DOI: 10.1111/ajae.12337

ARTICLE

Input subsidies, credit constraints, and expectations of future transfers: Evidence from Haiti

Jérémie Gignoux[1] | Karen Macours[1] | Daniel Stein[2] | Kelsey Wright[1]

[1]Paris School of Economics (PSE) and researchers at the French National Research Institute for Agriculture, Food and Environment (INRAE), Paris, France

[2]IDinsight, San Francisco, CA, USA

Correspondence
Jérémie Gignoux, Paris School of Economics, 48 Boulevard Jourdan, 75014 Paris, France
Email: jeremie.gignoux@psemail.eu

Funding information
Agence Nationale de la Recherche, Grant/Award Number: ANR-17-EURE-001; IADB; GAFSP

Abstract
We examine the effects of a subsidy program in Haiti that provided smallholders subsidies for inputs (rice seeds, fertilizer, pesticides, and specific labor tasks) using 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 clarification regarding their future receipt of vouchers, we find evidence suggesting that incorrect expectations of future transfers partially explain the disappointing outcomes. In addition, instead of increasing input use, the subsidies seem to have led farmers to pay off their loans and take fewer new ones. In complex post-emergency environments such 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
agricultural development, impacts, input subsidies, smallholders, yields

JEL CLASSIFICATION
O13, Q12, Q18

1 | INTRODUCTION

Although there have been remarkable reductions in poverty and hunger in recent decades, increasing agricultural yields of staple crops continues to be a policy priority of governments in many low-income countries. Despite economists' long-standing emphasis on profits rather than yield (Foster & Rosenzweig, 2010; Marenya & Barrett, 2009), many agricultural programs continue to target yield gains (Macours, 2019). Food security concerns, which have gained new urgency in light of the

COVID-19 pandemic (FAO et al., 2020), are sometimes specifically noted as a motivation for this focus on yield.

So-called "smart" input subsidy programs are often considered a useful policy option for increasing agricultural yield by encouraging the adoption of modern inputs (Jayne et al., 2018; World Bank, 2007). "Smart" subsidy programs distinguish themselves from traditional agricultural input subsidy programs by targeting specific (usually poor) groups of farmers with short term subsidies; supporting local input markets, for instance by providing vouchers redeemable at private suppliers; and often providing a package of complementary subsidies.

This article contributes new experimental evidence on the impact of an agricultural "smart" subsidy program in Haiti, called PTTA. The program provided smallholder rice farmers with a one-time package of vouchers that allowed them to purchase subsidized inputs (seeds, fertilizer, pesticides, and labor tasks) and was implemented by the Ministry of Agriculture, Rural Resources and Rural Development (MARNDR) between 2014 and 2017. It was designed in the wake of the destructive 2010 earthquake near Port-au-Prince, a period in which there were massive aid flows into the country, weak institutional capacity, and little data to inform the design of public programs such as the one we study.

In coordination with MARNDR, a cluster-randomized evaluation was built into the early phases of the program, with 245 farmers from 16 localities allocated randomly to a treatment group and 270 farmers from 23 localities allocated to a control group. The treatment group began receiving vouchers in January 2014. The control group would eventually receive vouchers in September 2015, after the completion of the impact evaluation.

We estimate the effects of the program on intermediary and final outcomes along the hypothesized causal pathway of the program, from input use, yields, and profits to food security. We estimate impacts on outcomes in the agricultural seasons 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 relative to control farmers and in fact produced less rice in the year they received the vouchers. Instead of increasing input use, the subsidies crowded out own-pocket expenditures and led to lower total quantities of input use. Moreover, treatment farmers had lower rice yields and production values one to two seasons after receiving the subsidies. These results were not transparent for the appropriateness of smart subsidy programs for certain contexts.

Our results contrast with experimental evidence on inputs as such. Results in Kenya (Duflo et al., 2011) show fertilizer provision increased yield gains in line with agronomist estimates. In India (Emerick et al., 2016) and Mali (Beaman et al., 2013), subsidized access to one input led to a re-optimization and increased use of other inputs. Unlike these programs, our study is embedded within a national subsidy program.

The only other experimental study of government smart subsidy programs as such is Carter et al. (2021, 2013), who show that a one-time fertilizer and seed subsidy in Mozambique was effective in promoting input use, leading to increased yield, learning, and consumption. A 3ie systematic review of (generally non-experimentals) studies, concludes that input subsidies are associated with increased use of inputs, higher yields and increased income among farming households (Hemming et al., 2018). Jayne et al. (2018)'s review of studies with observational data on sub-Saharan African smart subsidy programs in particular shares the assessment that these programs increase yields but emphasizes that the actual yield gains have been lower than expected. They argue that yield gains from smart subsidies have been attenuated by crowding out of unsubsidized inputs and lower crop yield response to fertilizer in the real world. Indeed, actual returns to new technologies are often substantially lower than estimated by agronomists (Laajaj et al., 2020). The results from this article are arguably a somewhat extreme example of the crowding out Jayne et al.'s 2018 assessment highlighted. In Haiti, the experimental input subsidy program resulted in yields for subsidized rice farmers that were actually less than those of unsubsidized farmers, even in the first year, with subsidized fertilizer crowding out unsubsidized fertilizer by more than one to one.

The 3ie review points to the importance of program implementation, design, and contextual factors for effectiveness and for the resulting variation in benefit–cost ratios across programs, as also highlighted by Chirwa and Dorward (2013); Jayne and Rashid (2013); Mason et al. (2013) or Ricker-Gilbert and Jayne (2017). Like any other program, the design of smart subsidy programs is based on a number of assumptions, and not meeting any one of them can contribute to not achieving the fully expected gains. Crucially, the inputs being promoted and subsidized must actually provide positive (risk-adjusted) profits to farmers by generating strong yield response, even in years with poor rainfall. Additionally, farmers must face constraints to input adoption that can be overcome by a subsidy that only lasts for a short period. For instance, farmers could be credit constrained and/or unable to afford the costs of experimenting themselves, but these constraints may be lifted after an initial season of high profits. Alternatively, farmers could lack information about the returns to the inputs but can learn about their value in a single season of experimentation. Such assumptions on profitability and constraints that are surmountable in the very short term may not hold in all settings. Furthermore, the introduction of 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 and investments.

To disentangle the potential mechanisms to explain the unintended effects of PTTA, the paper sheds light on the validity of these different assumptions for the study context. We also emphasize two additional previously unexplored issues for smart subsidies: the role of farmer expectations over future transfers and the role of existing loans and debt.

First, our results suggest that erroneous expectations about possible future transfers can help explain part of the 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, and differences in profits between informed and uninformed treatment farmers are just above the 10% significance threshold.

The role of existing loans further helps explain the findings. The lack of effects on input use in the year farmers received subsidies (2014) suggests that credit and information constraints were not 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 (forward sales to local rice traders), often with interlinked transactions involving rights to the harvest. Because such credit is costly, the vouchers allowed indebted farmers to pay off their loans and not engage in new borrowing, hence potentially switching to a new lower intensity equilibrium. Given 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 in reality—even if that was understandable given the extremely limited information available in the country post-earthquake. In addition to the role of expectations and loans, a number of developments in the design and the implementation of the program further help explain the initially unanticipated results. As a market "smart" subsidy program, PTTA was meant to stimulate the local input market with vouchers redeemable at existing input suppliers, for the same types of inputs available on the market. Nevertheless, the program suffered from delays at various points. Additionally, some of the vouchers for services were not redeemable due to lack of suppliers. Evidence from sub-Saharan Africa suggests that subsidy programs may work best for farmers who are not already using the inputs (Mason et al., 2013; Ricker-Gilbert & Jayne, 2017), whereas rice farmers in Haiti often already use pesticides and fertilizer in their irrigated lagoons, and live close to suppliers and roads. Additionally, unlike some other smart subsidy programs, instead of vouchers offering a percentage or fixed discount on commercial inputs, the PTTA vouchers were supposed to fully subsidize a specific quantity of inputs for a specific price, and a shock to fertilizer prices caused some suppliers to not be able to supply the full amount. Hence, some steps in program implementation were delayed, and others were incomplete. There were sudden changes in prices, low rainfall, and unclear communication with the beneficiaries regarding delays and future subsidies.

Our results thus provide a stark reminder that any one experimental result on the impact of a particular intervention, no matter how internally valid, is context and time specific, and can be conditional on the specific set of constraints and opportunities within which individuals targeted by an intervention make their decisions (Rosenzweig & Udry, 2020). The results also 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 results. The results highlight those incorrect assumptions and imperfect implementation not only reduce effectiveness but can actually lead to perverse results.

As such, in addition to providing new experimental evidence on smart subsidies, this time in a setting outside sub-Saharan Africa, this paper contributes to the broader literature on the challenges in design of food security programs (Barrett, 2002; del Ninno et al., 2007), in particular as it relates to post-emergency settings (Maxwell et al., 2012; Pingali et al., 2005). It also relates to the experimental literature on constraints to adoption of agricultural technologies and practices (see Magruder [2018] for a recent review of experimental studies on credit, information, and risk). Credit and information do not seem to be binding constraints to increased input adoption in this case; but even in this irrigated area of Haiti, weather risk may be a constraint to adoption, as in Karlan et al. (2014)'s study in Ghana. The paper is an experimental example showing the importance of focusing on economic returns, rather than yield gains, to understand adoption decisions and learning (as discussed by Michler et al., 2019).

The next section presents the context and the subsidy program design in more details. The third section presents a simple conceptual framework and the theory of change informing the analysis. The fourth discusses the experimental design, data, and specification. The fifth presents the main results on rice yield, profits, and input use. The sixth section 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. The seventh discusses impacts on welfare outcomes and nonagricultural investments. The eighth section discusses the article's limitations, and the last section concludes.

2 | CONTEXT AND INTERVENTION

2.1 | Context

The study area covered the subcommunes of Haut-Maribahoux (in the commune of Ouanaminthe) and Bas-Maribahoux (in the commune of Ferrier), both of which are located on the border with the Dominican Republic, as shown on the map of the Appendix S1, Figure A1. About 130 km from Port-au-Prince, the area was spared the physical destruction of the 2010 earthquake and has been less frequently touched by hurricanes than the southern part of the island.

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. Because the total evaluation area is less than 140 km^2 a plot's water access, not differences in rainfall, are most relevant for households. Water access will depend on irrigation quality and proximity to tributaries, and therefore on the lagoon in which the rice plot is located, in particular during drought years. Although the crop calendar is fairly fluid throughout the year, there are two main rice growing seasons, starting, respectively, in December/January (winter season) and in August/September (summer season).

Table 1 shows 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 for the voucher

TABLE 1 Farmers' characteristics

	Mean	*SD*
Head can read or write	0.287	0.453
Head has nonagricultural occupation	0.196	0.398
Quality of water access	0.522	0.500
Grows rice	0.717	0.451
Nb of plots cultivated with rice	1.068	0.950
Area of plots with rice (cond.)	1.039	1.886
Rice plot(s) irrigated (cond.)	0.913	0.282
Used urea on rice plots	0.633	0.482
Used NPK on rice plots	0.416	0.493
Used both NPK and urea on rice plots	0.398	0.490
If household used pesticide on rice plots	0.565	0.496
Used urea, NPK, and pesticides on rice plots	0.361	0.481
Total spending in seeds	77.482	139.3
Total spending in fertilizer	96.087	148.8
Total spending in pesticide	17.513	29.7
Rice production value	335.107	667.8
Sold rice to intermediary and/or on field	0.318	0.466
Asked for a bank loan in previous year	0.309	0.463
Total household income (annual)	785.9	898.9
Agricultural income (from sales)	309.0	478.3
Income from livestock and sales of charcoal and wood	149.9	316.9
Nonfarm income	327.1	625.7
Severe hunger	0.337	0.473
Months food insecure	3.031	2.283
Observations	515	

Note: 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.

program, as explained below). On average, farmers cultivated 1.07 rice plots and 1.04 ha of rice, with 72% cultivating rice in 2013. The mean value of their rice production in 2013 was $335 US. The rice farmers used moderately intensive cultivation practices, with irrigation and/or improved inputs. 92% of farmers used irrigation on their rice plots. 63% (88% of rice growers) used urea, 42% used NPK (58%), and 40% (56%) used both. 56% (79% of rice growers) used pesticides, and 36% used (50%) urea, NPK, and pesticides. Indeed, a majority of farmers were already using subsidized inputs before the intervention. In 2013, yearly spending on rice seeds, fertilizers, and pesticides amounted to $77 (i.e., 23% of the value of their rice production), $96 (28%), and $17 (5%).[1] The quantities of inputs used at baseline are less than the ones recommended by MARNDR (we provide more details in sections on main results and mechanisms).[2]

[1]Many farmers grew varieties of rice that could be harvested two or sometimes three times after planting (a practice called "retonn"). MARNDR did not recommend this practice because it generally generates lower yields than replanting.
[2]In an analysis conducted by the Inter-American Development Bank as part of the project's preparation, Bayard (2011) suggests that farmers in this area could obtain yields of above 5 tons per hectare (roughly five times the level at baseline) if they used the recommended quantities of inputs.

During the study period, a substantial share of farmers paid for their input purchases using loans. Loans were often provided by intermediary traders, who would prepurchase the harvest from the farmers, allowing them to pay off their loans in kind with their rice harvests. Among control farmers at the first follow up (2014), such merchant loans financed about 30% of purchases of fertilizer, 25% of pesticides, and 20% of seeds (This information was not collected at baseline). At baseline, 32% of farmers, that is, 44% of rice growers, reported selling rice directly from their plot and/or to an intermediary who directly harvested the field. 31% also report having requested a loan at a bank.

Water control in the area was imperfect, and rice yields as well as the number of rice cycles cultivated per year vary significantly across plots, seasons, and years. During 2014–2017, Haiti faced its worst drought conditions since 1980 (Monteleone et al., 2020). In both 2014 and the first season of 2015 (all seasons for which we gathered follow-up data), all farmers in the study faced drought conditions. During drought periods, in-kind loan repayments can represent a large share of the harvest.

Although irrigated land in the area was generally planted with rice, often for sale, many farmers also planted plots of dry land with food crops, such as corn, beans, manioc, yam, sweet potato, and peanuts. These crops require fewer inputs than rice, but rice cultivation constituted by far the main source of agricultural income. At baseline, mean total household agricultural income was estimated at \$309 US (with a high standard deviation and after winsorizing at 98th percentile). It was also common for farmers to use their land for livestock and/or charcoal and wood plank production. Income diversification outside of livestock was limited however, with only 20% of household heads having a nonagricultural occupation. At baseline, mean incomes from livestock and sales of charcoal and wood and from nonfarm activities were estimated at respectively \$150 US and \$327 US. Educational levels were low; only 29% of household heads were literate. Levels of food insecurity were high, with 34% reporting severe hunger[3] and an average of 3.0 months of food insecurity per year.

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 both the Global Agriculture and Food Security Program (GAFSP) and the Inter-American Development Bank (IADB), supervised by the IADB, and implemented locally by private operators. We study a part of the program that provided farmers with vouchers that they could exchange for agricultural inputs. This voucher program eventually 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, which covered rice farmers in the Northeast department.[4]

The program was designed and implemented as follows:

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

[3]The measure of household hunger 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.

[4]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 vegetable farmers in the Saint-Raphael area (North department) but suffered from low compliance with the experimental design. This paper therefore focuses on the Northeast experiment (with rice) only.

previous experience with rice cultivation. About 10% of registered farmers were deemed ineligible.[5] After being explained the vouchers they would receive, farmers signed a "contract" detailing the vouchers to which they were entitled.

3. The program distributed printed vouchers, issued by a participating bank, for free inputs to eligible farmers. The vouchers were meant to correspond to the recommended number of inputs for 0.5 hectares of rice. Farmers with less than 0.5 hectares of cultivable land available received vouchers for 0.25 hectares. Beneficiary farmers received the full package of all vouchers: seeds, fertilizer, pesticides, labor (either for plowing or transplanting), and pesticide application.
4. The vouchers for seeds, fertilizer, and pesticides could be redeemed at local input dealers who had agreed to participate, located within generally no more than an hour's walking distance from farmers' homes. Many suppliers agreed to participate, but farmers typically redeemed at one of three main input suppliers that were already well known in the area. Because the sources were the same, the quality of program inputs was likely the same as farmers normally purchased for themselves. The program team worked with these local dealers to encourage 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 for either plowing or for transplanting, and one for pesticide application) could be used to pay local providers of these services through a similar mechanism.

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	12 kg	200 kg	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%

Note: 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 are 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.

[5]Farmers were generally deemed ineligible due to not having any rice growing experience (and were more likely to sell the inputs)—not as they did not have enough land. Although plot sizes are small in Haiti, the 2001 LSMS survey shows that less than 2% of households who cultivate in the Northeast department do so on total land of less than 0.25 hectares, and less than 7% of plots were less than 0.25 ha.

Table 2 shows the 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 should have received, based on market prices when the voucher scheme was designed. Farmers received vouchers for land preparation, seeds, chemical fertilizer, and pesticides. Farmers with at least 0.5 ha in one rice plot were entitled to a package of vouchers with a value of $440; fertilizer and plowing service represented the highest value (each about a quarter of the full value of the package). Both administrative records and farmer self-reports confirmed that the large majority of the farmers who signed the contract received and used the vouchers, but in practice farmers received less than initially planned. The next two lines in Table 2 show administrative information reported by the bank on both the share of vouchers that was printed (and likely distributed) and the share that was paid to input suppliers (and therefore redeemed). The next three lines reflect what farmers reported in the survey regarding distribution and use of vouchers. The two sources are largely in agreement, with the exception of the voucher for pesticide application. 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 low receipt and usage of the pesticide application voucher (about 32%), likely due to bottlenecks in the supply as it required a service provider with a backpack sprayer and knowledge of the specific plot. Indeed, there is a 49% point difference between the bank records of payment for pesticides services, and farmers reported receipt of the services, but these vouchers represented less than 10% of the total voucher value. Accounting for this incomplete distribution, the mean total value of vouchers farmers report having received was $295, and mean total value of reported redeemed vouchers was $279. The latter represents 31% of spending on rice production in 2014 ($887) and 35% of total household income at baseline ($785, winsorized at 98th percentile). This transfer is comparable in size to some Latin American cash transfer programs.[6]

Then, the great majority of farmers reported using the inputs they received with the vouchers on their own farm. In particular, among those (86%) reporting receiving the fertilizer vouchers, 91% report using it all on their farm, 3% report not redeeming the voucher, 3% report keeping some fertilizer for later, 1% report loaning some out, and less than 1% report selling any. Although it is difficult to exclude that farmers purposely underreported reselling of inputs, such reselling is not illegal in this context so there is little incentive to do so. The available data hence suggests that reselling of inputs was limited, possibly because eligibility was determined based on an initial expression of interest, which may have screened out people without use for the inputs.

The timing of voucher distribution was affected by delays in bank printing operations and weather conditions. In early 2014, some farmers had to wait for a few weeks after the typical winter season planting period to receive their vouchers. Moreover, after voucher distribution began in 2014, it became apparent that farmers were facing severe drought conditions, resulting in many planning not to grow rice during the first season of 2014. The project therefore paused voucher distribution and allowed farmers to use the already distributed vouchers during the next season if they needed (summer 2014). The combination of the initial implementation-driven delays and the weather induced-delays meant only about 70% of farmers (or about 80% of those who received vouchers) reported they could use the vouchers for chemical inputs and plowing services when they would have preferred to do so. Because of these delays in voucher distribution and usage, we look at the treatment effect on all rice production in 2014.

In addition, although the value of the vouchers originally corresponded to the technically advised quantities of fertilizer, prices fluctuated. Specifically, the price of fertilizer rose sharply in the Dominican Republic (where most supplier sourced their fertilizer) between when the voucher scheme was designed and vouchers could be 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. Although the recommended quantity of chemical fertilizer was 200 kg per half-hectare (400 kg/ha), given price changes, farmers ended up

[6]See Fiszbein et al. (2009).

only being able to pay for 135 kg of chemical fertilizer (NPK and urea) with their voucher. Additionally, due to a supply-side shortage, sulfate (one of the recommended inputs) was not available.

During the period of the evaluation, PTTA was the main agricultural program in the region of study. USAID did start another agricultural development program ("AVANSE") in different localities within the same department, and we collected data on farmers' potential participation as part of AVANSE also offered vouchers. Some farmers were aware of both programs, but only one reported also receiving vouchers from AVANSE. Such an environment with different simultaneous interventions is not uncommon.

3 | CONCEPTUAL FRAMEWORK AND THEORY OF CHANGE

The theory of change underlying "smart" subsidy programs like PTTA is that a one-time subsidy leads to adoption of a package of inputs and practices that together increase yield and profits. These profits, apart from increasing welfare, can get partly reinvested in a similar technology package in the next season, to derive dynamic gains. The experimental design and analysis in this article follows this theory of change as we show impacts along the hypothesized causal pathway from inputs to yield and profits in the year of the intervention and similar outcomes a year later.

This theory of change, and the design of the intervention itself, was motivated by models of "poverty traps" (Balboni et al., 2021; Dasgupta & Ray, 1986; Kraay & McKenzie, 2014), which predict that a large-enough, one-time subsidy can lead to sustainable gains. Consider an agricultural poverty trap model (Carter & Barrett, 2006), in which a household cultivates land over multiple periods. The household has the ability to invest in a profitable, productivity-enhancing technology (the technology package promoted by PTTA) but faces credit constraints that prevent it from profitably accessing it. Once the household does adopt the technology, it can re-invest its profits the next period, putting it on a virtuous path of income growth. However, if it does not have enough wealth to invest in the initial period, it remains in the poverty trap. A single season of subsidized access to the productive technology can potentially lift the household out of the trap and move them into a higher income equilibrium.

This simple framework encompasses a number of key assumptions that need to hold for smart subsidies to be effective. Although Carter and Barrett (2006) lay out the conditions for an asset-based poverty trap to exist, Duflo and Banerjee (2011) warn that models of poverty traps fail to explain low adoption of agricultural technologies that can be purchased and used productively in small quantities (such as seeds or fertilizer), so farmers could slowly increase their investments over time. That said, if there are important complementarities between the use of the different components of the technology package, and some of them come with nontrivial fixed costs (such as hiring in services for land preparation and pesticide spraying in PTTA), the full package can constitute a lumpy investment that needs to be overcome.

Other assumptions or conditions are needed for this conceptual framework to be applicable for the design of a real-world intervention. First households need to understand the structure of the subsidies, specifically their one-time nature. If households erroneously believe that subsidies are forthcoming in future periods, they may underinvest in farming in preparation for these future periods.

Second, a model of poverty traps assumes that the household is credit constrained. If farmers actually have access to sufficient credit, and therefore adopting the technology is already in their option set, providing a one-time subsidy is unlikely to have a lasting effect. Moreover, in the specific case that access to credit is linked to interlinked loans for rice inputs and forward sales of rice harvests to small intermediary rice traders, as became clear over the course of the study is the case in Haiti, a subsidy for inputs can have other effects by breaking the interlinkage. The subsidized inputs can allow farmers to focus on maximizing their welfare across domains (rather than maximizing rice yields), which may well include shifting away from rice, particularly in risky seasons.

A third assumption is that the technology is indeed profitable and risk free. If the technology does not consistently lead to the desired returns, then a realization of a bad outcome may put the household back in the poverty trap. For instance, this may happen if there are weather shocks.

Fourth, the design implicitly assumes that households can learn how to use the technology (specifically the correct amounts of inputs in the promoted package) during implementation in the initial adoption year (or that they already know it). However, this may not be the case, and households may learn important aspects about the technology more gradually, and such learning can be imperfect (Bandiera & Rasul, 2006; Bardhan & Udry, 1999).

Violations of any of these assumptions could all be reasons for why a smart subsidy program fails to have short or longer -term impact. We hence explore each of the aforementioned assumptions in the course of our analysis, which will help interpret our primary results.

4 | EMPIRICAL STRATEGY, DATA AND ECONOMETRIC SPECIFICATION

4.1 | Empirical strategy

To evaluate the impacts of the voucher scheme, 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" (similar to rural villages). The program planned to operate in 39 habitations, which comprised all the rice-growing areas in the two study sub-communes. Of these 39 habitations, 16 were randomly chosen to receive the vouchers in 2014, whereas the rest only received vouchers after August 2015. We can therefore estimate the effects of the intervention until mid-2015. Randomization was done within 14 strata defined by the two subcommunes, 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 study area has limited spatial extent covering less than 140 square kms, and there are no significant altitude variations (see map of Appendix S1, Figure A1). Rainfall was similar across the treatment and control habitations, and both were equally affected by drought.

The sample was drawn from farmers who registered and were eligible for the program. In small habitations, we surveyed all eligible households. In habitations with 31 or more eligible households, we drew a random sample of 30 households. Five hundred and twenty-one farmer households were surveyed at baseline. Due to the attrition of six farmers, which is not significantly associated with the treatment, 515 farmers (240 farmers in 16 treatment habitations and 275 in 23 control habitations) were followed through August 2015 and make up 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% of treatment farmers report receiving, and 85% report using, at least one voucher. Contamination of the control group was low, with only three control farmers reporting receiving vouchers in 2014. Administrative project records of voucher distribution and use show similarly high level of compliance.

In November 2014, a small complementary information intervention aimed at clarifying farmers' status in the program was delivered to a random subset of farmers within each habitation. 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 clarifying the farmers' status in the program. It 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

[7]We initially planned for a staggered roll out with a third group, but delays due to printing and pausing of vouchers due to drought made the design impossible to implement; this explains the imbalance between the treatment and control groups.

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 were selected to be informed of their status in the program, whereas the others received no additional information.

4.2 | Data

Data were gathered from various sources between 2013 and 2015. When farmers registered for the program in late 2013, they provided basic information including estimates of their cultivated land area, previous experience with rice cultivation, and quality of access to irrigation. We then conducted three household surveys: a baseline in October–November 2013 (before the random selection of treatment habitations), a first followup in February 2015, and a final followup in August 2015. These surveys gathered detailed information on agricultural production, inputs and practices, experiences in the program, and 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. A total of 120 treatment and 140 control farmers answered this short survey.

The first follow-up survey (in February 2015) was conducted to measure the impacts of the program in 2014, the year farmers received the subsidy. The second follow-up survey (in August 2015) allows to measure impacts in the first semester of 2015, that is, one-to-two seasons after the end of subsidies for the treatment farmers when neither the treatment nor the control group received vouchers.

In Appendix S1, Table A1 reports balance checks using the specification described below 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 in 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. Distance to towns and roads are also balanced, so that physical access to inputs was similar for treatment and control farmers.

4.3 | Econometric specification

We estimate the following ANCOVA specification to test the intent-to-treat impact of the main intervention:

$$y_{ihst} = \alpha_1 + \beta_1 T_{hs} + \lambda_1 y^0_{ihs} + \mu_{1s} + \nu_{1i} + \epsilon_{1ihst} \tag{1}$$

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_{1s} are fixed effects for the 14 randomization strata.[9]

[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).

[9]As baseline yields and profits are not available by season, they cannot be controlled for in the 2015 estimates. That said, adding controls for baseline yield and profit for the full baseline year does not affect the results.

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_{1i} fixed effects for the six lagoon areas in which a household's plots were located at baseline.[10] 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 (the unit of randomization). Continuous outcome variables are winsorized at 99th percentile. Given that randomization of the main treatment was at the habitation level, and that there is a relatively small number of habitations (39), we report p-values based on randomization inference 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.[11]

Using the specification in Equation (1), we estimate the effects of the program on outcomes in 2014, the year farmers received the vouchers, as measured in the first follow-up survey in February 2015. We also estimate the effects in the first season of 2015, as measured in the second follow-up survey in August 2015, capturing post-program effects. 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. Because 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.[12] In Appendix S1, 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 I_{ihs}, representing the information treatment which told farmers their status in the program next season. We estimate the following specification:

$$y_{ihst} = \alpha_2 + \beta_2 T_{hs} + \gamma_2 I_{ihs} + \delta_2 T_{hs}{}^{*} I_{ihs} + \lambda_2 y^0_{ihs} + \mu_{2s} + \nu_{2i} + \epsilon_{2ihst} \tag{2}$$

where μ_{2s} and ν_{2s} remain fixed effects for randomization strata and lagoon areas, and I_{ihs} denotes the information treatment (randomized at the individual level). We use this specification primarily to test if the information treatment modifies the impacts of the voucher treatment in 2015. The main parameter of interest, δ_2, estimates this effect of information provision among treatment farmers, comparing treatment farmers who were informed they would not participate in 2015 and those who were not. When presenting the corresponding results, in order to assess the magnitude of the effects of information provision, we take uninformed control farmers as a reference, and report β_2, the estimate of the treatment (intent-to-treat) effect for the uninformed, and $\beta_2 + \delta_2$, the estimate of the effect (intent-to-treat) of the two treatments combined. To test if some control farmers incorporated the announcement of future subsidies into their decisions, we also report the parameter γ_2 which estimates the effect of the information treatment among control farmers, comparing control farmers who are informed they will participate to the program the next year and those who are not.

[10]In Appendix S1, Tables A19–A24, we show alternative estimates with more baseline controls selected through the LASSO post-double-selection method of Belloni et al. (2014). All results are robust, as expected given randomization.

[11]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 resampling 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.

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

5 | MAIN RESULTS

5.1 | Yields and profits

The PTTA program's explicit objective was to increase rice yields. Table 3 shows the estimates of the main intervention ITT effects on rice yields and rice profits. Rice yields 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 was used in two seasons (or if the household harvests twice from the same seeds). Yield is set to zero 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 with 966 kg/ha in the control), with large standard errors. In Appendix S1, Table A2 reports the corresponding estimates with yields and profits defined conditionally on rice cultivation but zero values if farmers did not harvest for some reasons; 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.[13] The mean profit from rice for control group farmers was low, at $114, in 2014. These profits should be compared with our estimates of total household agricultural income of $309 at baseline; the cash incomes of these farmers are low and particularly so in 2014 due to the bad weather conditions. When counting the input expenditures paid for with vouchers as costs, 2014

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	0.824	0.432	0.002
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	0.0475	0.6955	
Control mean	810.4	19.0	
Baseline outcome	no	no	no
Observations	515	515	515

Note: Estimates of intention-to-treat (ITT) effects of 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 excluding 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. *** denotes statistical significance, using the conventional test, at the 1% level.

[13]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.

profits of treatment farmers are not statistically different from 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).[14]

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/ha, which is a 36% reduction compared with 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. In Appendix S1, Table A3 shows estimates of the ITT effects on rice planting. Respectively, 88% and 81% of treatment and control farmers grew rice in 2014; the difference is not statistically significant (p-value of 0.13). The shares of treatment and control farmers growing rice were 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 with 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 estimate is also negative but not significant in 2015.

In Appendix S1, Table A4 reports the estimates of the ITT effects on rice harvests, a lower share of treatment than control farmers harvested in the first semester of 2015 (12% points less), in line with the declines in planting and yields. Treatment farmers also more often lost a planted harvest: 8 percentage points (p.p). more compared with a mean of 22% in control group (RI p-value of 0.11). 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 had 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 declined among treatment farmers. Treatment farmers benefited from the transfers in 2014 but only through the saved spending on inputs, not through higher yields.

5.2 | Input use

PTTA aimed to increase yields through input use. Table 4 shows the estimates of the main intervention ITT 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 with vouchers are accounted for in the measures of expenditures.

In spite of the vouchers, treatment farmers did not use more fertilizer per hectare than control farmers in 2014. Total expenditures on 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 0.05 and 0.14, respectively). That said, total fertilizer use was higher than the fertilizer directly obtained from the vouchers (with the total value of fertilizer expenditures of \$194 in the treatment, compared with \$135 obtained from the vouchers).[15] The point estimate of

[14]The vouchers also bring benefits by reducing the reliance on loans for purchasing inputs. Our data on the values of inputs paid with credit and the costs of credit do not allow us to precisely estimate benefits from the vouchers. However, we can use the value of redeemed vouchers (about \$280) and an estimated value of monthly interest rates (6%—see below) to estimate the cost of credit over 4 months (some inputs can be purchased later in the season) at about \$75. Incorporating these estimated values would further increase the treatment effects on profits.

[15]Treatment farmers report using less fertilizer per hectare than they were given in the vouchers for 0.5 or 0.25 hectares.

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	0.953	0.1375	0.4145
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	0.04	0.0345	0.1465
Control mean	195.0	97.8	137.6
Baseline outcome	yes	no	yes
Observations	515	515	515

Note: Estimates of ITT effects of 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, including the value of ones obtained with vouchers), 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 5%, and 10% levels, respectively.

the ITT effect is also negative for expenditures in 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.

Subsidies are for the same types of inputs that farmers can also obtain through direct commercial purchase (with their own resources). Table 4 reports the total value of purchases (including those obtained with vouchers and those obtained with own resources). Subtracting the value of the inputs they obtained by redeeming the vouchers (118.4 US$ on average for fertilizer and pesticides) from the difference between treatment and control (−51.1 US$ for fertilizer and pesticides) implies that crowding out of commercial purchases was large, amounting to about $170 US (the subsidies plus reduced spending) or about 55% of control group spending.

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

Disentangling these results, a bit further, in Appendix S1, Table A5 shows the estimates of the ITT effects on precise quantities of urea, NPK and sulfate. Although 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 continued to use less sulfate (the treatment effect is −10 kg, still about 40%) and also used 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.

6 | MECHANISMS

In this section, we explore a number of explanations for our core treatment effect results. We test the extent to which the assumptions that we laid out in our conceptual framework appear to hold in our context. Specifically, in the first subsection below we test if treatment farmers erroneously believe that subsidies are forthcoming in future periods investigating the effects of the information treatment. We test if farmers were credit constrained considering effects on access to loans before and after treatment in the second subsection and heterogeneity by baseline loans in the third subsection. In that subsection, we also test the hypotheses on risks associated with subsidized inputs by examining heterogeneity by water access, and hypotheses on learning how to use technologies with heterogeneity by prior use of inputs and receipt of technical advice.

6.1 | Uncertainty of future benefits and information treatment

One possible mechanism to explain treatment farmers decreasing their input use in 2015 is that they 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 randomly selected farmers whether or not they should 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 February 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 treatment farmers (62% of the uninformed) also still expected to receive vouchers. The information treatment reduced this share by only about 10% 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.[16]

[16]Given that the information treatment was randomized within villages and may have spread, contamination could have reduced the effects of the information treatment. However, such contamination seemed to have been limited as the majority of informed treatment farmers were, in February 2015, still expecting to receive vouchers in the coming agricultural season.

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

Note: Estimates of ITT 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% and 5% 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

Note: Estimates of interacted effects of 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 5% and 10% levels, respectively.

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/ha, was the largest for uninformed treatment farmers, but was also negative for informed farmers, at 215 kg/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.[17]

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, were almost twice as large for the uninformed than for the informed (respectively, 41 kg/ha, 24 US\$ and 21 US\$ compared with 27 kg/ha and \$15 US and \$11 US for the informed). The differences between uninformed and informed treatment farmers are not statistically significant.

On the other hand, although labor spending does increase (by \$31 US), the evidence on control farmers reacting to information on future transfers is limited, possibly because the share expecting transfers among those not receiving the information intervention was already very high.

As the information intervention was only partially successful in aligning farmers' expectations with the planned voucher distribution in 2015, power is reduced when contrasting the outcomes of the informed and uninformed. Nevertheless, the results suggest that wrong expectations partly explain the decline in cultivation intensity among 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

TABLE 7 Effects on input use, with information treatment

	Amount of chemical fertilizer (kg/ha)	Spending in chemical fertilizer 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

Note: Estimates of interacted ITT effects of 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. ** denotes statistical significance, using the conventional test, at the 5% level.

[17]The p-value is 0.10 with the post-selected controls, as shown in Appendix S1, Table A21.

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.

6.2 | Use of credit

As described in the conceptual framework, 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 show that farmers had experience using the promoted inputs and therefore were unlikely knowledge-constrained. Liquidity constraints could still have limited, however, the use of optimal levels or combinations of inputs. We hence consider the extent of the credit constraints they faced.

Table 8 describes loan taking among control farmers. Significant shares of these farmers requested and obtained loans, specifically for inputs. In late 2014, 66% reported having requested a loan from any source, and 48% requested one to pay for inputs. Loans were requested from both banks and informal sources, primarily small traders (loans for inputs are seldom requested from other households). In 2014, 31% of farmers reported having asked for, 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, many loans lasted longer than a single, agricultural season: 60% of bank loans and 31% of merchant loans were for 6 months or more. Although the data are noisy, estimates of the mean interest rates are higher for loans from traders (6.9% monthly in 2014) than for bank loans (5.3%). These same patterns are observed in 2015. Access to agricultural credit for plowing and fertilizer from a trader is often tied to an obligation to sell large quantities of rice to the creditor at harvest. Farmers thus had some access to finance, even if it was at high interest rates.

Table 9 reports the estimates of program ITT effects on access to finance. Consistent with receiving subsidies in 2014, treatment farmers requested fewer loans from either banks or merchants and on average borrow smaller amounts. Almost no treatment farmers requested a loan for inputs from

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

Note: 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	0.0055	0.5975	0.0925	0.074
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	0.5235	0.055	0.076	0.9705
Control mean	0.211	175.540	0.204	36.368
Baseline outcome	no	no	no	no
Observations	515	515	515	515

Note: Estimates of ITT effects of 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.

a bank, whereas 14% of control farmers do, and 17% of them requested such a loan from a merchant against 26% of control farmers. The mean amount borrowed to traders decreases significantly by about half, from $49 US among the control to $25 US among the treated. The point estimate is also negative but not statistically significant on the mean amount borrowed from a bank. Hence, farmers seem to have substituted the vouchers for own expenses to acquire inputs in 2014.

More strikingly and in accordance with the decline in cultivation intensity, farmers continued to request fewer loans from traders in 2015: Only 12% of treatment farmers do so against 20% of control ones. The amount treatment farmers borrowed from banks also decreased by $64 US from a control mean of $176 US. 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 allowed some of them to switch to a new less-intensive and low-debt equilibrium.[18]

[18]Table A6 in Appendix S1 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 loans less often in both 2014 and 2015 to pay for inputs. Treatment farmers also purchased fertilizer and pesticides less often in the Dominican Republic.

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

Note: Estimates of interacted ITT effects of 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 5% and 10% levels, respectively.

Table 10 introduces the additional effects of the information treatment. Although the effects on loans from traders do not vary with the information on future subsidies, only 16% of uninformed treatment farmers borrowed from banks, whereas 24% of informed ones do, and this difference is significant at 1%. The mean amounts borrowed were also significantly lower among uninformed treatment farmers.

6.3 | Further evidence on mechanisms

We now provide further evidence on mechanisms. First, to investigate how farmers' resources correlate with program impacts, Appendix S1, Tables A7–A10, document heterogeneity of the effects by baseline access to credit, experience with subsidized inputs, access to family labor, and water. The estimates in Appendix S1, Table A7, show that the decrease in yields in 2015 was significantly more pronounced for farmers who were already using subsidized fertilizers (both NPK and urea—40% of households report their joint use) at baseline, and decrease in profits that year was significant for these farmers. As seen in Appendix S1, Table A8, the effects on yields were also significantly more negative for farmers with baseline access to credit. As seen in Appendix S1, Table A9, the effects of the program otherwise did not differ significantly by available family labor. These patterns confirm that neither access to credit nor the lack of information and ability to use subsidized inputs were major constraints. In addition, Appendix S1, Table A10 shows that the effects of the program did not differ either significantly by access to water.

Second, to consider the constraints of program implementation, Appendix S1, Table A11, documents heterogeneity of program effects by late receipt of vouchers.[19] Twenty percent of farmers

[19]Heterogeneity results in Appendix S1, Tables A11 and A12 consider interaction effects with variables measuring aspects of program implementation. As these variables are endogenous, these results should be interpreted with caution. They are mainly presented to understand the plausibility of alternative mechanisms.

report they received some vouchers late. However, late receipt does not seem to explain the lack of increase in yields in 2014 and their decrease in 2015. Point estimates suggest that, if anything, there were somewhat lower decreases in yields for farmers who received the vouchers late, likely because the weather conditions were worse in the first rice season and late delivery actually allowed some of them to use vouchers during the second rice season.

Third, we consider changes in farming 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 them, generate uncertainty on optimal production decisions. It may have been 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; 50% reported having received visits from the program contractor agents, but only 29% report having received advice on specific techniques during these visits. Appendix S1, Table A12 shows heterogeneity of program effects by receipt of a visit with technical advice and shows that farmers who received advice exhibit somewhat larger yields than other treatment farmers and profits (when accounting for the transfers) but not statistically significantly so. 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 also 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 was not a *retonn* variety. Appendix S1, Table A13 shows the intervention effects on these farming practices. In 2014, the treatment indeed did seem to have induced a shift to these practices. Specifically, treatment farmers decreased quantity of seeds per hectare by 33 kg (30%), 20 percentage points more of them (twice as many as the control farmers) transplanted three plants per hole, 14 percentage points less of them (a third less) practiced *retonn*. Some of these effects, although declining (with the exception of decreased *retonn*), persisted in 2015. Treatment farmers are still using 11 kg (11%) less seeds per hectare, are 8 p.p. more likely to seed three plants per hole, and 15 p.p. less likely to practice *retonn*. As farmers did shift toward the recommended practices, it seems somewhat unlikely that the lack of results on rice productivity is driven by the limited technical assistance. Treatment farmers continued using some recommended practices in 2015, though these did not translate to higher yields.

In summary, the experiment showed unanticipated negative effects of the program on rice production and yields. This suggests that the assumptions underlying the design of the intervention did not hold, 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 was likely 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.

7 | WELFARE AND NONAGRICULTURAL INVESTMENTS

Even though farmers experienced reduced production and yields, the subsidies they received during one season in 2014 represent, on average, transfers of about $276. Subsidies have income effects and can change saving and investment behaviors. Farmers could be using some of the extra funds freed up by the transfers to clear the hurdle for some lumpy investment, like in migration or bulky farming or household assets, or they could also deliberately increase current consumption at the cost of lower future production borrowing on income gains from transfers. One could hypothesize such a potential effect to be particularly important for the poorest, but, as shown in Appendix S1, Table A14, there is no significant treatment heterogeneity by baseline food security (proxied by severe hunger—reported by 33% of households at baseline), which likely correlates with the discounting of future benefits and consumption. Instead, the intervention seemed to have reduced farmers reliance on loans with high-interest rates, often tied to rice production, and as such could have generated further welfare gains.

Considering such welfare gains directly, we estimate impacts on food security, and 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 S1, Tables A15–A18.

Food security, a relevant welfare economic outcome in this context and a target outcome of PTTA, was 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 summarize financial, physical, and land assets, 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-invested in land, as treatment farmers slightly reduced their number of plots and ownership of a lagoon plots. We further 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. The area cultivated with nonrice crops did not change significantly with treatment and, if anything, treatment farmers harvested and sold less of other crops. We further find no difference in the number of households working in the Dominican Republic and also find no effects on (permanent) migration: 7% of households report having a member migrate to a country out of Haiti (Brazil, Dominican Republic, Turks and Caicos, and Chile), but there is no significant difference between treatment and control. Unfortunately, we do not have information on other economic activities.

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 to measure welfare effects beyond food security and asset accumulation, it is possible that we are missing effects on other dimensions of welfare.

8 | LIMITATIONS

Although randomization ensures that our study is internally valid, the interpretation of the estimated effects needs to account for the specific context, as well as a number of limitations, that are likely to affect the estimates. First, key assumptions necessary for the success of a smart subsidy program did not appear to hold for rice farmers in Northeast Haiti. The inputs being provided led to risky returns (as they were rainfall dependent), and farmers were not credit-constrained before the program. This limits our ability to speak to the potential effect of smart subsidy programs in other contexts. Moreover, the period over which the experiment took place was characterized by a drought. As Rosenzweig and Udry (2020) highlight, such aggregate shocks affect the external validity of almost any study considering agricultural outcomes (and indeed many others too). Although this implies a further caveat to the findings, weather shocks are not uncommon in many low- and middle-income countries, and hence similarly could affect the functioning of subsidy programs in other settings.

Next, there were some implementation challenges as the vouchers were delayed and price increases resulted in the vouchers providing less than the recommended number of inputs. Both these factors likely led to impacts on farming than were less beneficial than expected. Although this means that our results may not speak to what would happen for a program implemented without facing any challenges, it is a valuable lesson to future implementers that smart subsidy programs are complicated to implement, and small (often unavoidable) deviations from the design may lead to unanticipated outcomes.

Like many studies, our analysis to a large extent relies on self-reported primary data and could be affected by strategic misreporting by farmers. A particular concern for any voucher program is the potential underreporting of reselling of vouchers or inputs. We are however partly able to draw on administrative data too, alleviating some of these concerns, at least with regard to program implementation. We further note there were no clear incentives for misreporting in this context but can certainly not fully rule it out.

This paper did not conduct its analysis according to a pre-analysis plan (Casey et al., 2012). Althoug this is increasingly common in development economics, it was not so at the time we were developing this research in 2013. Moreover, the research team did publish an impact evaluation concept note online before the baseline data collection (World Bank, 2013). This concept note laid out the broad experimental design, the primary outcome indicators, and the general research framework, followed in this article. The analysis of the mechanism underlying the unexpected results was (logically) not anticipated and hence is more exploratory in nature.

Finally, the main experiment was sufficiently powered to uncover the main results of this paper, showing a significant negative effect on yield, going directly against the objective of the intervention. Power limitations do however limit our ability to fully explain this unanticipated result.

9 | CONCLUSION

We use data from a randomized control trial to examine the effects of a large-scale government-run subsidy program in Haiti that provided, through vouchers, subsidies for inputs (seeds, fertilizer, pesticides, and specific labor tasks) for rice during one season. Our results stand in stark contrast to the positive effects of smart subsidies found in the only other randomized control trial of smart subsidies in Mozambique (Carter et al., 2021). Our results show that this "smart" subsidy program led to lower input use and lower yields both in the year the subsidies were received, as well as the following year. Rather than increasing their total input use, farmers substituted the subsidized inputs with (the same) inputs they otherwise would have financed with credit. Although this behavior lowered yield for treatment farmers, it did not subsequently 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.

Independent of the mechanisms, the results show that the voucher program did not result in higher yields in this setting. Although perhaps logical after the fact, the government or other stakeholders clearly did not anticipate or aim for decreased yields when designing the program. In this case, crowding out of commercially priced fertilizer was even more extreme than estimated in studies using panel surveys in sub-Saharan Africa (Jayne et al., 2018). The results highlight the risk associated with using expensive inputs during drought periods of which farmers are already aware. Information and credit constraints to technology adoption do not seem to apply in this instance, as farmers were aware of the inputs, many were purchasing them—often with loans. Although the vouchers provided a riskless way to experiment with the particular combination of inputs, the drought year did not provide the farmers with favorable feedback.

Although the majority of vouchers were used, and some new practices adopted, to evaluate the external validity of these findings, it is important to keep certain unexpected developments and challenges in the implementation of the program in mind. In particular, some steps in the program implementation were delayed and others were incomplete; there were sudden changes in prices; and unrealistic expectations about future subsidies 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 serve to highlight that in such environments, smart subsidy programs may need to be avoided, as they require considerable information to optimally design, careful coordination across many actors to deliver and, in the case of vouchers, functioning input markets. The results in this paper show that not getting an intervention's assumptions and implementation right not only reduces 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.

ACKNOWLEDGMENTS

This article is based on a collaboration between MARNDR (the Ministère de l'Agriculture, des Ressources Naturelles et du Développement Rural in Haiti), the Inter-American Development Bank (IADB), Development Impact Evaluation (DIME)—World Bank, and PSE. We are grateful to Hermann Augustin, Jean Marie Robert Chéry, and the staff at the MARNDR. Sébastien Gachot was instrumental in setting up the experiment and baseline data collection, and we are grateful to him, Gilles Damais, Bruno Jacquet, and IDB-Port-au-Prince team for their dedication to the project. We are also grateful to Diana Lopez-Avila, Hamza Benhhadou, and Paul Dutronc-Postel for field coordination and to Nitin Bharti, Michell Dong, and Tom Wright for supporting the analysis. At DIME, we are indebted to Florence Kondylis for support throughout the project, and to Kristoffer Bjarkefur, Maria Ruth Jones, and Cindy Sobieski for sharing their expertise in data collection. Herard Jean Pierre Osias and Marckendy Petit-Louis were superb survey leads. We thank the agronomy students of the Université d'État d'Haïti, Limonade Campus along with all surveyors. We are appreciative of inputs and comments at various parts of the study from Jean Dieugo Barthelus, Paul Christian, Bertrand Dayot, Estimable Frantz, Jery Rambao, Josias Toussaint, and Collins Zamor, among others. The article benefited from discussions with Tanguy Bernard, Andrew Foster, Markus Goldstein, Alain de Janvry, Jeremy Magruder, Paul Winters, and participants at a DIME workshop, EUDN conference, FAO, LACEA conference, and seminars at PSE, and from comments by the editor and three anonymous referees. We thank the rice growing residents of Bas and Haut Maribahoux for graciously sharing their experiences with the researchers. We gratefully acknowledge support and funding from GAFSP, IADB, and the Agence Nationale de la Recherche under grant ANR-17-EURE-001.

DATA AVAILABILITY STATEMENT

More empirical analysis was conducted than can be included in the article. The interested reader can find them in the online supplementary appendix and through this link: https://github.com/wrightkelsey/ptta_haiti.

REFERENCES

Balboni, Clare, Oriana Bandiera, Robin Burgess, Maitreesh Ghatak, and Anton Heil. 2021. "Why Do People Stay Poor?" *Quarterly Journal of Economics* 137: 785–844.

Bandiera, Oriana, and Imran Rasul. 2006. "Social Networks and Technology Adoption in Northern Mozambique." *Economic Journal* 116: 869–902.

Bardhan, Pranab, and Christopher Udry. 1999. *Development Microeconomics*. Oxford: OUP.

Barrett, Christopher B. 2002. "Food Security and Food Assistance Programs." In *Handbook of Agricultural Economics*, Vol 2, edited by Bruce L. Gardner and Gordon C. Rausser, 2103–90. Amsterdam: Elsevier.

Bayard, Budry 2011. "Projet de Transfert de Technologies aux Petits Agriculteurs de la Région Nord." Working paper, Ministère de l'Agriculture, des Ressources Naturelles et du Développement Rural (MARNDR) et Banque Interaméricaine de Développement IDB).

Beaman, Lori, Dean Karlan, Bram Thuysbaert, and Christopher Udry. 2013. "Profitability of Fertilizer: Experimental Evidence from Female Rice Farmers in Mali." *American Economic Review* 103: 381–6.

Belloni, Alexander, Victor Chernozhukov, and Christian Hansen. 2014. "Inference on Treatment Effects after Selection among High-Dimensional Controls." *Review of Economic Studies* 81: 608–50.

Carter, Michael R., and Christopher B. Barrett. 2006. "The Economics of Poverty Traps and Persistent Poverty: An Asset-Based Approach." *Journal of Development Studies* 42: 178–99.

Carter, Michael R., Rachid Laajaj, and Dean Yang. 2021. "Subsidies and the African Green Revolution: Direct Effects and Social Network Spillovers of Randomized Input Subsidies in Mozambique." *American Economic Journal: Applied Economics* 13: 206–29.

Carter, Michael R. 2013. "The Impact of Voucher Coupons on the Uptake of Fertilizer and Improved Seeds: Evidence from a Randomized Trial in Mozambique." *American Journal of Agricultural Economics* 95: 1345–51.

Casey, Katherine, Rachel Glennerster, and Edward Miguel. 2012. "Reshaping Institutions: Evidence on Aid Impacts Using a Preanalysis Plan." *Quarterly Journal of Economics* 127: 1755–812.

Chirwa, Ephraim, and Andrew Dorward. 2013. *Agricultural Input Subsidies: The Recent Malawi Experience*. Oxford: OUP.

Dasgupta, Partha, and Debraj Ray. 1986. "Inequality as a Determinant of Malnutrition and Unemployment: Theory." *Economic Journal* 96: 1011–34.

del Ninno, Carlo, Paul A. Dorosh, and Kalanidhi Subbarao. 2007. "Food Aid, Domestic Policy and Food Security: Contrasting Experiences from South Asia and Sub-Saharan Africa." *Food Policy* 32: 413–35.

Duflo, Esther, and Abhijit Banerjee. 2011. *Poor Economics: A Radical Rethinking of the Way to Fight Global Poverty*, Vol 619. New York City: PublicAffairs.

Duflo, Esther, Michael Kremer, and Jonathan Robinson. 2011. "Nudging Farmers to Use Fertilizer: Theory and Experimental Evidence from Kenya." *American Economic Review* 101: 2350–90.

Ebata, Ayako, Nick Nisbett, and Stuart Gillespie. 2020. *Food Systems and Building Back Better*." Positioning paper. Brighton: Institute of Development Studies.

Emerick, Kyle, Alain de Janvry, Elisabeth Sadoulet, and Manzoor H. Dar. 2016. "Technological Innovations, Downside Risk, and the Modernization of Agriculture." *American Economic Review* 106: 1537–61.

FAO, IFAD, UNICEF, WFP, and WHO. 2020. *The State of the Food Security and Nutrition in the World 2020. Transforming Food Systems for Affordable Healthy Diets*. Rome: FAO.

Ferreira, Francisco H. G., Fiszbein Ariel, Grosh Margaret Ellen, Keleher Niall, Olinto Pedro, Schady Norbert Rudiger, and Skoufias Emmanuel. 2009. "Conditional Cash Transfers : Reducing Present And Future Poverty - Overview." *A World Bank Policy Research Report*. Washington, D.C.: World Bank Group.

Foster, Andrew D., and Mark R. Rosenzweig. 2010. "Microeconomics of Technology Adoption." *Annual Review of Economics* 2: 395–424.

Hemming, D., E. Chirwa, H. Ruffhead, R. Hill, J. Osborn, L. Langer, L. Harman, C. Coffey, A. Dorward, and D. Phillips. 2018. "Agricultural Input Subsidies for Improving Productivity, Farm Income, Consumer Welfare and Wider Growth in Low- and Middle-Income Countries: A Systematic Review." *Campbell Systematic Reviews* 14: 1–153.

Jayne, Thomas, Nicole Mason, William Burke, and Joshua Ariga. 2018. "Review: Taking Stock of Africa's Second-Generation Agricultural Input Subsidy Programs." *Food Policy* 75: 1–14.

Jayne, Thomas, and Shahidur Rashid. 2013. "Input Subsidy Programs in Sub-Saharan Africa: A Synthesis of Recent Evidence." *Agricultural Economics* 44: 547–62.

Karlan, Dean, Robert Osei, Isaac Osei-Akoto, and Christopher Udry. 2014. "Agricultural Decisions after Relaxing Credit and Risk Constraints." *Quarterly Journal of Economics* 129: 597–652.

Kennedy, Adam, and Dnnielle Resnick. 2020. *"From Farm to Table: How Are Governments Keeping Food Systems Functioning during COVID-19?" COVID-19 Policy Response Portal Project Notes*, Vol 1. Washington, DC: International Food Policy Research Institute (IFPRI).

Kraay, Aart, and David McKenzie. 2014. "Do Poverty Traps Exist? Assessing the Evidence." *Journal of Economic Perspectives* 28(3): 127–48.

Laajaj, Rachid, Karen Macours, C. Masso, Moses Thuita, and Bernard Vanlauwe. 2020. "Reconciling Yield Gains in Agronomic Trials with Returns under African Smallholder Conditions." *Scientific Reports* 10(1): 14286.

Macours, Karen 2019. "Farmers' Demand and the Traits and Diffusion of Agricultural Innovations in Developing Countries." *Annual Review of Resource Economics* 11: 483–99.

Magruder, Jeremy 2018. "An Assessment of Experimental Evidence on Agricultural Technology Adoption in Developing Countries." *Annual Review of Resource Economics* 10: 299–316.

Marenya, Paswel, and Christopher Barrett. 2009. "State-Conditional Fertilizer Yield Response on Western Kenyan Farms." *American Journal of Agricultural Economics* 91: 991–1006.

Mason, Nicole M., Thomas Jayne, and Rhoda Mofya-Mukuka. 2013. "Zambia's Input Subsidy Programs." *Agricultural Economics* 44: 613–28.

Maxwell, Daniel, Luca Russo, and Luca Alinovi. 2012. "Constraints to Addressing Food Insecurity in Protracted Crises." *Proceedings of the National Academy of Sciences* 109: 12321–5.

Michler, Jeffrey D., Emilia Tjernström, Simone. Verkaart, and Kai Mausch. 2019. "Money Matters: The Role of Yields and Profits in Agricultural Technology Adoption." *American Journal of Agricultural Economics* 101: 710–31.

Monteleone, Beatrice, Brunella Bonaccorso, and Mario Martina. 2020. "A Joint Probabilistic Index for Objective Drought Identification: The Case Study of Haiti." *Natural Hazards and Earth System Sciences* 20: 471–87.

Pingali, Prabhu, Luca Alinovi, and Jacky Sutton. 2005. "Food Security in Complex Emergencies: Enhancing Food System Resilience." *Disasters* 29: S5–S24.

Ricker-Gilbert, Jacob, and Thomas Jayne. 2017. "Estimating the Enduring Effects of Fertiliser Subsidies on Commercial Fertiliser Demand and Maize Production: Panel Data Evidence from Malawi." *Journal of Agricultural Economics* 68: 70–9.

Rosenzweig, Mark, and Christopher Udry. 2020. "External Validity in a Stochastic World: Evidence from Low Income Countries." *Review of Economic Studies* 87: 343–81.

World Bank. 2013. *Haiti Project for Technology Transfer to Farmers (PTTA) Impact Evaluation Concept Note*." Working paper. Washington, DC: Development Impact Evaluation (DIME).

World Bank. 2007. *World Development Report 2008: Agriculture for Development*." Working paper. Washington, DC: World Bank.

Young, Alwyn. 2018. "Channeling Fisher: Randomization Tests and the Statistical Insignificance of Seemingly Significant Experimental Results." *Quarterly Journal of Economics* 134: 557–98.

SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

How to cite this article: Gignoux, Jérémie, Karen Macours, Daniel Stein, and Kelsey Wright. 2022. "Input Subsidies, Credit Constraints, and Expectations of Future Transfers: Evidence from Haiti." *American Journal of Agricultural Economics* 1–27. https://doi.org/10.1111/ajae.12337