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Agricultural input subsidies, credit constraints and expectations of future transfers: evidence from Haiti

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Abstract

We examine the effects of a subsidy program in Haiti which provided smallholders subsidies for modern inputs (rice seeds, fertilizer, pesticides and specific labor tasks) through a randomized control trial. The program led to lower input use and lower yields in the year subsidies were received, and the decline in input use and yields persisted through the following year. Using data from a complementary information intervention in which randomly selected farmers were provided clarifications regarding their status in the program, we find evidence suggesting that incorrect expectations of future transfers help explain the disappointing outcomes. In addition, instead of increasing input use, subsidies seem to have led farmers to pay off their loans and take fewer new ones. In a complex post-emergency environment as the one in which this program took place, input subsidies may need to be avoided, as they require considerable information to optimally design and careful coordination by many actors to achieve the expected gains.

Keywords: Smallholders, Input subsidies, Yields, Impacts, Agricultural development

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1 Introduction

While recent decades have seen remarkable reductions in poverty and hunger in developing countries, increasing agricultural yields of staple crops remains a policy priority of governments in many low-income countries.[1] This is often specifically motivated by food security concerns, which have gained new urgency in light of the global pandemic (FAO 2020).

So-called "smart" input subsidy programs are often considered a useful policy option for increasing the adoption of modern inputs and raising agricultural productivity in developing countries (Jayne et al. 2018, World Bank 2008). "Smart" subsidy programs distinguish themselves from traditional agricultural input subsidy programs by a) targeting specific (usually poor) groups of farmers; b) providing a package of complementary subsidies for a limited amount of time; and c) supporting local markets, for instance by providing vouchers redeemable at private input providers. There exists very little experimental evidence on the impacts of such programs, with the notable exception of Carter et al. (2013, 2021), who show that a one-time fertilizer and seed subsidy in Mozambique was effective in promoting input use, leading to increases in yield, learning, and consumption levels. A systematic review of studies using a variety of approaches concludes that input subsidies are associated with increased use of inputs, higher yields, and increased income among farming households (Hemming et al. (2018)). It also notes that almost all existing evidence comes from Sub-Saharan Africa, and points to the importance of program implementation, design and contextual factors for effectiveness, as also highlighed by Jayne & Rashid (2013), Mason et al. (2013), Ricker-Gilbert & Jayne (2017). Little causal evidence exist, however, on programs that were ineffective, and on the underlying reasons.

Like any other program, the design of smart subsidy programs relies on a number of assumptions. Crucially, the inputs being promoted and subsidized must actually provide positive (risk-adjusted) profits to farmers. Additionally, farmers must face constraints to input adoption that can be overcome by a short-term subsidy. For instance, farmers may be credit constrained and/or unable to afford the costs of experimenting themselves, but these constraints may be

[1] Despite economists' long-standing emphasis to focus on profits rather than yield (Marenya & Barrett 2009, Foster & Rosenzweig 2010), many agricultural programs continue to target yield gains (Macours 2019).

lifted after an initial season of high profits. Alternatively, farmers may lack information about the returns to the inputs, and can learn about their value in a single season of experimentation. Such assumptions might not hold in all settings. The introduction of smart subsidy programs, even if intended to be only a short-term measure, could also raise (false) expectations about future receipt of subsidies. These false expectations could distort farmers' decision making.

This paper contributes with new experimental evidence that speaks to these issues. We study the impact of an agricultural "smart" subsidy program implemented by the Ministry of Agriculture, Natural Resources and Rural Development (MARNDR) between 2014 and 2017. The program provided smallholder rice farmers with one-time vouchers that allowed them to purchase subsidized modern inputs (seeds, fertilizer, pesticides and labor tasks.) It was designed in the wake of the destructive 2011 Haiti earthquake, a period in which there were massive aid flows into the country, weak institutional capacity, and preciously little data or information to inform the design of public public programs such as the one we study. The earthquake also had exacerbated long standing national food security concerns.

In coordination with MARNDR, a randomized evaluation was built into the early phases of the program, with 245 farmers from 16 localities allocated randomly to an early treatment (January 2014) and 270 farmers from 23 localities allocated to a late treatment group (September 2015). The late treatment group serves as a control group for the early treatment for the period of study.

We first estimate the short and medium-term effects of the program on farmers' inputs use, farm productivity and food security. We estimate impacts on outcomes in the agricultural season in which the farmers received subsidies, as well as one to two agricultural seasons later. The results show that farmers randomly assigned to receive subsidized inputs did not achieve higher yields on their rice plots than control farmers, and in fact produced less in the year they receive the vouchers. Instead of increasing input use, subsidies crowded out own-pocket expenditures and led to lower total quantities of input use.[2] Moreover, treatment farmers achieved lower rice yields and production values one to two seasons after receiving subsidies. These results cast doubt upon the appropriateness of smart subsidy programs for

[2]This contrasts with experimental evidence from India (Emerick et al. (2016)) and Mali (Beaman et al. (2013)), where subsidized access to one input led to a re-optimization with increased use of other inputs.

certain contexts.

We attempt to disentangle the mechanisms that can help explain these unexpected impacts. To do so, we draw on a complementary information intervention, implemented at the end of the first year. In this intervention, randomly selected farmers were provided clarifications regarding their status in the program for the following year: the selected treatment farmers were told that they were not going to receive any more subsidies, and the selected control farmers were told that they would receive subsidies the following season. This information intervention was motivated by observations during qualitative work, which suggested that both treatment and control farmers were uncertain about what to expect from the program in 2015, the year after the treatment arm received their subsidies. Such lack of clarity is not surprising in a setting with multiple agricultural development interventions conducted by different actors (government, international organizations, NGOs, etc) that change over time. Imperfect expectations and uncertainty could influence farmers' investment and production decisions, and as such are important to understand as potential underlying mechanisms for the overall program results. For instance, if farmers thought they would receive subsidies in subsequent seasons, they may have neglected to save for input costs. If the subsidies then do not actually materialize, this could lead to less input use than planned.

Our results suggest that erroneous expectations can indeed help explain part of the medium term effects. When randomly selected farmers were provided clarification regarding their status in the program, they showed a more modest decline in input use than farmers without the extra information.

The role of existing loans further helps unpack the findings. The lack of effects on input use in the year farmers received subsidies (2014) suggests that credit and information constraints may not have been decisive in the farmers' input use. In contrast with many programs in Sub-Saharan Africa, our data show that the subsidies were given for inputs that the farmers already knew and used to a certain extent. They frequently purchased the same types of inputs promoted by the program using loans from local traders. Because such credit is costly, the vouchers allowed indebted farmers to pay off their loans and not engage in new lending, hence potentially switching to a new lower-intensity equilibrium. Given the unfavorable weather

conditions during this study, this may well have been an optimal strategy for some.

The program's design therefore was likely based on a set of assumptions that may not have reflected reality—even if that was understandable given the extremely limited information available in the country post-earthquake. A number of developments in the design and the implementation of the program further help explain the unanticipated results. Some steps in the program implementation were delayed and others were incomplete. There were sudden changes in prices, and unclear communication with the beneficiaries. Our results thus serve to highlight that in certain environments, designing and implementing relatively complex programs such as a smart subsidy program may not be advisable. This is the case because such programs require both considerable information to optimally design, as well as careful coordination of timing and delivery by many actors, in order to obtain the expected gains. The results highlight that not getting assumptions and implementation right not only reduce effectiveness but can actually lead to perverse results.

As such, this paper contributes to the broader literature on the challenges in design of food security programs (Barrett (2002), del Ninno et al. (2005)), in particular as it relates to post-emergency settings (Pingali et al. (2005), Maxwell et al. (2012)). It also relates to the experimental literature on adoption of agricultural technologies and practices (see Magruder (2018) for a recent review).

The next section presents the context and the subsidy program design in more details. Section 3 discusses the experimental design, data and specification. Section 4 presents the main results on rice yield, profits and input use. Section 5 unpacks the mechanisms, by first discussing the role of expectations and the information intervention, and then providing results on loans and shifts in cultivation practices. Section 6 further discusses interpretation and implications, and section 7 concludes.

2 Context and intervention

2.1 Context

The study area covers the sub-districts ('sections communales') of Haut-Maribahoux (district, or 'commune', of Ouanaminthe) and Bas-Maribahoux (district of Ferrier), both of which are located on the border with the Dominican Republic. The area consists of arid and semi-arid plains located just above sea level, parts of which become swampy during rainy seasons. The irrigated areas, or lagoons, are filled with water from rivers, and almost exclusively cultivated with rice. There are two main rice growing seasons, starting respectively in December/January (winter season) and in August/September (summer season).

Table 1 reports baseline characteristics (collected in October-November 2013) of farmers in the study, all of whom had experience in rice cultivation (one of the eligibility criteria, as explained further below). On average, farmers cultivated 1.07 rice plots and 1.04 hectares of rice, with 72% of them having cultivated rice in 2013. The mean value of their rice production in 2013 was 335$. The rice farmers use moderately intensive cultivation practices, with irrigation and/or improved inputs. At baseline, 92% of farmers used irrigation on their rice plots, with 66% using chemical fertilizers (for yearly expenditures of 96$, i.e. 28% of their rice production value), and 63% applied pesticides (for yearly expenditures of 17$). Average spending on rice seeds was 77$.[3] The quantities of inputs used at baseline are less than the ones recommended by MARNDR.[4]

A substantial share of farmers pay for their input purchases using loans. Loans are often provided by petty merchants, who purchase the harvest from the farmers, allowing them to pay off their loans in-kind with their rice harvests. Such merchant loans were used for about 30% of purchases of fertilizer, 25% of pesticides, and 20% of seeds, among the control farmers in 2014. (This information was not collected at baseline). 32% of farmers at baseline reported selling rice directly from their plot and/or to an intermediary who directly harvests the field.

[3]Many farmers cultivate varieties of rice that allow harvesting up to 2 or even 3 times after a single planting (a practice called "retonn"). MARNDR does not recommend this practice as it is estimated to give lower yields than replanting.

[4]In an analysis conducted by the Inter-American Development Bank as part of the project's preparation, Bayard (2011) suggests that farmers in this region could obtain yields of above 5 tons per hectare (roughly 5 times the level at baseline) if they used the recommended quantities of inputs.

Water control is imperfect, and rice yields as well as the number of rice cycles cultivated per year vary significantly across plots, seasons, and years. In the first semester of both 2014 and 2015 (the two seasons for which we gathered data) most farmers faced drought conditions. In those conditions, in-kind loan repayments can represent a large share of the harvest.

While irrigated land is planted with rice, many farmers also plant plots of dry land with food crops, such as corn, beans, manioc, yam, sweet potato and peanuts. These crops require fewer agricultural inputs than rice, but rice cultivation constitutes by far the main source of agricultural income. It is also common for farmers to use their land surpluses for livestock farming and/or to generate additional revenue with charcoal and wood plank production. Income diversification is limited however, with only 20% of household heads having a non-agricultural occupation. Educational levels are low, with only 29% of household heads are literate. Levels of food insecurity are high, with 34% reporting severe hunger (as measured with a standardized hunger score[5]) and 3.0 months of food insecurity per year on average.

2.2 Intervention

The voucher program was part of the Project of Technology Transfer to Small Farmers (PTTA), a project managed by Haiti's Ministry of Agriculture (MARNDR), funded by the Global Agriculture and Food Security Program (GAFSP), supervised by the Inter-American Development Bank (IADB), and implemented locally by private operators. The main component of the program consisted of providing farmers with vouchers that they could exchange for modern agricultural inputs. The program operated in 10 communes across two departments. Vouchers were distributed to around 30,000 smallholder farmers, targeting a variety of different crops. Our study was conducted during the initial phase of PTTA, covering rice farmers in the Nord-Est department. [6]

The program was designed to be implemented as follows:

[5]The household hunger score used is based on a food security scale developed by USAID that has been validated for cross-cultural use. Scores of 0 or 1 indicate little to no hunger, 2 or 3 moderate hunger, and 4 to 6 severe hunger in the household. More information on the score and its relation to other food security indicators can be found here: https://www.fantaproject.org/sites/default/files/resources/HHS-Indicator-Guide-Aug2011.pdf.

[6]This study focused on rice, as this was one of the first crops for which vouchers were distributed, and effects were expected to be measurable in one season. A year later, a separate RCT was set up among horticulture producers of the Saint-Raphael area, but suffered from low compliance with the experimental design. This paper therefore focuses on the North-East experiment only.

1. Potential participants registered for the program during a set of public meetings in September and October 2013.

2. Program officials visited each registered farmer to verify their eligibility - eligibility was conditional on current access to at least 0.25 hectares of land that could be cultivated with rice and previous experience in rice cultivation. About 10% of registered farmers were deemed ineligible.

3. The program distributed physical printed vouchers, issued by a participating bank, for free inputs to eligible farmers - which were meant to correspond to the recommended amount of inputs for 0.5 hectares of cultivation. Farmers with less than 0.5 hectares of cultivable land available received vouchers for 0.25 hectares.

4. The vouchers for seeds, fertilizer and pesticides could be redeemed at specific local input dealers. Therefore, the quality of inputs received in this program should be the same as farmers normally purchase for themselves, as they are obtained from the same sources. The program team worked with these local dealers to ensure their participation and to try to ensure that inputs were in stock. Local dealers were reimbursed the value of the vouchers via bank transfer after turning them in to the program team. The vouchers for labor services (one that was to be used either for plowing or transplanting, and one for pesticide application) could be used to pay selected local providers of these services through a similar mechanism.

Table 2 shows the transfers that were intended transfers from the vouchers and associated compliance. The first two lines show the values of the vouchers and the quantities of inputs or services that farmers were intended to receive, based on market prices when the voucher scheme was designed. Farmers received vouchers for land preparation, seeds, chemical fertilizer, and pesticides. Farmers with 0.5 hectares were entitled to a package of vouchers with a value of $440, with the fertilizer and plowing service having the highest value (each about a quarter of the full value). Both administrative records and farmer self-reports confirm that the large majority of the farmers who signed the contract received and used the vouchers. The next two lines in Table 2 show administrative information reported by the bank on both the share of

vouchers that was printed (and hence distributed) and the share that was paid to input suppliers. The next 3 lines reflect what farmers reported in the survey regarding distribution and use of vouchers. The two sources are largely in accordance, with the exception of the voucher for pesticide application (representing less than 10% of the total amount). Compliance was high for vouchers for seeds, fertilizer and plowing, transplanting, and pesticides (between 76 and 96% depending on and type). However, according to farmers there was less usage of the pesticide application voucher (about 32%), possibly due to bottlenecks in the supply of these services. Pesticide application represents less than 10% of the total voucher value.

The timing of vouchers distribution was affected by delays in bank operations and weather conditions, and varied across localities. In early 2014, some farmers had to wait for a few weeks after the standard planting time to receive their vouchers—a third of them report receiving seeds and fertilizers "late". Second, after voucher distribution began in 2014, it became apparent that farmers were facing severe drought conditions, resulting in many planning not to cultivate rice during the first season of 2014. The project therefore halted voucher distribution, and allowed farmers to use distributed vouchers during the following season (late 2014). As a result, only about 70% of of farmers (or about 80% of ones who received vouchers) report they could use the vouchers for chemical inputs and plowing services on time.

In addition, while the value of the vouchers was originally set to correspond to technically recommended amounts of inputs, this proved difficult to adhere to due to changing input prices. Specifically, the price of fertilizer rose sharply in the project area between when the voucher scheme was designed and vouchers were redeemed. The project explicitly allowed dealers to accept the vouchers while providing inputs at current market prices. This resulted in farmers receiving lower quantities of inputs than originally planned. While the recommended quantity of chemical fertilizer is 200 kilograms per half-hectare (400 kg per hectare), given price changes farmers ended up receiving only 135 kilograms of chemical fertilizer (NPK and urea) for free. Additionally, due to a supply-side shortage, sulfate (one of the recommended inputs) was not available.

3 Empirical strategy, data and econometric specification

3.1 Empirical strategy

To evaluate the impacts of the voucher scheme on rice yields, profits and food security, the program was phased in randomly in close collaboration with MARNDR. Farmers in this area of Haiti are organized into loosely defined geographical units known as "habitations", which are akin to rural villages. The program planned to operate in 39 habitations, which comprised all the rice-growing areas in the two study sub-districts. Of these 39 habitations, 16 were randomly chosen to receive the vouchers in 2014, while the rest only received vouchers after August 2015. This timeline allows for estimating the effects of the intervention until mid-2015. Randomization was done within 14 strata defined by the two sub-districts, a binary measure of the quality of water access in the village (based on an assessment by the program team), and the number of farmers who registered.

The sample was drawn from farmers who registered and were eligible for the program. We surveyed all the households in habitations with up to 30 eligible households. In habitations with 31 or more eligible households, we drew a random sample of 30 households. 521 farmer households were surveyed at baseline. Due to a small amount of attrition, 515 farmers (240 farmers in 16 treatment habitations and 275 in 23 control habitations) were followed up until August 2015 and comprise the final sample.[7]

Besides the aforementioned issues with voucher timing and value, compliance was high. Among the surveyed population, as reported in Table 2, 87 percent of treatment farmers report receiving, and 85% report using, at least one voucher. Contamination with the control group was low, with only 3 control farmers reporting receiving vouchers in 2014. This was verified through administrative project records of voucher distribution and use.

In November 2014, a small complementary information intervention aimed at clarifying farmers' status in the program was delivered to a random subset of farmers. This information was delivered after the second (and final) season in which treatment farmers could use their vouchers. The information intervention consisted of distributing and explaining a leaflet clar-

[7]We initially planned for a staggered incorporation with a third group but operational constraints made it impossible to implement such a design; this explains the imbalance between the treatment and control groups.

ifying the farmers' status in the program. This intervention was delivered by field staff hired through the research team, with the permission of the MARNDR and the private operator of the voucher scheme. Treatment group farmers (who received vouchers in 2014) were reminded they would not receive any further subsidies in subsequent agricultural seasons, and control group farmers (who had not received any vouchers in 2014) were informed they would be distributed vouchers in 2015.

To identify the impact of this information intervention, households were individually randomized into "informed" and "uninformed" groups within each treatment and control habitation. Half of the households in each arm (i.e. 120 treatment farmers and 138 control farmers) were selected to be informed of their status in the program, while the others received no additional information.

3.2 Data

Data was gathered from various sources between 2013 until 2015. When farmers registered for the program in late 2013 they provided basic information including cultivated land, previous experience in rice cultivation, and quality of access to irrigation. Next we conducted three household surveys: a baseline in October-November 2013 (before the random selection of treatment habitations), a first follow-up in February 2015, and a final follow-up in August 2015. These surveys gathered detailed information on agricultural production, inputs and practices, experience in the program as well as food security.

In addition, a short survey collected information on perceptions of the program and anticipation of future receipts of subsidies. This was conducted among half of treatment and control farmers in November 2014, as part of the information treatment described above. 120 treatment and 140 control farmers answered this short survey.

The first follow-up survey (in February 2015) covers the short-term impacts of the program in 2014, the year farmers received the subsidy. The second follow-up survey (in August 2015) covers the impacts in the first semester of 2015, when neither the early nor the late treatment group received vouchers. It hence captures medium-term impacts one-to-two seasons after the end of subsidies for the early treatment farmers.

Appendix Table A1 reports balance checks, and shows that baseline household characteristics and rice productivity were balanced between treatment and control.[8] The estimates confirm randomization led to reasonable balance in baseline observables: no more than about 10% of variables show statistically significant differences at 10%, as expected. Mean membership to an agricultural association is statistically significantly higher and mean number of children lower among treatment farmers with p-values lower than 0.01, but baseline mean values of important outcomes such as rice production and productivity, and food security, and seed and fertilizer use are all balanced between treatment and control farmers.

3.3 Econometric specification

We estimate the effects of the main intervention using the following ordinary-least-squares (OLS) regression model :

$$y_{ihst} = \alpha + \beta T_{hs} + \delta y^0_{ihs} + \mu_s + \nu_{ihs} + \varepsilon_{ihst} \tag{1}$$

where y_{ihst} denotes an outcome of farmer or household i in habitation h of stratum s at date t, T_{hs} is a dummy indicating treatment status of habitation h of stratum s, y^0_{ihs} is the outcome at baseline when collected (indicated in results tables), and μ_s are fixed effects for the 14 randomization strata. Although the randomization strata were designed to proxy for water access, during the droughts water access also depended heavily on the specific lagoon area a plot was located in. Therefore, in addition to fixed effects for the original strata, we include ν_{ihs} fixed effects for the six lagoon areas in which a household's plots are located at baseline. We estimate "intent-to-treat" (ITT) effects of having access to the program on eligible, registered households.

The standard errors are clustered at the habitation-level as the intervention was randomized at that level. Continuous outcome variables are windsorized at 99th percentile. Given that randomization of the main treatment was at the habitation level, and that there is a relatively limited number of habitations (39), we also report p-values based on randomization inference,

[8] We report these tests for the main characteristics and outcomes at baseline used in the analysis. We tested balancing for a broader set of variables and obtained similar results (available from the authors).

in addition to the conventional p-values for the OLS estimates. These p-values refer to exact Fisher tests of the sharp null hypothesis of zero effect (on any farmer in the sample) of the main treatment, and were obtained based on 2000 permutations.[9]

Using the specification in equation (1), we obtain the short-term effects of the program on outcomes in 2014, as measured in the first follow-up survey in February 2015. We also estimate the medium-term effects in the first season of 2015, as measured in the second follow-up survey in August 2015. With the exception of farming practices, all outcomes in the main tables are unconditional so the estimated effects capture both the extensive and intensive margins and are not driven by selection into rice farming (or loan taking). The unconditional variables have zero values for farmers who did not engage in rice production. As both in 2014, and especially in 2015, there is a substantial share of farmers that did not cultivate rice in the seasons covered by the survey, we also report results at the extensive margin (any harvest) as well as the total value of rice harvested.[10] Appendix tables report outcomes conditional on rice cultivation.

To estimate the effects of the complementary information treatment we use outcomes measured in the second follow-up survey, and interact the main treatment with a binary indicator variable capturing assignment to information provision. We estimate the model:

$$y_{ihst} = \alpha + \beta T_{hs} + \gamma T_{hs} * I_{hs} + \chi I_{hs} + \delta y^0_{ihs} + \mu_s + \nu_{ihs} + \varepsilon_{ihst} \quad (2)$$

4 Main results

4.1 Yields and profits

The PTTA program's explicit objective was to increase rice yields. Table 3 therefore first shows the estimates of the main intervention effects on rice yields and rice profits. Rice yields

[9] Following Young (2018) this accounts for the possibility that asymptotic properties of the statistics for average treatment effects do not hold, given limited number of clusters used in the experiment - with such sampling designs, the risk increases that some outliers concentrate coefficient leverage and drive the estimated effects. The distribution of the statistics for the average effect under the sharp null of no treatment effect is obtained by computing the statistic for each possible alternative assignment of treatment - we rely on automatic permutations considering only realizations of the re-sampling variable which exist in the data and accounting for the sampling strata and clusters. Exact p-values, defined as the fraction of potential outcomes that have a more extreme or equal test statistic value, are obtained using the rank of the observed absolute test statistic.

[10] A few farmers had already harvested a second crop by August 2015. The estimated effects remain qualitatively similar when these are included.

are evaluated in 2014 as total quantity of rice harvested in the two seasons of 2014, over total area planted with rice, where the area is counted twice if the same plot is used in two seasons (or if the household harvests twice from the same seeds). Yield is set to 0 for those who chose not to grow any rice. In 2014, the year the treatment group received vouchers, the treatment group had very similar yields to the control. The positive point estimate of the treatment effects is close to zero, (18 kg compared to 966 kg per hectare in the control) with large standard errors. Appendix Table A2 reports the corresponding estimates with yields and profits defined conditionnally on rice cultivation; the results are similar, indicating that these results are not driven by selection into rice cultivation.

Annual profits from rice are calculated using the value of rice production (evaluated at [median] rice price), and all costs incurred during the season including expenditures on fertilizers, pesticides and paid labor.[11] Mean profit from rice of control group farmers is low, at $114, in 2014. When counting the input expenditures paid for with vouchers as costs, 2014 profits of treatment farmers are not statistically different to those of the control, in line with the unchanged yields (column 4). Of course, as treatment farmers did not actually pay these expenditures out of pocket, their revenues in 2014 were higher than in the control farmers, by around $239. This increase in profits is notably lower than the market value of the vouchers ($440).

Surprisingly, in the first semester of 2015 (in which neither the treatment nor control group received vouchers), the treatment group had significantly lower yields than the control group. The difference is substantial: 295 kg per hectare, which is a 36% reduction compared to a control mean of 766 kg/hectare. These lower yields are not, however, reflected in significantly lower profits (the profit of control farmers are already very low, at only $19, in the first semester of 2015).

As changes in yield could result both from changes in the area dedicated to rice cultivation (the yield denominator) and from changes in harvest, we also consider these components separately. Appendix Table A3 shows estimates of the effects on rice planting. Respectively 88% and 81% of treatment and control farmers grow rice in 2014 with the difference not sta-

[11] We do not include family labor in the calculation of profit given that its valuation is difficult and we do not see differences between treatment and control regarding their reliance on paid versus family labor.

tistically significant (p-value of 0.13). The shares of treatment and control farmers growing rice are lower in the first semester of 2015, at respectively 59% and 66%, and a larger share of treatment than control farmers stopped producing in first semester of 2015.

When considering the cultivated area (counting the first planting and possible additional harvests as separate rice instances), treatment farmers cultivated rice on smaller areas in 2014 (1.37 ha compared to 1.58 ha in the control) but the difference is only marginally statistically significant (conventional p-value of 0.07 and RI p-value of 0.17). The point estimates is also negative but not significant in 2015.

Appendix Table A4 reports the estimates of the effects on rice harvests. A lower share of treatment than control farmers harvested in the first semester of 2015 (12 p.p. less), in line with the declines in planting and yields. Treatment farmers also more often lost a planted harvest: 8 p.p. more compared to a mean of 22% in control group (RI p-value of .11). The total value of rice harvests declined, significant with conventional p-values (0.06) and marginally so with RI ones (0.16).

Thus, in 2014, the vouchers have no effects on yields or cultivation, while only marginally decreasing cultivated areas. In the first semester 2015 (when no more vouchers were distributed) both extensive and intensive margins adjust in unexpected directions as rice cultivation and yields decline among treatment farmers. Treatment farmers benefited from the transfers in 2014 but only through the saved inputs spending, not through higher yields.

4.2 Input use

We now examine input use, as this was the primary mechanisms through which PTTA aimed to affect yields. Table 4 shows the estimates of the main intervention effects on the use of inputs, measured by quantity of chemical fertilizer per hectare, expenditures in chemical fertilizer and pesticides, and expenditures in labor. The value of input use paid for by vouchers are accounted for in the measures of expenditures.

In spite of the vouchers, treatment farmers do not use more fertilizer per hectare than control farmers in 2014. Total expenditures for fertilizer and pesticides (for all rice plots combined) is lower in the treatment than in the control group (though marginally not significant with exact p-

values; conventional and RI p-values are .05 and .14, respectively.) That said, total fertilizer use is higher than the fertilizer directly obtained from the vouchers (with the total value of fertilizer expenditures at 194 in the treatment, compared to 135 obtained from the vouchers).[12] The point estimate of the ITT effect is also negative for expenditures pn labor but standard errors are large. Overall, despite the input subsidies, farmers in the treatment shifted to a marginally less intensive use of inputs in 2014.

The treatment farmers maintained this lower input use in 2015. Treatment farmers then use significantly less fertilizer per hectare (56 kg compared to 195 kg in the control, a 29% decrease). Spending on fertilizer and pesticide declines by $29 (a 30% decline) in the first semester of 2015. Spending on labor also decreases by $32 (23%) (marginally significant with conventional and RI p-values of 0.07 and 0.14).

Disentangling these results a bit further, Appendix Table A5 shows the estimates of the effects on precise quantities of urea, NPK and sulfate. While the quantities of urea and NPK were not significantly affected (although the point estimate is negative for urea) in 2014, the quantity of sulfate significantly declined by 29 kg (from a mean in the control of 71 kg, a 42% decline). This is possibly because stocks of sulfate in the area had run out by the time farmers received their vouchers. In 2015, however, they then continue to use less sulfate (the treatment effect is -10kg, still about 40%), and also use significantly less urea (24 kg, also about 40% decline). The point estimate is also negative for NPK but not statistically significant (conventional and RI p-values of 0.17 and 0.24).

These results confirm first that, in 2014, the subsidies did not increase the input use of treatment farmers but crowded out their own purchases, and second, that the intervention persistently decreased the intensity of rice cultivation in 2015 with a lower usage of chemical fertilizers and also of labor.

[12]Treatment farmers report using less fertilizer per hectare than they were given in the vouchers.

5 Mechanisms and discussion

5.1 Uncertainty of future benefits and information treatment

One possible mechanism to explain farmers decreasing their input use in 2015 is that farmers may not have understood that they would receive vouchers for only one season. If farmers believed that they would receive vouchers in 2015, they may have neglected to purchase inputs themselves. To test this mechanism, we compare treatment farmers who did and did not receive the complementary information treatment. As explained earlier, the information intervention simply told farmers whether they could expect to receive vouchers in 2015. Farmers in the treatment group were reminded that they would not receive any more vouchers, and farmers in the control group were reminded that they would receive vouchers in the upcoming winter planting season in December 2014/January 2015.

Table 5 shows the effect of the experimental treatments on expectations of receiving vouchers in 2015. It reports the differences in expectations between the treatment and control groups in November 2014 (before the information intervention was implemented), and also the effects of the information treatment, measured in Febrary 2015. In November 2014, while 71% of control farmers correctly expected to receive vouchers in 2015, 42% of treatment farmers also expected to receive new vouchers in 2015, confirming that many of them have imperfect information. In February 2015, 87% of the control farmers who did not receive additional information expected to receive vouchers, and this increases to 94% among those who received information. But strikingly, the majority of early treatment farmers (62% of the uninformed) also still expected to receive vouchers. The information treatment reduced this share by about 10 percentage points (with the difference between informed and uninformed treatment farmers marginally not significant with a p-value of 0.11). These results show that there were considerable erroneous expectations about renewed vouchers among those that had benefited the year earlier, and the information intervention was only partially successful at addressing this misinformation.

Table 6 shows the effects of the main and information treatments effects on yields and profits. The decrease in yields in the first semester of 2015, of 290 kg per ha, is the largest

for uninformed treatment farmers, but is also negative for informed farmers, at 215 kg per ha. Yields are not statistically different between informed and uninformed treatment farmers. The decrease in profits in the same semester is significant only for the uninformed treatment farmers, and the estimate of the effects on the uninformed, at 80$, is much larger than the one for the informed, at 17$, This difference is marginally not significant with a p-value of 0.12.

Table 7 shows the effects of the two treatments on the use of inputs in the first semester of 2015. The point estimates suggest that the decline in the quantities of fertilizer used per hectare, expenditures in fertilizer and pesticides and expenditures in labor are almost twice as large for the uninformed than for the informed (respectively 41 kg/ha, $24 and $21 compared to 27 kg per ha and $15 and $11 for the informed). The differences between uninformed and informed treatment farmers are not statistically significant.

As the information intervention was only partially successful in aligning farmers' expectations with the planned voucher distribution in 2015, the contrasts between the informed and uninformed arguably lacks power. Nevertheless, the results suggest that wrong expectations can partly help explain the decline in cultivation intensity among early treatment farmers in the first semester of 2015. Indeed as half of the informed treatment farmers still expected vouchers, it is possible that all 2015 results are driven by these expectations. It is of course also possible that there is substantial misreporting in variables asking for farmers expectations, as farmers may have thought it strategic to say that they expected further transfers. Moreover, expectations cannot explain the decline in production in 2014. Therefore it is worth analyzing potential additional mechanisms underlying the move to lower intensity rice production, to which we turn next.

5.2 Use of credit

Smart subsidy programs are often advocated for contexts in which farmers may not know the return to specific inputs, and may be liquidity constrained to experiment and learn on their own. In our context, baseline data shows that farmers had experience using the promited inputs, and therefore were unlikely knowledge-constrained. Liquidity constraints could still have limited, however, use of optimal levels of inputs. We hence consider the extent of the credit constraints

they face.

Table 8 describes loan-taking among control farmers. Significant shares of these farmers request and obtain loans, specifically for inputs. In late 2014, 66% report having requested a loan from any source, and 48% requested one to pay for inputs. Loans are requested from both banks and informal sources, primarily small merchants. (Loans for inputs are seldom requested from other households). In 2014, 31% of farmers report having asked, and 27% obtained, a loan from a bank, The same year, 31% requested a loan from a trader and almost all of them obtained it. In addition, may loans are are longer-term than a single agricultural season: 60% of bank loans and 31% of merchant loans were for 6 months or more. Although the data is noisy, we obtained estimates of the mean interest rates and these are higher for merchant loans (6.9% monthly in 2014) than bank loans (5.3%). These same patterns are observed in 2015. Farmers thus to some extent have some access to finance.

Table 9 reports the estimates of program effects on access to finance. Consistent with receiving subsidies in 2014, treatment farmers request less loans from either banks or merchants and on average borrow smaller amounts. Almost no treatment farmers request a loan for inputs from a bank while 14% of control farmers do, and 17% of them request such a loan from a merchant against 26% of control farmers. The mean amount borrowed to traders decreases significantly by about half, from 49$ among the control to 25$ among the treated. The point estimate is also negative but not statistically significant on the mean amount borrowed a bank. Hence farmers seem to substitute the vouchers for own expenses to acquire inputs in 2014.

More strikingly and in accordance with the decline in cultivation intensity, farmers continue to request fewer loans from traders in 2015: only 12% of treatment farmers do so against 20% of control ones. The amount treatment farmers borrow from banks also decreases by $64 from a control mean of $176. This reduction in inputs loans in 2015 suggests that farmers' profit maximization might have led some of them to reduce input use, possibly to avoid taking up new loans in a context of high interest rates and low returns to cultivation with bad weather conditions. Contrary to the yield-increasing objectives of the government, the one-time subsidies might hence have allow some of them to switch to a new less-intensive and low-debt

equilibrium. [13]

Table 10 introduces the additional effects of the information treatment. While the effects on loans from traders do not vary with the information on future subsidies, only 16% of uninformed treatment farmers borrow from banks while 24% of informed ones do, and these differences are significant at 1%. The mean amounts borrowed are also lower among uninformed treatment farmers.

5.3 Cultivation practices

As described above, rice cultivation in the study area is highly dependent on access to water, which remains difficult to predict for farmers until late in the agricultural season. These risky weather conditions, together with the lack of technologies to protect from those, generate uncertainty on optimal investments. It may be hard for farmers to determine both the optimal quantities of inputs they should apply and the broader cultivation practices for maximizing rice profits.

Treatment farmers received limited technical assistance, with 35% reporting to have received visits from the program contractor agents. These agents seem to have advised farmers on a few specific techniques, like transplanting using three holes and leaving sufficient spacing between seeds (i.e. using fewer seeds overall). They would also have advised against practicing *retonn* (multiple harvesting from the same seeds), especially as the variety sold by the input dealers in exchange for the seed vouchers is not a retonn variety. The first 3 columns of Table 11 shows the intervention effects on these farming practices. In 2014, the treatment indeed did seem to have induced a shift to those practices. Specifically, treatment farmers report to lower quantity of seeds per hectare by 33 kg (30%), 20 p.p. more of them (twice as many as the control farmers) transplant three plants per hole, 14 p.p. more of them (a third less) practice *retonn*. Some of these effects, while declining (with the exception of *retonn*), persist in 2015. Treatment farmers are still using 11 kg (11%) less seeds per hectare, are 8 p.p. more likely

[13] Table A6 reports the effects of the program on the shares of households acquiring some of the main agricultural inputs (fertilizer, pesticides and seeds in panels A, B and C respectively) from various sources. The estimates confirm that the majority of treatment farmers used the PTTA vouchers to source inputs, but also that they used less often loans in both 2014 and 2015 to pay for inputs. Treatment farmers also less often purchased fertilizer and pesticides in the Dominican Republic.

to seed 3 plants per hole, and 15 p.p. less likely to practice *retonn*. As farmers thus shifted towards recommended practices, it seems somewhat unlikely that the lack of results on rice productivity is driven by the limited technical assistance. Hence, treatment farmers continue using some recommended practices in 2015, though these did not translate to higher yields.

5.4 Welfare

In sum, the experiment reveals unanticipated negative effects of the program on rice production and yields. This suggests that assumptions underlying the design of the intervention may not have held, and several pieces of evidence indeed point in that direction. Before the interventions, farmers had some knowledge of the subsidized inputs and access to credit, so possibly neither knowledge about the return to inputs nor liquidity were a major constraint. Additionally, weather risk seems a more severe constraint in this context. In periods of drought, farmers experience low yields, meaning they may prefer to use fewer inputs and take fewer loans, therefore mitigating their risk. This then was likely compounded by erroneous expectations about the continuation of the subsidies.

Even though farmers experienced reduced production and yields, the subsidies they received during one season in 2014 represent, on average, transfers of about 440$. This intervention seemed to have reduced farmers reliance on loans with high-interest rates, and as such could have generated further welfare gains. We hence turn to impacts on food security, and also invest impacts on asset ownership and other economic activities, to trace possibly alternative ways the transfers may have been used (we do not have information on total household consumption or income). These estimates are reported in Appendix tables A7 to A10.

Food security, a relevant welfare economic outcome in this context and a target outcome for this program, is measured using a household hunger score (the highest values of which indicate severe hunger) and the number of months of food insecurity (measured only in 2015). We find no evidence of gains in food security in 2014. For 2015, we find some weak evidence of gains as the mean hunger scale measure decreases significantly by about 15% (p-values of 0.03 and 0.16), but prevalence of severe hunger and the number of months of food insecurity do not vary significantly.

We have information on assets such as financial, physical and land assets, which we summarize using synthetic indexes using principal component analysis (we use the first components to construct the different indexes). There is no evidence of significant effects on any of the financial, housing, farming or livestock assets in either 2014 or 2015. If anything, treatment farmers dis-invest in land, as treatment farmers reduce slightly their number of plots and ownership of a lagoon plot. We also find a marginally significant reduction in spending on irrigation.

We also consider the effects on cultivation of other crops and work in the Dominican Republic. If anything, treatment farmers harvested and sold less of other crops. We do not find a difference in 2015 of the number of households working in the Dominican Republic. We do not have information on other economic activities.

Overall, and despite the fact that farmers received subsidies that added up to a sizable transfer, we are unable to trace welfare or investment effects of those transfers. However, as our survey instrument was not designed with welfare as a primary outcome, it is possible that we are missing other welfare effects of the program.

6 Conclusion

We used data from a randomized control trial to examine the effects of a subsidy program in Haiti which provided, through vouchers, subsidies for modern inputs (seeds, fertilizer, pesticides and specific labor tasks) for rice during one season. The results show that the ”smart” subsidy program led to lower input use and lower yields in the year subsidies were received, but also in the following year. Farmers who received vouchers chose to use them to substitute for inputs they otherwise would have financed with credit, rather than to increase input use. This may have been due to drought conditions in the year of the intervention. While this behavior lowered yield, it did not lead to lower profits, and indebtedness declined. In the following year, when no more subsidies were received, the farmers who had received vouchers in the previous year continued to use less inputs and had lower yields.

These results were likely due to a number of mechanisms. Using data from a complementary information intervention in which randomly selected farmers were provided clarifications

regarding their status in the program, we find evidence suggesting that incorrect expectations of future transfers led farmers to reduce their investments in inputs and can help explain the disappointing outcomes. In addition, instead of increasing input use, subsidies seem to have led farmers to use lower quantities of inputs, and to pay off their loans and take fewer new ones. The decline in input use persists in the next agricultural season.

Independent of the mechanisms, the results suggest that the voucher program did not result in high yield in this setting. This may appear logical after the fact, but clearly was not what the government or other stakeholders anticipated or aimed for when designing the program.

To evaluate the external validity of these findings, it is important to keep certain unexpected developments and flaws in the implementation of the program in mind. Some steps in the program implementation were delayed and others were incomplete, there were also sudden changes in prices, and unrealistic expectations about future benefits may have resulted from unclear communication with the beneficiaries. Yet, such events and imperfect implementation are arguably to be expected in the type of complex post-emergency environment in which this program took place. Our results therefore serve to highlight that in such environments, smart subsidy program may need to be avoided, as they require considerable information to optimally design, as well as careful coordination and timely and correct delivery by many actors, in order to deliver on the expected gains. The results in this paper show that not getting assumptions and implementation right not only reduce effectiveness but can actually lead to perverse results.

As food security concerns are high on the policy agenda following value chain interruptions related to the global COVID-19 pandemic, and as the policy response in many countries has been to revert to input subsidies to boast domestic agricultural production (Ebata et al. (2020), Kennedy & Resnick (2020)), these lessons arguably have broad and immediate relevance.

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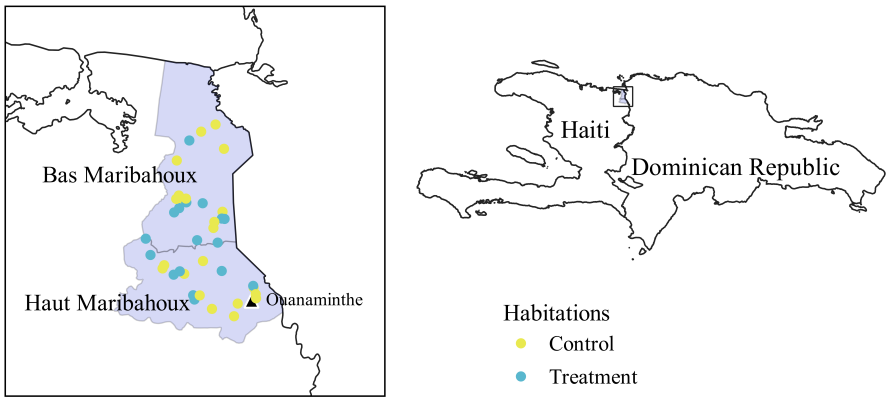
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Figure

Figure 1: Map



Notes: Map of the two sub-districts ('sections communales') and locations of localities ('habitations') in the study.

Tables

Table 1: Farmers' characteristics

Variable	Mean	s.d.
Head can read or write	0.287	.453
Head has non-agricultural occupation	0.196	.398
Grows rice	0.717	.451
Nb of plots cultivated with rice	1.068	.950
Total area of plots cultivated with rice (if any)	1.039	1.886
Rice production value	335.1	667.8
If household used fertilizer	0.676	.469
Total spending in fertilizer	96.1	148.8
If household used pesticide	0.629	.484
Total Spending in Pesticide	17.5	29.7
Total Spending in Seed	77.5	139.3
Sold rice to intermediary and/or on field	0.318	0.466
Asked for a bank loan in previous year	0.309	.463
Severe hunger	0.337	.473
Months food insecure	3.031	2.283
Observations	515	

Notes: Descriptive statistics of characteristics of study farmers at baseline in October-November 2013. Monetary amounts are in USD. Hunger is defined using the Household Hunger Scale.

Table 2: Program implementation

	Seeds	Fertilizer	Pesticides	Plowing services	Transplantation services	Pests application services
Voucher specifics (for 0.5 ha)						
Value (USD)	37.5	112.5	50	125	75	37.5
content	12kg	200kg	4 types	animal or mechanical		
Bank-recorded voucher distribution and payment						
Vouchers printed	90.0%	90.0%	87.0%	93.0%		87.0%
Vouchers paid	86.0%	77.0%	78.0%	84.0%		81.0%
Farmer-reported voucher receipt and usage						
Vouchers received	83.3%	85.8%	77.5%	85.4%	10.8%	35.0%
Vouchers redeemed	80.0%	83.3%	75.8%	79.6%	8.3%	32.1%
Vouchers could be used on time	71.2%	75.0%	71.2%	67.5%	7.5%	30.8%

Notes: Banks records give the shares of vouchers printed and paid among farmers of treatment habitations in the study sample. Bank records do not distinguish between vouchers for plowing and transplanting, so bank data is combined for these vouchers. Farmers reports give shares of farmers who received and used vouchers, and who said they were able to redeem vouchers for inputs and services sufficiently early for rice production during the season they used vouchers.

Table 3: Effects on yields and profits

	Yields (kg/ha)	Profit incl. voucher value	Profit excl. voucher value
2014			
	(1)	(2)	(3)
Treatment	17.834	43.175	238.575***
	(79.064)	(47.933)	(55.101)
p-value conv.	0.823	0.373	0.000
p-value RI	.824	.432	.002
Control mean	966.0	113.6	114.7
2015 - First season			
	(1)	(2)	
Treatment	-295.210***	-19.529	
	(100.016)	(41.259)	
p-value conv.	0.005	0.639	
p-value RI	.0475	.6955	
Control mean	810.4	19.0	
Baseline outcome	no	no	
Observations	515	515	

*Notes: Estimates of effects of early PTTA program treatment (following equation (1)), in 2014 and first agricultural season (January-July) of 2015, on (1) rice yields (in kg per hectare), (2) profits (in US dollars) from rice production including vouchers' values in costs, and (3) profits from rice cultivation exluding vouchers' values from costs. Outcome variables are winsorized at the 99th percentile. All regressions include fixed effects for randomization strata and six lagoon areas. Standard errors are clustered at the habitation-level. Standard and randomized inference (with 2000 iterations) p-values are reported for the null of zero treatment effect. ***, ** and * denote statistical significance, using the conventional test, at the 1%, 5% and 10% levels, respectively.*

Table 4: Effects on input use

	Amount of chemical fertilizer (kg/ha)	Spending in chemical fertilizer and pesticides (USD)	Spending in labor (USD)
2014			
	(1)	(2)	(3)
Treatment	2.069	-51.074**	-28.113
	(26.332)	(24.758)	(25.441)
p-value conv.	0.938	0.046	0.276
p-value RI	.953	.1375	.4145
Control mean	214.8	305.3	391.2
2015 - First season			
	(1)	(2)	(3)
Treatment	-56.030**	-29.249**	-31.747*
	(20.278)	(10.175)	(16.870)
p-value conv.	0.009	0.007	0.068
p-value RI	.04	.0345	.1465
Control mean	195.0	97.8	137.6
Baseline outcome	yes	no	yes
Observations	515	515	515

*Notes: Estimates of effects of early PTTA program treatment (following equation (1)), in 2014 and first agricultural season (January-July) of 2015, on (1) amount (quantity) of chemical fertilizer used (in kg per hectare), (2) spending in chemical fertilizer and pesticides (USD), and (3) spending in labor (USD) for rice cultivation. Outcome variables are winsorized at the 99th percentile. All regressions include fixed effects for randomization strata and six lagoon areas. Standard errors are clustered at the habitation-level. Standard and randomized inference (with 2000 iterations) p-values are reported for the null of zero treatment effect. ***, ** and * denote statistical significance, using the conventional test, at the 1%, 5% and 10% levels, respectively.*

Table 5: Information treatment

	Expects vouchers in 2015
November 2014	
Treatment	-0.298***
	(0.065)
Control mean	0.714
Observations	260
February 2015	
Treatment*informed	-0.344***
	(0.065)
Treatment*uninformed	-0.243**
	(0.072)
Control*informed	0.073
	(0.039)
Lagoons and randomization strata FE	Yes
Baseline outcome	no
p-val trt informed=trt uninformed	0.107
Uninformed control mean	0.865
Observations	515

*Notes: Estimates of effects of experimental provision of information on PTTA status in November 2014 on expectations of receiving a voucher in the future, reported in November 2014 and in February 2015. The regression includes fixed effects for randomization strata and six lagoon areas. Standard errors are clustered at the habitation-level. Standard p-values reported for the null of equal effects among informed and uninformed treatment farmers. ***, ** and * denote statistical significance, using the conventional test, at the 1%, 5% and 10% levels, respectively.*

Table 6: Effects on yields and profits, with information treatment

	Yields (kg/ha)	Profit incl. voucher value
2015 - First season		
	(1)	(2)
Treatment*informed	-214.721*	-17.147
	(113.833)	(44.695)
Treatment*uninformed	-289.794**	-80.155*
	(119.719)	(42.189)
Control*informed	86.452	-55.152
	(70.104)	(38.699)
p-val trt informed=trt uninformed	0.485	0.124
Uninformed control mean	810.437	18.987
Baseline outcome	no	no
Observations	515	515

*Notes: Estimates of interacted effects of early PTTA program treatment and experimental provision of information on PTTA status in November 2014 (following equation (2)), on (1) rice yields (in kg per hectare) and (2) profits (in US dollars) from rice production including vouchers' values in costs in first semester of 2015. All regressions include fixed effects for randomization strata and six lagoon areas. Standard errors are clustered at the habitation-level. Standard p-values reported for the null of equal effects among informed and uninformed treatment farmers. ***, ** and * denote statistical significance, using the conventional test, at the 1%, 5% and 10% levels, respectively.*

Table 7: Effects on input use, with information treatment

	Amount of chemical fertilizer (kg/ha)	Spending in chemical fertizer and pesticides (USD)	Spending in labor (USD)
2015 - First season			
	(1)	(2)	(3)
Treatment*informed	-26.897	-15.060	-10.836
	(27.271)	(13.281)	(19.394)
Treatment*uninformed	-40.768	-24.407	-21.113
	(30.103)	(15.040)	(20.181)
Control*informed	43.874	18.930	31.051**
	(28.873)	(11.310)	(12.934)
p-val trt informed=trt uninformed	0.394	0.401	0.443
Uninformed control mean	195.0	97.8	137.6
Baseline outcome	yes	no	yes
Observations	515	515	515

*Notes: Estimates of interacted effects of early PTTA program treatment and experimental provision of information on PTTA status in November 2014 (following equation (2)), on (1) amount (quantity) of chemical fertilizer used (in kg per hectare), (2) spending in chemical fertilizer and pesticides (USD), and (3) spending in labor (USD) for rice cultivation. All regressions include fixed effects for randomization strata and six lagoon areas. Standard errors are clustered at the habitation-level. Standard p-values reported for the null of equal effects among informed and uninformed treatment farmers. ***, ** and * denote statistical significance, using the conventional test, at the 1%, 5% and 10% levels, respectively.*

Table 8: Loans

All loans						
	Asked for a loan	Loan for inputs	Loan for education	Loan for health and food	Loan for business	
2014	0.662	0.476	0.106	0.131	0.189	
2015	0.687	0.436	0.127	0.142	0.204	
	Asked for a loan	Obtained a loan	Loan for inputs	Mean length of loan	Loan for more than 6 months	Median interest rate
Bank loans						
2014	0.305	0.265	0.138	4.76	0.595	5.30
2015	0.407	0.211	0.375	5.06	0.790	4.84
Merchant loans						
2014	0.305	0.295	0.258	3.60	0.314	6.90
2015	0.259	0.251	0.204	3.41	0.410	11.1

Notes: Descriptive statistics of access to finance among rice producers in control group in 2014 and 2015. The first panel is for loans from any source. The second panel is for loans from banks. The third panel is for loans from merchants.

Table 9: Effects on loans

	Bank loan requested for inputs	Amount of bank loan (USD)	Merchant loan requested for inputs	Amount of merchant loan (USD)
2014				
	(1)	(2)	(3)	(4)
Treatment	-0.083***	-19.152	-0.088***	-23.612**
	(0.026)	(32.963)	(0.032)	(9.823)
p-value conv.	0.003	0.565	0.009	0.021
p-value RI	.0055	.5975	.0925	.074
Control mean	0.138	100.852	0.258	48.734
2015 - First season				
	(1)	(2)	(3)	(4)
Treatment	-0.025	-64.325*	-0.080***	-0.384
	(0.029)	(32.893)	(0.023)	(9.105)
p-value conv.	0.399	0.058	0.001	0.967
p-value RI	.5235	.055	.076	.9705
Control mean	0.211	175.540	0.204	36.368
Baseline outcome	no	no	no	no
Observations	515	515	515	515

*Notes: Estimates of effects of early PTTA program treatment (following equation (1)), in 2014 and first agricultural season (January-July) of 2015, on (1) requests of loans for agricultural inputs to banks (indicator variable), (2) amounts of loans taken from banks (in USD), (3) requests of loans for agricultural inputs to merchants (indicator variable), (4) amounts of loans taken from merchants (in USD). Outcome variables (3) and (4) are windsorized at the 99th percentile. All regressions include fixed effects for randomization strata and six lagoon areas. Standard errors are clustered at the habitation-level. Standard and randomized inference (with 2000 iterations) p-values are reported for the null of zero treatment effect. ***, ** and * denote statistical significance, using the conventional test, at the 1%, 5% and 10% levels, respectively..*

Table 10: Effects on loans, with information treatment

	Bank loan requested for inputs	Amount of bank loan (USD)	Merchant loan requested for inputs	Amount of merchant loan (USD)
2015				
	(1)	(2)	(3)	(4)
Treatment*informed	0.031	-31.289	-0.046	2.849
	(0.043)	(42.229)	(0.033)	(11.101)
Treatment*uninformed	-0.055	-80.903**	-0.063*	6.588
	(0.042)	(37.454)	(0.032)	(11.045)
Control*informed	0.028	17.635	0.049*	9.884
	(0.045)	(28.369)	(0.027)	(9.349)
p-val trt informed=trt uninformed	0.004	0.041	0.682	0.745
Uninformed control mean	0.211	175.540	0.204	36.368
Baseline outcome	no	no	no	no
Observations	515	515	515	515

*Notes: Estimates of interacted effects of early PTTA program treatment and experimental provision of information on PTTA status in November 2014 (following equation (2)), on (1) requests of loans for agricultural inputs to banks (indicator variable), (2) amounts of loans taken from banks (in USD), (3) requests of loans for agricultural inputs to merchants (indicator variable), (4) amounts of loans taken from merchants (in USD). All regressions include fixed effects for randomization strata and six lagoon areas. Standard errors are clustered at the habitation-level. Standard p-values reported for the null of equal effects among informed and uninformed treatment farmers. ***, ** and * denote statistical significance, using the conventional test, at the 1%, 5% and 10% levels, respectively.*

Table 11: Effects on Practices

	Quantity of seeds used (kg/ha)	3 plants per hole transplanting	Retonn practiced
2014			
	(1)	(2)	(3)
	b/se	b/se	b/se
Treatment	-32.987***	0.197***	-0.140***
	(10.706)	(0.038)	(0.049)
p-value conv.	0.004	0.000	0.007
p-value RI	.0505	.0115	.017
Control mean	109.375	0.224	0.448
2015 - First season			
	(1)	(2)	(3)
	b/se	b/se	b/se
Treatment	-11.280	0.079	-0.151***
	(6.858)	(0.047)	(0.050)
p-value conv.	0.109	0.103	0.005
p-value RI	.208	.229	.0185
Control mean	100.976	0.215	0.322
Baseline outcome	yes	no	no
Observations	285	283	340

*Notes: Estimates of effects of early PTTA program treatment (following equation (1)), in 2014 and first agricultural season (January-July) of 2015, on (1) quantities of seeds used (in kg per hectare), (2) 3 plants per hole transplanting (indicator), and (3) practice of retonn (multiple harvests from same seed, indicator variable). Outcome variable (1) is windsorized at the 99th percentile. All regressions include fixed effects for randomization strata and six lagoon areas. Standard errors are clustered at the habitation-level. Standard and randomized inference (with 2000 iterations) p-values are reported for the null of zero treatment effect. ***, ** and * denote statistical significance, using the conventional test, at the 1%, 5% and 10% levels, respectively.*

Appendix. Additional Tables

Table A1: Balancing

	(1) Total income	(2) Agr. income	(3) Nb. plots	(4) Cultivated area	(5) Grows rice	(6) Nb. rice plots	(7) Rice area	(8) Harvested	(9) Lost harvest	(10) First month planted
Treatment	46.313	14.863	0.106	0.054	0.067	0.085	0.047	0.019	0.052	0.350
	(142.855)	(46.562)	(0.109)	(0.156)	(0.052)	(0.099)	(0.102)	(0.045)	(0.041)	(0.316)
p-value conv.	0.748	0.751	0.336	0.729	0.206	0.398	0.646	0.678	0.213	0.276
Control mean	911.853	310.234	1.796	1.294	0.651	0.927	0.623	0.407	0.087	5.667
Observations	515	515	515	515	515	515	515	515	515	355

	(1) Oth. crops prod. value	(2) Oth. crops sales	(3) Rice harvested (kg)	(4) Yield (kg/ha)	(5) Rice prod. value	(6) Profits	(7) Sold rice	(8) Sales qty	(9) Sales value
Treatment	19.029	5.785	0.226	97.335	4.161	-26.449	0.009	-14.924	-3.657
	(15.107)	(8.346)	(87.424)	(129.481)	(52.240)	(37.511)	(0.042)	(72.709)	(43.429)
p-value conv.	0.215	0.492	0.998	0.457	0.937	0.485	0.840	0.838	0.933
Control mean	80.972	36.672	492.787	611.021	303.497	11.297	0.393	371.867	229.338
Observations	515	515	515	515	515	515	515	515	515

(1)	(2) Improved seeds	(3) Fertilizer	(4) Pesticides	(5) Urea	(6) Seeds spending	(7) Urea spending	(8) NPK spending	(9) Chem. fert. spending	Total rice plot spending
Treatment	0.072	0.052	0.138*	0.063	-3.396	1.121	4.837	8.322	26.257
	(0.057)	(0.054)	(0.061)	(0.051)	(7.780)	(5.842)	(5.824)	(13.426)	(53.958)
p-value conv.	0.212	0.343	0.029	0.227	0.665	0.849	0.411	0.539	0.629
Control mean	0.131	0.615	0.556	0.575	56.121	45.654	24.321	76.942	286.655
Observations	515	515	515	515	515	515	515	515	515

Table continues on next page

	(1) Irrigated	(2) Water access	(3) Family work days	(4) Unpaid labor days	(5) Paid labor days	(6) Labor spending	(7) Rice labor spending
Treatment	0.027	-0.111	27.106*	-2.136	5.155	29.990	7.901
	(0.047)	(0.071)	(11.746)	(2.396)	(4.846)	(37.806)	(30.187)
p-value conv.	0.567	0.127	0.027	0.378	0.294	0.433	0.795
Control mean	0.647	0.495	104.102	12.447	25.935	204.330	133.161
Observations	515	515	515	515	515	515	515

	(1) Nb children	(2) Nb adults	(3) Female head	(4) Head age	(5) Agr. producer	(6) Agr. wage	(7) Other job	(8) Head primary	(9) Hear secondary	(10) Work group	(11) Agr. assoc.
Treatment	-0.377***	-0.142	-0.004	1.948	-0.014	-0.016	0.004	-0.095	0.050	0.040	0.150***
	(0.094)	(0.129)	(0.013)	(1.085)	(0.037)	(0.035)	(0.028)	(0.048)	(0.037)	(0.057)	(0.036)
p-value conv.	0.000	0.279	0.764	0.081	0.709	0.653	0.896	0.055	0.192	0.494	0.000
Control mean	1.633	4.265	0.055	45.441	0.335	0.095	0.218	0.724	0.258	0.276	0.127
Observations	515	515	515	512	515	515	515	515	515	515	515

	(1) Livestock own. index	(2) HH assets index	(3) Housing index	(4) Farm assets index	(5) Savings	(6) Bank account	(7) Asked bank loan	(8) Loan refused	(9) WDDS food diversity	(10) Months food insecure	(11) Hunger scale meas.
Treatment	0.051	-0.025	-0.025	-0.160	0.127*	0.011	-0.037	-0.029	0.015	0.541*	0.115
	(0.105)	(0.122)	(0.122)	(0.156)	(0.050)	(0.033)	(0.044)	(0.044)	(0.411)	(0.225)	(0.197)
p-value conv.	0.628	0.840	0.840	0.312	0.016	0.743	0.398	0.508	0.971	0.021	0.565
Control mean	-0.086	0.142	0.142	-0.000	0.308	0.264	0.315	0.233	4.112	2.651	3.004
Observations	515	509	509	515	511	511	511	515	389	515	508

*Notes: Balance between treatment and control at baseline. All regressions include fixed effects for randomization strata and six lagoon areas. Standard errors are clustered at the habitation-level. Standard p-values are reported for the null of zero treatment effect. ***, ** and * denote statistical significance, using the conventional test, at the 1%, 5% and 10% levels, respectively.*

Table A2: Effects on conditional yields and profits

	Yields (kg/ha)	Profit incl. voucher value	Profit excl. voucher value
2014			
	(1)	(2)	(3)
Treatment	-77.421	29.639	251.059***
	(61.112)	(58.909)	(57.386)
p-value conv.	0.213	0.618	0.000
p-value RI	.345	.665	.002
Control mean	1191.3	140.1	141.5
Observations	449	449	449
2015 - First season			
	(1)	(2)	
Treatment	-314.026**	-123.762	
	(122.993)	(89.632)	
p-value conv.	0.015	0.176	
p-value RI	.0405	.242	
Control mean	1150.4	3.6	
Baseline outcome	no	no	no
Observations	340	340	

*Notes: Estimates of effects, conditional on rice cultivation, of early PTTA program treatment (following equation (1)), in 2014 and first agricultural season (January-July) of 2015, on (1) rice yields (in kg per hectare), (2) profits (in US dollars) from rice production including vouchers' values in costs, and (3) profits from rice cultivation exluding vouchers' values from costs. Outcome variables are windsorized at the 99th percentile. The sample is restricted to farmers cultivating rice. All regressions include fixed effects for randomization strata and six lagoon areas. Standard errors are clustered at the habitation-level. Standard and randomized inference (with 2000 iterations) p-values are reported for the null of zero treatment effect. ***, ** and * denote statistical significance, using the conventional test, at the 1%, 5% and 10% levels, respectively.*

Table A3: Effects on rice cultivation

	Grew rice	Total area of rice planted (ha)
2014		
	(1)	(2)
Treatment	0.072	-0.209*
	(0.047)	(0.110)
p-value conv.	0.135	0.066
p-value RI	.1335	.169
Control mean	0.811	1.576
2015		
	(1)	(2)
Treatment	-0.079	-0.153
	(0.048)	(0.118)
p-value conv.	0.107	0.206
p-value RI	.2415	.3735
Control mean	0.665	1.037
Baseline outcome	yes	yes
Observations	515	515

*Notes: Estimates of effects of early PTTA program treatment (following equation (1)), in 2014 and first agricultural season (January-July) of 2015, on (1) rice cultivation (indicator), and (2) area planted with rice (in hectare). Outcome variable (2) is windsorized at the 99th percentile. All regressions include fixed effects for randomization strata and six lagoon areas. Standard errors are clustered at the habitation-level. Standard and randomized inference (with 2000 iterations) p-values are reported for the null of zero treatment effect. ***, ** and * denote statistical significance, using the conventional test, at the 1%, 5% and 10% levels, respectively.*

Table A4: Effects on rice harvests

	Any harvest	Lost an harvest	Total value of rice harvested (usd)
2014			
	(1)	(2)	(3)
Treatment	0.058	0.047	-83.056
	(0.049)	(0.035)	(69.126)
p-value conv.	0.241	0.184	0.237
p-value RI	.2755	.3275	.3515
Control mean	0.749	0.135	915.414
2015 - First season			
	(1)	(2)	(3)
Treatment	-0.122***	0.081**	-92.398*
	(0.045)	(0.034)	(52.885)
p-value conv.	0.009	0.021	0.089
p-value RI	.0765	.11	.225
Control mean	0.458	0.218	300.439
Baseline outcome	yes	yes	no
Observations	515	515	515

*Notes: Estimates of effects of early PTTA program treatment (following equation (1)), in 2014 and first agricultural season (January-July) of 2015, on (1) harvest occurence (indicator), (2) harvest losses (indicator), and (3) total value of rice harvested (in USD). Outcome variable (3) is windsorized at the 99th percentile. All regressions include fixed effects for randomization strata and six lagoon areas. Standard errors are clustered at the habitation-level. Standard and randomized inference (with 2000 iterations) p-values are reported for the null of zero treatment effect. ***, ** and * denote statistical significance, using the conventional test, at the 1%, 5% and 10% levels, respectively.*

Table A5: Effects on quantities of fertilizers used

	Urea	NPK	Sulfate
2014			
	(1)	(2)	(3)
Treatment	-19.892	-2.666	-29.452***
	(13.833)	(12.144)	(5.390)
p-value conv.	0.159	0.827	0.000
p-value RI	.3185	.8405	.005
Control mean	147.968	89.408	70.651
2015 - First season			
	(1)	(2)	(3)
Treatment	-23.967***	-7.808	-10.083**
	(6.933)	(5.535)	(4.401)
p-value conv.	0.001	0.166	0.028
p-value RI	.012	.2415	.1025
Control mean	62.728	34.317	24.239
Baseline outcome	yes	yes	no
Observations	515	515	515

*Notes: Estimates of effects of early PTTA program treatment (following equation (1)), in 2014 and first agricultural season (January-July) of 2015, on quantities used of (1) urea, (2) NPK, and (3) sulfate (all in kg). Outcome variables are windsorized at the 99th percentile. All regressions include fixed effects for randomization strata and six lagoon areas. Standard errors are clustered at the habitation-level. Standard and randomized inference (with 2000 iterations) p-values are reported for the null of zero treatment effect. ***, ** and * denote statistical significance, using the conventional test, at the 1%, 5% and 10% levels, respectively.*

Table A6: Effects on sources of inputs

Panel A. Fertilizers

2014	PTTA	Loan	Other		
	(1)	(2)	(3)		
Treatment	0.723***	-0.176***	0.021**		
	(0.032)	(0.042)	(0.010)		
p-value conv.	0.000	0.000	0.046		
p-value RI	0	.008	.0645		
Control mean	0.000	0.305	0.015		
2015	PTTA	Loan	Own purchases	Purchases in DR	Other
	(1)	(2)	(3)	(4)	(5)
Treatment	0.029***	-0.116***	-0.057	-0.085**	-0.004
	(0.008)	(0.031)	(0.070)	(0.034)	(0.022)
p-value conv.	0.001	0.001	0.424	0.017	0.869
p-value RI	.006	.0265	.524	.0905	.909
Control mean	0.004	0.265	0.531	0.120	0.113
Baseline outcome	no	no	no	no	no
Observations	515	515	515	515	515

Panel B. Pesticides

2014	PTTA	Loan	Other		
	(1)	(2)	(3)		
Treatment	0.667***	-0.139***	-0.011		
	(0.026)	(0.044)	(0.020)		
p-value conv.	0.000	0.003	0.598		
p-value RI	0	.015	.5905		
Control mean	0.004	0.240	0.062		
2015	PTTA	Loan	Own purchases	Purchases in DR	Other
	(1)	(2)	(3)	(4)	(5)
Treatment	0.031**	-0.108**	-0.000	-0.092*	0.049*
	(0.012)	(0.043)	(0.061)	(0.047)	(0.029)
p-value conv.	0.014	0.016	0.996	0.056	0.097
p-value RI	.015	.064	.998	.1655	.294
Control mean	0.007	0.240	0.509	0.196	0.091
Baseline outcome	no	no	no	no	no
Observations	515	515	515	515	515

Panel C. Seeds

2014	PTTA	Loan	Other
	(1)	(2)	(3)
Treatment	0.591***	-0.133***	0.004
	(0.041)	(0.024)	(0.014)
p-value conv.	0.000	0.000	0.749
p-value RI	0	.0015	.786
Control mean	0.000	0.196	0.029

2015	PTTA	Loan	Own purchases	Purchases in DR	Other
	(1)	(2)	(3)	(4)	(5)
Rreatment	0.028***	-0.122***	0.053*	-0.045*	-0.091*
	(0.009)	(0.029)	(0.030)	(0.025)	(0.046)
p-value conv.	0.004	0.000	0.090	0.078	0.056
p-value RI	.0045	.0095	.178	.221	.2025
Control mean	0.000	0.211	0.095	0.102	0.473
Baseline outcome	no	no	no	no	no
Observations	515	515	515	515	515

*Notes: Estimates of effects of early PTTA program treatment (following equation (1)), in 2014 and first agricultural season (January-July) of 2015, on procurement of fertilizers (panel A), pesticides (panel B) and seeds (panel C). Own spending and other sources are agregated in 2014. Outcome variables are windsorized at the 99th percentile. All regressions include fixed effects for randomization strata and six lagoon areas. Standard errors are clustered at the habitation-level. Standard and randomized inference (with 2000 iterations) p-values are reported for the null of zero treatment effect. ***, ** and * denote statistical significance, using the conventional test, at the 1%, 5% and 10% levels, respectively.*

Table A7: Effects on food security

	Household hunger score	Severe hunger	Months food insecure
2014			
	(1)	(2)	
Treatment	0.117	0.033	
	(0.121)	(0.023)	
p-value conv.	0.341	0.167	
p-value RI	.3955	.37	
Control mean	2.056	0.074	
2015			
	(1)	(2)	(3)
treatment	-0.289**	-0.026	-0.128
	(0.126)	(0.029)	(0.311)
p-value conv.	0.028	0.371	0.683
p-value RI	.1565	.5085	.750
Control mean	2.026	0.106	2.851
Baseline outcome	yes	yes	yes
Observations	513	513	515

*Notes: Estimates of effects of early PTTA program treatment (following equation (1)), in 2014 and first agricultural season (January-July) of 2015, on (1) Household Hunger Score, (2) severe hunger (value of 4-5 on the Household Hunger Score (3) number of months of food insecurity. All regressions include fixed effects for randomization strata and six lagoon areas. Standard errors are clustered at the habitation-level. Standard and randomized inference (with 2000 iterations) p-values are reported for the null of zero treatment effect. ***, ** and * denote statistical significance, using the conventional test, at the 1%, 5% and 10% levels, respectively.*

Table A8: Effects on asset ownership

	Any savings	Housing assets index	Farming assets index	Livestock index
2014				
	(1)	(2)	(3)	(4)
Treatment	-0.008	0.014	-0.046	-0.004
	(0.033)	(0.076)	(0.081)	(0.064)
p-value conv.	0.797	0.850	0.574	0.954
p-value RI	.8455	.883	.630	.9525
Control mean	0.105	0.050	-0.050	-0.108
2015				
	(1)	(2)	(3)	(4)
Treatment	-0.063*	0.013	-0.005	-0.085
	(0.034)	(0.086)	(0.080)	(0.091)
p-value conv.	0.071	0.882	0.946	0.355
p-value RI	.127	.9225	.9525	.4615
Control mean	0.201	0.058	-0.063	-0.060
Baseline outcome	no	yes	yes	yes
Observations	514	514	512	515

*Notes: Estimates of effects of early PTTA program treatment (following equation (1)), in 2014 and first agricultural season (January-July) of 2015, on (1) savings (indicator), (2) housing assets index, (3) farming assets index and (4) livestock index. Indices are defined as first components of a principal components analysis of a set of indicators for owned assets. All regressions include fixed effects for randomization strata and six lagoon areas. Standard errors are clustered at the habitation-level. Standard and randomized inference (with 2000 iterations) p-values are reported for the null of zero treatment effect. ***, ** and * denote statistical significance, using the conventional test, at the 1%, 5% and 10% levels, respectively.*

Table A9: Effects on land ownership

	Number of plots	Any lagoon plot	Any irrigated plot	Spending in irrigation
2014				
	(1)	(2)	(3)	(4)
Treatment	-0.096	-0.051	0.014	-2.366
	(0.106)	(0.036)	(0.052)	(3.181)
p-value conv.	0.371	0.163	0.782	0.462
p-value RI	.492	.036	.781	.4645
Control mean	3.545	0.978	0.840	7.414
2015				
	(1)	(2)	(3)	(4)
Treatment	-0.309	-0.049*	-0.041	-4.896
	(0.198)	(0.027)	(0.051)	(2.913)
p-value conv.	0.127	0.082	0.425	0.101
p-value RI	.153	.042	.5255	.252
Control mean	4.364	0.974	0.693	11.313
Baseline outcome	yes	no	yes	yes
Observations	515	514	514	514

*Notes: Estimates of effects of early PTTA program treatment (following equation (1)), in 2014 and first agricultural season (January-July) of 2015, on (1) number of plots owned, (2) ownership of a lagoon plot (indicator), (3) ownership of an irrigated plot, and (4) spending in irrigation. Outcome variable (4) is windsorized at the 99th percentile. All regressions include fixed effects for randomization strata and six lagoon areas. Standard errors are clustered at the habitation-level. Standard and randomized inference (with 2000 iterations) p-values are reported for the null of zero treatment effect. ***, ** and * denote statistical significance, using the conventional test, at the 1%, 5% and 10% levels, respectively.*

Table A10: Effects on other economic activities

	Other crops cultivated area	Other crops production value	Other crops sales value	Adults working in DR	Hours worked in DR
2014					
	(1)	(2)	(3)		
Treatment	-0.056	-55.652**	-47.113**		
	(0.101)	(24.889)	(20.394)		
p-value conv.	0.581	0.031	0.026		
p-value RI	.718	.0515	.041		
Control mean	1.856	197.976	114.931		
2015 - First season					
	(1)	(2)	(3)	(4)	(5)
Treatment	-0.000	-20.756	-14.567	-0.065	0.018
	(0.145)	(15.541)	(11.694)	(0.121)	(0.049)
p-value conv.	0.999	0.190	0.221	0.595	0.714
p-value RI	.998	.247	.2845	.7085	.808
Control mean	1.454	70.842	49.171	0.542	0.295
Baseline outcome	yes	yes	yes	no	no
Observations	515	515	515	515	515

*Notes: Estimates of effects of early PTTA program treatment (following equation (1)), in 2014 and first agricultural season (January-July) of 2015, on (1) areas cultivated with other crops (in hectares), (2) the value of other crops production USD), (3) the value of other crops sales (USD), (4) number of adults working in the Dominican Republic, and (5) total number of hours worked by family members in the Dominican Republic. Outcome variables are windsorized at the 99th percentile. All regressions include fixed effects for randomization strata and six lagoon areas. Standard errors are clustered at the habitation-level. Standard and randomized inference (with 2000 iterations) p-values are reported for the null of zero treatment effect. ***, ** and * denote statistical significance, using the conventional test, at the 1%, 5% and 10% levels, respectively.*