73
Observations	8,131	8,131	8,131	8,144	8,144	8,144

Notes: This table shows treatment effects on log urea use and log yield. Control variables include age, schooling, total plot area cultivated, income, rice variety.
Standard errors clustered at the household level are shown in parentheses. All regressions include strata fixed effects.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.7

Revenue, Cost & Profits: Price Data from Village Stores

All dependent variables in Takas per decimal

	Long Survey Sample			Full Sample		
	(1)	(2)	(3)	(4)	(5)	(6)
	Revenue	Total Cost	Profit	Revenue	Total Cost	Profit
Panel A: Without Controls						
Treatment*Post	35.597**	22.285	13.312			
	(15.810)	(19.882)	(22.114)			
Treatment	-21.416	-24.154	2.737	9.453**	-0.443	9.896
	(13.503)	(15.070)	(16.782)	(4.660)	(10.391)	(11.232)
Post	-30.629**	40.099***	-70.729***			
	(12.724)	(13.754)	(15.825)			
Means (Baseline/control group)	352.3	240.0	112.3	344.0	289.1	54.92
Observations	6,102	6,102	6,102	3,632	3,632	3,632
Panel B: Including Controls						
Treatment*Post	34.412**	20.126	14.286			
	(15.454)	(19.145)	(21.563)			
Treatment	-19.615	-22.176	2.561	10.035**	0.950	9.999
	(13.164)	(14.693)	(16.529)	(4.626)	(10.657)	(11.482)
Post	-28.206**	39.247***	-67.453***			
	(13.348)	(13.898)	(16.240)			
Means (Baseline/control group)	352.3	240.0	112.3	344.0	289.1	54.92
Observations	6,102	6,102	6,102	3,632	3,632	3,632

Notes: Controls variables include age, schooling, total plot area cultivated, non-agricultural income and rice variety. Standard errors clustered at the household level are shown in parentheses. All regressions include strata fixed effects.
100 decimals = 1 acre
*** p<0.01, ** p<0.05, * p<0.1.

Table A.8

Costs Breakdown (Long Survey Sample)

All costs are in Takas per decimal

	(1) Fertilizers	(2) Manure	(3) Pesticides	(4) Other Expenses	(5) Labor
Panel A: Without Controls					
Treatment*Post	6.711	1.079	0.848	7.493*	-2.241
	(6.872)	(1.252)	(1.165)	(3.894)	(5.493)
Treatment	-7.838	0.356	-0.829	-5.245*	-0.426
	(6.476)	(0.448)	(0.662)	(3.174)	(3.639)
Post	8.351	-0.570	-2.712***	2.065	13.211***
	(5.805)	(0.490)	(0.945)	(3.085)	(3.910)
Mean at Baseline	35.22	1.974	7.013	84.28	111.7
Observations	6,096	5,164	5,705	6,102	6,102
Panel B: Including Controls					
Treatment*Post	6.771	0.840	0.882	7.151*	-2.560
	(6.836)	(1.204)	(1.148)	(3.769)	(5.401)
Treatment	-7.810	0.488	-0.719	-4.834	0.322
	(6.502)	(0.450)	(0.632)	(3.073)	(3.563)
Post	9.759*	-0.456	-2.680***	2.241	13.737***
	(5.282)	(0.516)	(0.991)	(3.207)	(3.927)
Mean at Baseline	35.22	1.974	7.013	84.28	111.7
Observations	6,096	5,164	5,705	6,102	6,102

Notes: Controls variables include age, schooling, total plot area cultivated, non-agricultural income and rice variety.
Standard errors clustered at the household level are shown in parentheses. All regressions include strata fixed effects.
100 decimals = 1 acre
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Appendix B

Appendix to Chapter 2

B.1 Supplementary Tables

Table B.1

Characteristics of University Graduates

	(1) University Graduates
Trainer Age	-0.010**
	(0.005)
Female Trainer	-0.128*
	(0.069)
Experience (years)	0.003
	(0.005)
Extension Worker	-0.557***
	(0.141)
Observations	54
R-squared	0.467

Notes: The variable Extension Workers takes a value of 1 if the trainer is a DAE extension worker and 0 otherwise. The majority of trainers held the designation Sub-Assistant Agriculture Office (formerly known as block supervisors) and are classified as DAE Extension Workers. The variable University Graduates is a dummy variable that is 1 if the trainer has a bachelors or masters degree and is 0 otherwise.
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Appendix C

Appendix to Chapter 3

C.1 Supplementary Tables

Table C.1

Number of Days Spent by the Household in different activities in the last 7 days

The dependent variable is total days worked in each activity Includes District Fixed Effects and Year*Quarter Fixed Effects

	(1) All members (Age 10 to 60)	(2) Adults (Age 18 to 60)	(3) Children (Age 10 to 17)
Panel A: Total Days Working Outside			
NREG	0.187*	0.153	0.079*
	(0.114)	(0.107)	(0.044)
Observations	229,506	229,506	110,637
non-NREG mean of dependent variable	4.336	4.064	0.560
Panel B: Total Days Working in HH Enterprise			
NREG	-0.268*	-0.247*	-0.039
	(0.142)	(0.129)	(0.062)
Observations	229,506	229,506	110,637
non-NREG mean of dependent variable	6.767	6.402	0.753
Panel C: Total Days in Domestic work			
NREG	-0.046	-0.085	0.079
	(0.098)	(0.090)	(0.074)
Observations	229,506	229,506	110,637
non-NREG mean of dependent variable	6.081	5.498	1.201

Notes: Includes controls for household size, religion and social group. Standard errors adjusted for clustering at 570 districts in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table C.2

Number of Days Spent by Boys on Different Activities in the last 7 days

Boys: Age 6 to 17 years (Never married)

Includes District Fixed Effects and Year*Quarter Fixed Effects

	(1) Outside Work	(2) HH Enterprise	(3) Domestic Work	(4) Other
Panel A: Age 15 to 17				
NREG	0.231**	-0.167*	0.074*	0.083
	(0.093)	(0.093)	(0.044)	(0.082)
Observations	36,714	36,714	36,714	36,714
Non-NREG mean	0.959	1.104	0.129	0.745
Panel B: Age 10 to 14				
NREG	0.017	0.025	0.028	-0.019
	(0.024)	(0.035)	(0.025)	(0.055)
Observations	67,945	67,945	67,945	67,945
Non-NREG mean	0.141	0.208	0.092	0.542
Panel C: Age 6 to 9				
NREG	-0.007*	0.004	0.004	-0.201***
	(0.004)	(0.016)	(0.015)	(0.075)
Observations	52,724	52,724	52,724	52,724
Non-NREG mean	0.004	0.014	0.031	0.936

Notes: Includes controls for age, age^2, literacy, household size, religion and caste. Standard errors adjusted for clustering at 570 districts in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table C.3

Number of Days Spent by Girls on Different Activities in the last 7 days

Girls: Age 6 to 17 years (Never married)

Includes District Fixed Effects and Year*Quarter Fixed Effects

	(1) Outside Work	(2) HH Enterprise	(3) Domestic Work	(4) Other
Panel A: Age 15 to 17				
NREG	0.048	0.030	0.047	0.064
	(0.057)	(0.076)	(0.128)	(0.050)
Observations	29,982	29,982	29,982	29,982
Non-NREG mean	0.429	0.606	2.315	0.308
Panel B: Age 10 to 14				
NREG	0.001	0.039	0.025	-0.096**
	(0.020)	(0.055)	(0.071)	(0.044)
Observations	59,421	59,421	59,421	59,421
Non-NREG mean	0.105	0.183	0.828	0.466
Panel C: Age 6 to 9				
NREG	-0.001	-0.003	0.036	-0.196**
	(0.004)	(0.012)	(0.034)	(0.083)
Observations	47,698	47,698	47,698	47,698
Non-NREG mean	0.004	0.014	0.083	1.179

Notes: Includes controls for age, age^2, literacy, household size, religion and caste. Standard errors adjusted for clustering at 570 districts in parentheses. *** p<0.01, ** p<0.05, * p<0.1