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ABSTRACT

Contract Structure, Time Preference, and Technology Adoption*

Do constraints to technology adoption vary by behavioral traits? We randomize 150 villages in Bangladesh into being offered standard microcredit, loans with a grace period, the choice between those two contracts, and control. No discernible average effects are detected on the adoption of mechanized irrigation, hybrid seeds, and chemical fertilizers. However, credit access enhances technology adoption among present-biased farmers, whose output and profits increase. These effects are driven by the standard contract and choice villages, as present-biased farmers select out of the grace period contract. This suggests offering commitment and screening applicants on present bias to enhance agricultural technology adoption.

JEL Classification: D15, G51, O13, O33, Q14, Q16

Keywords: microfinance, technology adoption, time inconsistency, Bangladesh

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1 Introduction

The adoption of new agricultural technologies is key to the process of economic development and growth (Schultz, 1964; Hayami et al., 1971; Huffman and Evenson, 2008; Gollin et al., 2002; Restuccia et al., 2008). Growth in agriculture also reduces poverty to a greater extent than growth in other sectors (Ivanic and Martin, 2018; Ligon and Sadoulet, 2018), with a self-reinforcing feedback between low soil fertility and chronic poverty (Barrett and Bevis, 2015). Yet, despite high rates of return, agricultural innovations are often adopted slowly, limiting the growth of the production possibility frontier (Duflo et al., 2008; Magruder, 2018). This highlights the importance of understanding the determinants of and constraints to agricultural technology adoption, as well as the effectiveness of policies to abate those constraints. A lack of access to finance is often cited as being among the main constraints to agricultural technology adoption (Foster and Rosenzweig, 2010; de Janvry, 2016). Causal evidence on this is relatively scant, however, and the experimental literature of microcredit impacts suggests that increased credit access only has meaningful impacts on business performance for a small share of recipients (Meager, 2019). Little research has been conducted on population heterogeneity, especially along behavioral factors, in impacts of access to credit on technology adoption. Therefore, to enhance technology adoption, an avenue worth exploring is to identify subpopulations with behavioral traits that are likely to benefit from credit contracts that are tailored to those traits (Jayachandran, 2020).

This paper investigates the impact of access to credit on agricultural technology adoption through a cluster-randomized controlled trial (RCT) in rural Bangladesh, and studies heterogeneity in these impacts along farmer's risk preferences and time inconsistency. At baseline, we elicit risk and time preferences of a subsample of households through incentivized choice experiments. To study how time preferences interact with contract structure, we randomize the treatment villages into three arms: (i) a standard microcredit contract with weekly repayment starting two weeks after loan disbursement, (ii) an otherwise similar contract with a 3-month grace period, and (iii) contract of choice, wherein the prospective borrowers can choose between the standard or the grace period contract. With the endline being two years post-treatment, we do not observe an aggregate effect on borrowing in general, but do find a decrease in borrowing from moneylenders. We also cannot reject the null of no Intent-to-Treat (ITT) effects of any of the credit contracts on the adoption of mechanized irrigation, any of a set of 6 types of fertilizers, or hybrid seeds.

However, for present-biased individuals, credit access induces technology adoption, but only in the villages assigned to the standard contract or the contract of choice. Consistent with these findings on impact, we find that present-biased farmers self-select out of the grace period contract and farmers with time consistent preferences select into it. Assuming borrowers choose the contract they value most, and given that the grace period contract is cheaper, this signals that the desire to commit to save of these present-biased farmers prevails over the lower implicit cost of credit of the grace period contract and the possibly better fit of the grace period contract with the gestation period of agricultural investments.

The evidence that risk preferences matter is less robust. Assignment to the standard contract reduces agricultural technology adoption among risk averse farmers in some of the estimations, suggesting that these farmers are deterred by the downside risks of applying (more of) the technologies. This finding is in line with Dercon and Christiaensen (2011), who found that the possibility of a poor harvest (and hence negative returns and very low consumption) can account for the low use of fertilizer in Ethiopia. However, the statistical significance of these estimates on risk aversion disappear when accounting for multiple inference. A possible explanation for this is that risk preferences in one domain (monetary payoffs in our elicitation) may not (fully) translate to decisions under uncertainty in another domain (technology adoption decisions) (Einav et al., 2012).

Our findings contribute to at least three strands of literature. First, they contribute to the literature on the impact of access to credit on technology adoption. There have been three recent RCTs that used the expansion of a credit product to learn about the importance of credit constraints for technology adoption[1], in Morocco (Crépon et al., 2015), Mali (Beaman et al., 2020) and Ethiopia (Tarozzi et al., 2015). Across those studies, a minority of individuals took up credit: 17% in the Moroccan trial, 21% in Mali and 36% in Ethiopia. Borrowers respond by increasing input purchases, particularly of fertilizer. While each study presents input purchases somewhat differently, Crépon et al. (2015) report a 19% increase in business expenses (above the control mean), which include

[1] There have also been attempts to study this question using observational data. For example, Abate et al. (2016) used propensity score matching and found that access to microcredit increased the adoption of agricultural technologies in Ethiopia, including the adoption of modern seeds and the use of chemical fertilizers. In Bangladesh, Islam et al. (2012) examined the impact of access to microcredit on the adoption of modern rice varieties by estimating a stochastic frontier production function and found that households with access to microcredit had a relatively higher probability of adopting the technology. However, these studies are potentially prone to bias from selection into credit on unobservables.

agricultural inputs; Beaman et al. (2020) report an 11% increase in total inputs (fertilizer and other chemical inputs account for two-thirds to three-quarters of these gains); and Tarozzi et al. (2015) find an enormous 295% increase in input usage, in part because the base usage is extremely low.

Those studies differ from the current article in at least three important respects that have academic and policy implications. First, they offered joint liability loans; in contrast we offered individual liability loans. The peer monitoring associated with joint liability loans can induce borrowers to take too little risk in investment decisions relative to the social optimum (Banerjee et al., 1994; Fischer, 2013). Contract structure also differs, in that in two of our treatment arms, the loan contracts combine a grace period with weekly repayment. The loans in the Ethiopian RCT did not have a grace period, while loans in Mali were repaid in lumpsum at harvest time, which fits the agricultural cycle but does not address the time inconsistency of some farmers. Second, at baseline, we elicited risk and time preferences to evaluate differential uptake and treatment effect heterogeneity along those dimensions.[2] Third, we looked at an expanded set of technologies beyond fertilizers, including mechanized irrigation and high-yielding hybrid seeds.

This brings us to the second strand of literature in which our study fits. Traditional microfinance contracts, which require repayment to start immediately, may limit investments to low-risk, low-return projects, inhibiting high fixed-cost, more risky, indivisible investments (Field et al., 2013; Liu and Roth, 2020). Most of the agricultural technologies in this paper are divisible (mechanized irrigation being the exception), but investment in them may also be aided by a grace period, given the gestation periods due to the biological nature of agricultural production. The contrast between the standard, delayed, and choice villages provides evidence on whether grace periods are needed to enable such investments; and our findings suggest this aspect of contract structure not to be critical in this agricultural setting.

There are few tangential studies, which evaluate effects of contract flexibility, including repayment frequency (Field and Pande, 2008; Field et al., 2012). More relevant to our work is Barboni and Agarwal (2018), who randomized bank branches into offering standard contracts and offering

[2] Liu (2013) elicited the risk preferences of Chinese farmers and found risk aversion and loss aversion to correlate with slower adoption of Bacillus thuringiensis (Bt) cotton. However, the ex-post measurement of risk aversion may be problematic if farmers changed their risk preferences in response to the adoption of Bt cotton. Several studies indicate that time preferences may change over time (Cameron and Shah, 2015; Perez-Arce, 2017; Jung et al., 2019), and in response to returns on agricultural investment (Galor and Özak, 2016). In contrast, we elicited risk (and time) preferences ex-ante.

the choice between standard contracts and more expensive contracts with more flexibility (allowing for missing some repayments). They find higher repayment and business sales in the branches where clients could choose between the standard contract and the flexible contract. Like us, they find that time-consistent borrowers are significantly more likely to opt for the flexible repayment schedule. Battaglia et al. (2019) similarly randomize repayment flexibility (allowing to delay up to 2 out of 12 monthly repayments), and find the flexible contract to increase business outcomes and socio-economic status, combined with lower default rates. However, they find no significant relation between the take up decision and having time-inconsistent preferences, and they did not include a treatment arm wherein individuals could choose between the contracts.

A third literature to which our study contributes, relates to how time preferences - and time inconsistencies in particular, affect borrowing behavior and generate a demand for commitment.[3] Meier and Sprenger (2010) found present-biased individuals on average to have higher credit card debt in the US. Using observational data, Bauer et al. (2012) found a higher prevalence of having microcredit among present-biased individuals in India, which they explain by those individuals having higher demand for the commitment embodied in typical microcredit contracts with their frequent (often weekly) repayment schedules. Our experimental findings likewise suggest present-biased farmers to have a relatively higher demand for contracts wherein repayment starts immediately (as opposed to a contract with a grace period), and time-consistent farmers to have higher demand for flexibility (specifically, a grace period). This paper goes a step further by elucidating how credit as a commitment device helps present-biased individuals invest in profitable technologies.

The rest of this paper is organized as follows. Section 2 describes the experimental design and interventions. Section 3 describes the data collected and the estimation methods. Section 4 presents results: treatment take-up (Subsection 4.1), aggregate effects of the credit supply expansion on borrowing, agricultural technology adoption, output and profits (Subsection 4.2), treatment effect heterogeneity, including along risk and time preference (Subsection 4.3), and self-selection into loan contract structure along present-biasedness (Subsection 4.5). Section 5 discusses the implications and concludes.

[3] See DellaVigna (2009) for a review of the literature of psychology and economics, and Kremer et al. (2019) a review of its application in development economics - behavioral development economics.

2 Setting, Intervention and Experimental design

The experiment is set in Bangladesh, where 40% of of the population is employed in agriculture. Most of the farmers are smallholders, many of whom have not adopted, or under-utilize, high-yielding inputs and cropping techniques. This is illustrated by the amount of fertilizer applied in Bangladesh in contrast to the recommended amount. For example, during the Boro planting season (from February to March), the amount of Urea applied is approximately 30% less than the recommended dose per hectare (Jaim and Akter, 2012). In our baseline data, 37.2% of farmers applied mechanized irrigation, and only 19.3% used recently developed, high-yielding hybrid seeds,[4] which is notably lower than in other, similar countries (Mottaleb et al., 2015).[5]

Among rural households, access to finance from formal financial institutions (banks) has stagnated and is usually limited to wealthy farmers. Over time (2000 - 2013), such financing has declined in size relative to informal sources of credit (microfinance institutions (MFIs)[6], moneylenders) (Gautam and Faruqee, 2016). The share of agricultural credit to total credit is 5.17%, which is higher than in many Sub-Saharan African countries, but lower than other Asian countries such as India (9.03%) Vietnam (9.92%). (FAO, 2018).

Without convenient and timely access to agricultural credit, modern technologies and high-yielding inputs that require relatively large capital investments remain beyond the reach of most smallholder farmers (Jack, 2011).[7]

Our study was conducted with RDRS, a leading MFI in Bangladesh. In 2010, it had 2,047,219 borrowers (85 per cent female) and serviced 17 out of 64 districts nationwide (RDRS Bangladesh, 2017). Its outstanding loans amounted to five billion Taka (USD 60 million), distributed across 184 branches. The organization focuses on remote, marginalized and underserved communities, particularly in the deprived northern region. In collaboration with RDRS, we randomly varied

[4]These percentages are calculated from the whole baseline survey, and thus differ slightly from those in Table 1, which are calculated from the subsample that was selected for the endline survey.

[5]The hybrid seeds considered in this paper are newer, more productive varieties, in contrast to the high-yielding varieties promoted through the Green Revolution, which are already widely adopted in Bangladesh. The productivity gains from the green revolution during the 1960s and 1970s are almost exhausted (Mottaleb et al., 2015).

[6]Due to the history of microfinance in Bangladesh, with Grameen Bank as pioneer and progressively other NGOs providing microcredit, most current MFIs in Bangladesh are NGOs. Hence, we do not make a distinction between them and use the term MFI in this paper.

[7]Suri (2011) showed that in Kenya, adoption decisions are a function of the cost of acquiring the technologies, which in turn depends on the distance to distribution points. Unlike in much of Africa however, fertilizers and hybrid seeds are widely available in Bangladesh. As seen in Table 16 in the online Appendix, fertilizers are available in all of the 150 sampled villages and hybrid seeds in 143 of them.

its entry into communities by taking advantage of its recent expansion into the North-Eastern and North-Western regions. RDRS identified 7 sub-districts in 5 districts (Dinajpur, Lalmonirhat, Gaibanda, Moulavibazzar and Rangpur) into which it planned to expand. We randomly sampled 150 villages in these subdistricts. Randomization into treatment arms was at the village (rather than the household) level, given the possibility of spillover effects of credit supply. A village census was conducted in each village, and in an attempt to identify households likely to take up credit, they were asked whether they would borrow if an MFI would offer them credit in the next 6 months[8] ('willing' households) or not ('unwilling' households).

To understand how present-bias interacts with loan contract structure, villages were randomized into (i) standard loan contract offers, (ii) offers of loans with a 3-month grace period, (iii) choice villages wherein households could choose between the two contracts, and (iv) pure control villages (Figure 1). The loans have individual liability, a term of (mostly) one year, and weekly repayment. Other than the presence/absence of a grace period, the loans were homogeneous, with amounts varying very little: most loans were 10,000 Taka or approximately USD 128 (minimum 5,000 Taka; maximum 20,000 Taka; standard deviation 2,237 Taka).

The baseline survey was carried out in September and October 2012, and the treatments were rolled starting in November 2013. In treatment villages, credit was offered to the 'willing' households only, and this paper only considers this sample of households that indicated a willingness to borrow. In October-December 2015, we randomly sampled 10 willing and 10 unwilling households in each village for the endline survey.

[8]The phrasing was "If an MFI were to start giving out loans in your village in the next 6 months, would you be applying for a loan from them?"

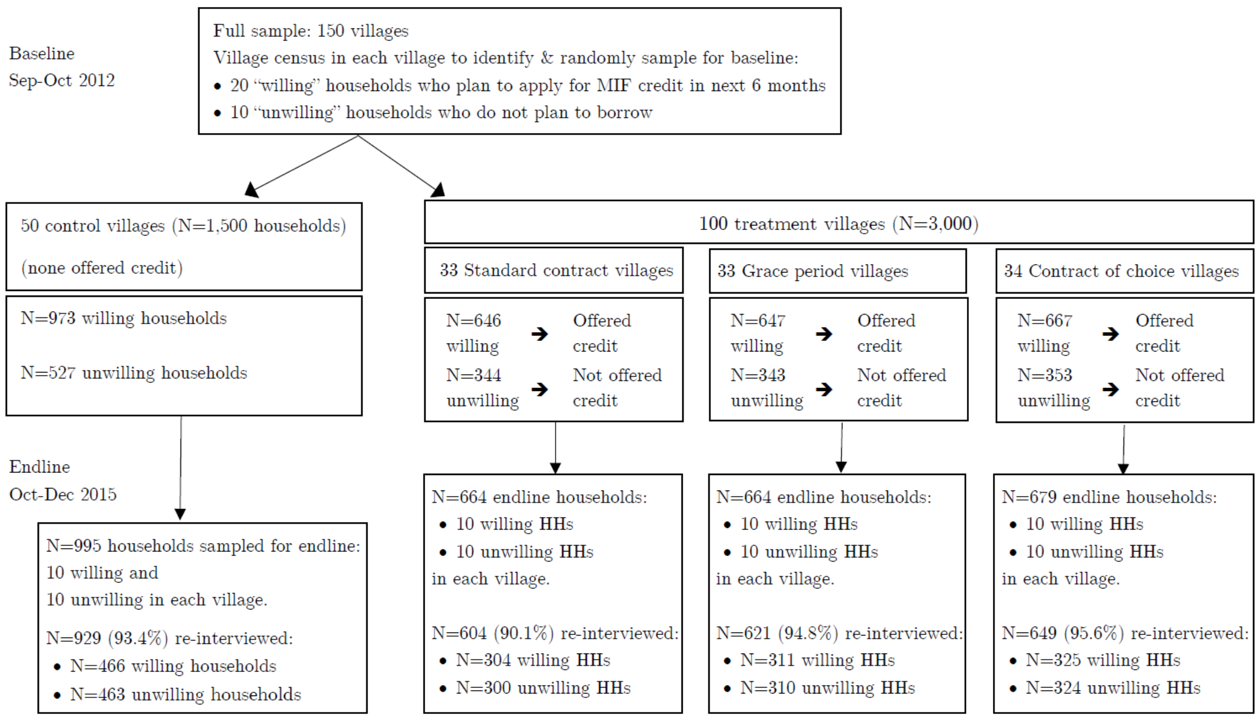

Figure 1: Experimental design

3 Data and Estimation Methods

This study uses four key data sources: a baseline survey, risk and time preference data from incentivized elicitations on a sub-sample of the baseline sample, administrative data from RDRS on product take-up, and a follow-up survey three years later.

The baseline and follow-up surveys included a variety of questions on the socio-demographic characteristics and other information about the household and its economic activities. Outcome variables are divided into three categories: (1) credit, including borrowing form both formal (MFIs, commercial banks) and informal (moneylenders) sources, (2) agricultural technology adoption, (3) agricultural output and profits.

Table 1 displays summary statistics and balance tests of baseline variables for the sample of households that indicated a willingness to borrow at baseline and were selected for the endline survey, which is the sample used in this paper (see discussion below). At baseline, 43.7% of this sample uses at least one of the agricultural technologies under study (45.3% in the whole baseline sample), and a similar percentage had taken an MFI loan in the cropping year preceding the baseline. According to the joint orthogonality test, treatment and control villages appear balanced in observables, but the contract of choice villages show imbalance with the other treatment arms in some variables - they have a somewhat higher baseline borrowing from MFIs and other informal sources (such as cooperatives), had more educated household heads, who are less likely to have a wage job. Table 16 in the online Appendix shows the prices of hybrid seeds and fertilizers, as well as the percentage of irrigation pumps that run with electricity, to not statistically significantly differ across contract (village) types.

Out of the 1,490 households that were offered credit and that were (randomly) selected for the endline survey, 1,406 were re-interviewed at endline, so the attrition rate is 5.64%. Table 15 in the online Appendix shows that in the sample that was successfully re-interviewed at endline, the sample is still balanced. Attrition does not statistically significantly differ between treatment arms (Table 1).

Given budget constraints, risk and time preferences were elicited from a subsample of baseline households. An attempt was made to elicit the preferences of all female-headed households, and 260 of the 285 female-headed households were included. Beyond that, additional households were randomly sampled in each village until 10 households in each village were included in this subsample. Hence, preferences were elicited of 1,500 households. With the preference elicitation subsample, 49.7% of respondents is the head of their household, 48.1% is the spouse of the head of their household, and the remaining 2.2% has another relationship to the head of the household. Therefore, in the regressions of treatment effect heterogeneity along risk and time preferences later in the paper, we include as additional controls the respondents' gender, age, educational attainment, occupation and indicators for his or her relationship to the head of the household. A comparison of those who were selected to take part in the risk and time preference elicitation and those that were not selected, shows, as expected, that the main difference is in whether the head of the household is female (Table 17 in the online Appendix). Despite a large majority of marriages in Bangladesh being arranged, several studies found evidence of assortative matching on risk and time preferences in the marriage market, as well as intergenerational transmission of those preferences (Ambrus et al., 2010; Chowdhury et al., 2018).

For time preferences, we followed a protocol similar to the one Bauer et al. (2012) implemented in rural India. Here, respondents faced a trade-off between a sooner, but smaller reward and a later, but larger reward. There were three choice sets; each choice set containing six choices represented in a single choice list format, where the choice sets varied in terms of payment delay. For the current purpose, we calculate if a respondent is present-bias or not. Finally, respondents' risk aversion was measured using the risk elicitation protocol pioneered by Binswanger (1980) in developing country settings, where respondents had to choose one out of six gambles that yielded either a high or a low payoff with an equal probability of 50%. The low payoff was decreasing and the high payoff was increasing for each successive gamble such that higher numbered gambles are riskier - they are characterized by an increase in expected earnings and in the variance of earnings. The online Appendix contains the experimental protocols associated with the time and risk preference elicitation, as well as the distribution of responses to the risk preference elicitation experiment. More than half (58.5%) of respondents are risk averse, 26.9% of respondents are risk-neutral, and 14.6% are risk-loving.

Table 2 in the online Appendix shows the distribution of responses to the time preference questions: 16.9% of individuals are categorized as weakly present-biased, and 19.9% of individuals are moderately or strongly present-biased. These proportions are similar to the those found in India by Bauer et al. (2012), in whose sample 13.2% of individuals were weakly present-biased, and 19.9% were strongly present-biased. To maximize statistical power, strong present-bias is our main measure used in this paper, but we also conduct robustness checks with an ordinal measure.

Table 1: Summary statistics and balance tests.

Panel A: Credit variables	Full sample (n=1490)	Control villages (n=497)	Treatment villages (n=993)	Treated - Controls (p-value)	Standard contract (n=325)	Grace period contract (n=328)	Contract of choice (n=340)	Equality of the 3 contracts (p-value)
Any (i.e., ≥ 1) MFI loan	0.44 (0.50)	0.45 (0.50)	0.44 (0.50)	0.954	0.45 (0.50)	0.44 (0.50)	0.44 (0.50)	0.965
# of MFI loans	0.54 (0.68)	0.52 (0.64)	0.55 (0.69)	0.650	0.54 (0.66)	0.52 (0.66)	0.57 (0.75)	0.853
Any moneylender loan	0.16 (0.36)	0.18 (0.38)	.015 (0.35)	0.207	0.16 (0.37)	0.16 (0.37)	0.11 (0.32)	0.172
Any friend/relative loan	0.42 (0.49)	0.45 (0.5)	0.4 (0.49)	0.158	0.43 (0.5)	0.4 (0.49)	0.37 (0.48)	0.431
Any other informal loan	0.09 (0.28)	0.07 (0.26)	0.1 (0.29)	0.299	0.09 (0.28)	0.09 (0.29)	0.11 (0.31)	0.796
Total # of loans	1.49 (1.23)	1.55 (1.22)	1.46 (1.24)	0.291	1.58 (1.33)	1.47 (1.22)	1.34 (1.16)	0.196
Total amount borrowed (1000 Taka)	9.34 (12.01)	9.66 (12.38)	9.17 (11.83)	0.582	9.72 (12.93)	8.88 (12.11)	8.92 (10.38)	0.728
Panel B: Outcome variables								
Any technology adopted	0.44 (0.5)	0.44 (0.5)	0.44 (0.5)	0.989	0.42 (0.49)	0.46 (0.5)	0.43 (0.5)	0.687
# of technology adopted	2.03 (2.54)	2.09 (2.6)	2 (2.51)	0.668	1.92 (2.49)	2.17 (2.57)	1.92 (2.46)	0.661
Land area on which technologies are applied	1.58 (2.96)	1.74 (3.21)	1.5 (2.82)	0.239	1.54 (2.91)	1.51 (2.89)	1.46 (2.66)	0.955
Agricultural output (1000 Taka)	12.2 (22.59)	13.43 (24.16)	11.59 (21.75)	0.262	11.51 (22.6)	11.43 (20.89)	11.81 (21.79)	0.98
Agricultural profits (1000 Taka)	5.89 (14.15)	6.74 (15.62)	5.46 (13.33)	0.279	5.52 (13.79)	5.26 (11.61)	5.59 (14.43)	0.961
Panel C: Control variables								
Female head of HH	0.11 (0.31)	0.08 (0.28)	0.12 (0.32)	0.102	0.13 (0.34)	0.13 (0.33)	0.09 (0.29)	0.231
Head of HH's Educ	2.02 (3.13)	1.85 (3.04)	2.1 (3.18)	0.162	1.66 (2.78)	2.34 (3.36)	2.3 (3.32)	0.008
Head of HH is Muslim	0.79 (0.41)	0.82 (0.39)	0.77 (0.42)	0.348	0.77 (0.42)	0.77 (0.42)	0.77 (0.42)	0.995
Household size	4.1 (1.32)	4.18 (1.27)	4.07 (1.35)	0.191	4.07 (1.4)	4.02 (1.37)	4.1 (1.29)	0.766
Average age	24.09 (8.14)	23.95 (8.04)	24.16 (8.2)	0.632	24.36 (8.41)	23.74 (8.11)	24.38 (8.09)	0.629
Land holdings	12.35 (21.4)	12.88 (21.59)	12.08 (21.31)	0.591	11.34 (19.27)	11.72 (20.19)	13.15 (24.07)	0.703
Asset index	-0.06 (1.7)	-0.08 (1.58)	-0.04 (1.76)	0.736	-0.13 (1.82)	0.04 (1.79)	-0.04 (1.68)	0.733
Head of HH primary occupation category 1: self-employed agriculture	0.07 (0.25)	0.07 (0.26)	0.07 (0.25)	0.692	0.06 (0.25)	0.07 (0.26)	0.06 (0.24)	0.825
Occ. cat. 2: agric wage labor	0.44 (0.5)	0.45 (0.5)	0.44 (0.5)	0.609	0.38 (0.49)	0.48 (0.5)	0.45 (0.5)	0.145
Occ. cat. 3 fisheries	0.03 (0.17)	0.03 (0.18)	0.03 (0.16)	0.739	0.02 (0.12)	0.05 (0.22)	0.01 (0.12)	0.26
Occ. cat. 4 self-employment	0.13 (0.33)	0.13 (0.34)	0.13 (0.33)	0.929	0.15 (0.36)	0.11 (0.31)	0.12 (0.33)	0.426
Occ. cat. 5: freelancing	0.13 (0.33)	0.12 (0.32)	0.13 (0.34)	0.486	0.15 (0.36)	0.1 (0.3)	0.14 (0.35)	0.161
Occ. cat. 6: housewife	0.04 (0.19)	0.03 (0.17)	0.04 (0.2)	0.332	0.05 (0.22)	0.04 (0.2)	0.04 (0.18)	0.769
Occ. cat. 7: salaried job	0.03 (0.18)	0.03 (0.18)	0.03 (0.18)	0.855	0.05 (0.22)	0.03 (0.17)	0.02 (0.13)	0.139
Occ. cat. 8: other	0.14 (0.34)	0.13 (0.34)	0.14 (0.35)	0.643	0.15 (0.36)	0.11 (0.32)	0.16 (0.37)	0.203
Panel D: Attrition	[n=1,490]	[n=497]	[n=993]		[n=325]	[n=328]	[n=340]	
Not surveyed at endline	0.06 (0.23)	0.06 (0.24)	0.05 (0.22)	0.547	0.06 (0.25)	0.05 (0.22)	0.04 (0.21)	0.165
Joint orthogonality test: p-value	(OLS, based on F-test)			0.555	OLS, F-test: Control vs. standard			0.701
					Control vs. grace period			0.185
					Control vs. choice			0.007
					Multinomial logit, X^2-test: all 4 arms			0.000

[a] Based on a regression of the variable in the leftmost column on a treatment indicator, with robust standard errors clustered at the village level.
[b] Regressions (with standard errors clustered at the village level) of the variable listed in the leftmost column on indicators for the three contract type villages; the p-value in the rightmost column is based on an F-test of equality of the coefficients on those three coefficients.

The agricultural technologies considered in this study, and recorded at baseline and endline, are mechanized irrigation, 6 types of fertilizer, and hybrid, high-yielding seeds - for more details see the online Appendix. We analyze three main outcome variables capturing agricultural technology adoption ($\mathbb{1}\{.\}$ is the indicator function):

- $\mathbb{1}\{\geq 1$ technology adopted$\}$;
- Number of technologies adopted;
- Gross land area (in decimals) to which technologies are applied (total over all cropping seasons in the last cropping year).

To estimate intent-to-treat (ITT) effects of the expansion of credit supply on household outcomes, we restrict the sample to households that had indicated to be 'willing' to borrow.[9] First, we run simple ITT regressions of the outcomes on an indicator that takes on 1 for households in treated villages (and 0 otherwise):

$$y_{ijk} = \alpha_k + \beta_k treatment_{ij} + X_{ij}\gamma_k + \varepsilon_{ijk} \tag{1}$$

where y_{ijk} is outcome k for household i in village j, $treatment_{ij}$ takes on 1 if the household resides in one of the treatment villages (and 0 otherwise), X_{ij} is a vector of baseline covariates (including district indicators), and ε_{ijk} are household-specific, idiosyncratic errors. To evaluate whether the different loan contract types had a differential effect on outcomes, an alternative ITT specification replaces $treatment_{ij}$ with a set of indicator variables for the 3 contracts (standard, grace period, contract of choice). Second, to increase power, we run ANCOVA by including baseline realizations of the dependent variable as regressor (McKenzie, 2012).[10] To correct for attrition, we re-do the key estimations of this paper using Weighted Least Squares (WLS) with weights based

[9] An alternative approach would be to include as well 'unwilling' households in control villages, and include an indicator variable for 'unwilling in control villages'. However, such approach would require potential outcomes to be additively separable in $treatment_{ij}$ and $unwilling_{ij}$, that is, it would assume the absence of essential heterogeneity (Heckman et al., 2006). Beaman et al. (2020) found evidence for essential heterogeneity in Mali, in that farmers with higher returns to capital are more likely to select into credit. Yet another alternative would be to include all households and include in the regressions the $treatment_{ij}$ indicator, an indicator for 'willing to borrow' and their interaction. However, that interaction term would be potentially endogenous due to the potential endogeneity of 'willingness' to borrow.

[10] The ANCOVA regressions are robustness checks and not our preferred estimations, as pre-treatment outcomes are endogenous in the regressions, and their inclusion thus potentially biases coefficients on treatment assignment(s) (Frölich, 2008)

on the inverse of the propensity of being re-interviewed: $w(z,x) = \left[\frac{Pr(R=1|z,x)}{Pr(R=1|x)}\right]^{-1}$, where R is an indicator variable taking on 1 if the household was re-interviewed at endline, x are covariates used in the regression the robustness of which is evaluated, and z are auxiliary variables that predict attrition (John et al., 1998; Wooldridge, 2002). We include in z the respondent's gender, age, educational attainment and occupation, enumerator indicators, an indicator for the baseline interview taking place on a Friday (which predicts attrition, likely due to it being the day of congregational prayer for Muslims), and baseline realizations of the borrowing and technology adoption outcomes.

Next, we consider treatment effect heterogeneity. A possible concern is that the main treatment effect heterogeneity reported in this paper - heterogeneity along present-bias, is merely the result of a data mining exercise. We therefore run causal forests to identify and rank the relative importance of each covariate in terms of the extent to which it moderates the treatment effect (Athey and Imbens, 2016; Athey et al., 2019). The fact that risk and time preferences were elicited at baseline through costly incentivized choice experiments, is also a signal of ex-ante hypothesized effect heterogeneity along those dimensions.

Finally, we estimate the local average treatment effect (LATE) to evaluate the effect of credit uptake on the technology adoption decisions by farmers who are induced by the randomized supply expansion to take up credit.

4 Results

4.1 First stage: treatment take-up and effects on borrowing

Across treatment villages, 364 out of 1,593 households who were offered loans took up the offer, so the take-up rate is 18.6%. This is a similar take-up rate as RCTs of credit expansions in Morocco (17%) (Crépon et al., 2015) and Mexico (19%) (Angelucci et al., 2015), and somewhat lower than in Ethiopia (31%) (Tarozzi et al., 2015).[11] The take-up in the grace period contract villages is higher (23.5%) than in the standard contract villages (18.7%), but saliently, take-up in the choice

[11] Unlike the RCTs in Morocco, Mexico, and Ethiopia, the RCT in India by Banerjee et al. (2015) was conducted exclusively in an urban context (Hyderabad). In their study, the MFI started lending in control areas before the endline survey. They found that 26.7 percent of households in treated neighborhoods had an MFI loan, compared to 18.3 percent of households in comparison neighborhoods.

villages is lowest (13.7%), and these differences are statistically significant ($\chi^2 = 9.917; p = 0.007$). The results of regressions of take-up on the contract type groups with and without control variables (Table 2), point to the same conclusion.[12] The lower take-up in the contract of choice villages as compared to both the standard contract villages and the grace period contract villages, indicates choice deferral.[13] It is consistent with choice overload (Iyengar and Lepper, 2000; Kuksov and Villas-Boas, 2010), regret avoidance (Luce, 1998) due to economising on time or cognitive resources (Ortoleva, 2013; Mani et al., 2013), or a changing reference point when presented with more options (Deb and Zhou, 2018). Bertrand et al. (2010) similarly found lower credit demand when a South African consumer lender offered four example loans instead of one.

Table 2: Loan take-up across treatment arms.

	(1)	(2)	(3)	(4)
	Sample: All HHs that were offered credit		Sample: offered credit & selected for endline	
Standard contract	0.187	0.192	0.188	0.194
	(0.0324)	(0.0334)	(0.0365)	(0.0363)
Grace period contract	0.235	0.240	0.226	0.234
	(0.0372)	(0.0312)	(0.0371)	(0.0328)
Contract of choice	0.137	0.136	0.132	0.132
	(0.0293)	(0.0248)	(0.0282)	(0.0245)
F-test equality of contracts	2.21	3.98	2.12	3.74
{p-value}	{0.113}	{0.021}	{0.124}	{0.026}
E(dep. var.)^a	0.181	0.134	0.181	0.181
Observations	2,929	2,929	1,490	1,490
Controls		✓		✓

Robust standard errors clustered at the village level in parentheses. Coefficient estimates are based on linear probability models; estimations of columns (1) and (3) are without a constant. The set of controls includes gender, years of education and religion (Muslim indicator) of the head of the household, household size, average age of household members, land ownings, (non-land) asset index, head of household's primary occupation category indicators, and district indicators. [a] Mean of loan take-up in treatment villages.

Next, we analyze effects on borrowing (Table 3). Note that the loans of RDRS had terms of mostly 1 year, and that the endline measures of borrowing correspond to loans taken over the year preceding the endline, which is two years post-treatment. In this light, it is not so surprising

[12] The bias of the linear probability model depends positively on the proportion of predicted probabilities outside the unit interval (Horrace and Oaxaca, 2006); in the regressions of Table 2, these proportion are 17.2% (column (2)) and 33.2% (column (4)). Probit regression estimates reported in Table 18 in the online Appendix point to the same conclusions as the estimates of Table 2.

[13] It violates the *Contractive Undesirability* axiom (Gerasimou, 2018), which states that if no option is chosen when offered a menu A, then no option is chosen when offered a menu B that is a subset of menu A.

that no statistical effects are found on the total number of loans or the total amount borrowed (columns (11)-(14)). Neither do we detect a statistical significant effect on borrowing from MFIs, except an 8% marginally statistically significant increase in the contract of choice villages (column (2)). Similar results are obtained when controlling for baseline outcomes (Table 19 in the online Appendix). This suggests more deliberation in the borrowing decision and/or a better fit of the contracted loan product in the choice villages, as they had lowest take-up but disproportionately repeat-borrow from MFIs[14]. The credit expansion crowded out borrowing from moneylenders, especially in the contract of choice villages, where the reduction in demand for monyelender loans matches the increase in demand for MFI loans (columns (1)-(2), (5)-(6)).

[14]We lack data to identify these potential mechanisms.

Table 3: ITT on loans taken during the year preceding the endline survey.

Panel A	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
(any contract)	$\geq$ 1 MFI loan		# of MFI loans		$\geq$ 1 loan from moneylender		$\geq$ 1 loan from friend/relative		$\geq$ 1 any other informal loan		# loans (any)		Amount borrowed (1000 Taka)[a]	
Treatment	0.0422	0.0401	0.0471	0.0405	-0.0596	-0.0614	-0.00517	0.00373	-0.0215	-0.0148	-0.0959	-0.0799	0.192	0.0574
	(0.0437)	(0.0320)	(0.0714)	(0.0499)	(0.0295)	(0.0261)	(0.0411)	(0.0316)	(0.0286)	(0.0243)	(0.125)	(0.0802)	(1.713)	(1.333)
R^2	0.002	0.134	0.001	0.164	0.005	0.073	0.000	0.115	0.001	0.067	0.001	0.183	0.000	0.130

Panel B	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
(by contract type)	$\geq$ 1 MFI loan		# of MFI loans		$\geq$ 1 loan from moneylender		$\geq$ 1 loan from friend/relative		$\geq$ 1 any other informal loan		# loans (any)		Amount borrowed (1000 Taka)[a]	
Standard	0.00376	0.0112	0.0344	0.0273	-0.0483	-0.0458	-0.00915	-0.00294	-0.0318	-0.0245	-0.100	-0.0859	1.682	1.366
	(0.0483)	(0.0414)	(0.0901)	(0.0597)	(0.0418)	(0.0357)	(0.0546)	(0.0380)	(0.0350)	(0.0312)	(0.171)	(0.101)	(2.302)	(1.572)
Grace period	0.0185	0.0270	0.00780	0.0121	-0.0431	-0.0491	0.00270	0.0137	-0.0280	-0.0162	-0.179	-0.145	-1.046	-0.945
	(0.0478)	(0.0411)	(0.0878)	(0.0629)	(0.0429)	(0.0354)	(0.0486)	(0.0417)	(0.0325)	(0.0270)	(0.151)	(0.113)	(2.155)	(1.711)
Choice	0.0585	0.0795	0.0965	0.0796	-0.0859	-0.0874	-0.00899	0.000601	-0.00560	-0.00439	-0.0122	-0.0125	-0.0153	-0.220
	(0.0467)	(0.0451)	(0.0968)	(0.0742)	(0.0347)	(0.0305)	(0.0535)	(0.0377)	(0.0373)	(0.0316)	(0.176)	(0.113)	(2.312)	(1.983)
F-test equality of the	0.66	0.98	0.39	0.36	0.61	0.88	0.03	0.08	0.28	0.18	0.41	0.48	0.60	0.89
3 contracts {p-value}	{0.517}	{0.378}	{0.677}	{0.699}	{0.546}	{0.416}	{0.967}	{0.919}	{0.753}	{0.831}	{0.667}	{0.623}	{0.552}	{0.413}
Observations	2,803	1,406	1,406	1,406	1,406	1,406	1,406	1,406	1,406	1,406	1,406	1,406	1,406	1,406
R^2	0.002	0.137	0.002	0.165	0.006	0.075	0.000	0.115	0.002	0.067	0.002	0.184	0.002	0.131
Control group mean	0.541	0.541	0.719	0.719	0.249	0.249	0.483	0.483	0.150	0.150	2.021	2.021	19.112	19.112
Controls		✓		✓		✓		✓		✓		✓		✓

Robust standard errors clustered at the village level in parentheses. [a]The amount borrowed is winsorized at the 99^{th} percentile to ameliorate the undue influence of outlying observations. The set of controls includes gender, years of education and religion (Muslim indicator) of the head of the household, household size, average age of household members, land ownings, (non-land) asset index, head of household's primary occupation category indicators, and district indicators.

4.2 Aggregate effects of credit access on agricultural technology adoption

On average, credit does not affect the adoption of agricultural technologies (Table 4). If anything, villages assigned to contracts of choice see a reduction in the land area to which technologies are applied (column (11)-(12)). The same holds when controlling for baseline outcomes (Table 20 in the online Appendix), and when weighing observations by the propensity of attrition (Table 21 in the online Appendix).

Risk-reducing technologies such as mechanized irrigation may allow farmers to increase productivity by crowding in other inputs and cultivation methods (Emerick et al., 2016). Hence, the impact of credit access on their adoption may differ from the effects of credit access on the adoption of technologies that increase expected yield but have negative returns in case of a bad harvest (Dercon and Christiaensen, 2011). We therefore also estimate the ITT for each of the three technologies (hybrid seeds, chemical fertilizers, mechanized irrigation) individually, and calculate the associated minimum detectable effect size (MDE). Results show that the null of no impact of credit access cannot be reject for either of the three technologies (hybrid seeds, chemical fertilizers, mechanized irrigation), despite power to detect, at 5% statistical significance levels, effects on the adoption propensity as small as 0.7 percentage points (Table 22).

As discussed in the Introduction, other RCTs of credit supply expansions did find positive and statistically significant ITT effects on input use and agricultural technology adoption in Mali and Ethiopia (Beaman et al., 2020; Tarozzi et al., 2015). This discrepancy may be due to at least two differences between their study contexts and ours. First, the farmers in Ethiopia owned more than 1 hectare of land and those in Mali more than 2 hectares, whereas the land holdings of the farmers in our sample merely averages 0.05 hectares. Second, the level of credit penetration in our study is relatively high, with 45% of households already having received a loan from an MFI in the year leading up to our baseline survey, compared to only 3% of baseline households who borrowed from a bank or MFI at baseline in Ethiopia. In Tanzania, Nakano and Magezi (2020) also found no effects of credit on agricultural technology adoption.

The local average treatment effects estimates are reported in Table 23 in the online appendix. We fail to reject the null that credit had no effect on agricultural technology adoption among the

Table 4: ITT of credit access on agricultural technology adoption.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	$\geq$ 1 technology adopted		# technologies adopted		Land area on which technologies applied		$\geq$ 1 technology adopted		# technologies adopted		Land area on which technologies applied	
Treatment	-0.0278	-0.0204	-0.178	-0.133	-0.0346	-0.0256						
	(0.0370)	(0.0283)	(0.232)	(0.156)	(0.0244)	(0.0176)						
Contract:												
Standard							-0.0469	-0.0115	-0.223	-0.0270	-0.0220	-0.000691
							(0.0437)	(0.0354)	(0.296)	(0.202)	(0.0322)	(0.0225)
Grace period							-0.00912	-0.0149	-0.101	-0.139	-0.0309	-0.0292
							(0.0482)	(0.0366)	(0.291)	(0.193)	(0.0293)	(0.0231)
Choice							-0.0277	-0.0338	-0.209	-0.227	-0.0499	-0.0456
							(0.0493)	(0.0372)	(0.293)	(0.192)	(0.0292)	(0.0204)
F-test equality of the 3 contracts {p-value}							0.31	0.18	0.09	0.45	0.42	2.04
							{0.736}	{0.835}	{0.911}	{0.639}	{0.655}	{0.134}
Observations	1,406	1,406	1,406	1,406	1,406	1,406	1,406	1,406	1,406	1,406	1,406	1,406
R^2	0.001	0.215	0.001	0.279	0.002	0.322	0.001	0.215	0.001	0.279	0.003	0.324
Control group mean	0.511	0.511	2.539	2.539	0.227	0.227	0.511	0.511	2.539	2.539	0.227	0.227
Controls^b		✓		✓		✓		✓		✓		✓

Robust standard errors clustered at the village level in parentheses. The total land area on which technologies are applied is winsorized at the 99th percentile to ameliorate the undue influence of outlying observations. The set of controls includes gender, years of education and religion (Muslim indicator) of the head of the household, household size, average age of household members, land ownings, (non-land) asset index, head of household occupation category indicators, and district indicators.

farmers who were induced to borrow.[15] Table 5 shows that the improved access to credit had no discernible aggregate impact on agricultural output or profits either.[16]

Table 5: ITT of credit access on agricultural output and profits (in 1000 Taka).

	Agricultural output		Agricultural profits		Agricultural output		Agricultural profits	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	-1.935	-1.412	-1.222	-0.936				
	(1.846)	(1.396)	(1.266)	(1.000)				
Contract:								
Standard					-2.500	-0.720	-1.955	-0.828
					(2.385)	(1.678)	(1.584)	(1.159)
Grace period					-0.816	-1.232	-0.375	-0.764
					(2.317)	(1.855)	(1.678)	(1.350)
Choice					-2.478	-2.227	-1.347	-1.198
					(2.283)	(1.662)	(1.630)	(1.235)
F-test equality of the 3 contracts {p-value}					0.31	0.39	0.39	0.06
					{0.732}	{0.677}	{0.675}	{0.939}
Observations	1,406	1,406	1,406	1,406	1,406	1,406	1,406	1,406
R^2	0.001	0.330	0.001	0.297	0.002	0.331	0.002	0.297
Control group mean	15.53	15.53	9.25	9.25	15.53	15.53	9.25	9.25

Robust standard errors clustered at the village level in parentheses. Both agricultural output and agricultural profits are winsorized at their 99^{th} percentile to ameliorate the undue influence of outlying observations. The set of controls includes gender, years of education and religion (Muslim indicator) of the head of the household, household size, average age of household members, land ownings, (non-land) asset index, head of household occupation category indicators, and district indicators.

4.3 Treatment effect heterogeneity

As a first step in looking beyond the average effects, we run causal forests to evaluate the relative importance of covariates according to their ability to predict treatment effect heterogeneity. Causal forests are an ensemble method in that they average predictions of treatment effect heterogeneity made by individual trees (Athey and Imbens, 2016; Athey et al., 2019). We select the tuning parameters (such as minimum node size) through cross-validation. Results, reported in the online Appendix (Figures 3-5), show that land ownings and wealth are important effect moderators, as is

[15] Given the lack of ITT on agricultural technology adoption, we do not estimate spillover effects.

[16] There may be complementarities between some of the technologies in the production function (Suri, 2011). However, the primary focus of this article is on technology adoption (not on estimating the production function), and we do not find discernible average effects on the number of technologies used.

average age of the household members. Conditional ITT estimates on these 3 variables are shown in Figures 6-8 in the online Appendix. For households that are not wealthy and for households that own very little land, the effect of credit access on technology adoption is negative. Present bias and risk aversion are moderately important in the ranking of covariates.

Next, we analyze treatment effect heterogeneity by risk preferences. In Table 7, we show results for risk aversion, where a farmers is categorized as risk averse if (s)he chooses gamble 1, 2, 3 or 4 (see Table 14 in the online Appendix). There is no statistically significant treatment effect heterogeneity along this dimension, except for the standard contract (columns (7)-(10)). For risk averse individuals, assignment to the standard contract reduces agricultural technology adoption, which is not surprising as what we measure are instantaneous risk preferences. Statistical significance is lost when controlling for baseline outcomes (Table 24 in the online Appendix). When categorizing risk aversion as an ordinal variable (=0 if gamble 6 is chosen, =1 if gamble 5 is chosen, etcetera, =5 if gamble 1 is chosen, so that higher numbers indicate stronger risk aversion), the estimates without controls return negative coefficients on the interaction of credit access and risk aversion (for the standard and choice contracts), but their statistical significance does not survive the inclusion of controls (Table 26 in the online Appendix).

In Table 27 in the online Appendix, we address the multiple inference problem, by first - in the spirit of Anderson (2008), constructing an empirically weighted technology adoption index as outcome[17], to reduce the number of hypotheses to be tested. We then use the procedure by List et al. (2019) which asymptotically controls the familywise error rate (FWER). All coefficients lose their statistical significance.

[17]Since the outcomes are a mixture of categorical and continuous variables, we use polychoric principal component analysis (PCA) to construct the weighted index (Kolenikov and Angeles, 2009), based on the factor loadings on the first principal component. The variables used in its construction are the 8 binary technology adoption indicators for each technology under consideration, as well as the total land area to which technologies are applied.

Table 6: Heterogeneity in ITT of credit access on agricultural technology adoption by risk preferences.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	$\geq$ 1 technology adopted		# technologies adopted		Land area on which technologies applied		$\geq$ 1 technology adopted		# technologies adopted		Land area on which technologies applied	
Treatment	0.000313	-0.0144	-0.226	-0.228	-0.0308	-0.0368						
	(0.0584)	(0.0536)	(0.345)	(0.292)	(0.0381)	(0.0349)						
Risk averse (RA)	0.0244	-0.00226	-0.0323	-0.0753	0.00916	-0.00749	0.0244	-0.00249	-0.0323	-0.0770	0.00916	-7.572
	(0.0580)	(0.0515)	(0.341)	(0.278)	(0.0446)	(0.0388)	(0.0581)	(0.0515)	(0.342)	(0.278)	(0.0447)	(38.96)
Treatment × RA	-0.0772	-0.0573	-0.177	-0.131	-2.230	18.20						
	(0.0622)	(0.0535)	(0.356)	(0.296)	(40.25)	(34.06)						
Standard							0.0690	0.0447	0.248	0.227	0.00103	-14.08
							(0.0739)	(0.0734)	(0.479)	(0.418)	(0.0441)	(39.50)
Grace period							-0.0475	-0.0228	-0.532	-0.363	-0.0519	-25.02
							(0.0714)	(0.0661)	(0.385)	(0.329)	(0.0444)	(43.01)
Choice							-0.0183	-0.0559	-0.374	-0.493	-0.0403	-65.30
							(0.0750)	(0.0635)	(0.408)	(0.328)	(0.0477)	(44.01)
Standard × RA							-0.182	-0.119	-0.731	-0.604	-0.0301	6.038
							(0.0857)	(0.0806)	(0.506)	(0.448)	(0.0584)	(53.43)
Grace period × RA							0.0148	0.0112	0.426	0.318	0.0135	7.710
							(0.0903)	(0.0815)	(0.455)	(0.406)	(0.0548)	(50.62)
Choice × RA							-0.0258	0.0140	0.235	0.301	0.0283	62.75
							(0.0995)	(0.0850)	(0.542)	(0.452)	(0.0650)	(55.09)
F-test {p-value}	1.76	1.25	0.67	0.96	0.61	0.17						
Treat + Treat × RA = 0	{0.187}	{0.266}	{0.416}	{0.329}	{0.438}	{0.685}						
Observations	915	914	915	914	915	914	915	914	915	914	915	914
R^2	0.002	0.241	0.002	0.300	0.002	0.361	0.007	0.244	0.008	0.305	0.004	0.363
Control group mean	0.506	0.506	2.513	2.513	0.214	0.214	0.506	0.506	2.513	2.513	0.214	0.214
Controls		✓		✓		✓		✓		✓		✓

Robust standard errors clustered at the village level in parentheses. The total land area on which technologies are applied is winsorized at the 99^{th} percentile to ameliorate the undue influence of outlying observations. The set of controls includes gender, years of education religion (Muslim indicator) and primary occupation of the head of the household (for categories see Table 1), household size, average age of household members, land ownings, (non-land) asset index, agriculture's share of household income, head of household occupation category indicators, and district indicators, as well as the gender, age, years of education, and primary occupation of the respondent.

The effect of credit on technology adoption is heterogeneous with respect to the time consistency of preferences (Table 7). Unless specified otherwise, present-bias in this paper means strong present-bias, as per the definition in Figure 2 in the online Appendix. Credit supply expansion has a marginally statistically significant and positive effect on using $\geq$1 agricultural technology and on total land area to which any of the technologies are applied for the subpopulation of households with present-biased respondents (F-tests of columns (1), (2), (5) and (6)). Columns (7)-(12) show that this heterogeneity is driven by the standard contract villages and the contract of choice villages. The results replicate when controlling for baseline outcomes (Table 29 in the online Appendix), when weighing by the the propensity of attrition (Table 30 in the online Appendix), or when using an ordinal measure of present-bias (Table 26 in the online Appendix).[18] In Table 32 in the online Appendix, we correct for multiple inference, and the interaction terms mostly retain statistical significance.

As a result, the improved credit access increases yields and profits among present-biased farmers, but not among farmers with time-consistent preferences (Table 8).[19] Here, there are no clear differences between the contract types.

[18]The ordinal measure takes on 1 if the respondent is moderately present-biased (is one place to the left/below of the diagonal in Figure 2, takes on 2 if the respondent is strongly present biased (two places to the left/below in Figure 2), and takes on 0 otherwise.

[19]Given output market imperfections (e.g., limits to storage and marketability), and given that relaxing credit constraints allows farmers to reallocate inputs and investments, profits are a more relevant outcome than yields (Macours, 2019; Michler et al., 2019).

Table 7: Heterogeneity in ITT of credit access on agricultural technology adoption by present-biasedness.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	≥ 1 technology adopted		# technologies adopted		Land area on which technologies applied		≥ 1 technology adopted		# technologies adopted		Land area on which technologies applied	
Treatment	-0.0777	-0.0699	-0.432	-0.418	-0.0631	-0.0529						
	(0.0448)	(0.0374)	(0.282)	(0.202)	(0.0302)	(0.0233)						
Present-biased (PB)	-0.0889	-0.0741	-0.380	-0.368	-0.0803	-0.0637	-0.0889	-0.0737	-0.380	-0.367	-0.0803	-0.0639
	(0.0721)	(0.0659)	(0.407)	(0.382)	(0.0344)	(0.0315)	(0.0722)	(0.0660)	(0.408)	(0.383)	(0.0345)	(0.0316)
Treatment × PB	0.205	0.194	0.987	1.008	0.172	0.155						
	(0.0883)	(0.0790)	(0.488)	(0.445)	(0.0487)	(0.0413)						
Contract:												
Standard							-0.0806	-0.0681	-0.414	-0.385	-0.0529	-0.0443
							(0.0593)	(0.0523)	(0.376)	(0.287)	(0.0416)	(0.0316)
Grace period							-0.0603	-0.0365	-0.352	-0.254	-0.0687	-0.0463
							(0.0552)	(0.0445)	(0.337)	(0.225)	(0.0328)	(0.0239)
Choice							-0.0958	-0.112	-0.549	-0.652	-0.0674	-0.0696
							(0.0587)	(0.0468)	(0.346)	(0.250)	(0.0358)	(0.0293)
Standard × PB							0.206	0.203	1.132	1.224	0.178	0.163
							(0.113)	(0.101)	(0.588)	(0.511)	(0.0720)	(0.0483)
Grace period × PB							0.125	0.100	0.369	0.307	0.160	0.161
							(0.143)	(0.145)	(0.730)	(0.796)	(0.0873)	(0.0947)
Choice × PB							0.250	0.252	1.196	1.275	0.172	0.155
							(0.104)	(0.0880)	(0.576)	(0.501)	(0.0601)	(0.0507)
F-test {p-value}:	2.80	3.23	1.66	2.17	7.14	9.09						
Treat + Treat×PB = 0	{0.096}	{0.075}	{0.199}	{0.142}	{0.008}	{0.003}						
Observations	915	914	915	914	915	914	915	914	915	914	915	914
R^2	0.009	0.247	0.008	0.307	0.014	0.371	0.010	0.250	0.010	0.310	0.015	0.372
Control group mean	0.506	0.506	2.549	2.549	15.510	15.510	0.506	0.506	2.549	2.549	15.510	15.510
Controls[b]		✓		✓		✓		✓		✓		✓

Robust standard errors clustered at the village level in parentheses. The total land area on which technologies are applied is winsorized at the 99^{th} percentile to ameliorate the undue influence of outlying observations. The set of controls includes gender, years of education religion (Muslim indicator) and primary occupation of the head of the household (for categories see Table 1), household size, average age of household members, land ownings, (non-land) asset index, agriculture's share of household income, head of household occupation category indicators, and district indicators, as well as the gender, age, years of education, and primary occupation of the respondent.

Table 8: ITT of credit access on agricultural output and profits (in 1000 Taka), by present-bias.

	Agricultural output		Agricultural profits		Agricultural output		Agricultural profits	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	-4.606	-3.482	-3.135	-2.313				
	(2.444)	(1.836)	(1.785)	(1.359)				
Present-biased (PB)	-5.946	-5.716	-3.490	-3.347	-5.946	-5.724	-3.490	-3.349
	(3.291)	(2.988)	(2.444)	(2.174)	(3.298)	(2.996)	(2.449)	(2.179)
Treatment × PB	11.19	10.75	6.170	5.770				
	(4.062)	(3.451)	(3.030)	(2.571)				
Contract:								
Standard					-5.883	-3.842	-4.425	-2.827
					(2.928)	(2.116)	(2.005)	(1.525)
Grace period					-3.140	-2.349	-1.828	-1.433
					(2.873)	(2.204)	(2.226)	(1.698)
Choice					-5.020	-4.449	-3.341	-2.821
					(2.728)	(2.033)	(1.964)	(1.483)
Standard × PB					13.48	12.07	8.302	7.206
					(5.973)	(4.319)	(4.514)	(3.621)
Grace period × PB					8.337	12.76	3.005	6.337
					(5.134)	(5.741)	(3.090)	(3.224)
Choice × PB					11.40	9.790	6.488	5.053
					(4.623)	(3.731)	(3.431)	(2.821)
F-test {p-value}:	4.29	8.58	1.65	3.43				
Treat + Treat×PB = 0	{0.040}	{0.004}	{0.201}	{0.066}				
Observations	915	914	915	914	915	914	915	914
R-squared	0.011	0.369	0.008	0.318	0.013	0.371	0.011	0.320
Control group mean	16.424	16.424	10.148	10.148	16.424	16.424	10.148	10.148

Robust standard errors clustered at the village level in parentheses. Both agricultural output and agricultural profits are winsorized at their 99^{th} percentile to ameliorate the undue influence of outlying observations. The set of controls includes gender, years of education religion (Muslim indicator) and primary occupation of the head of the household (for categories see Table 1), household size, average age of household members, land ownings, (non-land) asset index, agriculture's share of household income, head of household occupation category indicators, and district indicators, as well as the gender, age, years of education, and primary occupation of the respondent.

4.4 Time inconsistent preferences and demand for commitment

Within the villages where subjects could choose between the standard contract and the grace period contract, does present-bias predict the choice of contract type? Table 9 shows it does: compared to individuals with time-consistent preferences, present-biased individuals have statistically significantly lower demand for the loan contract with a grace period (and comparatively higher demand for the standard contract, though not statistically significantly). O'Donoghue and Rabin (1999); DellaVigna and Malmendier (2006) refer to individuals who are fully aware of their present-bias "sophisticated", and individuals who are unaware of their present-bias "naive". It can be argued that among present-biased farmers, only sophisticates (but not naives), disproportionately select into the standard contract.[20]

A very rough back-off the envelope calculation using the midpoint between the coefficients of column (6) and (8) of Table 9 (plus-minus their standard error) gives a proportion of sophisticates among the present-biased of 21-27%[21]. This is an extremely rough estimate, and is lower than what has been found in several lab experiments. For instance, in an internet-based survey of 2,386 Japanese adults, Kang and Ikeda (2016) found 40.0% of present-biased respondents to be sophisticates (according to the difference between their planned and actual behavior with respect to their imposed effort tasks). Among their smaller sample of 162 university students in a lab experiment in the UK, Cerrone and Lades (2017) found 31 out of 49 present-biased people (63.3%) to be sophisticates.

In considering the implications of lenders screening potential borrowers on present-bias (see the discussion in Section 5), it is worth exploring possible correlates of present-bias. None of the covariates appear to be statistically significant correlates of present-bias in our sample, alleviating concerns that such screening would lead to discrimination on variables such as gender, religion, or age (Table 33 in the online Appendix).

[20] A caveat is that Laibson (2015) showed that for sophisticated present-bias to imply demand for commitment, the perceived benefit from committing would need to exceed the perceived cost from reduced flexibility (or, in our case, the absence of a grace period). This would affect inference of sophistication from demand for commitment among present-biased farmers if sophistication about one's present-bias were to be correlated with the cost of not having a grace period (which in turn could be affected by the gestation period of loan-induced investments, some of which may be non-agricultural).

[21] Averaging the coefficients of columns (6) and (8) gives a share of $\frac{(6.18+0.72)/2}{14.52} = 23.76\%$ plus-minus a standard error of about 3%

Table 9: Take-up, take-up of the standard contract, and take-up of the grace period contract, by present-bias.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample:	All treatment villages		All treatment villages		Grace period & choice contract villages		Standard & choice contract villages	
Dependent variable:	Take-up of (any) loan		Take-up of (any) loan		Take-up of grace period contract		Take-up of standard contract	
Present-biased (PB)	-0.0262	-0.0341			-0.0715	-0.0618	0.00456	0.00700
	(0.0259)	(0.0254)			(0.0252)	(0.0253)	(0.0325)	(0.0319)
Standard			0.142	0.161				
			(0.0333)	(0.0350)				
Grace period			0.242	0.254				
			(0.0411)	(0.0356)				
Choice			0.157	0.158				
			(0.0353)	(0.0327)				
Standard × PB			0.0852	0.0743				
			(0.0769)	(0.0762)				
Delayed × PB			-0.0341	-0.0362				
			(0.0749)	(0.0728)				
Choice × PB			-0.0937	-0.0927				
			(0.0481)	(0.0436)				
E(dep. var. | not PB)	0.122	0.122	0.122	0.122	0.138	0.138	0.129	0.129
Observations	1,000	999	1,000	999	562	562	560	559
R^2	0.001	0.080	0.087	0.163	0.010	0.125	0.000	0.102
Controls		✓		✓		✓		✓

The regressions are linear probability models. Robust standard errors clustered at the village level in parentheses. The outcome of columns (1) and (2) equals 1 if the household takes up any loan from RDRS (and 0 otherwise). The outcome of columns (3) and (4) equals 1 if the household takes up a loan with a grace period. The outcome of columns (5) and (6) equals 1 if the household takes up a standard contract loan without a grace period. The set of controls includes gender, years of education religion (Muslim indicator) and primary occupation of the head of the household (for categories see Table 1), household size, average age of household members, land ownings, (non-land) asset index, agriculture's share of household income, head of household occupation category indicators, and district indicators, as well as the gender, age, years of education, and primary occupation of the respondent.

4.5 Mechanisms: present-bias, credit constraints, and technology adoption

Why does credit access increase technology adoption only among present-biased individuals? Present-bias may impede the ability to accumulate the funds needed to purchase agricultural technologies through saving. While we find no statistically significant treatment effects on assets other than agricultural technology,[22] we indeed find the treatment effect on savings to be heterogeneous by present-bias (Table 10. Treatment assignment statistically significantly decreases the propensity to self-report (at endline) to have had any savings over the past year for farmers with time consistent preferences, but there is no such negative effect on savings for present-biased borrowers (columns (1)-(2)). Consistent with the effects on technology adoption, these effects on savings are driven by the standard contract and contract of choice villages (column (3)-(4)), consistent with the findings of the previous subsection on sorting into and out of commitment by time consistency. At baseline, we asked respondents which of the technologies they applied in the past year, and for technologies not used by the household, the reason for their non-adoption. Conditional on the number of technologies used, present-biased individuals cite cash constraints more often as the reason (columns (5)-(6)). Present-bias only predicts higher loan take-up for farmers who report a lack of funds as the reason for not having used technologies.

If present-biased farmers are more credit constrained, then why do they not have higher demand for credit (columns (1)-(2) in Table 9)? First, it should be borne in mind that present-bias only affects demand among the subset of present-biased farmers who are sophisticates (aware of their present-bias and acting on it). Second, the coefficient on present-biased in columns (1)-(2) of Table 9 is attenuated, in that present-biased farmers who do not report their non-usage of technologies to be due to a lack of funds are *less* likely to take up credit than farmers with time consistent preferences (Table 10, columns (7)-(8)).[23]. Third, non-uptake due to choice deferral in the contract choice villages seems to be especially pertinent among present-biased farmers (columns (3)-(4) in Table 9). The drop in demand in contract of choice villages for present-biased farmers relative to their demand for their (on average) preferred, standard contract (6.3% vs. 22.7%), exceeds the

[22] Also, there was no statistically significant difference in the asset index between present-biased and time consistent farmers at baseline (Table 17 in the online Appendix), nor, on the propensity at baseline to report having had any savings in the past year (available on request).

[23] This could for instance be due to their anticipated difficulty, due to their present-bias, of meeting repayment obligations

drop in demand in contract of choice villages for time consistent farmers relative to their demand for their (on average) preferred, grace period contract (15.7% vs. 24.2%).[24]

[24]These numbers are the differences in coefficients on standard contract and contract of choice of two regressions of takeup on the three contract types (without a constant): one regression on the time-consistent subsample and one regression on the present-biased subsample (available on request).

Table 10: Exploratory regressions related to present-bias, credit constraints and technology adoption.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
					$\geq$ 1 technology	# of technologies		
	Asset index (endline)		Household had any savings in past year (endline)		for which lack of funds cited as reason for not adopting at baseline		Treatment (loan) take-up	
Present-biased (PB)	-0.155	-0.154	-0.0261	-0.0259	0.0514	0.147	-0.0413	-0.0451
	(0.116)	(0.116)	(0.0636)	(0.0638)	(0.0296)	(0.0744)	(0.0232)	(0.0222)
Treatment	-0.164		-0.0936					
	(0.0901)		(0.0350)					
Standard contract		-0.245		-0.104				
		(0.114)		(0.0496)				
Grace period		-0.139		-0.0727				
		(0.111)		(0.0477)				
Contract of choice		-0.107		-0.108				
		(0.124)		(0.0419)				
Treatment × PB	0.298		0.0772					
	(0.175)		(0.0780)					
Standard × PB		0.352		0.0913				
		(0.239)		(0.110)				
Grace period × PB		0.411		0.0142				
		(0.225)		(0.116)				
Choice × PB		0.206		0.106				
		(0.258)		(0.0903)				
$\geq$ 1 technology constrained							-0.0437	
							(0.0183)	
PB × $\geq$ 1 technology constrained							0.0918	
							(0.0508)	
# of technologies constrained								-0.0195
								(0.00716)
PB × # technologies constrained								0.0603
								(0.0225)
Observations	914	914	914	914	1,067	1,067	1,067	1,067
R^2	0.548	0.549	0.159	0.160	0.171	0.091	0.071	0.075

Robust standard errors clustered at the village level in parentheses. The set of controls includes gender, years of education religion (Muslim indicator) and primary occupation of the head of the household (for categories see Table 1), household size, average age of household members, land ownings, (non-land) asset index, agriculture's share of household income, head of household occupation category indicators, and district indicators, as well as the gender, age, years of education, and primary occupation of the respondent.

5 Conclusion

This study investigated how smallholder farmers' demand for credit and the impact of credit access on their agricultural technology adoption varies by (i) their time (in)consistency, and (ii) loan contract structure, particularly, a grace period. The null of no aggregate effect of credit access on agricultural technology adoption cannot be rejected in our sample. This stands in contrast to other studies' findings of increases in agricultural technology adoption in Sub-Saharan Africa, where both credit penetration and technology adoption, are lower. However, we find that access to credit does enhance technology adoption among present-biased farmers. This effect is driven by the villages supplied with the standard contract and the villages where farmers can choose between the standard contract and the grace period contract.

There is evidence of sorting into loan contract structure by time consistency: time consistent farmers sort into the grace period contract, and present-biased farmers select into the standard contract. The standard contract offers a commitment device by demanding repayment from the time of disbursement, which should be helpful for present-bias farmers. The sorting suggests that at least some of the present-biased farmers are sophisticated in that they are aware of their bias and act on it. This experimental evidence is in line with the correlation between present-bias and having (traditional, frequently repaying) microcredit observed by Bauer et al. (2012) in India, and with present-biased individuals in the Philippines having higher demand for a commitment savings product (Ashraf et al., 2006). While the portion of income spent on temptation goods is lower for the wealthy (Banerjee and Mullainathan, 2010), the extent of sorting into commitment by present-bias has implications for policies in wealthier countries as well. Governments, acting in a paternalistic fashion, routinely offer commitment devices without a choice to opt-out of them.[25] Our results indicate that at least some of the present-biased people who benefit from commitment would voluntarily opt into it when given the choice.

The policy implication of our findings for MFIs is to offer a flexible choice of contract structures to tailor behavioral profiles. However, lower demand in the contract of choice villages compared

[25] Liu et al. (2020) reviewed such policies, which include restrictions (or bans) on early withdrawals from defined contribution (DC) retirement schemes (Beshears et al., 2015). A germane example is the U.K. where, up until recently, residents on money-purchase pension schemes were forced to take an annuity — the income guaranteed by pension providers in exchange for receiving all or part of the funds in their pension pot. Additionally, a 55% tax rate was imposed on anyone who took out more than 25% of the savings in their pension pot. In the U.S., retirement savings accounts are partially illiquid: withdrawals before age 59 incur a 10% tax penalty.

to assignment to the (on average) preferred contract for both time consistent and present-biased farmers, reveals the importance of carefully designing the choice environment. For instance, the standard contract could be presented as the default option. In addition, to tailor product advice to present-biased individuals, including those who are naive about their present-bias, MFIs may apply behavioral screening of time consistency through hypothetical choices as in (Klinger et al., 2013) or through the analysis of mobile phone data (Björkegren and Grissen, 2018).

Several studies in other countries imply the timing in the agricultural cycle of taking up and repaying credit to matter (Burke et al., 2019; Fink et al., 2018). Our observed selection of present-biased farmers into the standard contract indicates that for at least a subset of farmers in the population, the alignment of the grace period with the gestation period of agricultural technology investments is less important than the ability to save through the commitment embedded in the standard contracts. To improve on this potential trade-off, a menu of contracts may include the option to repay smaller amounts initially (a 'semi-grace period'), with larger repayments due beginning at the time of harvest (or more generally, when investment returns are expected to be realized and the ability to save increases). Among other avenues worth exploring to increase agricultural technology adoption are farmer field days (Emerick and Dar, 2020), linking credit supply with nudges such as time-limited purchase discounts (Duflo et al., 2011), rainfall index, crop price or indemnity insurance (Giné and Yang, 2009; Karlan et al., 2011; Farrin and Miranda, 2015; Cole et al., 2017; Bulte et al., 2019; Ceballos and Kramer, 2019) and (traditional or digital) information or extension services (Cole and Fernando, 2016; Gupta et al., 2020), possibly exploiting network effects to enhance the diffusion of information (Beaman et al., 2018).

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6 Data Appendix

Outcome variables

The agricultural technologies recorded are:

- Hybrid seeds
- Chemical fertilizer (Urea)
- Chemical fertilizer (TSP)
- Chemical fertilizer (MOP)
- Chemical fertilizer (Zipsum)
- Chemical fertilizer (Zinc)
- Chemical fertilizer (NPKS)
- Mechanized irrigation (using either water from own source or purchased)

Hence, the *# of technologies adopted* variable takes on values between 0 and 8.

Script used for the elicitation of time preferences

[The order of elicitation (time preferences first or risk preferences first) was randomized). Within the time preferences elicitation, the order of the three sets of choices is also randomized.]

Instructions: You have a chance to receive additional sum of money. If you are selected to receive this sum of money, you need to make a choice between two payment options: Option A or Option B. There are three sets of choices and in each set, there are six choices. You have a 1-in-2 chance of receiving the money. The selection will be done using a six sided dice twice – first to decide the set, and the second to decide the choice. If 1, 2 or 3 are drawn, you will receive the money from the particular choice set, if 4, 5 or 6 are drawn, you will not receive any money. Depending on the outcome of the first draw, the second draw will determine the particular choice that you will be paid for.

In each choice set, there are six decisions. Each decision is a paired choice between Option A and Option B. You will be asked to make a choice between these two payment options in each decision row. For example, in the first row, you need to make a choice between payment option A and payment option B where payment option A pays you Taka 100 tomorrow and option B pays you Taka 105 after three months from today. Would you prefer to receive Taka 100 guaranteed tomorrow or Taka 100 + Taka X guaranteed in x months from today?

In the table, there is column labeled "annual interest rate". To explain this, let us consider the following payoff alternative (decision row 4 in the table):

Option A pays Taka 100 tomorrow. Option B pays Taka 125 after three months from today.

In this example, if you choose option B, you will earn an annual interest rate of 100%. Notice that annual interest rate is increasing in option B for each successive choice.

We are working with a local NGO RDRS who will make the payment.

Script used for the elicitation of risk preferences

Instructions: You have a chance to receive additional sum of money. If you are selected to receive this sum of money, you need to select from among six different gambles the one gamble you would like to play. The six different gambles are listed below. You must select one and only one of these gambles.

You have a 1-in-12 chance of receiving the money. The selection will be done using a six sided dice twice – first to decide the gamble, and the second to decide the outcome. Depending on the outcome of the first draw, the second draw will determine the outcome of the selected gamble. If 1, 2 or 3 are drawn, the outcome of the selected gamble is the low one, and if 4, 5 or 6 are drawn, the outcome of the gamble is the high one, and you will receive money accordingly. For example, if you have selected gamble 4, and the first draw of the dice is 4, you will receive one of the payoffs of gamble 4, which will be determined in the second draw.

Each gamble has two possible outcomes – low and high. Notice that low outcome is decreasing and the high outcome is increasing for each successive gamble. For example, in the first gamble, both outcomes are identical. If you select it and if you this number is drawn, your payoff will be 125 Taka. If on the other hand, you select gamble 2, and if it is drawn, your payoff could be 110 Taka or 240 Taka.

Table 11: Choice set 1

Payoff alternative	Payment Option A (pays amount below tomorrow)	Payment Option B (pays amount below after 3 months)	Annual interest rate	Preferred Payment Option (A or B)
1	100	105	20%	
2	100	110	40%	
3	100	120	80%	
4	100	125	100%	
5	100	150	200%	
6	100	200	400%	

Table 12: Choice set 2

Payoff alternative	Payment Option A (pays amount below after 1 month)	Payment Option B (pays amount below after 4 months)	Annual interest rate	Preferred Payment Option (A or B)
1	100	105	20%	
2	100	110	40%	
3	100	120	80%	
4	100	125	100%	
5	100	150	200%	
6	100	200	400%	

Table 13: Choice set 3

Payoff alternative	Payment Option A (pays amount below after 1 year)	Payment Option B (pays amount below after 1 year, 3 months)	Annual interest rate	Preferred Payment Option (A or B)
1	100	105	20%	
2	100	110	40%	
3	100	120	80%	
4	100	125	100%	
5	100	150	200%	
6	100	200	400%	

Table 14: Mark the gamble you select with an X in the last column (mark only one). [Note: the last column shows the sample distribution of responses.]

	Outcome	Payoff	Chances	Your selection
Gamble 1	Low	125	50%	8.2%
	High	125	50%	
Gamble 2	Low	110	50%	13.3%
	High	240	50%	
Gamble 3	Low	100	50%	19.0%
	High	300	50%	
Gamble 4	Low	75	50%	17.9%
	High	375	50%	
Gamble 5	Low	25	50%	26.9%
	High	475	50%	
Gamble 6	Low	0	50%	14.6%
	High	500	50%	

7 Table Appendix

Table 15: Balance tests for the households that responded to the endline survey.

Credit variables	Full sample (n=1406)	Control villages (n=466)	Treatment villages (n=940)	Difference Treated - Controls (p-value)[a]	Standard contract villages (n=304)	Grace period contract villages (n=311)	Contract of choice villages (n=325)	Standard = grace period = choice (p-value)[b]
Any (i.e., ≥ 1) MFI loan	0.45 (0.50)	0.45 (0.50)	0.45 (0.50)	0.969	0.47 (0.50)	0.44 (0.50)	0.45 (0.50)	0.863
# of NGO loans	0.55 (0.68)	0.53 (0.64)	0.56 (0.7)	0.558	0.56 (0.67)	0.53 (0.67)	0.59 (0.76)	0.819
≥ 1 moneylender loan	0.16 (0.36)	0.18 (0.38)	0.15 (0.36)	0.27	0.17 (0.37)	0.16 (0.37)	0.12 (0.32)	0.225
≥ 1 friend/relative loan	0.41 (0.49)	0.45 (0.5)	0.4 (0.49)	0.168	0.43 (0.5)	0.4 (0.49)	0.36 (0.48)	0.289
≥ 1 other informal loan	0.09 (0.29)	0.08 (0.27)	0.09 (0.29)	0.482	0.09 (0.28)	0.09 (0.29)	0.1 (0.31)	0.873
Total # of loans	1.51 (1.24)	1.56 (1.23)	1.48 (1.24)	0.388	1.62 (1.33)	1.48 (1.22)	1.35 (1.17)	0.17
Total amount borrowed (1000 Taka)	9.59 (12.19)	9.89 (12.56)	9.44 (12.01)	0.628	10.17 (13.18)	9.07 (12.33)	9.12 (10.48)	0.584
Panel B: Outcome variables								
≥ 1 technologies adopted	0.45 (0.5)	0.45 (0.5)	0.45 (0.5)	0.934	0.43 (0.5)	0.47 (0.5)	0.45 (0.50	0.793
# of technologies adopted	2.09 (2.55)	2.17 (2.62)	2.05 (2.51)	0.576	1.97 (2.5)	2.18 (2.56)	2.01 (2.48)	0.788
Land area on which technologies are applied	1.63 (2.99)	1.82 (3.28)	1.54 (2.83)	0.191	1.58 (2.93)	1.52 (2.86)	1.53 (2.7)	0.974
Agricultural output (1000 Taka)	12.59 (22.97)	14.05 (24.72)	11.87 (22.03)	0.203	11.58 (22.84)	11.69 (21.14)	12.33 (22.15)	0.931
Agricultural profits (1000 Taka)	6.03 (14.35)	7.04 (15.94)	5.53 (13.47)	0.218	5.4 (13.78)	5.35 (11.73)	5.82 (14.72)	0.923
Panel C: Control variables								
Female head of HH	0.1 (0.3)	0.08 (0.26)	0.11 (0.32)	0.033	0.12 (0.33)	0.13 (0.34)	0.09 (0.29)	0.307
Head of HH's Education	2.02 (3.15)	1.85 (3.05)	2.11 (3.2)	0.157	1.67 (2.81)	2.32 (3.34)	2.32 (3.36)	0.012
Head of HH is Muslim	0.78 (0.41)	0.81 (0.39)	0.76 (0.42)	0.367	0.76 (0.43)	0.77 (0.42)	0.77 (0.42)	0.979
Household size	4.14 (1.33)	4.21 (1.28)	4.1 (1.36)	0.236	4.11 (1.41)	4.05 (1.37)	4.14 (1.29)	0.722
Average age	24.26 (8.18)	24.18 (8.1)	24.3 (8.23)	0.789	24.61 (8.51)	23.79 (8.02)	24.51 (8.16)	0.476
Land holdings (in decimals)	12.73 (21.75)	13.28 (22.12)	12.45 (21.56)	0.594	11.73 (19.75)	11.88 (19.96)	13.68 (24.49)	0.674
Asset index	-0.01 (1.72)	-0.05 (1.6)	0.01 (1.77)	0.647	-0.06 (1.84)	0.08 (1.8)	0.02 (1.69)	0.819
Head of HH primary occupation category 1: self-employed agriculture	0.07 (0.25)	0.08 (0.26)	0.07 (0.25)	0.611	0.06 (0.24)	0.07 (0.26)	0.06 (0.25)	0.814
Occ. cat. 2: agric. wage labor	0.44 (0.5)	0.46 (0.5)	0.44 (0.5)	0.574	0.38 (0.49)	0.48 (0.5)	0.45 (0.50)	0.189
Occ. cat. 3: fisheries	0.03 (0.17)	0.03 (0.18)	0.03 (0.16)	0.772	0.01 (0.11)	0.05 (0.23)	0.02 (0.12)	0.222
Occ. cat. 4: self-employment	0.12 (0.33)	0.12 (0.33)	0.12 (0.33)	0.961	0.15 (0.36)	0.11 (0.31)	0.12 (0.33)	0.35
Occ. cat. 5: freelancing	0.13 (0.33)	0.12 (0.33)	0.13 (0.33)	0.782	0.14 (0.35)	0.1 (0.3)	0.14 (0.35)	0.2
Occ. cat. 6: housewife	0.04 (0.19)	0.03 (0.17)	0.04 (0.2)	0.265	0.05 (0.22)	0.04 (0.2)	0.04 (0.19)	0.818
Occ. cat. 7: salaried job	0.03 (0.18)	0.03 (0.18)	0.03 (0.18)	0.905	0.05 (0.22)	0.03 (0.18)	0.02 (0.13)	0.17
Occ. cat. 8: other	0.14 (0.34)	0.12 (0.33)	0.14 (0.35)	0.365	0.15 (0.36)	0.11 (0.32)	0.16 (0.37)	0.148
					OLS, F-test: Control vs. standard			0.480
Joint orthogonality test: p-value	(OLS, based on F-test)			0.403	Control vs. grace period			0.051
					Control vs. choice			0.007
					Multinomial logit, X^2-test: all 4 arms			0.000

[a] Based on a regression of the variable listed in the leftmost column on an indicator for treatment villages, with robust standard errors clustered at the village level. [b] Regressions (with standard errors clustered at the village level) were run of the variables listed in the leftmost column on three indicators for the three contract type villages, and the p-value in the rightmost column is based on an F-test of equality of the coefficients on these three indicators.

Table 16: Balance tests of baseline village characteristics.

	All villages		Control villages		Treatment villages				Standard contract		Grace period		Contract of choice		Equality across contract types
	# of villages	Mean (sd)	# of villages	Mean (st. dev.)	# of villages	Mean (st. dev.)	Diff	p-value	# of villages	Mean (st. dev.)	# of villages	Mean (st. dev.)	# of villages	Mean (st. dev.)	(p-value)
Price of hybrid seeds (Taka)	143	220.85 (38.61)	96	219.55 (35.87)	47	223.51 (43.97)	3.959	0.592	32	222.81 (34.8)	32	218.19 (36.73)	32	217.66 (36.96)	0.815
Price of Urea fertilizer (Taka)	150	20.66 (0.73)	100	20.63 (0.73)	50	20.72 (0.73)	.09	0.478	33	20.79 (0.82)	33	20.61 (0.70)	34	20.5 (0.66)	0.29
Price of TSP fertilizer (Taka)	150	24.77 (2.77)	100	24.8 (2.81)	50	24.7 (2.7)	-.1	0.833	33	25.39 (3.41)	33	24.76 (2.78)	34	24.26 (2.08)	0.252
Price of Potash fertilizer (Taka)	150	17.97 (4.13)	100	17.85 (4.21)	50	18.2 (3.98)	.35	0.619	33	18.27 (5.19)	33	18.09 (4.21)	34	17.21 (3.04)	0.459
Price of Zipsum fertilizer (Taka)	150	10.07 (3.93)	100	10.15 (3.73)	50	9.92 (4.36)	-.23	0.749	33	10.21 (3.76)	33	10.55 (3.58)	34	9.71 (3.9)	0.654
% of irrigation pumps run with electricity	150	12.55 (21.05)	100	11.82 (20.87)	50	14 (21.54)	2.18	0.555	33	9.3 (18.41)	33	12.61 (21.03)	34	13.5 (23.24)	0.666

[a] Based on a regression of the variable listed in the leftmost column on an indicator for treatment villages, with robust standard errors clustered at the village level. [b] Regressions (with standard errors clustered at the village level) were run of the variables listed in the leftmost column on three indicators for the three contract type villages, and the p-value in the rightmost column is based on an F-test of equality of the coefficients on these three indicators.

Table 17: Predictors of being in the preference elicitation sample (outcome =1 if in preference elicitation sample).

	(1)	(2)	(3)	(4)
Treatment	-0.0154	-0.0216		
	(0.0162)	(0.0165)		
Contract:				
Standard			-0.000433	-0.0120
			(0.0192)	(0.0200)
Grace period			-0.00953	-0.0222
			(0.0213)	(0.0220)
Choice			-0.0355	-0.0303
			(0.0201)	(0.0204)
Female head of HH		0.392		0.392
		(0.0219)		(0.0221)
Head of HH's educ.		-0.00165		-0.00148
		(0.00405)		(0.00407)
Head of HH Muslim		0.0246		0.0249
		(0.0267)		(0.0267)
HH size		0.0117		0.0117
		(0.0108)		(0.0108)
Average age		-0.00165		-0.00164
		(0.00161)		(0.00161)
Land holdings		0.000745		0.000751
		(0.000631)		(0.000631)
Asset index		-0.0172		-0.0172
		(0.00793)		(0.00798)
Occ. cat. 2: agric. wage labor		0.00818		0.00912
		(0.0605)		(0.0606)
Occ. cat. 3: fisheries		0.104		0.105
		(0.0814)		(0.0823)
Occ. cat. 4: self-employment		0.00520		0.00506
		(0.0646)		(0.0643)
Occ. cat. 5: freelancing		-0.00917		-0.00892
		(0.0654)		(0.0653)
Occ. cat. 6: housewife		-0.000885		-0.000303
		(0.0634)		(0.0630)
Occ. cat. 7: salaried job		0.0210		0.0191
		(0.0895)		(0.0896)
Occ. cat. 8: other		-0.00398		-0.00310
		(0.0663)		(0.0660)
Observations	1,490	1,490	1,490	1,490
R^2	0.000	0.067	0.001	0.068
Control group mean	0.662	0.662	0.662	0.662
Controls		✓		✓

Robust standard errors clustered at the village level in parentheses. [a]The land to which technologies are applied (in 1000 decimals) is winsorized at the 99^{th} percentile to ameliorate the undue influence of outlying observations.

		Future discount rate							
		Patient DR=0.1	DR=0.1	DR=0.6	DR=0.9	DR=1.5	DR=3	Impatient DR=5	Total
Patient	DR=0.1	250 16.7%	42 2.8%	15 1.0%	3 0.2%	6 0.4%	7 0.5%	9 0.6%	332 22.1%
	DR=0.3	73 4.9%	42 2.8%	20 1.3%	1 0.1%	4 0.3%	1 0.1%	4 0.3%	145 9.7%
	DR=0.6	37 2.5%	36 2.4%	42 2.8%	23 1.5%	10 0.7%	2 0.1%	4 0.3%	154 10.3%
Current discount rate	DR=0.9	24 1.6%	21 1.4%	53 3.5%	52 3.5%	30 2.0%	1 0.1%	6 0.4%	187 12.5%
	DR=1.5	19 1.3%	5 0.3%	30 2.0%	48 3.2%	95 6.3%	13 0.9%	10 0.7%	220 14.7%
	DR=3	7 0.5%	7 0.5%	10 0.7%	17 1.1%	31 2.1%	34 2.3%	8 0.5%	114 7.6%
Impatient	DR=5	35 2.3%	12 0.8%	22 1.5%	26 1.7%	26 1.7%	13 0.9%	214 14.3%	348 23.2%
Total		445 29.7%	165 11.0%	192 12.8%	170 11.3%	202 13.5%	71 4.7%	255 17.0%	1,500 100.0%

16.9% of individuals	"Weakly present-biased": More patient over future tradeoffs than current tradeoffs (next to the diagonal)
19.9% of individuals	"Strongly present-biased": More patient over future tradeoffs than current tradeoffs (further off the diagonal)
14.6% of individuals	"Patient now, impatient later": Less patient over future tradeoffs than current tradeoffs

Figure 2: Distribution of Responses to Time Preference Questions

Table 18: Loan take-up across treatment arms: marginal effects based on probit regressions.

	(1)	(2)	(3)	(4)
	Sample: All HHs that were offered credit		Sample: offered credit & selected for endline	
Standard contract	-0.275	0.838	-0.272	0.819
	(0.0327)	(0.0610)	(0.0363)	(0.0628)
Grace period contract	-0.223	0.861	-0.231	0.840
	(0.0339)	(0.0549)	(0.0341)	(0.0588)
Contract of choice	-0.339	0.789	-0.342	0.768
	(0.0347)	(0.0551)	(0.0341)	(0.0571)
F-test equality of contracts	4.34	9.62	4.09	8.96
{p-value}	{0.114}	{0.008}	{0.129}	{0.011}
E(dep. var.)[a]	0.181	0.181	0.181	0.181
Observations	2,929	2,929	1,490	1,490
Proportion correctly classified	0.544	0.875	0.546	0.879
Controls		✓		✓

Robust standard errors clustered at the village level in parentheses. Coefficient estimates are marginal effects base on probit regressions; estimations of columns (1) and (3) are without a constant. The set of controls includes gender, years of education and religion (Muslim indicator) of the head of the household, household size, average age of household members, land ownings, (non-land) asset index, head of household's primary occupation category indicators, and district indicators. [a] Mean of loan take-up in treatment villages.

Table 19: ITT on loans taken during the year preceding the endline survey, controlling for baseline outcomes.

Panel A	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
(any contract)	≥ 1 MFI loan		# of MFI loans		≥ 1 loan from moneylender		≥ 1 loan from friend/relative		≥ 1 any other informal loan		# loans (any)		Amount borrowed (1000 Taka)[a]	
Treatment	0.0430	0.0410	0.0255	0.0242	-0.0554	-0.0589	-0.00549	0.00362	-0.0219	-0.0154	-0.0768	-0.0664	0.630	0.391
	(0.0326)	(0.0285)	(0.0483)	(0.0417)	(0.0285)	(0.0258)	(0.0412)	(0.0317)	(0.0287)	(0.0244)	(0.127)	(0.0809)	(1.393)	(1.187)
R^2	0.196	0.243	0.286	0.329	0.024	0.083	0.000	0.115	0.001	0.068	0.045	0.221	0.190	0.249

Panel B	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
(by contract type)	≥ 1 MFI loan		# of MFI loans		≥ 1 loan from moneylender		≥ 1 loan from friend/relative		≥ 1 any other informal loan		# loans (any)		Amount borrowed (1000 Taka)[a]	
Standard	0.00836	0.00713	0.0117	0.0113	-0.0470	-0.0463	-0.00922	-0.00293	-0.0319	-0.0249	-0.116	-0.113	1.478	1.141
	(0.0420)	(0.0358)	(0.0605)	(0.0503)	(0.0392)	(0.0348)	(0.0546)	(0.0380)	(0.0351)	(0.0313)	(0.172)	(0.0996)	(1.771)	(1.425)
Grace period	0.0297	0.0298	0.00484	0.00603	-0.0413	-0.0482	0.00240	0.0136	-0.0284	-0.0167	-0.160	-0.131	-0.104	-0.292
	(0.0433)	(0.0381)	(0.0615)	(0.0541)	(0.0418)	(0.0353)	(0.0487)	(0.0418)	(0.0326)	(0.0272)	(0.151)	(0.111)	(1.817)	(1.575)
Choice	0.0881	0.0833	0.0582	0.0535	-0.0768	-0.0809	-0.00955	0.000411	-0.00624	-0.00516	0.0400	0.0384	0.537	0.331
	(0.0424)	(0.0406)	(0.0643)	(0.0600)	(0.0341)	(0.0306)	(0.0537)	(0.0377)	(0.0374)	(0.0315)	(0.178)	(0.112)	(1.835)	(1.667)
F-test equality of the 3 contracts {p-value}	2.20 {0.115}	2.39 {0.096}	0.84 {0.436}	1.10 {0.337}	0.03 {0.968}	0.07 {0.937}	0.06 {0.937}	0.26 {0.774}	1.56 {0.214}	1.78 {0.172}	0.42 {0.656}	1.13 {0.325}	0.71 {0.495}	0.72 {0.488}
Observations	2,803	2,803	2,803	2,803	2,803	2,803	2,803	2,803	2,803	2,803	2,803	2,803	2,803	2,803
R^2	0.200	0.245	0.286	0.330	0.025	0.084	0.000	0.115	0.002	0.068	0.048	0.223	0.190	0.249
Control group mean	0.541	0.541	0.719	0.719	0.249	0.249	0.483	0.483	0.150	0.150	2.021	2.021	19.112	19.112
Controls		✓		✓		✓		✓		✓		✓		✓

Robust standard errors clustered at the village level in parentheses. [a]The amount borrowed is winsorized at the 99^{th} percentile to ameliorate the undue influence of outlying observations. The set of controls includes gender, years of education and religion (Muslim indicator) of the head of the household, household size, average age of household members, land ownings, (non-land) asset index, head of household's primary occupation category indicators, and district indicators.

Table 20: ITT of credit access on agricultural technology adoption, controlling for baseline outcomes.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	$\geq$ 1 technology adopted		# technologies adopted		Land area on which technologies applied		$\geq$ 1 technology adopted		# technologies adopted		Land area on which technologies applied	
Treatment	-0.0263	-0.0236	-0.113	-0.112	-0.0160	-0.0169						
	(0.0286)	(0.0250)	(0.163)	(0.140)	(0.0175)	(0.0160)						
Contract:												
Standard							-0.0376	-0.0170	-0.111	-0.0172	-0.00591	0.00239
							(0.0338)	(0.0303)	(0.202)	(0.175)	(0.0249)	(0.0221)
Grace period							-0.0175	-0.0223	-0.105	-0.147	-0.0109	-0.0179
							(0.0356)	(0.0315)	(0.195)	(0.167)	(0.0217)	(0.0208)
Choice							-0.0243	-0.0309	-0.121	-0.169	-0.0303	-0.0340
							(0.0363)	(0.0319)	(0.198)	(0.170)	(0.0196)	(0.0177)
F-test equality of the 3 contracts {p-value}							0.18	0.10	0.00	0.39	0.71	1.57
							{0.838}	{0.905}	{0.997}	{0.675}	{0.493}	{0.211}
Observations	1,406	1,406	1,406	1,406	1,406	1,406	1,406	1,406	1,406	1,406	1,406	1,406
R^2	0.251	0.316	0.283	0.363	0.355	0.422	0.251	0.316	0.283	0.363	0.355	0.423
Control group mean	0.511	0.511	2.539	2.539	0.227	0.227	0.511	0.511	2.539	2.539	0.227	0.227
Controls		✓		✓		✓		✓		✓		✓

Robust standard errors clustered at the village level in parentheses. The total land area on which technologies are applied is winsorized at the 99^{th} percentile to ameliorate the undue influence of outlying observations. Besides baseline outcomes, the set of controls includes gender, years of education and religion (Muslim indicator) of the head of the household, household size, average age of household members, land ownings, (non-land) asset index, head of household's primary occupation category indicators, and district indicators.

Table 21: ITT of credit access on agricultural technology adoption, correcting for attrition.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	$\geq$ 1 technology adopted		# technologies adopted		Land area on which technologies applied		$\geq$ 1 technology adopted		# technologies adopted		Land area on which technologies applied	
Treatment	-0.0262	-0.0198	-0.172	-0.130	-0.0335	-0.0253						
	(0.0371)	(0.0285)	(0.232)	(0.156)	(0.0242)	(0.0173)						
Contract:												
Standard							-0.0482	-0.0133	-0.231	-0.0374	-0.0219	-0.00126
							(0.0442)	(0.0359)	(0.297)	(0.204)	(0.0321)	(0.0222)
Grace period							-0.00652	-0.0137	-0.0884	-0.131	-0.0290	-0.0284
							(0.0483)	(0.0366)	(0.292)	(0.193)	(0.0292)	(0.0228)
Choice							-0.0246	-0.0317	-0.195	-0.215	-0.0485	-0.0448
							(0.0492)	(0.0374)	(0.292)	(0.193)	(0.0290)	(0.0201)
Observations	1,405	1,405	1,405	1,405	1,405	1,405	1,405	1,405	1,405	1,405	1,405	1,405
R^2	0.001	0.216	0.001	0.279	0.002	0.325	0.001	0.217	0.001	0.280	0.003	0.327
Control group mean	0.511	0.511	2.539	2.539	0.227	0.227	0.511	0.511	2.539	2.539	0.227	0.227
Controls		✓		✓		✓		✓		✓		✓

Results shown re-weight the data using the inverse of the propensity to be observed at endline, so that the distribution of observable characteristics (at baseline) among households observed at endline resembles that in the entire baseline sample. Robust standard errors clustered at the village level in parentheses. The land to which technologies are applied (in 1000 decimals) is winsorized at the 99^{th} percentile to ameliorate the undue influence of outlying observations. Besides baseline outcomes, the set of controls includes gender, years of education and religion (Muslim indicator) of the head of the household, household size, average age of household members, land ownings, (non-land) asset index, head of household's primary occupation category indicators, and district indicators.

Table 22: ITT of credit access on adopting individual agricultural technologies (specified as column headers).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A (any contract)	Hybrid seeds		$\geq$ fertilizer		# of fertilizers		Mechanized irrigation	
Treatment	-0.0249	-0.0169	-0.0277	-0.0205	-0.0668	-0.0628	-0.0110	-0.00448
	(0.0282)	(0.0238)	(0.0372)	(0.0283)	(0.0874)	(0.0629)	(0.0449)	(0.0274)
R^2	0.001	0.135	0.001	0.217	0.000	0.101	0.000	0.267
MDE[a]	0.083		0.161		0.156		0.143	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel B (by contract type)	Hybrid seeds		$\geq$ fertilizer		# of fertilizers		Mechanized irrigation	
Standard	-0.0322	-0.00947	-0.0481	-0.0128	-0.0880	-0.0548	-0.0379	-0.00491
	(0.0358)	(0.0309)	(0.0443)	(0.0356)	(0.109)	(0.0806)	(0.0573)	(0.0330)
Grace period	-0.0259	-0.0216	-0.0102	-0.0164	-0.0368	-0.0588	0.00812	-0.00259
	(0.0379)	(0.0322)	(0.0488)	(0.0369)	(0.108)	(0.0766)	(0.0580)	(0.0358)
Choice	-0.0172	-0.0194	-0.0255	-0.0315	-0.0755	-0.0743	-0.00429	-0.00585
	(0.0337)	(0.0280)	(0.0494)	(0.0371)	(0.111)	(0.0789)	(0.0559)	(0.0361)
Observations	1,406	1,406	1,406	1,406	1,406	1,406	1,406	1,406
R^2	0.001	0.135	0.001	0.217	0.000	0.101	0.001	0.267
Control group mean	0.180	0.180	0.509	0.509	1.815	1.815	0.423	0.423
Controls		✓		✓		✓		✓

Robust standard errors clustered at the village level in parentheses. [a]The minimum detectible effect size (MDE) is the minimum detectible difference (statistically significant at $\alpha = 5\%$ with a two-sided test) in propensities between treatment (μ_1) and control groups (μ_0) based on an ex-post power calculation using the formula $\mu_1 = \frac{1}{n(ak^2-J+1)}(-\frac{1}{2})(a - 2n\mu_0 + 2Jn\mu_0 - \sqrt{a(a+(-4)\mu_0 n(aK^2 - 2J + 2)(K^2 n\mu_0 + 1))})$, where n is the number of observations per cluster, β is the desired power of the test (here 0.8), z_1 is the z-value corresponding to the desired significance level of the test, z_2 is the z-value corresponding to the desired power of the design, J is the number of clusters in each group (using 50, the number of clusters in the control group), and k is the intra-cluster correlation coefficient (Djimeu and Houndolo, 2016). The set of controls includes gender, years of education and religion (Muslim indicator) of the head of the household, household size, average age of household members, land ownings, (non-land) asset index, head of household's primary occupation category indicators, and district indicators.

Table 23: LATE of credit access on agricultural technology adoption.

	(1)	(2)	(3)	(4)	(5)	(6)
	$\geq$ 1 technology adopted		# technologies adopted		Land area on which technologies applied	
Take-up	-0.204	-0.175	-1.303	-1.249	-0.159	-0.137
	(0.214)	(0.170)	(1.364)	(0.972)	(0.146)	(0.109)
Constant	0.506	0.637	2.513	3.436	0.214	0.368
	(0.0319)	(0.121)	(0.211)	(0.651)	(0.0225)	(0.0870)
Observations	915	915	915	915	915	915
R^2		0.217		0.270		0.332
Controls		✓		✓		✓

Robust standard errors clustered at the village level in parentheses. The land to which technologies are applied (in decimals) is winsorized at the 99^{th} percentile to ameliorate the undue influence of outlying observations. The set of controls includes gender, years of education and religion (Muslim indicator) of the head of the household, household size, average age of household members, land ownings, (non-land) asset index, head of household's primary occupation category indicators, and district indicators.

Table 24: Heterogeneity in ITT effects of credit access on agricultural technology adoption by risk preferences, controlling for baseline outcomes.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	$\geq$ 1 technology adopted		# technologies adopted		Land area on which technologies applied		$\geq$ 1 technology adopted		# technologies adopted		Land area on which technologies applied	
Treatment	-0.0246	-0.0302	-0.302	-0.295	-0.0345	-0.0417						
	(0.0513)	(0.0501)	(0.295)	(0.282)	(0.0320)	(0.0323)						
Risk averse (RA)	-0.0299	-0.0385	-0.362	-0.297	-0.0336	-0.0320	-0.0300	-0.0390	-0.361	-0.298	-0.0336	-31.98
	(0.0521)	(0.0489)	(0.288)	(0.265)	(0.0380)	(0.0369)	(0.0522)	(0.0489)	(0.288)	(0.265)	(0.0381)	(37.00)
Treatment × RA	0.0123	0.0198	0.345	0.268	0.0473	0.0528						
	(0.0641)	(0.0608)	(0.346)	(0.331)	(0.0436)	(0.0421)						
Standard							0.0534	0.0419	0.0841	0.137	-0.0227	-27.26
							(0.0635)	(0.0678)	(0.387)	(0.391)	(0.0360)	(37.43)
Grace period							-0.0657	-0.0513	-0.517	-0.461	-0.0337	-30.50
							(0.0609)	(0.0589)	(0.321)	(0.302)	(0.0373)	(39.74)
Choice							-0.0570	-0.0747	-0.451	-0.526	-0.0454	-64.02
							(0.0664)	(0.0596)	(0.346)	(0.315)	(0.0411)	(39.77)
Standard × RA							-0.112	-0.0833	-0.212	-0.295	0.0287	37.10
							(0.0798)	(0.0769)	(0.448)	(0.434)	(0.0528)	(52.54)
Grace period × RA							0.0923	0.0739	0.660	0.540	0.0513	40.72
							(0.0776)	(0.0751)	(0.389)	(0.379)	(0.0478)	(48.58)
Choice × RA							0.0521	0.0605	0.565	0.514	0.0603	77.31
							(0.0861)	(0.0806)	(0.467)	(0.437)	(0.0559)	(52.52)
F-test {p-value}	1.76	1.25	0.67	0.96	0.61	0.17						
Treat + Treat × RA = 0	{0.187}	{0.266}	{0.416}	{0.329}	{0.438}	{0.685}						
Observations	915	914	915	914	915	914	915	914	915	914	915	914
R^2	0.239	0.326	0.271	0.370	0.334	0.434	0.244	0.329	0.275	0.374	0.335	0.435
Control group mean	0.506	0.506	2.513	2.513	0.214	0.214	0.506	0.506	2.513	2.513	0.214	0.214
Controls		✓		✓		✓		✓		✓		✓

Robust standard errors clustered at the village level in parentheses. The risk aversion measure is binary and its construction is described in Subsection 4.3. The land to which technologies are applied (in decimals) is winsorized at the 99^{th} percentile to ameliorate the undue influence of outlying observations. Besides baseline realizations of the outcome, the set of controls includes gender, years of education religion (Muslim indicator) and primary occupation of the head of the household (for categories see Table 1), household size, average age of household members, land ownings, (non-land) asset index, agriculture's share of household income, head of household occupation category indicators, and district indicators, as well as the gender, age, years of education, and primary occupation of the respondent.

Table 25: Heterogeneity in ITT effects of credit access on agricultural technology adoption by risk aversion, correcting for attrition.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	≥ 1 technology adopted		# technologies adopted		Land area on which technologies applied		≥ 1 technology adopted		# technologies adopted		Land area on which technologies applied	
Treatment	0.000218	-0.0158	-0.232	-0.247	-0.0317	-0.0372						
	(0.0586)	(0.0535)	(0.348)	(0.296)	(0.0383)	(0.0357)						
Risk averse (RA)	0.0249	0.000952	-0.0332	-0.0678	0.00780	-0.00865	0.0249	0.000962	-0.0332	-0.0681	0.00780	-8.690
	(0.0586)	(0.0507)	(0.345)	(0.281)	(0.0451)	(0.0397)	(0.0587)	(0.0508)	(0.346)	(0.281)	(0.0452)	(39.80)
Treatment × RA	-0.0633	-0.0330	-0.00863	0.0108	0.00642	0.0272						
	(0.0722)	(0.0640)	(0.409)	(0.351)	(0.0506)	(0.0452)						
Standard							0.0749	0.0430	0.274	0.205	0.00477	-14.37
							(0.0744)	(0.0732)	(0.480)	(0.418)	(0.0448)	(39.73)
Grace period							-0.0499	-0.0255	-0.548	-0.391	-0.0543	-24.78
							(0.0716)	(0.0651)	(0.388)	(0.332)	(0.0443)	(44.23)
Choice							-0.0220	-0.0578	-0.402	-0.516	-0.0442	-68.05
							(0.0746)	(0.0640)	(0.407)	(0.333)	(0.0472)	(44.20)
Standard × RA							-0.187	-0.125	-0.750	-0.616	-0.0320	6.010
							(0.0859)	(0.0803)	(0.505)	(0.447)	(0.0587)	(54.01)
Grace period × RA							0.0194	0.00718	0.453	0.295	0.0173	6.170
							(0.0907)	(0.0814)	(0.461)	(0.409)	(0.0551)	(51.06)
Choice × RA							-0.0232	0.01000	0.257	0.300	0.0342	65.70
							(0.0999)	(0.0856)	(0.547)	(0.460)	(0.0651)	(55.94)
Observations	915	915	915	915	915	915	915	915	915	915	915	915
R-squared	0.002	0.002	0.002	0.002	0.002	0.002	0.007	0.007	0.008	0.008	0.004	0.004
Control group mean	0.506	0.506	2.513	2.513	0.214	0.214	0.506	0.506	2.513	2.513	0.214	0.214
Controls		✓		✓		✓		✓		✓		✓

Results shown re-weight the data using the inverse of the propensity to be observed at endline, so that the distribution of observable characteristics (at baseline) among households observed at endline resembles that in the entire baseline sample. Robust standard errors clustered at the village level in parentheses. The risk aversion measure is binary and its construction is described in Subsection 4.3. The land to which technologies are applied (in decimals) is winsorized at the 99^{th} percentile to ameliorate the undue influence of outlying observations. Besides baseline realizations of the outcome, the set of controls includes gender, years of education religion (Muslim indicator) and primary occupation of the head of the household (for categories see Table 1), household size, average age of household members, land ownings, (non-land) asset index, agriculture's share of household income, head of household occupation category indicators, and district indicators, as well as the gender, age, years of education, and primary occupation of the respondent.

Table 26: Heterogeneity in ITT effects of credit access on agricultural technology adoption by risk aversion, using an ordinal measure of risk aversion.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	≥ 1 technology adopted		# technologies adopted		Land area on which technologies applied		≥ 1 technology adopted		# technologies adopted		Land area on which technologies applied	
Treatment	0.0441	0.0218	0.249	0.0782	0.0322	0.0137						
	(0.0513)	(0.0423)	(0.303)	(0.232)	(0.0334)	(0.0288)						
(Ordinal) risk aversion (RA)	0.0294	0.0152	0.145	0.0581	0.0170	0.00793	0.0294	0.0152	0.145	0.0584	0.0170	0.00804
	(0.0158)	(0.0124)	(0.0991)	(0.0728)	(0.0111)	(0.00953)	(0.0159)	(0.0124)	(0.0993)	(0.0728)	(0.0112)	(0.00954)
Treatment × RA	-0.0422	-0.0205	-0.217	-0.0851	-0.0237	-0.00996						
	(0.0193)	(0.0154)	(0.112)	(0.0851)	(0.0127)	(0.0110)						
Standard							0.0678	0.0401	0.489	0.338	0.0534	0.0227
							(0.0678)	(0.0602)	(0.420)	(0.333)	(0.0435)	(0.0356)
Grace period							0.0107	0.0294	0.0499	0.0481	0.0198	0.0408
							(0.0626)	(0.0510)	(0.343)	(0.262)	(0.0401)	(0.0343)
Choice							0.0548	0.000371	0.226	-0.114	0.0254	-0.0172
							(0.0649)	(0.0526)	(0.363)	(0.268)	(0.0401)	(0.0355)
Standard × RA							-0.0476	-0.0200	-0.276	-0.132	-0.0231	-0.00368
							(0.0245)	(0.0213)	(0.142)	(0.155)	(0.0164)	(0.0143)
Grace period × RA							-0.0257	-0.0217	-0.138	-0.0852	-0.0211	-0.0234
							(0.0249)	(0.0192)	(0.129)	(0.0953)	(0.0144)	(0.0125)
Choice × RA							-0.0537	-0.0211	-0.245	-0.0504	-0.0278	-0.00401
							(0.0247)	(0.0199)	(0.135)	(0.105)	(0.0152)	(0.0137)
Observations	1,376	1,373	1,376	1,373	1,376	1,373	1,376	1,373	1,376	1,373	1,376	1,373
R^2	0.006	0.248	0.005	0.304	0.004	0.320	0.007	0.249	0.006	0.306	0.005	0.323
Control group mean	0.506	0.506	2.513	2.513	0.214	0.214	0.506	0.506	2.513	2.513	0.214	0.214
Controls		✓		✓		✓		✓		✓		✓

Robust standard errors clustered at the village level in parentheses. The land to which technologies are applied (in decimals) is winsorized at the 99^{th} percentile to ameliorate the undue influence of outlying observations. The set of controls includes gender, years of education religion (Muslim indicator) and primary occupation of the head of the household (for categories see Table 1), household size, average age of household members, land ownings, (non-land) asset index, agriculture's share of household income, head of household occupation category indicators, and district indicators, as well as the gender, age, years of education, and primary occupation of the respondent.

Table 27: Heterogeneity in ITT estimates of credit access on agricultural technology adoption by risk aversion, with p-values correcting for the Familywise Error Rate (FWER).

	(1)	(2)	(3)	(4)
Treatment	0.000313	-0.100		
	(0.624)	(0.454)		
	{0.995}	{0.978}		
Risk averse (RA)	0.0244	-0.0236	0.0387	-0.0241
	(0.675)	(0.861)	(0.807)	(0.858)
	{0.996}	{0.993}	{0.996}	{0.993}
Treatment × RA	-0.0639	0.0112		
	(0.374)	(0.944)		
	{1.000}	{0.948}		
Contract:				
Standard			0.103	0.0479
			(0.581)	(0.778)
			{0.994}	{0.998}
Grace period			-0.190	-0.113
			(0.279)	(0.481)
			{0.862}	{0.979}
Choice			-0.125	-0.219
			(0.499)	(0.169)
			{0.980}	{0.757}
Standard × RA			-0.329	-0.185
			(0.132)	(0.346)
			{0.627}	{0.938}
Grace period × RA			0.0755	0.0543
			(0.723)	(0.773)
			{0.999}	{0.999}
Choice × RA			0.0391	0.146
			(0.875)	(0.481)
			{0.978}	{0.982}
Observations	915	914	915	914
R^2	0.002	0.340	0.006	0.343
Control group mean	0.506	0.506	0.506	0.506
Controls		✓		✓

P-values based on robust standard errors clustered at the village level in parentheses, and corrected for Familywise Error Rate (FWER) in curly brackets. The risk aversion measure is binary and its construction is described in Subsection 4.3. The land to which technologies are applied (in decimals) is winsorized at the 99^{th} percentile to ameliorate the undue influence of outlying observations. The set of controls includes gender, years of education religion (Muslim indicator) and primary occupation of the head of the household (for categories see Table 1), household size, average age of household members, land ownings, (non-land) asset index, agriculture's share of household income, head of household occupation category indicators, and district indicators, as well as the gender, age, years of education, and primary occupation of the respondent.

Table 28: Heterogeneity in ITT effects of credit access on agricultural technology adoption by present bias, using an ordinal measure of present-bias.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	$\geq$ 1 technology adopted		# technologies adopted		Land area on which technologies applied		$\geq$ 1 technology adopted		# technologies adopted		Land area on which technologies applied	
Treatment	-0.0819	-0.0821	-0.431	-0.472	-69.67	-64.24						
	(0.0486)	(0.0424)	(0.296)	(0.224)	(31.30)	(25.52)						
(Ordinal) present-bias (PB)	-0.0280	-0.0309	-0.0836	-0.119	-25.65	-24.16	-0.0280	-0.0308	-0.0836	-0.119	-25.65	-24.16
	(0.0357)	(0.0331)	(0.188)	(0.177)	(16.57)	(15.55)	(0.0358)	(0.0331)	(0.188)	(0.178)	(16.60)	(15.60)
Treatment × PB	0.0782	0.0885	0.337	0.441	71.03	73.42						
	(0.0436)	(0.0402)	(0.228)	(0.211)	(23.65)	(20.31)						
Standard							-0.100	-0.0859	-0.529	-0.488	-62.18	-50.73
							(0.0651)	(0.0602)	(0.395)	(0.319)	(43.78)	(34.62)
Grace period							-0.0461	-0.0355	-0.251	-0.245	-61.71	-48.50
							(0.0595)	(0.0509)	(0.358)	(0.260)	(35.33)	(26.45)
Choice							-0.102	-0.133	-0.528	-0.720	-84.52	-96.02
							(0.0628)	(0.0510)	(0.367)	(0.269)	(34.57)	(29.02)
Standard × PB							0.102	0.0984	0.563	0.579	75.63	65.89
							(0.0566)	(0.0534)	(0.284)	(0.260)	(35.62)	(26.10)
Grace period × PB							0.00901	0.0294	-0.0888	0.0970	34.42	53.24
							(0.0672)	(0.0680)	(0.336)	(0.360)	(37.64)	(37.86)
Choice × PB							0.103	0.125	0.412	0.574	87.81	95.61
							(0.0504)	(0.0432)	(0.272)	(0.232)	(28.76)	(25.68)
Observations	1,376	1,373	1,376	1,373	1,376	1,373	1,376	1,373	1,376	1,373	1,376	1,373
R^2	0.006	0.248	0.005	0.304	0.004	0.320	0.007	0.249	0.006	0.306	0.005	0.323
Control group mean	0.506	0.506	2.513	2.513	0.214	0.214	0.506	0.506	2.513	2.513	0.214	0.214
Controls		✓		✓		✓		✓		✓		✓

Robust standard errors clustered at the village level in parentheses. The land to which technologies are applied (in decimals) is winsorized at the 99^{th} percentile to ameliorate the undue influence of outlying observations. The set of controls includes gender, years of education religion (Muslim indicator) and primary occupation of the head of the household (for categories see Table 1), household size, average age of household members, land ownings, (non-land) asset index, agriculture's share of household income, head of household occupation category indicators, and district indicators, as well as the gender, age, years of education, and primary occupation of the respondent.

Table 29: Heterogeneity in ITT of credit access on agricultural technology adoption by present bias, controlling for baseline outcomes.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	$\geq$ 1 technology adopted		# technologies adopted		Land area on which technologies applied		$\geq$ 1 technology adopted		# technologies adopted		Land area on which technologies applied	
Treatment	-0.0485	-0.0503	-0.252	-0.307	-0.311	-0.0369						
	(0.0378)	(0.0348)	(0.220)	(0.192)	(0.0224)	(0.0210)						
Present-biased (PB)	-0.0634	-0.0581	-0.277	-0.297	-0.0605	-0.0562	-0.0633	-0.0580	-0.277	-0.297	-60.31	-56.19
	(0.0717)	(0.0656)	(0.414)	(0.381)	(0.0360)	(0.0316)	(0.0719)	(0.0657)	(0.415)	(0.381)	(36.21)	(31.78)
Treatment × PB	0.159	0.159	0.784	0.850	0.122	0.128						
	(0.0828)	(0.0771)	(0.466)	(0.433)	(0.0458)	(0.0407)						
Standard							-0.0395	-0.0366	-0.182	-0.224	-19.08	-25.86
							(0.0460)	(0.0470)	(0.276)	(0.265)	(31.08)	(30.21)
Grace period							-0.0323	-0.0258	-0.205	-0.208	-28.71	-30.80
							(0.0461)	(0.0415)	(0.255)	(0.210)	(25.10)	(21.84)
Choice							-0.0781	-0.0942	-0.386	-0.517	-46.32	-55.31
							(0.0482)	(0.0426)	(0.263)	(0.229)	(24.19)	(24.95)
Standard × PB							0.131	0.140	0.698	0.894	66.97	99.75
							(0.0961)	(0.0915)	(0.531)	(0.485)	(62.14)	(50.23)
Grace period × PB							0.146	0.124	0.524	0.414	184.4	173.0
							(0.117)	(0.126)	(0.593)	(0.693)	(80.11)	(88.14)
Choice × PB							0.201	0.212	1.013	1.103	143.7	139.5
							(0.0978)	(0.0897)	(0.542)	(0.499)	(52.19)	(49.32)
F-test {p-value}:	2.40	2.68	1.73	1.99	5.22	7.18						
Treat + Treat×PB = 0	{0.123}	{0.104}	{0.191}	{0.161}	{0.024}	{0.008}						
Observations	915	914	915	914	915	914	915	914	915	914	915	914
R^2	0.243	0.330	0.274	0.374	0.339	0.439	0.244	0.332	0.275	0.376	0.342	0.441
Control group mean	0.506	0.506	2.513	2.513	0.214	0.214	0.506	0.506	2.513	2.513	0.214	0.214
Controls		✓		✓		✓		✓		✓		✓

Robust standard errors clustered at the village level in parentheses. The land to which technologies are applied (in decimals) is winsorized at the 99^{th} percentile to ameliorate the undue influence of outlying observations. Besides baseline realizations of the outcome, the set of controls includes gender, years of education religion (Muslim indicator) and primary occupation of the head of the household (for categories see Table 1), household size, average age of household members, land ownings, (non-land) asset index, agriculture's share of household income, head of household occupation category indicators, and district indicators, as well as the gender, age, years of education, and primary occupation of the respondent.

Table 30: Heterogeneity in ITT of credit access on agricultural technology adoption by present bias, correcting for attrition.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	≥ 1 technology adopted		# technologies adopted		Land area on which technologies applied		≥ 1 technology adopted		# technologies adopted		Land area on which technologies applied	
Treatment	-0.0485	-0.0503	-0.252	-0.307	-0.311	-0.0369						
	(0.0378)	(0.0348)	(0.220)	(0.192)	(0.0224)	(0.0210)						
Present-biased (PB)	-0.0634	-0.0581	-0.277	-0.297	-0.0605	-0.0562	-0.0633	-0.0580	-0.277	-0.297	-60.31	-56.19
	(0.0717)	(0.0656)	(0.414)	(0.381)	(0.0360)	(0.0316)	(0.0719)	(0.0657)	(0.415)	(0.381)	(36.21)	(31.78)
Treatment × PB	0.159	0.159	0.784	0.850	0.122	0.128						
	(0.0828)	(0.0771)	(0.466)	(0.433)	(0.0458)	(0.0407)						
Standard							-0.0395	-0.0366	-0.182	-0.224	-19.08	-25.86
							(0.0460)	(0.0470)	(0.276)	(0.265)	(31.08)	(30.21)
Grace period							-0.0323	-0.0258	-0.205	-0.208	-28.71	-30.80
							(0.0461)	(0.0415)	(0.255)	(0.210)	(25.10)	(21.84)
Choice							-0.0781	-0.0942	-0.386	-0.517	-46.32	-55.31
							(0.0482)	(0.0426)	(0.263)	(0.229)	(24.19)	(24.95)
Standard × PB							0.131	0.140	0.698	0.894	66.97	99.75
							(0.0961)	(0.0915)	(0.531)	(0.485)	(62.14)	(50.23)
Grace period × PB							0.146	0.124	0.524	0.414	184.4	173.0
							(0.117)	(0.126)	(0.593)	(0.693)	(80.11)	(88.14)
Choice × PB							0.201	0.212	1.013	1.103	143.7	139.5
							(0.0978)	(0.0897)	(0.542)	(0.499)	(52.19)	(49.32)
F-test {p-value}:	2.40	2.68	1.73	1.99	5.22	7.18						
Treat + Treat×PB = 0	{0.123}	{0.104}	{0.191}	{0.161}	{0.024}	{0.008}						
Observations	915	914	915	914	915	914	915	914	915	914	915	914
R^2	0.243	0.330	0.274	0.374	0.339	0.439	0.244	0.332	0.275	0.376	0.342	0.441
Control group mean	0.506	0.506	2.513	2.513	0.214	0.214	0.506	0.506	2.513	2.513	0.214	0.214
Controls		✓		✓		✓		✓		✓		✓

Results shown re-weight the data using the inverse of the propensity to be observed at endline, so that the distribution of observable characteristics (at baseline) among households observed at endline resembles that in the entire baseline sample. Robust standard errors clustered at the village level in parentheses. The land to which technologies are applied (in decimals) is winsorized at the 99^{th} percentile to ameliorate the undue influence of outlying observations. The set of controls includes gender, years of education religion (Muslim indicator) and primary occupation of the head of the household (for categories see Table 1), household size, average age of household members, land ownings, (non-land) asset index, agriculture's share of household income, head of household occupation category indicators, and district indicators, as well as the gender, age, years of education, and primary occupation of the respondent.

Table 31: Heterogeneity in ITT of credit access on agricultural technology adoption by present bias, controlling (in each regression) for all baseline outcomes, including all baseline borrowing, technology adoption and agricultural output and profit outcomes.

	(1) ≥ 1 tech adopted	(2) # tech adopted	(3) Land area techs applied	(4) ≥ 1 tech adopted	(5) # techs adopted	(6) Land area techs applied
Treatment	-0.0538	-0.296	-0.0321			
	(0.0350)	(0.187)	(0.0206)			
Present-biased (PB)	-0.0609	-0.274	-0.0478	-0.0614	-0.279	-0.0478
	(0.0669)	(0.383)	(0.0323)	(0.0671)	(0.384)	(0.0325)
Treatment $\times$ PB	0.160	0.785	0.110			
	(0.0788)	(0.437)	(0.0426)			
Standard				-0.0374	-0.195	-0.0199
				(0.0473)	(0.260)	(0.0292)
Grace period				-0.0300	-0.193	-0.0243
				(0.0419)	(0.205)	(0.0216)
Choice				-0.101	-0.539	-0.0541
				(0.0427)	(0.221)	(0.0231)
Standard $\times$ PB				0.141	0.830	0.0804
				(0.0943)	(0.483)	(0.0520)
Grace period $\times$ PB				0.115	0.321	0.146
				(0.127)	(0.690)	(0.0898)
Choice $\times$ PB				0.219	1.071	0.126
				(0.0920)	(0.514)	(0.0512)
F-test {p-value}:	2.40	1.53	4.65			
Treat + Treat$\times$PB = 0	{0.241}	{0.219}	{0.033}			
Observations	914	914	914	914	914	914
R^2	0.334	0.384	0.462	0.336	0.387	0.464
Control group mean	0.482	2.356	0.195	0.482	2.356	0.195
Controls	✓	✓	✓	✓	✓	✓

Robust standard errors clustered at the village level in parentheses. The land to which technologies are applied (in decimals) is winsorized at the 99^{th} percentile to ameliorate the undue influence of outlying observations. Besides baseline realizations of agricultural output and profits and the three technology adoption indicators, the set of controls includes gender, years of education religion (Muslim indicator) and primary occupation of the head of the household (for categories see Table 1), household size, average age of household members, land ownings, (non-land) asset index, agriculture's share of household income, head of household occupation category indicators, and district indicators, as well as the gender, age, years of education, and primary occupation of the respondent.

Table 32: Heterogeneity in ITT estimates of credit access on agricultural technology adoption by present-bias, with p-values correcting for the Familywise Error Rate (FWER).

	(1)	(2)	(3)	(4)
Treatment	-0.0777	-0.207		
	(0.0847)	(0.0197)		
	{0.239}	{0.155}		
Present-bias (PB)	-0.0889	-0.215	-0.262	-0.215
	(0.219)	(0.150)	(0.115)	(0.151)
	{0.549}	{0.404}	{0.549}	{0.405}
Treatment × PB	0.205	0.564		
	(0.0214)	(0.00192)		
	{0.044}	{0.044}		
Contract:				
Standard			-0.218	-0.187
			(0.164)	(0.125)
			{0.493}	{0.536}
Grace period			-0.215	-0.142
			(0.112)	(0.145)
			{0.539}	{0.524}
Choice			-0.275	-0.307
			(0.0597)	(0.00576)
			{0.326}	{0.080}
Standard × PB			0.619	0.602
			(0.0247)	(0.00514)
			{0.158}	{0.063}
Grace period × PB			0.426	0.390
			(0.222)	(0.293)
			{0.434}	{0.404}
Choice × PB			0.683	0.665
			(0.00681)	(0.00115)
			{0.574}	{0.589}
Observations	915	914	915	914
R^2	0.009	0.350	0.014	0.352
Control group mean	0.506	0.506	0.506	0.506
Controls		✓		✓

P-values based on robust standard errors clustered at the village level in parentheses, and corrected for Familywise Error Rate (FWER) in curly brackets. The risk aversion measure is binary and its construction is described in Subsection 4.3. The land to which technologies are applied (in decimals) is winsorized at the 99^{th} percentile to ameliorate the undue influence of outlying observations. The set of controls includes gender, years of education religion (Muslim indicator) and primary occupation of the head of the household (for categories see Table 1), household size, average age of household members, land ownings, (non-land) asset index, agriculture's share of household income, head of household occupation category indicators, and district indicators, as well as the gender, age, years of education, and primary occupation of the respondent.

Table 33: Predictors of present-bias (dependent variable =1 if respondent is spouse of the head of the household, =0 otherwise).

	(1)	(2)	(3)
Baseline borrowing			
# NGO loans	-0.0273		
	(0.333)		
loan from a moneylender	-0.0417		
	(0.301)		
loan from acquintance	-0.0169		
	(0.635)		
other informal	0.0148		
	(0.702)		
nr loans	-0.00163		
	(0.938)		
total amount borrowed	1.48e-06		
Baseline tech adoption	(0.238)		
Any tech adopted		0.0315	
		(0.582)	
nr techs adopted		-0.0128	
		(0.319)	
Land used		0.00470	
HH-level covariates		(0.405)	
Female head of HH			-0.118
			(0.0444)
Education head of HH			-0.00325
			(0.432)
Head of HH Muslim			-0.00148
			(0.961)
HH size			0.0168
			(0.115)
Average age			0.00138
			(0.442)
Land holdings			-0.000561
			(0.317)
Asset index			-0.00377
			(0.603)
Occ. cat. 2: agric. wage labor			-0.0504
			(0.480)
Occ. cat. 3: fisheries			-0.0855
			(0.333)
Occ. cat. 4: self-employment			-0.0315
			(0.672)
Occ. cat. 5: freelancing			-0.0774
			(0.285)
Occ. cat. 6: housewife			0.0709
			(0.426)
Occ. cat. 7: salaried job			-0.0319
			(0.760)
Occ. cat. 8: other			-0.0785
Respondent-level covariates			(0.285)
Female respondent			0.0588
			(0.298)
Age of respondent			-0.00181
			(0.262)
Education of respondent			-0.000451
			(0.921)
Respondent's primary occupation:			
Occ. cat. 2: agric. wage labor			0.132
			(0.113)
Occ. cat. 3: fisheries			0.158
			(0.0716)
Occ. cat. 4: self-employment			0.0910
			(0.339)
Occ. cat. 5: freelancing			0.225
			(0.0227)
Occ. cat. 6: housewife			0.117
			(0.185)
Occ. cat. 7: salaried job			0.0696
			(0.600)
Occ. cat. 8: other			0.141

Robust standard errors clustered at the village level in parentheses.
Additional controls included but not displayed are district indicators.

8 Figure Appendix

For the implementation of causal forests, we use the R package Generalized Random Forests `grf` by Tibshirani et al. (2018). Following Basu et al. (2018), we first train a pilot random forest on all 22 covariates, and then train a second forest on only those features that saw a reasonable number of splits in the first step. In particular, the final causal forest is trained only on the covariates with a variable importance exceeding the median importance. This enables the forest to make more splits on the most important covariates. Ten thousand trees are grown in both steps.

Figures 3-5 show the frequency with which the tree is split along each covariate, for the three outcomes.

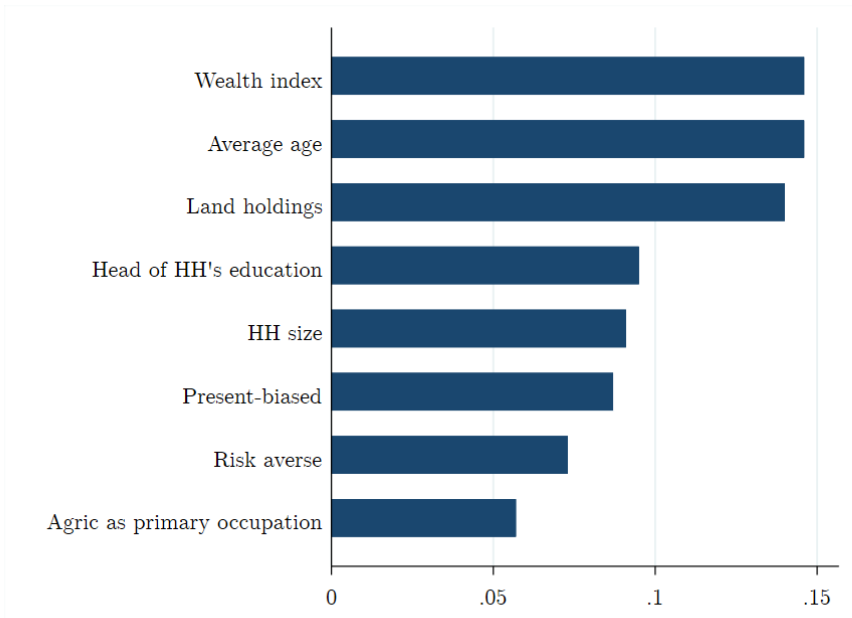

Figure 3: Frequency of tree split by covariate for Adoption indicator 1.

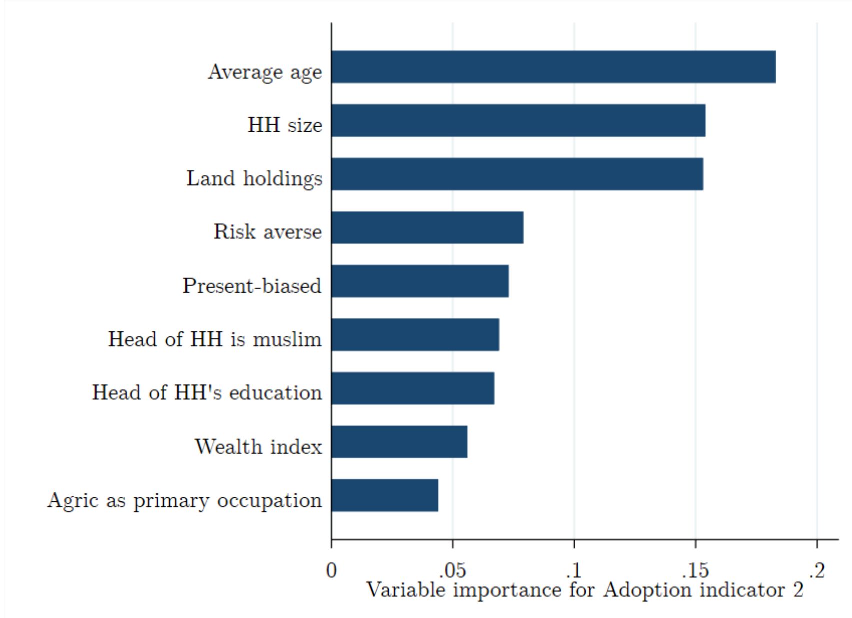

Figure 4: Frequency of tree split by covariate for Adoption indicator 2.

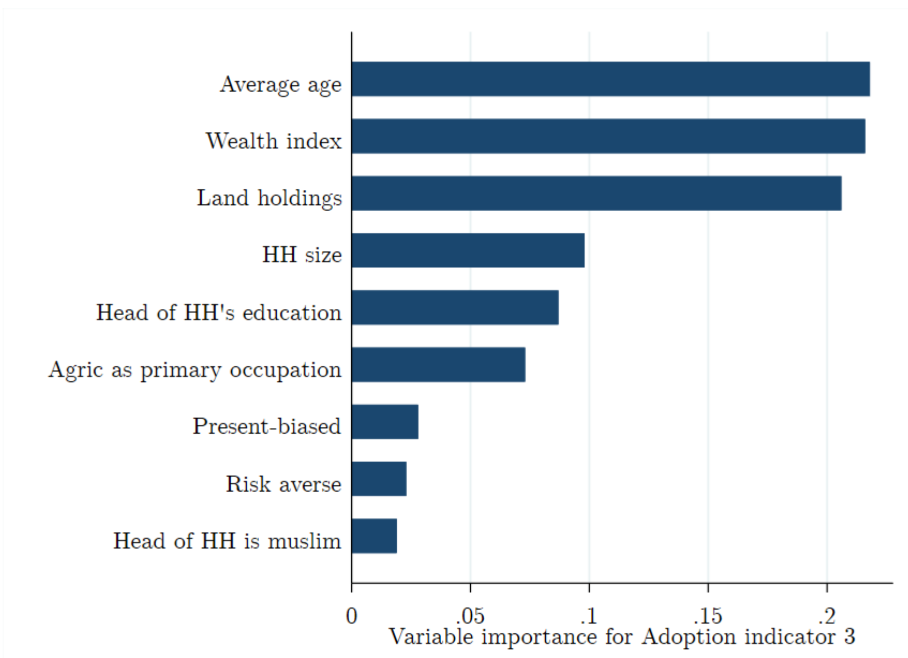

Figure 5: Frequency of tree split by covariate for Adoption indicator 3.

Figures 6 - 8 display the Conditional ITT on adoption indicator 1, 2 and 3, respectively. In each figure, panel (a) displays the histogram of the CITT estimates, panel (b) plots the ITT conditional on land holdings, panel (c) plots the ITT conditional on the asset index, and panel (d) plots the ITT conditional on the average age of household members.

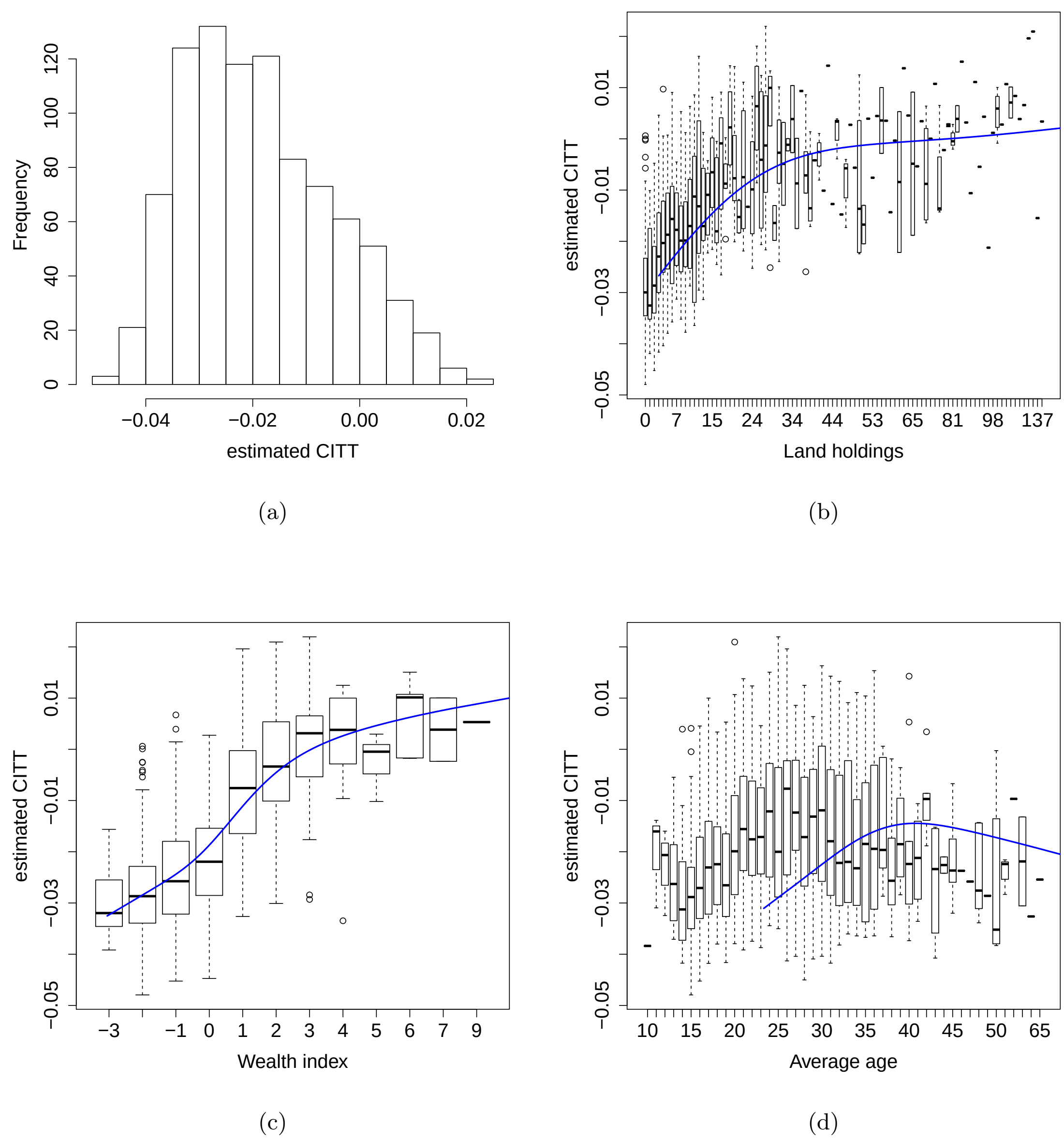

Figure 6: Causal forest results for adoption indicator 1: (a) histogram of CITT estimates; (b) CITT by land holdings; (c) CITT by wealth; (d) CITT by average age of household members.

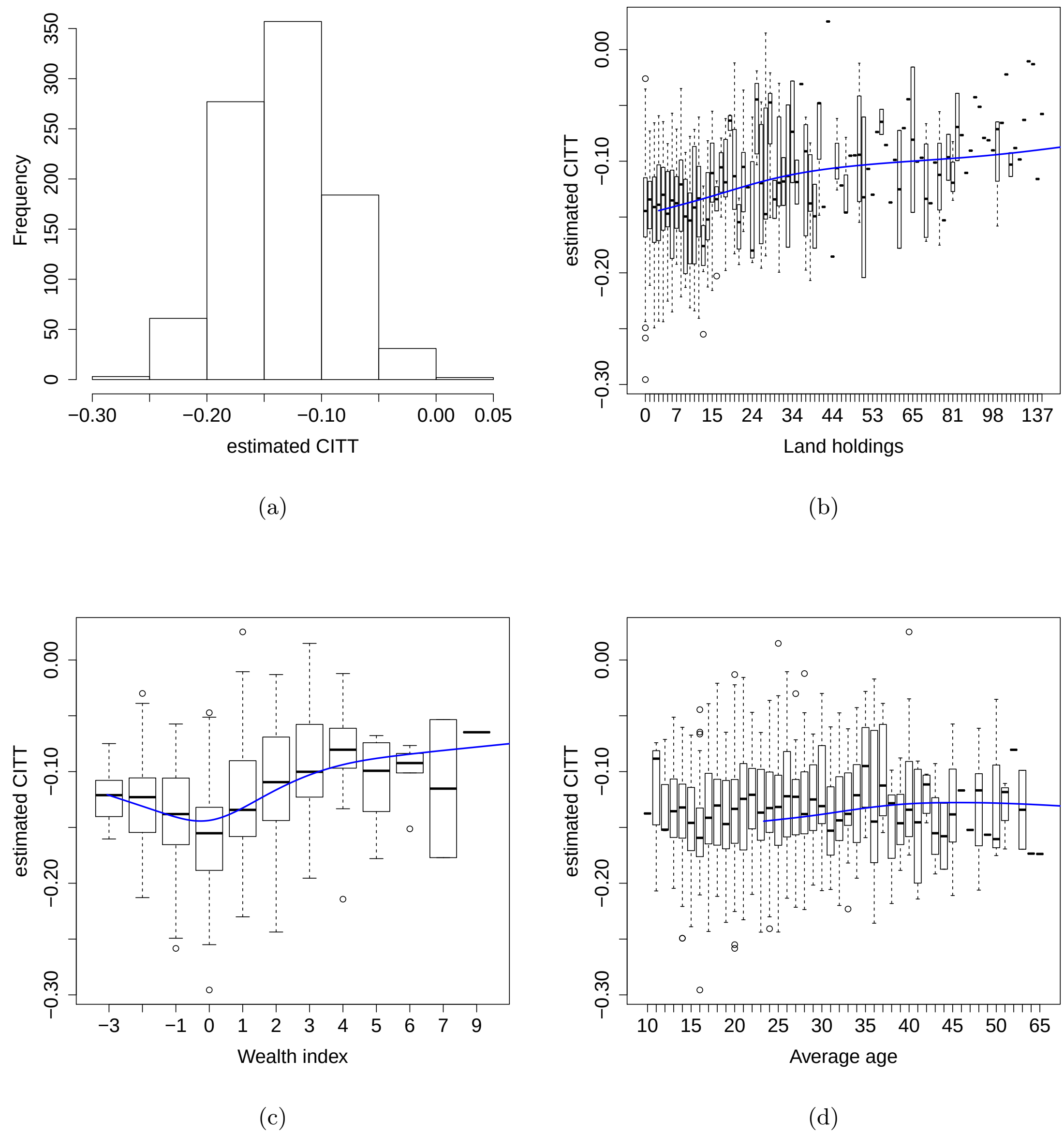

Figure 7: Causal forest results for adoption indicator 2: (a) histogram of CITT estimates; (b) CITT by land holdings; (c) CITT by wealth; (d) CITT by average age of household members.

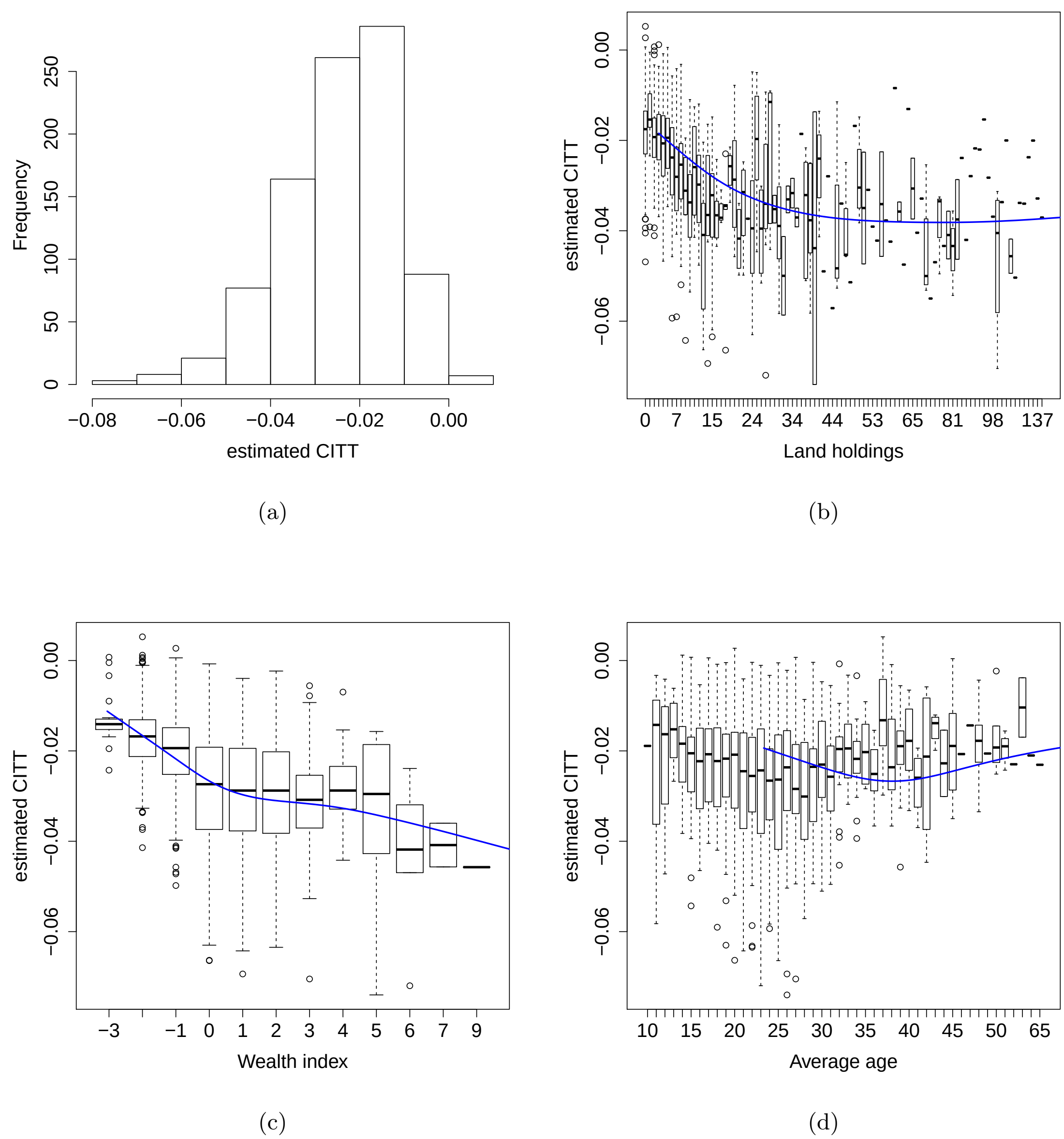

Figure 8: Causal forest results for adoption indicator 3: (a) histogram of CITT estimates; (b) CITT by land holdings; (c) CITT by wealth; (d) CITT by average age of household members.