Inefficient water pricing and incentives for conservation

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Abstract

Farmers across the world pay for water with fixed fees — rather than with marginal prices where payments are linked to usage. This pricing structure may discourage efficient water use in agriculture. We use two randomized controlled trials in Bangladesh to study the link between marginal prices and adoption of a new water conservation technology. Our first experiment shows that the new technology only saves water in villages with marginal water prices. Our second experiment finds that randomly converting farmers to hourly billing for irrigation water increases their demand for the conservation technology.

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1 Introduction

Climate change requires more efficient use of the earth's natural resources. Water is a resource that has received little attention, but is expected to become increasingly scarce as populations grow and temperatures climb (Vörösmarty et al., 2000; Schewe et al., 2014). Agriculture is the natural place to look for more efficient ways to use water, accounting for almost 70 percent of all the water consumed (FAO, 2016).

The problem of agricultural water conservation is complicated by the fact that in many settings, marginal prices do not exist. Instead, farmers pay fixed charges that are unrelated to water use. Figure 1 shows that of the 80 countries where we could find information, 54 had regions where water is not priced by volume.[1] Introducing marginal prices for irrigation water is a task that is fundamentally more complicated than raising an already existing price, which has been studied in the case of energy markets.[2] For one reason, farms in developing countries are small and a single source may supply water to a large number of farmers, which makes it prohibitively costly to meter water at each individual farm.[3]

The link between fixed charges and conservation practices of farmers has yet to receive a careful empirical investigation. Our paper studies this issue by measuring how the lack of marginal prices affects the adoption and benefits of a modern irrigation technology for conserving water. We use two randomized experiments with a technology designed to save water in rice farming. The technology is a perforated plastic pipe, open at both ends, that is planted in a rice field to help the farmer irrigate only when the crop needs water. Using this pipe to schedule irrigations is referred to as practicing "Alternate Wetting and Drying (AWD)."[4] The technology has been around for almost four decades but is not widely adopted, despite its simplicity and numerous agronomic trials showing that it can reduce water use by about 30 percent.[5]

Do fixed fees for irrigation water inhibit adoption of this technology? Does the technology save water only when farmers face volumetric prices? Does randomly introducing a marginal price for water lead farmers to put more value on conservation technology?

[1] The pricing methods were obtained from FAO (2004). Additional countries were classified using Johansson et al. (2002), Molle (2009), or Wichelns (2010).

[2] Studies on induced innovation show a positive association between electricity prices and development of energy-efficiency technologies (Newell, Jaffe, and Stavins, 1999; Popp, 2002).

[3] Average farm size in Bangladesh is less than an acre, it is about 442 acres in the U.S.

[4] We refer to the technology throughout the paper as an AWD pipe. To be exact, the treatment will be providing farmers with one of these pipes, which is meant to encourage their usage of the AWD irrigation method.

[5] Agronomic studies include Cabangon, Castillo, and Tuong (2011) and Bueno et al. (2010) in the Philippines, and Belder et al. (2004) and Yao et al. (2012) in China. Other trials have been carried out in Vietnam and Bangladesh (Lampayan et al., 2015).

To answer these questions, our first experiment shows that AWD only saves water when farmers face volumetric prices. This is done by randomly providing 2,000 farmers in Bangladesh with a free pipe and training on how to use it, and help with installing it on a specific plot that was identified prior to the experiment in all villages. The 2,000 control farmers continued to irrigate as before. We placed our sample of 400 villages in different geographical areas. About 35 percent of the sample faced non-zero marginal prices for irrigation water, while the remainder purchased water using a seasonal contract where the price is based solely on area cultivated, not the volume used. We use this variation to characterize the efficacy of the technology across the various ways in which farmers pay for water.

Using about 7,600 observations of water levels, we find that *on average* AWD leads to a modest and statistically insignificant change in water use. This finding is in sharp contrast to the evidence from agronomic trials. However, in the sub-sample with volumetric pricing, treatment plots had 19 percent less water and were 21 percent more likely to be dry when observed on random days — estimates that are in line with agronomic evidence.[6] This small plastic pipe generates large water savings. We estimate that the savings amount to about 0.14 acre feet of water on a single plot, which is equivalent to about half of the annual residential usage in the United States. In contrast, no such savings exist when farmers face fixed charges.

The profitability of the technology also depends crucially on whether farmers face volumetric prices. The technology has no effect on profits with seasonal water charges, consistent with the observation that water management did not change in this setting. Volumetric prices, on the other hand, incentivize use of the pipe: we find a significant increase in farm profits of about 7 percent. Overall, this first experiment suggests that there may be an important market failure that explains why farmers do not value a water-saving technology with proven results in the laboratory: they face a zero marginal price for water.

In the first experiment, we relied on the natural variation across farms to measure the relationship between volumetric pricing and the effectiveness of conservation technology. We further show that the heterogeneous effects we observe are robust to controlling for interactions between the treatment and many farmer characteristics. We use machine learning methods to investigate the most important dimensions of heterogeneity (Chernozhukov et al., 2018). This analysis picks marginal prices for water as one of the most important sources of heterogeneity. While these results suggest a critical role for marginal water prices in incentivizing agricultural conservation, the non-experimental variation in pricing cannot eliminate the possibility that the observed heterogeneity is due to an omitted factor that correlates

[6] We collected impact estimates from about 90 agronomic trials. Our estimate falls at the 25th percentile of this distribution.

with pricing but also mediates the impact of AWD.

We thus conducted a second RCT. Our second experiment is not the two-by-two analog to the first experiment. Rather, we estimate the causal effect of encouraging hourly irrigation prices on demand for AWD. Estimating the entire demand curve allows us to answer the question of whether marginal prices cause farmers to put more value on conservation technology.

In our sample area, there are 4,000 community tube wells that are equipped with meters that can take prepaid debit cards and release irrigation water. Farmers can load their own card with funds at a nearby kiosk and obtain irrigation water on demand. This solution is low-cost, implementable and aligns incentives for efficient water use. However, most farmers do not possess their own card and pay a fixed per acre fee instead. Our treatment seeks to increase the penetration of prepaid card usage in order to examine the causal link between the introduction of a marginal price and demand for the conservation technology.

We identified 144 villages which have installed meters, but use of prepaid cards by individual farmers is non-existent.[7] In order to encourage hourly pricing for water, we randomly selected 96 villages for a campaign to assist farmers in obtaining their own debit cards. Many farmers attribute the low rate of individual card ownership to the costs associated with the application process. Our treatment sought to reduce these costs by organizing a meeting with farmers to explain the purpose of the prepaid cards, help them fill out the paper application, obtain the photograph needed, pay the application fee of $1.9, deliver the forms to the irrigation authority, pick up the cards once complete, and deliver them to farmers. Once in hand, a farmer can load the card with funds — the same way as a mobile phone — and purchase water from the village tube well.

This nudge towards hourly water pricing changes how farmers value water-saving technology. We estimate the demand curve for AWD by sending sales teams to all villages and offering farmers a pipe at a randomly determined village-level price, along with information on its use. The eight different random prices ranged from 15 to 70 percent of the marginal cost of the pipe.

Encouraging hourly billing causes the demand for conservation technology to become less price responsive. The demand elasticity falls by 33 percent from 1.7 to 1.14 when comparing

[7] In most cases the tube well operator maintains a few cards, manages the allocation of water to farmers, and provides them with equal per-acre bills regardless of their individual consumption. The bills are most often paid in two installments: at the beginning and end of the season. One of the main benefits of this approach — from the perspective of the tube well operator — is the ease of tracking. The operator only needs to observe how much money is being used on his cards and acreage cultivated by each farmer, rather than keep track of the individual hours pumped. The operator levies a markup before calculating the per-acre cost to be charged to each farmer. The per-acre charge makes it easier to conceal this markup: the per hour cost of pumping is generally known to farmers.

treatment and control villages. At the four highest prices, the hourly cards increase purchase of the pipes by 35 percent. We find no effect on uptake at the four lowest prices. This demand experiment also lets us estimate the value farmers place on this conservation technology. Consumer surplus — when measured at our median price of \$0.7 — increases by 64 percent in the treatment villages.

Yet, demand for AWD is low. Only about 20 percent of the purchasing farmers were using the technology during the season. The prepaid cards do not themselves lead to less water use, suggesting that at least in our sample, despite increased willingness to pay for water-saving technology, there are other frictions that prevent the hourly irrigation cards from leading directly to water conservation.

These results present a puzzle when taken together with our first experiment where we show that the technology saves water and increases profits for farmers facing volumetric prices. One takeaway is that introducing marginal prices alone in complex developing country settings with small farm sizes and fragmented landholdings may not guarantee water conservation. For example, paying by the hour may adversely impact farmers located farther away from the water source relative to those located nearby. A desire for fairness in the community may then cause fixed prices to be favored. Additionally, the use of hourly cards by a subset of villagers can be problematic when communities irrigate multiple fields at a time to save on conveyance losses. These factors may have been less relevant in our first experiment because volumetric prices were already in place before our study. From the second experiment, we conclude that encouraging marginal prices in these complex environments increases willingness to pay for conservation technology, yet these prices do not translate into lower water use. Further work is needed to understand precisely what interventions can be coupled with pricing reform to achieve the ultimate policy goal of water conservation.

The main contribution of our paper is to study how marginal irrigation prices affect water conservation practices — particularly the adoption of water-saving technology. Despite the widespread existence of fixed irrigation fees, and calls from economists for institutional reform that introduces marginal prices (see Zilberman and Schoengold (2005)), there is little rigorous field evidence investigating the role seasonal water charges play in discouraging farmers from practicing more efficient irrigation methods. To our knowledge, this is the first paper that randomly introduces volumetric pricing for agricultural water.[8] This random

[8]Observational studies from both the U.S. and developing countries have found mixed results on introducing charges for irrigation water. Fishman et al. (2016) use non-experimental variation to study the water savings from a program in India where farmers voluntarily installed meters and were compensated for electricity savings relative to baseline consumption. They find no effect of the program on groundwater pumping. Smith et al. (2017) show difference-in-difference estimates from an irrigation district in Colorado where farmers self introduced a groundwater pumping fee. They find large water savings of around a third.

variation allows us to look at how pricing irrigation water on the margin affects demand and usage of conservation technology. While technology adoption is an intermediate outcome, our paper makes progress in an important area where the evidence base has been limited, despite the policy relevance of this seemingly inefficient method for pricing water.

Our work adds to the broader literature on energy and resource conservation. This literature has focused on both pecuniary and non-pecuniary mechanisms for inducing conservation. Several papers have considered the sensitivity of demand for energy and natural resources to changes in existing *marginal prices* (Nataraj and Hanemann, 2011; Ito, 2014; Jack, Jayachandran, and Rao, 2018). In contrast to prices, other mechanisms for natural resource conservation have been considered in the literature. These include paying people directly to avoid cutting down trees (Jayachandran et al., 2017) and using peer comparisons to nudge people to conserve (Ferraro and Price, 2013; Allcott and Rogers, 2014).

We offer rigorous evidence on a possible policy mechanism for introducing volumetric water pricing.[9] Metering individual fields is infeasible because farms are small and each tube well supplies water to many. Even if tube wells are metered, we observe that farmers may set up local institutions to retain seasonal charges. We show that a simple digital payment technology moves the pricing regime closer to marginal cost pricing and induces farmers to put more value on the conservation technology.

Our results suggest that inefficient factor pricing may explain why technologies that are available, proven in the laboratory, and seemingly in reach of farmers continue to exhibit low rates of adoption. Earlier explanations have focused on failures in output markets (Ashraf, Giné, and Karlan, 2009), behavioral biases (Duflo, Kremer, and Robinson, 2011), frictions in insurance or credit markets (Karlan et al., 2014; Cole, Giné, and Vickery, 2017), unobservable input quality (Bold et al., 2017), heterogeneity in the net benefits and costs of adoption (Suri, 2011), and learning frictions (Conley and Udry, 2010; Hanna, Mullainathan, and Schwartzstein, 2014; Beaman et al., 2018).[10] We add to this literature by showing that the pricing mechanism for a critical factor of production inhibits technology adoption.

The structure of the paper is as follows. The next section outlines the experimental design of the first RCT. Section 3 presents the results of that experiment showing how conservation technology only saves water and increases profits in villages where marginal prices exist.

[9] We do not experiment with the price level. Instead, we encourage a switch from a seasonal contract to hourly billing. Our treatment approximates volumetric pricing because hours pumped is imperfectly correlated with the volume of water extracted. Given that electricity accounts for a large share of the pumping cost, our treatment moves the pricing regime towards marginal cost pricing. But we do not necessarily introduce the socially optimal hourly price because that price would need to incorporate the externality costs of electricity generation.

[10] Jack (2011), de Janvry, Sadoulet, and Suri (2017), and Magruder (2018) provide comprehensive reviews of the literature on technology adoption in developing country agriculture.

Section 4 describes the second experiment that estimates the effect of encouraging prepaid hourly billing on demand for the AWD pipes. We show in Section 5 how demand becomes less price responsive and farmers put more value on conservation technology after being encouraged to adopt hourly billing. Section 6 uses our combined findings to calculate a rough estimate of the environmental benefits from using this technology when water has a marginal price. Section 7 provides concluding remarks.

2 Experimental design to estimate the impact of the conservation technology

This section describes the experimental design and data collection for the first experiment to characterize the impact of conservation technology on water usage and farm profitability. In particular, we estimate these impacts across a wide geographic region, covering places where water is priced by cropped area and others where it is priced by the hour of pumping.

Sampling

The experiment took place in three districts: Mymensingh, Rangpur, and Rajshahi (see Figure A1 for a map). There is considerable variation in the way water is priced in these three regions. The groundwater table is deeper in Rajshahi and Rangpur. Hence, tube wells are costly to dig and therefore almost always government owned. Within these tube wells in Rajshahi, water is priced volumetrically where farmers can pay for each hour of pumping using a prepaid card. The card is loaded with funds at local shops in the same way that mobile phones are loaded with air time. The farmer can then obtain water by providing his card to a tube well operator — known locally as the "deep driver" — who is employed by the responsible government agency to manage the system. Farmers in our sample villages in Rangpur pay a per-acre fee for the right to irrigate their field for the entire season. They simply arrange each irrigation with the tube well operator. Finally, tube wells in Mymensingh are privately owned because a shallower groundwater table reduces the cost of digging a borehole. Tube well owners in this area largely use per-acre charges. Contracts occasionally take the form of two-part tariffs where the per-acre fee is coupled with a charge for each unit of fuel or electricity used during pumping. We assume that the farmer faces a volumetric price if he resides in a village with a prepaid pump or if he is responsible for the fuel costs of pumping. Farmers not facing volumetric prices pay a fixed seasonal fee per acre cultivated. They do not pay labor costs for applying irrigation. Instead, the tube well operator employs "linemen" who manage irrigation for the entire command area.

We first identified 12 upazilas (administrative units two levels above villages) in these three districts.[11] In Rajshahi and Rangpur, we obtained a list of villages where water is sold to farmers from government-operated deep tube wells.[12] All villages in Mymensingh were included in the sampling frame since each village usually has at least one tube well owner that sells water to other farmers. Using this sampling frame, we drew a random sample of 400 villages — split evenly across the three districts.

Field staff visited each selected village to ensure that farmers were growing rice during the boro (dry) season. If not, then the village was replaced with a randomly drawn village from the same upazila.[13] Once deemed eligible, the teams worked with a village leader to identify 10 farmers that were cultivating land near the village tube well.[14] For each of these farmers, the plot located closest to the tube well was mapped out. We refer to this plot as the "study plot" for the remainder of the paper.

Data collection and treatment assignment

Each of the 4,000 farmers were visited for a baseline survey in November-December of 2016. The survey collected information on household demographics, agricultural production, water management and water prices for the study plot and one other randomly selected plot of each farmer. Farmers almost entirely plant two rice crops — one in the rainy (*aman*) season and another in the dry (*boro*) season. Precipitation is rare during the boro season and therefore rice cultivation requires irrigation.

We randomly assigned each village to one of two groups prior to the start of boro cultivation in 2017 — with stratification at the upazila level.[15] Our field staff visited the 200 treatment villages during the period between planting and 10 days after planting. These visits took place from January to March, depending on village-specific planting dates. They trained the 10 farmers on the purpose of AWD and how to use it. Most importantly, they instructed farmers on the precise timing of when to practice AWD during the season. After the training, field staff provided each of the farmers with an AWD pipe. Staff then visited the study plots with the group of farmers and assisted with installation.[16] Nothing was done

[11] Each of these upazilas has 260 villages on average.

[12] A government agency, the Barind Multipurpose Development Authority (BMDA), maintains the tube wells and irrigation canals and employs the tube well operator.

[13] Replacement occurred in less than 10 percent of villages (36 out of 400).

[14] In the event that a village had more than one tube well, mostly in Mymensingh, survey teams selected the tube well with the largest command area.

[15] We knew that almost all the variation in volumetric pricing exists across upazilas, making it unnecessary to stratify by both upazila and volumetric pricing.

[16] Installation is close to costless. It simply requires inserting the pipe deep enough into the mud to allow the farmer to periodically monitor soil moisture up to 15 centimeters below ground.

in the remaining 200 villages which serve as a pure control.

Figure A2 shows an AWD pipe on one of the study plots. The plastic PVC pipe is open at both ends and has holes drilled into the sides, allowing the farmer to observe moisture below the soil surface. Rather than keep the field flooded to ensure continuous absorption by the plant, the farmer can use the pipe to determine when the below-ground water level falls below a 15 centimeter trigger. The field should be irrigated at this time and the process can be repeated until the crop starts to flower, i.e. the reproductive stage begins. The crop needs constant water during this flowering period and therefore farmers should stop implementation of AWD at this time.[17] The guidelines suggest that the practice of alternatively wetting and drying can be resumed after flowering stops and until the field is drained before harvest.

Table A1 shows summary statistics and demonstrates covariate balance. Note that baseline knowledge of AWD is low. Only about 17 percent of farmers had heard of AWD and nobody was using the technology at baseline. This suggests that AWD usage in the control group — at least in terms of using a pipe to monitor soil moisture and plan irrigations — should be low.[18] More importantly, just over a third of the farmers face a nonzero marginal price for water. This variation is mostly across upazilas, rather than within. Specifically, 89 percent of farmers in Rajshahi reside in villages where prepaid irrigation cards are used to pump water by the hour. About 15 percent of farmers in Mymensingh face a two-part tariff where they are responsible for fuel costs. This variation in the sample lets us observe how farmers exposed naturally (although not randomly) to volumetric pricing use AWD relative to those facing the more standard seasonal contract. Table A2 shows that observable covariates remain balanced within this subsample exposed to volumetric pricing.

The experiment required objective measurement of water usage. However, no villages in our sample were equipped to measure individual-level pumping volumes. We therefore designed a unique data collection strategy to observe water usage without individual meters. Survey teams visited each of the study plots on two randomly chosen and *unannounced* days.[19] These visits enable us to observe whether the field was being dried and how much irrigation water stood in it. The random assignment of villages to days allows the treatment-control comparison to be made throughout the growing season. Having this ability is critical because the pipe should not be used during the reproductive stage of crop growth. Hence, visiting fields on random days gives us the ability to verify if the tool is being properly used

[17]This reproductive or flowering stage occurs around 60-80 days after planting.

[18]A farmer can of course dry his field without using the AWD pipe, as shown in the results that follow. The lack of uptake at baseline should be interpreted as a lack of usage of the pipe to facilitate this process, not evidence that farmers never dry their fields.

[19]The timing of these visits is balanced across treatment and control villages. Regressing the days after planting of the visit on the treatment indicator and strata fixed effects yields a coefficient of -0.65 days and a p-value of 0.54.

and whether its causal effect varies by the type of water pricing.[20] Appendix B uses data on hourly card usage from our second experiment to verify that observed water levels correlate with pumping activity during the previous four days.

Our teams then carried out a follow-up survey in July 2017 after the boro rice crop had been harvested and close to the time of planting for the next rainy season. This survey collected information on self-reported irrigation management, input use, crop yield, revenue, and profit. The data provide the basis for our calculations of profitability and treatment effects of the AWD technique on profit — both with and without volumetric pricing.

3 Results: Marginal prices and the causal effect of conservation technology

In this section we use the first-year experiment to estimate the causal impact of AWD technology on water management, input costs, and agricultural profits. Following our pre-analysis plan, we report the average effect across our entire sample as well as the differential effect for farmers with seasonal water charges versus those with volumetric pricing. The analysis on water use is further broken down by time of the growing season — based on the recommendation that AWD not be practiced during the flowering stage of crop growth.

Our preferred specification is therefore,

$$y_{ivs} = \beta_0 + \beta_1 Treatment_v + \beta_2 Volumetric_{ivs} + \beta_3 Treatment_v * Volumetric_{ivs} + \alpha_s + \varepsilon_{ivs}, \quad (1)$$

where y_{ivs} is the observed outcome for farmer i in village v and upazila s. The treatment indicator, $Treatment_v$, varies only at the village level. The indicator for volumetric pricing varies mostly across upazilas, but can occasionally vary within these strata.[21] We estimate equation (1) for the sample of 4,000 study plots, regardless of whether the farmer kept the AWD pipe in that field, chose to move it elsewhere, or removed it entirely — all of which

[20] The schedule for the measurement of water management included 8,000 observations. We obtained data for 7,596 of them (95 percent). The missing observations resulted from random measurement dates falling after harvesting was completed. Harvesting dates were estimated from information on planting dates and length of the growing cycle from the baseline survey. This is obviously an imperfect proxy for current-year harvesting dates and therefore explains why the data are missing for a small number of cases. Missing data due to this scheduling issue is balanced across treatment and control groups.

[21] Upazila fixed effects explain 77 percent of the variation in the indicator variable for volumetric pricing. The remaining variation within upazilas is largely due to three factors: 1) some villages in Mymensingh have a system where the tube well owner collects payment for the fuel used in pumping, while other nearby villages do not, 2) a few villages in Rajshahi did not have the prepaid card system for irrigation and 3) the tube well owner (who always faces a nonzero marginal price) may be part of the sample in Mymensingh villages.

happened rarely.

The average effect of the pipe on water management — across the entire sample — is both small and statistically insignificant. Table 1 shows in column 1 that the average study plot in treatment villages had only 0.06 cm less water standing in the field. Increased uptake of the AWD practice should increase the likelihood that study plots of treatment farmers are being dried, i.e. have no standing water in the field. Column 2 shows that the treatment increases the effect on drying by about 1.9 percentage points — or about 4 percent — but this average effect is noisy. It is also clear that farmers practice some form of the AWD technique without using PVC pipes: fields in the control group were dry 45 percent of the time. Thus, the correct counterfactual differs from the one used in agronomic experiments where water is maintained in the control field for the entire season.[22]

The rest of the table shows that the treatment is only effective for farmers who face volumetric water prices. In column 3, the pipe generates an effect on water levels only for farmers facing nonzero marginal prices. Introducing it in places with volumetric pricing lowers the amount of observed irrigation water by 0.43 centimeters, or an 18 percent decrease. The probability of a plot being dry also increases by 8.4 percentage points (19 percent). This heterogeneity result is robust to interacting treatment with a large set of baseline covariates (Appendix C). Finally, the third row of the table shows that the correlation between volumetric pricing and water use (within strata) is small and statistically insignificant. This result could be driven either by the limited variation within strata, or correlation between unobservables and volumetric pricing. We show in appendix Table A3 that the volumetric pricing indicator has a negative correlation with water levels and a positive correlation with the probability of dry fields when omitting strata fixed effects.

The proper usage of the tool also depends on the time during the growing season. Table 2 shows that treatment effects exist only during the first 70 days of the growing season. We pre-specified this split in the data to approximately divide the season into the time before and after the start of flowering. Farmers practice AWD during the time up to flowering. Treatment plots had about 13 percent less water during the first 70 days of the season (column 1). This effect also exists only with volumetric pricing. Turning to column 2, AWD causes water levels to be lower by 0.83 cm (31 percent) under volumetric pricing. Columns 3 and 4 show similar results with the indicator for dry fields. Treatment plots were 5.9 percentage points, or 19 percent, more likely to be dry during the first part of the season (column 3). Under volumetric pricing, column 4 shows that the treatment led to a

[22] Agronomic experiments generally compare AWD to "continuous flooding." This is a system where the farmer never lets the field go dry. The field is re-irrigated when water reaches a low level, but before evaporating entirely.

17.3 percentage point increase in the occurrence of dry fields (54 percent). In contrast, the treatment effect during this time is close to zero and statistically insignificant for farmers facing seasonal contracts. Columns 5-8 show that plots of treatment farmers were managed in the same fashion as those of the control group after the first 70 days of the growing season, regardless of the type of water contract. Therefore, farmers did follow the directions to stop practicing AWD during the time when crop water requirements are high.

These results are insensitive to the choice of splitting the sample using a threshold of 70 days: we show in Tables A4 - A7 that results are similar when we divide the season using a 60 or 80 day cutoff. The online appendix shows that we also detect treatment effects on self-reported water usage. We do not observe heterogeneous impacts when asking farmers how many times they irrigated their fields, but do when considering the number of times the field was drained (Table A8).

Combining these findings, Figure 2 demonstrates how treatment effects varied both across time and by type of water pricing. It shows nonparametric regressions of water levels (top panel) and the indicator for dry fields (middle panel) on days after planting, separately for treatment and control villages. The upper left panel shows that the technology caused a decrease in irrigation withdrawals during the pre-flowering period of crop growth — but only for farmers paying for water on the margin. The same estimates in the upper right panel establish that AWD had no impact on measured water levels for farmers facing seasonal charges. The middle panel shows a similar pattern with dry fields: we observe that introducing the pipe leads to a noticeable increase in drying in places with volumetric pricing during the early part of the growing season, but no changes are observed for the two thirds of farmers that pay for water on a seasonal basis. The figure also helps visualize how farmers conserve water when facing volumetric prices, even without AWD. Namely, farmers tend to keep fields dry after flowering, regardless of whether they are using the pipes.

The magnitude of our estimates is reasonable. In fact, the estimates line up with findings from agronomic trials — but only when prices are set volumetrically. Figure 3 shows 87 impact estimates reported in 26 different agronomic studies. The estimated water savings from these experiments range from 5 to 65 percent, with median savings of 27 percent. Our 19.2 percent effect on water levels when prices are volumetric — from Table 1 column 3 — falls right at the 25th percentile of the agronomic estimates. In contrast, the null effect with area-based pricing is outside the range of estimates from agronomic trials. This gap between the predicted effects from the laboratory and the field estimates has also been observed in the literature on energy efficiency (Fowlie, Greenstone, and Wolfram, 2018). In our case, the failure of markets to efficiently price water appears to be a critical factor causing the field-based RCT estimates to deviate from those in the laboratory.

Adoption of AWD only increases profit when water is priced at the margin.[23] Column 1 in Table 3 shows that the causal effect of AWD on profits per acre, in the absence of volumetric pricing, is close to zero and statistically insignificant. In contrast, the AWD technology increases profits by approximately 1,870 taka (about $23) per acre, or about 7 percent, when water has a marginal price. Columns 2-4 decompose the heterogeneous profit effect into three parts. First, the interaction effect on water costs is negative (column 2). The coefficient is not individually significant (p=0.12), but its magnitude suggests that a non-trivial share of the profit effect comes from a reduction in water costs. We show later that the reductions in water cost were concentrated among farmers that were actively using their own prepaid irrigation cards. Second, we see no effects on crop yield (column 3). This finding is consistent with multiple agronomic trials showing that the practice leaves yield unchanged (e.g., Belder et al. (2004) and Yao et al. (2012)). Third, we see a positive — but insignificant — effect on revenue which is being driven by a small effect on output prices since yield is unchanged.[24]

Columns 5-8 in Table 3 report similar results when all outcomes are measured in logs rather than levels. Overall, AWD leads to positive returns only when water is priced at the margin. This conclusion is robust to trimming outliers in the profit distribution, controlling for a broad set of baseline covariates, and interacting those covariates with treatment (Table A9). Consistent with the survey estimates, Figure A3 shows no difference in satellite-measured greenness between treatment and control plots. Despite using less water, the plots of treatment farmers appear no less green.

We provide estimates for other inputs in the online appendix (Tables A10-A14). Of course, these inputs are included in the profit calculation above. We find no direct or heterogeneous effects on aggregate expenditures for other non-water inputs (fertilizer, pesticides, herbicides, hired, and family labor). While the individual coefficients for some inputs are significant, the signs go in opposite directions — leading to the absence of any aggregate effects.

One possibility is that the treatment causes water (or other inputs) to be reallocated away from the study plot and towards other plots of the household. This would cause the treatment

[23] We measure revenue per acre by dividing the total output from the plot by plot size to obtain yield, regardless of how much of the output was sold or kept for consumption. We then multiply the yield by the output price for the 98.5 percent of farmers that reported selling output. We use the average sale price for the remaining 1.5 percent of farmers that did not sell any output. We collected input expenditures for fertilizer, pesticide, herbicide, water, planting labor, weeding labor, and harvesting labor. Labor inputs included both family labor and hired labor. We valued family labor by multiplying the number of person days by the daily wage rate from the survey.

[24] A regression with log price as the dependent variable yields an interaction effect of 0.018 and a t-statistic of 1.16 (regression not shown). Any modest effect of output prices would be consistent with a claim sometimes made that periodic drying of fields improves grain quality.

effect on household-level outcomes to differ from the plot-level outcomes we have observed thus far. To consider this, Table A15 shows treatment effects on a randomly selected plot for each farmer, other than the study plot. We find no evidence that the treatment causes water use or other inputs to increase on that plot. If anything, the treatment lowers water costs in areas with volumetric pricing, which is consistent with farmers irrigating more than one plot at a time.

Another concern is that our main finding is a false positive resulting from multiple outcomes being tested. We address this by adjusting the p-values for multiple inference in Table A16. Our main effect — that conservation technology only saves water with marginal prices — remains significant when controlling the false discovery rate using the methods in Anderson (2008). A more conservative test is to control the probability of making at lease one false rejection, as in List, Shaikh, and Xu (2016). The effects on self-reported water usage and objective measurements during the pre-flowering period continue to be significant with this alternative method.

Finally, we find that within Rajshahi district — where prepaid pumps allow water to be priced by the hour — some farmers do not have their own prepaid cards.[25] Instead, farmers rely on the deep driver (tube well operator) to use his card and then charge them a fixed seasonal price. This charge is a function only of acreage cultivated, and not the number of hours of pumping. The deep driver essentially averages out the total pumping cost over the entire command area and bills farmers accordingly. This local institution provides additional heterogeneity. In particular, the effect on water costs should be higher for farmers that hold their own cards and thus stand to gain by pumping less groundwater. We test this idea in the study villages in Rajshahi.[26]

Column 1 of Table 4 shows that AWD lowers water costs by about 931 taka — or 17 percent — for cardholders and has no effect for farmers that pay the deep driver for water. The effect on profits and log profits in columns 2 and 3 are noisier, but go in the same direction. AWD increases profit by 11 to 12 percent for farmers with cards, but has a smaller effect in villages where individual card ownership is absent. The system where farmers hold their own prepaid cards and pay for water by the hour is however not randomly assigned.[27] The observed heterogeneity could therefore result from factors correlated with card ownership, rather than card ownership itself. Columns 4 through 6 test whether the interaction effects

[25]Our baseline survey, and hence the analysis until this point, classified these farmers as paying volumetric prices because their village already had a prepaid pump installed.

[26]We did not know about this heterogeneity at the time of designing the study. Therefore, these estimates were not pre-specified in our analysis plan.

[27]Farmers who have their own cards are older, have larger households, own more livestock, are less likely to own their own private tube well, and report irrigating their field more often during the boro season at baseline.

are sensitive to interacting the treatment indicator with a large set of baseline characteristics. The interaction effects between the treatment and having an individual prepaid card remain similar — and actually increase — when allowing for the impact of the pipe to also depend on observable characteristics. The evidence further points to inefficient water pricing as a barrier to uptake of conservation technology.

Appendix C investigates which characteristics explain the most treatment effect heterogeneity we see in this first experiment. It presents two additional findings. First, the result that AWD technology only saves water with volumetric pricing is robust to interacting the treatment indicator with a large set of 17 other baseline covariates. This reduces worries that the heterogeneous effect is driven by correlates of volumetric pricing, at least for the observables we have in our data. Second, we use the machine learning methods in Chernozhukov et al. (2018) to ask the data which characteristics explain the most heterogeneity. This analysis points to volumetric pricing (in the overall sample) and owning an individual prepaid card (in the Rajshahi sample) as an important source of heterogeneity in the water savings induced by the AWD technology. Taken together, these results offer suggestive evidence that the heterogeneity we observe is due to volumetric pricing, rather than an unobserved covariate.

4 Experimental design to estimate the effect of hourly irrigation on demand for conservation technology

Building on the results from our first experiment, we designed a second experiment to randomly facilitate volumetric pricing and measure its effect on demand for the conservation technology. Our approach allows us to test whether farmers place additional value on conservation technology when they face marginal prices for irrigation. This section outlines the timing of events for this experiment.

In many villages, the ratio of prepaid irrigation cards to farmers is less than one. In some villages this phenomenon is extreme: the deep driver or water user's committee maintains a small number of prepaid cards, uses them to provide water to farmers, and then charges each farmer the same fee per acre. In effect, this local institution keeps water pricing on a per-acre basis, despite the fact that technology is in place for each farmer to pay for their pumping by the hour. Multiple factors may explain why individual card usage, and hence volumetric pricing, has not taken effect in these villages: it is costly and time consuming for farmers to obtain an individual card, coordination difficulties — i.e. problems in creating an efficient queueing system if each person is individually using a card, and concerns about fairness because some plots are far from the tube well and water is lost during transport due to the earthen canals used for conveyance. Combined with highly fragmented landholdings, this will result in differential prices per unit of actual water between farmers and plots as well. Our treatment targets the fixed costs of obtaining a card as a barrier to individual ownership.

We first identified 144 villages in Rajshahi district — not included in the sample of our first RCT — where farmers were not using their own prepaid card for pumping. These villages are spread across three upazilas, two of which were included in our first experiment. Field staff worked with a local village leader in November 2017 to identify 25 farmers cultivating rice during the boro season in each of these villages. The villages were then randomly divided into two groups. 96 were assigned to a treatment group where we sought to increase the share of farmers paying for irrigation by the hour by using their own cards: the remaining 48 serve as a control group that retained the status quo of seasonal charges.

Field teams started by organizing a meeting with these 25 farmers. These meetings took place in December 2017 and served four objectives. First, a short baseline questionnaire was administered. Second, farmers were instructed on how the irrigation system can be operated with the individual cards. Third, our field staff explained to farmers that their local NGO was running a program to help with applying for the prepaid card. Specifically, the field staff assisted each farmer in filling out the application form — including obtaining a passport-style photo to be printed on the card. Fourth, there is an application fee of 150 Bangladeshi Taka (around $1.8) to be paid at the time of submitting the application. Farmers were instructed that the program would be covering these costs. In addition, our partner delivered the application forms to the local upazila office of the agency responsible for producing the cards, collected the printed cards when they were complete, and delivered them to each treatment village prior to planting. Overall, 2,279 of the 2,400 (95 percent) farmers in the treatment group agreed to receive the cards as part of the program.

Our design sought to eliminate the possibility that any future behavior could be a function of the small 150 Taka gift to cover the application cost. Therefore, we provided each of the 25 farmers in the control group with 150 Taka of mobile phone credits right after administration of the baseline survey.[28]

Table A17 shows baseline characteristics for the treatment and control groups in this second RCT. Household and farm characteristics are generally similar across the two groups. The average farmer in this sample pays around 1500 taka (approximately $18) to irrigate one bigah of land (a bigah equals one-third of an acre). 70 percent pay this money directly

[28]We chose mobile phone credits to make the funds equally illiquid between the treatment and control groups.

to the deep driver as a per-bigah fee. The remaining 30 percent pay the fee to a water users committee.

Does this effort to introduce volumetric pricing cause farmers to place greater value on conservation technology? To get at this question, we conducted a revealed-preference demand experiment in all 144 villages. A sales person visited each of the 25 farmers in January or early February 2018, depending on the planting dates in the village. S(he) gave each farmer the opportunity to purchase an AWD pipe at a randomly determined village-level price. We let the price range from 20-90 taka. As points of reference, the daily wage for casual agricultural work during the previous boro season was about 350 taka. The estimated profit advantage of the pipe was about 561 taka per plot — when farmers faced nonzero marginal prices for water. Farmers who bought the pipe were required to pay cash. The pipe was handed to the farmer, along with instructions on its use, immediately after purchase. Unlike in the first RCT, field staff did not provide any further training or assistance with actually installing the AWD pipe.

In addition to observing these purchasing decisions, and tracing out the demand curve with and without the introduction of individual volumetric water pricing, we collected data on whether the pipe was installed and water levels in the field. Similar to our first RCT, we randomly drew dates to visit each of the 144 villages. These dates were drawn to fall mostly in the 10-70 day period after planting, when we observed farmers from the first experiment practicing AWD.[29] During each visit, the enumerator checked all the plots of each farmer to see if an AWD pipe was being used. In addition, water levels were measured on the plot closest to the tube well for a random 75 percent of farmers and the farthest plot for the rest of the sample. These additional data allow us to decompose any treatment effects into effects on initial valuation at the time of purchase and actual usage during the season.

5 Results: Hourly irrigation and the demand for conservation technology

We start by showing some descriptive "first stage" evidence that some farmers did use the prepaid cards. The experiment was carried out in three upazilas, one of which provided us complete data on card usage for the 800 treatment farmers. We found that 40.3 percent of them (323) loaded their card at least once during the period from January 12th to August 7th, 2018. The median farmer — conditional on loading at least once — spent 3,000 taka ($37.5 or the equivalent of irrigating about 2 plots with seasonal charges) and loaded the card

[29] The visits took place during February 2nd - May 23rd 2018, with the median visit occurring on April 1st.

five times. These distributions have a substantial right tail: a farmer at the 90th percentile reloaded the card 22 times and spent 21,800 taka.

Does the demand curve for AWD change when farmers are encouraged to pay for water by the hour of pumping? To answer this question, we combine the random variation in village-level AWD prices with the random encouragement of prepaid card usage. The main specification is,

$$Adoption_{ivs} = \beta_0 + \beta_1 Card_{vs} + \beta_2 Price_{vs} + \beta_3 Card_{vs} * Price_{vs} + \alpha_s + \varepsilon_{ivs}, \qquad (2)$$

where $Adoption_{ivs}$ is an indicator for whether farmer i purchased the pipe, $Card_{vs}$ equals one if village v in upazila s is one of the 96 prepaid card villages, and $Price_{vs}$ is the random price offered in the village. As in our previous analysis, standard errors continue to be clustered at the village level.

Figure 4 shows the fitted demand estimates from (2) as lines with the raw adoption rates as dots. Shifting farmers to hourly charges reduces price sensitivity for conservation technology. Our lower prices result in high take up rates and no statistical difference between the prepaid card treatment and control. About 65 percent of farmers in the control group purchased pipes at the lowest four prices: this rate remains roughly the same in treatment villages. In contrast, introducing hourly irrigation cards caused demand to increase at higher prices. Only 21 percent of farmers in the control group purchased pipes when priced at 60 taka or higher. Hourly pricing increased purchases by approximately 35 percent at these four higher prices.

Two additional results are apparent in Figure 4. First, demand is elastic. The demand elasticity in the control group is about 1.7 at the midpoint price of 55 taka. Delta-method standard errors lead to a rejection of unit elastic demand in the control. This result is consistent with the common finding that demand for improved technology in developing countries is highly price sensitive — even for technologies proven beneficial. As examples, experimental estimates of demand show high sensitivity to prices for health technologies in Kenya (Kremer and Miguel, 2007; Dupas, 2014b) and crop insurance in Ghana (Karlan et al., 2014). This demand elasticity suggests that even modest subsidies have the potential to induce large increases in the demand for AWD.

Second, willingness to pay for AWD is low when compared to both the profitability of the technology and the estimated marginal production cost. In the first experiment, AWD with volumetric pricing increases profits by about 1,870 taka per acre. The median plot in our first-year sample is 0.3 acres, implying that using an AWD pipe on a single plot increases profits by about 561 taka — a value well above what farmers are willing to pay.[30] We estimate the marginal cost of AWD production to be 133 taka — based on surveys conducted with 10 engineering shops.[31] Our findings show no demand at this price, even after promoting hourly pricing for water. However, the socially optimal price of the pipe depends on its external benefits. These may include reduced greenhouse gas emissions from electricity, reduced methane emissions from rice fields, and the social benefit of the groundwater not extracted and available to others, discussed later in Section 6.

Table 5 shows the corresponding regression results. Column 1 gives the average treatment effect across all price levels. The irrigation card treatment led to an increase in the purchasing rate by about 4.3 percentage points, or roughly 10 percent. The average effect is indistinguishable from zero due to the significant heterogeneity across price levels. Column 2 provides the main estimates corresponding to the specification in (2). Demand for water-saving technology is less responsive to price in villages where we introduce hourly irrigation cards. Increasing the price by 1 taka leads to a 1.29 percentage point decrease in adoption without volumetric pricing. This price responsiveness falls significantly by 0.34 percentage points when we facilitate volumetric pricing. The demand elasticity at a price of 55 taka — reported at the bottom of column 2 — falls by 33 percent from 1.7 to 1.14 with the prepaid card treatment. This difference in elasticities is statistically significant at the one percent level.[32] We obtain similar results when prices are measured in logs (Table A18). Overall, introducing a pricing mechanism that puts a marginal price on water increases farmers willingness to pay for water-conserving technology.

The estimated demand curves can be used to calculate the gain in consumer surplus that results from encouraging volumetric prices.[33] Figure 5 shows the percentage increase in consumer surplus between farmers with and without hourly irrigation cards. For instance, when priced at 55 taka — the median price in our demand experiment — nudging farmers to adopt volumetric pricing causes consumer surplus from conservation technology to increase by almost 64 percent. These gains in consumer surplus are largest at higher prices, as seen in Figure 4.

While take up is reasonably high, when measured by purchasing an AWD pipe, instal-

[30]Similar observations have been made in the health and development literature: revealed willingness to pay for water purification in Ghana is orders of magnitude below the estimated benefits to households (Berry, Fischer, and Guiteras, 2018).

[31]Field staff visited each shop in June 2018 and asked the owner for a quote to produce two different randomly selected quantities of AWD pipes. Regressing the estimated quotes on quantity delivers a coefficient of 133 taka.

[32]We rely on delta-method standard errors for this statistical test since the elasticities (and their difference) are a non-linear function of the parameter estimates.

[33]Using the estimates from Equation 2, the consumer surplus at a given price p in the control villages is $\frac{-\beta_0^2}{2\beta_2} - \beta_0 p - \frac{\beta_2 p^2}{2}$. The consumer surplus in prepaid card treatment villages is $\frac{-(\beta_0+\beta_1)^2}{2(\beta_2+\beta_3)} - (\beta_0+\beta_1)p - \frac{(\beta_2+\beta_3)p^2}{2}$.

lation and use of the pipe is modest. Only 18.4 percent of purchasing farmers installed the AWD pipes on one of their rice plots.[34] Anecdotally, there are numerous explanations for not installing AWD. Farmers sometimes report having lost the pipe between the time of purchase and planting. Some farmers reported that they would install the pipe "in a few days."[35] After conferring with others, some farmers suggested that it was not feasible to use AWD individually because of coordination externalities. Two examples were common. Farmers with low-lying land often get water that spills over into their plot when it is being pumped into a nearby higher field. Also, a common per-acre water price makes it easy for the tube well operator to irrigate multiple fields at a time. Adoption of AWD by a subset of the farmers becomes less practical when each farmer does not have full control over when their field is irrigated.

The low take up in our second experiment presents a puzzle when considering the results from our first experiment. Unlike our first experiment, the second RCT took place in villages where farmers were previously not using individual prepaid cards. Some of the coordination difficulties mentioned above may explain both the lack of individual card usage before the experiment and the low uptake of AWD. The intervention in our first experiment included assistance with installing the AWD pipe. The large gap between purchasing and using AWD in the second experiment might also highlight the importance of basic training and installation support to ensure that the full benefits of AWD are realized.

Figure 6 shows that despite the low rate of installation, the unconditional price-usage relationship remains steeper in prepaid-card villages. The dashed lines in the figure show usage (installation), while the solid lines show the demand curves (purchasing). At prices above 60 taka, only 1.4 percent of farmers installed AWD in control villages. Approximately 7.4 percent did so in treatment villages. The regression estimates in columns 3-4 of Table 5 provide exact magnitudes. In column 3, increasing price by one taka (about 1.8 percent of the midpoint price of 55 taka) causes a decrease in the usage rate by 0.16 percentage points, or 2.3 percent of the mean usage rate amongst control villages. Column 4 again shows the heterogeneity in price responsiveness. A one taka price increase causes a decrease in adoption by 0.33 percentage points in control villages and 0.10 percentage points in treatment villages.

[34] A low rate of usage, conditional on purchasing, has been observed for fertilizer trees in Zambia (Jack et al., 2015) and improved latrines in Cambodia (Ben Yishay et al., 2017). The literature on technology adoption of health products, on the other hand, has generally found larger rates of follow-through (Dupas, 2014a).

[35] Farmers that purchased pipes were told that AWD should be practiced starting 10 days after transplanting. The date of the verification survey was randomized and survey teams arrived less than 10 days after planting in fewer than one percent of cases. Moreover, the rate of uptake (conditional on purchasing) is only 20 percent for the farmers that were visited more than 50 days after transplanting. Therefore, procrastination, combined with our surveys being early in the season, cannot fully explain the low rate of installation.

While the interaction term is not quite statistically significant (p=0.135), the point estimate shows that around two thirds of the price responsiveness in control villages is eliminated when introducing hourly pricing. The estimated elasticities at the bottom of the table make this clear. The price-usage elasticity in control villages is 2.58 and this falls by over 75 percent to 0.6 in treatment villages. The difference between the two elasticities is highly significant. The online appendix further shows that these results are more precisely estimated when accounting for the binary nature of the dependent variable with logit regressions (Table A19).

The difference in elasticities appears to result from how the prepaid cards change the screening ability of prices. Among farmers who purchased a pipe, the correlation between price and usage is significantly larger in prepaid card villages (Table A20). In fact, this correlation is negative in control villages and weakly positive in prepaid card villages. Screening offers one potential explanation. The prepaid cards put a marginal price on water. Realizing this, farmers carefully evaluate the merits of the AWD pipe. The farmers induced to buy the pipe at higher prices are those that value them most and are the ones most likely to install. In contrast, prices for conservation technology do not screen effectively in the absence of volumetric water pricing because farmers stand to gain little from using the pipe for irrigation.[36]

Despite the effects on the demand curve for water-saving technology, we do not find differences in water management for prepaid card and control villages (Table 6). This could occur for a few reasons. First, the low usage rate of AWD limits our ability to detect any direct effects coming through usage of the pipes. Second, there might be other frictions that prevent the prepaid cards from leading to direct changes in water management. For example, use of the prepaid cards may be limited by concerns for equity in the community: the traditional earthen ditches used to move water leak and this makes paying by the hour costlier for farmers located farther from the tube well. Or, the deep drivers (tube well operators) benefitting from the per-acre pricing system may have resisted usage of individual cards by farmers. Finally, it may take time for farmers to learn how the prepaid cards work and to adjust behavior. We find some evidence that actually using the card is correlated with using less water later in the season in the one upazila where we have data on card usage (Figure A4). However, this finding is only correlational. In sum, covering the signup costs for hourly irrigation changes how farmers initially value a conservation technology, but there

[36]Sunk costs represent another reason why price would be positively correlated with usage. People may use a product more if they paid a higher price to avoid the feeling of "wasting" their investment. Empirical research from health products in Zambia finds no evidence for this behavioral explanation and instead finds evidence for screening (Ashraf, Berry, and Shapiro, 2010). Other work on health products finds no relationship between price and usage, conditional on adoption (Cohen and Dupas, 2010; Tarozzi et al., 2014).

are likely other constraints that need to be addressed before any physical water savings can be realized.

5.1 Alternative Explanations

The prepaid cards transitioned farmers from a zero to a positive marginal price for water, leading them to place more value on the water-saving technology. This interpretation is consistent with findings from our first experiment where farmers only used and benefited from the technology when facing marginal prices. But other factors could explain why the treatment in the second experiment changed the demand for AWD. For one, farmers now had to pay for water up front, rather than throughout the season.[37] Jack and Smith (2020) discuss a number of mechanisms that might explain why South African households use less electricity when converted to a prepaid meter. These include 1) an increase in the effective price of electricity because people can no longer default 2) a tighter liquidity constraint that forces people to pay before consuming, and 3) the increased salience of pricing making it easier to observe when consumption is about to cause a change in marginal prices with increasing-block tariffs.[38] While we cannot definitively conclude that it is the switch to volumetric pricing that drives our results, we argue below that some of these alternative mechanisms seem less likely in our setting.

First, discussions with deep drivers during our fieldwork suggested that default on water bills is rare, mostly because of dynamic incentives. Farmers that do not pay can be cut off from future water access for dry-season rice, which is the main income source for most. This explains why default was rare for the seasonal contracts that existed before our treatment.

Second, liquidity constraints could be important in our context. By forcing them to pay for water up front, the prepaid cards exacerbate liquidity problems for farmers that previously could pay for at least part of the water bill after harvesting. If this is important, then the treatment should cause liquidity constrained farmers to place greater value on the technology. Appendix D tests for interaction effects between the card treatment and various proxies for liquidity constraints. We do not find any evidence that supports the liquidity explanation.

Third, reloading the cards to pay in advance might make prices more salient for farmers, causing them to opt for conservation technology. But in our context farmers chose whether or not to purchase AWD pipes before the season, i.e. before they had started using the cards.

[37] The agreements which existed prior to our treatment often involve informal credit where the water user pays the per-acre fee in installments, one at the beginning of the season and another after the harvest.

[38] Residential electricity is often priced in blocks where the marginal price jumps up at pre-determined consumption thresholds.

Thus, any increase in salience from using the cards could not have taken place at the time AWD pipes were purchased. This differs from Jack and Smith (2020) who find that using prepaid cards for household electricity makes people more aware of their consumption and when they are close to crossing into a higher price bracket.

6 Marginal prices and the environmental benefits of conservation technology

This section briefly considers the environmental benefits of AWD. First, the technology reduces groundwater extraction which lowers electricity demand and therefore greenhouse gas emissions from electricity generation. Ideally, electricity should be priced at its marginal social cost, which would include the negative externalities from electricity generation. However, taxing electricity has proven to be elusive in practice. In the absence of a socially optimal electricity price, subsidizing energy efficiency is a second-best alternative to reducing these externalities (Allcott and Greenstone, 2017).

We quantify one part of such a subsidy by approximating the dollar value of reduced carbon emissions from an installed AWD pipe. We base our estimate on both the results from the experiment and additional data we collected for this purpose. The remainder of the section describes the different steps of this computation.

Reduced groundwater pumping: We do not have survey measures of pumping hours to compare treatment and control farmers from our first experiment. However, column 1 in Table 4 finds that AWD reduces water costs by 931.1 taka per acre for farmers with hourly irrigation cards. The median plot size is 0.3 acres and the cost per hour of pumping is 120 taka. Combining these three figures delivers an estimated savings of 2.3 hours of pumping per AWD device.

Electricity consumption per hour of pumping: We sent enumerators to 26 random villages in March/April 2018 to observe electricity usage by monitoring electric meters during tube well operation. We use the starting and ending time of operation, combined with electricity consumption, to estimate an electricity usage of 18.1 kilowatt hours (kwh) per hour of operation.[39]

Electricity produced per unit of consumption: The ratio of electricity produced to consumed in Bangladesh is 1.14. We adjust this number following Borenstein and Bushnell (2018) to allow for 75 percent of the transmission losses to be attributed to electricity flowing through power lines, while the other 25 percent are fixed and independent of consumption.

[39]As a benchmark, annual household electricity consumption per capita in Bangladesh is about 300 kwh.

We therefore end up with 1.105 kwh of production needed per kwh of consumption.

Marginal CO_2 emissions from electricity production in Bangladesh: A reduction in electricity demand for irrigation reduces CO_2 emissions from generating electricity. Marginal CO_2 emissions from electricity depend on a number of factors, including the type of fuel and the efficiency of power plants. Ideally, we need data from Bangladesh power plants with repeated observations on plant load and emissions. Such an approach has been used to estimate marginal emissions rates from electricity in the United States (Zivin, Kotchen, and Mansur, 2014; Holland et al., 2016). Without this data for Bangladesh, we instead use annual panel data from about 3,900 U.S. power plants to estimate marginal CO_2 emissions as a function of fuel type and thermal efficiency of the plant (see Table A21 for regression results). We then obtain these two characteristics (fuel type and efficiency) for the universe of Bangladesh power plants and estimate marginal emissions per plant using the regression estimates from U.S. plants. We take the average of plant-level marginal emissions where each plant is weighted by its share of annual electricity generation for the whole country.

This approach delivers a marginal emissions rate of 1.4 lbs of CO_2 per kwh of electricity. This number is roughly on par with CO_2 emissions generated by the electricity grid in the eastern United States (Zivin, Kotchen, and Mansur, 2014). The estimate is also similar to the grid emission factor released by the Bangladesh Department of Environment in 2014 (1.47 lbs per kwh).

Social cost of carbon: We use the estimate in Nordhaus (2017) which is 31 US$ per ton of CO_2.

Combining these figures, the estimated one-year benefit of the pipe on a single rice plot — due to reduced carbon emissions from electricity — is 79.91 taka. This annual benefit represents about 60 percent of the marginal cost of production. Moreover, these are not the only external benefits of the technology. Agronomic studies find that adopting AWD lowers methane emissions from rice by approximately 50 percent (Ole Sander, Samson, and Buresh, 2014; Xu et al., 2015).[40]

An additional social benefit of AWD is in valuing the groundwater that is not pumped, and remains in the aquifer for future use, which delivers benefits to other farmers relying on the same groundwater source. To approximate these benefits, we first need to compute the volume of water saved by the treatment. The calculations above suggest that the method reduces pumping times by 2.3 hours per plot. The standard government deep tube well has a capacity of 1 cusec, i.e. 1 ft^3/sec or 101.941 m^3/hr. Thus, a reasonable estimate of averted pumping by using AWD on a single plot is 234.46 m^3 or 0.19 acre feet of water. Column 3

[40]We attempted to measure methane gas on a sample of 104 plots from the first experiment. A malfunction in our partner's gas chromatograph delayed analysis of the samples and made these results unreliable.

of Table 1 shows water savings of about 18.3 percent, suggesting total water use of 1.04 acre feet for the rice plots in our sample. A conservative agronomic estimate of the return flow for rice is 25 percent (Qureshi et al., 2010). That is, 25 percent of the averted pumping caused by AWD is water that would have returned to the aquifer anyway. Thus, an estimate of the true water savings is 75 percent of the averted pumping, or 0.1425 acre-ft.[41] This volume of water is not trivial. It represents about half of the mean annual household residential consumption in the United States.

What is the value of this conserved groundwater? The average value of water in rice farming in our sample can be obtained by multiplying the profit per acre from column 1 of Table 3 (which is 27,133 taka) by plot size (0.3 acres) and dividing by total water use (1.04 acre-ft) which gives 7,827 taka per acre-ft of water. This is approximately $93 per acre-ft, which is high for a developing country, but shows the value of water for dry-season rice in Bangladesh. Our estimate of the value of conserved water from using AWD on a single plot is therefore 1,115 taka per year ($13.9). The estimated benefits from water conservation are an order of magnitude greater than the benefits from reduced CO_2 emissions.

In summary, available conservation technology can deliver substantial environmental benefits. However, farmers valuing the technology, and using it properly, depends on water having a marginal price.

7 Concluding Remarks

Agriculture in developing countries uses a large share of the world's water. Agricultural water conservation is complex because in many settings, irrigation water has no marginal price. Introducing marginal prices is not easy because small farm sizes make individual metering costly. Moreover, pricing agricultural water remains a sensitive issue for elected officials that desire to retain the support of farmers. There have been no experimental studies that test policy mechanisms for putting a marginal price on water.

In this paper, we carry out two RCTs to study the link between adoption of conservation technology and marginal prices for irrigation. We make use of a simple technology (a perforated pipe) that is known to reduce water use by about 30 percent in rice farming. In the first experiment, we randomly provide 2,000 farmers with these pipes and observe that the technology only conserves water in areas where farmers face a volumetric price for water. Relative to the control group, plots of these farmers have 19 percent less water and are 21

[41] We arrive at a similar figure when using an estimate of the water requirement for rice (2,500 liters of water per kilogram of output in Bouman (2009)) and the average rice yield in our sample of 2,269 kg per acre from column 3 of Table 3.

percent more likely to be dry when observed on random days. Farm profits increase by 7 percent.

Our second RCT experiments with the introduction of volumetric pricing. We do so by facilitating the adoption of debit cards that allow farmers to buy water by the hour from the village tube well. The treatment — which reduces the application costs for such a card — increased ownership to 95 percent. About 40 percent of treatment farmers eventually buy water with the card. We find that encouraging volumetric prices in this fashion alters the demand for the water-saving technology. Demand elasticity in the treatment group falls by 33 percent. Purchase of the technology went up by 35 percent at the highest prices. Consumer surplus at the median price increased by over 50 percent. However, this increased demand for technology did not translate into water savings, suggesting the presence of other frictions that prevent the cards from saving water.

These results provide valuable insights on the issue of resource conservation in a world with a warming climate. Policymakers have several options for preserving agricultural water. These include supporting efficient irrigation technology, encouraging better irrigation scheduling, and promoting crops that require less water. Our results show that farmers have greater incentives to invest in conservation when they face marginal prices for irrigation water. However, more work needs to be done to resolve the additional frictions that may prevent these investments from generating water savings at the farm level.

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Tables

Table 1: Effects of conservation technology on water usage

	(1) Level	(2) Dry	(3) Level	(4) Dry
Treatment	-0.061	0.019	0.119	-0.012
	(0.161)	(0.023)	(0.220)	(0.027)
Treatment * Volumetric Pricing			-0.544* (0.287)	0.096* (0.050)
Volumetric Pricing			-0.107	-0.058
			(0.333)	(0.060)
Strata Fixed Effects	Yes	Yes	Yes	Yes
Mean in Control	2.32	0.45	2.32	0.45
p-Value: Treat+Treat*Volumetric			0.021	0.047
Number of Observations	7598	7598	7596	7596
R squared	0.033	0.035	0.036	0.037

The data are from random unannounced visits to the study plots of sample farmers during the 2017 boro (dry) growing season. The dependent variable in columns 1 and 3 is the amount of standing water in the field, measured in centimeters. The dependent variable in columns 2 and 4 is an indicator variable for a dry field with no standing water. Volumetric pricing is an indicator for farmers for whom the water price is tied to usage, either through hourly charges or fuel payments. Standard errors are clustered at the village level. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table 2: Separate effects of conservation technology by time of growing season and volumetric pricing

	0-70 Days After Planting				70+ Days After Planting			
	(1) Level	(2) Level	(3) Dry	(4) Dry	(5) Level	(6) Level	(7) Dry	(8) Dry
Treatment	-0.350**	-0.048	0.059**	-0.012	0.250	0.258	-0.021	-0.003
	(0.152)	(0.208)	(0.027)	(0.032)	(0.286)	(0.376)	(0.033)	(0.039)
Treatment * Volumetric Pricing		-0.788***		0.185***		0.014		-0.071
		(0.287)		(0.054)		(0.474)		(0.075)
Volumetric Pricing		0.026		-0.082		-0.488		0.023
		(0.363)		(0.065)		(0.420)		(0.066)
Strata Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	2.71	2.71	0.32	0.32	1.86	1.86	0.59	0.59
p-Value: Treat+Treat*Volumetric		0.000		0.000		0.328		0.244
Number of Observations	4188	4187	4188	4187	3410	3409	3410	3409
R squared	0.020	0.027	0.035	0.043	0.085	0.086	0.113	0.114

The data are from random unannounced visits to the study plots of sample farmers during the 2017 boro (dry) growing season. Columns 1 and 2 are for measurements taken up to 70 days after transplanting. Columns 3 and 4 are for measurements taken more than 70 days after transplanting. The dependent variable in columns 1 and 3 is the amount of standing water in the field, measured in centimeters. The dependent variable in columns 2 and 4 is an indicator variable for a dry field with no standing water. Standard errors are clustered at the village level. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table 3: Effects of conservation technology on costs, revenues, and profits

					Log:			
	(1) Profit	(2) Water Cost	(3) Yield	(4) Revenue	(5) Profit	(6) Water Cost	(7) Yield	(8) Revenue
Treatment	-338.316	133.089	4.051	178.885	-0.036	0.023	-0.001	0.001
	(901.831)	(124.857)	(30.189)	(820.028)	(0.046)	(0.024)	(0.014)	(0.017)
Treatment *	2205.179*	-435.469	10.295	1222.695	0.122**	-0.081	0.009	0.029
Volumetric Pricing	(1293.913)	(279.224)	(37.221)	(1153.819)	(0.061)	(0.053)	(0.017)	(0.022)
Volumetric Pricing	-711.475	372.471	27.800	350.272	-0.123*	0.066	0.013	0.003
	(1332.595)	(226.509)	(37.246)	(1321.963)	(0.070)	(0.043)	(0.018)	(0.028)
Strata Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	27133.39	4897.18	2269.16	52696.04	10.12	8.45	7.71	10.85
p-Value: Treat+Treat*Volumetric	0.049	0.226	0.515	0.091	0.035	0.225	0.417	0.045
Number of Observations	3982	3983	3982	3982	3932	3983	3982	3982
R squared	0.298	0.365	0.352	0.390	0.273	0.347	0.329	0.351

The data are taken from the followup survey after harvesting. The dependent variables are profit per acre (column 1), water cost in taka per acre (column 2), crop yield in kilograms per acre (column 3), and revenue in taka per acre (column 4). Columns 5 through 8 show the same regressions with the log of profit, water cost, yield, and revenue, respectively. Standard errors are clustered at the village level. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table 4: Effects separately by card ownership in villages with prepaid irrigation pumps

	(1) Water Cost	(2) Profit	(3) Log Profit	(4) Water Cost	(5) Profit	(6) Log Profit
Treatment	108.3	1210.6	0.0260	-76.76	350.8	-0.00565
	(358.0)	(1202.1)	(0.0438)	(408.3)	(1571.8)	(0.0527)
Treatment * Has Card	-1039.4**	2524.1	0.112	-1164.6**	3793.0**	0.147**
	(485.1)	(2074.7)	(0.0773)	(472.1)	(1827.4)	(0.0646)
Has Card	1184.3***	-1253.7	-0.0722	1321.4***	-2246.7	-0.0868
	(409.1)	(1872.9)	(0.0708)	(411.2)	(1576.9)	(0.0560)
Strata Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	No	No	Yes	Yes	Yes
Treatment*Covariates	No	No	No	Yes	Yes	Yes
Mean in Control	5611.93	29999.22	10.27	5608.07	30025.35	10.27
p-Value: Treat+Treat*Has Card	0.006	0.028	0.030	0.000	0.009	0.007
Number of observations	1340	1340	1332	1337	1337	1329

The data are from the follow up survey and are limited to the Rajshahi district where some farmers have their own prepaid irrigation card to pay for water by the hour. The variable "Has Card" is an indicator variable for farmers that report having their own prepaid card. The dependent variables are the cost of water per acre (columns 1 and 4), profit per acre (columns 2 and 5), and log profit per acre (columns 3 and 6). Columns 4-6 include demeaned farmer covariates from baseline and interactions between these demeaned covariates and the AWD treatment indicator. The covariates included are all of those in Table A1 (age, years of education, household size, number of livestock owned, landholdings, television ownership, refrigerator ownership, tube well ownership, indicator for knowledge of AWD, indicator for a rented or sharecropped plot, plot area, number of crops grown, indicator for growing two rice crops, number of boro irrigations, revenue per acre in boro, boro total cost per acre, and aman revenue per acre). Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table 5: Impacts of hourly irrigation cards on demand for conservation technology

	Purchase		Usage	
	(1)	(2)	(3)	(4)
Card Treatment	0.0430	-0.1428	0.0200	-0.1071
	(0.0436)	(0.1044)	(0.0278)	(0.1074)
Pipe Price	-0.0105***	-0.0129***	-0.0016***	-0.0033**
	(0.0008)	(0.0012)	(0.0006)	(0.0014)
Pipe Price * Card Treatment		0.0034**		0.0023
		(0.0015)		(0.0015)
Strata Fixed Effects	Yes	Yes	Yes	Yes
Mean in Control	0.413	0.413	0.068	0.068
Elasticity at Price=55 Treat	-1.26	-1.14	-1.01	-0.60
Elasticity at Price=55 Control	-1.39	-1.70	-1.31	-2.58
P-value: Equal Elasticities		0.009		0.001
Number Obs	3569	3569	3600	3600
R squared	0.249	0.254	0.033	0.041

The data are from the 144 villages that were part of the second-year experiment. The sample consists of 25 farmers per village. The dependent variable columns 1 and 2 is an indicator if the farmer purchased the AWD pipe at the randomly set price. The dependent variable in columns 3 and 4 is an indicator for installing the pipe. Prices were set randomly at the village level and range from 20 to 90 taka (around \$0.24 to \$1.1). The volumetric treatment variable is an indicator for villages where the 25 farmers were provided assistance with filling out the application for a prepaid (hourly) irrigation card and a waiver of the 150 taka sign-up fee. The p-value for equal elasticities is based on standard errors from the delta method. Standard errors are clustered at the village level. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table 6: Relationship between the hourly card treatment and observed water management on one field per farmer

	(1) AWD installed	(2) Water Level	(3) Dry Field
Card Treatment	0.0424	0.3651	-0.0988
	(0.0268)	(0.6997)	(0.1334)
Pipe Price	-0.0002	0.0002	0.0010
	(0.0003)	(0.0121)	(0.0021)
Pipe Price * Card Treatment	-0.0001	-0.0040	0.0008
	(0.0004)	(0.0132)	(0.0024)
Strata Fixed Effects	Yes	Yes	Yes
Mean in Control	0.008	2.214	0.393
P-value: Price+Price*Volumetric	0.165	0.469	0.136
Number Obs	3598	3600	3600
R squared	0.017	0.012	0.014

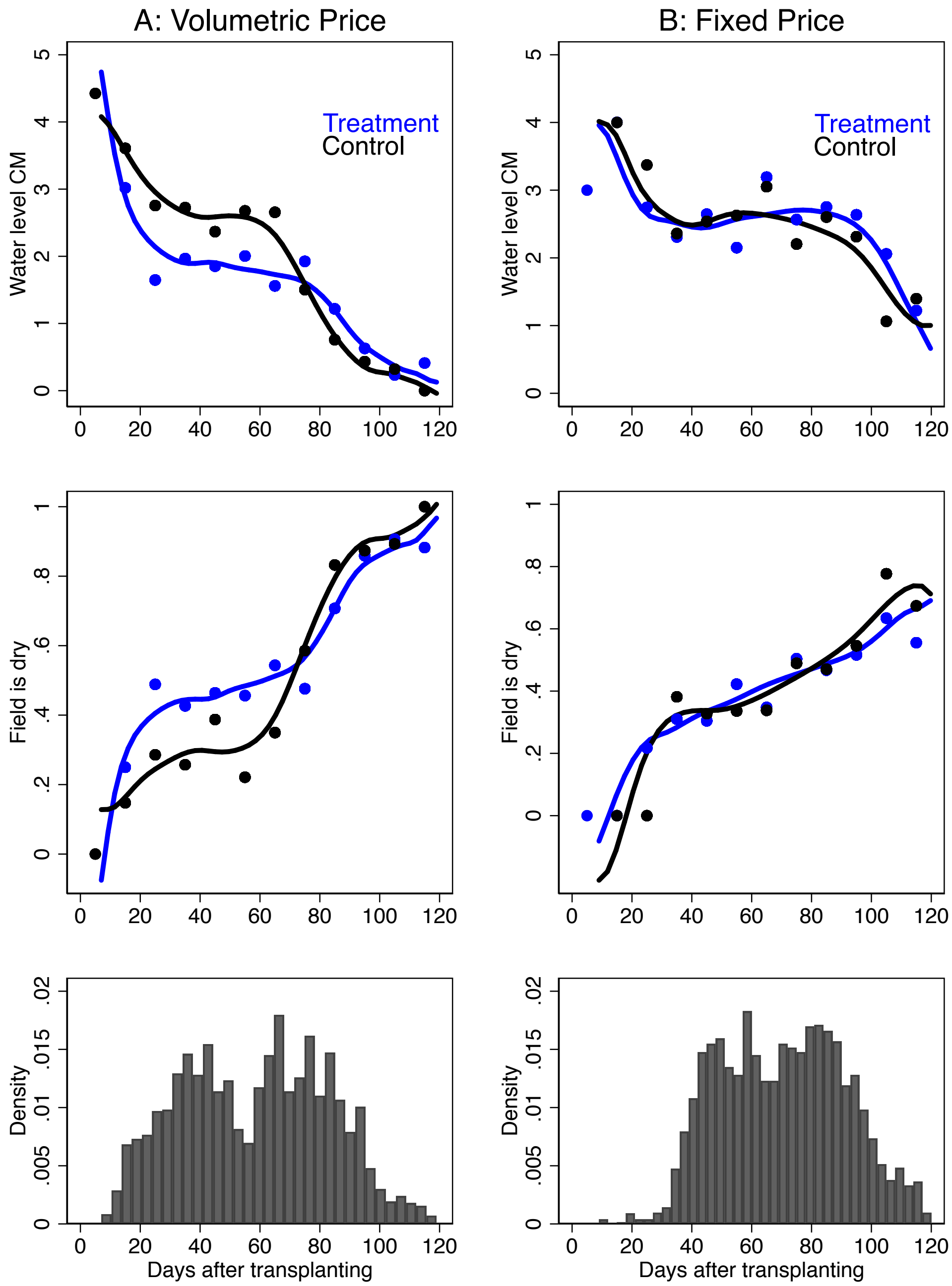
The data are from the 144 villages that were part of the second-year experiment. The sample consists of 25 farmers per village. The data are for one plot per farmer. The chosen plot is the closest to the village tube well for 75 percent of random farmers and the furthest plot for the remaining 25 percent of farmers. Prices were set randomly at the village level and range from 20 to 90 taka (around \$0.24 to \$1.1). The volumetric treatment variable is an indicator for villages where the 25 farmers were provided assistance with filling out the application for a prepaid (hourly) irrigation card and a waiver of the 150 taka sign-up fee. Standard errors are clustered at the village level. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Figures

Figure 1: The distribution of agricultural water pricing across the world

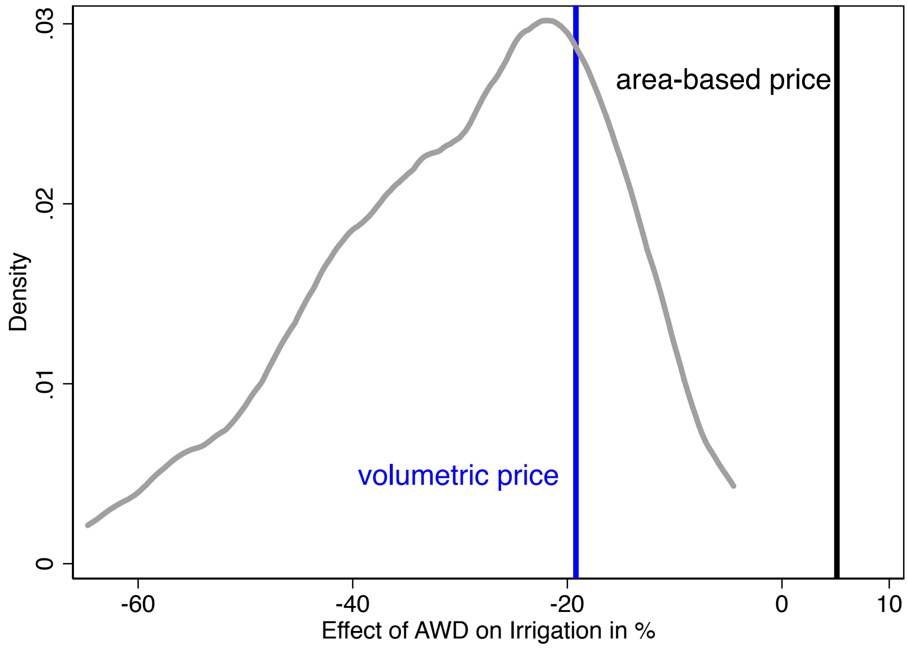
Notes: The top panel of the map shows shaded countries where at least some irrigation water is not priced volumetrically, usually priced with seasonal contracts by the acre or acre-crop. The bottom figure adds areas shaded in light blue to denote irrigated agricultural area.

Figure 2: Nonparametric estimates of treatment effect as a function of days after planting



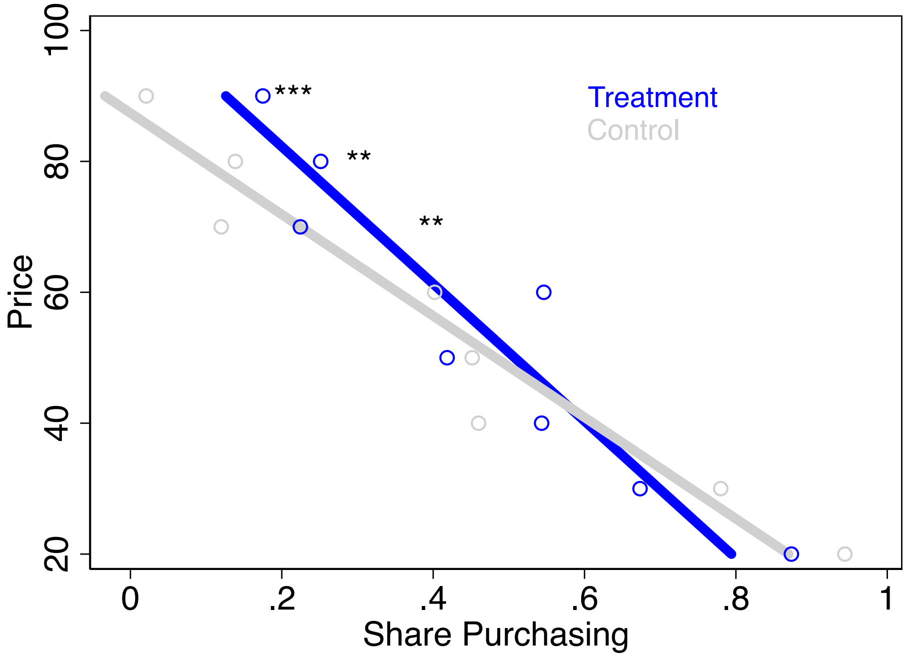
Notes: Figure shows non-parametric fan regressions of water levels in centimeters (top panel) and an indicator for fields with no standing water (middle panel) on the days after transplanting. The dots show average values from 10 day bins, where each dot is centered at the bin midpoint. The bottom panel shows the density of days after transplanting.

Figure 3: Comparison between impacts from the RCT and agronomic experiments



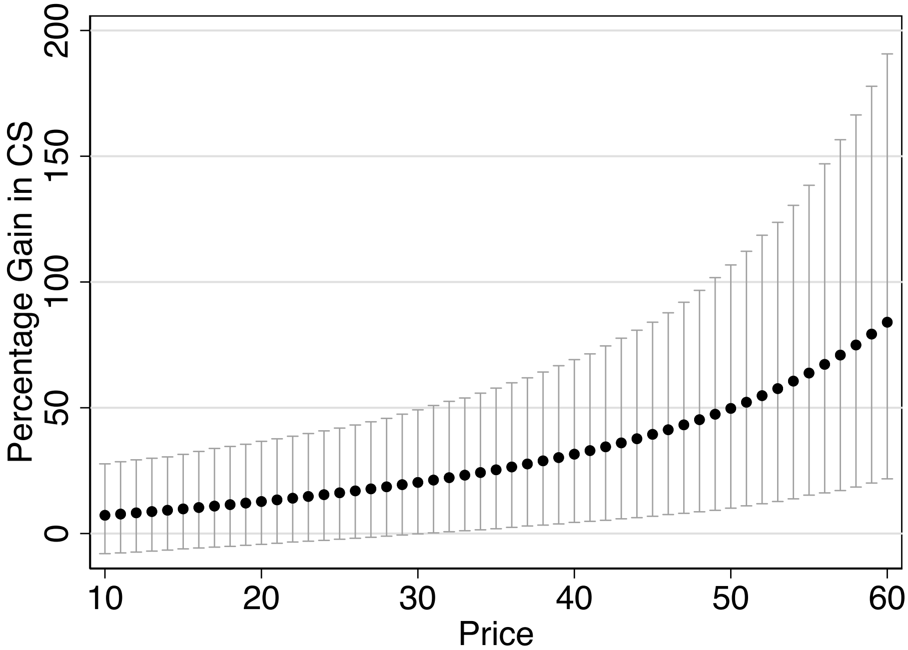
Notes: Figure shows the kernel density of the impacts of AWD on irrigation volumes (grey line) from 26 studies. These studies report a total of 87 impact estimates, as a single agronomic trial often includes more than one experiment in a single season, is done over multiple seasons, or tests different variants of the AWD technique. The black line shows our estimated treatment effect on water levels with area-based pricing and the blue line for areas with volumetric pricing (from Table 1 column 3).

Figure 4: Demand curve for conservation technology by hourly card treatment



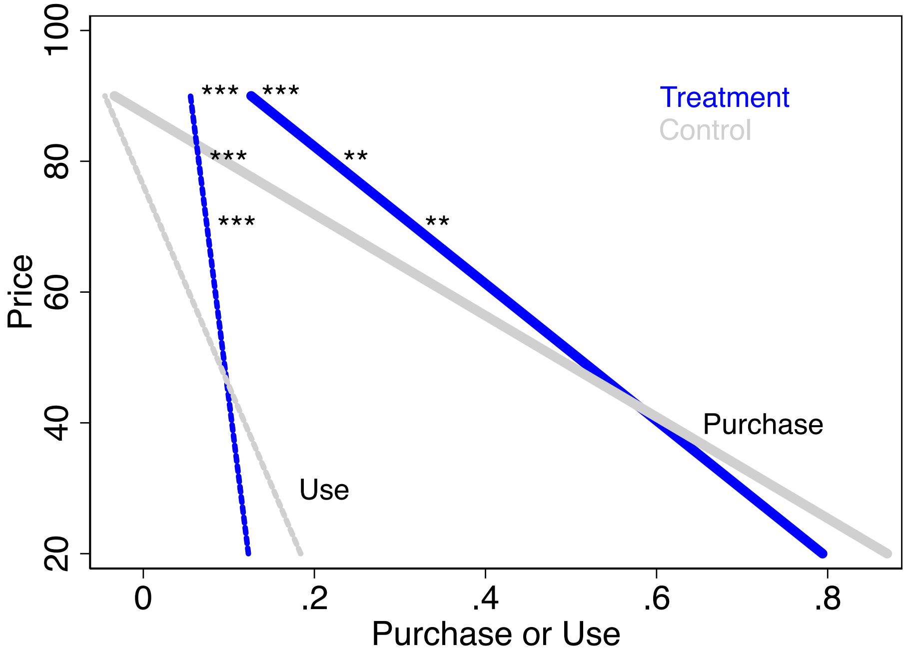
Notes: Figure shows linear demand estimates for farmers in the 144 villages that were part of the second-year experiment. The blue dots are raw adoption rates for the 96 treatment villages where prepaid hourly irrigation cards were provided. The blue line is the linear demand estimate for treatment villages. The grey dots are adoption rates in the 48 control villages and the grey line presents the corresponding linear demand estimate. Asterisks denote that the marginal impact of the treatment (from the linear demand estimates) is statistically significant (1% ***, 5% **, and 10% *). The estimation sample includes all 25 farmers in each village.

Figure 5: Effect of hourly card treatment on consumer surplus from conservation technology



Notes: The figure shows the gain in consumer surplus (of AWD) from the prepaid card treatment (measured in percent) as black dots. Specifically, referring to Equation 2, the consumer surplus in control villages is $\frac{-\beta_0^2}{2\beta_2} - \beta_0 p - \frac{\beta_2 p^2}{2}$ and in treatment villages is $\frac{-(\beta_0+\beta_1)^2}{2(\beta_2+\beta_3)} - (\beta_0 + \beta_1)p - \frac{(\beta_2+\beta_3)p^2}{2}$. The black dots are the percentage difference between these two values at various prices p. The 90 percent confidence intervals (whiskers) are estimated from 1,000 bootstrapped samples where the range of each whisker shows the 5th to 95th percentiles of the distribution of percentage changes in consumer surplus.

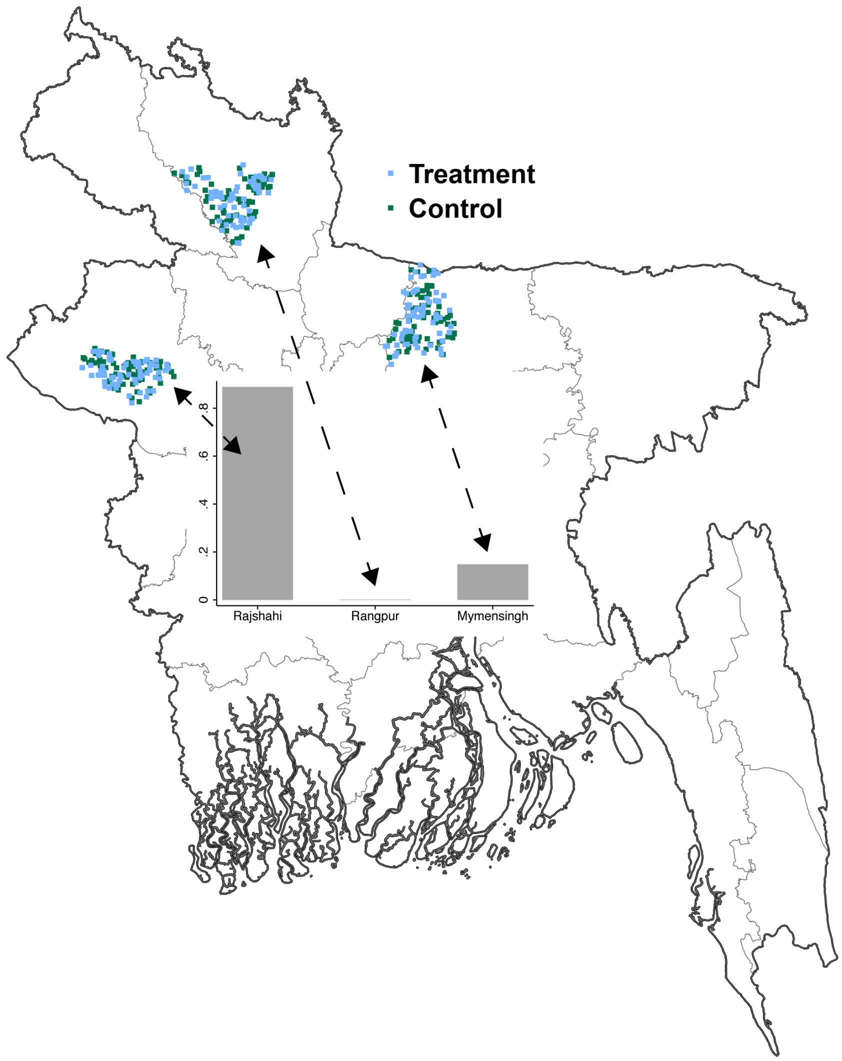
Figure 6: Usage of conservation technology as a function of price and hourly card treatment



Notes: The figure shows the demand curves for AWD as solid lines, where uptake is measured as purchasing the pipe from the door-to-door salesperson. The solid lines merely replicate the demand curves from Figure 4. The dashed lines instead consider usage, where usage is defined as an enumerator being able to verify that an AWD pipe was installed in one of the farmer's fields. The blue lines are for farmers in the 96 treatment villages where prepaid hourly irrigation cards were provided. The grey lines are for the 48 control villages. Asterisks denote a statistically significant treatment effect of the hourly irrigation cards (1% ***, 5% **, and 10% *). The sample in each village is the 25 farmers that were identified at the start of the experiment.

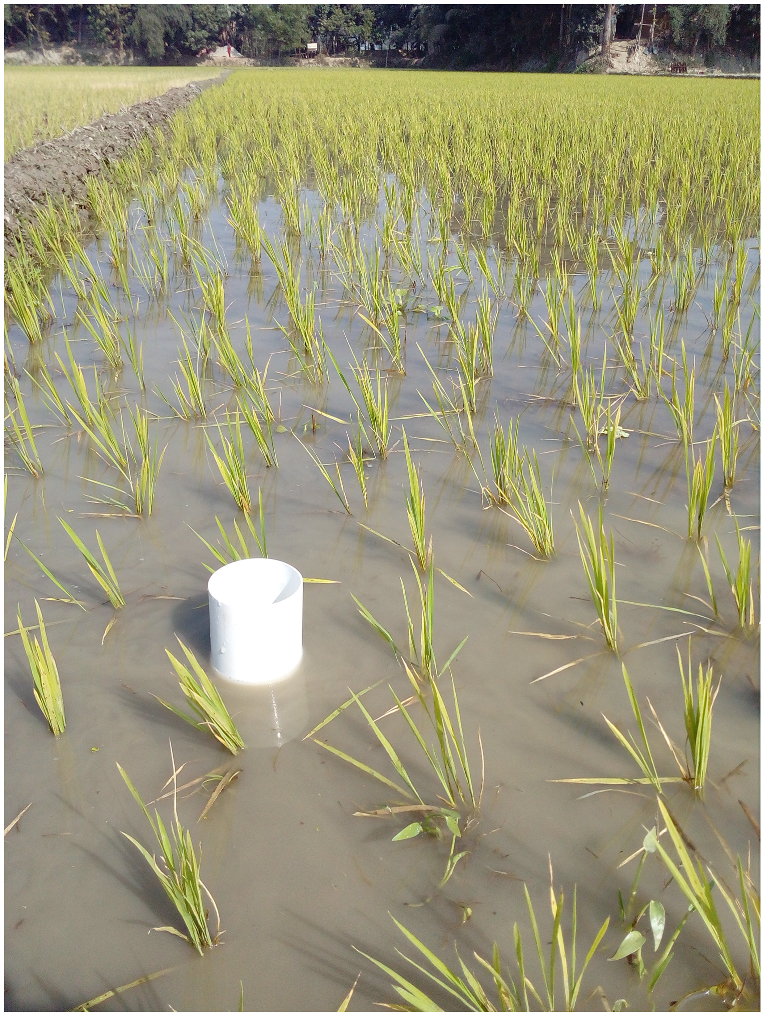
Appendix A: Additional Figures and Tables for Online Publication

Figure A1: Location of villages in first-year experiment



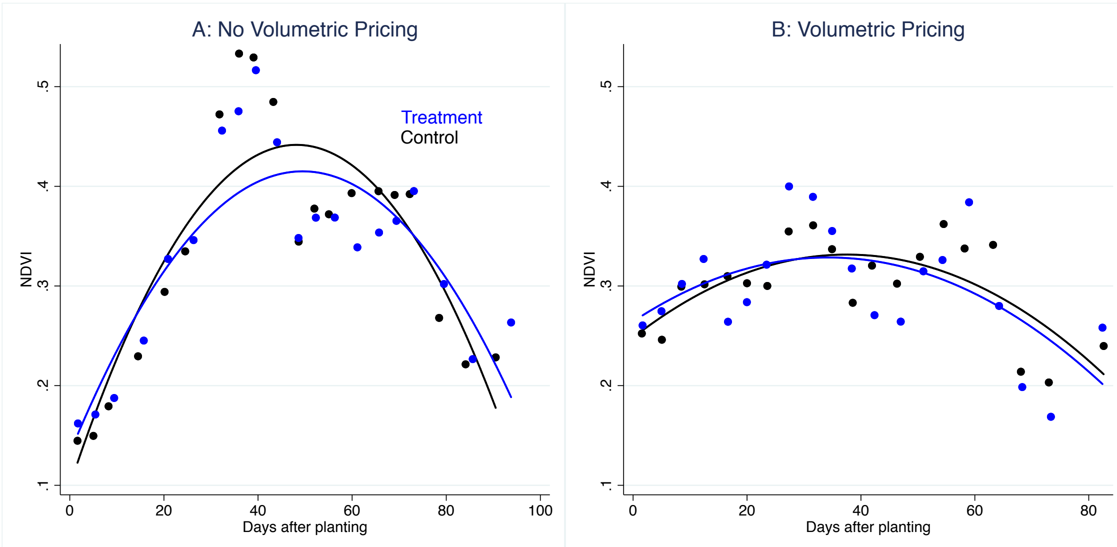
Notes: The figure shows the location of the 400 villages in the first-year RCT. The blue dots represent treatment villages and the green dots control. The bar chart embedded in the figure shows the frequency of volumetric pricing within each of the three districts - measured across farmers during our baseline survey from November/December 2016.

Figure A2: Image of AWD pipe installed in a farmer's field



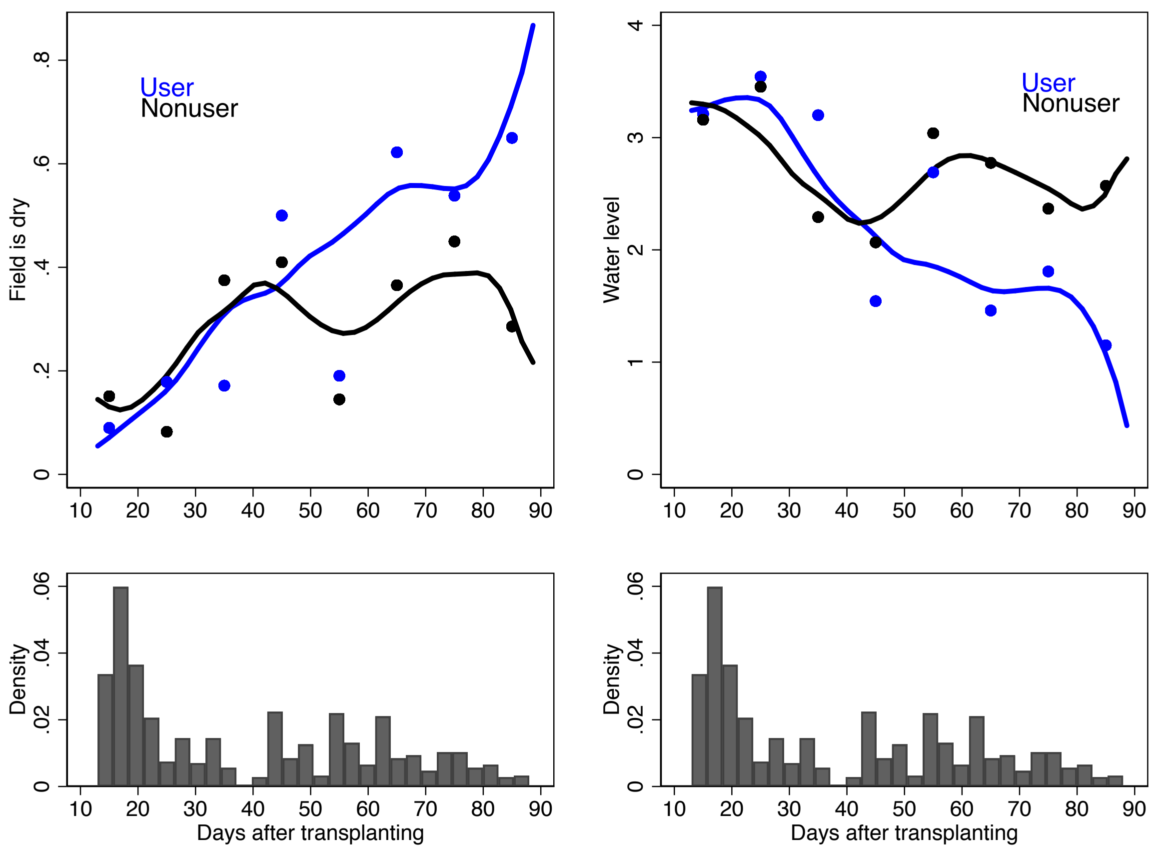
Notes: Figure shows an image of an AWD pipe installed in the farmer's field. The pipe is inserted to a level more than 15 cm below the soil surface. Holes are drilled into the plastic pipe, allowing the farmer to monitor soil moisture below the surface. A small net is wrapped around the bottom of the pipe to prevent mud from clogging the pipe. The farmer uses the pipe to monitor soil moisture. The field can be dried until the water level falls below 15 cm below the surface, marked with a line in the pipe. The field is then re-irrigated, hence the name "Alternate Wetting and Drying." This procedure should be used during the period up until the crop starts to reproduce (flower), when water should be kept in the field.

Figure A3: Treatment effects on satellite measures of greenness



Notes: The figure shows fitted quadratic relationships between NDVI (greenness) and the days after planting. The dots are averages across 20 bins of days after planting. The NDVI is measured using 8 day composites from Landsat available on the Google Earth Engine database. The images have a 30 meter resolution meaning that each pixel is approximately 0.2 acres, about two thirds the size of the median plot in the experiment.

Figure A4: Correlation between water management and use of the hourly irrigation card



Notes: The figure shows non-parametric fan regressions of an indicator for fields with no standing water (top left) and water levels in centimeters (top right) on the days after transplanting. Observations were taken for one plot per farmer. The blue lines are for the 323 farmers that used the cards while the black lines are for the 477 farmers that did not. The dots show average values from 10 day bins, where each dot is centered at the bin midpoint. The bottom panel shows the density of days after transplanting. The figure is for the one upazila where we received data on card usage.

Table A1: Summary Statistics and Covariate Balance by Treatment

	Means		
	Control	Treatment	p-value
Panel A: Household Characteristics			
Age	42.33	42.93	0.251
	(12.05)	(12.23)	
Years Education	6.645	6.330	0.125
	(4.863)	(4.525)	
Household Size	4.888	4.802	0.467
	(2.202)	(2.159)	
Number Livestock Owned	2.892	2.701	0.0935
	(2.745)	(2.502)	
Landholdings in Acres	2.026	2.003	0.769
	(2.168)	(2.046)	
Owns Television	0.636	0.612	0.314
	(0.481)	(0.487)	
Owns Refrigerator	0.139	0.129	0.639
	(0.346)	(0.335)	
Owns Irrigation Shallow Tubewell	0.0655	0.0595	0.520
	(0.247)	(0.237)	
Heard of AWD?	0.182	0.163	0.328
	(0.386)	(0.369)	
Panel B: Characteristics of Study Plot			
Plot is Rented or Sharecropped	0.0875	0.0675	0.136
	(0.283)	(0.251)	
Area in Acres	0.427	0.405	0.195
	(0.494)	(0.421)	
Volumetric Water Price	0.344	0.350	0.754
	(0.475)	(0.477)	
Number Crops Grown	2.194	2.174	0.611
	(0.480)	(0.481)	
Rice-Rice Cropping System	0.697	0.698	0.989
	(0.460)	(0.459)	
Number Irrigations in Boro	20.80	20.55	0.695
	(8.757)	(8.097)	
Revenue per Acre in Boro	39866.3	40133.4	0.700
	(10534.0)	(14796.8)	
Cost per Acre in Boro	22651.0	22939.6	0.625
	(10526.1)	(9190.8)	
Water Cost per Acre in Boro	6663.9	6199.8	0.357
	(8768.0)	(5636.1)	
Revenue per Acre in Aman	27622.6	27763.4	0.868
	(11668.1)	(19959.8)	

The table shows mean values of baseline characteristics for control and AWD treatment households in columns 1 and 2, respectively. Column 3 shows the p-value from the regression of each characteristic on the treatment indicator and strata (Upazila) fixed effects. Panel A contains household-level variables and Panel B contains variables specific to the study plot nearest the irrigation tubewell. “Boro” is the dry-season from January to May and “Aman” is the wet season from June to November. All data are based on the baseline survey from November-December 2016.

Table A2: Summary Statistics and Covariate Balance by Treatment for places with volumetric water pricing

	Means		
	Control	Treatment	p-value
Panel A: Household Characteristics			
Age	42.76 (11.99)	42.88 (12.25)	0.784
Years Education	6.565 (4.879)	6.629 (4.365)	0.723
Household Size	4.754 (2.136)	4.791 (2.126)	0.860
Number Livestock Owned	2.651 (2.818)	2.316 (2.379)	0.0834
Landholdings in Acres	2.411 (2.315)	2.339 (2.291)	0.997
Owns Television	0.696 (0.460)	0.719 (0.450)	0.499
Owns Refrigerator	0.0959 (0.295)	0.114 (0.318)	0.392
Owns Irrigation Shallow Tubewell	0.0785 (0.269)	0.0529 (0.224)	0.213
Heard of AWD?	0.119 (0.324)	0.136 (0.343)	0.449
Panel B: Characteristics of Study Plot			
Plot is Rented or Sharecropped	0.102 (0.303)	0.0571 (0.232)	0.0454
Area in Acres	0.380 (0.532)	0.374 (0.390)	0.850
Number Crops Grown	2.425 (0.627)	2.320 (0.624)	0.333
Rice-Rice Cropping System	0.382 (0.486)	0.409 (0.492)	0.474
Number Irrigations in Boro	19.99 (9.643)	20.75 (8.375)	0.334
Revenue per Acre in Boro	45455.4 (9352.6)	46416.6 (20243.7)	0.316
Cost per Acre in Boro	25731.0 (15180.6)	26070.9 (12215.2)	0.762
Water Cost per Acre in Boro	9637.6 (14293.5)	8200.9 (8846.5)	0.107
Revenue per Acre in Aman	31138.6 (13754.1)	29215.6 (23735.9)	0.639

The table shows mean values of baseline characteristics for control and AWD treatment households in columns 1 and 2, respectively. Column 3 shows the p-value from the regression of each characteristic on the treatment indicator and strata (Upazila) fixed effects. Panel A contains household-level variables and Panel B contains variables specific to the study plot nearest the irrigation tubewell. "Boro" is the dry-season from January to May and "Aman" is the wet season from June to November. All data are based on the baseline survey from November-December 2016 and only include households that reported volumetric water pricing at baseline.

Table A3: Effects of conservation technology when omitting strata fixed effects

	(1) Level	(2) Dry	(3) Level	(4) Dry
Volumetric Pricing	-0.621***	0.061**	-0.363**	0.018
	(0.147)	(0.026)	(0.183)	(0.035)
Treatment			0.109	-0.009
			(0.229)	(0.029)
Treatment *			-0.510*	0.085
Volumetric Pricing			(0.293)	(0.052)
Mean in Control	2.32	0.45	2.32	0.45
p-Value: Treat+Treat*Volumetric			0.031	0.078
Number of Observations	7596	7596	7596	7596
R squared	0.009	0.003	0.011	0.005

The data are from random unannounced visits to the study plots of sample farmers during the 2017 boro (dry) growing season. The dependent variable in columns 1 and 3 is the amount of standing water in the field, measured in centimeters. The dependent variable in columns 2 and 4 is an indicator variable for a dry field with no standing water. Volumetric pricing is an indicator for farmers for whom the water price is tied to usage, either through hourly charges or fuel payments. Standard errors are clustered at the village level. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table A4: Separate effects by time of growing season, 0-60 and 60+ days after planting

	0-60 Days After Planting		60+ Days After Planting	
	(1)	(2)	(3)	(4)
	Level	Dry	Level	Dry
Treatment	-0.357**	0.071**	0.094	0.001
	(0.149)	(0.030)	(0.248)	(0.030)
Strata Fixed Effects	Yes	Yes	Yes	Yes
Mean in Control	2.65	0.31	2.11	0.54
Number of Observations	3148	3148	4450	4450
R squared	0.037	0.036	0.057	0.068

The data are from random unannounced visits to the study plots of sample farmers during the 2017 boro (dry) growing season. Columns 1 and 2 are for measurements taken up to 60 days after transplanting. Columns 3 and 4 are for measurements taken more than 60 days after transplanting. The dependent variable in columns 1 and 3 is the amount of standing water in the field, measured in centimeters. The dependent variable in columns 2 and 4 is an indicator variable for a dry field with no standing water. Standard errors are clustered at the village level. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table A5: Separate effects by time of growing season, 0-80 and 80+ days after planting

	0-80 Days After Planting		80+ Days After Planting	
	(1)	(2)	(3)	(4)
	Level	Dry	Level	Dry
Treatment	-0.213	0.045*	0.251	-0.029
	(0.152)	(0.025)	(0.334)	(0.039)
Strata Fixed Effects	Yes	Yes	Yes	Yes
Mean in Control	2.55	0.36	1.80	0.63
Number of Observations	5316	5316	2282	2282
R squared	0.033	0.052	0.100	0.130

The data are from random unannounced visits to the study plots of sample farmers during the 2017 boro (dry) growing season. Columns 1 and 2 are for measurements taken up to 80 days after transplanting. Columns 3 and 4 are for measurements taken more than 80 days after transplanting. The dependent variable in columns 1 and 3 is the amount of standing water in the field, measured in centimeters. The dependent variable in columns 2 and 4 is an indicator variable for a dry field with no standing water. Standard errors are clustered at the village level. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table A6: Heterogeneous effects by first 60 days of the growing season

	0-60 Days After Planting		60+ Days After Planting	
	(1)	(2)	(3)	(4)
	Level	Dry	Level	Dry
Treatment	-0.103	0.008	0.219	-0.001
	(0.188)	(0.035)	(0.335)	(0.035)
Treatment *	-0.670**	0.164***	-0.386	0.008
Volumetric Pricing	(0.298)	(0.062)	(0.429)	(0.068)
Volumetric Pricing	-0.035	-0.038	-0.365	-0.011
	(0.363)	(0.074)	(0.418)	(0.072)
Strata Fixed Effects	Yes	Yes	Yes	Yes
Mean in Control	2.65	0.31	2.11	0.54
p-Value: Treat+Treat*Volumetric	0.001	0.001	0.519	0.916
Number of Observations	3147	3147	4449	4449
R squared	0.043	0.043	0.059	0.068

The data are from random unannounced visits to the study plots of sample farmers during the 2017 boro (dry) growing season. Columns 1 and 2 are for measurements taken up to 60 days after transplanting. Columns 3 and 4 are for measurements taken more than 60 days after transplanting. The dependent variable in columns 1 and 3 is the amount of standing water in the field, measured in centimeters. The dependent variable in columns 2 and 4 is an indicator variable for a dry field with no standing water. Volumetric pricing is an indicator for farmers for whom the water price is tied to usage, either through hourly charges or payments for diesel fuel. Standard errors are clustered at the village level. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table A7: Heterogeneous effects by first 80 days of the growing season

	0-80 Days After Planting		80+ Days After Planting	
	(1)	(2)	(3)	(4)
	Level	Dry	Level	Dry
Treatment	0.049	-0.007	0.294	-0.020
	(0.209)	(0.030)	(0.442)	(0.049)
Treatment * Volumetric Pricing	-0.719**	0.144***	-0.055	-0.037
	(0.279)	(0.052)	(0.514)	(0.071)
Volumetric Pricing	0.097	-0.087	-0.718	0.023
	(0.345)	(0.063)	(0.522)	(0.070)
Strata Fixed Effects	Yes	Yes	Yes	Yes
Mean in Control	2.55	0.36	1.80	0.63
p-Value: Treat+Treat*Volumetric	0.000	0.001	0.346	0.264
Number of Observations	5315	5315	2281	2281
R squared	0.037	0.057	0.102	0.130

The data are from random unannounced visits to the study plots of sample farmers during the 2017 boro (dry) growing season. Columns 1 and 2 are for measurements taken up to 80 days after transplanting. Columns 3 and 4 are for measurements taken more than 80 days after transplanting. The dependent variable in columns 1 and 3 is the amount of standing water in the field, measured in centimeters. The dependent variable in columns 2 and 4 is an indicator variable for a dry field with no standing water. Volumetric pricing is an indicator for farmers for whom the water price is tied to usage, either through hourly charges or payments for diesel fuel. Standard errors are clustered at the village level. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table A8: Effects of conservation technology on self-reported water use

	Number Irrigations		Times Drained	
	(1)	(2)	(3)	(4)
Treatment	-3.589***	-3.590***	2.207***	1.888***
	(0.486)	(0.607)	(0.225)	(0.258)
Treatment * Volumetric Pricing		-0.015		0.918*
		(0.994)		(0.497)
Volumetric Pricing		1.082		0.032
		(1.263)		(0.433)
Strata Fixed Effects	Yes	Yes	Yes	Yes
Mean in Control	19.10	19.10	2.42	2.42
p-Value: Treat+Treat*Volumetric		0.000		0.000
Number of Observations	3985	3984	3983	3982
R squared	0.539	0.540	0.359	0.366

The data are taken from the followup survey after harvesting. The dependent variables are the number of times the field was irrigated (columns 1-2) and the number of times the field was drained or dried (columns 3-4). Standard errors are clustered at the village level. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table A9: Profit effects when trimming top and bottom 1.5 percent of distribution

	Profit			Log Profit		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-192.9	-61.98	-213.9	-0.0266	-0.0227	-0.0380
	(751.0)	(843.5)	(872.7)	(0.0422)	(0.0431)	(0.0422)
Treatment * Volumetric Pricing	2325.6**	1927.0	2237.7	0.115**	0.115*	0.153**
	(1108.6)	(1280.9)	(1413.9)	(0.0544)	(0.0606)	(0.0623)
Volumetric Pricing	-1605.6	-124.2	-629.9	-0.111*	-0.102	-0.140**
	(1092.5)	(1416.9)	(1362.4)	(0.0576)	(0.0684)	(0.0702)
Trim Top and Bottom 1.5%	Yes	No	No	Yes	No	No
Strata Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	No	Yes	Yes
Controls X Treatment	No	No	Yes	No	No	Yes
Mean in Control	27125.15	27137.22	27137.22	10.11	10.12	10.12
p-Value Treat+Treat*Volumetric	0.010	0.052	0.046	0.013	0.029	0.010
Number Obs	3863	3978	3978	3863	3928	3928

The data are taken from the followup survey after harvesting. The dependent variables are profit in taka per acre (columns 1-3) and log profit (columns 4-6). Columns 1 and 4 trim the top and bottom 1.5 percent of the profit distribution. The controls in the remaining columns are age, education, household size, number of livestock owned, landholdings, television ownership, refrigerator ownership, tube well ownership, baseline knowledge of AWD, indicator for renting/sharecropping at baseline, plot area, number of crops grown, indicator for a rice-rice cropping system, number of irrigations during the boro season, boro revenue per acre, boro total cost per acre, boro water cost per acre, and aman revenue per acre. All control variables were measured during the baseline survey. The controls are all demeaned before being interacted with treatment in columns 3 and 6. Standard errors are clustered at the village level. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table A10: Effects on material input expenditure

	Fertilizer					Chemicals	
	(1) N apps	(2) Urea	(3) TSP	(4) Potash	(5) Other	(6) Pesticide	(7) Herbicide
Treatment	-0.004	-5.653	3.685	5.868	-24.266*	-106.318*	34.564***
	(0.044)	(31.897)	(36.014)	(18.581)	(13.634)	(56.998)	(12.265)
Strata Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	2.67	1513.80	1073.34	586.13	115.56	1542.37	301.71
Number of Observations	3986	3983	3983	3983	3983	3983	3983
R squared	0.187	0.270	0.215	0.187	0.150	0.391	0.131

The data are taken from the followup survey after harvesting. The dependent variables are number of times fertilizer was applied (column 1), fertilizer expenditure per acre (columns 2-5), and chemical expenditure per acre (columns 6-7). All expenditures are recorded in Bangladeshi taka per acre. Standard errors are clustered at the village level. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table A11: Effects on labor expenditure

	Hired			Family		
	(1) Plant	(2) Weed	(3) Harvest	(4) Plant	(5) Weed	(6) Harvest
Treatment	107.067	172.178**	120.103	25.970	-94.987	-49.090
	(82.276)	(83.377)	(174.900)	(59.703)	(72.594)	(75.184)
Strata Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	3706.13	1907.60	6605.49	862.73	1298.77	1160.69
Number of Observations	3983	3981	3983	3978	3983	3982
R squared	0.234	0.138	0.216	0.259	0.204	0.271

The data are taken from the followup survey after harvesting. The dependent variables are expenditure per acre on hired labor (columns 1-3), and imputed expenditure on family labor (columns 4-6). All expenditures are recorded in Bangladeshi taka per acre. Family labor expenditure is imputed by multiplying observed person days by the daily wage rate. Standard errors are clustered at the village level. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table A12: Heterogeneous effects on material input expenditure

	Fertilizer					Chemicals	
	(1) N apps	(2) Urea	(3) TSP	(4) Potash	(5) Other	(6) Pesticide	(7) Herbicide
Treatment	-0.019	37.135	14.264	16.740	-38.196**	-49.034	51.928***
	(0.052)	(37.960)	(39.172)	(23.173)	(17.651)	(60.291)	(17.250)
Treatment * Volumetric Pricing	0.041	-124.456*	-30.126	-30.644	40.721	-167.998	-50.193**
	(0.095)	(69.209)	(82.734)	(38.796)	(26.707)	(132.486)	(21.988)
Volumetric Pricing	0.028	77.670	-53.716	-35.319	-49.417**	199.235**	25.228
	(0.075)	(49.332)	(72.425)	(34.355)	(24.619)	(93.076)	(20.514)
Strata Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	2.67	1513.80	1073.34	586.13	115.56	1542.37	301.71
p-Value: Treat+Treat*Volumetric	0.776	0.131	0.828	0.655	0.901	0.067	0.899
Number of Observations	3985	3982	3982	3982	3982	3982	3982
R squared	0.188	0.273	0.216	0.189	0.155	0.395	0.134

The data are taken from the followup survey after harvesting. The dependent variables are number of times fertilizer was applied (column 1), fertilizer expenditure per acre (columns 2-5), and chemical expenditure per acre (columns 6-7). All expenditures are recorded in Bangladeshi taka per acre. Standard errors are clustered at the village level. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table A13: Heterogeneous effects on labor expenditure

	Hired			Family		
	(1) Plant	(2) Weed	(3) Harvest	(4) Plant	(5) Weed	(6) Harvest
Treatment	121.638	96.450	214.949	-12.322	-78.577	-1.534
	(117.075)	(94.393)	(225.466)	(56.321)	(84.846)	(77.506)
Treatment * Volumetric Pricing	-43.480	213.977	-279.576	112.744	-43.809	-134.140
	(141.671)	(185.537)	(352.897)	(147.297)	(162.296)	(179.529)
Volumetric Pricing	215.358	211.368	671.095**	-198.722	-212.623	-173.712
	(153.256)	(170.082)	(269.756)	(125.856)	(233.701)	(197.290)
Strata Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	3706.13	1907.60	6605.49	862.73	1298.77	1160.69
p-Value: Treat+Treat*Volumetric	0.341	0.053	0.811	0.460	0.372	0.401
Number of Observations	3982	3980	3982	3977	3982	3981
R squared	0.235	0.142	0.219	0.260	0.205	0.273

The data are taken from the followup survey after harvesting. The dependent variables are expenditure per acre on hired labor (columns 1-3), and imputed value of family labor (columns 4-6). All expenditures are recorded in Bangladeshi taka per acre. Standard errors are clustered at the village level. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table A14: Effects of conservation technology on revenues and profit

				Log:		
	(1) Yield	(2) Revenue	(3) Profit	(4) Yield	(5) Revenue	(6) Profit
Treatment	7.736	604.360	425.276	0.002	0.011	0.007
	(21.221)	(614.012)	(681.853)	(0.010)	(0.012)	(0.034)
Strata Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	2269.16	52696.04	27133.39	7.71	10.85	10.12
Number of Observations	3983	3983	3983	3983	3983	3933
R squared	0.352	0.389	0.296	0.328	0.349	0.270

The data are taken from the followup survey after harvesting. The dependent variables are crop yield in kilograms per acre (column 1), revenue in Bangladeshi taka per acre (column 2) and profit in Bangladeshi taka per acre (column 3). Columns 4 through 6 show the same regressions with log yields, revenue, and profits, respectively. Standard errors are clustered at the village level. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table A15: Treatment effects on a randomly selected non-study plot

	(1) Profit	(2) Revenue	(3) Water Cost	(4) Other Input Cost
Treatment	-338.428	223.523	175.130	384.935
	(889.866)	(805.980)	(125.225)	(449.143)
Treatment * Volumetric Pricing	2046.941	1206.193	-451.348	-387.501
	(1487.757)	(1425.549)	(287.409)	(722.794)
Volumetric Pricing	-1751.422	-815.267	343.016	592.198
	(1512.624)	(1500.246)	(249.677)	(741.170)
Strata Fixed Effects	Yes	Yes	Yes	Yes
Mean in Control	26900.72	52917.74	4927.41	21093.57
p-Value: Treat+Treat*Volumetric	0.156	0.229	0.284	0.996
Number of Observations	3463	3463	3462	3461
R squared	0.189	0.235	0.377	0.175

The data are taken from the followup survey after harvesting for the 3,463 farmers that cultivated more than one plot. Each regression shows effects on a randomly selected plot *other than the study plot* for each farmer. The dependent variables are profit per acre (column 1), revenue in taka per acre (column 2), water cost in taka per acre (column 3), and total cost of other inputs in taka per acre (column 4). Standard errors are clustered at the village level. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table A16: Multiple inference corrections for effects of conservation technology with volumetric pricing

	Effect	Unadjusted p-value	FDR q value Anderson (2008)	FWER adjusted List et al. (2016)
Irrigations	-3.61	0.000	0.001	0.072
Times Dried	2.81	0.000	0.001	0.000
Water Level	-0.42	0.021	0.043	0.398
Dry Field	0.08	0.047	0.071	0.444
Water Level 0-70 days	-0.84	0.000	0.001	0.026
Dry Field 0-70 days	0.17	0.000	0.001	0.011
Water Cost	-302.38	0.226	0.252	0.529
Other Input Cost	-163.06	0.780	0.780	0.674
Revenue	1,401.58	0.091	0.115	0.656
Profit	1,866.86	0.049	0.071	0.373

The table shows p-values adjusted for multiple inference. For each of the outcomes, the second column provides the estimated treatment effect for farmers facing volumetric prices, i.e. $\beta_1 + \beta_3$ in equation 1. The third column shows the unadjusted p-value. The fourth column adjusts the p-values to control the false discovery rate, following Anderson (2008). The fifth column uses the method in List, Shaikh, and Xu (2016), which more conservatively controls the familywise error rate (the probability of at lease one false rejection).

Table A17: Balance of baseline characteristics for volumetric pricing experiment

	Means		
	Control	Hourly Card	p-value
Age	39.24	39.74	0.445
	(10.28)	(11.18)	
Years Education	7.253	7.008	0.451
	(4.131)	(4.267)	
Household Size	4.489	4.232	0.0184
	(1.649)	(1.840)	
Number Livestock Owned	2.686	2.812	0.507
	(2.052)	(2.357)	
Landholdings in Acres	1.598	1.609	0.967
	(1.640)	(1.418)	
Owns Television	0.887	0.870	0.366
	(0.317)	(0.336)	
Owns Refrigerator	0.195	0.192	0.824
	(0.396)	(0.394)	
Owns Irrigation Shallow Tubewell	0.0569	0.0421	0.439
	(0.232)	(0.201)	
Seasonal Water Price (taka per bigah)	1522.3	1481.9	0.626
	(427.6)	(372.3)	
Usual Number Irrigations	18.98	18.74	0.985
	(8.178)	(8.506)	
Pays Deep Driver for Irrigation	0.708	0.707	0.919
	(0.455)	(0.455)	

The table shows mean values of baseline characteristics for farmers in the 48 control (column 1) and 96 prepaid-card treatment villages (column 2). Standard deviations are displayed below each mean value in parentheses. Column 3 shows the p-value from the regression of each characteristic on the treatment indicator and strata (Upazila) fixed effects. The data are based on the baseline survey carried out with 25 farmers per village during December 2017.

Table A18: Impacts of hourly irrigation cards on demand with log functional form

	Purchase		Usage	
	(1)	(2)	(3)	(4)
Card Treatment	0.0353	-0.5510**	0.0187	-0.4848
	(0.0428)	(0.2622)	(0.0279)	(0.3321)
Log Pipe Price	-0.5084***	-0.6123***	-0.0763**	-0.1665**
	(0.0351)	(0.0489)	(0.0307)	(0.0739)
Log Pipe Price *		0.1497**		0.1287
Card Treatment		(0.0654)		(0.0795)
Strata Fixed Effects	Yes	Yes	Yes	Yes
Mean in Control	0.413	0.413	0.068	0.068
Elasticity at Price=55 Treat	-1.25	-1.13	-0.95	-0.45
Elasticity at Price=55 Control	-1.37	-1.70	-1.23	-3.08
P-value: Equal Elasticities		0.025		0.005
Number Obs	3569	3569	3600	3600
R squared	0.256	0.260	0.033	0.043

The data are from the 144 villages that were part of the second-year experiment. The sample consists of 25 farmers per village. The dependent variable columns 1 and 2 is an indicator if the farmer purchased the AWD pipe at the randomly set price. The dependent variable in columns 3 and 4 is an indicator for installing the pipe. Prices were set randomly at the village level and range from 20 to 90 taka (around \$0.24 to \$1.1). The volumetric treatment variable is an indicator for villages where the 25 farmers were provided assistance with filling out the application for a prepaid (hourly) irrigation card and a waiver of the 150 taka sign-up fee. The p-value for equal elasticities is based on standard errors from the delta method. Standard errors are clustered at the village level. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table A19: Logit estimates of demand functions

	Purchase		Usage	
	(1)	(2)	(3)	(4)
main				
Card Treatment	-0.889	-3.426*	-2.197**	-8.390***
	(0.638)	(2.029)	(1.062)	(3.057)
Pipe Price	-0.0667***		-0.0722***	
	(0.00913)		(0.0212)	
Pipe Price * Card Treatment	0.0208*		0.0601***	
	(0.0108)		(0.0223)	
Log Pipe Price		-3.168***		-2.815***
		(0.429)		(0.748)
Log Pipe Price * Card Treatment		0.924*		2.379***
		(0.508)		(0.810)
Strata Fixed Effects	Yes	Yes	Yes	Yes
Mean in Control	0.413	0.413	0.068	0.068
Number Obs	3569	3569	3600	3600

The data are from the 144 villages that were part of the second-year experiment. The sample consists of 25 farmers per village. The table shows *coefficients* from logit regressions where the dependent variable is an AWD purchase indicator (columns 1 and 2) and an indicator for installing the pipe (columns 3 and 4). Prices were set randomly at the village level and range from 20 to 90 taka (around \$0.24 to \$1.1). The volumetric treatment variable is an indicator for villages where the 25 farmers were provided assistance with filling out the application for a prepaid (hourly) irrigation card and a waiver of the 150 taka sign-up fee. Standard errors are clustered at the village level. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table A20: Relationship between price and usage conditional on purchase of conservation technology

	(1)	(2)
Card Treatment	-0.2501	-1.0292**
	(0.1522)	(0.4638)
Pipe Price	-0.0044*	
	(0.0024)	
Pipe Price * Card Treatment	0.0066**	
	(0.0027)	
Log Pipe Price		-0.1910*
		(0.1067)
Log Pipe Price * Card Treatment		0.2904**
		(0.1193)
Strata Fixed Effects	Yes	Yes
Mean in Control	0.162	0.162
P-value: Price+Price*Volumetric	0.086	0.058
Number Obs	1580	1580
R squared	0.046	0.049

The data are from the 144 villages that were part of the second-year experiment. The sample is limited to the farmers that bought AWD pipes during the demand experiment. The dependent variable in all regressions is an indicator if it was verified that the farmer installed AWD on one of their plots. Prices were set randomly at the village level and range from 20 to 90 taka (around \$0.24 to \$1.1). The volumetric treatment variable is an indicator for villages where the 25 farmers were provided assistance with filling out the application for a prepaid (hourly) irrigation card and a waiver of the 150 taka sign-up fee. Standard errors are clustered at the village level. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table A21: Marginal CO2 emissions for U.S. power plants in lbs/kwh

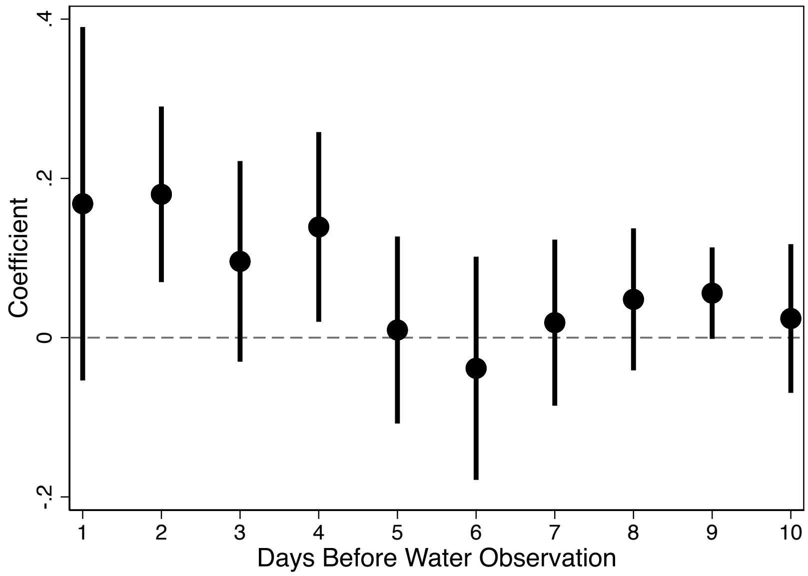
	(1) All Plants	(2) Balanced Panel
Generation	3.046***	3.600***
	(0.136)	(0.144)
Generation * Coal	0.647***	0.617***
	(0.0426)	(0.0795)
Generation * Oil	0.248***	0.328***
	(0.0560)	(0.0961)
Generation * Thermal Efficiency	-4.492***	-5.894***
	(0.288)	(0.293)
Plant Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
Number Obs	21238	5136
R squared	0.944	0.968

The data are from the Emissions & Generation Resource Integrated Database (eGRID) database of the U.S Environmental Protection Agency. The data include annual information for U.S. power plants on the amount of electricity produced, CO2 emissions, the fuel source of the plant, thermal efficiency, and a number of other variables for the years 1998-2000, 2005, 2007, 2009, 2010, 2012, 2014, and 2016. Both columns are fixed effects regressions where annual CO2 emissions (in lbs) are regressed on electricity generated (in kwh) and its interaction with fuel type and thermal efficiency. Standard errors are clustered at the level of the power plant. Column 1 includes all observations and column 2 includes only the power plants for which we have a balanced panel. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Appendix B: The relationship between observed water management and recent pumping behavior

Observed water levels serve as a proxy for actual water use. We chose this method because meters do not exist on most plots in our sample. Yet, a reasonable question to ask is whether observed water levels correlate with actual water use. A subset of 125 of the water-level observations were taken for farmers where we have data on daily pumping times on the prepaid cards. For this sample, Figure B1 shows the relationship between the water level and 10 daily lags of hours pumped using the prepaid card. Regressing the water level on total hours pumped in the previous four days yields a point estimate of 0.15 and a t-statistic of 4.08. Hours pumped during the previous four days — an objective measure of actual usage — is positively correlated with water levels. The correlation coefficient is 0.4. These results suggest that our water-level observations do partly measure water usage during the previous few days.

Figure B1: Relationship between observed water levels and daily pumping hours



Notes: Figure shows the coefficient estimates from a distributed lag model where the observed water level in the field (in cm) is regressed on the number of hours pumped for each of the previous 10 days. The dots show coefficient estimates for each of the 10 variables and the vertical lines display 95 percent confidence intervals. The data on pumping activity — for the upazila which we have data — were matched to the water-level observations for 125 farmers who started using their prepaid cards before the water-level observation and stopped using their cards after the observation. Focusing on this sample ensures that the estimation is only relying on farmers who were actively using their cards during the time when the enumerator observed the water level.

Appendix C: Which covariates drive heterogeneous treatment effects of water-saving technology?

This appendix digs deeper into our data to examine the importance of volumetric pricing as a source of heterogeneity in our first experiment — relative to other covariates. Table C1 starts by interacting baseline controls with the volumetric pricing indicator. We continue to find large interaction effects on water usage even when interacting the AWD treatment with this large set of covariates. This offers a first piece of suggestive evidence that pricing incentives — rather than an unobserved covariate — are responsible for the observed heterogeneity.

We also implement recent machine-learning techniques to investigate which farmers are most impacted by the AWD treatment (Chernozhukov et al., 2018). This method involves looping through our dataset 100 times. For each iteration, we randomly divide the data into two equal-sized groups: an estimation and validation dataset. For the estimation dataset, we estimate separate conditional expectation functions of the outcome (water use) for the treatment and control villages. We do so by using LASSO to select the covariates from the vector z_i that best predict measured water levels. The difference between the conditional expectation functions for the treatment and control groups delivers a "heterogeneity score", i.e. a predicted treatment effect as a function of the farmer characteristics z_i.

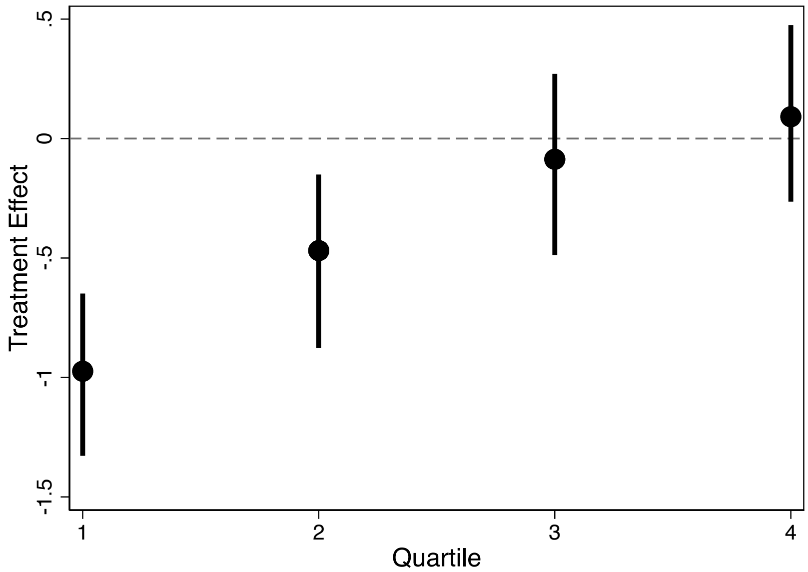
Figure C1 shows that the heterogeneity accurately predicts the treatment effect heterogeneity across the 100 validation datasets. Put differently, water savings from the AWD treatment are larger for the farmers with the most negative values (bottom quartile) of the heterogeneity score. Table C2 shows that being in a village with volumetric pricing is one of the most important determinants of the predicted heterogeneity. Ninety three percent of farmers in the first quintile of the heterogeneity score (those predicted to save the most water with the technology) have volumetric water pricing. In contrast, only 5 percent of the least affected farmers do. Of the 19 covariates included, volumetric pricing explains the most variation in the predicted treatment effect. Using the same procedure with the heterogeneity on card ownership from Table 4, Table C3 shows that owning an individual prepaid card is also one of the most significant determinants of the heterogeneous water savings from using AWD.

Table C1: Robustness of water-usage results to interactions between volumetric pricing and covariates

	Overall		0-70 Days After Planting	
	(1) Level	(2) Dry	(3) Level	(4) Dry
Treatment	0.165	-0.018	0.012	-0.019
	(0.212)	(0.028)	(0.195)	(0.031)
Treatment * Volumetric Pricing	-0.673**	0.117**	-0.825**	0.195***
	(0.301)	(0.053)	(0.325)	(0.062)
Volumetric Pricing	-0.077	-0.051	-0.006	-0.071
	(0.339)	(0.060)	(0.378)	(0.064)
Strata Fixed Effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Controls X Treatment	Yes	Yes	Yes	Yes
Mean in Control	2.32	0.45	2.70	0.33
p-Value: Treat+Treat*Volumetric	0.013	0.020	0.001	0.000
Number of Observations	7588	7588	4181	4181
R squared	0.051	0.052	0.052	0.068

The data are from random unannounced visits to the study plots of sample farmers during the 2017 boro (dry) growing season. Columns 1 and 2 are for the entire season, while columns 3 and 4 are for measurements taken up to 70 days after transplanting. The dependent variable in columns 1 and 3 is the amount of standing water in the field, measured in centimeters. The dependent variable in columns 2 and 4 is an indicator variable for a dry field with no standing water. The (baseline) controls are all of those in Table A1 (age, years of education, household size, number of livestock owned, landholdings, television ownership, refrigerator ownership, tube well ownership, indicator for knowledge of AWD, indicator for a rented or sharecropped plot, plot area, number of crops grown, indicator for growing two rice crops, number of boro irrigations, revenue per acre in boro, boro total cost per acre, and aman revenue per acre). Standard errors are clustered at the village level. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Figure C1: Heterogeneous treatment effect by quartiles of predicted heterogeneity score



Notes: Figure shows the treatment effect of AWD across 100 different sample divisions — separately by the quartile of the predicted heterogeneity score. Following the methodology in Chernozhukov et al. (2018), we estimate the predicted heterogeneity score $\hat{s_{0i}}$ as described ABOVE. The figure shows the actual treatment effects in the other half of the data (not used to develop the heterogeneity score). The lowest quartile are the farmers predicted to have the *most negative* treatment effect of AWD. Each dot is the mean across the 100 sample divisions, and the vertical lines show the range from the 5th to the 95th percentile.

Table C2: Characteristics of farmers most and least affected by conservation technology

	Mean Most Affected	Mean Least Affected	Share Variation Explained
Volumetric Water Price	0.934	0.053	0.456
Age	40.941	45.355	0.007
Years Education	7.495	4.413	0.037
Household Size	4.875	4.586	0.003
Number Livestock Owned	2.461	2.552	0.000
Landholdings in Acres	2.643	1.394	0.049
Owns Television	0.760	0.431	0.049
Owns Refrigerator	0.144	0.068	0.008
Owns Irrigation Shallow Tubewell	0.136	0.011	0.040
Heard of AWD?	0.207	0.030	0.030
Plot is Rented or Sharecropped	0.081	0.074	0.001
Area in Acres	0.377	0.359	0.001
Number Crops Grown	2.729	1.902	0.308
Rice-Rice Cropping System	0.271	0.836	0.148
Number Irrigations in Boro	22.514	18.687	0.011
Revenue per Acre in Boro	48,567.625	34,707.254	0.205
Cost per Acre in Boro	26,842.287	23,260.350	0.026
Water Cost per Acre in Boro	10,046.160	5,258.934	0.057
Revenue per Acre in Aman	40,410.621	19,279.115	0.367

The table classifies farmers according to their predicted treatment effect from AWD, i.e. the predicted decrease in water usage during the first 70 days after planting. Column 1 shows mean values of characteristics for the 20% of farmers that are predicted to conserve the most water if treated. Similarly, column 2 shows mean values for the 20% of least-affected farmers. Column 3 shows the R^2 of a bivariate regression of the predicted heterogeneity score, s_0, on each characteristic.

Table C3: Characteristics of farmers most and least affected by conservation technology for Rajshahi sample only

	Mean Most Affected	Mean Least Affected	Share Variation Explained
Has Card	0.713	0.109	0.158
Age	42.847	41.453	0.001
Years Education	6.414	7.281	0.001
Household Size	4.739	4.693	0.000
Number Livestock Owned	1.828	3.011	0.016
Landholdings in Acres	1.730	2.945	0.022
Owns Television	0.552	0.891	0.063
Owns Refrigerator	0.090	0.146	0.001
Owns Irrigation Shallow Tubewell	0.007	0.221	0.078
Heard of AWD?	0.340	0.015	0.112
Plot is Rented or Sharecropped	0.037	0.184	0.024
Area in Acres	0.310	0.360	0.000
Number Crops Grown	2.341	2.610	0.019
Rice-Rice Cropping System	0.328	0.213	0.006
Number Irrigations in Boro	19.007	24.266	0.041
Revenue per Acre in Boro	41,840.078	53,002.148	0.367
Cost per Acre in Boro	23,921.357	29,440.611	0.035
Water Cost per Acre in Boro	7,128.027	12,498.362	0.046
Revenue per Acre in Aman	23,613.660	36,514.520	0.065

The table classifies farmers according to their predicted treatment effect from AWD, i.e. the predicted decrease in water usage during the first 70 days after planting. Column 1 shows mean values of characteristics for the 20% of farmers that are predicted to conserve the most water if treated. Similarly, column 2 shows mean values for the 20% of least-affected farmers. Column 3 shows the R^2 of a bivariate regression of the predicted heterogeneity score, s_0, on each characteristic.

Appendix D: Liquidity Constraints as a Possible Mechanism

This appendix investigates whether liquidity constraints explain our result that prepaid cards change the demand for AWD.[42] Our approach is to estimate whether the treatment effect of prepaid cards on demand differs by observable measures of liquidity constraints. The literature commonly proxies for liquidity constraints using income or liquid asset holdings (Zeldes, 1989; Johnson, Parker, and Souleles, 2006). Column 1 in Table D1 tests for heterogeneity along three dimensions: landholdings, livestock ownership, and the number of durable assets owned.[43] We find no evidence that prepaid cards increase AWD demand any more for farmers that are smaller, owner fewer livestock, or less durable assets.

We also take a different approach by proxying liquidity constraint tightness using data on actual card recharging behavior for the 323 treatment farmers for whom we obtained data on card usage. We observe the date, time, and total amount spent for each time the card was charged. Aggregating these data across the entire growing season, we first estimate the regression

$$Nrecharge_i = \beta_0 + \beta_1 TotalSpent_i + u_i, \tag{D1}$$

where $Nrecharge_i$ is the number of times the card was loaded with funds by farmer i and $TotalSpent_i$ is the total amount spent by him throughout the season. We use the fitted residual from this regression, $\hat{u}_i$, as a proxy for liquidity constraint tightness. This is a reasonable proxy because it measures the deviation between the actual and expected number of times a card was recharged, conditional on the total amount spent. In other words, we expect a higher value of $\hat{u}_i$ for a liquidity constrained farmer since he likely needs to load the card more often in order to spend the same amount on water.

We next estimate a function $\hat{u}_i = g(z_i) + \varepsilon_i$, where z_i is a set of baseline observables.[44] We estimate the function g using both a LASSO selection method and a Random forests estimator. The predicted values from each of these models (for all farmers in the sample) generates our measure of liquidity constraint tightness.[45]

[42] We did not pre specify the test of this alternative mechanism in our pre-analysis plan.

[43] The specific assets are a motorbike, indoor toilet, electric fan, television, refrigerator, and washing machine.

[44] z_i consists of age, landholdings, education, number of livestock owned, number of adults in the household, number of children in the household, baseline number of times a field is irrigated during the season, baseline per-acre water price, number of assets owned, access to electricity, tractor ownership, ownership of a shallow tube well for irrigation, and an indicator for whether water fees were paid to the deep driver (as opposed to the water user's committee).

[45] We first randomly divide the 323 observations into training and validation datasets. The training dataset is used to estimate the LASSO or Random forests model. The predictions from the Random forests

The treatment effect of prepaid cards on demand for the pipes should be concentrated on the more liquidity constrained farmers if the liquidity mechanism is important for our estimated demand effect. The results in Table D1 do not line up with the liquidity explanation. The effect of the prepaid cards is no larger for farmers that are predicted to have the tightest liquidity constraint.

are slightly more correlated with the actual $\hat{u}_i$ terms in the validation dataset: the correlations between predicted and actuals are 0.29 for Random forests and 0.23 for LASSO. The covariates selected by LASSO and the signs of their relationship with liquidity constraint tightness are age (+), landholdings (-), baseline seasonal water price (+), number of durable assets (-), and connection to the deep driver (+).

Table D1: Heterogenous effects of the prepaid card treatment by a predicted measure of liquidity constraints

	(1) Interactions	(2) Lasso	(3) Random Forest
Card Treatment	-0.0082	0.0500	0.0348
	(0.1130)	(0.0643)	(0.0616)
Card Treatment * Landholdings	-0.0029		
	(0.0053)		
Card Treatment * Number Livestock	0.0140		
	(0.0146)		
Card Treatment * Number Assets	0.0106		
	(0.0327)		
Landholdings	0.0076**		
	(0.0036)		
Number Livestock	-0.0097		
	(0.0131)		
Number Assets	-0.0035		
	(0.0265)		
Card Treatment * Liquidity Constraint		-0.0013	0.0084
		(0.0231)	(0.0216)
Liquidity Constraint		-0.0268	-0.0423**
		(0.0202)	(0.0184)
Strata Fixed Effects	Yes	Yes	Yes
Mean in Control	0.415	0.413	0.413
Number Obs	3477	3460	3569
R squared	0.025	0.032	0.034

The data are from the 144 villages that were part of the second-year experiment. The table tests whether the effect of the prepaid card treatment varies as a function of predicted liquidity constraints. The predicted measure of liquidity constraints is from a two step procedure where in the first step the total number of times a prepaid card was recharged (throughout the season) is regressed on the total amount spent. The residual from this regression gives a measure of liquidity constraint tightness since it measures the deviation between the actual and expected number of times a given farmer needed to recharge their card in order to spend a given amount on irrigation water. The second step involves predicting this measure of liquidity constraint as a function of observable characteristics z_i. Columns 1 uses predictions from a LASSO regression, while column 2 uses the prediction from a random forest algorithm. The dependent variable in both regressions is an indicator if the farmer purchased the AWD pipe at the randomly set price. Standard errors are clustered at the village level. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.