Reshaping Agricultural Subsidies*

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Abstract

We study the optimal shape of agricultural input subsidies. We randomize subsidy increases to Mozambican farmers, relative to a reference group with a subsidy rate that is decreasing in input quantity. A sharply decreasing subsidy (higher for small quantities) increases payouts to poorer farmers. A uniform subsidy (higher for large quantities) increases production by 36% by increasing input use among marginally productive farmers, overcoming both informational and financial constraints. A high and decreasing subsidy (higher for both quantities) diverts these farmers from large to small input quantities. Planners with plausible preferences over productivity, transfers, and equity prefer a uniform subsidy.

Keywords: subsidy design, agricultural input subsidies, self-targeting, equity–efficiency trade-off, agricultural productivity, Mozambique.

JEL Codes: O1, D6, Q1.

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Among the wickedmost problems in development economics is the persistence of low agricultural yields, which go hand in hand with low levels of modern input use. In developing countries, multiple market failures constrain productive investments in modern agricultural inputs (Udry, 1999; Karlan et al., 2014; Jones et al., 2022). These constraints have motivated large expansions of input subsidy programs in many African countries, increasing input use and partially closing yield gaps (Carter et al., 2021). Yet, during these expansions, input-adjusted agricultural productivity in Africa has fallen (Wollburg et al., 2024).

One hypothesis that explains this finding is that the *shape* of input subsidies (how subsidy rates vary across input quantities) has caused the wrong farmers to increase their input use. Many input subsidy programs offer a high subsidy rate on a fixed quantity of inputs. If farmers with low baseline input use also have low marginal returns (the "wrong" farmers), such designs may induce inefficient increases in input use (Duflo et al., 2011; Suri, 2011; Diop, 2025). While policymakers would ideally increase input use by the "right" farmers, those with high marginal returns, marginal returns are challenging to observe. An alternative is to leverage self-targeting, reshaping input subsidies by changing subsidy rates across input quantities. Subsidy changes shift which farmers increase input use and could increase agricultural productivity if the responsive farmers are the right ones.

In this paper, we experimentally manipulate the shape of agricultural input subsidies by cross-randomizing increased subsidies on small and on large quantities of inputs across 1,280 farmers in northern Mozambique. We estimate both distributional incidence and impacts on agricultural productivity: inappropriately shaped agricultural input subsidies may be doubly ineffective, redistributing subsidy payouts regressively, and reducing productivity.

Our experiment is embedded in the roll-out of a new agricultural input subsidy technology, the *eVoucher*. As in many countries in Africa, cereal yields in Mozambique remain stubbornly low and are associated with limited fertilizer use. This suggests the possibility of high returns to increasing the use of modern inputs, and motivates the eVoucher program. In contrast with paper vouchers often used in subsidy programs, eVouchers enable farmers to choose from multiple possible quantities of inputs at distinct subsidy rates and from a flexible menu including tools, seeds, fertilizer, and pesticides. The eVouchers were redeemable at enrolled agrodealers.

To induce self-targeting, eVoucher eligibility was broad and three quantities of inputs were offered with distinct prices. The reference *Decreasing* subsidy schedule allowed farmers to choose between a 90% subsidy for a *Small* quantity of inputs (pay 400 MZN for

4,000 MZN of value), an 82% subsidy for *Medium* (pay 1,200 for 6,500), or a 62% subsidy for *Large* (pay 4,200 for 11,000).[1]

We cross-randomize one season of additional subsidies for smaller and larger input quantities, making use of the eVoucher technology. We randomize four experimental arms across up to 8 farmers within each of 187 villages, 1,280 farmers in total. One quarter of farmers were assigned to Decreasing, and serve as our reference group. One quarter of farmers were assigned to *Sharply Decreasing*, a 300 MZN reduction in the price of Small (pay 100 for 4,000) and Medium (pay 900 for 6,500). Sharply Decreasing involves near-free distribution of Small. One quarter of farmers were assigned to *Uniform*, a 2,600 MZN reduction in the price of Large (pay 1,600 for 11,000). With an 85% subsidy for Large the subsidy rates under Uniform are approximately uniform across the three quantities. Finally, one quarter of farmers were assigned to *High & Decreasing*, combining the price reductions under Sharply Decreasing and Uniform for all three quantities.

We collect detailed data on farmers' household characteristics and agricultural production in two rounds of household surveys: one baseline and one follow-up referencing the experimental season. We complement these data with administrative data on eVoucher redemptions covering the experimental season and two subsequent years.

Our central finding is that reshaping input subsidies comes with an equity–efficiency trade-off. Our experimental results reveal that farmers who purchase larger input quantities are more marginally productive. In addition, these farmers are descriptively richer. Relative to input subsidy programs with a high subsidy rate on a fixed quantity of inputs, a more uniform subsidy rate across input quantities maximizes productive efficiency but is strongly regressive.

We begin our analysis by showing that farmers' response to experimental variation in the shape of input subsidies is consistent with theory. Subsidies for a given quantity increase demand for that quantity while inducing substitution away from adjacent quantities. We also find no evidence of resale, storage, or crowding out in response to the additional subsidies, so productive impacts are informative about the marginal returns to additional inputs. In addition, the average farmer redeeming Large had 44% higher predicted baseline consumption than the average farmer redeeming the Small package: subsidizing larger quantities is regressive.

Turning to productive impacts, we find that Uniform (a higher subsidy for Large)

[1] We discuss the interpretation of these package sizes in additional detail in Section I.B and I.C. In brief, the value of and subsidy on the Large package are similar to the vouchers for a fixed input package studied by Carter et al. (2021) in central Mozambique. These are much smaller than the package sizes for input subsidy programs for which resale, a possibility with nonlinear or targeted subsidies, is commonly observed (Diop, 2025).

increased total agricultural production by 36%. We conduct a "compliers" analysis: 19% of farmers shift into Large and these compliers are 78% more productive under Decreasing than the average farmer under Decreasing, so our estimate corresponds to a 103% increase in production among compliers. This estimate is statistically indistinguishable from the 80% increase estimated by Carter et al. (2021) for a similarly sized input package, and implies high returns to agricultural inputs for these farmers—conceptually, the right farmers.

In contrast, Sharply Decreasing (higher subsidies for Small and Medium) and High & Decreasing (higher subsidies for Small, Medium, and Large) had no impact on agricultural production, despite increasing input use. Uniform therefore increased input use by more marginally productive farmers on average.

Impacts on profits are similar to impacts on production, as productive impacts are large relative to impacts on inputs and labor. Although consistent with existing work on agricultural inputs (Duflo et al., 2008; Carter et al., 2021), this need not hold in general for productive technologies (Jones et al., 2022).

Are the Uniform compliers more marginally productive because of fundamentals, or simply due to increasing returns to scale in input quantity? We show that our productive impacts, and demand for Large, are concentrated among row-planting farmers. The productive returns to inputs are low absent row planting (Beaman et al., 2013), but row planting is also difficult to learn and adopt (Cefala et al., 2025, 2026). This suggests productive heterogeneity underpins our findings.

The large effects of Uniform on profits are inconsistent with profit maximization; we provide evidence that farmers underestimate the productive impacts of inputs and learn from experience. We estimate the impacts of additional subsidies on input quantities in the two years after they were withdrawn. Uniform has persistent impacts on the redemption of Large, consistent with learning. A back-of-the-envelope calculation suggests experiencing Large increases willingness to pay by 62%. However, these long-run impacts of Uniform on redemption all but vanish for poorer farmers: financial constraints may also limit adoption.

Our results reveal an equity–efficiency trade-off, as farmers using more inputs are richer yet more marginally productive; we develop a simple quantitative framework to assess this trade-off. We begin with farmers that may not fully internalize the productive impacts of inputs, consistent with our finding that farmers learn from experience. The social planner therefore values both increasing farmers' perceived surplus, which farmers maximize when choosing inputs, and also any uninternalized impacts of inputs on farmers' production; our modeling approach closely follows existing work on optimal

taxation with internalities (Allcott et al., 2019; Farhi and Gabaix, 2020). In this framework, we derive three sufficient statistics to rank treatment arms by their marginal value of public funds (Finkelstein and Hendren, 2020; Hendren and Sprung-Keyser, 2020; Hahn et al., 2026): impacts on subsidy payouts and equity-weighted impacts on production, which we directly estimate, and equity-weighted perceived surplus, which we nonparametrically bound. Our approach extends naturally to complementary work that estimates demand and treatment effects from experimental variation in extensive margin prices (e.g., Ashraf et al., 2010; Cohen and Dupas, 2010; Berry et al., 2020; Mahmoud, 2025) and, as in this paper, intensive margin price schedules (Abubakari et al., 2025; Dizon-Ross and Zucker, 2026).

We apply our quantitative framework to derive three key results on the tension between increasing transfers to poorer farmers, achieved by subsidizing smaller quantities, and increasing agricultural production by richer farmers, achieved by subsidizing the larger quantities. First, when the planner views farmers as fully optimizing, shifting to Sharply Decreasing is preferred to shifting to Uniform, as subsidizing smaller quantities targets poorer farmers. Second, when a weight is placed directly on productive impacts that reflects our observed impacts on learning, the planner prefers shifting to Uniform. Third, there are no planner preferences under which shifting to High & Decreasing is both preferred to shifting to Sharply Decreasing and to shifting to Uniform. While agricultural input subsidies can be an effective policy, they are in fact doubly ineffective when inappropriately shaped.

Contributions to Literature. This paper most centrally contributes to a literature on the under-adoption of productive technologies in development economics (reviewed by, e.g., de Janvry et al., 2017; Magruder, 2018; Suri and Udry, 2022). Under-adoption in our context is stark, as we study a highly productive technology with near-zero baseline adoption. Subsidizing under-adopted productive technologies is an obvious, albeit costly, remedy; this paper conceptually and empirically explores subsidy shape in the context of agricultural inputs, complementing work that considers other design features including timing (Duflo et al., 2011), permanence (Fishman et al., 2022), flexibility (Bushong et al., 2025), and facilitated resale (Diop, 2025).

The equity–efficiency trade-off we document is consistent with a diverse body of evidence in development economics finding the largest productive impacts from targeting investments in ways that reach the less poor (e.g., Hussam et al., 2022; Banerjee et al., 2025; Cingano et al., 2025; Haushofer et al., 2025). We replicate this finding by targeting with prices in the context of agricultural input subsidies, complementing evidence from Beaman et al. (2023) on selection into agricultural loans.

Our results also complement work on factor misallocation in agriculture, which finds that land and labor allocations are substantially more equal than productively efficient allocations (Adamopoulos and Restuccia, 2014; Foster and Rosenzweig, 2022); among the input subsidies we test, those that maximize dispersion of input usage also maximize productive impacts. A related literature additionally highlights an important role of selection into, and out of, agriculture in explaining links between structural transformation out of agriculture and agricultural productivity growth (Lewis, 1954; Lagakos and Waugh, 2013; Adamopoulos et al., 2022); through this lens, our findings imply the shift towards more productively efficient agricultural input subsidies complements the selection that accompanies structural transformation (Diop, 2025).

I. SETTING AND EXPERIMENT

A. Agriculture in Northern Mozambique

We study the roll-out of a new agricultural input subsidy technology, the eVoucher, in 10 Districts in Nampula and Zambezia Provinces in northern Mozambique. Average cereal yields are approximately 0.8 tons per hectare, in the first quartile of sub-Saharan Africa and far below the global average of 4.2 tons per hectare (Appendix Figure D1a). Agronomic evidence, including from Mozambique, suggests large productivity gains to modern inputs: appropriately applied inorganic fertilizer to crops planted with improved seeds (Duflo et al., 2008; Goujard et al., 2011).[2] Fertilizer use in Mozambique is in the second quartile of the region and an order of magnitude below agronomic recommendations (Appendix Figure D1b).[3]

However, simply increasing fertilizer use is unlikely to have positive returns for all farmers. Agronomic practices matter: farmer-led agronomic trials have found low returns to adoption of pre-planting fertilizer conditional on adoption of post-planting fertilizer for maize in Kenya (Duflo et al., 2008), and low returns to adoption of fertilizer for rice farmers who do not row plant in Mali (Beaman et al., 2013). In Mozambique, agronomic trials have found high returns to deep application of fertilizer pre-planting when combined with improved seeds (Goujard et al., 2011), a technique that requires farmers to row plant. Consistent with this agronomic evidence, an experimental evaluation of subsidies for a package of improved seed and fertilizer in Mozambique found high productive and economic returns among a selected group of farmers (Carter et al., 2021).

[2] Suri (2011) reviews agronomic evidence of returns to adoption of improved seeds and inorganic fertilizer.

[3] Several African countries have managed to increase modern input use and cereal yields in the last decade (Appendix Figures D1c and D1d). These increases are attributable in part to input subsidy programs (Carter et al., 2021).

It is likely that multiple constraints limit the adoption of modern inputs in Mozambique.[4] First, while fertilizer and improved seeds must be purchased before planting, primary season crops are harvested approximately 6 months after planting. Credit constraints may prevent farmers from borrowing out of harvest revenues to finance expenditures before planting. Second, search and transport costs to purchase inputs can be substantial (Aggarwal et al., 2024). Third, 38% of our sample reports that they do not row plant. Row planting is skill-intensive and costly to adopt, and is required for deep application of fertilizer (Cefala et al., 2025, 2026). Fourth, farmers may be uncertain about or underestimate the productive impacts of fertilizer and improved seeds (Carter et al., 2021).

B. Agricultural Input Subsidies

Agricultural input subsidies are an intuitively appealing, albeit costly remedy to under-adoption of modern inputs. If demand for agricultural inputs is downward-sloping, subsidizing agricultural inputs will increase their adoption. Yet, as subsidies do not directly address the underlying market failures that limit adoption, they may not increase input use by farmers with the highest returns to inputs.

An evolving understanding of the benefits and limitations of agricultural input subsidies has led to multiple waves of their redesign in sub-Saharan Africa. From the 1960s through the 1980s, many African countries implemented large input subsidies with government import and distribution. These programs were inefficiently managed and failed to reach poorer farmers and, as a result, most were phased out under structural adjustment (Morris et al., 2007; Jayne and Rashid, 2013; Holden, 2019). Starting in the 2000s, a second generation of input subsidy programs has worked through private input suppliers and aims to increase profits while also reaching poorer farmers (Morris et al., 2007; Carter et al., 2021). To meet these objectives, a typical second-generation program distributes vouchers to eligible farmers that can be redeemed for a fixed package of inorganic fertilizer and improved seeds at local agrodealers (Jayne and Rashid, 2013).

Recent work has highlighted limitations of the design of second-generation input subsidy programs. First, while eligibility criteria are specified with the intent of reaching poorer farmers and farmers with higher returns to additional inputs, these eligibility criteria are rarely followed in practice (Pan and Christiaensen, 2012; Jayne et al., 2018). When eligibility criteria were well enforced in a smaller-scale program, Carter et al. (2021) find large productive impacts of input subsidies. Second, subsidizing a fixed package

[4] Suri and Udry (2022) provide an exhaustive review of the constraints that limit the adoption of productive agricultural technologies in Africa.

may distort choices and exclude smaller-scale farmers. While packages aim to cover a typical farm, their sizes are calculated from recommendations based on model farms rather than representative farmers for whom optimal fertilizer use is much lower (Duflo et al., 2008). As a result, input packages are inefficiently large for all but a small fraction of farms.[5]

C. The eVoucher

The eVoucher, implemented by FAO in Mozambique, shares many features with the agricultural input subsidy programs described in Section I.B. This includes a previous generation of the input subsidy program we study, implemented in central Mozambique and studied by Carter et al. (2021). Farmers receive an input voucher from FAO extension agents during a registration campaign. The voucher can be redeemed for agricultural inputs at agrodealers certified and supported by FAO, or sales agents employed by the agrodealers. As in an increasing number of agricultural input subsidy programs, farmers receive an eVoucher card (Appendix Figure D2) rather than a paper voucher.[6]

We highlight two key differences between the eVoucher and the second-generation input subsidy programs described in Section I.B. First, since eligibility criteria for these programs are rarely followed in practice, the eVoucher features minimal targeting criteria: all agricultural households without government officials were eligible. Second, as input packages in these programs are too large for many farms yet appropriately sized for others, the eVoucher offers a menu of packages and prices with the aim of leveraging self-targeting.

Packages. The eVoucher offers farmers the choice between three package sizes—"Small," "Medium," and "Large"—with a decreasing subsidy rate, as summarized in Table 1. Farmers who choose Small pay 400 MZN for 4,000 MZN of inputs, a 90% subsidy rate. Farmers who choose Medium pay 1,200 MZN for 6,500 MZN of inputs, a 82% subsidy rate and a 68% marginal subsidy rate. Farmers who choose Large pay 4,200 MZN for 11,000 MZN of inputs, a 62% subsidy rate and a 33% marginal subsidy rate.

[5] Duflo et al. (2008) find 120kg/ha maximized farmers' profits in Kenya, and larger quantities in fact decrease profits. Recent studies of second-generation input subsidy programs included 100kg of fertilizer in Malawi (Kumar et al., 2026), 100kg in Tanzania (Pan and Christiaensen, 2012; Giné et al., 2022), and 400kg in Zambia (Diop, 2025). For an average farm in their samples, these correspond to 146kg/ha in Malawi, 155kg/ha in Tanzania, and 135kg/ha in Zambia. In contrast, the median redemption we observe of the largest package we study included 100kg of fertilizer, corresponding to 67kg/ha for an average farm. Carter et al. (2021) find large productive impacts of an input subsidy program in Mozambique that provided a fixed package with 100kg of fertilizer.

[6] Digital vouchers redeemable at private shops have been shown to improve implementation fidelity relative to in-kind distribution (Banerjee et al., 2023).

Table 1. Decreasing (Reference Group) Input Subsidy Schedule

Package	Value (MZN)	Price (MZN)	Subsidy rate
Small	4,000	400	90.0%
Medium	6,500	1,200	81.5%
Large	11,000	4,200	61.8%

Registration. At the start of each agricultural season, FAO extension agents register farmers in communities targeted for the eVoucher. A photograph of the registration process is presented in Appendix Figure D3a. Extension agents first work through local informants to organize a community meeting during which they register farmers. At registration, each farmer receives their eVoucher card and is informed of the menu of packages and prices. Farmers select their preferred package, and can revise their package choice through the extension agent. Extension agents use a tablet to register the farmer and their package choice, and photograph the farmer for verification in case the farmer loses their eVoucher card.

Redemption. At any point during the agricultural season, farmers can visit an agrodealer or their sales agent to redeem the eVoucher. A photograph of the redemption process is presented in Appendix Figure D3b. The agrodealer or sales agent first scans the farmer's eVoucher card in their sales app on an NFC-enabled phone, and confirms the farmer's registered package and subsidy. The farmer then chooses and receives agricultural inputs up to the value of the package, and pays the agrodealer or sales agent the package price. The agrodealer or sales agent reports the redeemed inputs into their sales app and confirms redemption. FAO transfers subsidy payouts, calculated from the resulting receipts, to the agrodealer's bank account on a monthly basis. Farmers can of course opt out of the program and simply not redeem their eVoucher.

Inputs. Farmers choose from four broad categories of inputs. First, improved seeds certified by FAO. Seeds could be open-pollinated or hybrid varieties, imported or from local seed multipliers, and covered a range of crops. Second, inorganic fertilizer, 50kg sacks of NPK and, sometimes, urea. Third, tools. Farmers faced tool-specific caps on the number they could purchase, with caps on hoes (5) and machetes (2) often binding. And fourth, pesticides, although these represent a negligible fraction of purchases. Each agrodealer proposes to FAO prices of inputs, and the specific inputs they will make available, before the start of each season, which are then registered into the sales app.

D. Timeline, Sample, and Data

Timeline. Our experimental evaluation of the impacts of the shape of agricultural input subsidies is embedded in a broader experiment cross-randomizing the roll-out of Farmer Field Schools (FFS) and eVouchers across communities (AEARCTR-008721, Christian et al., 2023). We present a timeline of data collection and eVoucher implementation across agricultural seasons in Figure 1. We conducted an initial listing of all households in each community between August and November 2020, recording basic household characteristics. We conducted a baseline survey from July to December 2021 covering the 2020 secondary and 2021 primary agricultural seasons, and a follow-up survey from July to September 2023 covering the 2022 secondary and 2023 primary agricultural seasons.

Baseline Sample. In each community at baseline, we surveyed approximately 8 households ("farmers") who had been randomly assigned, stratified on FFS-interest, to be prioritized by extension agents for eVoucher registration in eVoucher-assigned communities.[7,8] The additional subsidies we analyze in this paper were randomized across these priority-assigned farmers in eVoucher-assigned communities, and all analysis restricts to this baseline sample.

eVoucher Implementation. eVoucher registration was implemented in the 192 eVoucher-assigned surveyed communities prior to the 2022 secondary season. The experimental additional subsidies, which we describe in Section I.E, were implemented during registration in advance of the 2023 primary season.[9]

Survey Data. Our analysis of agricultural production focuses on outcomes measured during our baseline and follow-up household surveys. We describe details on the construction of all variables from our household survey and administrative data in Appendix A. Each survey round covered basic household characteristics, including a

[7] More precisely, our baseline sample comprised 1,538 farmers in 192 communities: 1 community with 6 farmers, 2 communities with 7 farmers, 183 communities with 8 farmers, and 6 communities with 9 farmers.

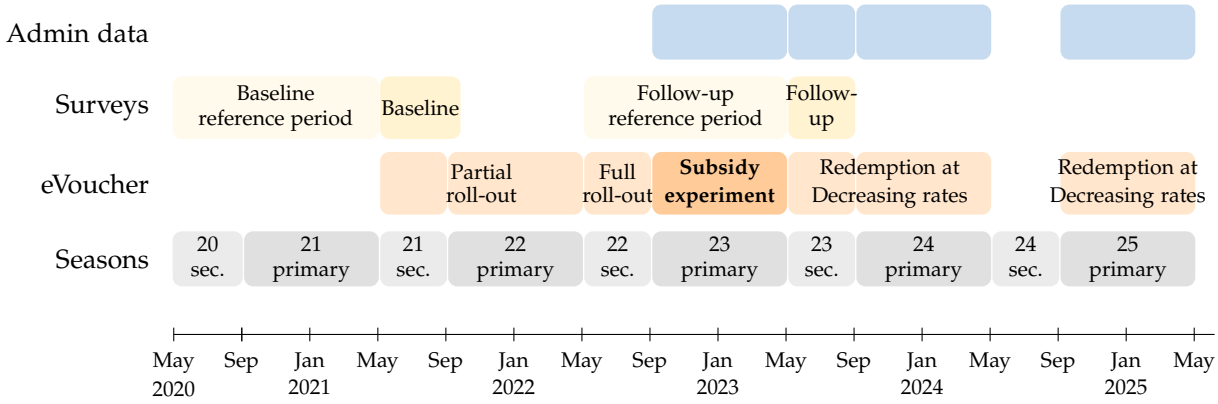
[8] These farmers are part of a larger sample of up to 12 farmers per community, which includes 4 farmers assigned to be deprioritized by extension agents and also includes not eVoucher-assigned communities. The sample excluded households who were not eligible for the eVoucher, that is non-agricultural households and households with a government official, and oversampled farmers identified by extension agents as likely to be interested in participating in an FFS ("FFS-interest").

[9] Implementing this experiment took a few attempts due to the COVID-19 pandemic and reduced access to field locations in its aftermath. We verify that assignment to these early attempts is balanced across our experimental arms. First, in 111 of the 192 communities, the implementation of the eVoucher began in advance of the 2021 secondary season. Second, we randomized additional subsidies during the 2022 primary season, but low registration rates yielded a small registered sample (362 farmers) and survey data referencing this agricultural season is not available, so we do not analyze their impacts.

member roster and an asset module, knowledge and adoption of agricultural practices, and detailed agricultural modules.[10] The agricultural modules captured seasonal crop production, agricultural labor, input purchases, and agricultural sales. Our analysis of impacts on agricultural outcomes focuses on the 2023 primary season.[11]

Administrative Data. We use administrative data on eVoucher registration and redemption to analyze the impacts of additional subsidies on input demand. For each redemption, the data include the agricultural season, the package redeemed (Small, Medium, or Large), and the quantities and prices of redeemed inputs. We group redeemed inputs into four categories: improved seeds, inorganic fertilizer, pesticides, and tools.

Figure 1. Timeline



E. Experimental Design

We cross-randomize additional subsidies for Small and Medium, and Large, during the 2023 primary season across farmers within our baseline sample in each community. We present the four resulting experimental subsidy schedules in Figure 2. Our reference group is the *Decreasing* subsidy schedule, described in Section I.C. Under *Sharply Decreasing*, farmers' prices of Small and Medium are reduced by 300 MZN relative to under

[10] In Section II.E, we estimate heterogeneity in effects with respect to adoption of row planting. We find that baseline- and follow-up-reported row planting are very weakly correlated, suggesting concerns with our baseline measure of row planting; as a result, our heterogeneity analysis uses follow-up-reported row planting. This could introduce bias if additional subsidies affect decisions to row plant; we test for endogeneity of row planting in Appendix Table E1 and fail to reject no endogeneity of row planting to additional subsidies.

[11] We additionally included questions on stated beliefs about the returns to agricultural inputs and willingness to pay (both stated and, in a random subsample during the baseline, revealed preference) for agricultural inputs. In practice, we find these measures at both baseline and follow-up are poor predictors of input demand, and we therefore do not use them in our analysis.

Decreasing, from 400 MZN and 1,200 MZN to 100 MZN and 900 MZN, respectively. Under *Uniform*, the farmers' price of Large is reduced by 2,600 MZN relative to under Decreasing, from 4,200 MZN to 1,600 MZN. Under *High & Decreasing*, farmers receive both sets of price reductions. In almost all communities, our baseline sample comprised 8 farmers, 2 of which we assigned to each of the four experimental arms.[12]

During the eVoucher registration campaign prior to the 2023 primary season, the extension agent assigned to each community was provided with one envelope for each farmer in the experimental sample in that community. The name of each farmer was printed on their envelope. Extension agents were instructed to first identify the farmer, and then open the envelope to read the farmer a script on the eVoucher. The English-translated contents of the envelope are presented in Figure 3; only the printed copayments and subsidy values were varied across the four experimental arms. Extension agents were then instructed to register farmers under their preferred package; registration is non-binding and costless to farmers. To ensure registration adhered to the experimental assignment, farmers could only register under their experimentally assigned subsidy rates on the eVoucher digital platform.

Figure 2. Experimental Design Featuring Cross-Randomized Subsidy Increases

Decreasing (Reference Group) 329 farmers	Value	Price	Rate
	4,000	400	90.0%
	6,500	1,200	81.5%
	11,000	4,200	61.8%

Sharply Decreasing 323 farmers	Value	Price	Rate
	4,000	**100**	**97.5%**
	6,500	**900**	**86.2%**
	11,000	4,200	61.8%

Uniform 305 farmers	Value	Price	Rate
	4,000	400	90.0%
	6,500	1,200	81.5%
	11,000	**1,600**	**85.5%**

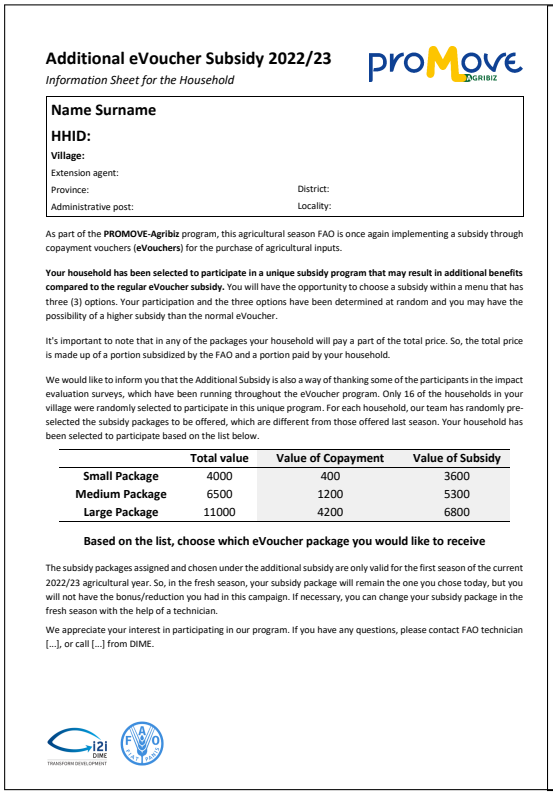
High & Decreasing 323 farmers	Value	Price	Rate
	4,000	**100**	**97.5%**
	6,500	**900**	**86.2%**
	11,000	**1,600**	**85.5%**

Notes: The four experimental arms and the package-specific subsidies under each arm are described in this table. Reported observation counts are for our primary analysis sample of registered farmers in our baseline sample, described in Sections I.D and II.A.

[12] We also randomized our four experimental arms across up to 8 additional farmers per community, whom we did not survey as discussed in Section I.D. We report impacts of additional subsidy assignment on package choice (observed in administrative data) when we include these farmers in Appendix Table D1. Relative to Table 2, estimates are qualitatively similar but slightly more precise.

Figure 3. Experimentally Assigned Envelope with Offered Subsidy Rates

(a) Decreasing



Additional eVoucher Subsidy 2022/23

Information Sheet for the Household

Name Surname

HHID:

Village:

Extension agent:

Province: District:

Administrative post: Locality:

As part of the **PROMOVE-Agribiz** program, this agricultural season FAO is once again implementing a subsidy through copayment vouchers (**eVouchers**) for the purchase of agricultural inputs.

Your household has been selected to participate in a unique subsidy program that may result in additional benefits compared to the regular eVoucher subsidy. You will have the opportunity to choose a subsidy within a menu that has three (3) options. Your participation and the three options have been determined at random and you may have the possibility of a higher subsidy than the normal eVoucher.

It's important to note that in any of the packages your household will pay a part of the total price. So, the total price is made up of a portion subsidized by the FAO and a portion paid by your household.

We would like to inform you that the Additional Subsidy is also a way of thanking some of the participants in the impact evaluation surveys, which have been running throughout the eVoucher program. Only 16 of the households in your village were randomly selected to participate in this unique program. For each household, our team has randomly pre-selected the subsidy packages to be offered, which are different from those offered last season. Your household has been selected to participate based on the list below.

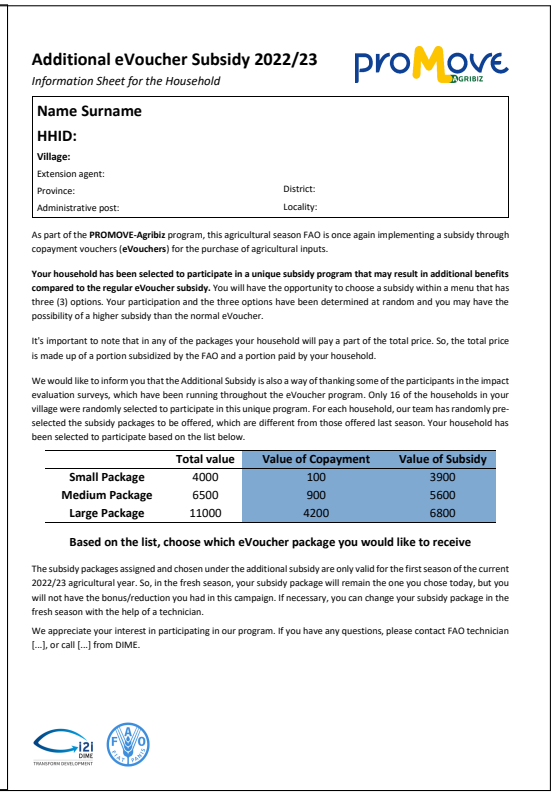
	Total value	Value of Copayment	Value of Subsidy
Small Package	4000	400	3600
Medium Package	6500	1200	5300
Large Package	11000	4200	6800

Based on the list, choose which eVoucher package you would like to receive

The subsidy packages assigned and chosen under the additional subsidy are only valid for the first season of the current 2022/23 agricultural year. So, in the fresh season, your subsidy package will remain the one you chose today, but you will not have the bonus/reduction you had in this campaign. If necessary, you can change your subsidy package in the fresh season with the help of a technician.

We appreciate your interest in participating in our program. If you have any questions, please contact FAO technician [...], or call [...] from DIME.

(b) Sharply Decreasing



Additional eVoucher Subsidy 2022/23

Information Sheet for the Household

Name Surname

HHID:

Village:

Extension agent:

Province: District:

Administrative post: Locality:

As part of the **PROMOVE-Agribiz** program, this agricultural season FAO is once again implementing a subsidy through copayment vouchers (**eVouchers**) for the purchase of agricultural inputs.

Your household has been selected to participate in a unique subsidy program that may result in additional benefits compared to the regular eVoucher subsidy. You will have the opportunity to choose a subsidy within a menu that has three (3) options. Your participation and the three options have been determined at random and you may have the possibility of a higher subsidy than the normal eVoucher.

It's important to note that in any of the packages your household will pay a part of the total price. So, the total price is made up of a portion subsidized by the FAO and a portion paid by your household.

We would like to inform you that the Additional Subsidy is also a way of thanking some of the participants in the impact evaluation surveys, which have been running throughout the eVoucher program. Only 16 of the households in your village were randomly selected to participate in this unique program. For each household, our team has randomly pre-selected the subsidy packages to be offered, which are different from those offered last season. Your household has been selected to participate based on the list below.

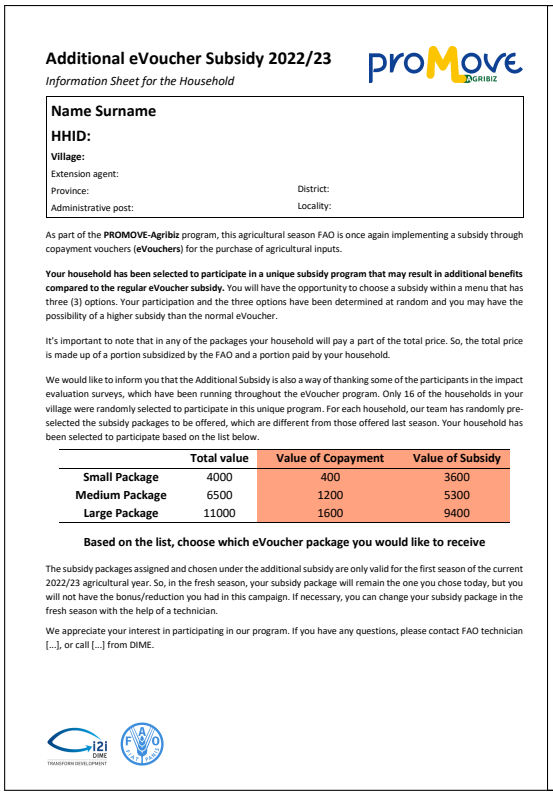
	Total value	Value of Copayment	Value of Subsidy
Small Package	4000	100	3900
Medium Package	6500	900	5600
Large Package	11000	4200	6800

Based on the list, choose which eVoucher package you would like to receive

The subsidy packages assigned and chosen under the additional subsidy are only valid for the first season of the current 2022/23 agricultural year. So, in the fresh season, your subsidy package will remain the one you chose today, but you will not have the bonus/reduction you had in this campaign. If necessary, you can change your subsidy package in the fresh season with the help of a technician.

We appreciate your interest in participating in our program. If you have any questions, please contact FAO technician [...], or call [...] from DIME.

(c) Uniform



Additional eVoucher Subsidy 2022/23

Information Sheet for the Household

Name Surname

HHID:

Village:

Extension agent:

Province: District:

Administrative post: Locality:

As part of the **PROMOVE-Agribiz** program, this agricultural season FAO is once again implementing a subsidy through copayment vouchers (**eVouchers**) for the purchase of agricultural inputs.

Your household has been selected to participate in a unique subsidy program that may result in additional benefits compared to the regular eVoucher subsidy. You will have the opportunity to choose a subsidy within a menu that has three (3) options. Your participation and the three options have been determined at random and you may have the possibility of a higher subsidy than the normal eVoucher.

It's important to note that in any of the packages your household will pay a part of the total price. So, the total price is made up of a portion subsidized by the FAO and a portion paid by your household.

We would like to inform you that the Additional Subsidy is also a way of thanking some of the participants in the impact evaluation surveys, which have been running throughout the eVoucher program. Only 16 of the households in your village were randomly selected to participate in this unique program. For each household, our team has randomly pre-selected the subsidy packages to be offered, which are different from those offered last season. Your household has been selected to participate based on the list below.

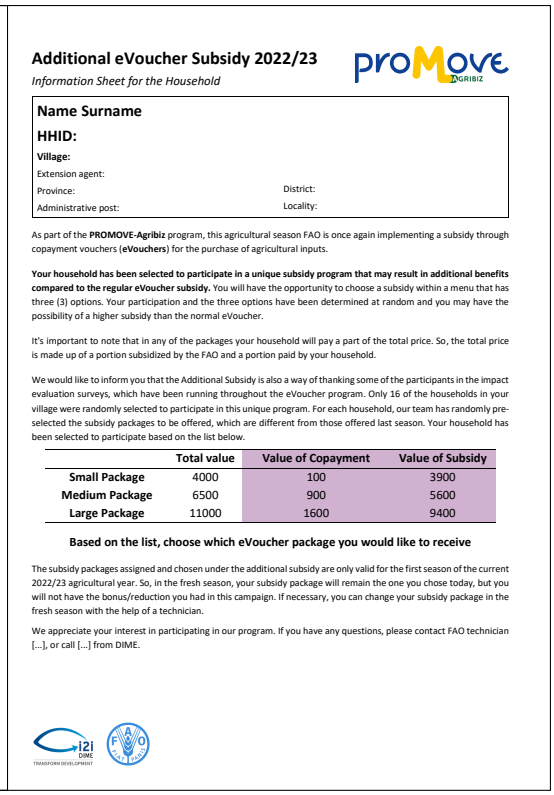
	Total value	Value of Copayment	Value of Subsidy
Small Package	4000	400	3600
Medium Package	6500	1200	5300
Large Package	11000	1600	9400

Based on the list, choose which eVoucher package you would like to receive

The subsidy packages assigned and chosen under the additional subsidy are only valid for the first season of the current 2022/23 agricultural year. So, in the fresh season, your subsidy package will remain the one you chose today, but you will not have the bonus/reduction you had in this campaign. If necessary, you can change your subsidy package in the fresh season with the help of a technician.

We appreciate your interest in participating in our program. If you have any questions, please contact FAO technician [...], or call [...] from DIME.

(d) High & Decreasing



Additional eVoucher Subsidy 2022/23

Information Sheet for the Household

Name Surname

HHID:

Village:

Extension agent:

Province: District:

Administrative post: Locality:

As part of the **PROMOVE-Agribiz** program, this agricultural season FAO is once again implementing a subsidy through copayment vouchers (**eVouchers**) for the purchase of agricultural inputs.

Your household has been selected to participate in a unique subsidy program that may result in additional benefits compared to the regular eVoucher subsidy. You will have the opportunity to choose a subsidy within a menu that has three (3) options. Your participation and the three options have been determined at random and you may have the possibility of a higher subsidy than the normal eVoucher.

It's important to note that in any of the packages your household will pay a part of the total price. So, the total price is made up of a portion subsidized by the FAO and a portion paid by your household.

We would like to inform you that the Additional Subsidy is also a way of thanking some of the participants in the impact evaluation surveys, which have been running throughout the eVoucher program. Only 16 of the households in your village were randomly selected to participate in this unique program. For each household, our team has randomly pre-selected the subsidy packages to be offered, which are different from those offered last season. Your household has been selected to participate based on the list below.

	Total value	Value of Copayment	Value of Subsidy
Small Package	4000	100	3900
Medium Package	6500	900	5600
Large Package	11000	1600	9400

Based on the list, choose which eVoucher package you would like to receive

The subsidy packages assigned and chosen under the additional subsidy are only valid for the first season of the current 2022/23 agricultural year. So, in the fresh season, your subsidy package will remain the one you chose today, but you will not have the bonus/reduction you had in this campaign. If necessary, you can change your subsidy package in the fresh season with the help of a technician.

We appreciate your interest in participating in our program. If you have any questions, please contact FAO technician [...], or call [...] from DIME.

II. RESULTS

A. Empirical Strategy

We estimate the impact of the experimentally assigned agricultural input subsidy schedules described in Section I.E. As subsidy schedules were randomized across farmers stratified by community, we estimate impacts conditional on community fixed effects. For precision, we also control for the baseline (2021 primary season) value of the outcome when it is available.

We restrict our analysis sample to registered farmers. As the experimental assignment was only revealed to farmers and extension agents in the later stage of the registration process (Section I.D), registration should not be affected by assignment to treatment. We present evidence that registration is not endogenous, and this restriction does not affect internal validity, in Section II.B.

We estimate impacts relative to the reference Decreasing subsidy schedule in the following specification

$$Y_{i,t} = \beta^{\text{SM}} \text{Sharply Decreasing}_i + \beta^{\text{L}} \text{Uniform}_i + \beta^{\text{SML}} \text{High \& Decreasing}_i + \alpha_{c(i)} + \delta Y_{i,\text{BL}} + \varepsilon_{i,t} \quad (1)$$

where $Y_{i,t}$ is the outcome of interest of farmer i in period t. $\alpha_{c(i)}$ are community fixed effects, and $Y_{i,\text{BL}}$ is the baseline value of the outcome. Our analysis focuses on the impacts of Sharply Decreasing (β^{SM}), Uniform (β^{L}) and High & Decreasing (β^{SML}), relative to the Decreasing reference group. As the assignment probabilities for each subsidy schedule were identical across randomization strata, linear regression recovers unbiased estimates of treatment effects (Goldsmith-Pinkham et al., 2024). We follow De Chaisemartin and Ramirez-Cuellar (2024) and cluster standard errors at the farmer level.

B. Experimental Validity

We perform three tests of experimental validity: no differential attrition, no differential sampling, and in-sample balance. First, we find no evidence of differential attrition in Appendix Table D2. Attrition is low at only 2% in our analysis sample. Second, as our analysis sample conditions on registered farmers, we test for and find no evidence of differential registration in Appendix Table D3. This is consistent with the implementation of the experiment, as farmers and extension agents did not learn farmers' assigned subsidy schedule until immediately after registration.[13] Third, we consistently fail to

[13] Average non-registration rates were relatively low, at only 17% of our baseline sample and 14% of our non-attrited sample.

reject that household characteristics are balanced at baseline with respect to assigned subsidy schedule in Appendix Table D4.[14] Across 66 tests, we reject balance at the 10 percent level for only 5 tests, as one would expect under random assignment. Three of the test rejections are on balance in asset index; in Appendix Table E3, we show that our main results are robust to controlling for asset index.

C. Input Demand ("First Stage")

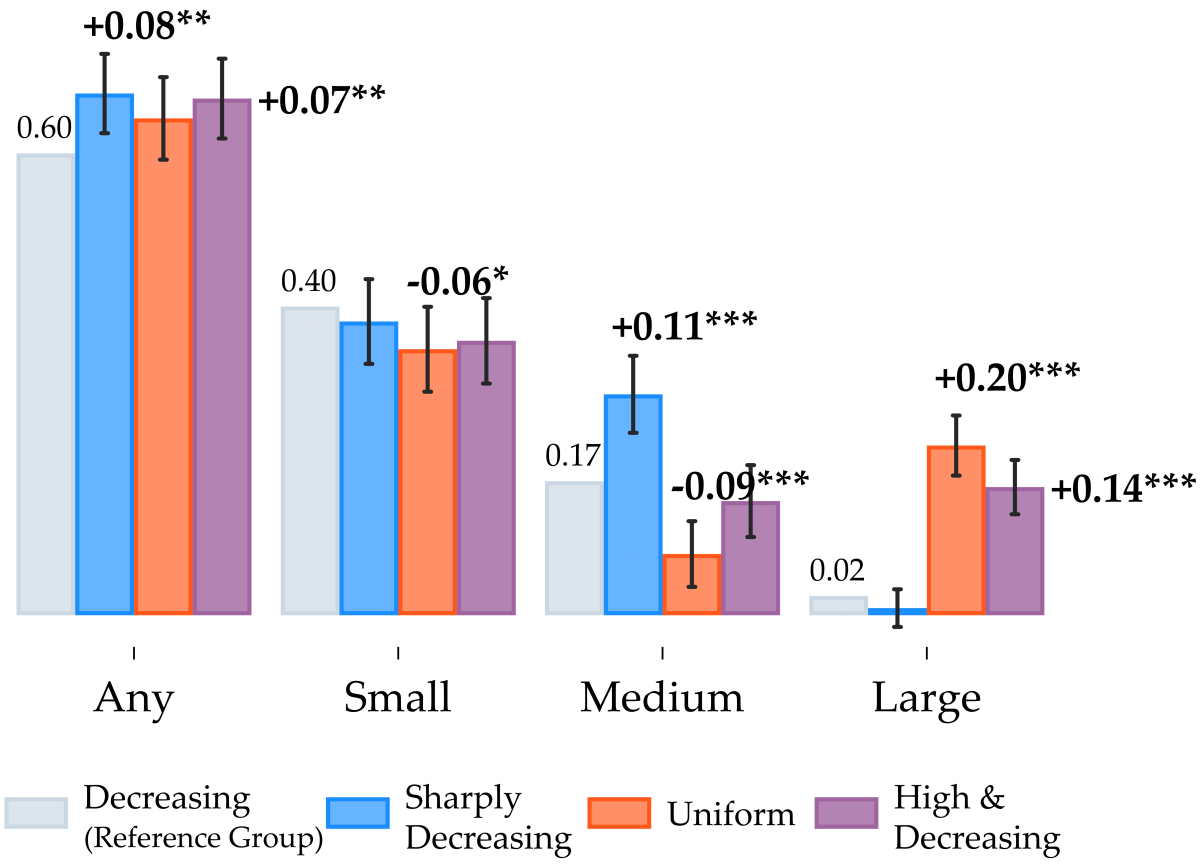
We begin by describing input choices under the reference Decreasing subsidy schedule: take-up is high, and farmers take advantage of the flexibility over both the value and types of inputs. First, most farmers use the eVoucher to purchase inputs: 60% of registered farmers "redeem." This is substantially higher than the 41% redemption rate observed in Carter et al. (2021) in Mozambique and substantially lower than 95% redemption rate observed in Kumar et al. (2026) in Malawi; both programs offered a input vouchers for fixed input quantities at prices in between Medium and Large under our Decreasing arm. Second, farmers' choice of package responds strongly to the decreasing subsididy schedule with higher prices of larger quantities of inputs: 40% redeem Small and 17% redeem Medium, while just 3% redeem Large. Paying 400 MZN for 4,000 MZN of inputs (Small under Decreasing) is desirable for a majority of farmers, and the availability of this option may be responsible for high take-up. In contrast, paying 3,000 MZN for an additional 4,500 MZN of inputs (shifting from Medium to Large under Decreasing) appears prohibitive for most farmers. Third, farmers redeem a mix of inputs. 52% is used for improved seeds, 14% is used for inorganic fertilizer, and 33% is used for tools. In Appendix Figure D4, we show that the average value redeemed towards tools is relatively constant across package sizes, while the average value redeemed towards inorganic fertilizer increases sharply across package sizes.

Next, we present the impacts of additional subsidies on input demand, in both Figure 4 and Table 2.

Intuitively, additional subsidies increase farmers' demand for any package. This increased demand is driven by increased demand for additionally subsidized quantities, which also induces substitution away from adjacent quantities. First, Uniform (higher subsidy for Large) increases redemption of Large by 19.5pp. Substitution is primarily from Medium (-9.4pp), with weaker evidence of substitution from Small or non-redemption. Second, Sharply Decreasing (higher subsidies for Small and Medium) increases redemption of Small and Medium by 9.4pp. Substitution is primarily from non-redemption (-7.8pp), as few farmers redeem Large under Decreasing. Third, under High

[14] We also test for balance in the full baseline sample in Appendix Table E2, with qualitatively identical results.

Figure 4. First Stage: Package Take-up Across Subsidy Schedules



& Decreasing (higher subsidies for Small, Medium, and Large), additional subsidies for smaller and larger input quantities substitute, with partially offsetting effects. Relative to Sharply Decreasing, High & Decreasing (higher subsidy for Large) increases redemption of Large by 15.7pp, by inducing substitution away from Small and Medium (−16.4pp). In contrast, relative to Uniform, High & Decreasing (higher subsidies for Small and Medium) increases redemption of Small and Medium by 7.9pp, by inducing substitution away from Large (−5.4pp). As a result, the impacts of High & Decreasing on redemption of smaller and larger input quantities are in between those of Sharply Decreasing and Uniform.

Our estimates of input demand imply that increasing subsidies for smaller input quantities can have non-monotonic impacts on input demand. Most strikingly, relative to Uniform, High & Decreasing decreases redemption of Large. Additional subsidies for smaller quantities impact choices through both an "inclusion" margin, wherein farmers are induced to shift from non-redemption to Small or Medium, and a "diversion" margin, wherein farmers are induced to shift from Large to Small or Medium (Mountjoy, 2022).

When we estimate productive impacts in Section II.D, we interpret productive impacts

Table 2. First Stage on Package Choice: Inclusion, Intensification, and Diversion

	Package type			
	Any	Small	Medium	Large
	(1)	(2)	(3)	(4)
Sharply Decreasing	0.078	-0.019	0.113	-0.016
	(0.031)	(0.033)	(0.030)	(0.015)
	[0.013]	[0.560]	[0.000]	[0.291]
Uniform	0.045	-0.055	-0.094	0.195
	(0.032)	(0.033)	(0.026)	(0.024)
	[0.162]	[0.097]	[0.000]	[0.000]
High & Decreasing	0.071	-0.044	-0.026	0.141
	(0.031)	(0.034)	(0.028)	(0.021)
	[0.024]	[0.186]	[0.362]	[0.000]
Sample mean of the reference group	0.599	0.401	0.174	0.025
Community fixed effects	✓	✓	✓	✓
Number of observations	1,254	1,254	1,254	1,254
Number of farmers	1,254	1,254	1,254	1,254

Notes: Unit of observation: household. Data are from administrative eVoucher records. Sample: households interviewed at follow-up and registered for an eVoucher. All regressions are least squares with strata (i.e., community) fixed effects as in Equation 1. Standard errors clustered at the household ('farmer') level in parentheses; p-values in brackets. Appendix Table D1 replicates the estimates on the full sample of registered households (i.e., regardless of survey status) and on the sample of registered households interviewed at baseline (i.e., regardless of being interviewed again at follow-up).

(or lack thereof) as attributable to changes in input use and any complementary reoptimization. In Appendix Table D5, we find no impacts of additional subsidies on resale, storage, or non-eVoucher input value (which is less than 10% of eVoucher input value); impacts on input redemption therefore correspond to impacts on input use.

What inputs do farmers redeem? We estimate impacts on total input value and by category in Table 3.

All three additional subsidies increase use of improved seeds, while only additional subsidies for Large increase use of fertilizer.[15] Impacts are qualitatively large: additional subsidies increase improved seed redeemed value by 38%–49%, while additional subsidies for Large increase fertilizer redeemed value by 73%–89%.[16] We present one possible

[15] Input value is significantly larger under Uniform and High & Decreasing than under Sharply Decreasing, while input values under Uniform and High & Decreasing are not statistically significantly different from one another.

[16] In Appendix Table E4, we show that proportional impacts are much smaller on the extensive margin. Extensive margin impacts on improved seeds and tools are almost identical to impacts on any redemption,

explanation of the differences in impacts of additional subsidies for smaller and larger quantities in Section II.E: farmers who use practices complementary to fertilizer (row planting) redeem fertilizer as a larger fraction of their inputs redeemed, and are more likely to redeem Large.

Agricultural input subsidies are often regressive, as farmers who are induced to purchase inputs, or more inputs, are on average wealthier and more productive than farmers who are not (Duflo et al., 2011). In Appendix Table E5, we confirm that farmers who redeem Large (and, to a lesser degree, Medium) appear much better off than farmers who redeem Small. They are 91% less likely to be female headed, 0.7 standard deviations wealthier, have 1.3 additional household members, cultivate 59% more land, and have 83% higher production. These differences imply that additional subsidies for larger packages are likely to be much more regressive than additional subsidies for smaller packages. We consider the implications of this for optimal agricultural input subsidies in Section III.

as almost all farmers redeem tools and improved seeds. Additional subsidies for Large increase probability of fertilizer redemption by 27%–51%, which suggests there may be both intensive and extensive margin effects on fertilizer use.

Table 3. First Stage on Inputs: Subsidizing either package increases improved seed use, subsidizing larger packages increases fertilizer use

	Input category				
	Any	*Improved seeds*	*Inorganic fertilizer*	*Pesticides*	*Tools*
	(1)	(2)	(3)	(4)	(5)
Sharply Decreasing	679	646	4	42	-13
	(262)	(171)	(118)	(12)	(92)
	[0.010]	[0.000]	[0.970]	[0.001]	[0.889]
Uniform	1,414	843	342	18	210
	(324)	(231)	(139)	(9)	(94)
	[0.000]	[0.000]	[0.014]	[0.047]	[0.025]
High & Decreasing	1,246	773	419	23	31
	(266)	(168)	(134)	(9)	(94)
	[0.000]	[0.000]	[0.002]	[0.013]	[0.742]
Sample mean of the reference group	3,279	1,713	466	16	1,083
Community fixed effects	✓	✓	✓	✓	✓
Number of observations	1,254	1,254	1,254	1,254	1,254
Number of farmers	1,254	1,254	1,254	1,254	1,254

Notes: Unit of observation: household. Data are from administrative eVoucher records. Sample: households interviewed at follow-up and registered for an eVoucher. All regressions are least squares with strata (i.e., community) fixed effects as in Equation 1. Standard errors clustered at the household ('farmer') level in parentheses; *p*-values in brackets. Outcomes, i.e., input values by the column header category, are in nominal Mozambican *meticais* (100 MZN $\approx$ 1.5 USD). Appendix Table D6 replicates the estimates on the full sample of registered households (i.e., regardless of survey status) and on the sample of registered households interviewed at baseline (i.e., regardless of being interviewed again at follow-up).

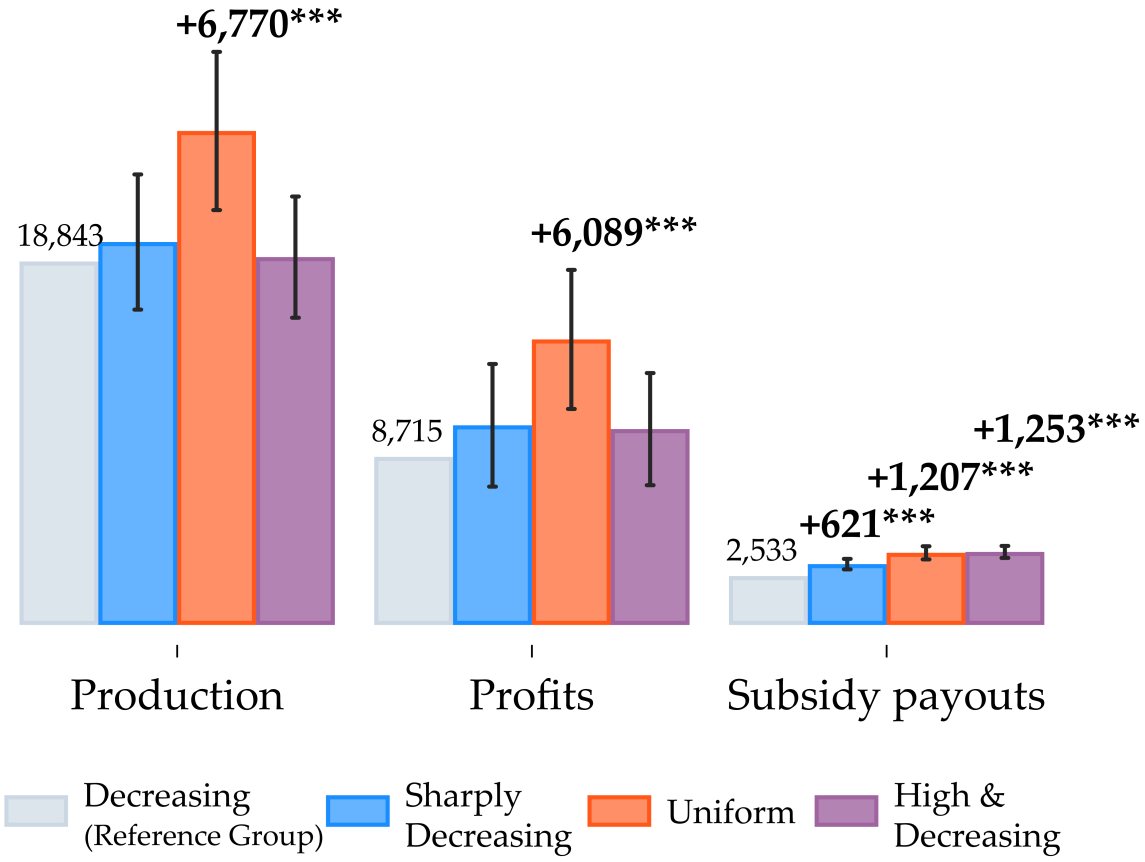
D. Productive Impacts ("Reduced Form")

We present the impacts of additional subsidies on agricultural production in Figure 5 and Table 4.

Our first finding is that Uniform (higher subsidy for Large) increases agricultural production by 36% (6,770 MZN).[17] This impact on production is almost five times larger than the impact on input value, suggesting high marginal returns to agricultural inputs for farmers induced to choose Large. We benchmark this production effect by conducting a complier analysis. Our estimates imply farmers who increase input use under Uniform relative to Decreasing increase their agricultural production from 33,786 MZN to 68,503

[17] In Appendix Tables E6 and E7, we decompose this effect. Uniform significantly increases both consumption and sales of agricultural production, and significantly increases production of cash crops and oilseeds, legumes and pulses, and grains.

Figure 5. Impacts on Main Outcomes



MZN, a 103% increase.[18] This effect is not statistically distinguishable from the 80% increase estimated by Carter et al. (2021) for a similarly sized input package.

In contrast, we find almost no significant impacts of Sharply Decreasing (higher subsidies for Small and Medium) and High & Decreasing (higher subsidies for Small, Medium, and Large) on agricultural production. The lack of impacts of Sharply Decreasing on production suggests that Sharply Decreasing-compliers may be less marginally productive than Uniform-compliers. It is perhaps more surprising that High & Decreasing did not increase production, given the large productive impacts of Uniform. This suggests that the "diverted" farmers, who would have taken up Large under Uniform but instead take up Small or Medium under High & Decreasing, were particularly marginally productive. As a consequence, the additional subsidies for smaller and larger quantities we test appear to substitute in optimal policy: under High & Decreasing, additional subsidies for smaller quantities erode the productive impacts of additional subsidies for larger quantities.

[18] We report estimates of average outcomes under Decreasing for Uniform-"compliers" in Appendix Table D7.

Impacts on production are large relative to impacts on inputs and labor, and impacts on production and profits are therefore similar regardless of how we calculate profits.[19,20,21] Uniform increases profits net of input value by 4,886 MZN, over four times larger than impacts on subsidy payouts (1,207 MZN) and three times larger than impacts on input value (1,414 MZN). Impacts of Sharply Decreasing and High & Decreasing on profits remain small and statistically insignificant.

We highlight three qualitative interpretations of these findings, which we quantitatively test in sequence. We derive the three tests in Appendix B.3. First, the large impacts of Uniform on profits appear inconsistent with profit maximization. Second, farmers who increase their input use in response to Uniform appear to be more marginally productive than other farmers. Third, this heterogeneity in marginal productivity suggests heterogeneity in the strength of deviations from profit maximization ("wedges").

[19] Our preferred construction of profits subtracts non-eVoucher expenditures on variable inputs, tools and land rental, hired labor, and household labor valued at 60% of the median observed wage (following the rule-of-thumb proposed by Agness et al., 2025). In Appendix Table E8, we show profit impacts are robust to alternative valuations of labor.

[20] Duflo et al. (2008) and Carter et al. (2021) also find limited impacts of fertilizer use on labor. Beaman et al. (2013) strikingly find that fertilizer use causes large increases in labor for rice farmers who broadcast seed, rather than row planting it; one possible explanation is that the returns to fertilizer are low absent row planting (we present evidence row planting is an important determinant of fertilizer adoption in Section II.E), but row planting requires substantial skilled labor (Cefala et al., 2025).

[21] Land tenure in our setting is predominantly informal, so farmers could potentially increase their cultivated area without purchasing or renting land. In Appendix Table E7, we do not find any significant impacts on total area cultivated.

Table 4. Impacts on Main Outcomes

		Profits				
	Production value	*Gross of inputs*	*Net of copay at realized prices*	*Net of copay at reference group prices*	*Net of input value*	Subsidy payout
	(1)	(2)	(3)	(4)	(5)	
Sharply Decreasing	1,012	1,458	1,635	1,441	1,029	621
	(2,124)	(1,929)	(1,927)	(1,929)	(1,938)	(165)
	[0.634]	[0.450]	[0.396]	[0.455]	[0.595]	[0.000]
Uniform	6,770	6,157	6,089	5,512	4,886	1,207
	(2,486)	(2,182)	(2,186)	(2,184)	(2,191)	(207)
	[0.007]	[0.005]	[0.005]	[0.012]	[0.026]	[0.000]
High & Decreasing	229	1,279	1,439	855	203	1,253
	(1,905)	(1,769)	(1,762)	(1,760)	(1,762)	(189)
	[0.905]	[0.470]	[0.414]	[0.627]	[0.908]	[0.000]
Over-identification test						
Hansen J statistic	6.9	4.8	4.4	4.6	4.7	
p-value	0.032	0.090	0.111	0.100	0.097	
Sample mean of the reference group	18,843	9,188	8,715	8,715	6,182	2,533
Community fixed effects	✓	✓	✓	✓	✓	✓
Number of observations	1,254	1,254	1,254	1,254	1,254	1,254
Number of farmers	1,254	1,254	1,254	1,254	1,254	1,254

Notes: Unit of observation: household. Data on 'Production value' and 'Profits' are from our household surveys; 'Subsidy payout' and 'input value' are based on administrative eVoucher records. Sample: households interviewed at follow-up and registered for an eVoucher. All regressions are least squares with strata (i.e., community) fixed effects—and controlling linearly for the baseline value of the dependent variable in Columns 1 and 2—as in Equation 1; if the latter variable is missing, we impute it by taking the sample mean in the estimation sample and further include a binary indicator that the variable is missing. Standard errors clustered at the household ('farmer') level in parentheses; *p*-values in brackets. Outcomes are in nominal Mozambican *meticais* (100 MZN $\approx$ 1.5 USD). 'Profits' are defined as the sum of agricultural production value (i.e., the value of harvest regardless of whether it was consumed, sold, or stored using median prices in the whole study sample) during the 2023 primary season minus expenses on improved seeds, inorganic fertilizer, pesticides, tools and land rental, hired labor, and household labor (priced at 60% of the median wage across casual labor days). The over-identification tests are implemented with the three random treatments as excluded instruments and input value as endogenous variable.

Do Farmers Profit Maximize? Under profit maximization, any change to subsidy rates from Decreasing should reduce profits when inputs are priced at Decreasing subsidy rates. We strongly reject profit maximization for Uniform: Uniform increases profits when inputs are priced at Decreasing subsidy rates by 5,512 MZN (Table 4, Column 4). To quantitatively interpret this rejection, we estimate bounds on the "wedge" between farmers' willingness to pay for inputs and their impacts on profits. Under profit maximization, willingness to pay is equal to impacts on profits, and wedges are zero. Generically, complier average wedges-plus-one are bounded between the ratios of impacts on profits gross of inputs (Column 2) to impacts on input expenditures valued at either reference prices (Column 4 minus Column 2) or comparison prices (Column 4 minus Column 3). The bounds follow from revealed preference: for example, Uniform-compliers' willingness to pay for the additional inputs is at most their price under Decreasing, which they decline, and at least their price under Uniform, which they accept. Our estimates imply a lower bound on the average wedge for Uniform-compliers of 8.[22] This lower bound comes with economically meaningful uncertainty; we cannot reject a value of 1.5, our preferred calibration in our welfare analysis in Section III.A.2.

Are Marginal Products of Inputs Heterogeneous? Our results could be explained by increases in production value proportional to increases in input value, plus statistical noise. We test for heterogeneity in marginal products by using our three experimental arms as instruments for input value and running an over-identification test, with either agricultural production or profits gross of inputs as an outcome. As each arm shifts input use for a different set of farmers, an over-identification test will reject if the farmers changing their input use vary in their marginal products across arms.[23] Results are shown in Table 4: we reject the null of homogeneous marginal products for both production ($p = 0.032$, Column 1) and gross profits ($p = 0.090$, Column 2).

Are Deviations from Profit Maximization Heterogeneous? One potential explanation of heterogeneity in marginal products of inputs is that marginal prices are heterogeneous under Decreasing; the incremental subsidy rate falls from 90% for Small to 33% for moving from Medium to Large. We test whether the distortion introduced by nonlinear pricing can fully explain heterogeneity in marginal products of inputs by testing for homogeneous wedges. We implement one-sided tests of whether the lower bound on

[22] This lower bound is the ratio of the impact of Uniform on profits gross of inputs (6,157 MZN, Table 4, Column 2) to the impact of Uniform on input expenditures valued at reference group prices (645 MZN, Table 4, Column 4 minus Column 2), minus one.

[23] Formally, we test for heterogeneity in a weighted average of complier marginal products (Angrist et al., 2000), which may include negative weights.

complier average wedges for Uniform is above the upper bound on complier average wedges for Sharply Decreasing and High & Decreasing; we reject the null of homogeneous wedges for High & Decreasing ($p = 0.011$, not reported elsewhere), but fail to reject for Sharply Decreasing ($p = 0.383$, not reported elsewhere) albeit with less power.

This last finding implies that, at least among farmers who shift their input use under High & Decreasing relative to Uniform, the farmers using more inputs also relatively "undervalue" them. This is perhaps surprising. It conflicts with the intuition that farmers using more inputs are richer, and should therefore be less financially constrained and closer to profit maximization. However, farmers using more inputs are also more productive, which may simultaneously cause financial constraints to bind more tightly. Concretely, a farmer who can borrow 1,000 MZN and optimally uses 1,000 MZN of inputs will be less marginally productive than a farmer who can borrow 2,000 MZN and optimally uses 3,000 MZN of inputs.

We present complementary evidence of larger productive impacts among more productive farmers by estimating quantile treatment effects on profits. In Appendix Figure D5, we find the largest impacts of Uniform on profits in the higher quantiles of the profit distribution.

E. Is There Heterogeneity in Average Productivity?

One possible interpretation of our results is that we should distribute a large quantity of inputs to all farmers to maximize efficiency, as in many input subsidy programs in Africa. This would be the case if all heterogeneity in input demand across farmers is attributable to distortions. Our results above do not reject this possibility, as a higher marginal product for farmers with more input use could be attributed to increasing returns to scale.

While we do not directly test the impacts of free distribution of a large quantity of inputs, we present evidence that there is meaningful heterogeneity in productivity that shapes input demand, which would imply that free distribution (absent resale) is inefficient. Specifically, we present evidence that there are large differences in input demand and productive impacts between farmers who do and do not row plant. Row planting is an agricultural practice that is strongly complementary to the use of inorganic fertilizers, but difficult to learn (Cefala et al., 2025, 2026). Moreover, inorganic fertilizer is complementary to the use of improved seeds (Suri, 2011). In other words, adoption of row planting transforms the agricultural technology in a manner that increases optimal use of improved seeds and inorganic fertilizer.[24]

[24] One might be inclined to apply this evidence to suggest targeted distribution of inputs to row-planting

We estimate the impacts of additional subsidies on input use and production, split between farmers who do and do not row plant, in Table 5. Under assignment to Decreasing, these two groups of farmers appear very different: row planters use 50% more inputs, 215% more fertilizer, produce 46% more output, and earn 53% higher profits. While additional subsidies increase input and improved seed use for both row planters and non-row planters, they increase fertilizer use and agricultural production only for row planters. Given existing evidence that non-row-planting farmers do not experience large increases in profits from fertilizer use (Beaman et al., 2013), these results are consistent with productive heterogeneity shaping input demand.

farmers could be an optimal policy. However, targeting on row planting ability is likely infeasible in practice. Evidence of one unobservable (to the policymaker) determinant of productivity and input demand also suggests the existence of others (e.g., land quality, cultivated area).

Table 5. Impacts by Row Planting

	Input values			Main outcomes		
	Any	Improved seeds	Inorganic fertilizer	Production value	Profits	Subsidy payout
	(1)	(2)	(3)	(4)	(5)	(6)
Farmers not row planting						
Sharply Decreasing	1,043	818	78	-1,240	-912	369
	(533)	(335)	(189)	(3,173)	(2,912)	(297)
	[0.051]	[0.015]	[0.678]	[0.696]	[0.754]	[0.215]
Uniform	484	278	-37	2,903	2,853	359
	(435)	(280)	(157)	(3,009)	(2,799)	(316)
	[0.267]	[0.323]	[0.814]	[0.335]	[0.309]	[0.256]
High & Decreasing	866	644	114	-1,575	921	828
	(416)	(278)	(142)	(2,654)	(2,517)	(320)
	[0.038]	[0.021]	[0.421]	[0.553]	[0.715]	[0.010]
Sample mean of the reference group	2,455	1,366	176	14,239	6,124	2,090
Community fixed effects	✓	✓	✓	✓	✓	✓
Number of observations	486	486	486	486	486	486
Number of farmers	486	486	486	486	486	486
Farmers row planting						
Sharply Decreasing	513	563	5	2,296	3,531	642
	(357)	(236)	(191)	(3,469)	(3,042)	(235)
	[0.151]	[0.017]	[0.980]	[0.508]	[0.246]	[0.007]
Uniform	2,088	1,209	601	9,777	8,979	1,708
	(497)	(368)	(226)	(3,957)	(3,357)	(301)
	[0.000]	[0.001]	[0.008]	[0.014]	[0.008]	[0.000]
High & Decreasing	1,589	795	720	1,118	2,511	1,552
	(385)	(246)	(227)	(2,956)	(2,693)	(269)
	[0.000]	[0.001]	[0.002]	[0.705]	[0.352]	[0.000]
Sample mean of the reference group	3,680	1,927	556	20,722	9,383	2,758
Community fixed effects	✓	✓	✓	✓	✓	✓
Number of observations	726	726	726	726	726	726
Number of farmers	726	726	726	726	726	726

Notes: Unit of observation: household. Data on 'input values' and 'Subsidy payouts' are based on administrative eVoucher records; data on 'row planting', 'Production value', and 'Profits' are from our household surveys. Sample: households interviewed at follow-up and registered for an eVoucher. Subsample in the bold panel header. All regressions are least squares with strata (i.e., community) fixed effects—and controlling linearly for the baseline value of the dependent variable in Columns 4 and 5—as in Equation 1; if the latter variable is missing, we impute it by taking the sample mean in the estimation sample and further include a binary indicator that the variable is missing. Standard errors clustered at the household ('farmer') level in parentheses; p-values in brackets. Outcomes are in nominal Mozambican *meticais* (100 MZN $\approx$ 1.5 USD). 'Profits' are defined as the sum of agricultural production value (i.e., the value of harvest regardless of whether it was consumed, sold, or stored using median prices in the whole study sample) during the 2023 primary season minus expenses on improved seeds, inorganic fertilizer, pesticides, tools and land rental, hired labor, and household labor (priced at 60% of the median wage across casual labor days). First-stage estimates on package choice by row planting are in Appendix Table E9.

F. Why Is Marginal Productivity So High?

A key finding in Section II.D is that the large productive impacts of Uniform require a large wedge between willingness to pay and profits to rationalize; what explains this large wedge? We consider two prominent sources of wedges in input demand, without ruling out other potential explanations. First, farmers may underestimate the productive returns to inputs, and learn these returns from observing input use (Carter et al., 2021). Second, farmers may face credit, risk, or other financial constraints alleviated by wealth that cause them to discount the expected value of agricultural production at harvest relative to input expenditures at planting (Karlan et al., 2014). Our results above do not separate these distinct potential sources of wedges.

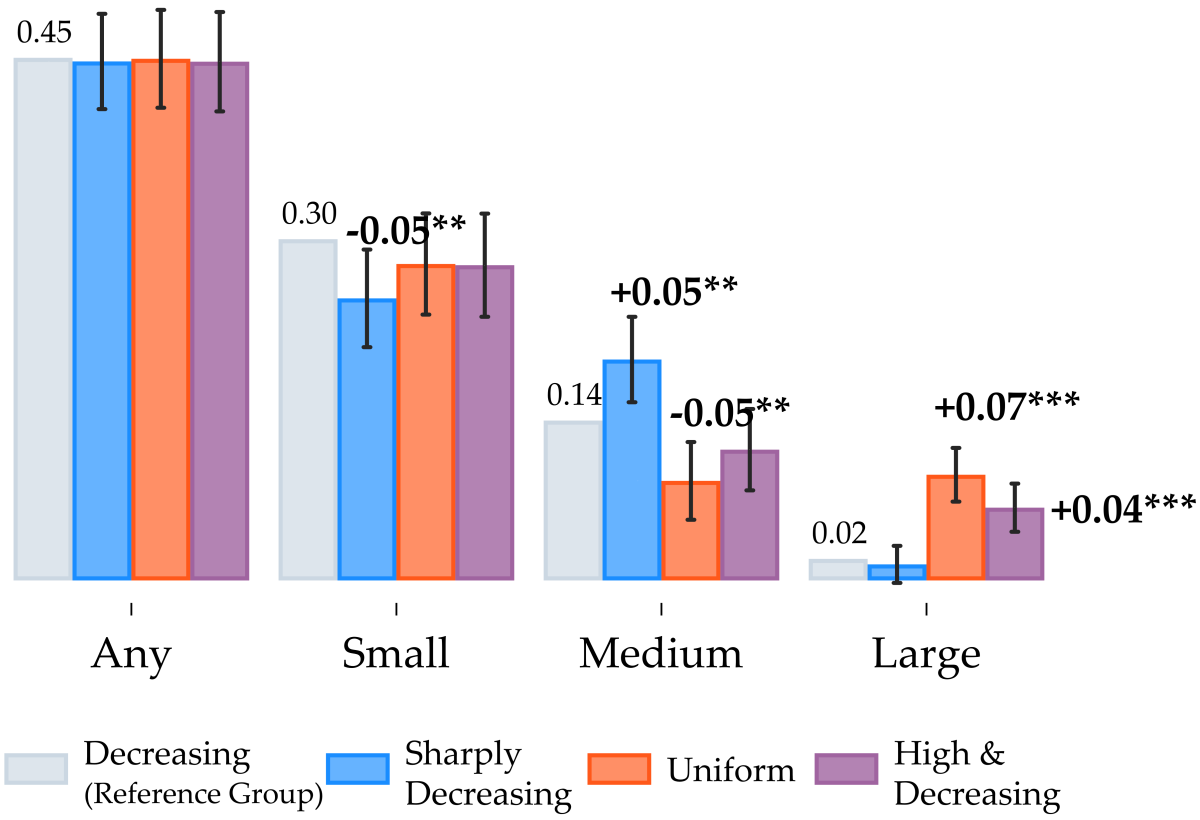
We present evidence below of learning and financial constraints from the impacts of additional subsidies on long run adoption after additional subsidies were phased out. First, we show that additional subsidies persistently increased adoption, consistent with learning. Second, we show that persistent impacts of additional subsidies on adoption are concentrated among richer farmers, suggesting financial constraints. These results imply that learning can partly, but not fully, explain wedges between willingness to pay and profits.

Results from our test of learning are shown in Figure 6. Additional subsidies continue to have significant impacts on adoption in the two years after they ended (Appendix Table E10). Effects are between one and two thirds as large as the short run effects. We conduct a simple back-of-envelope using these effects to calibrate the effect of Uniform on willingness to pay, which we attribute to learning. The long-run adoption impact of Uniform is 38% of the short-run adoption impact, while the price of Large is 163% higher under Decreasing and in the long run, suggesting willingness to pay increased by 62%.

We present suggestive evidence that financial constraints may also affect input demand in Table 6, by documenting heterogeneity in short-run and long-run adoption impacts of additional subsidies for larger quantities.[25] If additional subsidies overcome financial constraints, then more constrained farmers should see long-run adoption fall even if they learn. We use baseline wealth as a proxy for financial constraints; while wealth is likely to be correlated with other determinants of input demand, including row planting, sufficiently wealthy farmers are unlikely to face binding credit constraints when purchasing agricultural inputs. With this caveat, we find that the impacts of additional

[25] We pool Uniform and High & Decreasing, relative to a pooled reference group of Decreasing and Sharply Decreasing, to focus on the impacts of additional subsidies for Large on Large adoption for power and parsimony. Results are similar in a specification estimating effects separately for each experimental arm in Appendix Table E11.

Figure 6. Impacts on Long-Run Adoption



subsidies on long-run adoption all but vanish for poorer farmers. Predicted effects fall by 76% at the 25th percentile of wealth, but only 59% at the 75th percentile of wealth. These results suggest that even fully-informed farmers may not profit maximize when choosing inputs; we allow for this possibility in our welfare analysis in Section III.

III. QUANTITATIVE FRAMEWORK

The results above reveal an equity–efficiency trade-off. Increasing subsidy rates for large quantities of inputs under Uniform has the largest productive impacts. Increasing subsidy rates for smaller quantities of inputs directs subsidy payouts towards the relatively poorer farmers who redeem smaller quantities.

How do we get from these experimental results to the "right" price? To evaluate the trade-off between transfer progressivity and productive impacts, we calculate the marginal value of public funds ("MVPF," Hendren and Sprung-Keyser, 2020) of our three treatment arms. We do so in a quantitative framework where farmers do not fully

Table 6. Short- and Long-Run Impacts on Adoption by Wealth

	Package type			
	Any		Large	
	Short run	*Long run*	*Short run*	*Long run*
	(1)	(2)	(3)	(4)
Uniform or High & Decreasing	0.047	0.033	0.113	0.018
	(0.041)	(0.031)	(0.027)	(0.016)
	[0.259]	[0.300]	[0.000]	[0.273]
Asset index	0.005	0.017	0.006	0.001
	(0.009)	(0.007)	(0.005)	(0.003)
	[0.548]	[0.012]	[0.229]	[0.716]
Uniform or High & Decreasing × Asset index	-0.009	-0.012	0.020	0.014
	(0.012)	(0.009)	(0.009)	(0.005)
	[0.454]	[0.192]	[0.020]	[0.008]
Predicted effect on:				
25th percentile of Asset index	0.038	0.021	0.133	0.032
	(0.032)	(0.025)	(0.021)	(0.012)
	[0.238]	[0.401]	[0.000]	[0.010]
75th percentile of Asset index	0.002	-0.027	0.212	0.087
	(0.033)	(0.027)	(0.026)	(0.015)
	[0.946]	[0.312]	[0.000]	[0.000]
Sample mean of the comparison group	0.599	0.452	0.025	0.019
Community fixed effects	✓	✓	✓	✓
Number of observations	1,254	2,508	1,254	2,508
Number of farmers	1,254	1,254	1,254	1,254

Notes: Unit of observation: household. Data on package choice are from administrative eVoucher records; 'Asset index' is from our baseline household surveys. Sample: households interviewed at follow-up and registered for an eVoucher. All regressions are least squares with strata (i.e., community) fixed effects. Standard errors clustered at the household ('farmer') level in parentheses; p-values in brackets. Estimates pooling all waves and including a triple interaction with a 'Long run' indicator variable (equal to one for the 2024 and 2025 primary seasons, and zero for the 2023 primary season) are in Appendix Table E10. Estimates from a fully-saturated model, i.e., including the three treatments, are in Appendix Table E11.

internalize the productive impacts of agricultural inputs, formalized in Appendix B.1. We link the MVPF in this framework to sufficient statistics identified by our experiment in Section III.A. We analyze how MVPF, and in turn the optimal policy, vary as a function of planner preferences over transfer progressivity and productive efficiency in Section III.B.

A. Welfare Analysis (with Internalities) Meets Causal Inference

(Finkelstein and Hendren, 2020)

We consider the problem of a social planner choosing between the three treatment arms in our experiment, who values both equity and productive efficiency. The planner values equity in that they would prefer to transfer resources and increase agricultural production for poorer farmers relative to richer farmers. The planner also directly values productive efficiency because they believe that farmers underestimate or undervalue the productive impacts of inputs. These beliefs are consistent with our finding of very high returns to inputs from Uniform (Section II.D), and that temporary additional subsidies had persistent impacts on input use (Section II.F).

Given the planner's problem, we rank the optimality of the three treatment arms using the MVPF. The MVPF is simply the ratio of farmers' willingness to pay for a policy to the policy's cost to the government. Formally, the MVPF ranks policies such that the social planner prefers a budget neutral rebalancing from one policy towards a second if and only if the social-marginal-utility-weighted MVPF of the second policy is larger than that of the first (Hendren and Sprung-Keyser, 2020). Recent work extends the MVPF to allow for *externalities*: one simply adds any uninternalized social benefits from farmers' behavioral responses to farmers' private willingness to pay (Hahn et al., 2026). Our context instead features *internalities*: when farmers choose inputs, they do not internalize the underestimated fraction of their productive impacts.

We therefore extend the MVPF to allow for internalities by applying frameworks used to analyze optimal taxation with internalities (Allcott et al., 2019; Farhi and Gabaix, 2020). In Appendix B.1, we show that uninternalized productive impacts are weighed by the farmer's willingness to pay for agricultural production, and added to farmers' benefits. This approach yields an MVPF with four ingredients, which we present in Equation 2. First, household consumer surplus ΔWTP_i, that is their willingness to pay for the price changes. Second, impacts on household production, $\Delta\text{Production}_i$, times the planner's direct weight on productive efficiency γ. The sum of these two terms is the "benefits" from the policy. Third, the planner weighs benefits by household social marginal utility λ_i. Fourth, the planner calculates the MVPF as the ratio of social-marginal-utility-weighted benefits to the change in subsidy payouts, that is the costs of the policy to the government.

$$\text{MVPF} = \frac{\sum_i \lambda_i\text{-weighted}\left(\Delta\text{WTP}_i + \gamma \times \Delta\text{Production}_i\right)}{\sum_i \Delta\text{Costs to government}_i} \tag{2}$$

We estimate the MVPF using three sufficient statistics identified by our experiment, whose estimation we discuss in Section III.A.1. First, the social-marginal-utility-weighted

consumer surplus from treatment. Second, the social-marginal-utility-weighted treatment effect on agricultural production. And third, the treatment effect on subsidy payouts. In addition, the MVPF depends on two additional parameters that reflect the social planner's preferences, whose calibration we discuss in Section III.A.2. First, γ, the planner's direct weight on productive efficiency. And second, λ, the vector of social marginal utility weights themselves, which we parameterize below as a function of the planner's inequality aversion η and farmers' baseline wealth.

1. Sufficient Statistics for Marginal Value of Public Funds

Consumer Surplus from Treatment. We calculate nonparametric bounds on consumer surplus from each treatment using our experimental demand estimates.[26] We begin by constructing naive bounds: the lower bound is the change in consumer surplus if choices are fixed as under the reference group Decreasing, and the upper bound is the change in consumer surplus if choices were fixed as under treatment. For instance, for High & Decreasing, the naive lower bound is 300 MZN times the fraction adopting Small/Medium under Decreasing, plus 2,600 MZN times the fraction adopting Large under Decreasing. For comparison, the naive upper bound is 300 MZN times the fraction adopting Small/Medium under High & Decreasing, plus 2,600 MZN times the fraction adopting Large under High & Decreasing.

We are able to sharpen naive bounds on consumer surplus, leveraging two additional sources of information from our 2×2 experimental design. First, anyone who does not choose Large under High & Decreasing, including those who choose Large under Uniform, cannot gain more than 300 MZN of consumer surplus from High & Decreasing relative to Decreasing. We apply this to sharpen the upper bound on consumer surplus from Uniform relative to Decreasing. Second, anyone who chooses Large under Decreasing, including those who do not under Sharply Decreasing, gains at least 2,300 MZN of consumer surplus from Uniform or High & Decreasing relative to Sharply Decreasing. We apply this to sharpen the lower bound on consumer surplus from Uniform or High & Decreasing relative to Sharply Decreasing.

These bounds on consumer surplus are functions of unweighted choice probabilities. Bounds on social-marginal-utility-weighted consumer surplus use social-marginal-utility-weighted choice probabilities.

Treatment Effect on Agricultural Production. Treatment effects on agricultural production were presented in Table 4. With social marginal utility weights, we reestimate

[26] We report expressions for all bounds in Appendix B.2. For a generic analysis of the problem, see Tebaldi et al. (2023).

social-marginal-utility-weighted treatment effects.

Treatment Effect on Subsidy Payouts. Treatment effects on subsidy payouts were presented in Table 4. Treatment effects on subsidy payouts are the net costs to the government, and are therefore unweighted.

2. Calibrated Parameters for Marginal Value of Public Funds

Direct Weight on Productive Efficiency γ. When farmers underestimate or undervalue the productive impacts of inputs, a paternalistic planner directly incorporates productive impacts into benefits. In Appendix B.1, we show that the planner's weight γ on productive impacts is the product of a discount factor, farmers' willingness to pay for agricultural production, and a bias, the fraction of productive impacts that farmers did not internalize. We calibrate our preferred value of γ in two steps. First, we apply a discount factor of 0.6; over a 6 month cycle for typical primary season crops, this implies an 8 percent required monthly return on investment.[27] Second, in Section II.F we estimated that Uniform increased willingness to pay by approximately one half, which we interpret as evidence that farmers underestimate the returns to inputs by at least one third. Multiplying the discount factor by this bias yields our preferred value of $\gamma = 0.2$; in Section III.B we consider $\gamma \in [0.0, 0.3]$.[28]

Inequality Aversion η and Social Marginal Utility λ_i. We calculate social marginal utility weights following Eden and Freitas (2024, 2026), who recommend using constant relative risk aversion (CRRA) preferences with inequality aversion of $\eta = 1$ for Mozambique and $\eta \in [0.6, 1.6]$ more generally. With CRRA preferences, $\lambda_i \propto \exp(-\eta \log C_i)$, where C_i is farmer i's consumption. We use farmers' baseline asset index to construct predicted log consumption, $\widehat{\log C_i}$.[29] In Section III.B, we apply social marginal utility weights $\lambda_i \propto \exp(-\eta \widehat{\log C_i})$, and we consider $\eta \in [0, 2]$.

[27] Selected estimates of the monthly return on investment in developing countries range from 5% in Sri Lanka (De Mel et al., 2008), to 10% in India (Hussam et al., 2022), to 18% in Mali (Beaman et al., 2023), to over 20% in Mexico (McKenzie and Woodruff, 2008).

[28] Alternatively interpreted, this calibration implies that farmers are indifferent between 0.6 units of input expenditures and 1 unit of output, but farmers estimated output is two thirds of actual output. These correspond to revealed indifference between 1 unit of input expenditures and 2.5 units of actual output, a "wedge" of 1.5 which we fail to reject for Uniform-compliers in Section II.D.

[29] Since we did not include a consumption module in our survey, we estimate the relationship between our asset index and log consumption in a separate household survey including asset and consumption modules, the 2022 Mozambique Household Budget Survey (INE, 2023), with additional details in Appendix C.

B. Welfare Analysis of the Experiment

We calculate the MVPF for each of our three treatment arms, at our preferred values for planner preferences ($\gamma = 0.2, \eta = 1$) and also without direct weight on productive efficiency ($\gamma = 0$) or without inequality aversion ($\eta = 0$), in Table 7. We additionally report all sufficient statistics used to construct the MVPF, including social-marginal-utility-weighted consumer surplus bounds and treatment effects on production.

Table 7. Welfare

	Comparison		
	Sharply Decreasing *versus* Decreasing	Uniform *versus* Decreasing	High & Decreasing *versus* Decreasing
	(1)	(2)	(3)
Panel A: Impacts on ...			
Production			
Unweighted	1,012	6,770	229
λ_i-weighted ($\eta = 1$)	-70	4,229	305
Consumer surplus			
Unweighted	[172, 198]	[65, 451]	[237, 589]
λ_i-weighted ($\eta = 1$)	[172, 197]	[33, 379]	[205, 528]
Subsidy payouts	621	1,207	1,253
Panel B: MVPF			
Unweighted			
$\gamma = 0$	[0.28, 0.32]	[0.05, 0.37]	[0.19, 0.47]
$\gamma = 0.20$	[0.60, 0.65]	[1.18, 1.50]	[0.23, 0.51]
λ_i-weighted ($\eta = 1$)			
$\gamma = 0$	[0.28, 0.32]	[0.03, 0.31]	[0.16, 0.42]
$\gamma = 0.20$	[0.25, 0.30]	[0.73, 1.02]	[0.21, 0.47]

Notes: Unit of observation: household. Data on package choice and 'Subsidy payouts' are from administrative eVoucher records; 'Production' and 'Asset index' are from our follow-up and baseline household surveys, respectively. Sample: households interviewed at follow-up and registered for an eVoucher.

Our sufficient statistics descriptively replicate the equity–efficiency trade-off. Lower bounds on consumer surplus, relative to impacts on subsidy payouts, are largest for Sharply Decreasing (higher subsidies for Small and Medium), and smallest for Uniform

(higher subsidy for Large). Introducing inequality aversion amplifies these differences: consumer surpluses from High & Decreasing (higher subsidies for Small, Medium, and Large) and Uniform fall sharply, because wealthier farmers are more likely to choose Large, while consumer surplus from Sharply Decreasing is unaffected. When productive impacts are ignored, Sharply Decreasing appears optimal. In contrast, productive impacts are largest under Uniform. While the social-marginal-utility-weighted impacts of Uniform on production are smaller than the unweighted, weighted impacts remain large relative to subsidy payouts. When consumer surplus is ignored, Uniform appears optimal.

At our preferred calibration of the planner's preferences, the MVPF of Uniform is at least 55% larger than the MVPF of Sharply Decreasing and High & Decreasing. The bounds on the MVPF for Uniform include 1, the MVPF for an untargeted unconditional cash transfer. Reshaping agricultural input subsidies therefore compares favorably to a commonly applied benchmark for cost-effective policy.

The planner's inequality aversion and weight on productive impacts shape the high MVPF of Uniform in opposing directions. Eliminating inequality aversion increases the MVPF for Uniform by 47%, as productive impacts are concentrated among wealthier farmers.[30] Absent weight on productive impacts, the MVPF is equal to the ratio of consumer surplus to subsidy payouts. Consequentially, without weight on productive impacts, Sharply Decreasing has the highest lower bound on MVPF.

To more flexibly evaluate the impact of the planner's preferences on optimal policy, we plot the optimal policy as a function of the planner's preferences in Figure 7. Note that as the MVPF is only partially identified, policies may only be partially ranked.[31] We highlight two key takeaways from Figure 7.

First, the planner's optimal policy is robust to the choice of inequality aversion, and is instead primarily driven by the weight placed on productive impacts. With a weight on productive impacts that is even half of our preferred value, Uniform is optimal at all values of inequality aversion we consider.

Second, we apply the insight from Björkegren et al. (2026) that the mapping from planner preferences to optimal policies can be inverted. A planner who chooses a high subsidy rate on a fixed quantity of inputs, similar to Sharply Decreasing, either places a low weight on productive impacts or has implausibly high inequality aversion. This suggests such a planner either views large productive impacts as rationalized by high

[30] This proportional increase is equal to the benefit-weighted average social marginal utility, relative to average social marginal utility. Given the choice of inequality aversion $\eta = 1$, this corresponds to the average Uniform beneficiary having 47% higher consumption than the average farmer.

[31] Bounds on differences between MVPF are sometimes narrower than differences in bounds; we discuss details of comparisons between MVPF in Appendix B.2.

discount rates, rather than bias, or faces alternative constraints potentially linked to political economy or state capacity. In contrast, a planner who chooses a uniform subsidy rate, similar to Uniform, places at least a moderate weight on productive impacts.

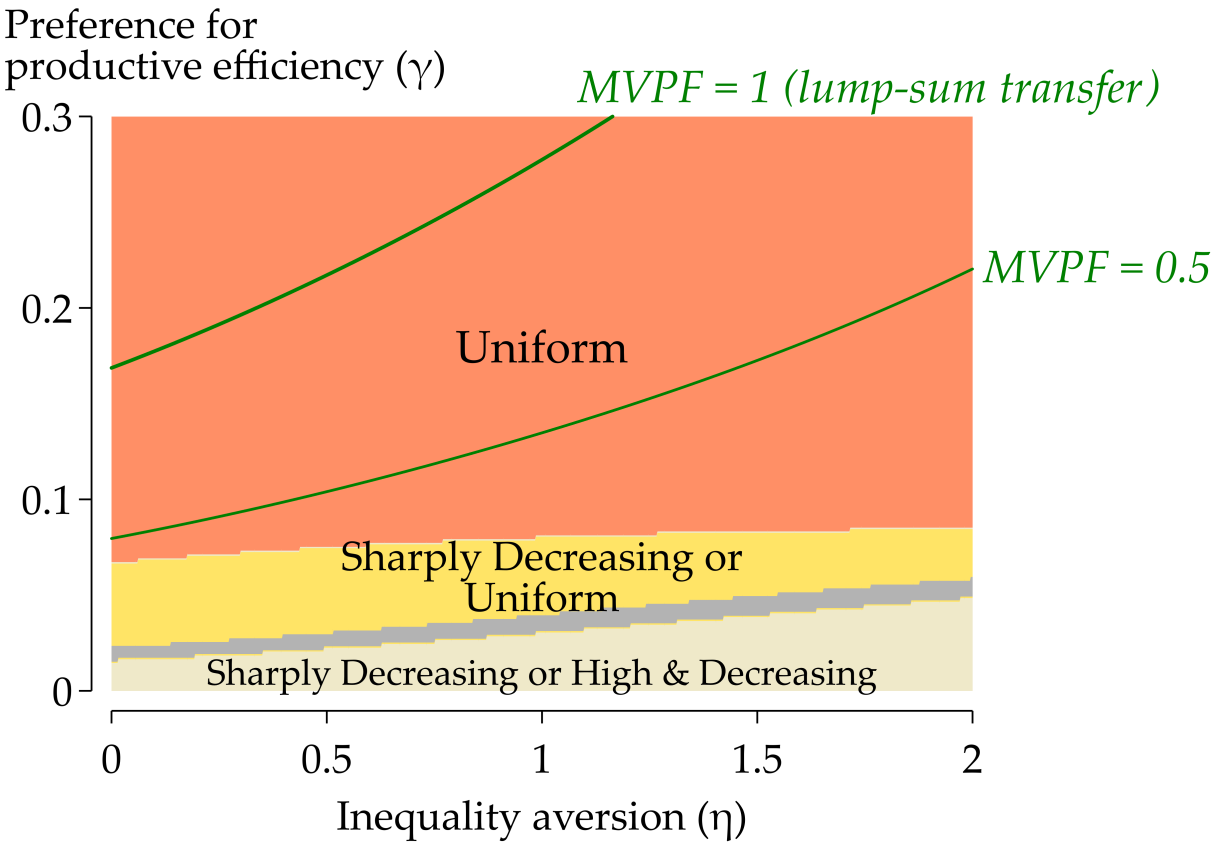
Third, High & Decreasing compares poorly to both Uniform and Sharply Decreasing at almost all values of the planner's preferences. With even a very small weight placed on productive impacts, Uniform is preferred to High & Decreasing. The additional subsidy for Large under High & Decreasing requires a high subsidy payout, yet High & Decreasing does not cause large productive impacts. When a low weight is placed on productive impacts, Sharply Decreasing and High & Decreasing cannot be ranked. However, this is attributable to the possibility that many of the farmers who choose Large under High & Decreasing have a willingness to pay for Large that is close to the Decreasing price; this is prima facie unlikely given High & Decreasing significantly reduces the number of farmers choosing Large relative to Uniform. Duflo et al. (2011) raise the possibility that agricultural input subsidies may be both regressive and inefficient, and consequentially doubly ineffective. We find High & Decreasing is doubly ineffective. That is, double inefficacy is a function of the shape of agricultural input subsidies.

IV. CONCLUSION

Most agricultural input subsidy programs in Africa offer high subsidies for a large fixed package of agricultural inputs. In this paper, we show that this design is inefficient. We experiment with the shape of agricultural input subsidies in the context of rural Mozambique and show that (i) farmers respond to prices, and (ii) farmers are heterogeneous in their input demand. Prices therefore enable self-targeting. The most uniform subsidy rate among those we tested maximizes productive efficiency. Despite being the most regressive of the four subsidy schedules we test, it is optimal under plausible social preferences.

To normatively evaluate subsidy schedules, we develop and apply a simple approach to welfare analysis with internalities. We allow a flexible specification of welfare weights and rich heterogeneity across households. This framework is particularly relevant for interpreting results from experiments in development economics, where subsidies or taxes are widely used to address internalities. Common approaches to evaluate these interventions include either assuming households are fully optimizing or ignoring that households optimize at all; we present evidence that neither extreme is true. Instead, we find that optimizing the shape of agricultural input subsidies increases their productive impacts by increasing input use by farmers who underestimate these impacts and learn from experience.

Figure 7. Welfare and the Planner's Preferences



Notes: This figure displays which treatment is optimal across different preferences of the social planner. A treatment is optimal, or rationalizable, if the upper bound of its marginal value of public funds (MVPF) exceeds the lower bound of all other treatments. Colored regions indicated which treatment is optimal for each pair of preference for productive efficiency and inequality aversion, (γ, η). The gray region is a zone of indeterminacy where no single treatment is dominated by all the other treatments. Green contour lines are level sets for the maximum lower-bound MVPF across treatments. Details on MVPF construction and comparisons are in Appendix B.2.

What is the optimal shape of input subsidies? And given the role of learning, how should shape evolve over time? While these questions are beyond the scope of this paper, our findings imply that their answers are consequential for agricultural productivity and welfare in low-income contexts.

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APPENDIX

A. DATA CONSTRUCTION AND VARIABLE DEFINITION

Our empirical analysis uses household-level agricultural outcomes, based on the 2021 primary (i.e., rainy) season for baseline and on the 2023 primary season for follow-up. Outcomes are considered at their baseline value for balance and compliers analysis, unless otherwise noted, and at follow-up for the reduced form. (ANCOVA regression

specifications, such as Equation 1, control for the baseline value of the dependent variable, and heterogeneous treatment effects are estimated with regard to baseline characteristics.)

In this section, we report details on how variables were defined and constructed from our different data sources. Unless we explicitly state otherwise, baseline and follow-up measures are based on identical survey questions and thus are constructed the same way. All variables are at the household level and monetary values are in nominal Mozambican *meticais* (100 MZN $\approx$ 1.5 USD). Throughout the paper, 'farmer' denotes the household.

Variables based on FAO administrative records from eVoucher system.[32]

- *Registration*: whether a household is registered to the eVoucher program by—and including—the 2023 primary (i.e., rainy) season.
- *Package choice*: eVoucher input package type for which a household is registered for the 2023 primary season. Types are the following ones: Small (*Básico* in the original Portuguese label, worth 4,000 MZN of inputs), Medium (*Regular*, 6,500 MZN), or Large (*Mais*, 11,000 MZN).
- *Redemption*: whether a household redeemed the eVoucher input package they were registered for during the 2023 primary season.
- *Redeemed inputs*: monetary value that a household redeemed for tools (bulk/silage bag, hoe, machete, sickle, sprayer), improved seeds (either open-pollinated or hybrid varieties, any crop), fertilizer (NPK, urea), and post-harvest pesticided during the 2023 primary season.

Variables based on our (baseline and follow-up) household surveys.

- *Household size*: number of persons that normally live in the household.
- *Asset index*: unweighted sum of ten binary variables for whether a household has an oil lamp, a radio, a bicycle, a table, a cellphone, electricity, a solar panel, a motorbike, a television, and a fridge.
- *Row planting*: whether a household practiced row planting in at least a part of their plots in the last 12 months.
- *eVoucher registration*: whether a household registered for an eVoucher from December 2020 to the day of the survey interview.
- *eVoucher use*: how a household used the inputs purchased from their eVoucher package (if they did not use all the inputs purchased in their own agricultural plots). A household can choose from the following multiple options: they gave them to other households who did not have an eVoucher; they gave them to other households who had an eVoucher; they sold them to other households who did not have an eVoucher; they saved the inputs to use them at a later time. We combine these into three, non-exclusive categories: 'giving away', 're-selling', and 'saving'.

[32] These variables are all defined at follow-up.

- *Input expenditures other than eVoucher:* monetary value of agricultural inputs that a household purchased in excess of the quantities redeemed from their eVoucher package.
- *Mechanized equipment:* whether a household used any of the following types of equipment for agricultural production: electric pump, motor pump, plow with mechanical traction, rototiller, seeder, or tractor; mill, motorcycle, thresher, or truck.
- *Landholdings*: unweighted sum of agricultural plot sizes owned by the household, regardless of whether they are cultivated or not, and being used by the household, even if the household is not the landowner.
- *Cultivated area*: unweighted sum of areas cultivated.
- *Production value* (sometimes abbreviated to *Production*): monetary value of the production harvested. To transform physical quantities into monetary values, we use median crop prices. The resulting value can be decomposed into sold production, stored production, and consumed production.
- *Sales*: monetary value of production that has been sold, priced as self-reported by the household.
- *Expected sales*: monetary value of production that has not yet been harvested and that the household is planning to sell, priced as self-reported by the household.
- *Rental*: total monetary value paid for renting the agricultural plot (including payments in goods, cash, or debts) and using equipment for agricultural production.
- *Expenditures on temporary labor*: monetary value paid for agricultural labor (land preparation and sowing, other activities during the growing period, and harvesting) performed by temporary workers. We exclude labor on livestock.
- *Expenditures on permanent labor*: monetary value paid for agricultural labor performed by permanent (i.e., full-time) workers. We exclude labor on livestock.
- *Household labor*: total number of 8-hour days worked by household members on land preparation and sowing, other activities during the growing period, and harvesting. For robustness, we value household labor at different shadow wage rates: 0 MZN, 21 MZN (60% of the sample median wage for temporary labor, weighted by number of labor days, in the 2023 primary season), or 35 MZN (the sample median wage). See Appendix E.
- *Profits*: sum of agricultural production minus expenses on improved seeds, inorganic fertilizer, pesticides, tools and land rental, hired and household labor. For robustness, we consider different types of labor and vary the valuation of household labor. See Appendix E.

B. QUANTITATIVE FRAMEWORK: DETAILS

B.1. Microfoundation of MVPF with Internalities

Farmer i chooses an input quantity $x \in \mathcal{X}$, facing a price schedule $p : \mathcal{X} \to \mathbb{R}$. Household utility takes the form

$$w_i(x; p) \equiv u_i\big(y_i(x)\big) - c_i(x) - p(x) \tag{B1}$$

Utility is quasilinear, with numeraire of input expenditures $p(x)$ plus additional production costs $c_i(x)$. The utility index u_i, which incorporates both discounting and decreasing marginal utility, is applied to agricultural production $y_i(x)$.[33]

Household Perceived Utility and the Household Problem. However, farmers choose input quantities as if their production technology were $\hat{y}_i(x)$, instead of the true $y_i(x)$. Multiple interpretations of this wedge are possible. Farmers may not know the productive impacts of inputs, and may learn these productive impacts with experience. Alternatively, farmers may be present focused, overly discounting the future, and choose input quantities as if productive impacts were lower than they actually are. For concreteness, we refer to $\hat{y}_i(x)$ as perceived production.

$$\text{WTP}_i(p) \equiv \max_x u_i\big(\hat{y}_i(x)\big) - c_i(x) - p(x) \tag{B2}$$

We use $\text{WTP}_i(p)$ to denote the maximized value of the farmer's objective function, as with quasilinear utility $\text{WTP}_i(p') - \text{WTP}_i(p)$ is farmer i's willingness to pay (in input expenditures) for price schedule p' relative to price schedule p.

Household Welfare. We consider the impacts on the utility of farmer i of a price change from p to p'. Let $X_i(p)$ denote farmer i's choice of input quantity under the price schedule p. Similarly, let $Y_i(p) \equiv y_i\big(X_i(p)\big)$ denote realized production, $\hat{Y}_i(p) \equiv \hat{y}_i\big(X_i(p)\big)$ ex-ante perceived realized production, and $W_i(p) \equiv w_i(X_i(p), p)$ realized utility. Letting $\Delta(f(\cdot)) \equiv f(p') - f(p)$ denote changes in values from the price schedule p to p', we decompose

$$\Delta W_i(\cdot) = \Delta\text{WTP}_i + \underbrace{\left(1 - \frac{\Delta u_i\big(\hat{Y}_i(\cdot)\big)}{\Delta u_i\big(Y_i(\cdot)\big)}\right)}_{\equiv 1-\beta_i} \underbrace{\frac{\Delta u_i\big(Y_i(\cdot)\big)}{\Delta Y_i(\cdot)}}_{\equiv \delta_i} \Delta Y_i(\cdot) \tag{B3}$$

Impacts on household welfare are simply impacts on willingness to pay, plus rescaled impacts on production. Production impacts are rescaled by two terms. The first, δ_i, is simply a generalized discount factor: it is the ratio of farmers' willingness to pay for the change in agricultural production $\Delta u_i\big(Y_i(\cdot)\big)$ to the change in agricultural production itself $Y_i(\cdot)$. The second, $1 - \beta_i$, is one minus the ratio of the willingness to pay for perceived

[33] Uncertainty can also be accommodated. For instance, with multiplicative shocks that are distributed independently of x and a CRRA utility index, uncertainty is equivalent to additional discounting of agricultural production.

changes in production to the willingness to pay for actual changes in production. When there are no distortions, and farmers choose input quantities as if their production technologies were the true $y_i(x)$, then $\hat{Y}_i = Y_i$ and $\Delta W_i = \Delta \text{WTP}_i$. We note that both δ_i and β_i implicitly depend on the price schedules considered, through their dependence on $X_i(p)$ and $X_i(p')$.

We refer to $(1-\beta_i)\delta_i$ as the "discounted wedge:" it is the farmer's discount factor, times the farmer's uninternalized fraction of willingness to pay for the productive impacts $\Delta Y_i(\cdot)$.

Social Utility. We consider a social planner who places weight λ_i on farmer i's utility. Social utility is

$$W(p;\lambda) = \sum_i \lambda_i W_i(p) \tag{B4}$$

We suppose the government must pay the costs of the policy p, that is the differences between prices paid $p(x)$, and the social costs of those inputs; without loss of generality, we assume the social cost of the input quantity x is x. We let $G_i(p) \equiv X_i(p) - p(X_i(p))$ denote the government costs from farmer i, that is the difference between the social cost of inputs and the price the farmer pays; empirically, this is the subsidy payout to farmer i.

MVPF and Optimal Policy. With this, we are equipped to define the marginal value of public funds. We follow Hahn et al. (2026), who define the MVPF to be the ratio of unweighted benefits to individuals (that is, $\sum_i \Delta W_i(\cdot)$) to net costs to the government (that is, $\sum_i \Delta G_i(\cdot)$).

$$\text{MVPF}(p, p'; \lambda) \equiv \frac{\sum_i \frac{\lambda_i}{\sum_i \lambda_i} \Delta W_i(\cdot)}{\sum_i \Delta G_i(\cdot)} \tag{B5}$$

Note that our definition generalizes the definition in Hahn et al. (2026) in two ways. First, we include the reference policy p; we do this because in our empirical analysis, welfare impacts are partially identified, with bounds specific to the pair of policies compared. Second, we allow the MVPF to be a function of λ, in that we substitute λ_i-weighted benefits for unweighted benefits. Hahn et al. (2026) note that a budget neutral policy that increases the fraction of farmers assigned to p'' instead of p, and to p instead of p', increases social welfare given λ if and only if

$$\text{MVPF}(p, p''; \lambda) > \text{MVPF}(p, p'; \lambda)$$

We use this principle to explore directly how alternative parametrizations of λ affect the ranking of MVPF, and in turn policies.

Sufficient Statistics for MVPF. We substitute into our expression for MVPF our expression for household welfare. Additionally, we make the substitution

$$\gamma \equiv \frac{\sum_i (1-\beta_i)\delta_i \lambda_i \Delta Y_i(\cdot)}{\sum_i \lambda_i \Delta Y_i(\cdot)} \tag{B6}$$

That is, γ is an average of farmers' discounted wedges $(1-\beta_i)\delta_i$, weighted by social marginal utility weights λ_i times productive impacts ΔY_i. With this definition, the MVPF is

$$\text{MVPF}(p, p'; \lambda) = \frac{\sum_i \frac{\lambda_i}{\sum_i \lambda_i}(\Delta\text{WTP}_i(\cdot) + \gamma \Delta Y_i(\cdot))}{\sum_i \Delta G_i(\cdot)} \tag{B7}$$

We note that γ is not identified, and we therefore explore the robustness of our MVPF to alternative values of γ; in Section III.A, we also apply our experimental results to suggest reasonable values for γ. However, we note that in general γ may vary across policies: γ will be larger for policies that generate relatively larger productive impacts for farmers with larger discounted wedges.

Conditional on γ, there are three sufficient statistics for $\text{MVPF}(p, p'; \lambda)$. The first, $\sum_i \lambda_i \Delta\text{WTP}_i(\cdot) / \sum_i \lambda_i$ is the social-marginal-utility-weighted impact on willingness to pay; we derive bounds on this in Section III.A. The second, $\sum_i \lambda_i \Delta Y_i(\cdot) / \sum_i \lambda_i$ is the social-marginal-utility-weighted impact on production, and the third, $\sum_i \Delta G_i(\cdot)$, is the impact on subsidy payouts; we estimate these directly using our experimental variation in price schedules.

B.2. Bounds on Consumer Surplus

Bounds on Consumer Surplus. We summarized our construction of bounds on consumer surplus in Section III.A.1; we report expressions for all bounds we apply in Table B1a. We report estimates of bounds on consumer surplus from Sharply Decreasing (higher subsidies for Small and Medium), Uniform (higher subsidy for Large), and High & Decreasing (higher subsidies for Small, Medium, and Large) in Table 7.

Comparing MVPF. In Figure 7, we compare relative values of MVPF to report the set of rationalizable policies; we report the comparisons used to determine whether a policy can be rationalized in Table B1b. Let B_d and C_d denote the benefits and costs of treatment d, either Sharply Decreasing, Uniform, or High & Decreasing, relative to Decreasing. We say treatment d is rationalizable relative to d' if we cannot rule out that $B_d/C_d \geq B_{d'}/C_{d'}$ or equivalently $(B_d - B_{d'})/(C_d - C_{d'}) \geq B_{d'}/C_{d'}$. That is, a treatment is rationalizable relative to another treatment if, given our bounds, we cannot rule out the possibility that it has a larger MVPF. We say treatment d is rationalizable if it is rationalizable relative to all other treatments.

B.3. Testing for Profit Maximization, Homogeneous Marginal Products, and Homogeneous Wedges

In Sections B.1 and B.2, we did not assume additional production costs $C_i(p)$ were observable at assigned prices p. In this section we treat $C_i(p)$ as observable; our preferred construction is described in Section II.D. For notational convenience, we define "profits gross of inputs" $\pi_i(x) \equiv y_i(x) - c_i(x)$ and $\Pi_i(p) \equiv \pi_i(X_i(p)) = Y_i(p) - C_i(p)$.

Table B1. Bounds on Consumer Surplus and Comparing MVPF

(a) Bounds on Consumer Surplus

Parameter	Lower Bound (LB)	Upper Bound (UB)
CS_{SM}	300 $P_{SM}(0)$	300 $P_{SM}(SM)$
CS_{L}	2,600 $P_{L}(0)$	300 $P_{L}(L)$ + 2,300 $P_{L}(SML)$
CS_{SML}	300 $P_{SM}(0)$ + 2,600 $P_{L}(0)$	300 $P_{SM}(SML)$ + 2,600 $P_{L}(SML)$
$CS_{L} - CS_{SM}$	$LB(CS_{SML} - CS_{SM})$ - $UB(CS_{SML} - CS_{L})$	$UB(CS_{L})$ - $LB(CS_{SM})$
$CS_{SML} - CS_{SM}$	300 $P_{L}(SM)$ + 2,300 $P_{L}(0)$	2,600 $P_{L}(SML)$
$CS_{SML} - CS_{L}$	300 $P_{SM}(L)$	300 $P_{SM}(SML)$

Notes: CS_d denotes consumer surplus from assignment to treatment d, either Sharply Decreasing (SM), Uniform (L), or High & Decreasing (SML), relative to Decreasing. $P_x(d)$ denotes the fraction of farmers choosing x, either Small/Medium (SM) or Large (L), under treatment d, either Decreasing (0), SM, L, or SML.

(b) Comparing MVPF across Treatment Arms

Sharply Decreasing (SM) rationalizable...	
relative to Uniform (L)	$\frac{UB(B_{SM})}{C_{SM}} > \frac{LB(B_{L})}{C_{L}}$
relative to High & Decreasing (SML)	$\frac{UB(B_{SM})}{C_{SM}} > \frac{LB(B_{SML}-B_{SM})}{C_{SML}-C_{SM}}$
Uniform (L) rationalizable...	
relative to Sharply Decreasing (SM)	$\frac{UB(B_{L})}{C_{L}} > \frac{LB(B_{SM})}{C_{SM}}$
relative to High & Decreasing (SML)	$\frac{UB(B_{L})}{C_{L}} > \frac{LB(B_{SML}-B_{L})}{C_{SML}-C_{L}}$
High & Decreasing (SML) rationalizable...	
relative to Sharply Decreasing (SM)	$\frac{UB(B_{SML}-B_{SM})}{C_{SML}-C_{SM}} > \frac{LB(B_{SM})}{C_{SM}}$
relative to Uniform (L)	$\frac{UB(B_{SML}-B_{L})}{C_{SML}-C_{L}} > \frac{LB(B_{L})}{C_{L}}$

Notes: B_d and C_d denote the benefits to individuals and net costs to government, respectively, from treatment d, either Sharply Decreasing (SM), Uniform (L), or High & Decreasing (SML), relative to Decreasing.

Test of Profit Maximization. If farmer i profit maximizes, choosing a counterfactual input quantity cannot increase profits. Therefore, for any p',

$$\pi_i(X_i(p)) - p(X_i(p)) \geq \pi_i(X_i(p')) - p(X_i(p')) \tag{B8}$$

Equation B8 implies that under profit maximization, no price schedule p' can increase profits net of inputs priced under price schedule p, relative to price schedule p.

Test of Homogeneous Marginal Products. We test whether the average marginal product over changes in input use caused by counterfactual price schedules is homogeneous across price schedules. Homogeneity implies

$$\frac{\sum_i \frac{\Pi_i(p') - \Pi_i(p)}{X_i(p') - X_i(p)} (X_i(p') - X_i(p))}{\sum_i X_i(p') - X_i(p)} = \frac{\sum_i \frac{\Pi_i(p'') - \Pi_i(p)}{X_i(p'') - X_i(p)} (X_i(p'') - X_i(p))}{\sum_i X_i(p'') - X_i(p)} \tag{B9}$$

The instrumental variable estimator of the impacts of input use X_i on profits gross of inputs Π_i, using assignment to p' and p'' relative to p as an instrument, estimates the left hand side and right hand side of Equation B9, respectively; an over-identification test is therefore a test of Equation B9.

Bounds on Wedges and a Test of Homogeneous Wedges. Let $\hat{\Pi}_i(p) \equiv u_i(\hat{Y}_i(p)) - C_i(p)$. Define the complier average wedge for a change in price schedule from p to p' to be

$$\omega(p', p) \equiv \frac{\sum_i \left(\frac{\Pi_i(p') - \Pi_i(p)}{\hat{\Pi}_i(p') - \hat{\Pi}_i(p)} - 1 \right) \left(\hat{\Pi}_i(p') - \hat{\Pi}_i(p) \right)}{\sum_i \hat{\Pi}_i(p') - \hat{\Pi}_i(p)} \tag{B10}$$

Note that $\omega(p', p)$ is a weighted average of $\frac{\Pi_i(p') - \Pi_i(p)}{\hat{\Pi}_i(p') - \hat{\Pi}_i(p)} - 1$, the wedge between impacts on profits and impacts on the farmer's objective function. We note that the household objective function in Equation B2 implies, by a standard revealed preference argument

$$p'(X_i(p')) - p'(X_i(p)) \leq \hat{\Pi}_i(p') - \hat{\Pi}_i(p) \leq p(X_i(p')) - p(X_i(p)) \tag{B11}$$

Equation B11 enables the following bounds on Equation B10 for $\sum_i p'(X_i(p')) - p'(X_i(p)) > 0$.

$$\frac{\sum_i \Pi_i(p') - \Pi_i(p)}{\sum_i p(X_i(p')) - p(X_i(p))} \leq 1 + \omega(p', p) \leq \frac{\sum_i \Pi_i(p') - \Pi_i(p)}{\sum_i p'(X_i(p')) - p'(X_i(p))} \tag{B12}$$

The instrumental variable estimator of the impacts of input expenditures priced under p or p' ($p(X_i)$ or $p'(X_i)$) on profits gross of inputs Π_i, using assignment to p' relative to p as an instrument, estimates the lower bound and upper bound on one plus the complier average wedge, respectively. A test of whether the lower bound on $\omega(p'', p)$ is greater than the upper bound on $\omega(p', p)$ is a test of homogeneous wedges.

SUPPLEMENTAL APPENDIX

C. CONSTRUCTING PREDICTED LOG CONSUMPTION

Our household survey does not include a consumption module. To predict log consumption in our experimental sample, and so obtain social marginal utility weights for the welfare analysis of our experiment, we turn to a separate household survey: the 2022 Mozambique Household Budget Survey (INE, 2023). Importantly for us, this survey includes both asset and consumption modules. The two variables we use are defined as follows.

- *Asset index*: unweighted sum of ten binary variables for whether a household has an oil lamp, a radio, a bicycle, a table, a cellphone, electricity, a solar panel, a motorbike, a television, and a fridge. This variable exactly matches the list of items we measured in our household survey.
- *Consumption aggregate*: total annual consumption (nominal Mozambican *meticais*; 100 MZN $\approx$ 1.5 USD). The aggregate is composed of food consumption (valuing non-purchased food using market prices) and non-food consumption, including health, education, housing, and durable goods (using subjective rent value for housing for non-renters and calculating the flow values of durables). The methodology is described in INE (2023) and follows guidelines from Deaton and Zaidi (2002).

We estimate the relationship between log consumption and asset index using the following regression,

$$\log C_i = \alpha + \beta \cdot \text{Asset}_i + \varepsilon_i \tag{C1}$$

where i indexes a household. Our estimate is in Table C1.

Table C1. Relationship between Consumption and Assets

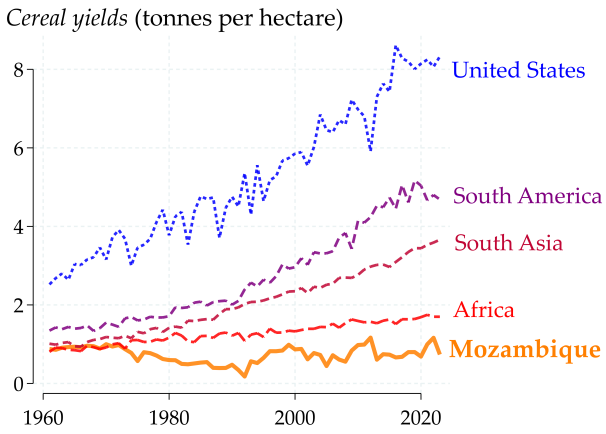
	Log consumption (1)
Asset index (0–10)	0.333
	(0.005)
	[0.000]
Sample mean of the outcome variable	11.488
Number of observations	67,688
Number of farmers	14,743
Adjusted R-squared	0.315

Notes: Unit of observation: household. Data are from the 2022 Mozambique Household Budget Survey (INE, 2023). All regressions are ordinary least squares as in Equation C1. Standard errors clustered at the household ('farmer') level in parentheses; p-values in brackets.

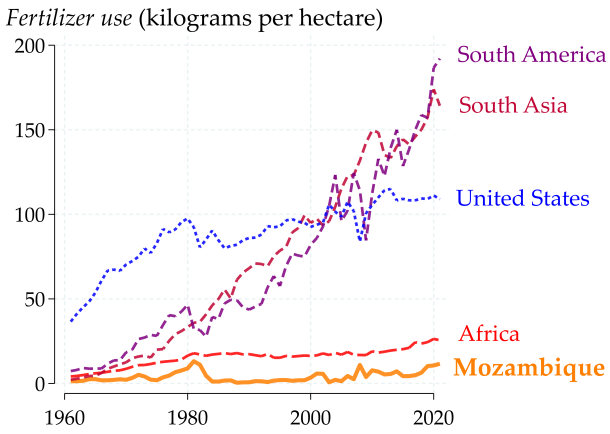
D. ADDITIONAL FIGURES AND TABLES

Figure D1. Agricultural productivity and input use in Africa are low and stagnant

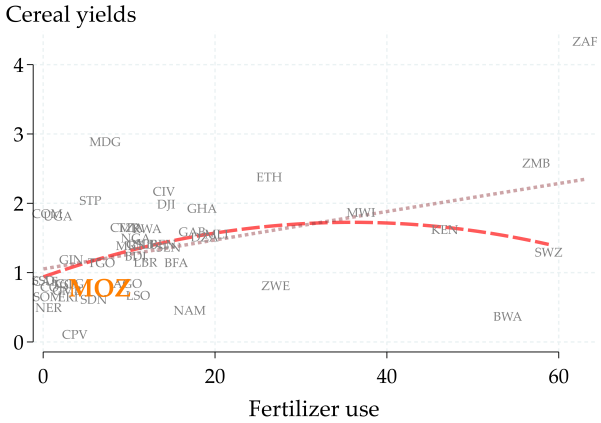
(a) Global Trend in Yields



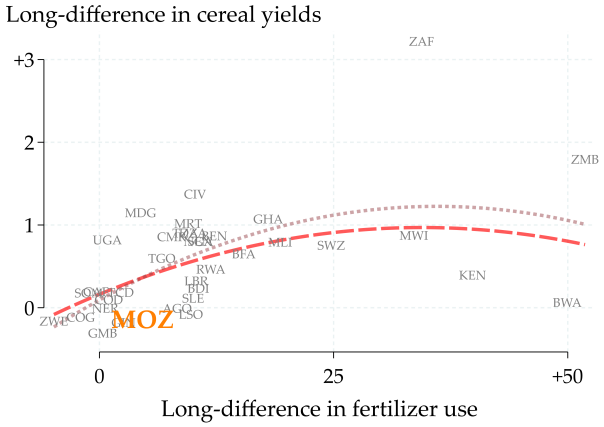
(b) Global Trend in Fertilizer Use



(c) Relationship between Yields and Fertilizer Use in Africa South of the Sahara in the Last Decade (2011-2020)



(d) Relationship between Yields and Fertilizer Use in Africa South of the Sahara: Long-Difference between 1961-1970 and 2011-2020



Notes: Data from FAOSTAT (Food and Agriculture Organization of the United Nations). "Cereals" include wheat, rice, maize, barley, oats, rye, millet, sorghum, buckwheat, and mixed grains. "Fertilizer" includes nitrogenous, potash, and phosphate fertilizers, while the area corresponds to the sum of arable land and permanent cropland. The pink and red dashed lines in panels (c) and (d) plot the quadratic best fit with and without South Africa (ZAF), respectively.

Figure D2. An "eVoucher"

(a) Front



(b) Back



Figure D3. Registration and Redemption Process

(a) Registration



(b) Redemption



Table D1. First Stage on Package Choice: Alternative Samples

	Package type			
	Any	Small	Medium	Large
	(1)	(2)	(3)	(4)
Full sample				
Sharply Decreasing	0.066	-0.020	0.097	-0.011
	(0.022)	(0.024)	(0.021)	(0.009)
	[0.003]	[0.413]	[0.000]	[0.227]
Uniform	0.007	-0.072	-0.090	0.170
	(0.023)	(0.024)	(0.018)	(0.016)
	[0.757]	[0.003]	[0.000]	[0.000]
High & Decreasing	0.049	-0.047	-0.027	0.122
	(0.022)	(0.025)	(0.020)	(0.014)
	[0.029]	[0.057]	[0.183]	[0.000]
Sample mean of the reference group	0.613	0.422	0.171	0.020
Community fixed effects	✓	✓	✓	✓
Number of observations	2,383	2,383	2,383	2,383
Number of farmers	2,383	2,383	2,383	2,383
Baseline sample				
Sharply Decreasing	0.084	-0.013	0.110	-0.012
	(0.031)	(0.033)	(0.030)	(0.015)
	[0.007]	[0.686]	[0.000]	[0.406]
Uniform	0.055	-0.046	-0.093	0.194
	(0.032)	(0.033)	(0.025)	(0.023)
	[0.091]	[0.161]	[0.000]	[0.000]
High & Decreasing	0.081	-0.038	-0.024	0.143
	(0.031)	(0.033)	(0.028)	(0.021)
	[0.009]	[0.249]	[0.387]	[0.000]
Sample mean of the reference group	0.590	0.395	0.170	0.024
Community fixed effects	✓	✓	✓	✓
Number of observations	1,280	1,280	1,280	1,280
Number of farmers	1,280	1,280	1,280	1,280

Notes: Unit of observation: household. Data are from administrative eVoucher records. Sample in the panel header: 'full' includes all registered households in the eVoucher system (i.e., regardless of survey status) and 'baseline' includes all registered households interviewed at baseline (i.e., regardless of being re-interviewed at follow-up). All regressions are least squares with strata (i.e., community) fixed effects as in Equation 1. Standard errors clustered at the household ('farmer') level in parentheses; p-values in brackets. Estimates on the main analysis sample (i.e., registered households interviewed at baseline and re-interviewed at follow-up) in Table 2.

Table D2. No Differential Attrition by Treatment Assignment

	Sample	
	Whole baseline (1)	*Registered* (2)
Sharply Decreasing	0.001	-0.003
	(0.014)	(0.011)
	[0.924]	[0.798]
Uniform	0.000	-0.005
	(0.014)	(0.010)
	[1.000]	[0.612]
High & Decreasing	0.006	-0.004
	(0.014)	(0.011)
	[0.696]	[0.741]
Sample mean of the reference group	0.041	0.021
Community fixed effects	✓	✓
Number of observations	1,538	1,280
Number of farmers	1,538	1,280

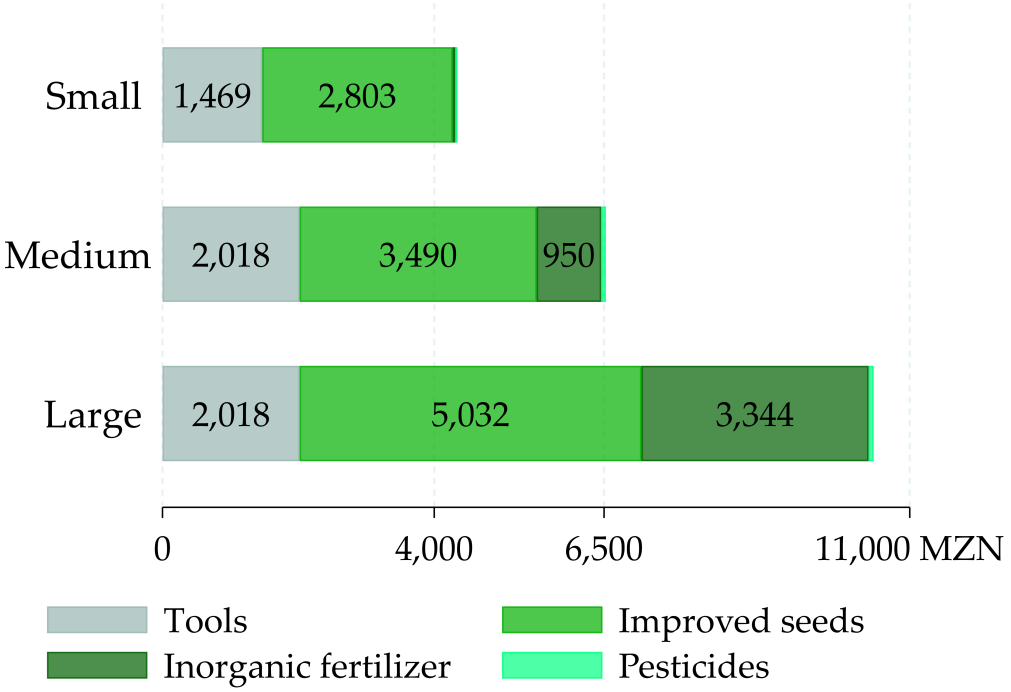
Notes: Unit of observation: household. Sample in the italic column header. All regressions are least squares with strata (i.e., community) fixed effects as in Equation 1. Standard errors clustered at the household ('farmer') level in parentheses; p-values in brackets. The dependent variable is equal to one if the household was *not* re-interviewed at follow-up (i.e., attrited) and to zero otherwise.

Table D3. No Differential Registration by Treatment Assignment

Data source:	**Administrative records**	**Survey self-report**		
Outcome:	*Registered*	*Heard about*	*Was contacted*	*Registered*
	(1)	(2)	(3)	(4)
Sharply Decreasing	-0.001	-0.007	0.024	0.021
	(0.021)	(0.020)	(0.023)	(0.024)
	[0.969]	[0.713]	[0.297]	[0.396]
Uniform	-0.022	0.002	0.013	0.005
	(0.023)	(0.019)	(0.023)	(0.025)
	[0.340]	[0.904]	[0.574]	[0.832]
High & Decreasing	-0.003	0.005	-0.009	0.006
	(0.022)	(0.019)	(0.023)	(0.025)
	[0.880]	[0.794]	[0.692]	[0.796]
Sample mean of the reference group	0.861	0.920	0.864	0.832
Community fixed effects	✓	✓	✓	✓
Number of observations	1,473	1,473	1,473	1,473
Number of farmers	1,473	1,473	1,473	1,473

Notes: Unit of observation: household. Data source in the bold column header. Sample: households interviewed at follow-up. All regressions are least squares with strata (i.e., community) fixed effects as in Equation 1. Standard errors clustered at the household ('farmer') level in parentheses; *p*-values in brackets.

Figure D4. Input Composition of Packages



Notes: Unit of observation: household. Data are from administrative eVoucher records. Sample: households interviewed at follow-up and registered for an eVoucher. The horizontal bars plot the sample mean of input values by package chosen. Values are in nominal Mozambican *meticais* (100 MZN $\approx$ 1.5 USD).

Table D4. Balance of Treatment Assignment in Sample of Registered Households ($N = 1,280$)

	Mean (SD)				Coefficient (SE) [p]					
	Decreasing	Sharply Decreasing	Uniform	High & Decreasing	Difference					
	(1)	(2)	(3)	(4)	(2)-(1)	(3)-(1)	(4)-(1)	(3)-(2)	(4)-(2)	(4)-(3)
Household size	5.356	5.440	5.570	5.567	0.012	0.239	0.172	0.128	0.148	-0.075
	(2.342)	(2.257)	(2.309)	(2.477)	(0.183)	(0.191)	(0.183)	(0.190)	(0.200)	(0.191)
					[0.949]	[0.212]	[0.348]	[0.499]	[0.460]	[0.695]
Household head is female	0.213	0.201	0.161	0.198	-0.007	-0.041	-0.020	-0.048	-0.004	0.041
	(0.410)	(0.402)	(0.368)	(0.399)	(0.032)	(0.033)	(0.032)	(0.033)	(0.032)	(0.033)
					[0.834]	[0.213]	[0.527]	[0.147]	[0.906]	[0.211]
Asset index (0–10)	2.915	2.805	3.216	2.950	-0.113	0.330	0.014	0.366	0.182	-0.290
	(1.974)	(2.059)	(2.057)	(1.955)	(0.158)	(0.151)	(0.159)	(0.167)	(0.164)	(0.164)
					[0.474]	[0.029]	[0.928]	[0.029]	[0.266]	[0.076]
Used pre-harvest mechanized equipment	0.036	0.043	0.056	0.046	0.009	0.023	0.005	0.015	-0.002	-0.005
	(0.188)	(0.204)	(0.230)	(0.211)	(0.016)	(0.017)	(0.015)	(0.017)	(0.017)	(0.018)
					[0.596]	[0.178]	[0.753]	[0.374]	[0.914]	[0.789]
Used post-harvest processing or transport	0.134	0.164	0.138	0.118	0.037	0.019	-0.017	-0.035	-0.053	-0.024
	(0.341)	(0.371)	(0.345)	(0.323)	(0.029)	(0.026)	(0.026)	(0.030)	(0.028)	(0.027)
					[0.201]	[0.463]	[0.512]	[0.247]	[0.060]	[0.371]
Used improved seeds	0.094	0.084	0.102	0.074	-0.018	0.004	-0.031	0.007	-0.017	-0.023
	(0.293)	(0.277)	(0.303)	(0.263)	(0.023)	(0.026)	(0.023)	(0.023)	(0.022)	(0.024)
					[0.425]	[0.876]	[0.189]	[0.767]	[0.450]	[0.334]
Used inorganic fertilizer	0.030	0.031	0.052	0.043	-0.003	0.019	0.013	0.018	0.015	-0.007
	(0.172)	(0.173)	(0.223)	(0.204)	(0.013)	(0.016)	(0.016)	(0.016)	(0.015)	(0.019)
					[0.827]	[0.250]	[0.440]	[0.239]	[0.330]	[0.704]
Cultivated area (ha)	1.499	1.617	1.704	1.518	0.074	0.237	-0.017	0.070	-0.032	-0.135
	(1.507)	(1.567)	(2.062)	(1.452)	(0.112)	(0.154)	(0.115)	(0.154)	(0.116)	(0.139)
					[0.508]	[0.123]	[0.880]	[0.648]	[0.782]	[0.330]
Production value ('000 MZN)	18.654	23.439	21.449	20.984	4.221	2.453	2.152	-2.911	-2.099	1.253
	(26.219)	(38.489)	(30.510)	(41.662)	(2.556)	(2.186)	(2.972)	(2.754)	(3.090)	(3.215)
					[0.099]	[0.262]	[0.469]	[0.291]	[0.497]	[0.697]
Sold or planning to sell	0.766	0.789	0.807	0.814	0.010	0.047	0.040	0.023	0.017	0.011
	(0.424)	(0.408)	(0.396)	(0.390)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)
					[0.747]	[0.124]	[0.186]	[0.449]	[0.573]	[0.716]
Omnibus *F*-stat [p]					3.3	2.7	0.8	0.9	0.4	0.1
					[0.069]	[0.098]	[0.382]	[0.343]	[0.505]	[0.794]
Number of observations	329	323	305	323	652	634	652	628	646	628
Number of farmers	329	323	305	323	652	634	652	628	646	628

Notes: Unit of observation: household. Data are from our baseline household surveys. Sample: households interviewed at baseline and registered for an eVoucher. Coefficients are obtained from least-squares regressions with strata (i.e., community) fixed effects as in Equation 1, but only comparing two treatment assignments. Standard errors clustered at the household ('farmer') level in parentheses; *p*-values in brackets. Omnibus *F*-statistics are obtained from least-squares regressions with strata (i.e., community) fixed effects, stacking all the variables in the left-most column. 'Pre-harvest mechanized equipment' includes tractor, motor and electric pumps, and sprayer. 'Post-harvest processing or transport' includes grain mill, oil press, motorbike, and truck.

Table D5. No Evidence of Reselling or Crowding Out

	eVoucher inputs			*Non-eVoucher*
	Gave away	*Sold*	*Stored*	*Input value*
	(1)	(2)	(3)	(4)
Sharply Decreasing	-0.015	0.004	0.014	-5
	(0.013)	(0.006)	(0.022)	(153)
	[0.247]	[0.579]	[0.519]	[0.976]
Uniform	0.010	0.011	0.003	143
	(0.015)	(0.007)	(0.023)	(238)
	[0.527]	[0.129]	[0.909]	[0.549]
High & Decreasing	0.008	0.001	0.008	-142
	(0.015)	(0.006)	(0.022)	(148)
	[0.577]	[0.876]	[0.723]	[0.338]
Sample mean of the reference group	0.031	0.003	0.093	230
Community fixed effects	✓	✓	✓	✓
Number of observations	1,254	1,254	1,254	1,254
Number of farmers	1,254	1,254	1,254	1,254

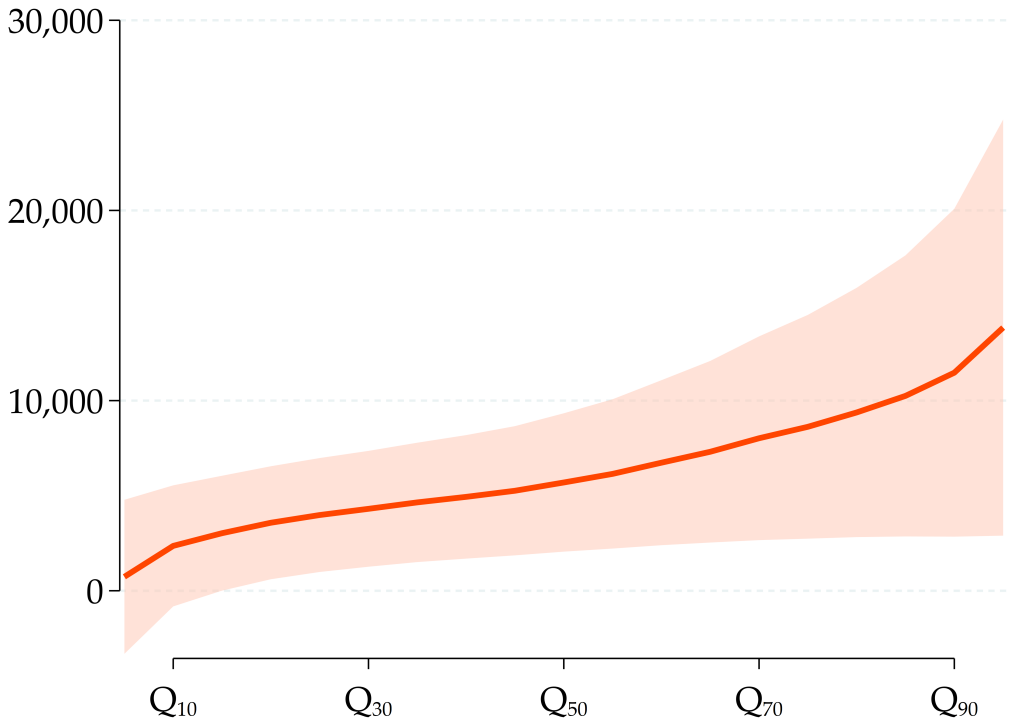
Notes: Unit of observation: household. Data are from our follow-up household surveys. Sample: households interviewed at follow-up and registered for an eVoucher. All regressions are least squares with strata (i.e., community) fixed effects as in Equation 1. Standard errors clustered at the household ('farmer') level in parentheses; p-values in brackets. 'Input value' is in nominal Mozambican *meticais* (100 MZN $\approx$ 1.5 USD).

Table D6. First Stage on Input Values: Alternative Samples

	Input category				
	Any	*Improved seeds*	*Inorganic fertilizer*	*Pesticides*	*Tools*
	(1)	(2)	(3)	(4)	(5)
Full sample					
Sharply Decreasing	535	515	-20	29	11
	(172)	(114)	(76)	(8)	(60)
	[0.002]	[0.000]	[0.792]	[0.000]	[0.853]
Uniform	988	471	381	15	121
	(211)	(146)	(97)	(7)	(62)
	[0.000]	[0.001]	[0.000]	[0.041]	[0.053]
High & Decreasing	971	559	376	16	21
	(185)	(118)	(93)	(7)	(63)
	[0.000]	[0.000]	[0.000]	[0.024]	[0.745]
Sample mean of the reference group	3,319	1,832	405	23	1,058
Community fixed effects	✓	✓	✓	✓	✓
Number of observations	2,383	2,383	2,383	2,383	2,383
Number of farmers	2,383	2,383	2,383	2,383	2,383
Baseline sample					
Sharply Decreasing	722	643	23	40	16
	(261)	(172)	(117)	(12)	(90)
	[0.006]	[0.000]	[0.844]	[0.001]	[0.857]
Uniform	1,474	870	351	19	234
	(324)	(232)	(141)	(9)	(92)
	[0.000]	[0.000]	[0.013]	[0.036]	[0.012]
High & Decreasing	1,330	791	452	23	64
	(265)	(165)	(136)	(9)	(93)
	[0.000]	[0.000]	[0.001]	[0.011]	[0.490]
Sample mean of the reference group	3,221	1,686	457	16	1,062
Community fixed effects	✓	✓	✓	✓	✓
Number of observations	1,280	1,280	1,280	1,280	1,280
Number of farmers	1,280	1,280	1,280	1,280	1,280

Notes: Unit of observation: household. Data are from administrative eVoucher records. Sample in the panel header: 'full' includes all registered households in the eVoucher system (i.e., regardless of survey status) and 'baseline' includes all registered households interviewed at baseline (i.e., regardless of being re-interviewed at follow-up). All regressions are least squares with strata (i.e., community) fixed effects as in Equation 1. Standard errors clustered at the household ('farmer') level in parentheses; p-values in brackets. Outcomes are in nominal Mozambican *meticais* (100 MZN $\approx$ 1.5 USD). Estimates on the main analysis sample (i.e., registered households interviewed at baseline and re-interviewed at follow-up) in Table 3.

Figure D5. Quantile Treatment Effects of Uniform on Profits



Notes: Unit of observation: household. Data are from administrative eVoucher records. Sample: households interviewed at follow-up and registered for an eVoucher. Quantile treatment effects are estimated via the method of moments proposed by Machado and Silva (2019), using strata (i.e., community) fixed effects and controlling linearly for the baseline value of the dependent variable; if the latter variable is missing, we impute it by taking the sample mean in the estimation sample and further include a binary indicator that the variable is missing. Standard errors are clustered at the household ('farmer') level. The outcome, i.e., 'Profits', is in nominal Mozambican *meticais* (100 MZN $\approx$ 1.5 USD) and is defined as the sum of agricultural production value (i.e., the value of harvest regardless of whether it was consumed, sold, or stored using median prices in the whole study sample) during the 2023 primary season minus expenses on improved seeds, inorganic fertilizer, pesticides, tools and land rental, hired labor, and household labor (priced at 60% of the median wage across casual labor days).

Figure D6. Selected Complier Groups

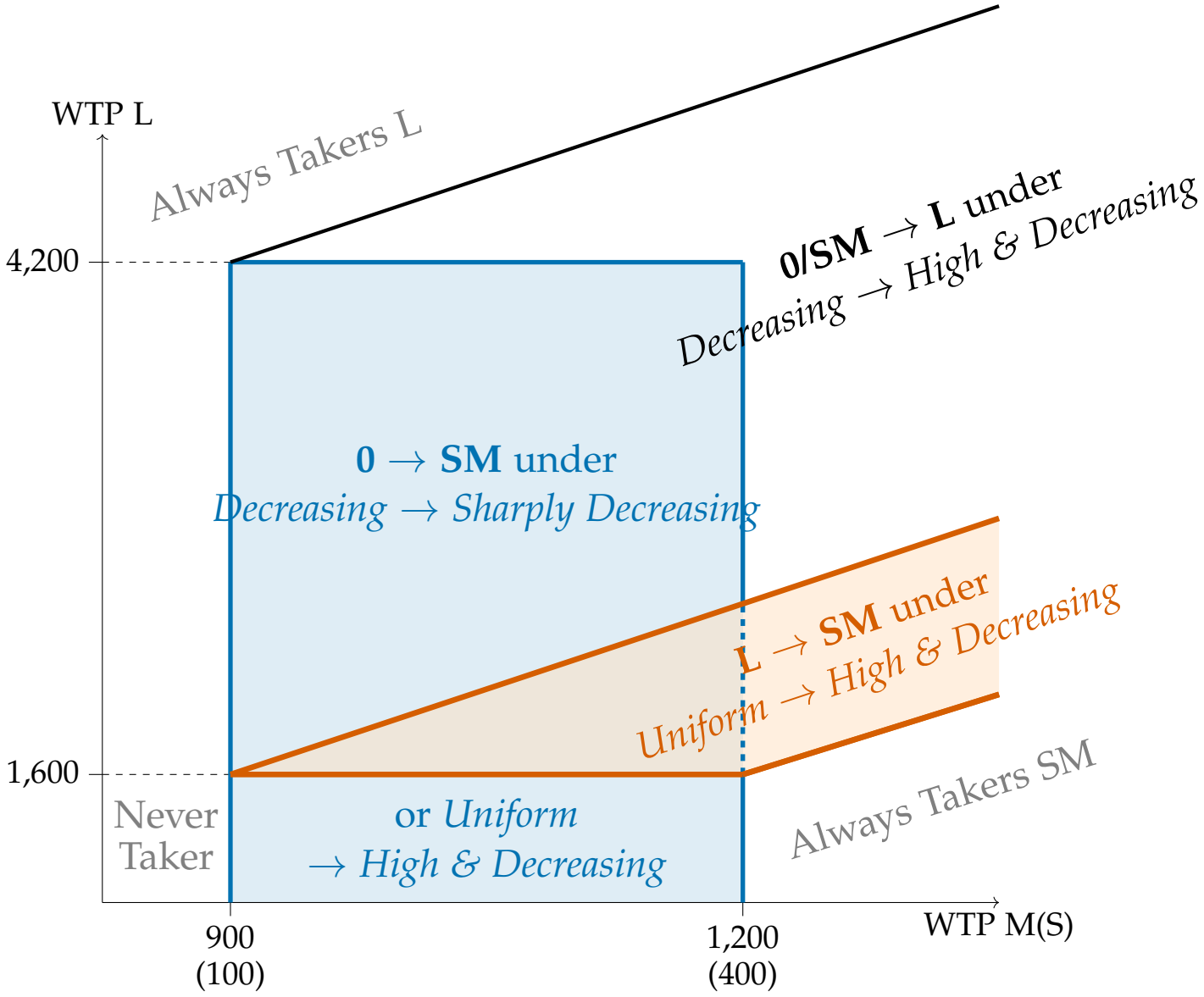


Table D7. Characteristics of Selected Complier Groups

		Compliers group:					
	Population mean	0 → SM	0/SM → L	L → SM	Never takers 0	Always takers SM	Always takers L
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Household size	5.484	5.631	6.216	5.717	5.324	5.464	11.000
	(0.066)	(1.029)	(0.379)	(1.667)	(0.215)	(0.198)	(1.732)
Household head is female	0.195	0.199	0.199	-0.105	0.167	0.192	0.000
	(0.011)	(0.169)	(0.060)	(0.291)	(0.037)	(0.035)	(0.000)
Asset index (0–10)	2.956	2.041	3.248	4.302	2.990	2.896	6.333
	(0.057)	(0.976)	(0.299)	(1.472)	(0.201)	(0.185)	(0.882)
Used pre-harvest mechanized equipment	0.042	0.035	0.155	-0.112	0.049	0.032	0.000
	(0.006)	(0.066)	(0.054)	(0.257)	(0.021)	(0.016)	(0.000)
Used post-harvest processing or transport	0.137	0.190	0.178	0.372	0.098	0.128	0.000
	(0.010)	(0.162)	(0.061)	(0.309)	(0.030)	(0.030)	(0.000)
Used improved seeds	0.088	0.070	0.133	0.365	0.069	0.048	0.333
	(0.008)	(0.111)	(0.057)	(0.297)	(0.025)	(0.019)	(0.333)
Used inorganic fertilizer	0.037	0.017	0.089	-0.054	0.069	0.032	0.000
	(0.005)	(0.064)	(0.040)	(0.185)	(0.025)	(0.016)	(0.000)
Cultivated area (ha)	1.576	1.197	2.313	1.344	1.563	1.532	2.549
	(0.046)	(0.616)	(0.370)	(1.608)	(0.138)	(0.163)	(1.151)
Production value ('000 MZN)	20.931	20.877	33.489	-7.098	21.243	18.305	75.589
	(0.977)	(12.794)	(13.940)	(50.238)	(2.755)	(2.024)	(48.316)
Production value ('000 MZN) *at follow-up*	21.960	16.711	33.786	41.566	18.760	20.969	101.495
	(0.991)	(9.198)	(7.626)	(40.271)	(2.732)	(2.381)	(82.107)
Sold or planning to sell	0.792	0.960	0.911	1.054	0.765	0.776	0.667
	(0.011)	(0.178)	(0.043)	(0.185)	(0.042)	(0.037)	(0.333)
Number of observations	1,254	1,254	638	616	102	125	3
Number of farmers	(1,254)	187	185	616	102	125	3
Effective *F*-statistic		13.5	34.9	2.4			
Instrumented variable:		*Redeemed Small or Redeemed Medium*	*Redeemed Large*	*Redeemed Large*			
Instrument:		*Sharply Decreasing or High & Decreasing*	*High & Decreasing*	*High & Decreasing*			
Reference group:		*Decreasing or Uniform*	*Decreasing*	*Uniform*	*High & Decreasing*	*Uniform*	*Sharply Decreasing*

Notes: Unit of observation: household. Data on characteristics are from our baseline household surveys (unless otherwise noted in italic); data on package choice are from administrative eVoucher records. Sample: households interviewed at follow-up and registered for an eVoucher. The estimates in Columns (2) to (4) are obtained from the second stage of an instrumental variable regression, where the dependent variable is indicated in the leftmost column and the regressors in the bottom rows. We graph the selected complier groups in the space of willingness to pay for Small/Medium and Large in Appendix Figure D6. Column (2) controls for a binary variable equal to one for *Uniform or High & Decreasing* and zero otherwise. Standard errors clustered at the household ('farmer') level in parentheses; *p*-values in brackets. The effective *F*-statistics is based on the weak instrument test of Montiel Olea and Pflueger (2013).

E. ROBUSTNESS

Table E1. Balance in Row Planting at Follow-up

	Row planting (1)
Sharply Decreasing	-0.047
	(0.038)
	[0.216]
Uniform	-0.028
	(0.038)
	[0.458]
High & Decreasing	-0.034
	(0.038)
	[0.369]
Sample mean of the reference group	0.618
Community fixed effects	✓
Number of observations	1,254
Number of farmers	1,254

Notes: Unit of observation: household. Data are from our follow-up household surveys. All regressions are least squares with strata (i.e., community) fixed effects. Standard errors clustered at the household ('farmer') level in parentheses; *p*-values in brackets.

Table E2. Balance of Treatment Assignment in Whole Baseline Sample ($N = 1,538$)

	Mean (SD)				Coefficient (SE) [p]					
	Decreasing	Sharply Decreasing	Uniform	High & Decreasing	Difference					
	(1)	(2)	(3)	(4)	(2)-(1)	(3)-(1)	(4)-(1)	(3)-(2)	(4)-(2)	(4)-(3)
Household size	5.318	5.527	5.462	5.503	0.175	0.118	0.134	-0.076	-0.046	0.056
	(2.339)	(2.260)	(2.315)	(2.446)	(0.162)	(0.165)	(0.165)	(0.164)	(0.171)	(0.170)
					[0.283]	[0.474]	[0.418]	[0.643]	[0.786]	[0.740]
Household head is female	0.208	0.195	0.151	0.207	-0.008	-0.050	0.001	-0.041	0.015	0.053
	(0.406)	(0.397)	(0.359)	(0.406)	(0.029)	(0.028)	(0.029)	(0.028)	(0.029)	(0.028)
					[0.773]	[0.072]	[0.965]	[0.143]	[0.618]	[0.063]
Asset index (0–10)	2.826	2.771	3.178	2.935	-0.081	0.352	0.087	0.372	0.188	-0.249
	(1.955)	(2.005)	(2.023)	(1.985)	(0.138)	(0.132)	(0.140)	(0.140)	(0.143)	(0.144)
					[0.556]	[0.008]	[0.532]	[0.008]	[0.189]	[0.084]
Used pre-harvest mechanized equipment	0.033	0.042	0.045	0.041	0.009	0.014	0.008	0.004	-0.002	-0.004
	(0.180)	(0.200)	(0.208)	(0.200)	(0.013)	(0.014)	(0.013)	(0.014)	(0.013)	(0.014)
					[0.518]	[0.307]	[0.536]	[0.777]	[0.889]	[0.792]
Used post-harvest processing or transport	0.141	0.166	0.135	0.124	0.033	-0.006	-0.022	-0.042	-0.046	-0.013
	(0.348)	(0.373)	(0.342)	(0.330)	(0.026)	(0.024)	(0.024)	(0.025)	(0.025)	(0.024)
					[0.203]	[0.788]	[0.371]	[0.098]	[0.065]	[0.571]
Used improved seeds	0.085	0.078	0.090	0.075	-0.004	0.008	-0.013	0.008	-0.006	-0.014
	(0.279)	(0.268)	(0.287)	(0.264)	(0.019)	(0.021)	(0.020)	(0.019)	(0.019)	(0.020)
					[0.826]	[0.684]	[0.523]	[0.660]	[0.762]	[0.473]
Used inorganic fertilizer	0.028	0.029	0.045	0.039	0.000	0.017	0.011	0.014	0.011	-0.004
	(0.166)	(0.167)	(0.208)	(0.194)	(0.011)	(0.013)	(0.013)	(0.012)	(0.013)	(0.015)
					[0.967]	[0.197]	[0.384]	[0.237]	[0.410]	[0.781]
Cultivated area (ha)	1.416	2.020	1.636	1.477	0.594	0.226	0.046	-0.427	-0.549	-0.114
	(1.422)	(8.567)	(1.987)	(1.442)	(0.451)	(0.124)	(0.098)	(0.466)	(0.454)	(0.118)
					[0.188]	[0.068]	[0.634]	[0.360]	[0.227]	[0.335]
Production value ('000 MZN)	17.540	22.919	20.093	19.883	4.898	2.405	2.166	-3.090	-2.783	0.562
	(24.480)	(37.424)	(28.527)	(38.712)	(2.216)	(1.861)	(2.291)	(2.226)	(2.598)	(2.416)
					[0.027]	[0.197]	[0.345]	[0.165]	[0.284]	[0.816]
Sold or planning to sell	0.754	0.782	0.801	0.795	0.020	0.052	0.040	0.021	0.016	-0.011
	(0.431)	(0.414)	(0.400)	(0.404)	(0.026)	(0.028)	(0.027)	(0.027)	(0.027)	(0.027)
					[0.449]	[0.064]	[0.144]	[0.442]	[0.547]	[0.688]
Omnibus *F*-stat [p]					7.2	3.2	1.4	2.4	1.7	0.0
					[0.007]	[0.075]	[0.242]	[0.123]	[0.187]	[0.907]
Number of observations	390	385	377	386	775	767	776	762	771	763
Number of farmers	390	385	377	386	775	767	776	762	771	763

Notes: Unit of observation: household. Data are from our baseline household surveys. Sample: all households interviewed. Coefficients are obtained from least-squares regressions with strata (i.e., community) fixed effects as in Equation 1, but only comparing two treatments. Standard errors clustered at the household ('farmer') level in parentheses; *p*-values in brackets. Omnibus *F*-statistics are obtained from least-squares regressions with strata (i.e., community) fixed effects, stacking all the variables in the left-most column. 'Pre-harvest mechanized equipment' includes tractor, motor and electric pumps, and sprayer. 'Post-harvest processing or transport' includes grain mill, oil press, motorbike, and truck.

Table E3. Impacts on Main Outcomes, Controlling for Assets

	Production value (1)	Profits (2)	Subsidy payout (3)
Sharply Decreasing	1,407	1,858	628
	(2,088)	(1,908)	(164)
	[0.501]	[0.330]	[0.000]
Uniform	5,902	5,498	1,166
	(2,454)	(2,159)	(206)
	[0.016]	[0.011]	[0.000]
High & Decreasing	262	1,449	1,248
	(1,865)	(1,743)	(188)
	[0.888]	[0.406]	[0.000]
Sample mean of the reference group	18,843	8,715	2,533
Community fixed effects	✓	✓	✓
Number of observations	1,254	1,254	1,254
Number of farmers	1,254	1,254	1,254

Notes: Unit of observation: household. Data on 'Production value' and 'Profits' are from our household surveys; 'Subsidy payouts' are based on administrative eVoucher records. Sample: households interviewed at follow-up and registered for an eVoucher. All regressions are least squares with strata (i.e., community) fixed effects and controlling linearly for the baseline value of asset index—and of the dependent variable in Columns 1 and 2—-as in Equation 1. Standard errors clustered at the household ('farmer') level in parentheses; p-values in brackets. Outcomes are in nominal Mozambican *meticais* (100 MZN $\approx$ 1.5 USD). 'Profits' are defined as the sum of agricultural production value (i.e., the value of harvest regardless of whether it was consumed, sold, or stored using median prices in the whole study sample) during the 2023 primary season minus expenses on improved seeds, inorganic fertilizer, pesticides, tools and land rental, hired labor, and household labor (priced at 60% of the median wage across casual labor days).

Table E4. First Stage on Inputs: Extensive Margin

	Input category				
	Any	*Improved seeds*	*Inorganic fertilizer*	*Pesticides*	*Tools*
	(1)	(2)	(3)	(4)	(5)
Sharply Decreasing	0.078	0.088	0.006	0.059	0.073
	(0.031)	(0.032)	(0.017)	(0.019)	(0.033)
	[0.013]	[0.006]	[0.740]	[0.002]	[0.027]
Uniform	0.045	0.059	0.043	0.032	0.052
	(0.032)	(0.033)	(0.018)	(0.019)	(0.033)
	[0.162]	[0.078]	[0.020]	[0.106]	[0.114]
High & Decreasing	0.071	0.073	0.024	0.041	0.039
	(0.031)	(0.032)	(0.017)	(0.019)	(0.033)
	[0.024]	[0.024]	[0.173]	[0.035]	[0.231]
Sample mean of the reference group	0.599	0.581	0.084	0.050	0.571
Community fixed effects	✓	✓	✓	✓	✓
Number of observations	1,254	1,254	1,254	1,254	1,254
Number of farmers	1,254	1,254	1,254	1,254	1,254

Notes: Unit of observation: household. Data are from administrative eVoucher records. Sample: households interviewed at follow-up and registered for an eVoucher. All regressions are least squares with strata (i.e., community) fixed effects as in Equation 1. Standard errors clustered at the household ('farmer') level in parentheses; p-values in brackets. The dependent variable is equal to one if the household redeemed the input in the column header and to zero otherwise. Intensive-margin estimates are in Table 3 (robustness to alternative samples in Appendix Table D6).

Table E5. Comparison of Baseline Characteristics By Package Choice

	Mean (SD)				Coefficient (SE) [p]					
	No redemption	Small	Medium	Large	Difference					
	(1)	(2)	(3)	(4)	(2)-(1)	(3)-(1)	(4)-(1)	(3)-(2)	(4)-(2)	(4)-(3)
Household size	5.387	5.208	5.982	5.938	-0.194	0.653	0.335	0.993	1.284	-0.213
	(2.195)	(2.236)	(2.701)	(2.410)	(0.210)	(0.306)	(0.328)	(0.258)	(0.325)	(0.409)
					[0.356]	[0.034]	[0.306]	[0.000]	[0.000]	[0.603]
Household head is female	0.198	0.245	0.126	0.124	0.056	-0.035	-0.032	-0.166	-0.223	0.010
	(0.399)	(0.431)	(0.332)	(0.331)	(0.039)	(0.044)	(0.054)	(0.042)	(0.059)	(0.060)
					[0.148]	[0.427]	[0.555]	[0.000]	[0.000]	[0.873]
Asset index (0–10)	2.944	2.543	3.430	3.643	-0.501	0.351	0.659	1.044	1.462	0.415
	(2.033)	(1.896)	(2.028)	(1.911)	(0.179)	(0.237)	(0.307)	(0.204)	(0.296)	(0.298)
					[0.005]	[0.140]	[0.032]	[0.000]	[0.000]	[0.164]
Used pre-harvest mechanized equipment	0.052	0.013	0.054	0.093	-0.013	0.000	0.005	0.027	0.068	0.041
	(0.222)	(0.114)	(0.226)	(0.292)	(0.017)	(0.034)	(0.051)	(0.015)	(0.032)	(0.042)
					[0.445]	[0.993]	[0.924]	[0.087]	[0.036]	[0.324]
Used post-harvest processing or transport	0.128	0.116	0.170	0.186	-0.048	-0.006	0.001	0.091	0.089	-0.071
	(0.335)	(0.321)	(0.377)	(0.391)	(0.034)	(0.049)	(0.060)	(0.034)	(0.054)	(0.055)
					[0.157]	[0.897]	[0.988]	[0.008]	[0.097]	[0.196]
Used improved seeds	0.092	0.050	0.112	0.163	-0.049	-0.022	0.047	0.050	0.101	0.005
	(0.290)	(0.219)	(0.316)	(0.371)	(0.029)	(0.043)	(0.051)	(0.027)	(0.049)	(0.053)
					[0.095]	[0.599]	[0.351]	[0.066]	[0.037]	[0.927]
Used inorganic fertilizer	0.052	0.011	0.054	0.054	-0.020	-0.020	-0.018	0.019	0.015	-0.027
	(0.222)	(0.104)	(0.226)	(0.227)	(0.015)	(0.029)	(0.042)	(0.015)	(0.013)	(0.037)
					[0.176]	[0.491]	[0.664]	[0.204]	[0.239]	[0.466]
Cultivated area (ha)	1.674	1.270	1.775	1.981	-0.269	0.046	0.379	0.417	0.756	0.244
	(1.838)	(0.999)	(1.867)	(2.093)	(0.128)	(0.186)	(0.282)	(0.142)	(0.242)	(0.294)
					[0.037]	[0.807]	[0.179]	[0.003]	[0.002]	[0.407]
Production value ('000 MZN)	22.200	14.872	24.940	31.088	-6.205	0.380	11.822	11.633	12.369	6.019
	(33.337)	(18.333)	(37.040)	(63.370)	(1.844)	(4.018)	(11.724)	(3.464)	(4.590)	(8.691)
					[0.001]	[0.925]	[0.314]	[0.001]	[0.007]	[0.489]
Sold or planning to sell	0.748	0.755	0.870	0.938	-0.022	-0.006	0.105	0.085	0.163	0.067
	(0.434)	(0.431)	(0.337)	(0.242)	(0.039)	(0.045)	(0.041)	(0.039)	(0.057)	(0.044)
					[0.567]	[0.896]	[0.010]	[0.031]	[0.004]	[0.125]
Omnibus *F*-stat [p]					11.3	2.4	2.2	19.4	14.9	0.8
					[0.001]	[0.120]	[0.140]	[0.000]	[0.000]	[0.371]
Number of observations	445	457	223	129	902	668	574	680	586	352
Number of farmers	445	457	223	129	902	668	574	680	586	352

Notes: Unit of observation: household. Data are from our baseline household surveys. Sample: all households interviewed. Coefficients are obtained from least-squares regressions with strata (i.e., community) fixed effects. Standard errors clustered at the household ('farmer') level in parentheses; *p*-values in brackets. Omnibus *F*-statistics are obtained from least-squares regressions with strata (i.e., community) fixed effects, stacking all the variables in the left-most column. 'Pre-harvest mechanized equipment' includes tractor, motor and electric pumps, and sprayer. 'Post-harvest processing or transport' includes grain mill, oil press, motorbike, and truck.

Table E6. Impact on Production Value: Decomposition

	Imputed using sample median prices				Self-reported	
Value of:	*Production*	*Production sold*	*Production stored*	*Production consumed*	*Sales*	*Expected sales*
	(1)	(2)	(3)	(4)	(5)	(6)
Sharply Decreasing	1,012	1,720	-667	1,105	1,275	-1,000
	(2,124)	(1,671)	(493)	(1,041)	(1,807)	(705)
	[0.634]	[0.304]	[0.176]	[0.288]	[0.480]	[0.157]
Uniform	6,770	2,624	943	3,608	2,484	972
	(2,486)	(1,411)	(834)	(1,501)	(1,595)	(992)
	[0.007]	[0.063]	[0.259]	[0.016]	[0.120]	[0.327]
High & Decreasing	229	78	369	243	-1,011	844
	(1,905)	(1,417)	(640)	(884)	(1,514)	(1,077)
	[0.905]	[0.956]	[0.564]	[0.783]	[0.504]	[0.433]
Sample mean of the reference group	18,843	6,696	1,327	10,819	7,592	1,604
Community fixed effects	✓	✓	✓	✓	✓	✓
Number of observations	1,254	1,254	1,254	1,254	1,254	1,254
Number of farmers	1,254	1,254	1,254	1,254	1,254	1,254

Notes: Unit of observation: household. Sample: households interviewed at follow-up and registered for an eVoucher according to FAO administrative records. Outcomes are in Mozambican *meticais* (100 MZN $\approx$ 1.5 USD). Production values refer to the 2023 primary season. All regressions are least squares with community fixed effects and controlling linearly for the baseline value of the dependent variable; if the latter variable is missing, we impute it by taking the sample mean in the estimation sample and further include a binary indicator that the variable is missing. Standard errors clustered at the household ('farmer') level in parentheses.

Table E7. Impact on Area and Production Value: Heterogeneity by Crop Type

	Crop category						**Total**
	Grains	Rice	Tubers	Legumes/ pulses	Vegetables	Cash crops/ oilseeds	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Area							
Sharply Decreasing	0.044	-0.005	0.002	0.125	-0.018	0.032	0.116
	(0.071)	(0.049)	(0.042)	(0.080)	(0.020)	(0.040)	(0.181)
	[0.538]	[0.919]	[0.968]	[0.116]	[0.376]	[0.421]	[0.522]
Uniform	0.075	-0.008	0.035	0.173	0.011	-0.020	0.169
	(0.067)	(0.051)	(0.046)	(0.079)	(0.025)	(0.035)	(0.169)
	[0.263]	[0.871]	[0.444]	[0.028]	[0.653]	[0.562]	[0.320]
High & Decreasing	-0.013	-0.041	0.018	0.047	0.025	0.078	0.064
	(0.058)	(0.044)	(0.042)	(0.064)	(0.030)	(0.055)	(0.159)
	[0.826]	[0.354]	[0.674]	[0.468]	[0.411]	[0.154]	[0.687]
Sample mean of the reference group	0.614	0.328	0.404	0.796	0.064	0.183	2.390
Community fixed effects	✓	✓	✓	✓	✓	✓	✓
Number of observations	1,254	1,254	1,254	1,254	1,254	1,254	1,254
Number of farmers	1,254	1,254	1,254	1,254	1,254	1,254	1,254
Production value							
Sharply Decreasing	355	627	235	1,055	507	-566	1,012
	(675)	(501)	(150)	(1,102)	(712)	(905)	(2,124)
	[0.599]	[0.211]	[0.118]	[0.339]	[0.477]	[0.532]	[0.634]
Uniform	2,561	-62	128	2,415	302	2,032	6,770
	(992)	(263)	(132)	(1,249)	(381)	(1,191)	(2,486)
	[0.010]	[0.814]	[0.330]	[0.053]	[0.428]	[0.088]	[0.007]
High & Decreasing	255	-179	261	584	564	-365	229
	(652)	(301)	(153)	(947)	(651)	(837)	(1,905)
	[0.696]	[0.552]	[0.089]	[0.537]	[0.386]	[0.663]	[0.905]
Sample mean of the reference group	5,135	1,519	488	7,989	547	3,164	18,843
Community fixed effects	✓	✓	✓	✓	✓	✓	✓
Number of observations	1,254	1,254	1,254	1,254	1,254	1,254	1,254
Number of farmers	1,254	1,254	1,254	1,254	1,254	1,254	1,254

Notes: Unit of observation: household. Data on 'Area' and 'Production value' are from our household surveys. Sample: households interviewed at follow-up and registered for an eVoucher. All regressions are least squares with strata (i.e., community) fixed effects—and controlling linearly for the baseline value of the dependent variable in Columns 1 and 2—as in Equation 1; if the latter variable is missing, we impute it by taking the sample mean in the estimation sample and further include a binary indicator that the variable is missing. Standard errors clustered at the household ('farmer') level in parentheses; p-values in brackets. Outcomes are in hectares for the 'Area' panel and in nominal Mozambican *meticais* (100 MZN $\approx$ 1.5 USD) for the 'Production value' panel.

Table E8. Impact on Profits: Robustness to Valuation of Labor

Profits net of expenditures on:	**Temporary labor**			**and permanent labor**		
Shadow wage =	*0 MZN*	*21 MZN*	*35 MZN*	*0 MZN*	*21 MZN*	*35 MZN*
	(1)	(2)	(3)	(4)	(5)	(6)
Sharply Decreasing	1,373	1,189	1,047	1,863	1,635	1,491
	(2,011)	(1,912)	(1,988)	(2,101)	(1,927)	(2,009)
	[0.495]	[0.534]	[0.598]	[0.376]	[0.396]	[0.458]
Uniform	6,822	6,450	6,188	6,429	6,089	5,830
	(2,356)	(2,377)	(2,533)	(2,302)	(2,186)	(2,357)
	[0.004]	[0.007]	[0.015]	[0.005]	[0.005]	[0.014]
High & Decreasing	976	1,077	1,147	805	1,439	1,525
	(1,794)	(1,768)	(1,910)	(1,868)	(1,762)	(1,918)
	[0.587]	[0.543]	[0.548]	[0.666]	[0.414]	[0.427]
Sample mean of the reference group	17,008	9,456	4,421	17,461	8,715	3,680
Community fixed effects	✓	✓	✓	✓	✓	✓
Number of observations	1,254	1,254	1,254	1,254	1,254	1,254
Number of farmers	1,254	1,254	1,254	1,254	1,254	1,254

Notes: Unit of observation: household. Data on 'Profits' and 'Labor' are from our household surveys. Sample: households interviewed at follow-up and registered for an eVoucher. All regressions are least squares with strata (i.e., community) fixed effects—-and controlling linearly for the baseline value of the dependent variable in Columns 1 and 2—as in Equation 1' if the latter variable is missing, we impute it by taking the sample mean in the estimation sample and further include a binary indicator that the variable is missing. Standard errors clustered at the household ('farmer') level in parentheses; p-values in brackets. Outcomes are in nominal Mozambican *meticais* (100 MZN $\approx$ 1.5 USD). 'Profits' are defined as the sum of agricultural production value (i.e., the value of harvest regardless of whether it was consumed, sold, or stored using median prices in the whole study sample) during the 2023 primary season minus expenses on improved seeds, inorganic fertilizer, pesticides, tools and land rental; hired labor is subtracted as indicated in the bold column header while household labor is priced according to the shadow wage in the italic column header.

Table E9. First Stage on Package Choice by Row Planting

	Package type			
	Any	Small	Medium	Large
	(1)	(2)	(3)	(4)
Farmers not row planting				
Sharply Decreasing	0.062	-0.009	0.112	-0.041
	(0.060)	(0.060)	(0.048)	(0.022)
	[0.309]	[0.875]	[0.020]	[0.056]
Uniform	-0.041	-0.077	-0.071	0.106
	(0.062)	(0.063)	(0.041)	(0.032)
	[0.503]	[0.224]	[0.085]	[0.001]
High & Decreasing	0.073	0.002	0.009	0.063
	(0.061)	(0.061)	(0.047)	(0.029)
	[0.227]	[0.980]	[0.851]	[0.030]
Sample mean of the reference group	0.517	0.390	0.119	0.008
Community fixed effects	✓	✓	✓	✓
Number of observations	486	486	486	486
Number of farmers	486	486	486	486
Farmers row planting				
Sharply Decreasing	0.071	-0.060	0.153	-0.022
	(0.041)	(0.046)	(0.042)	(0.025)
	[0.088]	[0.190]	[0.000]	[0.371]
Uniform	0.089	-0.080	-0.075	0.243
	(0.042)	(0.045)	(0.038)	(0.037)
	[0.036]	[0.077]	[0.052]	[0.000]
High & Decreasing	0.065	-0.109	-0.012	0.186
	(0.041)	(0.047)	(0.039)	(0.034)
	[0.111]	[0.019]	[0.762]	[0.000]
Sample mean of the reference group	0.643	0.413	0.194	0.036
Community fixed effects	✓	✓	✓	✓
Number of observations	726	726	726	726
Number of farmers	726	726	726	726

Notes: Unit of observation: household. Data are from administrative eVoucher records; 'row planting' is from our follow-up household surveys. Sample: households interviewed at follow-up and registered for an eVoucher. Subsample in the bold panel header. All regressions are least squares with strata (i.e., community) fixed effects as in Equation 1. Standard errors clustered at the household ('farmer') level in parentheses; *p*-values in brackets. Reduced-form estimates (on input values, production, profits, and subsidy payouts) by row planting are in Table 5.

Table E10. Impacts on Long-Run Adoption: Triple Interaction

	Package type							
	Any		Small		Medium		Large	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Long-run	-0.188	-0.215	-0.121	-0.181	-0.066	-0.048	-0.002	0.015
	(0.020)	(0.035)	(0.017)	(0.031)	(0.014)	(0.022)	(0.006)	(0.007)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.032]	[0.795]	[0.031]
Asset index		0.007		-0.034		0.034		0.006
		(0.009)		(0.009)		(0.008)		(0.004)
		[0.440]		[0.000]		[0.000]		[0.089]
Asset index × Long-run		0.009		0.021		-0.006		-0.006
		(0.010)		(0.008)		(0.007)		(0.003)
		[0.345]		[0.007]		[0.389]		[0.057]
Uniform or High & Decreasing	0.018	0.060	-0.041	-0.020	-0.114	-0.039	0.174	0.119
	(0.024)	(0.042)	(0.024)	(0.044)	(0.020)	(0.033)	(0.016)	(0.026)
	[0.433]	[0.155]	[0.084]	[0.648]	[0.000]	[0.246]	[0.000]	[0.000]
Uniform or High & Decreasing × Long-run	-0.018	-0.034	0.045	0.053	0.050	0.017	-0.113	-0.104
	(0.029)	(0.051)	(0.025)	(0.046)	(0.018)	(0.029)	(0.015)	(0.023)
	[0.534]	[0.497]	[0.068]	[0.250]	[0.007]	[0.565]	[0.000]	[0.000]
Uniform or High & Decreasing × Asset index		-0.014		-0.005		-0.027		0.017
		(0.012)		(0.012)		(0.010)		(0.008)
		[0.246]		[0.700]		[0.008]		[0.032]
Uniform or High & Decreasing × Asset index × Long-run		0.005		-0.004		0.011		-0.002
		(0.014)		(0.012)		(0.009)		(0.007)
		[0.740]		[0.738]		[0.218]		[0.744]
[Uniform or High & Decreasing] +	0.000	0.026	0.004	0.033	-0.064	-0.022	0.061	0.015
[Uniform or High & Decreasing × Long-run]	(0.019)	(0.032)	(0.019)	(0.034)	(0.015)	(0.025)	(0.010)	(0.016)
	[0.984]	[0.419]	[0.839]	[0.333]	[0.000]	[0.386]	[0.000]	[0.372]
[Uniform or High & Decreasing × Asset index] +		-0.009		-0.009		-0.016		0.015
[Uniform or High & Decreasing × Asset index		(0.009)		(0.009)		(0.008)		(0.005)
× Long-run]		[0.310]		[0.360]		[0.053]		[0.005]
Sample mean of the comparison group	0.509	0.509	0.305	0.305	0.188	0.188	0.016	0.016
Community fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Number of observations	3,762	3,762	3,762	3,762	3,762	3,762	3,762	3,762
Number of farmers	1,254	1,254	1,254	1,254	1,254	1,254	1,254	1,254

Notes: Unit of observation: household. Data on package choice are from administrative eVoucher records; 'Asset index' is from our baseline household surveys. Sample: households interviewed at follow-up and registered for an eVoucher. All regressions are least squares with strata (i.e., community) fixed effects. Standard errors clustered at the household ('farmer') level in parentheses; p-values in brackets. Separate estimates on short- and long-run adoption are in Table 6. Estimates from a fully-saturated model, i.e., including the three treatments, are in Appendix Table E11.

Table E11. Impacts on Long-Run Adoption: Fully-Saturated Treatments

	Package type							
	Any		Small		Medium		Large	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Long-run	-0.148	-0.157	-0.106	-0.168	-0.036	-0.001	-0.006	0.012
	(0.029)	(0.051)	(0.025)	(0.046)	(0.019)	(0.030)	(0.009)	(0.009)
	[0.000]	[0.002]	[0.000]	[0.000]	[0.054]	[0.985]	[0.504]	[0.174]
Asset index		0.014		-0.038		0.047		0.006
		(0.013)		(0.013)		(0.011)		(0.005)
		[0.267]		[0.002]		[0.000]		[0.253]
Asset index × Long-run		0.003		0.021		-0.012		-0.006
		(0.014)		(0.012)		(0.010)		(0.004)
		[0.819]		[0.069]		[0.223]		[0.099]
Sharply Decreasing	0.078	0.115	-0.020	-0.044	0.113	0.177	-0.015	-0.018
	(0.033)	(0.058)	(0.034)	(0.061)	(0.030)	(0.048)	(0.012)	(0.017)
	[0.018]	[0.045]	[0.549]	[0.474]	[0.000]	[0.000]	[0.228]	[0.290]
Sharply Decreasing × Long-run	-0.082	-0.112	-0.030	-0.026	-0.061	-0.092	0.009	0.006
	(0.041)	(0.069)	(0.034)	(0.062)	(0.028)	(0.044)	(0.012)	(0.013)
	[0.043]	[0.104]	[0.370]	[0.672]	[0.030]	[0.038]	[0.435]	[0.662]
Sharply Decreasing × Asset index		-0.013		0.007		-0.021		0.001
		(0.017)		(0.017)		(0.016)		(0.007)
		[0.452]		[0.662]		[0.174]		[0.870]
Sharply Decreasing × Asset index × Long-run		0.011		-0.001		0.011		0.001
		(0.019)		(0.016)		(0.014)		(0.006)
		[0.573]		[0.959]		[0.453]		[0.863]
Uniform	0.042	0.095	-0.055	-0.052	-0.097	0.011	0.194	0.136
	(0.034)	(0.062)	(0.034)	(0.065)	(0.025)	(0.044)	(0.024)	(0.038)
	[0.225]	[0.129]	[0.106]	[0.420]	[0.000]	[0.796]	[0.000]	[0.000]
Uniform × Long-run	-0.041	-0.085	0.034	0.058	0.046	-0.025	-0.120	-0.119
	(0.042)	(0.073)	(0.037)	(0.069)	(0.024)	(0.038)	(0.024)	(0.037)
	[0.330]	[0.243]	[0.353]	[0.401]	[0.058]	[0.509]	[0.000]	[0.001]
Uniform × Asset index		-0.018		0.003		-0.038		0.017
		(0.017)		(0.017)		(0.013)		(0.011)
		[0.298]		[0.857]		[0.004]		[0.126]
Uniform × Asset index × Long-run		0.014		-0.010		0.023		-0.000
		(0.020)		(0.017)		(0.012)		(0.011)
		[0.493]		[0.576]		[0.048]		[0.998]
High & Decreasing	0.072	0.141	-0.047	-0.034	-0.022	0.085	0.141	0.090
	(0.033)	(0.062)	(0.033)	(0.063)	(0.028)	(0.046)	(0.021)	(0.033)
	[0.030]	[0.023]	[0.156]	[0.593]	[0.437]	[0.066]	[0.000]	[0.006]
High & Decreasing × Long-run	-0.076	-0.096	0.026	0.022	-0.005	-0.031	-0.097	-0.087
	(0.040)	(0.073)	(0.034)	(0.065)	(0.026)	(0.042)	(0.020)	(0.029)
	[0.061]	[0.188]	[0.441]	[0.733]	[0.832]	[0.467]	[0.000]	[0.003]
High & Decreasing × Asset index		-0.024		-0.004		-0.037		0.017
		(0.018)		(0.017)		(0.015)		(0.011)
		[0.184]		[0.804]		[0.014]		[0.107]
High & Decreasing × Asset index × Long-run		0.007		0.001		0.009		-0.003
		(0.020)		(0.017)		(0.013)		(0.010)
		[0.737]		[0.940]		[0.518]		[0.748]
Sample mean of the comparison group	0.501	0.501	0.330	0.330	0.150	0.150	0.021	0.021
Community fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Number of observations	3,762	3,762	3,762	3,762	3,762	3,762	3,762	3,762
Number of farmers	1,254	1,254	1,254	1,254	1,254	1,254	1,254	1,254

Notes: Unit of observation: household. Data on package choice are from administrative eVoucher records; 'Asset index' is from our baseline household surveys. Sample: households interviewed at follow-up and registered for an eVoucher. All regressions are least squares with strata (i.e., community) fixed effects. Standard errors clustered at the household ('farmer') level in parentheses; *p*-values in brackets. Separate estimates on short- and long-run adoption are in Table 6.