Reshaping Agricultural Subsidies*

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Abstract

We study the optimal shape of agricultural input subsidies. We cross-randomize subsidy rates for small and for large input quantities in Mozambique. Increased subsidy rates for small quantities increase payouts to poorer farmers, but divert farmers from large quantities. Increased subsidy rates for large quantities increase production 36% by increasing input use among more marginally productive farmers. Subsidies overcome both informational and financial constraints. We derive and estimate sufficient statistics to quantify how planner preferences over productivity, transfers, and equity shape optimal subsidies. Under plausible preferences, the most uniform rate we test is preferred.

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1 Introduction

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 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. This shifts which farmers increase input use and could increase agricultural productivity if these 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 households 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 rollout 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 low 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 redemption values 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 redemption values were offered with distinct prices. The *Default* 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 Default, and serve as our reference group. One quarter of farmers were assigned to *Reduced Small/Medium*, a 300 MZN reduction in the price of Small (pay 100 for 4,000) and Medium (pay 900 for 6,500). Reduced Small/Medium involves near-free distribution of Small. One quarter of farmers were assigned to *Reduced Large*, a 2,600 MZN reduction in the price of Large (pay 1,600 for 11,000). With an 85% subsidy for Large, subsidy rates under Reduced Large are approximately uniform across the three quantities. Finally, one quarter of farmers were assigned to *Reduced All*, combining Reduced Small/Medium and Reduced Large for price reductions for all three quantities.

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

Our central finding is that reshaping input subsidies comes with an equity–efficiency tradeoff. Our experimental results reveal that farmers who purchase larger input quantities are more marginally productive. In addition, these farmers are descrip-

[1] We discuss the interpretation of these package sizes in additional detail in Section 2.2 and 2.3. 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).

tively 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 respond to experimental variation in the shape of input subsidies in a manner 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 crowd-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 Reduced Large increased total agricultural production by 36%. We conduct a "compliers" analysis: 19% of farmers shift into Large and these compliers are 77% more productive under Default than the average farmer under Default, so our estimate corresponds to a 102% 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, Reduced Small/Medium and Reduced All had no impacts on agricultural production, despite increasing input use. Reduced Large 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. While 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 Reduced Large 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 challenging to learn and adopt (Cefala et al., 2025a,b). This suggests productive heterogeneity underpins our findings.

The large effects of Reduced Large 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. Reduced Large has persistent impacts on 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 Reduced Large on redemption all but vanish for poorer farmers: financial constraints may also limit adoption.

Our results reveal an equity–efficiency tradeoff, as farmers using more inputs are richer yet more marginally productive; we develop a simple quantitative framework to assess this tradeoff. 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., 2025): 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., 2024; Dizon-Ross and Zucker, 2025).

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, Reduced Small/Medium is preferred to Reduced Large, 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 Reduced Large. Third, there are no planner preferences under which Reduced All is both preferred to Reduced Small/Medium and to Reduced Large. 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 underadoption of productive technologies in development economics (reviewed by, e.g., de Janvry et al., 2017; Magruder, 2018; Suri and Udry, 2022). Underadoption in our context is stark, as we study a highly productive technology with near-zero baseline adoption. Subsidizing underadopted 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), flexibility (Bushong et al., 2025), and facilitated resale (Diop, 2025).

The equity–efficiency tradeoff 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).

2 Setting and Experiment

2.1 Agriculture in Northern Mozambique

We study the rollout 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 second quartile of the region and far below the global average of 4.2 tons per hectare (Appendix Figure E1a). 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 E1b).

Mozambique's very low fertilizer use suggests high returns to increasing adoption of modern inputs. Several African countries have managed to increase modern input use and cereal yields in the last decade (Appendix Figures E1c and E1d). These increases are attributable in part to input subsidy programs (Carter et al., 2021). However, simply increasing fertilizer use is unlikely to have positive returns for every farmer. 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.

Multiple constraints may limit adoption of modern inputs in Mozambique.[3] 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., 2025a,b). Fourth, farmers may be uncertain about or underestimate the productive impacts of fertilizer and improved seeds (Carter et al., 2021).

2.2 Agricultural Input Subsidies

Agricultural input subsidies are an intuitively appealing, albeit costly remedy to underadoption 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). With these objectives in mind, a typical second-generation program involves distribution of vouchers to eligible farmers that can be redeemed for a fixed package of chemical fertilizer and improved seeds at local agrodealers (Jayne and Rashid, 2013).

[3] Suri and Udry (2022) more exhaustively review constraints that limit adoption of productive agricultural technologies in Africa.

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 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.[4]

2.3 The eVoucher

The eVoucher, implemented by FAO in Mozambique, shares many features with the agricultural input subsidy programs described in Section 2.2. 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 A1) rather than a paper voucher.[5]

We highlight two key differences between the eVoucher and the second-generation input subsidy programs described in Section 2.2. First, as eligibility criteria for these programs are rarely followed in practice, the eVoucher features minimal targeting cri-

[4] Duflo et al. (2008) find 120kg/ha maximized farmer 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., 2025), 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.

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

teria: all agricultural households with no 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.

Menu. 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.

Table 1. Input Subsidy Schedule under Default

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 A2a. 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 visit an agrodealer or their sales agent to redeem the eVoucher. A photograph of the redemption process is presented in Appendix Figure A2b. 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.

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.

2.4 Timeline, Sampling, 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 rollout 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.

Sampling. In each community, we sampled up to 12 households for our baseline survey, oversampling households identified by extension agents as likely to be interested in participating in an FFS.[6] 8 of these households were randomly assigned, stratified on FFS-interest, to be prioritized by extension agents for eVoucher registra-

[6] We excluded from our sample households who were not eligible for the eVoucher, that is non-agricultural households and households with a government official.

tion in eVoucher-assigned communities. The additional subsidies we analyze in this paper were randomized across priority-assigned households in eVoucher-assigned communities, and all analysis restricts to this sample.

eVoucher implementation. eVoucher registration reached all 192 eVoucher-assigned surveyed communities in advance of the 2022 secondary season. The experimental additional subsidies, which we describe in Section 2.5, were implemented during registration in advance of the 2023 primary season.[7]

Data (Survey). 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 both our household survey and administrative data in Appendix B. Each survey round covered basic household characteristics, including a member roster and an asset module, knowledge and adoption of agricultural practices, and detailed agricultural modules.[8] The agricultural modules captured seasonal crop production, agricultural labor, input purchases, and agricultural sales. All analysis of impacts on agricultural outcomes uses the 2023 primary season.[9]

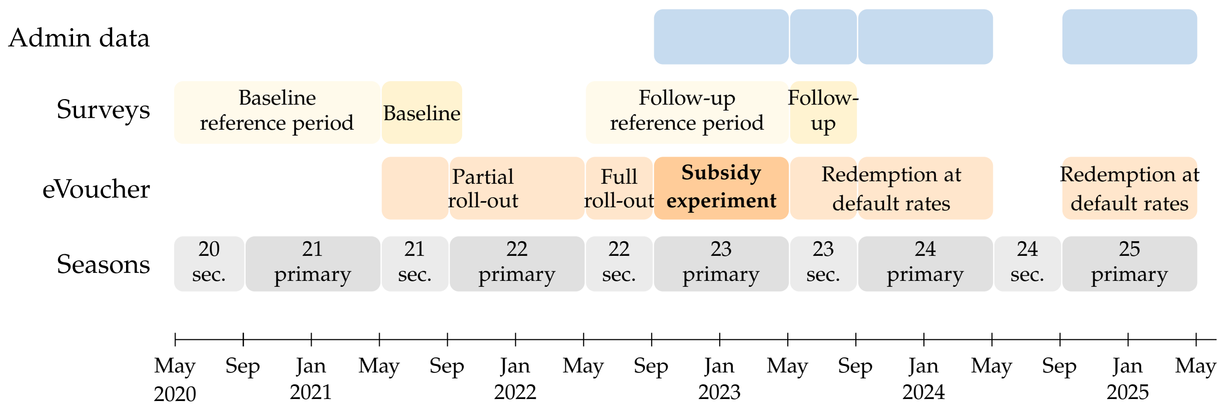
Data (Administrative). In analysis of the impacts of additional subsidies on input demand, we use administrative data on eVoucher registration and redemption. For each redemption, the data include the agricultural season, the package redeemed (Small, Medium, or Large), and the quantities and prices of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

[7] In 111 of the 192 communities, the implementation of the eVoucher began in advance of the 2021 secondary season. We also randomized additional subsidies during the 2022 primary season, but low registration rates yielded a small registered sample (362 households) and survey data referencing this agricultural season is not available, so we do not analyze their impacts. 2022 primary season registration and the assigned additional subsidy during the 2022 primary season are balanced across our experimental arms.

[8] In Section 3.5, 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, and 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 Supplementary Appendix Table F1 and fail to reject no endogeneity of row planting to additional subsidies.

[9] 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.

Figure 1. Timeline



2.5 Experimental Design

We cross-randomize additional subsidies for Small and Medium, and Large, during the 2023 primary season across farmers within our survey sample in each community. We present the four resulting experimental subsidy schedules in Figure 2. Our reference group is the *Default* subsidy schedule, described in Section 2.3. Under *Reduced Small/Medium*, farmers' prices of Small and Medium are reduced by 300 MZN relative to under Default, from 400 MZN and 1,200 MZN to 100 MZN and 900 MZN, respectively. Under *Reduced Large*, the farmers' price of Large is reduced by 2,600 MZN relative to under Default to 1,600 MZN. Under *Reduced All*, farmers receive both sets of price reductions. In almost all communities, our survey sample comprised 8 households, 2 of which we assigned to each of the four experimental arms.[10,11]

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

[10] Before dropping not Registered households, our baseline survey sample comprised 1,538 households in 192 communities: 1 community with 6 households, 2 communities with 7 households, 183 communities with 8 households, and 6 communities with 9 households.

[11] We also randomized our four experimental arms across an additional up to 8 households per community, who we did not survey as discussed in Section 2.4. We report impacts of additional subsidy assignment on package choice (observed in administrative data) when we include these households in Appendix Table E1. Relative to Table 2, estimates are qualitatively similar but slightly more precise.

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 experimentally consistent registration, farmers were only eligible in the eVoucher digital platform to be registered under their experimentally assigned subsidy rates.

Figure 2. Experimental Design

Arm	Value	Price
Default 329 HH's	4,000	400
	6,500	1,200
	11,000	4,200
Reduced Small/Medium 323 HH's	4,000	**100**
	6,500	**900**
	11,000	4,200
Reduced Large 305 HH's	4,000	400
	6,500	1,200
	11,000	**1,600**
Reduced All 323 HH's	4,000	**100**
	6,500	**900**
	11,000	**1,600**

3 Results

3.1 Empirical Strategy

We estimate the impact of the experimentally assigned agricultural input subsidy schedules described in Section 2.5. As subsidy schedules were randomized across households ("farmers") stratified by community, we estimate impacts conditional on community fixed effects. For precision, we additionally control for the baseline (2021 primary season) value of the outcome when it is available.

We restrict our analysis sample to registered farmers, as neither farmers nor extension agents were aware of the experimentally assigned subsidy until the registration process had started. We present evidence that registration is not endogenous, and this restriction does not affect internal validity, in Section 3.2.

We estimate impacts relative to the Default subsidy schedule in the following specification

$$\begin{aligned} Y_{i,t} = \beta^{\text{SM}} \text{Reduced Small/Medium}_i + \beta^{\text{L}} \text{Reduced Large}_i \\ + \beta^{\text{All}} \text{Reduced All}_i + \gamma_{c(i)} + \delta Y_{i,\text{BL}} + \varepsilon_{i,t} \quad (1) \end{aligned}$$

Figure 3. Experimentally Assigned Envelope with Offered Subsidy Rates

(a) Default

Additional eVoucher Subsidy 2022/23

Information Sheet for the Household

PROMOVE AGRIBIZ

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.

i2i DIME TRANSFORM DEVELOPMENT

(b) Reduced Small/Medium

Additional eVoucher Subsidy 2022/23

Information Sheet for the Household

PROMOVE AGRIBIZ

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.

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	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.

i2i DIME TRANSFORM DEVELOPMENT

(c) Reduced Large

Additional eVoucher Subsidy 2022/23

Information Sheet for the Household

PROMOVE AGRIBIZ

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.

i2i DIME TRANSFORM DEVELOPMENT

(d) Reduced All

Additional eVoucher Subsidy 2022/23

Information Sheet for the Household

PROMOVE AGRIBIZ

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.

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	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.

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where $Y_{i,t}$ is the outcome of interest of farmer i in period t. $\gamma_{c(i)}$ are community fixed effects, and $Y_{i,\text{BL}}$ is the baseline value of the outcome. Our analysis focuses on the impacts of Reduced Small/Medium (β^{SM}), Reduced Large (β^{L}), and Reduced All (β^{All}), relative to Default. As assignment probabilities to each subsidy schedule were identical across randomization strata, linear regression recovers unbiased estimates of treatment effects (Goldsmith-Pinkham et al., 2024). We cluster standard errors at the farmer level, following the recommendation by De Chaisemartin and Ramirez-Cuellar (2024) for small-strata experiments with one observation per unit-of-randomization.

3.2 Experimental Validity

We implement three tests of experimental validity: no differential sampling, in-sample balance, and no differential attrition.

First, as our analysis sample conditions on registered farmers, we test for and find no evidence of differential registration in Appendix Table E2. This is consistent with the implementation of the experiment, as households and extension agents did not learn households' assigned subsidy schedule until immediately after registration.

Second, we consistently fail to reject that household characteristics are balanced at baseline with respect to assigned subsidy schedule in Appendix Table E3.[12] Across 66 tests, we reject balance at the 10 percent level for only 5 tests, as one would expect under random assignment. 3 of the tests that reject are for balance on asset index; in Supplementary Appendix Table F3, we show our main results are robust to controlling for asset index.

Third, we find no evidence of differential attrition in Appendix Table E4. Average attrition is low, at only 2% in our analysis sample.

[12] We also test for balance in the full baseline sample in Supplementary Appendix Table F2, with qualitatively identical results.

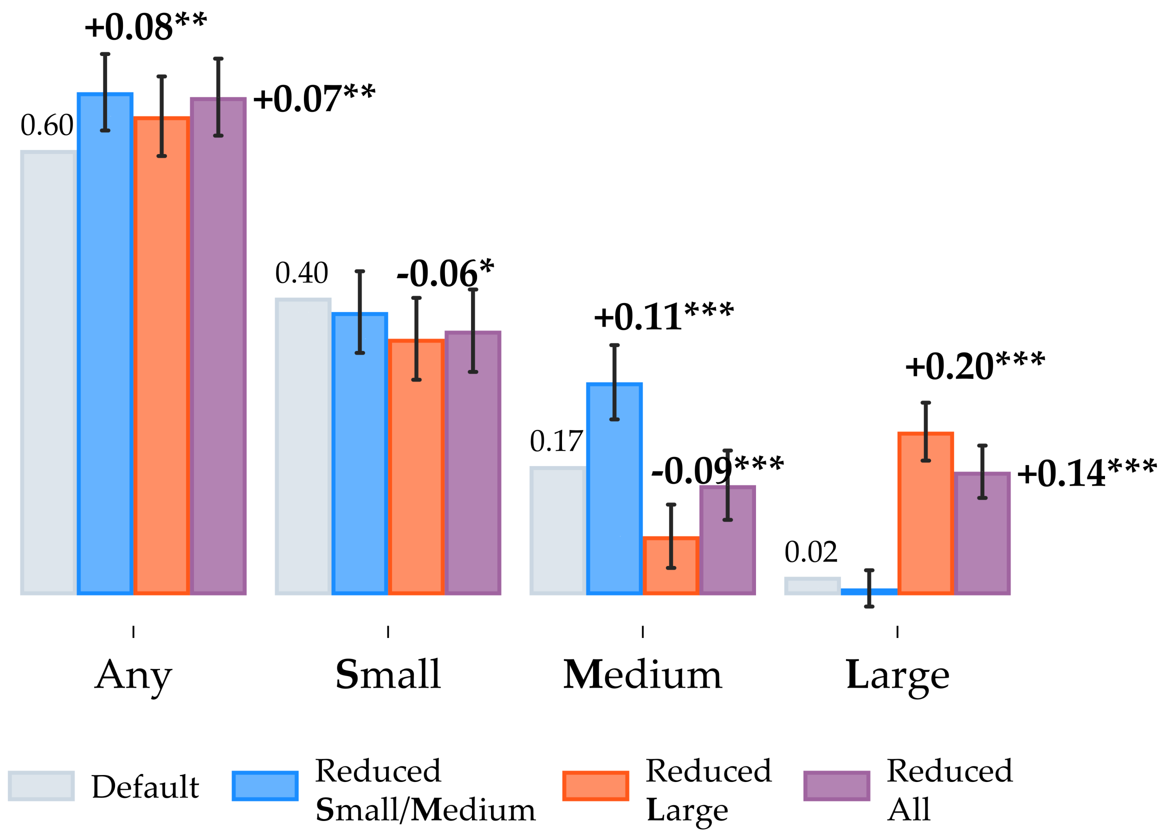
3.3 Input Demand ("First Stage")

We begin by describing input choices under the Default subsidy schedule: takeup is high, and farmers take advantage of the flexibility over both the value and types of inputs. First, 60% of registered farmers "redeem", that is purchase inputs through the eVoucher. This is substantially higher than the 41% redemption observed in Carter et al. (2021) in Mozambique, and substantially lower than 95% redemption observed in Kumar et al. (2025) in Malawi, both for a fixed input voucher for input quantities and at prices in between Medium and Large. Second, farmers appear to respond strongly to the 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, that is choosing Small under Default, is desirable for a majority of farmers, and the availability of this option may be responsible for high takeup. In contrast, paying 3,000 MZN for an additional 4,500 MZN of inputs, that is shifting from Medium to Large under Default, 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 equipment. In Appendix Figure E2, we show average value redeemed of equipment is relatively constant across package sizes, while average value redeemed of inorganic fertilizer sharply increases across package sizes.

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

Intuitively, additional subsidies increase demand for additionally subsidized quantities, by inducing substitution away from adjacent quantities. First, Reduced 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, Reduced Small/Medium increases redemption of Small and Medium by 9.4pp. Substitution is primarily from non-redemption (-7.8pp), as few farmers redeem Large under Default. Third, under Reduced All, additional subsidies for smaller and larger input quantities substitute, with partially offsetting effects. Relative to Reduced Small/Medium, Reduced All increases redemption of Large by 15.7pp, by inducing substitution away from Small and Medium (-16.4pp). In contrast, relative to Reduced Large, Reduced All increases redemption of Small and Medium by 7.9pp,

Figure 4. First Stage



by inducing substitution away from Large (−5.4pp). As a result, the impacts of Reduced All on redemption of smaller and larger input quantities are in between those of Reduced Small/Medium and Reduced Large.

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 Reduced Large, Reduced All 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 3.4, we interpret productive impacts (or lack thereof) as attributable to changes in input use and any complementary re-optimization. In Appendix Table E5, 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.[13] 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%.[14] We present one possible explanation of the differences in impacts of additional subsidies for smaller and larger quantities in Section 3.5; 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 purchase inputs, or who purchase more inputs, are on average wealthier and more productive than farmers who do not (Duflo et al., 2011). In Supplementary Appendix Table F4, 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 4.

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

	Package type			
	Any (1)	**S**mall (2)	**M**edium (3)	**L**arge (4)
Reduced **S**mall/**M**edium	0.078**	-0.019	0.113***	-0.016
	(0.031)	(0.033)	(0.030)	(0.015)
	[0.013]	[0.560]	[0.000]	[0.291]
Reduced **L**arge	0.045	-0.055*	-0.094***	0.195***
	(0.032)	(0.033)	(0.026)	(0.024)
	[0.162]	[0.097]	[0.000]	[0.000]
Reduced All	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 default group	0.599	0.401	0.174	0.025
Number of observations	1,254	1,254	1,254	1,254
Number of farmers	1,254	1,254	1,254	1,254

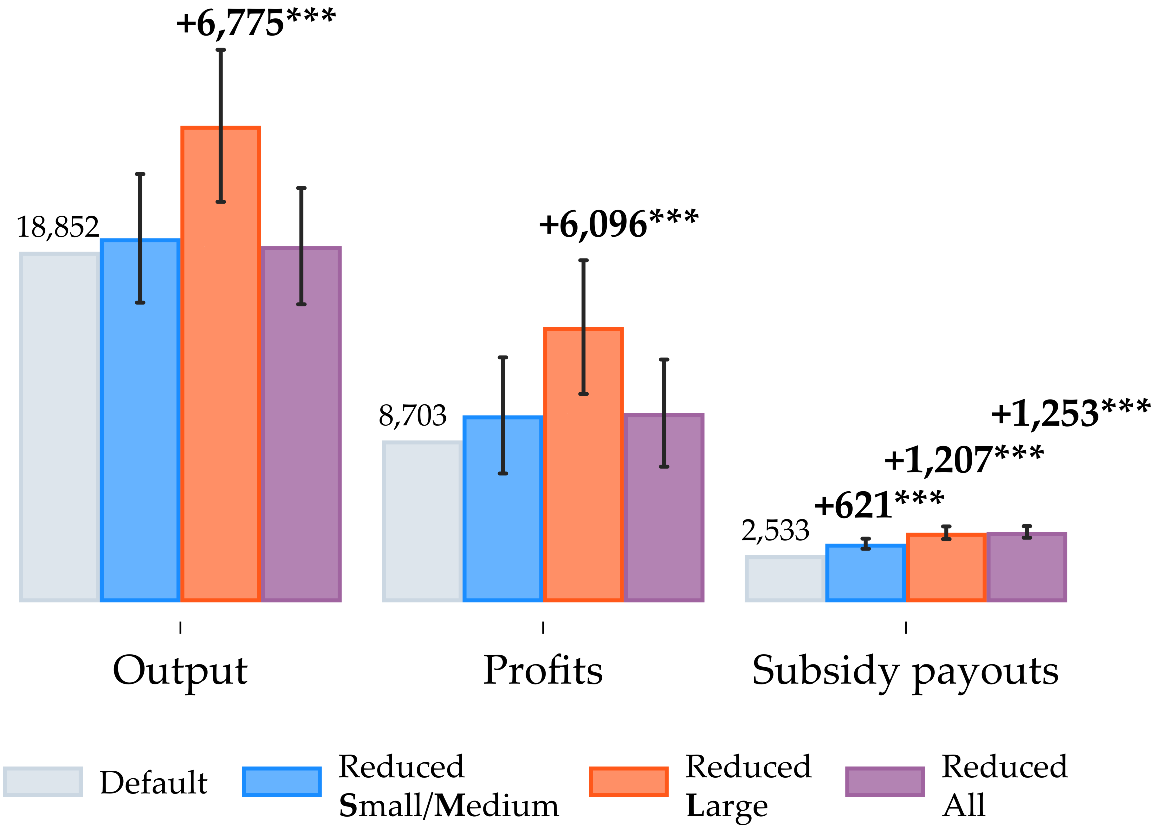
Notes: *Significant at 10%. **Significant at 5%. ***Significant at 1%. Unit of observation: household. Data are from administrative eVoucher records. Sample: households interviewed at follow-up and registered for an eVoucher. All regressions are least squares with strata (i.e., community) fixed effects as in Equation 1. Standard errors clustered at the farmer level in parentheses; *p*-values in brackets. Appendix Table E1 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).

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*	*Insecticide/ pesticides*	*Equipment*
	(1)	(2)	(3)	(4)	(5)
Reduced **S**mall/**M**edium	679***	646***	4	42***	-13
	(262)	(171)	(118)	(12)	(92)
	[0.010]	[0.000]	[0.970]	[0.001]	[0.889]
Reduced **L**arge	1,414***	843***	342**	18**	210**
	(324)	(231)	(139)	(9)	(94)
	[0.000]	[0.000]	[0.014]	[0.047]	[0.025]
Reduced All	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 default group	3,279	1,713	466	16	1,083
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: *Significant at 10%. **Significant at 5%. ***Significant at 1%. 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 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 E6 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).

Figure 5. Impacts on Main Outcomes



3.4 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 Reduced Large increases agricultural production by 36% (6,800 MZN).[15] 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 compliers analysis. Our estimates imply farmers who increase input use under Reduced

[13] Input value is significantly larger under Reduced Large and Reduced All than under Reduced Small/Medium, while input values under Reduced Large and Reduced All are not statistically significantly different from one another.

[14] In Supplementary Appendix Table F5, we show that proportional impacts are much smaller on the extensive margin. Extensive margin impacts on improved seeds and equipment are almost identical to impacts on any redemption, as almost all farmers redeem equipment 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.

[15] In Supplemental Appendix Table F6 and F7, we decompose this effect. Reduced Large significantly increases both consumption and sales of agricultural production, and significantly increases production of cash crops and oilseeds, legumes and pulses, and grains.

Large relative to Default increase their agricultural production from 33,900 MZN to 68,600 MZN, a 102% increase.[16] 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 Reduced Small/Medium and Reduced All on agricultural production. The lack of impacts of Reduced Small/Medium on production suggests that Reduced Small/Medium-compliers may be less marginally productive than Reduced Large-compliers. It is perhaps more surprising that Reduced All did not increase production, given the large productive impacts of Reduced Large. This suggests that the "diverted" farmers, who would have taken up Large under Reduced Large but instead take up Small or Medium under Reduced All, were particularly marginally productive. As a consequence, the additional subsidies for smaller and larger quantities we test appear to substitute in optimal policy: under Reduced All, additional subsidies for smaller quantities erode the productive impacts of additional subsidies for larger quantities.

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.[17][18][19] Reduced Large increases profits net of input value by 4,900 MZN, over four times larger than impacts on subsidy payouts (1,200 MZN) and three times larger than impacts on input value (1,400 MZN). Impacts of Reduced Small/Medium and Reduced All on profits remain small and statistically insignificant.

We highlight three qualitative interpretations of these findings, which we quantitatively test in sequence.[20] First, the large impacts of Reduced Large on profits appear

16 We report estimates of average outcomes under Default for Reduced Large-"compliers" in Appendix Table E7.

17 Our preferred construction of profits subtracts non-eVoucher expenditures on variable inputs, equipment and land rental, and 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 Supplemental Appendix Table F8, we show profit impacts are robust to alternative valuations of labor.

18 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 3.5), but row planting requires substantial skilled labor (Cefala et al., 2025b).

19 Land tenure in our setting is predominantly informal, so farmers could potentially increase area cultivated without purchasing or renting land. In Supplemental Appendix Table F7, we do not find any significant impacts on total area cultivated.

20 The formalization of these tests is presented in Appendix C.3.

inconsistent with profit maximization. Second, the farmers increasing their input use in response to Reduced Large 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").

Do farmers profit maximize? Under profit maximization, any change to subsidy rates from Default should reduce profits when inputs are priced at Default subsidy rates. We strongly reject profit maximization for Reduced Large; Reduced Large increases profits when inputs are priced at Default subsidy rates by 5,500 MZN. To quantitatively interpret this rejection, we estimate bounds on the "wedge" between farmers' willingness to pay for inputs and their impacts on profits; wedges are zero under profit maximization. We note that complier average wedges-plus-one are bounded between the ratio of impacts on profits gross of inputs to impacts on input expenditures valued at either reference prices or comparison prices. Our estimates imply an average wedge of at least 8 for Reduced Large-compliers. This point estimate comes with economically meaningful uncertainty; we cannot reject a value of 1.5, our preferred calibration in our welfare analysis in Section 4.1.2.

Are marginal products of inputs heterogeneous? We test for heterogeneity in marginal products by using our three experimental arms as instruments for input value and running an overidentification test, with either agricultural production or profits gross of inputs as an outcome.[21] We strongly reject the null of homogeneous marginal products in both cases ($p = 0.036$ and $p = 0.095$, respectively).

Are deviations from profit maximization heterogeneous? One potential explanation of heterogeneity in marginal products of inputs is that marginal prices are heterogeneous under Default; 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 complier average wedges for Reduced Large is above the upper bound on complier average wedges for Reduced Small/Medium and Reduced All; we reject the null of homogeneous wedges for Reduced All ($p = 0.011$), but fail to reject for

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

Reduced Small/Medium ($p = 0.383$) albeit with less power.

This last finding implies that, at least among farmers who shift their input use under Reduced All relative to Reduced Large, 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 E3, we find the largest impacts on profits of Reduced Large on higher quantiles of the profit distribution.

3.5 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 fertilizer, but hard to learn (Cefala et al., 2025a,b). Moreover, inorganic fertilizer is complementary to the use of improved seeds (Suri, 2011). In other words,

Table 4. Impacts on Main Outcomes

	Production value	Profits: *Gross of inputs*	Profits: *Net of copay at realized prices*	Profits: *Net of copay at default prices*	Profits: *Net of input value*	Subsidy payout
	(1)	(2)	(3)	(4)	(5)	
Reduced **S**mall/**M**edium	723	1,169	1,347	1,152	741	621***
	(2,095)	(1,894)	(1,892)	(1,894)	(1,903)	(165)
	[0.730]	[0.537]	[0.477]	[0.543]	[0.697]	[0.000]
Reduced **L**arge	6,775***	6,164***	6,096***	5,519**	4,893**	1,207***
	(2,478)	(2,174)	(2,178)	(2,176)	(2,184)	(207)
	[0.006]	[0.005]	[0.005]	[0.011]	[0.025]	[0.000]
Reduced All	301	1,312	1,472	888	236	1,253***
	(1,895)	(1,753)	(1,747)	(1,745)	(1,746)	(189)
	[0.874]	[0.454]	[0.400]	[0.611]	[0.893]	[0.000]
Over-identification test						
Hansen J statistic	6.6	4.7	4.2	4.4	4.5	
p-value	0.036	0.095	0.120	0.108	0.103	
Sample mean of the default group	18,852	9,177	8,703	8,703	6,171	2,533
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: *Significant at 10%. **Significant at 5%. ***Significant at 1%. 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. Standard errors clustered at the farmer level in parentheses; *p*-values in brackets. Outcomes are in nominal Mozambican *meticais* (100 MZN ≈ 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, herbicide, insecticide, pesticides, equipment 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.

adoption of row planting transforms the agricultural technology in a manner that increases optimal use of improved seeds and inorganic fertilizer.[22]

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. Descriptively under Default, these two groups of farmers appear very different: row planters use 50% more inputs, 215% more fertilizer, have 47% higher output, and earn 57% 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.

3.6 Why is Marginal Productivity so High?

A key finding in Section 3.4 is that the large productive impacts of Reduced Large 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 between 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, suggestive of financial constraints.

[22] One might be inclined to apply this evidence to suggest targeted distribution of inputs to row-planting farmers could be an optimal policy. However, targeting on row planting 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

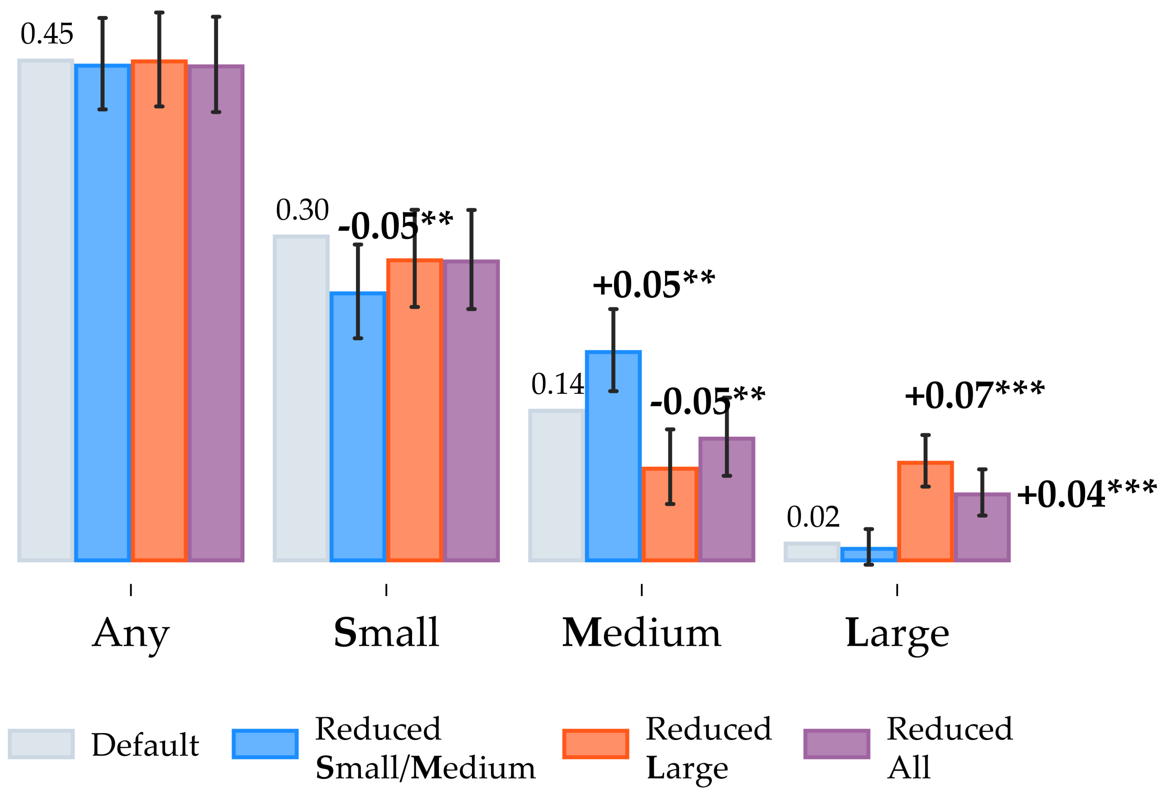
	Input values			Main outcomes		
	Any	Improved seeds	Chemical fertilizer	Production value	Profits	Subsidy payout
	(1)	(2)	(3)	(4)	(5)	(6)
Households not row planting						
Reduced **S**mall/**M**edium	1,043*	818**	78	-1,077	-739	369
	(533)	(335)	(189)	(3,071)	(2,814)	(297)
	[0.051]	[0.015]	[0.678]	[0.726]	[0.793]	[0.215]
Reduced **L**arge	484	278	-37	3,085	3,032	359
	(435)	(280)	(157)	(2,923)	(2,721)	(316)
	[0.267]	[0.323]	[0.814]	[0.292]	[0.266]	[0.256]
Reduced All	866**	644**	114	-1,474	995	828**
	(416)	(278)	(142)	(2,579)	(2,446)	(320)
	[0.038]	[0.021]	[0.421]	[0.568]	[0.684]	[0.010]
Sample mean of the default group	2,455	1,366	176	14,141	6,004	2,090
Number of observations	486	486	486	486	486	486
Number of farmers	486	486	486	486	486	486
Households row planting						
Reduced **S**mall/**M**edium	513	563**	5	1,907	3,114	642***
	(357)	(236)	(191)	(3,449)	(3,012)	(235)
	[0.151]	[0.017]	[0.980]	[0.581]	[0.302]	[0.007]
Reduced **L**arge	2,088***	1,209***	601***	9,868**	9,068***	1,708***
	(497)	(368)	(226)	(3,950)	(3,346)	(301)
	[0.000]	[0.001]	[0.008]	[0.013]	[0.007]	[0.000]
Reduced All	1,589***	795***	720***	1,369	2,688	1,552***
	(385)	(246)	(227)	(2,955)	(2,680)	(269)
	[0.000]	[0.001]	[0.002]	[0.643]	[0.316]	[0.000]
Sample mean of the default group	3,680	1,927	556	20,786	9,434	2,758
Number of observations	726	726	726	726	726	726
Number of farmers	726	726	726	726	726	726

Notes: *Significant at 10%. **Significant at 5%. ***Significant at 1%. 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. 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. Standard errors clustered at the 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, herbicide, insecticide, pesticides, equipment 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 Supplementary Appendix Table F9.

These results imply that learning can partly, but not fully, explain wedges between willingness to pay and profits.

We present our test of learning in Figure 6. Additional subsidies continue to have significant impacts on adoption in the two years after they ended.[23] 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 Reduced Large on willingness to pay, which we attribute to learning. The long-run adoption impact of Reduced Large is 38% of the short-run adoption impact, while the price of Large is 163% higher under Default and in the long run, suggesting willingness to pay increased by 62%.

Figure 6. Impacts on Long-Run Adoption



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.[24] If additional subsidies over-

[23] Coefficients are reported in Supplementary Appendix Table F11.

[24] We pool Reduced Large or All, relative to a pooled reference group of Default or Reduced Small/Medium, and focus on Large adoption for power and parsimony; results are similar in a specification estimating effects separately for each experimental arm in Appendix Table F11.

come 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 households are unlikely to face binding credit constraints when purchasing agricultural inputs. With this caveat, we find that the impacts of additional 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 4.

4 Quantitative Framework

The results above reveal an equity–efficiency tradeoff. Increasing subsidy rates for large quantities of inputs under Reduced Large 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 tradeoff 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 households do not fully internalize the productive impacts of agricultural inputs, formalized in Appendix C.1. We link the MVPF in this framework to sufficient statistics identified by our experiment in Section 4.1. 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 4.2.

4.1 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 agri-

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

	Package type			
	Any		**L**arge	
	Short run	*Long run*	*Short run*	*Long run*
	(1)	(2)	(3)	(4)
Reduced **L**arge or All	0.047	0.037	0.113***	0.018
	(0.041)	(0.031)	(0.027)	(0.016)
	[0.259]	[0.245]	[0.000]	[0.273]
Asset index	0.005	0.018***	0.006	0.001
	(0.009)	(0.007)	(0.005)	(0.003)
	[0.548]	[0.008]	[0.229]	[0.716]
Reduced **L**arge or All × Asset index	-0.009	-0.013	0.020**	0.014***
	(0.012)	(0.009)	(0.009)	(0.005)
	[0.454]	[0.143]	[0.020]	[0.008]
Predicted effect on:				
25th percentile of Asset index	0.038	0.023	0.133***	0.032***
	(0.032)	(0.025)	(0.021)	(0.012)
	[0.238]	[0.345]	[0.000]	[0.010]
75th percentile of Asset index	0.002	-0.031	0.212***	0.087***
	(0.033)	(0.027)	(0.026)	(0.015)
	[0.946]	[0.258]	[0.000]	[0.000]
Sample mean of the comparison group	0.599	0.453	0.025	0.019
Number of observations	1,254	2,508	1,254	2,508
Number of farmers	1,254	1,254	1,254	1,254

Notes: *Significant at 10%. **Significant at 5%. ***Significant at 1%. 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 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 1 for the 2024 and 2025 primary seasons, and 0 for the 2023 primary season) are in Supplementary Appendix Table F10. Estimates from a fully-saturated model, i.e., including the three treatment assignments, are in Supplementary Appendix Table F11.

cultural production for poorer households relative to richer households. The planner also directly values productive efficiency, because they believe that households underestimate or undervalue the productive impacts of inputs. Our findings of very high returns to inputs from Reduced Large (Section 3.4) and that temporary additional subsidies had persistent impacts on input use (Section 3.6) strongly suggest that households underestimated or undervalued the productive impacts of inputs.

Given the planner's problem, we rank the optimality of the three treatment arms using the MVPF. The MVPF is simply the ratio of households' willingness to pay for a policy to the policy's cost to the government. Formally, the MVPF ranks policies in 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 households' behavioral responses to households' private willingness to pay (Hahn et al., 2025). Our context instead features *internalities*: when households 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 C.1, we show that uninternalized productive impacts are weighed by the household's willingness to pay for agricultural production, and added to households' 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.

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

We estimate the MVPF using three sufficient statistics identified by our experiment, whose estimation we discuss in Section 4.1.1. 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 4.1.2. First, γ, the planner's direct weight on productive efficiency. And second, λ, the vector of social marginal utility weights themselves, which we parametrize below as a function of the planner's inequality aversion η and households' baseline wealth.

4.1.1 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.[25] We begin by constructing naive bounds: the lower bound is the change in consumer surplus if choices are fixed as under Default, and the upper bound is the change in consumer surplus if choices were fixed as under treatment. For instance, for Reduced All, the naive lower bound is 300 MZN times the fraction adopting Small/Medium under Default, plus 2,600 MZN times the fraction adopting Large under Default. For comparison, the naive upper bound is 300 MZN times the fraction adopting Small/Medium under Reduced All, plus 2,600 MZN times the fraction adopting Large under Reduced All.

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 Reduced All, including those who choose Large under Reduced Large, cannot gain more than 300 MZN of consumer surplus from Reduced All relative to Default. We apply this to sharpen the upper bound on consumer surplus from Reduced Large relative to Default. Second, anyone who chooses Large under Default, including those who do not under Reduced Small/Medium, gains at least 2,300 MZN of consumer surplus from Reduced Large or Reduced All relative to Reduced Small/Medium. We apply this to sharpen the lower bound on consumer

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

surplus from Reduced Large or Reduced All relative to Reduced Small/Medium.

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 social-marginal-utility-weighted treatment effects.

Treatment Effect on Subsidy Payouts. Treatment effects on subsidy payouts were presented in Table 4.

4.1.2 Calibrated Parameters for Marginal Value of Public Funds

Direct Weight on Productive Efficiency γ. When households underestimate or undervalue the productive impacts of inputs, a paternalistic planner directly incorporates productive impacts into benefits. In Appendix C.1, we show that the planner's weight γ on productive impacts is the product of a discount factor, households' willingness to pay for agricultural production, and a wedge, the fraction of productive impacts that households 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.[26] Second, in Section 3.6 we estimated that Reduced Large increased willingness to pay by approximately one half, which we interpret as evidence that households underestimate the returns to inputs by at least one third. Multiplying the discount factor by the wedge yields our preferred value of $\gamma = 0.2$; in Section 4.2 we consider $\gamma \in [0.0, 0.3]$.

Inequality Aversion η and Social Marginal Utility λ_i. We calculate social marginal utility weights following Eden and Freitas (2024a,b), who recommend using 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 household

[26] 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).

i's consumption. As we did not include a consumption module in our household survey, we instead use households' baseline asset index to construct predicted log consumption, $\widehat{\log C_i}$.[27] In Section 4.2, we apply social marginal utility weights $\lambda_i \propto \exp(-\eta \widehat{\log C_i})$, and we consider $\eta \in [0, 2]$.

4.2 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.

Our sufficient statistics descriptively replicate the equity–efficiency tradeoff. Lower bounds on consumer surplus, relative to impacts on subsidy payouts, are largest for Reduced Small/Medium, and smallest for Reduced Large. Introducing inequality aversion amplifies these differences: consumer surplus from Reduced Large and Reduced All falls sharply, because wealthier households are more likely to choose Large, while consumer surplus from Reduced Small/Medium is unaffected. When productive impacts are ignored, Reduced Small/Medium appears optimal. In contrast, productive impacts are largest under Reduced Large. While the social-marginal-utility-weighted impacts of Reduced Large on production are smaller than the unweighted, weighted impacts remain large relative to subsidy payouts. When consumer surplus is ignored, Reduced Large appears optimal.

At our preferred calibration of the planner's preferences, the MVPF of Reduced Large is at least 50% larger than the MVPF of Reduced Small/Medium and Reduced All. The bounds on the MVPF for Reduced Large 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

[27] 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 D.

Table 7. Welfare

	Comparison		
	Reduced **S**mall/**M**edium *versus* Default	Reduced **L**arge *versus* Default	Reduced All *versus* Default
	(1)	(2)	(3)
Panel A: Impacts on ...			
Production			
Unweighted	723	6,775***	301
λ_i-weighted ($\eta = 1$)	-178	4,383**	446
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]
$\gamma = 0.20$	[0.51, 0.55]	[1.18, 1.50]	[0.24, 0.52]
λ_i-weighted ($\eta = 1$)			
$\gamma = 0$	[0.28, 0.32]	[0.03, 0.31]	[0.16, 0.42]
$\gamma = 0.20$	[0.22, 0.26]	[0.75, 1.04]	[0.23, 0.49]

Notes: *Significant at 10%. **Significant at 5%. ***Significant at 1%. 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.

MVPF of Reduced Large in opposing directions. Eliminating inequality aversion increases the MVPF for Reduced Large by 50%, as productive impacts are concentrated among wealthier households. The proportional increase is equal to the benefit-weighted average social marginal utility, and implies that the average Reduced Large beneficiary has 40% higher consumption than the average household. Absent weight on productive impacts, the MVPF is equal to the ratio of consumer surplus to subsidy payouts. Consequentially, without weight on productive impacts, Reduced Small/Medium 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.[28] We highlight two key takeaways from Figure 7.

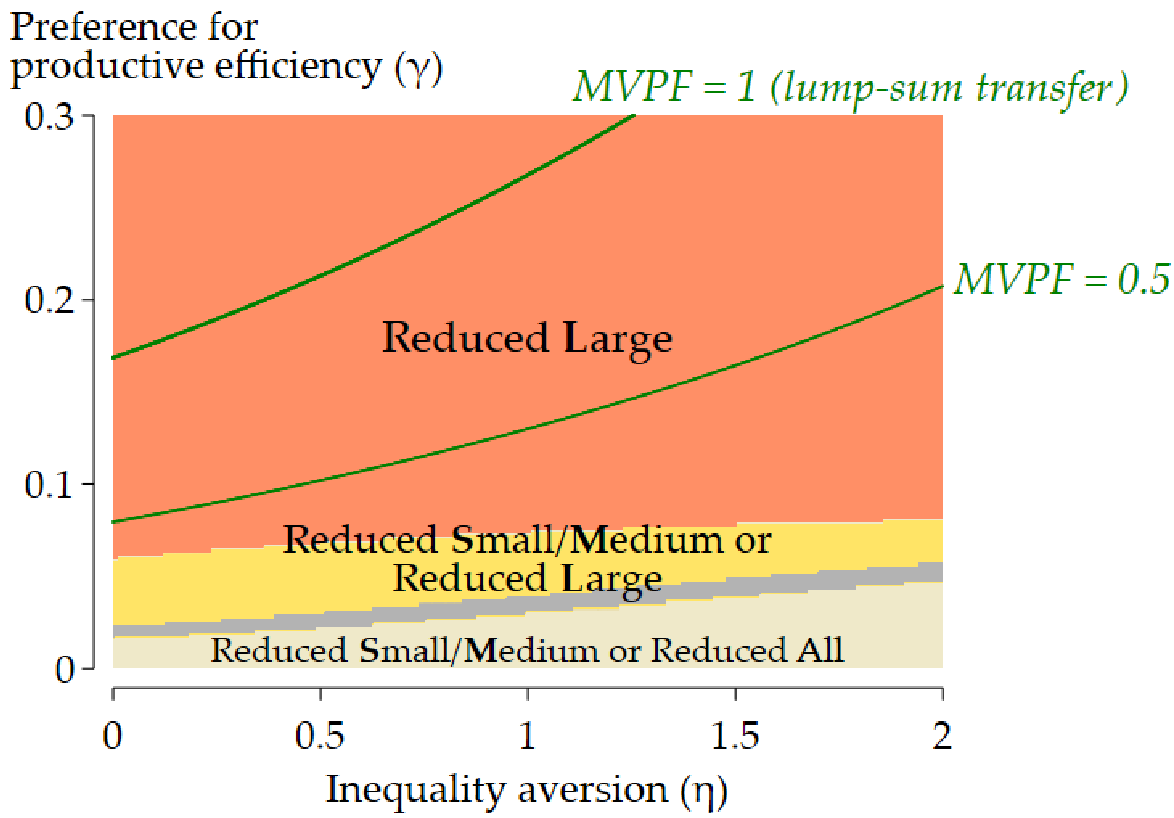
First, the planner's optimal policy is robust to the choice of inequality aversion, and is instead primarily driven by the weight placed on productive impacts. With a weight on productive impacts that is even half of our preferred value, Reduced Large is optimal at all values of inequality aversion we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

Second, we apply the insight from Björkegren et al. (forthcoming) 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 Reduced Small/Medium, 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 discount rates, rather than wedges, or faces alternative constraints potentially linked to political economy or state capacity. In contrast, a planner who chooses a uniform subsidy rate, similar to Reduced Large, places at least a moderate weight on productive impacts.

Third, Reduced All compares poorly to both Reduced Large and Reduced Small/Medium at almost all values of the planner's preferences. With even a very small weight placed on productive impacts, Reduced Large is preferred to Reduced All. The additional subsidy for Large under Reduced All requires a high subsidy pay-

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

Figure 7. Welfare and the Planner's Preferences



out, yet Reduced All does not cause large productive impacts. When a low weight is placed on productive impacts, Reduced Small/Medium and Reduced All cannot be ranked. However, this is attributable to the possibility that many of the households who choose Large under Reduced All have a willingness to pay for Large that is close to the Default price; this is prima facie unlikely given Reduced All significantly reduces the number of households choosing Large relative to Reduced Large. Duflo et al. (2011) raise the possibility that agricultural input subsidies may be both regressive and inefficient, and consequentially doubly ineffective. We find Reduced All is doubly ineffective. That is, double inefficacy is a function of the shape of agricultural input subsidies.

5 Conclusion

The most common 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 1/ farmers respond to prices, and 2/ 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 to interpret 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.

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 Project and Experiment Implementation

Figure A1. An "eVoucher"

(a) Front

(b) Back

Figure A2. Registration and Redemption Process

(a) Registration

(b) Redemption

B 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* (MZN).

Variables based on FAO administrative records from eVoucher system.[29]

- *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 equipment (bulk/silage bag, hoe, machete, sickle, sprayer), improved seeds (either open-pollinated or hybrid varieties, any crop), fertilizer (NPK, urea), and post-harvest insecticide during the 2023 primary season.

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

- *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.
- *Livestock*: unweighted sum of the number of animals (chickens, cows, ducks, fowls, geese, goats, pigs, rabbits, sheep, and turkeys) that a household owned in June 2021 or June 2023 for baseline or follow-up, respectively.
- *Fruit trees*: count of different types of fruit trees or permanent crops that a household collected or sold in the last 12 months.
- *Row planting*: whether a household practiced row planting in at least a part of their plots in the last 12 months.

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

- *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'.
- *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*: 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 Supplementary Appendix F.1.

- *Profits*: sum of agricultural production minus expenses on improved seeds, inorganic fertilizer, herbicide, insecticide, pesticides, equipment and land rental, hired and household labor. For robustness, we consider different types of labor and vary the valuation of household labor. See Supplementary Appendix F.1.

C Quantitative Framework: Details

C.1 Microfoundation of MVPF with Internalities

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

$$w_i(x; p) \equiv u_i\big(y_i(x)\big) - c_i(x) - p(x) \tag{C1}$$

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 $y_i(x)$.[30]

Household Perceived Utility and the Household Problem. However, households 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. Households may not know the productive impacts of inputs, and may learn these productive impacts with experience. Alternatively, households 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.

$$\text{WTP}_i(p) \equiv \max_x u_i\big(\hat{y}_i(x)\big) - c_i(x) - p(x) \tag{C2}$$

We use $\text{WTP}_i(p)$ to denote the maximized value of the household's objective function, as with quasilinear utility $\text{WTP}_i(p') - \text{WTP}_i(p)$ is household 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 household i of a price change from p to p'. Let $X_i(p)$ denote household 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

$$\Delta W_i(\cdot) = \Delta\text{WTP}_i + \underbrace{\left(1 - \frac{\Delta u_i\big(\hat{Y}_i(\cdot)\big)}{\Delta u_i\big(Y_i(\cdot)\big)}\right)}_{\equiv 1-\beta_i} \underbrace{\frac{\Delta u_i\big(Y_i(\cdot)\big)}{\Delta Y_i(\cdot)}}_{\equiv \delta_i} \Delta Y_i(\cdot) \tag{C3}$$

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 households' 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 changes in production to the willingness to pay for actual changes in production. When there are no distortions, and households choose input quantities as if their production technologies were the true $y_i(x)$, then $\hat{Y}_i = Y_i$ and

[30] 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.

$\Delta W_i = \Delta \text{WTP}_i$. 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 household's discount factor, times the household'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 household i's utility. Social utility is

$$W(p;\lambda) = \sum_i \lambda_i W_i(p) \tag{C4}$$

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 household i, that is the difference between the social cost of inputs and the price the household pays; empirically, this is the subsidy payout to household i.

MVPF and Optimal Policy. With this, we are equipped to define the marginal value of public funds. We follow Hahn et al. (2025), 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, $\sum_i \Delta G_i(\cdot)$).

$$\text{MVPF}(p,p';\lambda) \equiv \frac{\sum_i \frac{\lambda_i}{\sum_i \lambda_i} \Delta W_i(\cdot)}{\sum_i \Delta G_i(\cdot)} \tag{C5}$$

Note that our definition generalizes the definition in Hahn et al. (2025) 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. (2025) note that a budget neutral policy that increases the fraction of households 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

$$\gamma \equiv \frac{\sum_i (1-\beta_i)\delta_i \lambda_i \Delta Y_i(\cdot)}{\sum_i \lambda_i \Delta Y_i(\cdot)} \tag{C6}$$

That is, γ is an average of households' 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

$$\text{MVPF}(p,p';\lambda) = \frac{\sum_i \frac{\lambda_i}{\sum_i \lambda_i} (\Delta \text{WTP}_i(\cdot) + \gamma \Delta Y_i(\cdot))}{\sum_i \Delta G_i(\cdot)} \tag{C7}$$

We note that γ is not identified, and we therefore explore the robustness of our MVPF to alternative values of γ; in Section 4.1, 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 households with larger discounted wedges.

Conditional on γ, there are three sufficient statistics for 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 4.1. 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.

C.2 Bounds on Consumer Surplus

Bounds on Consumer Surplus. We summarized our construction of bounds on consumer surplus in Section 4.1.1; we report expressions for all bounds we apply in Table C1a. We report estimates of bounds on consumer surplus from Reduced Small/Medium, Reduced Large, and Reduced All 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 C1b. Let B_d and C_d denote the benefits and costs of treatment d, either Reduced Small/Medium, Reduced Large, or Reduced All, relative to Default. 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.

C.3 Testing for profit maximization and homogeneous marginal products and wedges

In Sections C.1 and C.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 3.4. For notational convenience, we define "profits gross of inputs" $\pi_i(x) \equiv y_i(x) - c_i(x)$ and $\Pi_i(p) \equiv \pi_i(X_i(p)) = Y_i(p) - C_i(p)$.

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

$$\pi_i(X_i(p)) - p(X_i(p)) \geq \pi_i(X_i(p')) - p(X_i(p')) \tag{C8}$$

Equation C8 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 homo-

Table C1. 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}	$2600\ P_{L}(0)$	$300\ P_{L}(L) + 2300\ P_{L}(All)$
CS_{All}	$300\ P_{SM}(0) + 2600\ P_{L}(0)$	$300\ P_{SM}(All) + 2600\ P_{L}(All)$
$CS_{L} - CS_{SM}$	$LB(CS_{All} - CS_{SM})$ - $UB(CS_{All} - CS_{L})$	$UB(CS_{L})$ - $LB(CS_{SM})$
$CS_{All} - CS_{SM}$	$300\ P_{L}(SM) + 2300\ P_{L}(0)$	$2600\ P_{L}(All)$
$CS_{All} - CS_{L}$	$300\ P_{SM}(L)$	$300\ P_{SM}(All)$

Notes: CS_d denotes consumer surplus from assignment to treatment d, either Reduced Small/Medium (SM), Reduced Large (L), or Reduced All (All), relative to Default. $P_x(d)$ denotes the fraction of households choosing x, either Small/Medium (SM) or Large (L), under treatment d, either Default (0), SM, L, or All.

(b) Comparing MVPF across Treatment Arms

Reduced Small/Medium rationalizable...	
relative to Reduced Large	$\frac{UB(B_{SM})}{C_{SM}} > \frac{LB(B_{L})}{C_{L}}$
relative to Reduced All	$\frac{UB(B_{SM})}{C_{SM}} > \frac{LB(B_{All}-B_{SM})}{C_{All}-C_{SM}}$
Reduced Large rationalizable...	
relative to Reduced Small/Medium	$\frac{UB(B_{L})}{C_{L}} > \frac{LB(B_{SM})}{C_{SM}}$
relative to Reduced All	$\frac{UB(B_{L})}{C_{L}} > \frac{LB(B_{All}-B_{L})}{C_{All}-C_{L}}$
Reduced All rationalizable...	
relative to Reduced Small/Medium	$\frac{UB(B_{All}-B_{SM})}{C_{All}-C_{SM}} > \frac{LB(B_{SM})}{C_{SM}}$
relative to Reduced Large	$\frac{UB(B_{All}-B_{L})}{C_{All}-C_{L}} > \frac{LB(B_{L})}{C_{L}}$

Notes: B_d and C_d denote the benefits to individuals and net costs to government, respectively, from treatment d, either Reduced Small/Medium (SM), Reduced Large (L), or Reduced All (All), relative to Default.

geneous across price schedules. Homogeneity implies

$$\frac{\sum_i \frac{\Pi_i(p')-\Pi_i(p)}{X_i(p')-X_i(p)}(X_i(p')-X_i(p))}{\sum_i X_i(p')-X_i(p)} = \frac{\sum_i \frac{\Pi_i(p'')-\Pi_i(p)}{X_i(p'')-X_i(p)}(X_i(p'')-X_i(p))}{\sum_i X_i(p'')-X_i(p)} \tag{C9}$$

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 C9, respectively; an overidentification test is therefore a test of Equation C9.

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

$$\omega(p',p) \equiv \frac{\sum_i \left(\frac{\Pi_i(p')-\Pi_i(p)}{\hat{\Pi}_i(p')-\hat{\Pi}_i(p)} - 1\right)(\hat{\Pi}_i(p')-\hat{\Pi}_i(p))}{\sum_i \hat{\Pi}_i(p')-\hat{\Pi}_i(p)} \tag{C10}$$

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 household's objective function. We note that the household objective function in Equation C2 implies, by a standard revealed preference argument

$$p'(X_i(p')) - p'(X_i(p)) \leq \hat{\Pi}_i(p') - \hat{\Pi}_i(p) \leq p(X_i(p')) - p(X_i(p)) \tag{C11}$$

Equation C11 enables the following bounds on Equation C10 for $\sum_i p'(X_i(p')) - p'(X_i(p)) > 0$.

$$\frac{\sum_i \Pi_i(p')-\Pi_i(p)}{\sum_i p(X_i(p'))-p(X_i(p))} \leq 1+\omega(p',p) \leq \frac{\sum_i \Pi_i(p')-\Pi_i(p)}{\sum_i p'(X_i(p'))-p'(X_i(p))} \tag{C12}$$

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.

D 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 MZN). 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,

$$\log C_i = \alpha + \beta \cdot \text{Asset}_i + \varepsilon_i \tag{D1}$$

where i indexes a household. Our estimate is in Table D1.

Table D1. Relationship between Consumption and Assets

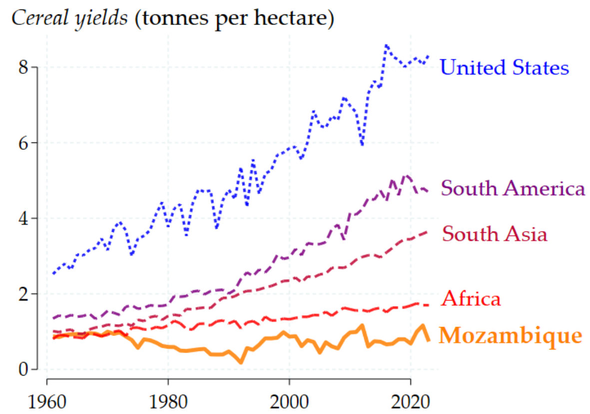
	Log consumption (1)
Asset index (0-10)	0.333***
	(0.005)
	[0.000]
Sample mean of the outcome variable	11.488
Number of observations	67,688
Adjusted R-squared	0.315

Notes: *Significant at 10%. **Significant at 5%. ***Significant at 1%. Unit of observation: household. Data are from the 2022 Mozambique Household Budget Survey (INE, 2023). All regressions are ordinary least squares as in Equation D1. Standard errors clustered at the farmer level in parentheses; p-values in brackets.

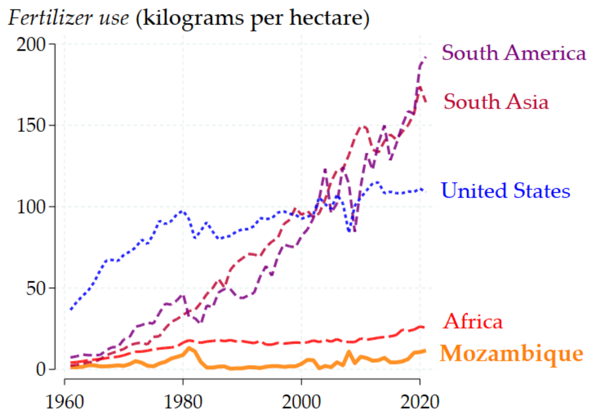
E Additional Figures and Tables

Figure E1. Agricultural productivity and input use in Africa are low and stagnant

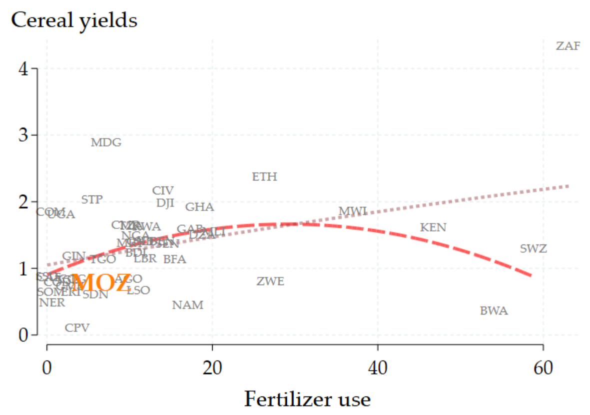
(a) Global Trend in Yields



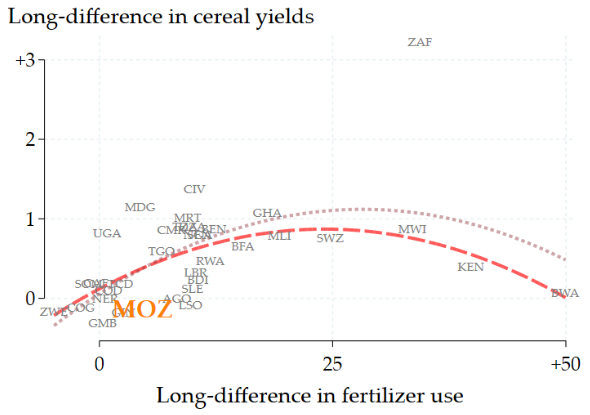
(b) Global Trend in Fertilizer Use



(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, respectively.

Table E1. First Stage on Package Choice: Alternative Samples

	Package type			
	Any (1)	**S**mall (2)	**M**edium (3)	**L**arge (4)
Full sample				
Reduced **S**mall/**M**edium	0.066***	-0.020	0.097***	-0.011
	(0.022)	(0.024)	(0.021)	(0.009)
	[0.003]	[0.413]	[0.000]	[0.227]
Reduced **L**arge	0.007	-0.072***	-0.090***	0.170***
	(0.023)	(0.024)	(0.018)	(0.016)
	[0.757]	[0.003]	[0.000]	[0.000]
Reduced All	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 default group	0.613	0.422	0.171	0.020
Number of observations	2,383	2,383	2,383	2,383
Number of farmers	2,383	2,383	2,383	2,383
Baseline sample				
Reduced **S**mall/**M**edium	0.084***	-0.013	0.110***	-0.012
	(0.031)	(0.033)	(0.030)	(0.015)
	[0.007]	[0.686]	[0.000]	[0.406]
Reduced **L**arge	0.055*	-0.046	-0.093***	0.194***
	(0.032)	(0.033)	(0.025)	(0.023)
	[0.091]	[0.161]	[0.000]	[0.000]
Reduced All	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 default group	0.590	0.395	0.170	0.024
Number of observations	1,280	1,280	1,280	1,280
Number of farmers	1,280	1,280	1,280	1,280

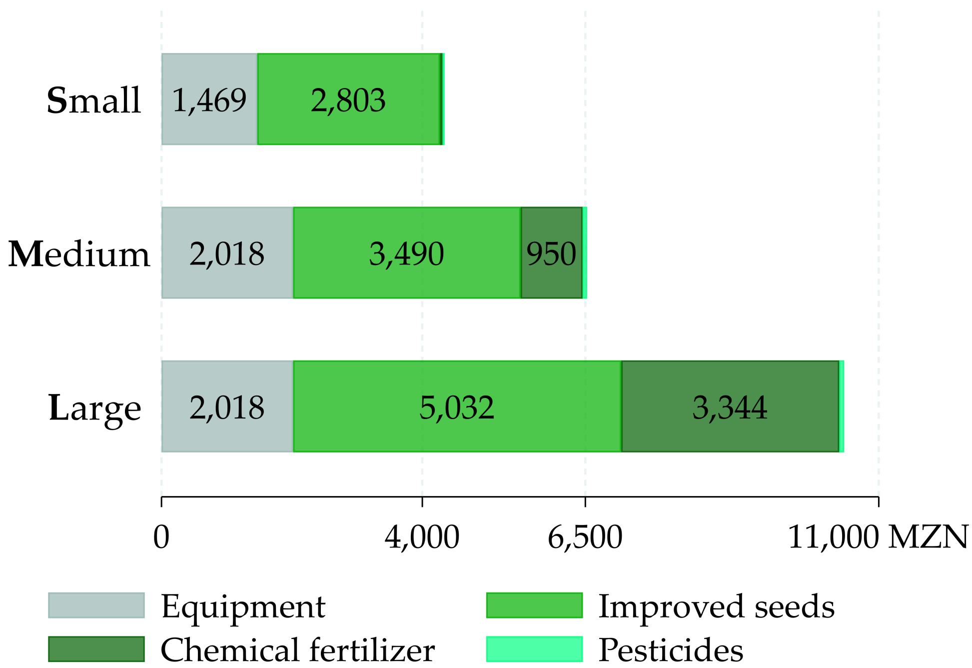
Notes: *Significant at 10%. **Significant at 5%. ***Significant at 1%. Unit of observation: household. Data are from administrative eVoucher records. Sample in the panel header: 'full' includes all registered households in the eVoucher system (i.e., regardless of survey status) and 'baseline' includes 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 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 E2. No Differential Registration by Treatment Assignment

Data source:	**Administrative records**	**Survey self-report**		
Outcome:	*Registered* (1)	*Heard about* (2)	*Was contacted* (3)	*Registered* (4)
Reduced **S**mall/**M**edium	-0.001	-0.007	0.024	0.021
	(0.021)	(0.020)	(0.023)	(0.024)
	[0.969]	[0.713]	[0.297]	[0.396]
Reduced **L**arge	-0.022	0.002	0.013	0.005
	(0.023)	(0.019)	(0.023)	(0.025)
	[0.340]	[0.904]	[0.574]	[0.832]
Reduced All	-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 default group	0.861	0.920	0.864	0.832
Number of observations	1,473	1,473	1,473	1,473
Number of farmers	1,473	1,473	1,473	1,473

Notes: *Significant at 10%. **Significant at 5%. ***Significant at 1%. 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 farmer level in parentheses; *p*-values in brackets.

Figure E2. 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 ≈ 1.5 USD).

Table E3. Balance of Treatment Assignment in Sample of Registered Households ($N = 1,280$)

	Mean (SD)				Coefficient (SE) [p]					
	Default	Reduced **S**mall/**M**edium	Reduced **L**arge	Reduced All	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)	(0.200)
					[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)	(0.032)
					[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)	(0.017)
					[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)	(0.028)
					[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)	(0.022)
					[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)	(0.015)
					[0.827]	[0.250]	[0.440]	[0.239]	[0.330]	[0.704]
Cultivated area (ha)	1.500	1.620	1.706	1.521	0.076	0.239	-0.014	0.069	-0.030	-0.134
	(1.506)	(1.576)	(2.063)	(1.455)	(0.112)	(0.154)	(0.115)	(0.154)	(0.116)	(0.116)
					[0.497]	[0.121]	[0.900]	[0.654]	[0.797]	[0.337]
Production value ('000 MZN)	18.661	23.470	21.462	20.998	4.244*	2.460	2.159	-2.923	-2.100	1.252
	(26.215)	(38.520)	(30.512)	(41.659)	(2.558)	(2.187)	(2.972)	(2.755)	(3.089)	(3.089)
					[0.098]	[0.261]	[0.468]	[0.289]	[0.497]	[0.697]
Sold or planning to sell	0.766	0.789	0.807	0.811	0.010	0.047	0.036	0.023	0.013	0.007
	(0.424)	(0.408)	(0.396)	(0.392)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)
					[0.747]	[0.124]	[0.227]	[0.449]	[0.652]	[0.806]
Omnibus *F*-stat [p]					3.3	2.8	0.8	0.9	0.4	0.1
					[0.068]	[0.098]	[0.380]	[0.341]	[0.505]	[0.795]
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: *Significant at 10%. **Significant at 5%. ***Significant at 1%. 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 treatments. Standard errors clustered at the 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 E4. No Differential Attrition by Treatment Assignment

	Sample		
	Whole baseline	*Registered (administrative records)*	*Registered (survey self-reports)*
	(1)	(2)	(3)
Reduced **S**mall/**M**edium	0.001	-0.003	0.000
	(0.014)	(0.011)	(.)
	[0.924]	[0.798]	[.]
Reduced **L**arge	0.000	-0.005	0.000
	(0.014)	(0.010)	(.)
	[1.000]	[0.612]	[.]
Reduced All	0.006	-0.004	0.000
	(0.014)	(0.011)	(.)
	[0.696]	[0.741]	[.]
Sample mean of the default group	0.041	0.021	0.000
Number of observations	1,538	1,280	1,109
Number of farmers	1,538	1,280	1,109

Notes: *Significant at 10%. **Significant at 5%. ***Significant at 1%. 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 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 E5. No Evidence of Reselling or Crowding Out

	eVoucher inputs			*Non-eVoucher*
	Gave away	*Sold*	*Stored*	*Input value*
	(1)	(2)	(3)	(4)
Reduced **S**mall/**M**edium	-0.015	0.004	0.014	-5
	(0.013)	(0.006)	(0.022)	(154)
	[0.247]	[0.579]	[0.519]	[0.975]
Reduced **L**arge	0.010	0.011	0.003	144
	(0.015)	(0.007)	(0.023)	(242)
	[0.527]	[0.129]	[0.909]	[0.549]
Reduced All	0.008	0.001	0.008	-145
	(0.015)	(0.006)	(0.022)	(150)
	[0.577]	[0.876]	[0.723]	[0.335]
Sample mean of the default group	0.031	0.003	0.093	233
Number of observations	1,254	1,254	1,254	1,240
Number of farmers	1,254	1,254	1,254	1,240

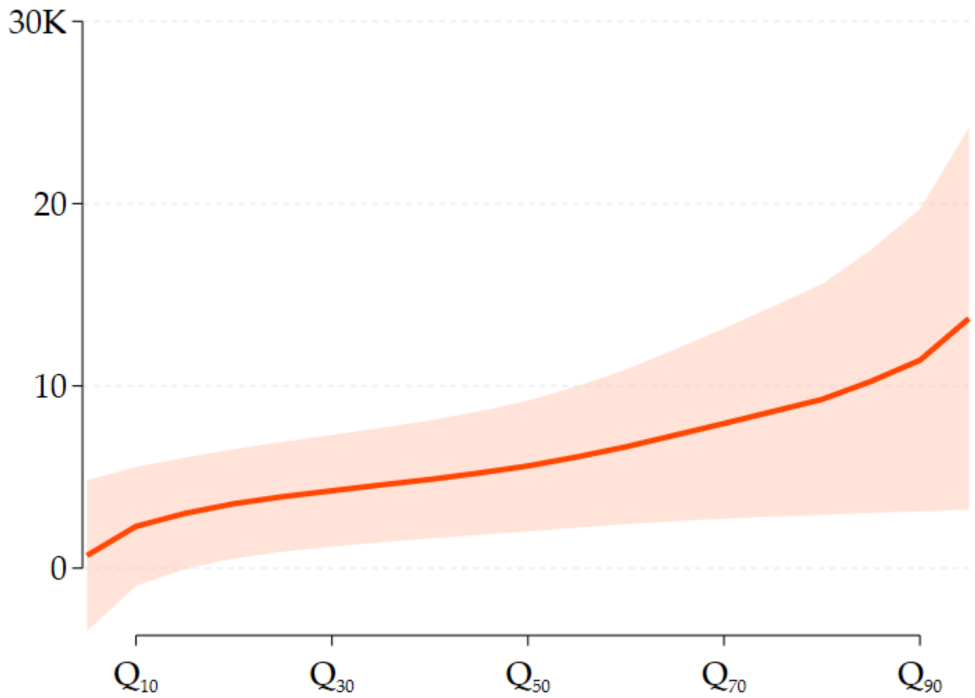
Notes: *Significant at 10%. **Significant at 5%. ***Significant at 1%. Unit of observation: household. Data are from our household surveys. Sample: households interviewed at follow-up. All regressions are least squares with strata (i.e., community) fixed effects as in Equation 1. Standard errors clustered at the farmer level in parentheses; p-values in brackets. 'Input value' is in nominal Mozambican *meticais* (100 MZN $\approx$ 1.5 USD).

Table E6. First Stage on Input Values: Alternative Samples

	Input category				
	Any	*Improved seeds*	*Inorganic fertilizer*	*Insecticide/ pesticides*	*Equipment*
	(1)	(2)	(3)	(4)	(5)
Full sample					
Reduced **S**mall/**M**edium	535***	515***	-20	29***	11
	(172)	(114)	(76)	(8)	(60)
	[0.002]	[0.000]	[0.792]	[0.000]	[0.853]
Reduced **L**arge	988***	471***	381***	15**	121*
	(211)	(146)	(97)	(7)	(62)
	[0.000]	[0.001]	[0.000]	[0.041]	[0.053]
Reduced All	971***	559***	376***	16**	21
	(185)	(118)	(93)	(7)	(63)
	[0.000]	[0.000]	[0.000]	[0.024]	[0.745]
Sample mean of the default group	3,319	1,832	405	23	1,058
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					
Reduced **S**mall/**M**edium	722***	643***	23	40***	16
	(261)	(172)	(117)	(12)	(90)
	[0.006]	[0.000]	[0.844]	[0.001]	[0.857]
Reduced **L**arge	1,474***	870***	351**	19**	234**
	(324)	(232)	(141)	(9)	(92)
	[0.000]	[0.000]	[0.013]	[0.036]	[0.012]
Reduced All	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 default group	3,221	1,686	457	16	1,062
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

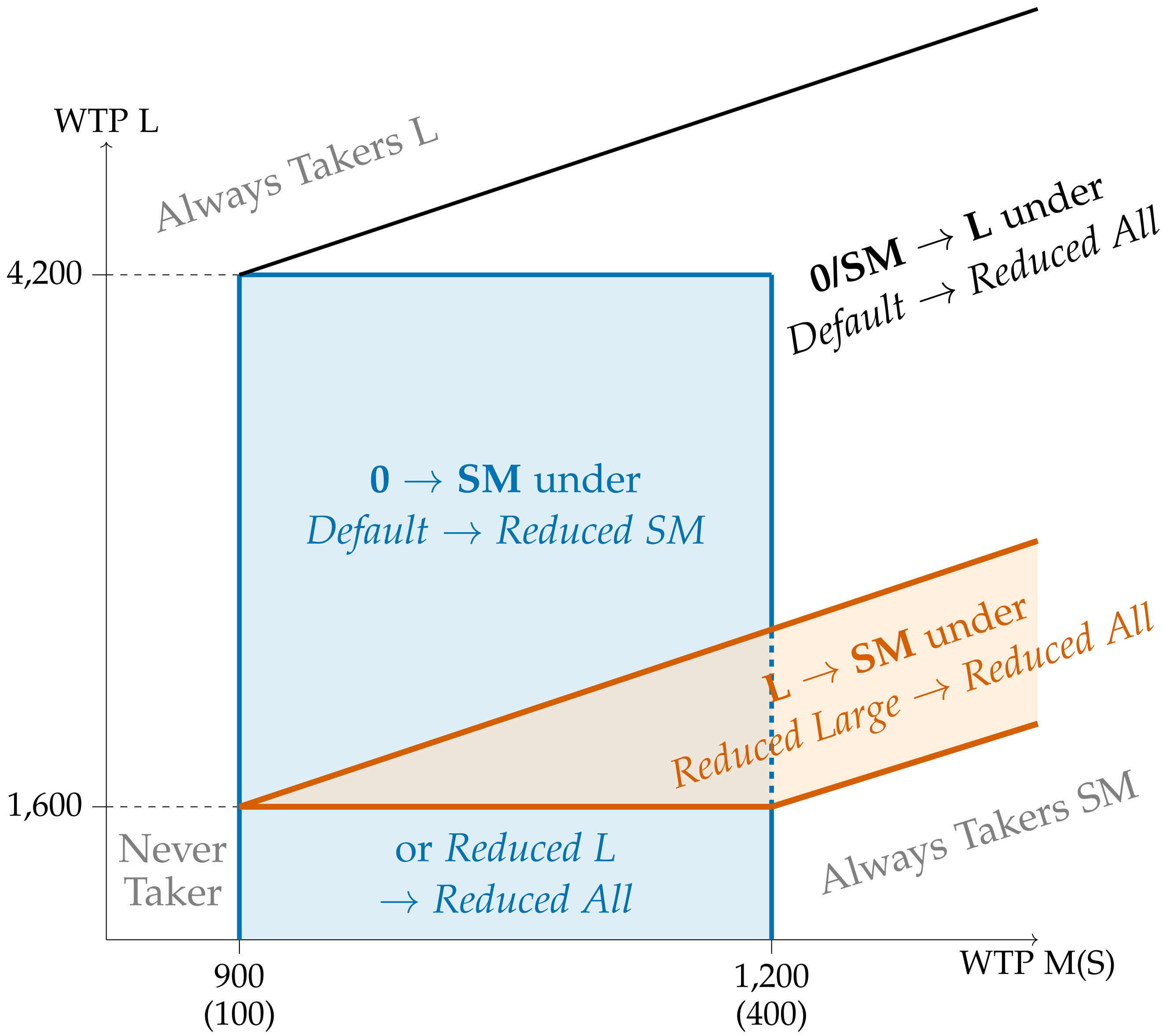
Notes: *Significant at 10%. **Significant at 5%. ***Significant at 1%. Unit of observation: household. Data are from administrative eVoucher records. Sample in the panel header: 'full' includes all registered households in the eVoucher system (i.e., regardless of survey status) and 'baseline' includes 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 farmer level in parentheses; *p*-values in brackets. Outcomes are in nominal Mozambican *meticais* (100 MZN ≈ 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 E3. Quantile Treatment Effects of *Reduced Large* on Profits



Notes: *Significant at 10%. **Significant at 5%. ***Significant at 1%. 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 community fixed effects and standard errors clustered at the farmer level. The outcome, i.e., 'Profits', is in nominal Mozambican *meticais* (100 MZN ≈ 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, herbicide, insecticide, pesticides, equipment and land rental, hired labor, and household labor (priced at 60% of the median wage across casual labor days).

Figure E4. Selected Complier Groups



Notes: Decreasing returns to scale (DRTS) excludes the rectangle between 1,600-4,200 WTP L and 0-900 WTP M(S).

Table E7. Characteristics of Selected Complier Groups

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
			Compliers group:				
	Population mean	**0 → SM**	**0/SM → L**	**L → SM**	Never takers **0**	Always takers **SM**	Always takers **L**
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)
Landholdings (ha)	2.420	2.237	3.190	3.074	2.577	2.343	3.377
	(0.066)	(0.981)	(0.466)	(2.391)	(0.263)	(0.195)	(0.705)
Used mechanized equipments	0.163	0.172	0.310	0.188	0.127	0.144	0.000
	(0.010)	(0.168)	(0.069)	(0.326)	(0.033)	(0.032)	(0.000)
Willingness to pay for Large package ('000 MZN)	1.096	1.146	2.216	-1.011	0.854	0.803	2.733
	(0.076)	(0.845)	(1.040)	(3.767)	(0.149)	(0.120)	(2.136)
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)
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*	21.899	15.165	33.897	41.154	18.757	20.982	101.529
	(0.986)	(9.062)	(7.631)	(40.242)	(2.731)	(2.382)	(82.140)
Sold or planning to sell	0.791	0.943	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		187	185	616	102	125	3
Effective *F*-statistic		.	.	.			
Instrumented variable:		*Redeemed Small or Redeemed Medium*	*Redeemed Large*	*Redeemed Large*			
Instrument:		*Reduced Small/Medium and Reduced All*	*Reduced All*	*Reduced All*			
Reference group:		*Default and Reduced Large*	*Default*	*Reduced Large*	*Reduced All*	*Reduced Large*	*Reduced Small/Medium*

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 E4. Column (2) controls for a binary variable equal to one for *Reduced Large and Reduced All* and zero otherwise. Standard errors clustered at the 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).

SUPPLEMENTARY APPENDIX

F Robustness

F.1 Robustness of Experimental Results

Table F1. Balance in Row Planting at Follow-up

	Row planting (1)
Reduced **S**mall/**M**edium	-0.047
	(0.038)
	[0.216]
Reduced **L**arge	-0.028
	(0.038)
	[0.458]
Reduced All	-0.034
	(0.038)
	[0.369]
Sample mean of the default group	0.618
Number of observations	1,254
Number of farmers	1,254

Notes: *Significant at 10%. **Significant at 5%. ***Significant at 1%. Unit of observation: household. Data are from our baseline household surveys. All regressions are least squares with strata (i.e., community) fixed effects. Standard errors clustered at the farmer level in parentheses; *p*-values in brackets.

Table F2. Balance of Treatment Assignment in Whole Baseline Sample ($N = 1,538$)

	Mean (SD)				Coefficient (SE) [p]					
	Default	Reduced **S**mall/**M**edium	Reduced **L**arge	Reduced All	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)	(0.171)
					[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)	(0.029)
					[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)	(0.143)
					[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)	(0.013)
					[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)	(0.025)
					[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)	(0.019)
					[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)	(0.013)
					[0.967]	[0.197]	[0.384]	[0.237]	[0.410]	[0.781]
Cultivated area (ha)	1.417	2.023	1.637	1.480	0.597	0.227*	0.049	-0.428	-0.547	-0.112
	(1.422)	(8.568)	(1.988)	(1.445)	(0.451)	(0.124)	(0.098)	(0.466)	(0.454)	(0.454)
					[0.186]	[0.067]	[0.616]	[0.358]	[0.229]	[0.342]
Production value ('000 MZN)	17.546	22.944	20.103	19.895	4.918**	2.410	2.174	-3.104	-2.784	0.564
	(24.476)	(37.451)	(28.528)	(38.709)	(2.217)	(1.862)	(2.291)	(2.227)	(2.598)	(2.598)
					[0.027]	[0.196]	[0.343]	[0.164]	[0.284]	[0.815]
Sold or planning to sell	0.754	0.782	0.801	0.793	0.020	0.052*	0.037	0.021	0.014	-0.014
	(0.431)	(0.414)	(0.400)	(0.406)	(0.026)	(0.028)	(0.027)	(0.027)	(0.027)	(0.027)
					[0.449]	[0.064]	[0.173]	[0.442]	[0.615]	[0.618]
Omnibus *F*-stat [p]					7.2	3.2	1.4	2.4	1.7	0.0
					[0.007]	[0.074]	[0.241]	[0.121]	[0.187]	[0.906]
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: *Significant at 10%. **Significant at 5%. ***Significant at 1%. 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 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 F3. Impacts on Main Outcomes, Controlling for Assets

	Production value (1)	Profits (2)	Subsidy payout (3)
Reduced **S**mall/**M**edium	1,114	1,564	628***
	(2,057)	(1,872)	(164)
	[0.588]	[0.403]	[0.000]
Reduced **L**arge	5,915**	5,517**	1,166***
	(2,445)	(2,150)	(206)
	[0.016]	[0.010]	[0.000]
Reduced All	333	1,482	1,248***
	(1,854)	(1,727)	(188)
	[0.857]	[0.391]	[0.000]
Sample mean of the default group	18,852	8,703	2,533
Number of observations	1,254	1,254	1,254
Number of farmers	1,254	1,254	1,254

Notes: *Significant at 10%. **Significant at 5%. ***Significant at 1%. 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 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, herbicide, insecticide, pesticides, equipment and land rental, hired labor, and household labor (priced at 60% of the median wage across casual labor days).

Table F4. Comparison of Baseline Characteristics By Package Choice

	Mean (SD)				Coefficient (SE) [p]					
	No redemption	**S**mall	**M**edium	**L**arge	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)	(0.325)
					[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)	(0.059)
					[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)	(0.296)
					[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)	(0.032)
					[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)	(0.054)
					[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)	(0.049)
					[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)	(0.013)
					[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)	(0.242)
					[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)	(4.590)
					[0.001]	[0.925]	[0.314]	[0.001]	[0.007]	[0.489]
Sold or planning to sell	0.748	0.753	0.870	0.938	-0.025	-0.006	0.105**	0.085**	0.163***	0.067
	(0.434)	(0.432)	(0.337)	(0.242)	(0.039)	(0.045)	(0.041)	(0.039)	(0.057)	(0.057)
					[0.529]	[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: *Significant at 10%. **Significant at 5%. ***Significant at 1%. 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 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 F5. First Stage on Inputs: Extensive Margin

	Input category				
	Any	*Improved seeds*	*Inorganic fertilizer*	*Insecticide/ pesticides*	*Equipment*
	(1)	(2)	(3)	(4)	(5)
Reduced **S**mall/**M**edium	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]
Reduced **L**arge	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]
Reduced All	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 default group	0.599	0.581	0.084	0.050	0.571
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: *Significant at 10%. **Significant at 5%. ***Significant at 1%. 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 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 E6).

Table F6. Impact of Treatment Assignment on Output

	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)
Reduced **S**mall/**M**edium	723	1,709	-650	809	1,282	-991
	(2,095)	(1,646)	(493)	(1,012)	(1,797)	(705)
	[0.730]	[0.299]	[0.188]	[0.424]	[0.476]	[0.160]
Reduced **L**arge	6,775***	2,630*	960	3,589**	2,519	972
	(2,478)	(1,398)	(834)	(1,498)	(1,588)	(991)
	[0.006]	[0.060]	[0.250]	[0.017]	[0.113]	[0.327]
Reduced All	301	101	373	287	-986	794
	(1,895)	(1,399)	(640)	(886)	(1,505)	(1,075)
	[0.874]	[0.942]	[0.560]	[0.746]	[0.513]	[0.461]
Sample mean of the default group	18,852	6,674	1,312	10,866	7,550	1,598
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: *Significant at 10%. **Significant at 5%. ***Significant at 1%. 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* (1 USD ≈ 64 MZN). 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 farmer level in parentheses. The control group is the default eVoucher menu with no additional subsidy.

Table F7. Impact of Treatment Assignment on Area and Output: Heterogeneity by Crop

	Crop category						**Total**
	Grains	Rice	Tubers	Legumes/ pulses	Vegetables	Cash crops/ oilseeds	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Area							
Reduced **S**mall/**M**edium	0.049	-0.005	0.001	0.130	-0.018	0.033	0.125
	(0.072)	(0.049)	(0.042)	(0.080)	(0.020)	(0.040)	(0.183)
	[0.498]	[0.915]	[0.987]	[0.105]	[0.380]	[0.412]	[0.494]
Reduced **L**arge	0.075	-0.009	0.031	0.174**	0.011	-0.020	0.165
	(0.067)	(0.051)	(0.046)	(0.079)	(0.025)	(0.035)	(0.171)
	[0.266]	[0.866]	[0.501]	[0.027]	[0.647]	[0.557]	[0.335]
Reduced All	-0.006	-0.041	0.019	0.049	0.025	0.074	0.079
	(0.059)	(0.044)	(0.042)	(0.065)	(0.030)	(0.055)	(0.162)
	[0.922]	[0.358]	[0.662]	[0.449]	[0.408]	[0.174]	[0.625]
Sample mean of the default group	0.615	0.328	0.405	0.796	0.064	0.184	2.393
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
Output							
Reduced **S**mall/**M**edium	364	604	237	811	467	-527	751
	(676)	(503)	(150)	(1,071)	(694)	(903)	(2,100)
	[0.590]	[0.230]	[0.115]	[0.449]	[0.501]	[0.560]	[0.721]
Reduced **L**arge	2,580***	-88	133	2,417*	279	2,065*	6,735***
	(992)	(266)	(132)	(1,246)	(377)	(1,190)	(2,487)
	[0.009]	[0.740]	[0.314]	[0.053]	[0.459]	[0.083]	[0.007]
Reduced All	309	-172	261*	549	553	-326	355
	(654)	(304)	(153)	(944)	(645)	(835)	(1,893)
	[0.636]	[0.571]	[0.089]	[0.561]	[0.392]	[0.696]	[0.851]
Sample mean of the default group	5,135	1,541	488	7,960	560	3,127	18,811
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: *Significant at 10%. **Significant at 5%. ***Significant at 1%. 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 the dependent variable in Columns 1 and 2 – as in Equation 1. Standard errors clustered at the farmer level in parentheses; p-values in brackets. Outcomes are in hectares for the 'Area' panel and in nominal Mozambican *meticais* (100 MZN ≈ 1.5 USD) for the 'Output' panel.

Table F8. Impact of Treatment Assignment on Profits

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)
Reduced **S**mall/**M**edium	1,080	899	759	1,574	1,347	1,204
	(1,980)	(1,877)	(1,954)	(2,072)	(1,892)	(1,975)
	[0.586]	[0.632]	[0.698]	[0.447]	[0.477]	[0.542]
Reduced **L**arge	6,825***	6,456***	6,197**	6,434***	6,096***	5,839**
	(2,348)	(2,369)	(2,526)	(2,294)	(2,178)	(2,350)
	[0.004]	[0.007]	[0.014]	[0.005]	[0.005]	[0.013]
Reduced All	1,043	1,109	1,157	878	1,472	1,534
	(1,783)	(1,753)	(1,894)	(1,858)	(1,747)	(1,902)
	[0.559]	[0.527]	[0.542]	[0.637]	[0.400]	[0.420]
Sample mean of the default group	17,017	9,445	4,396	17,470	8,703	3,655
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: *Significant at 10%. **Significant at 5%. ***Significant at 1%. 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 the dependent variable in Columns 1 and 2 – as in Equation 1. Standard errors clustered at the farmer level in parentheses; p-values in brackets. Outcomes are in nominal Mozambican *meticais* (100 MZN ≈ 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, herbicide, insecticide, pesticides, equipment 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 F9. First-Stage Package Choice by Row Planting

	Package type			
	Any	**S**mall	**M**edium	**L**arge
	(1)	(2)	(3)	(4)
Households not row planting				
Reduced **S**mall/**M**edium	0.062	-0.009	0.112**	-0.041*
	(0.060)	(0.060)	(0.048)	(0.022)
	[0.309]	[0.875]	[0.020]	[0.056]
Reduced **L**arge	-0.041	-0.077	-0.071*	0.106***
	(0.062)	(0.063)	(0.041)	(0.032)
	[0.503]	[0.224]	[0.085]	[0.001]
Reduced All	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 default group	0.517	0.390	0.119	0.008
Number of observations	486	486	486	486
Number of farmers	486	486	486	486
Households row planting				
Reduced **S**mall/**M**edium	0.071*	-0.060	0.153***	-0.022
	(0.041)	(0.046)	(0.042)	(0.025)
	[0.088]	[0.190]	[0.000]	[0.371]
Reduced **L**arge	0.089**	-0.080*	-0.075*	0.243***
	(0.042)	(0.045)	(0.038)	(0.037)
	[0.036]	[0.077]	[0.052]	[0.000]
Reduced All	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 default group	0.643	0.413	0.194	0.036
Number of observations	726	726	726	726
Number of farmers	726	726	726	726

Notes: *Significant at 10%. **Significant at 5%. ***Significant at 1%. 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. All regressions are least squares with strata (i.e., community) fixed effects as in Equation 1. Standard errors clustered at the 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 F10. Impacts on Long-Run Adoption: Triple Interaction

	Package type							
	Any		Small		Medium		Large	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Long-run	-0.187***	-0.216***	-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)
		[0.435]		[0.000]		[0.000]		[0.089]
Asset index × Long-run		0.010		0.021***		-0.006		-0.006*
		(0.010)		(0.008)		(0.007)		(0.003)
		[0.301]		[0.007]		[0.389]		[0.057]
Reduced Large or All	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)
	[0.437]	[0.154]	[0.084]	[0.648]	[0.000]	[0.246]	[0.000]	[0.000]
Reduced Large or All × Long-run	-0.018	-0.031	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]	[0.542]	[0.068]	[0.250]	[0.007]	[0.565]	[0.000]	[0.000]
Reduced Large or All × Asset index		-0.014		-0.005		-0.027***		0.017**
		(0.012)		(0.012)		(0.010)		(0.008)
		[0.242]		[0.700]		[0.008]		[0.032]
Reduced Large or All × Asset index × Long-run		0.004		-0.004		0.011		-0.002
		(0.014)		(0.012)		(0.009)		(0.007)
		[0.805]		[0.738]		[0.218]		[0.744]
[Reduced Large or All] + [Reduced Large or All × Long-run]	0.000	0.030	0.004	0.033	-0.064	-0.022	0.061	0.015
	(0.019)	(0.032)	(0.019)	(0.034)	(0.015)	(0.025)	(0.010)	(0.016)
	[0.989]	[0.353]	[0.839]	[0.333]	[0.000]	[0.386]	[0.000]	[0.372]
[Reduced Large or All × Asset index] + [Reduced Large or All × Asset index × Long-run]		-0.011		-0.009		-0.016		0.015
		(0.009)		(0.009)		(0.008)		(0.005)
		[0.244]		[0.360]		[0.053]		[0.005]
Sample mean of the comparison group	0.510	0.502	0.305	0.330	0.188	0.150	0.016	0.021
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: *Significant at 10%. **Significant at 5%. ***Significant at 1%. 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 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 treatment assignments, are in Supplementary Appendix Table F11.

Table F11. Impacts on Long-Run Adoption: Fully Saturated Treatments

	Package type							
	Any		**S**mall		**M**edium		**L**arge	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Long-run	-0.146***	-0.160***	-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
		(0.012)		(0.013)		(0.011)		(0.005)
		[0.267]		[0.002]		[0.000]		[0.253]
Asset index × Long-run		0.005		0.021*		-0.012		-0.006*
		(0.013)		(0.012)		(0.010)		(0.004)
		[0.722]		[0.069]		[0.223]		[0.099]
Reduced **S**mall/**M**edium	0.079**	0.115**	-0.020	-0.044	0.113***	0.177***	-0.015	-0.018
	(0.033)	(0.057)	(0.034)	(0.061)	(0.030)	(0.048)	(0.012)	(0.017)
	[0.018]	[0.046]	[0.549]	[0.474]	[0.000]	[0.000]	[0.228]	[0.290]
Reduced **S**mall/**M**edium × Long-run	-0.083**	-0.109	-0.030	-0.026	-0.061**	-0.092**	0.009	0.006
	(0.040)	(0.069)	(0.034)	(0.062)	(0.028)	(0.044)	(0.012)	(0.013)
	[0.039]	[0.114]	[0.370]	[0.672]	[0.030]	[0.038]	[0.435]	[0.662]
Reduced **S**mall/**M**edium × Asset index		-0.012		0.007		-0.021		0.001
		(0.017)		(0.017)		(0.016)		(0.007)
		[0.459]		[0.662]		[0.174]		[0.870]
Reduced **S**mall/**M**edium × Asset index × Long-run		0.009		-0.001		0.011		0.001
		(0.019)		(0.016)		(0.014)		(0.006)
		[0.629]		[0.959]		[0.453]		[0.863]
Reduced **L**arge	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]	[0.128]	[0.106]	[0.420]	[0.000]	[0.796]	[0.000]	[0.000]
Reduced **L**arge × Long-run	-0.041	-0.078	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]	[0.285]	[0.353]	[0.401]	[0.058]	[0.509]	[0.000]	[0.001]
Reduced **L**arge × Asset index		-0.018		0.003		-0.038***		0.017
		(0.017)		(0.017)		(0.013)		(0.011)
		[0.296]		[0.857]		[0.004]		[0.126]
Reduced **L**arge × Asset index × Long-run		0.011		-0.010		0.023**		-0.000
		(0.020)		(0.017)		(0.012)		(0.011)
		[0.572]		[0.576]		[0.048]		[0.998]
Reduced All	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)
	[0.031]	[0.023]	[0.156]	[0.593]	[0.437]	[0.066]	[0.000]	[0.006]
Reduced All × Long-run	-0.077*	-0.093	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)
	[0.056]	[0.202]	[0.441]	[0.733]	[0.832]	[0.467]	[0.000]	[0.003]
Reduced All × 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]
Reduced All × Asset index × Long-run		0.005		0.001		0.009		-0.003
		(0.020)		(0.017)		(0.013)		(0.010)
		[0.796]		[0.940]		[0.518]		[0.748]
Sample mean of the comparison group	0.502	0.502	0.330	0.330	0.150	0.150	0.021	0.021
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: *Significant at 10%. **Significant at 5%. ***Significant at 1%. 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 farmer level in parentheses; *p*-values in brackets. Separate estimates on short- and long-run adoption are in Table 6.