Going the Extra Mile: Farm Subsidies and Spatial Convergence in Agricultural Input Adoption*

Naresh Kumar Rolly Kapoor Shilpa Aggarwal Dahyeon Jeong
David Sungho Park Jonathan Robinson Alan Spearot

January 16, 2025

Abstract

Many countries subsidize agricultural inputs but require farmers to travel to retailers to access them, just as for normal purchases. What effect do travel costs have on subsidy take-up and input usage, particularly for remote farmers? We analyze Malawi's Farm Input Subsidy Program (FISP), and show that though retail prices are close to uniform, travel-cost-adjusted prices are substantially higher in remote areas due to travel costs. Nevertheless, subsidy redemption is nearly universal and only modestly lower in remote areas, suggesting that these travel costs are not enough to dissuade redemption. We make use of a policy change in the 2017-18 to 2019-20 agricultural seasons which took centralized control of beneficiary selection and find that FISP substantially mitigates the sizeable remoteness gradient that exists for non-beneficiaries. Our results demonstrate that subsidy programs may narrow spatial inequities.

JEL Codes: O12, O13, Q12, Q16, Q18

Keywords: input subsidies, technology adoption, transport costs, FISP

*Kumar: World Bank (nkumar18@worldbank.org); Kapoor: University of California, Santa Cruz (rokapoor@ucsc.edu); Aggarwal: Indian School of Business (shilpa_aggarwal@isb.edu); Jeong: World Bank (dahyeonjeong@worldbank.org); Park: KDI School of Public Policy and Management (park@kdis.ac.kr); Robinson: University of California, Santa Cruz, and NBER (jmrtwo@ucsc.edu); Spearot: University of California, Santa Cruz (aspearot@ucsc.edu). We are grateful to Jenny Aker for her collaboration and to Patrick Baxter, Emanuele Clemente, Calvin Mhango, Monica Shandal, Patrick Simbewe, and Asman Suleiman at IPA Malawi. USAID funded the data collection. We thank Sanjana Gupta for research assistance, and Justin Marion and seminar participants at UCSC for helpful comments. The data collection protocol was approved by the IRBs of the University of California, Santa Cruz and the Malawi National Committee on Research in the Social Sciences and Humanities. The findings, interpretations, and conclusions expressed in this paper are entirely those of the authors, and do not necessarily represent the views of the World Bank or its affiliated organizations, its Executive Directors or the governments they represent.

1 Introduction

Poor transportation infrastructure impedes access to input markets (World Bank 2008; World Bank 2017), lowering input usage and agricultural productivity in remote areas (**aggarwal2022market**; Minten et al. 2013; Shamdasani 2021). To reduce these spatial inequalities, much of the policy and research discussion has been on the role of reducing transport costs, for example via road construction (Aggarwal 2018; Brooks and Donovan 2020; Gebresilasse 2023; Porteous 2019). In this paper, we examine the spatial properties of another policy instrument: input subsidies. While input subsidies are used across the developing world to spur agricultural productivity generally,[1] they are usually aimed at improving the general level of input usage, and not necessarily designed for mitigating spatial gradients.

A priori, input subsidies should generate spatial convergence, since remote farmers are less likely to already be using inputs. However, in practice, many subsidy programs still require beneficiaries to procure inputs at retail or specialized locations that may be located far away. For example, in the setting of this study, the Farm Input Subsidy Program (FISP) in Malawi, beneficiaries received coupons that had to be redeemed at selected fertilizer retailers; thus, remote farmers still had to incur transport costs to travel to distant input retailers. It is therefore possible that a subsidy program such as this could leave the distance gradient unchanged or may even worsen it if subsidy take-up is sufficiently elastic to travel costs.

In this paper, we rigorously study spatial differences in FISP prices, redemption, and input usage by households using survey data of 2,664 maize farmers in 300 villages in 2 districts of Southern Malawi, as well as surveys of the universe of agricultural input dealers in the area. FISP beneficiaries constitute about 15% of the farming population in our data,[2] and the program provides a subsidy worth about 75% of the retail cost of $75 worth

[1] At least 10 countries in this region of Africa have large-scale subsidy programs, representing the largest outlay of their respective agricultural budgets (Dorward et al. 2004; Jayne et al. 2018).

[2] This is significantly scaled down relative to the period from 2004 to 2011 when the benefits went out

of chemical fertilizer and seeds, i.e., a subsidy worth about \$56, a substantial sum for our sample in which we measure average monthly expenditure of a household to be just over \$30. During the 2017-18 to 2019-20 agricultural seasons, the period of our study, the subsidy was disbursed in the form of a paper coupon, which had to be presented for redemption at selected participating local retailers.[3]

We follow our prior work in Tanzania (**aggarwal2022market**) to construct a measure of market access for fertilizer by calculating the travel cost-adjusted price farmers face. We collected prices of fertilizer at the universe of agricultural input dealers in the region, and use Google Maps API to calculate distances from each of the 300 villages in our sample to every retailer. To estimate travel costs, we estimate a multinomial choice model in which we regress the revealed choice of retailer on a location-specific fixed effect (capturing price and unobserved quality) and the distance from the farmer's village to that location (measured separately by road quality). We then calculate travel cost-adjusted prices for every location, imputing travel costs using the model, and find the minimum such price a farmer faces. For both subsidized and unsubsidized fertilizer, we find that travel costs are substantial, on average about \$5.4 for market fertilizer and \$9.1 for subsidized fertilizer (the larger value for subsidized fertilizer is because only certain retailers are selected to participate in the program, so farmers have to travel farther on average to reach them.). Because the subsidy reduces the retail price by 75%, the implied ad-valorem travel cost is sizeable for unsubsidized fertilizer (about 20%) but enormous for subsidized fertilizer (over 100%).

Consistent with our prior work, we find that prices are substantially higher in more remote areas: a standard deviation increase in remoteness is associated with a \$1.9 increase in the travel-cost-adjusted prices of unsubsidized fertilizer (about 6%), and since not all locations sell subsidized fertilizer, a larger \$3.8 increase in the price of subsidized fertilizer (about 21%). Essentially all of this gradient is driven by travel costs, as retail prices are nearly identical across the various locations in our sample (the standard deviation in retail

to about half of all farming households

[3]Typically, larger chains with a strong local presence were selected to participate in the program.

prices is only \$0.56, less than 2% of the mean price of \$29).

However, somewhat surprisingly, we find only modestly lower subsidy redemption in more remote areas: a one standard deviation increase in remoteness is associated with a reduction in the probability of redemption of only about 2 percentage points (on a base of 95%) and is associated with a reduction in the quantity of fertilizer redeemed by 2.6 kgs (on a base of 70 kg).

Nearly universal redemption strongly implies that receiving the FISP subsidy should largely eliminate the gradient in fertilizer usage. To examine this causally, we utilize a three-year panel of households from 300 villages in Southern Malawi. Historically, FISP had been officially targeted towards disadvantaged farmers and beneficiary selection was largely delegated to local chiefs. However, there was also evidence of elite capture and nepotism, due to which, in 2017-18, the government of Malawi transitioned from identifying beneficiaries via local leaders to one in which beneficiaries were selected by officials in the Ministry of Agriculture, purportedly randomly. We examine this policy for the 2017-18, 2018-19, and 2019-20 planting seasons.[4]

While the program was purportedly random, it is not clear that it was so in practice. To select beneficiaries, a list of farming households was first constructed, and then beneficiaries were picked from this list through a computerized lottery in the district office. Unfortunately, we do not have access to this list, so we cannot verify if selection was truly random. Instead, the data we have is from our own surveys, which could show imbalance between beneficiaries and non-beneficiaries even if the selection itself was random, for example if the original allocation was later undone by other actors such as local officials, or if there were selection into the beneficiary list in the first place. It is possible that some households might have been "split" into two households and listed twice, and such splitting may have been more likely for certain groups (such as older or larger households) or perhaps elites may have been

[4]Although we collected data for 2020-21 as well, we do not use it in the analysis because in that year FISP was rebranded as AIP (Affordable Inputs Program) and transitioned from being targeted to universal. Due to the changed nature of the program, we do not include data from 2020-21 in our analysis.

more likely to be listed.

Nevertheless, we use our survey data to examine the allocation, in which we directly asked people about FISP receipt for a number of years prior to the survey. We first examine whether FISP receipt is correlated with household characteristics, and find that older and larger households, households related to the chief, and households who received the program in a prior year are more likely to receive the program in a given year. Thus, we are unable to rely on a randomization to estimate the effect of FISP itself. However, this does not necessarily invalidate our key question of interest, which is not about the impact of FISP itself but on how FISP affects the remoteness gradient. Here, we find that selection does not vary differentially with remoteness.

To causally estimate effects, we use a difference-in-difference specification with household fixed effects, utilizing the fact that we have 3 years of data on FISP receipt and fertilizer usage. We establish the validity of this approach by conducting a placebo check in which we regress FISP receipt in the next year on usage in the current year, and find no effect on current usage, suggesting that contemporaneous fertilizer usage by FISP beneficiaries is not driven by selection into FISP.[5]

In the cross-section, i.e., without using household fixed effects, we find that a standard deviation of remoteness is associated with lower input usage on the extensive margin by 10 percentage points and on the intensive margin by 7.5 kgs. Although we cannot interpret the FISP coefficients causally, we do find descriptively that households that receive FISP use more fertilizer, and that the remoteness gap is non-existent for such households.

To provide causal evidence, we include household fixed effects. We find that fixed effects attenuate this correlation by about a third, but we continue to find that the remoteness gradient is mitigated for FISP households, suggesting that FISP reduces spatial inequality

[5] Note, however, that doing the placebo test for the 2019-20 season would require us to use subsidy receipt data for the 2020-21 season, when the FISP program no longer existed and the fertilizer subsidy program under the AIP became universal. Therefore, we do the placebo test only for 2 out of the 3 years, i.e., only for the 2017-18 and the 2018-19 agricultural seasons. For the sake of consistency then, we also show the main results for just the two years for which we do the placebo check, and find that the results are very similar.

in input usage. While this effect is to be expected from the simple fact that there is no gradient in subsidized fertilizer usage, and because the subsidy is large enough to cover the average farmer's entire land, the result speaks to the (often overlooked) role that subsidies can play in equalizing access gradients.

Our paper adds to the literature about agricultural subsidies, including recent randomized evaluations such as Carter et al. (2013), Carter et al. (2021), Fishman et al. (2022) and Gignoux et al. (2023). In doing so, we corroborate their findings that the subsidy had a large effect on contemporaneous use,[6] though our focus is on extending this literature to look at the heterogenous effect of subsidy by remoteness. Within this literature, we contribute to the substantial workstream about FISP specifically, which also finds large productivity benefits of the subsidy, but is from a period that pre-dates the centralization of the program.[7] By utilizing the centralized allocation, we corroborate the findings from this set of papers in a convincingly causal framework. As a group, these studies show that subsidies can be effective even in contexts with high baseline fertilizer use, such as Malawi.

Our paper is also related to a large and growing literature about the effect of market access on agricultural input adoption. As we note above, this work is closely related to our prior work in Tanzania (**aggarwal2022market**), wherein we documented correlations between remoteness and access to input retailers, and eventually on input usage. We find a steep gradient between remoteness and input usage, and in the current paper, we extend a similar methodology to subsidized inputs. Other parallel work in this literature has tried to causally measure the impact of distance on input usage by focusing on interventions that directly reduce transport costs, such as those that build roads or organize input fairs close to rural villages (in their local markets); papers in this area include Porteous (2019), Gebresilasse (2023), Aggarwal et al. (2024) and Dillon and Tomaselli (2022). We extend this

[6] The findings in Gignoux et al. (2023) are an exception, in that they find that an input subsidy in Haiti actually *decreased* input usage among beneficiaries, a phenomenon that the authors attribute not to the subsidy itself but to misinformation about continued subsidy receipt which crowded-out private input usage.

[7] For example, see Arndt et al. (2016), Dorward et al. (2004), Chirwa et al. (2011), Holden and Lunduka (2012), Chirwa and Dorward (2013), Lunduka et al. (2013), Kilic et al. (2015), Ricker-Gilbert and Jayne (2017), and Basurto et al. (2020)

second literature by studying the interplay between market access and technology adoption in the context of heavily subsidized inputs. In doing so, we also contribute to earlier work on the costs of accessing subsidies, which documents the large effect of small co-pays on the take-up of health products (i.e. Cohen and Dupas 2010, Ashraf et al. 2010, Chang et al. 2019, Kremer and Miguel 2007); though we focus on how consumer decisions are impacted by travel costs rather than by the purchase price at the point of sale.

Our paper is also related to a long-standing literature on overcoming "ordeals" in order to access the benefits of public goods and subsidies, going back to work by Nichols et al. (1971). Typically, the literature has posited these ordeals as imposing non-monetary or "inconvenience" costs, such as standing in line or filing paperwork, and not as pecuniary transaction costs, such as those incurred on transportation. However, as we document in **aggarwal2022market**, in Tanzania, the implied trade costs of procuring fertilizer for farmers are much larger than the measured pecuniary costs, suggesting the presence of other costs, such as the opportunity cost of time or uncertainty related to stock-outs etc, which may be more in the nature of ordeals.

In theory, such ordeals can serve a useful targeting function as only those who sufficiently value the good in question will overcome the ordeal to acquire it, and this has been borne out by some recent empirical evidence (Sylvia et al. 2022; Dupas et al. 2016). However, when the good in question is universally valued, such ordeals could have unintended downsides. For example, there could be exclusion of legitimate beneficiaries, as shown by Deshpande and Li (2019) in the context of US disability benefits. In India, Nagavarapu and Sekhri (2016) show that households that live within half a kilometer of shops which sell grain under the country's subsidy program (PDS) are about 25 percentage points more likely to redeem their benefits than those who live more than half a kilometer away. In Ecuador, Carrillo and Ponce Jarrín (2009) document a distance gradient even in the case of a cash transfer, finding that beneficiaries redeem an average of 1 month less of a $15 monthly cash transfer for every hour of travel time to the redemption center. Even in instances where take-up is inelastic to the costs of overcoming the ordeal, the mechanism may end up being regressive by imposing higher costs on those who are disadvantaged, as is the case in our findings about FISP, as well as those of Carrillo and Ponce Jarrín (2009). Thus, these results also imply that the design of subsidies for highly valued goods should strive to minimize transaction costs for intended recipients.

2 Background and Data

2.1 Institutional Context

Malawi has a unimodal rainfall pattern with a single rainy season which runs from approximately November to April/May.[8] Between 2004 and 2020, the Malawian Ministry of Agriculture provided agricultural input subsidies via the Farm Input Subsidy Program (FISP). While FISP reached as much as two-thirds of farming households in earlier years, only about 15% of households in our sample received the subsidy in any given year between 2014 and 2020.

Traditionally, local leaders had authority over the selection of beneficiaries. However, this system was shown to be subject to nepotism and elite capture (Kilic et al. 2015, Lunduka et al. 2013, Holden and Lunduka 2012). In response, in 2017, the program was transitioned to a centralized beneficiary selection by district officials using a list of eligible farmers.

During the time period that we study (2017-18, 2018-19, and 2019-20), FISP included subsidies for 4 inputs - 50 kg each of NPK and Urea fertilizer, 5 kg of hybrid maize seeds,[9] and 2 kg of hybrid legume seeds. The full package is sufficient for using the recommended amounts of inputs to cultivate the average smallholder's plot (1-2 acres). The market value of this package was about $75 during the years of study, and the FISP subsidy was worth 75% of the market cost, meaning that a beneficiary would have to pay approximately $18

[8] See FEWSNET for the crop calendar for a typical year.

[9] Farmers could also choose to use this voucher for 7 kg of open-pollinated variety maize or sorghum seeds.

for the full package.

Farmers receive these coupons as a single leaf with 4 separate detachable coupons, one for each input, and farmers can redeem as many or as few of these coupons as they wish. However, each individual coupon must be redeemed in its entirety, i.e., it is not possible to redeem a coupon for less than the full quantity (which is likely one reason why there is widespread sharing of subsidized inputs). Because the subsidy is generous, take-up is nearly universal. In our data covering the 2017-18 to 2019-20 agricultural seasons, we find that 95% of beneficiaries redeemed their coupons.

In large part because of the longstanding nature of FISP, Malawi has a well-developed input retail sector with a high density of shops. In our sample, nearly 90% of the villages had an agro-retailer within a 10 km radius, while in our companion work in Tanzania, a far more developed country (but where input markets are weaker), this number was only 62%. However, we also document that the extensive reach of Malawi's input markets is limited largely to full-price retail fertilizer, and the availability of FISP fertilizer is much sparser. Road connectivity in the country is at par for the region - Malawi has 130 km of road per 1,000 sq km, of which about a quarter is paved, which mimics the averages for sub-Saharan Africa as a whole, although it is higher than Tanzania (92 km and 13% respectively).

2.2 Data

For our analysis, we use surveys of households and agricultural input dealers that were collected for an evaluation of unconditional cash transfers in 300 villages in Chiradzulu and Machinga districts in the Southern Region of Malawi.[10] To construct an analysis sample of households, we randomly selected 10 households per village in each of the 300 villages for surveys, successfully completing baseline surveys with 2,944 households in April-July 2019, out of which we drop 380 farmers because either they do not own land (one of the criteria for being listed), do not grow maize (the focus crop for FISP), or because they were either

[10] See **GD_draft_2023** for more details.

spouse of children of the village chief. Thus our final sample for this analysis is 2,564 farmers (i.e., 87% of the baseline sample). In the baseline survey, we collected information on input usage and harvest for the 2017-18 season, and input usage for the 2018-19 planting seasons; in the endline, we collected information on the 2018-19 harvest and the entire 2019-20 season. Our surveys collected standard demographic data, as well as for each season, information about FISP receipt, redemption, and subsidy sharing. We also collected detailed questions on input usage (from all sources) and harvest. To precisely measure market access, we also included questions on the location and cost of purchasing inputs (including travel costs).

For information on the price and purchase location for fertilizer, we conducted a census of agriculture input sellers in the area (encompassing the 2 study districts as well as 7 contiguous districts - Balaka, Blantyre, Mangochi, Mulanje, Phalombe, Thyolo, and Zomba).[11] The census collected basic information on the availability and price of seeds and fertilizer. After the census, we followed up with detailed surveys with each retailer, which took 1-1.5 hours to complete. We identified a total of 466 retailers that sold fertilizer in the census, 431 of which completed the longer survey (92%).

The census occurred in 2 waves: March 2019 in our study districts, and November 2019 in the remaining districts. The longer surveys were conducted in November-December 2019. Because the latter survey was conducted in a shorter temporal frame, we rely on this as our primary measure of prices (to minimize temporal variation). For the 35 agro-dealers that completed the census but not the longer survey, we use the prices reported in the census. Once this database was constructed, we examined it for outliers and found that 8 retailers clearly had data errors (1.7% of the sample), and had prices far from the mean (likely due to missing zeroes or because of unit conversion mistakes). We rely on the law of one price within markets to correct these as 7 out of these 8 were located in markets with at least one other agro-dealer.[12]

[11]We included these neighboring districts to be able to accurately calculate price dispersion within our study region, since farmers near district borders may travel across them to access inputs.

[12]In our data, we find that the law of one price holds within locations for which we have data on multiple agro-dealers: the standard deviation of the price within a location is only 10% of the mean.

We show some agro-dealer summary statistics in Table A1. The average agro-dealer has been in business for 6 years and about half the agro-dealers are authorized to sell under FISP. The average operation is fairly large, having earned nearly $33,000 in revenue in the previous year, which is more than 50 times the per capita income of Malawi; however, there is a lot of heterogeneity in size. Agro-dealers almost universally sell both NPK and Urea, the two most common forms of fertilizer in Malawi.

3 Market access, remoteness and prices

Our analysis is closely related to our prior work in Tanzania (**aggarwal2022market**), which examined correlations between remoteness, market access, and input usage. This paper extends that work to exploit variation in the subsidy (and hence input usage). We use many of the same variable definitions and regression specifications as in that prior work. In this section, we briefly describe these concepts (and refer the reader to our prior paper for a more extended discussion, as well as alternative robustness checks).

3.1 Defining market access and remoteness

Similar to Donaldson and Hornbeck (2016), we define market access in terms of distance to local population centers. Specifically, we focus on 3 major markets: Blantyre, Lilongwe, and Zomba, the three biggest cities in the country, of which Blantyre and Zomba are the closest regional market towns for the sample of villages in our study (the median distance to Zomba is 44 km and that to Blantyre is 34 km). The specific measure is:

$$MA_v = \sum_h \tau_{hv}^{-\theta} pop_h \tag{1}$$

where pop_h is the population of the hub, and $\tau_{hv}^{-\theta}$ is the elasticity-adjusted trade costs of reaching each hub. We measure τ_{hv} from surveys in which we asked about the cost of travel to purchase fertilizer.[13] For $-\theta$, we appeal to the substitution elasticity across agro-retailers that we estimated in our prior work in Tanzania (-7.9).[14] We then standardize the remoteness measure to have mean 0 and standard deviation 1 (and invert the sign of MA_v to be negative, so that it measures remoteness rather than market access). Note that market access is indexed at the village-level v, rather than by farmer; this is because much of the variation in distance is across rather than within villages, and because we have found Google Maps API information to not be very reliable within small rural villages.

As would be expected, more remote villages differ from more proximate villages in several dimensions: Table A2 shows the relationship between farmers' characteristics and our measure of market access. We find several significant correlations: farmers in more remote villages are more likely to be related to the local chief, have fewer years of education, and are less likely to have received FISP in the prior year. In addition, remote villages have higher productivity as measured by FAO-GAEZ agricultural productivity measures. See Appendix B for more detail on these measures. As discussed later, to account for these differences, we use household fixed effects in our main specifications, and perform a placebo test to justify our main specification.

3.2 Agrodealer choice and estimating travel costs

In **aggarwal2022market**, we estimate travel costs via two methods: a calibration method that includes only pecuniary costs, and a method that estimates a structural choice model to account for all pecuniary and non-pecuniary costs (such as the risk of stock-outs requiring multiple trips, lack of information about options in distant locations, the valuation of own time, and related issues). We find that total costs are about 4-5 times higher than pecuniary

[13]Farmers must make a round-trip, and the return trip is more costly since the farmer carries the fertilizer with her. In our surveys, we estimate that the cost of transporting a bag is about 40% of that of a person; therefore, a farmer effectively incurs 2.4 times the one-way travel costs.

[14]In that prior work, we backed out structural parameters from an equilibrium of spatial demand and agroretailer supply. Doing this required survey data from a representative sample of farmers, which we do not have in the current study (since villages were selected based on characteristics such as size). For this reason, we instead rely on our published work for these parameters.

costs in Tanzania, and we find that model-estimated costs and behavior match observable moments quite well. In this paper, we, therefore, rely solely on model-estimated costs.

As mentioned in Section 2.2, our surveys with agro-dealers give us the universe of prices in the area. We then use Google Maps API to estimate driving times and distances from every village in our sample to every identified agricultural input dealer. In our survey, we asked respondents about every instance in which they purchased inputs, the name and location of the relevant input dealer and the cost of travel (as well as other pertinent details of the transaction, such as the price, quantity, and whether a FISP coupon was used). However, we note that many farmers did not accurately recall the name of the retailer, and thus the estimation is based instead on the location of purchase (which people did recall).

We estimate trade costs using a multinominal logit (similar in form to Eaton and Kortum 2002), where farmers choose the location of fertilizer purchase based on price, quality, and bilateral ad-valorem trade costs. For the trade costs, we assume that the ad-valorem rate is a function of distance traveled on different road types (i.e. main roads and rural roads).[15] Further technical details are relegated to Appendix D.[16]

To measure baseline trade costs, we use a sample of farmers who did not receive FISP. We do this because redemption of FISP is nearly universal but FISP is only offered at certain retailers, so non-FISP fertilizer offers a richer variety of choices for farmers to make (including the option to not buy at all). The results from the estimation (presented in Table D1) indicate that rural roads have a higher impact on ad-valorem costs than main roads. Specifically, a one km increase in main road travel increases the ad-valorem trade cost by roughly 1.8%, while a 1 km increase in rural road travel increases costs by 2.6% (i.e. the marginal effect of rural road travel is about 45% greater). To calculate a dollar value of

[15]In Malawi, road names are based on pavement classification, which are "M" (main roads, primarily paved), "S" (secondary, often unpaved) and "T" (tertiary, unpaved local feeder roads). Google API includes this information.

[16]This discrete choice estimation is similar to Kremer et al. 2011, who use distance to local springs to quantify the willingness to pay for protections for water quality. The main difference with our work is that they do not adopt a within-destination approach to their discrete choice model since that would absorb the water sanitation treatment by destination. We use destination dummies to absorb all observed and unobserved factors in choosing an agro-dealer location, leaving only distance variation to explain choices.

estimated trade costs, we use the retail price and apply the estimated coefficients to recover the unit-value of trade costs.

3.3 Travel cost-adjusted prices

We now turn to calculating the travel cost-adjusted price farmers face at every possible location. The farmer's best option is the location at which this cost is minimized:

$$p_v^{min} = \min_j \{r_j + c_{vj}\} \tag{2}$$

where r_j is the price at agro-retailer j and c_{vj} is the cost of traveling to agro-retailer j, and returning to village v with a bag of fertilizer.

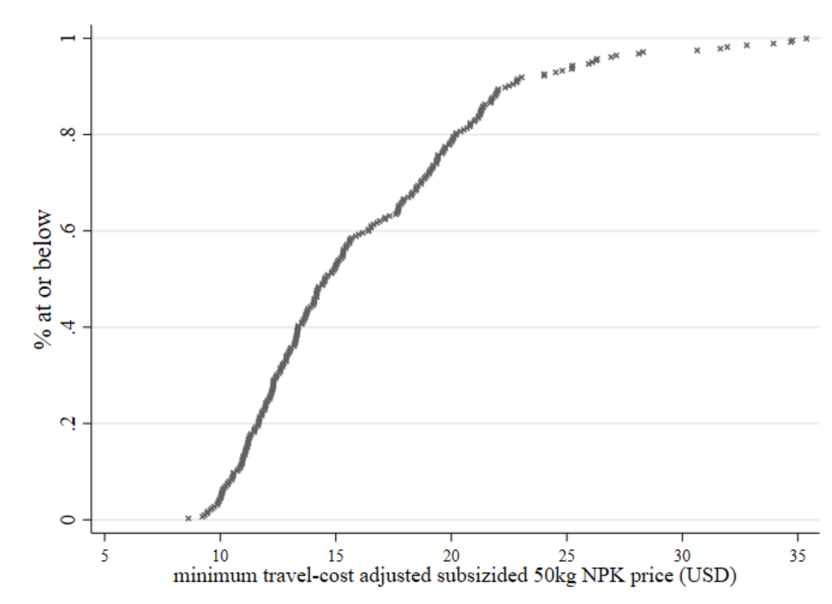
Figure A1 plots distributions of prices which show how the decision rule affects calculated price dispersion. First, Panel A shows the unconditional distribution of subsidized FISP prices at all of the 153 agro-retailers in the censused area that accept FISP. The mean price is \$10.5 with a standard deviation of about \$3. However, when we implement the decision rule (2), we find that only 12 total retailers are chosen. Eight of these are located within the study districts (out of 65 total ag-dealers in those districts), and 4 are located outside (all in the hub locations of Blantyre or Zomba). Panel B shows prices for those 12 retailers. As expected, the mean price is lower than in the full sample (\$8.75) and the standard deviation is much smaller (\$0.56). Thus retail price heterogeneity is minimal under this decision rule, and variation in travel cost-adjusted prices is driven by variation in travel costs.[17]

Figure 1 plots the distribution of travel cost-adjusted prices for the lowest-cost option for each village. Despite the modest variation in retail prices, there is clear heterogeneity after accounting for travel costs: the travel cost-adjusted price at the 90th percentile is \$24.7 compared to \$10.7 at the 10th percentile. The average retail price is \$8.75 (SD \$0.56), while

[17]For reference, Figure A2 shows a similar figure for the *retail* price. Here in Panel A, we find an unconditional mean and standard deviation of \$31.13 and \$3 for the universe of shops in the area. In Panel B, we find that only 20 retailers are chosen by the decision rule (17 within the study area), and the mean and standard deviation is \$28.9 and \$0.5.

the average travel cost is \$9.1 (SD \$9).

Figure 1: CDF of Travel Cost Adjusted Prices across Villages - Subsidized Fertilizer



Note: Unit of observation is the village (N = 300).

3.4 The relationship between remoteness and market access

Table 1 shows the relationship between remoteness and various measures of access to NPK fertilizer, the most widely available input in the country, along with Urea.[18] First, in Panel A we show some summary measures of access to retailers. We find that 88% of villages have at least 1 input retailer within 10 km. Access is superior to that documented in our prior work in Tanzania (**aggarwal2022market**), where only about 60% of villages were within 10 km of a retailer. However, since only a subset of retailers participate in FISP, we find that only 62% of the villages have at least 1 retailer accepting FISP. In fact, this level of access is on par with Tanzania; that is, access to a retailer accepting FISP is roughly on par with access to any retailer in Tanzania, even though the overall retail market is substantially larger in Malawi. The average distance to the nearest retailer is 5.8 km for market fertilizer, and 10.1 km for FISP.

[18] We show results only for NPK here. Those for Urea are very similar and available upon request.

Despite a deeper overall market, we still observe a large remoteness penalty. A standard deviation increase in remoteness leads to a 14 percentage point decline in the likelihood of having an agrodealer within 10 km of the village (and an 11 percentage point decline in the probability for having a retailer that accepts FISP within 10 km). The distance to the nearest agro-retailer also increases substantially, with every standard-deviation of remoteness adding between about 1.9 km (mean 5.8 km) for market fertilizer and 3.6 km (mean 10.1 km) for FISP fertilizer, i.e., about a 33% increase for either "type" of fertilizer.

We then turn to analyzing prices in Panels B1 (for market fertilizer) and B2 (for FISP fertilizer). For both subsidized and unsubsidized fertilizer, we first show the price inclusive of transport cost (at that agro-dealer for each village where the travel-cost adjusted price is minimized), and then decompose the price into the retail price and the travel cost (at the same agro-dealer). For market fertilizer, we find the average minimum price to be \$34.3, with a standard deviation of \$4.5. In the decomposition, we see that \$29 of this is the retail price, with a small SD of only \$0.6. To this, another \$5.4 gets added due to travel costs (i.e. 18.6% ad valorem at the mean, with a large SD of \$4.4), leading to an effective price of \$34.3. In the regression in Column 2, we see that 1 standard deviation increase in remoteness is associated with a substantial increase in this cost (of about \$2 per SD, or 5.5%), coming almost entirely from travel costs.[19]

[19] Note that despite its input market depth, Malawi has much higher fertilizer prices than the world price, which was about \$19 for 50 kg of NPK during this period. We observe a retail price of about \$29, nearly 50% higher. Fertilizer prices in Africa are higher than the world price, largely due to travel costs since fertilizer is typically imported. Such costs are substantial in a landlocked country like Malawi.

Table 1: Access to retailers, travel cost-adjusted price heterogeneity, and remoteness

	Mean (SD)	Coefficient on remoteness measure
Panel A: Summary Measures of access to input retailers		
Has atleast 1 agro-retailer within 10 kms of village which		
sells fertilizers	0.88	-0.14***
	(0.33)	(0.05)
sells FISP fertilizers	0.62	-0.11**
	(0.49)	(0.04)
Distance (in kms) to nearest agro-retailer which		
sells fertilizer	5.83	1.93***
	(4.62)	(0.49)
sells FISP fertilizer	10.14	3.61***
	(8.76)	(0.92)
Panel B1: Market Fertilizer		
Minimum travel cost adjusted price	34.34	1.90***
	(4.51)	(0.30)
Decomposition of price between retail price and travel costs		
Retail price at location w. lowest travel cost adjusted price	28.91	0.06**
	(0.56)	(0.03)
Cost of travel	5.43	1.84***
	(4.41)	(0.29)
Panel B2: FISP Fertilizer		
Minimum travel cost adjusted price	17.89	3.81***
	(9.52)	(0.66)
Decomposition of price between retail price and travel costs		
Retail price at location w. lowest travel cost adjusted price	8.78	-0.08***
	(0.58)	(0.03)
Cost of travel	9.11	3.89***
	(9.39)	(0.66)
Observations	300	300

Notes: Results are shown for NPK fertilizer. Regressions are at the village level. Each row represents a separate regression of the given dependent variables on standardized remoteness. Robust standard errors are in parentheses. ***, **, and * represent significance at 1%, 5%, and 10%, respectively. All costs are in USD, calculated using an exchange rate of 714 MWK to 1 USD.

For FISP fertilizer, we find that while farmers only pay 25% of the retail price, they still incur roughly $9 in trade costs on average (about 64% more than for retail fertilizer, due to the fact that only some retailers accept coupons). We also see a much larger correlation with remoteness, where 1 SD of remoteness is associated with $3.9 higher costs, suggesting that retailers in more remote locations are less likely to redeem FISP - another manifestation of the costs of being remote.

3.5 Predictions

In the status quo, i.e., without FISP, the expected net return to using fertilizer for farmer i in village v is:

$$E[R_{iv}] = E[p_v * (y_{iv}^{fert} - y_{iv}^0)] - c - \tau_v$$

where p_v is the price of output, and y_{iv}^{fert} and y_{iv}^0 are yields with and without fertilizer, c is the pecuniary cost of fertilizer and τ_v is the travel cost. We write c as unrelated to remoteness (as shown in Table 1, because price dispersion on that measure is minimal. However, τ_v does vary dramatically between villages.

Thus, conditional on productivity, we will observe a remoteness gradient, simply because fertilizer is more expensive in remote areas due to travel costs.[20] The subsidy dramatically reduces c_v by about 75%, but increases τ_v, because only some retailers accept FISP. Overall, however, the travel cost-adjusted price of fertilizer falls by about 50% (Table 1), and thus the expected return will be higher for most or all farmers, and will be less likely to be negative for farmers with large values of τ_v. Thus, we will expect to observe a much smaller distance gradient for subsidized fertilizer.

3.6 Remoteness and Coupon Redemption

Having established that travel cost-adjusted prices for subsidized (and unsubsidized) fertilizer are higher in remote areas, our next question is about the extent to which these prices translate into lower redemption of the subsidy (and mute its adoption benefits). For each FISP beneficiary, we asked whether the coupon was redeemed and, if so, for what quantity.

[20] Assuming for the moment that farmer are risk neutral, then we interpret this as saying that that τ_v varies enough such that the expected return is positive in some areas and negative in others.

Table 2: Remoteness and Coupon Redemption

	Probability of Redemption		Quantity redeemed (kg)	
	(1)	(2)	(3)	(4)
Remoteness (β)	-0.02***	-0.02**	-2.05*	-2.61*
	(0.01)	(0.01)	(1.15)	(1.53)
Mean	0.95	0.95	70.35	70.35
Observations	930	930	930	930
Household controls	N	Y	N	Y

Notes: Regressions are restricted to FISP beneficiaries. The dependent variable is an indicator for redeeming FISP in Columns 1-2 and the quantity redeemed in Columns 3-4. See text for definition of remoteness measure. Data is from three agricultural seasons, 2017-18 to 2019-20. All regressions include year fixed effects. Standard errors clustered at the village level are in parentheses. ***, **, and * represent significance at 1%, 5%, and 10%, respectively.

In Table 2, we regress subsidy redemption on remoteness, on the extensive margin (Columns 1-2) and on quantities (Columns 3-4). Odd-numbered columns show regressions without controls, while even-numbered columns include all the variables analyzed in Table A2 as covariates. Columns 1-2 show that redemption is nearly universal to begin with: 95% of those who receive the subsidy redeem it. More remote farmers are less likely to redeem, but only slightly: an additional standard deviation of remoteness is associated with a decline of only 2 percentage points on the extensive margin. Columns 3-4 show evidence of modestly lower quantities redeemed in remote areas: an additional standard deviation is associated with about 2-2.6 kg lower quantities (about 3.5% on a base of 70 kg), significant at 10%. This is a relatively small quantity, and therefore, has limited economic significance. As discussed in more detail later, we conjecture that the reason for the modest correlation between distance on redemption is because the ∼75% subsidy is so generous that people are willing to incur the transport costs to avail of it, whereas they are less likely to do so at full market prices.

4 Measuring the impact of FISP

4.1 Was the FISP allocation exogenous?

As discussed above, official guidelines traditionally targeted FISP towards certain groups, such as older or resource-poor farmers, and in addition chiefs had at least some amount of discretion over distribution within their village (i.e. Basurto et al. 2020). While FISP was purportedly randomized during the study period, it may not necessarily have been implemented as such in practice, and even if it were, may appear non-random in our sample due to mismatch between our list of farmers and the official list used for the allocation. To examine the allocation, we examine how time-invariant characteristics vary between FISP beneficiaries and non-beneficiaries in Table A3. Here, we pool the 3 study years and check for covariate balance between FISP beneficiaries and non-beneficiaries during this period.

For each of the characteristics in this table, we first report its sample mean, followed by the coefficient from a bivariate regression of that variable and an indicator for FISP receipt (Column 2), and then in Column 3, we report the coefficients from a multivariate regression for all the coefficients together. We find several statistically significant coefficients, including household head age, being related to the chief, household size, land size, and receiving FISP in the prior year. While these results do not necessarily imply that the program was not random in the original allocation, they do show that it is not possible to causally estimate the impact of FISP directly across households.

While the above results suggest that there is some amount of selection into FISP, our primary goal in this paper is not to estimate the effect of FISP itself, but to examine how FISP affects the spatial gradient in input usage. The main threat to identification is thus whether any selection into FISP differs by proximity to market centers. To examine this, we run a specification as follows:

$$Y_{ivt} = \alpha FISP_{ivt} + \beta R_v + \gamma FISP_{ivt} * R_v + \tau_t + \lambda_v + \varepsilon_{ivt} \quad (3)$$

The primary coefficient here is γ, which shows how the difference in observables between FISP beneficiaries and non-beneficiaries varies with remoteness. Results are shown in Table 3. Again, while we see differences between FISP and non-FISP households on several dimensions,[21] we find that the interaction between FISP and remoteness is statistically insignificant for all variables with the exception of receiving FISP in the prior year. In regards to that variable, prior receipt of FISP is less strongly correlated with current receipt in more remote villages; all else equal, this should work against our empirical results (to the extent that receiving FISP in a prior year predicts current usage). Altogether, we view these results as suggestive that selection into FISP does not vary with remoteness.

[21]Note that the coefficients on FISP in this table appear different than those in Table A3 because Table A3 regresses the likelihood of getting FISP on the covariates while the later tables regress the coefficient on FISP (and the interaction).

Table 3: FISP Status and Remoteness

	Mean (SD)	FISP	FISP × Remoteness
		Coefficients on variables:	
	(1)	(2)	(3)
Household head age (in 10 years)	4.37	0.20***	-0.01
	(1.51)	(0.06)	(0.06)
Related to chief	0.48	0.03*	0.01
	(0.50)	(0.02)	(0.02)
=1 if female headed household	0.40	-0.01	-0.01
	(0.49)	(0.02)	(0.02)
Number of household members	4.88	0.15*	0.10
	(2.04)	(0.08)	(0.08)
Education level of respondent	4.68	-0.13	0.12
	(3.38)	(0.12)	(0.12)
FISP coupon received last year	0.14	0.04**	-0.03*
	(0.35)	(0.02)	(0.02)
Land size (acres)	1.71	0.16***	-0.00
	(1.40)	(0.05)	(0.05)
Observations	6,827		
Households	2,564		

Notes: Data is from three agricultural seasons, 2017-18, 2018-19 and 2019-20. Mean of FISP status is 13%. Data is restricted to households that grew maize. Each row represents a separate regression of the dependent variable on on FISP status and FISP × Remoteness. Column 1 shows the mean and standard deviation of household characteristics, while Columns 2-3 show regression coefficients. All regressions include village fixed effects. Standard errors are clustered at the village level and are in parentheses. ***, **, and * represent significance at 1%, 5%, and 10%, respectively.

4.2 Difference-in-differences approach

Based on the results so far, it is not fully clear as an outside observer if the randomization rule was followed. As a result, the rest of the paper incorporates household fixed effects in a difference-in-differences framework, allowing us to abstract away from any potential selection issues.

$$Y_{ivt} = \beta R_v + \gamma FISP_{ivt} + \delta FISP_{ivt} * R_v + \mu_i \ + \tau_t + \varepsilon_{iv} \tag{4}$$

where Y_{ivt} is the relevant variable for input usage by household i in village v in year t, R_v is our (standardized) measures of remoteness (see Equation 1 in Section 3.1), $FISP_{ivt}$ is an indicator for receiving FISP, X_{iv} are time-invariant household-level controls, μ_i is a household fixed effect, and τ_t is a year fixed effect. Standard errors are clustered at the village level. In this specification, β shows the input adoption - remoteness gradient for non-FISP beneficiaries, and δ shows whether this gradient is attenuated for FISP beneficiaries. Please note that in specifications with the household fixed effect, the β coefficient will fall out as remoteness is a time-invariant, village-level trait.

Placebo check

To justify this specification, we first run a placebo check in which we regress current input usage on FISP status in the subsequent season, as well as an interaction with FISP in the subsequent year and remoteness, including household fixed effects. Our goal is to examine if FISP beneficiaries would have used fertilizer anyway, even in a counterfactual scenario with no subsidy. Note, however, that we can only run this specification for the 2017-18 and 2018-19, because the placebo is impossible in 2019-20 (since FISP was subsequently transitioned to AIP). We run the following specification:

$$Y_{ivt} = \beta R_v + \gamma FISP_{ivt+1} + \delta FISP_{ivt+1} * R_v + \mu_i \ + \tau_t + \varepsilon_{iv} \tag{5}$$

Results are shown in Table 4. Columns 1 and 3 show regressions including only the indicator for FISP in the next year, while Columns 2 and 4 also include the interaction between remoteness and FISP next year. We see negative, insignificant point estimates on

FISP in all specifications. FISP * remoteness is also negative and small, but significant at 10% in Column 4. While this coefficient is small and borderline significant, it also goes against our main results and if anything should attenuate effects.

Table 4: Placebo Check

	=1 if used fertilizer		KGs of fertilizer used	
	(1)	(2)	(3)	(4)
Received FISP next year	-0.01	-0.01	-0.30	-0.74
	(0.02)	(0.02)	(2.45)	(2.47)
Received FISP next year × Remoteness		-0.03		-3.35*
		(0.02)		(2.00)
Dependent variable mean	0.80	0.80	43.75	43.75
Observations	4455	4455	4455	4455
Households	2530	2530	2530	2530

Notes: Table regresses current fertilizer usage in year t on FISP allocation status in year $t+1$. Data is from two agricultural seasons, 2017-18 and 2018-19 (year t). All regressions include year and household fixed effects. Standard errors clustered by village are in parentheses. ***, **, and * represent significance at 1%, 5%, and 10%, respectively.

Main results

The main difference-in-difference results are shown in Table 5. In the table, we first present results with only household controls, to descriptively show the correlation between remoteness and input usage. As in our work in Tanzania, a context which does not subsidize fertilizer, we find that FISP non-beneficiaries' usage is decreasing in remoteness: one standard deviation of remoteness is associated with a 10 percentage point decline in the likelihood of using fertilizer, and about a 7.5 kg (or 12%) decline in input quantities. These differences are substantial but relatively smaller than in our prior work in Tanzania in 2016-18, where we find a similar point estimate of about 6-14 kg per SD, but where the baseline usage was much lower at that time (only 19 kg, compared to 54 kgs here).

We also note that in this specification, FISP is associated with a 13 percentage point increase in usage on the extensive margin and 15 kgs on the intensive margin. Finally, the coefficient for the interaction between FISP and remoteness is 11 percentage points and 12 kgs on the extensive and intensive margins respectively. Thus, in the cross-section, a standard deviation increase in remoteness is associated with a sizeable reduction in fertilizer use among non-beneficiaries, but there is no such gap among beneficiaries.[22]

Table 5: FISP and the Input Adoption-Remoteness Gradient

	=1 if used fertilizer		KGs of fertilizer used	
	(1)	(2)	(3)	(4)
FISP	0.13***	0.09***	14.78***	9.56***
	(0.01)	(0.01)	(1.88)	(1.95)
Remoteness (β)	-0.10***		-7.49***	
	(0.01)		(1.19)	
FISP $\times$ Remoteness (γ)	0.11***	0.07***	12.14***	9.98***
	(0.01)	(0.01)	(2.11)	(2.03)
p-value: $\beta + \gamma$	0.85		0.04	
Dependent variable mean	0.83	0.83	53.71	53.71
Observations	6827	6827	6827	6827
Households	2564	2564	2564	2564
Household FE	N	Y	N	Y

Notes: Columns 1 and 3 include all the variables in Table 3, while Columns 2 and 4 include household fixed effects. Data is restricted to households that grew maize. Table includes data from three agricultural seasons, 2017-18, 2018-19 and 2019-20. Remoteness is a standardized measure at the village level. All regressions include year fixed effects. Standard errors clustered by village are in parentheses. ***, **, and * represent significance at 1%, 5%, and 10%, respectively.

To provide causal evidence, we include individual fixed effects in Columns 2 and 4 and find an attenuation in coefficients of about one-third, suggesting that there is at least some selection into FISP. Now, turning to examining the coefficients, we find that FISP alone

[22]It is somewhat surprising that beneficiary farmers in remote areas use more fertilizer (the sum of β and γ is about 4 kg, significant at 5%) since we show earlier in Table 2 that remote farmers redeem about 2 fewer kg of fertilizer. We investigate this in Table A4, which shows how fertilizer is shared between beneficiaries and non-beneficiaries. We find that beneficiaries in remote areas are less likely to share fertilizer with non-beneficiaries, explaining how remote farmers are able to use more fertilizer despite redeeming less.

increases the likelihood of usage by 9 percentage points and the quantities used by 10 kgs. These point estimates are in the same ballpark as those in Carter et al. (2021) which show about a 16 percentage point effect on the extensive margin and a 17 kg one on the intensive margin, albeit on a much smaller base than in our context.[23]

Our main coefficient of interest, however, is not FISP alone, but the interaction between FISP and remoteness. Here, we find effect sizes of 0.07 on the extensive margin and 10 kg in terms of quantities. Therefore, our results suggest that FISP has a larger effect in more remote areas than in the non-remote ones, which is due to lower baseline input usage in remote areas. This suggests that FISP narrows the spatial input gap.[24] Finally, we also examine our results using only the 2 seasons for which we are able to conduct the placebo test (2017-18 and 18-19) in Table A7, results look qualitatively similar.

In Appendix C, we perform several robustness checks, to address issues about the possible selection into the sample. In Table C1-Table C3, we redo our analysis while dropping those who received FISP in 2016-17, to (partially) address the issue that FISP is autocorrelated over time. In Table C4-Table C6, we drop those who are related to the chief. Even with these restrictions, results are very similar to what we find in our main specifications.

5 Conclusion

Fertilizer subsidies are one of the most common policy tools to increase input usage, but farmers must often redeem their subsidies at existing retailers, incurring travel costs in the process. What effect do these travel costs have on how the benefits of these programs accrue over space? We study this question in the context of Malawi's generous Farm Input Subsidy Program, which provides a 75% subsidy on about $75 worth of inputs.

[23]We also calculate p-values using randomization inference in Table A5 and results are qualitatively unchanged.

[24]An important question for policymakers interested in understanding the efficacy of FISP for farmers (both proximate and remote) is about its eventual impact on harvest output. In general, empirical researchers have found fertilizer use to be a noisy predictor of yields. We check the effect of FISP on harvests for our sample, and report the results in Table A6. As with other work, we do not find significant effects, but they are directionally consistent with the findings in the remainder of the paper.

Despite meaningfully higher travel costs in remote villages, we find that redemption rates are only slightly lower in such villages. This result stands in some contrast to earlier work showing how small costs discourage the adoption of a variety of products, largely in the context of preventive health (i.e. see the review in Dupas and Miguel 2017), as well as some from other kinds of products, such as index insurance (Cole et al. 2013) and electricity (Lee et al. 2020). The most likely explanation for the difference in our results is that in the case of subsidized fertilizer, the product is so valued that relatively smaller travel costs are not a major deterrent. Moreover, in the case of FISP, fertilizer has market value and could potentially be shared or sold if necessary.

Our main contribution is to examine how the subsidy affects usage among more remote farmers. As in our prior work in Tanzania, we document a statistically significant and economically meaningful negative relationship between remoteness and input usage. However, we find that this gradient is attenuated among FISP beneficiaries, meaning that FISP lowers the remoteness penalty in input adoption.

Thus, we show that in addition to increasing average input usage, the FISP subsidy plays an important role in reducing spatial disparities, and can complement other policies such as reducing transportation costs via infrastructure improvements. This additional benefit afforded by subsidies should be explored further in future work on the design of subsidy programs. Nevertheless, even with nearly universal redemption, the welfare benefit of the subsidy is still smaller in remote areas, since remote farmers must travel further to redeem and thus pay higher travel cost-adjusted prices. While these added travel costs were modest enough (relative to the benefit of the inputs) to not discourage redemption in this case, this will not necessarily be true in other contexts.

References

Aggarwal, Shilpa (2018). “Do rural roads create pathways out of poverty? Evidence from India”. *Journal of Development Economics* 133: 375–395.

Aggarwal, Shilpa, Dahyeon Jeong, Naresh Kumar, David Sungho Park, Jonathan Robinson, and Alan Spearot (2024). “Shortening the path to productive investment: Evidence from input fairs and cash transfers in Malawi”. *Journal of Development Economics* 170: 103288.

Arndt, Channing, Karl Pauw, and James Thurlow (2016). “The Economy-wide Impacts and Risks of Malawi’s Farm Input Subsidy Program”. *American Journal of Agricultural Economics* 98 (3): 962–980.

Ashraf, Nava, James Berry, and Jesse M Shapiro (2010). “Can higher prices stimulate product use? Evidence from a field experiment in Zambia”. *American Economic Review* 100 (5): 2383–2413.

Basurto, Maria Pia, Pascaline Dupas, and Jonathan Robinson (2020). “Decentralization and efficiency of subsidy targeting: Evidence from chiefs in rural Malawi”. *Journal of Public Economics* 185: 104047.

Brooks, Wyatt and Kevin Donovan (2020). “Eliminating Uncertainty in Market Access: The Impact of New Bridges in Rural Nicaragua”. *Econometrica* 88 (5): 1965–1997.

Carrillo, Paul E. and Juan Ponce Jarrín (2009). “Efficient delivery of subsidies to the poor: Improving the design of a cash transfer program in Ecuador”. *Journal of Development Economics* 90 (2): 276–284.

Carter, Michael, Rachid Laajaj, and Dean Yang (2013). “The impact of voucher coupons on the uptake of fertilizer and improved seeds: evidence from a randomized trial in Mozambique”. *American Journal of Agricultural Economics* 95 (5): 1345–1351.

— (2021). “Subsidies and the African Green Revolution: Direct Effects and Social Network Spillovers of Randomized Input Subsidies in Mozambique”. *American Economic Journal: Applied Economics* 13 (2): 206–29.

Chang, Wei, Primrose Matambanadzo, Albert Takaruza, Karin Hatzold, Frances M. Cowan, Euphemia Sibanda, and Harsha Thirumurthy (2019). “Effect of Prices, Distribution Strategies, and Marketing on Demand for HIV Self-testing in Zimbabwe: A Randomized Clinical Trial”. *JAMA Network Open* 2 (8): e199818–e199818.

Chirwa, Ephraim and Andrew Dorward (2013). *Agricultural input subsidies: The recent Malawi experience*. OUP Oxford.

Chirwa, Ephraim, Mirriam Matita, and Andrew Dorward (2011). *Factors influencing access to agricultural input subsidy coupons in Malawi*. Future Agricultures Consortium.

Cohen, Jessica and Pascaline Dupas (2010). "Free Distribution or Cost-Sharing? Evidence from a Randomized Malaria Prevention Experiment". *The Quarterly Journal of Economics* 125 (1): 1–45.

Cole, Shawn, Xavier Giné, Jeremy Tobacman, Petia Topalova, Robert Townsend, and James Vickery (2013). "Barriers to Household Risk Management: Evidence from India". *American Economic Journal: Applied Economics* 5 (1): 104–35.

Deshpande, Manasi and Yue Li (2019). "Who Is Screened Out? Application Costs and the Targeting of Disability Programs". *American Economic Journal: Economic Policy* 11 (4): 213–48.

Dillon, Andrew and Nicolo Tomaselli (2022). "Making Markets: Experiments in Agricultural Input Market Formation".

Donaldson, Dave and Richard Hornbeck (2016). "Railroads and American economic growth: A "market access" approach". *The Quarterly Journal of Economics* 131 (2): 799–858.

Dorward, Andrew, Jonathan Kydd, Jamie Morrison, and Ian Urey (2004). "A policy agenda for pro-poor agricultural growth". *World Development* 32 (1): 73–89.

Dupas, Pascaline, Vivian Hoffmann, Michael Kremer, and Alix Peterson Zwane (2016). "Targeting health subsidies through a nonprice mechanism: A randomized controlled trial in Kenya". *Science* 353 (6302): 889–895.

Dupas, Pascaline and Edward Miguel (2017). "Impacts and determinants of health levels in low-income countries". *Handbook of economic field experiments*. Vol. 2. Elsevier: 3–93.

Eaton, Jonathan and Samuel Kortum (2002). "Technology, geography, and trade". *Econometrica* 70 (5): 1741–1779.

Fishman, Ram, Stephen C Smith, Vida Bobić, and Munshi Sulaiman (2022). "Can Agricultural Extension and Input Support Be Discontinued? Evidence from a Randomized Phaseout in Uganda". *Review of Economics and Statistics* 104 (6): 1273–1288.

Gebresilasse, Mesay (2023). "Rural roads, agricultural extension, and productivity". *Journal of Development Economics* 162: 103048.

Gignoux, Jérémie, Karen Macours, Daniel Stein, and Kelsey Wright (2023). "Input subsidies, credit constraints, and expectations of future transfers: Evidence from Haiti". *American Journal of Agricultural Economics* 105 (3): 809–835.

Holden, Stein and Rodney Lunduka (2012). “Do fertilizer subsidies crowd out organic manures? The case of Malawi”. *Agricultural Economics* 43 (3): 303–314.

Jayne, Thomas S, Nicole M Mason, William J Burke, and Joshua Ariga (2018). “Taking stock of Africa’s second-generation agricultural input subsidy programs”. *Food Policy* 75: 1–14.

Kilic, Talip, Edward Whitney, and Paul Winters (2015). “Decentralised beneficiary targeting in large-scale development programmes: insights from the Malawi farm input subsidy programme”. *Journal of African Economies* 24 (1): 26–56.

Kremer, Michael, Jessica Leino, Edward Miguel, and Alix Peterson Zwane (2011). “Spring cleaning: Rural water impacts, valuation, and property rights institutions”. *The Quarterly Journal of Economics* 126 (1): 145–205.

Kremer, Michael and Edward Miguel (2007). “The Illusion of Sustainability”. *The Quarterly Journal of Economics* 122 (3): 1007–1065.

Lee, Kenneth, Edward Miguel, and Catherine Wolfram (2020). “Experimental Evidence on the Economics of Rural Electrification”. *Journal of Political Economy* 128 (4): 1523–1565.

Lunduka, Rodney, Jacob Ricker-Gilbert, and Monica Fisher (2013). “What are the farm-level impacts of Malawi’s farm input subsidy program? A critical review”. *Agricultural Economics* 44 (6): 563–579.

Minten, Bart, Bethlehem Koru, and David Stifel (2013). “The last mile(s) in modern input distribution: Pricing, profitability, and adoption”. *Agricultural Economics* 44 (6): 629–646.

Nagavarapu, Sriniketh and Sheetal Sekhri (2016). “Informal monitoring and enforcement mechanisms in public service delivery: Evidence from the public distribution system in India”. *Journal of Development Economics* 121: 63–78.

Nichols, D., E. Smolensky, and T. N. Tideman (1971). “Discrimination by Waiting Time in Merit Goods”. *The American Economic Review* 61 (3): 312–323.

Porteous, Obie (2019). “High Trade Costs and Their Consequences: An Estimated Dynamic Model of African Agricultural Storage and Trade”. *American Economic Journal: Applied Economics* 11 (4): 327–66.

Ricker-Gilbert, Jacob and Thomas S Jayne (2017). “Estimating the enduring effects of fertiliser subsidies on commercial fertiliser demand and maize production: panel data evidence from Malawi”. *Journal of Agricultural Economics* 68 (1): 70–97.

Shamdasani, Yogita (2021). "Rural road infrastructure & agricultural production: Evidence from India". *Journal of Development Economics* 152: 102686.

Sylvia, Sean, Xiaochen Ma, Yaojiang Shi, and Scott Rozelle (2022). "Ordeal mechanisms, information, and the cost-effectiveness of strategies to provide subsidized eyeglasses". *Journal of Health Economics* 82: 102594.

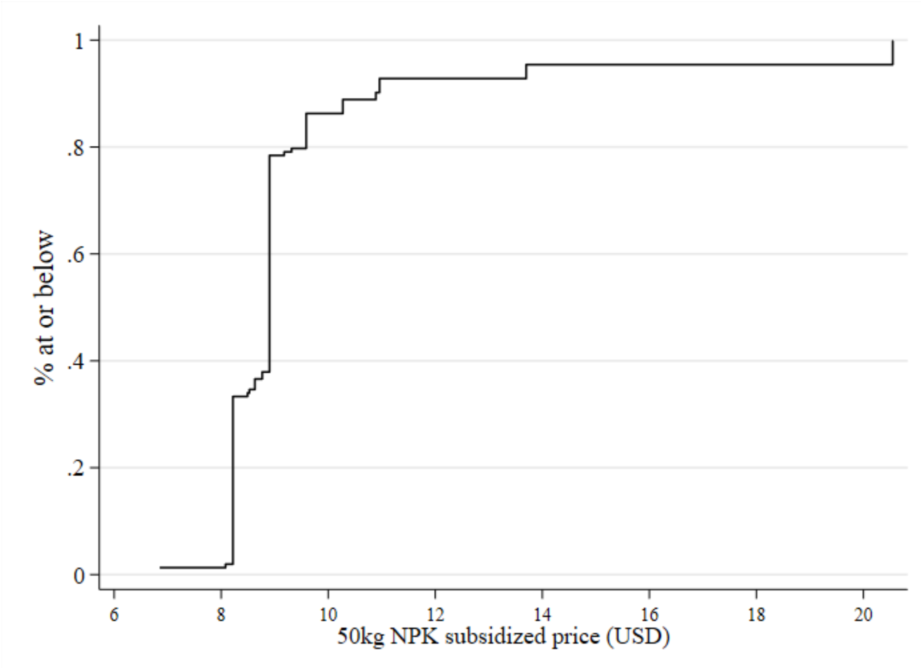
World Bank (2008). "World Development Report 2008: Agriculture for Development". The World Bank.

— (2017). "Enabling the Business of Agriculture 2017". The World Bank.

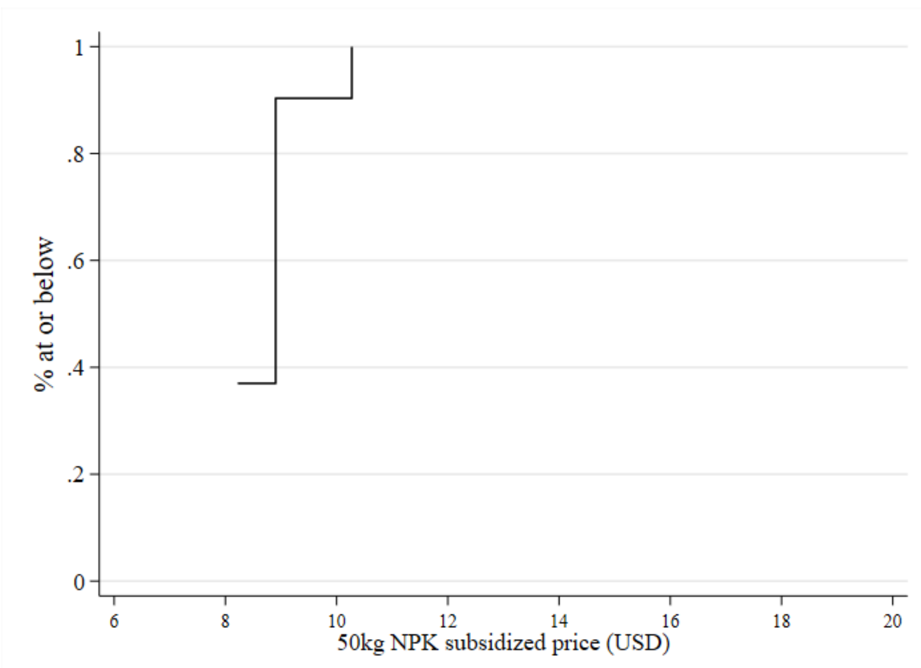
Appendix A: Additional Results

Figure A1: CDF of Subsidized FISP Prices at Agdealers

(a) CDF across universe of retailers



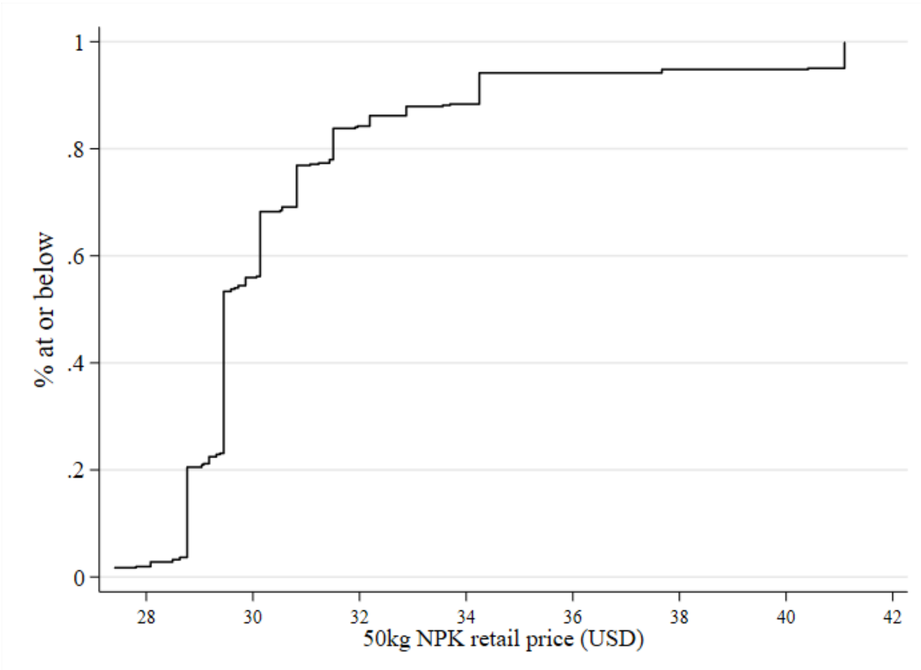
(b) CDF across Retailers Identified as Lowest Cost option



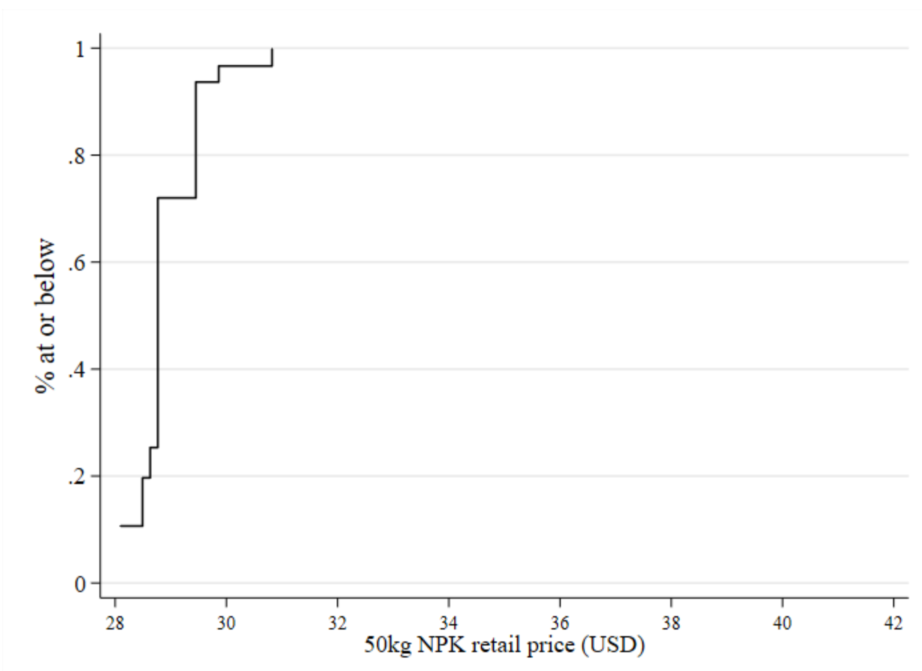
Note: Figure presents the CDF of the travel cost-adjusted price for subsidzed FISP fertilizer. Panel A shows the distribution across all the retailers in the sample (N=153), while Panel B shows the distribution across only those retailers chosen by our algorithm as the lowest cost option (N=12).

Figure A2: CDF of Retail Prices at Agdealers

(a) CDF across universe of retailers



(b) CDF across retailers identified as lowest cost option



Note: Figure presents the CDF of the travel cost-adjusted price for retail fertilizer. Panel A shows the distribution across all the retailers in the sample (N=463), while Panel B shows the distribution across only those retailers chosen by our algorithm as the lowest cost option (N=20).

Table A1: Agroinput Dealer Summary Statistics

	(1) Mean	(2) SD
Number of years in business	6.15	5.64
=1 if selected for FISP in past 2 years	0.47	
At the time of survey:		
=1 if sells NPK	0.97	
=1 if sells Urea	0.95	
=1 if sells CAN	0.43	
=1 if sells DAP	0.09	
In last year (2018):		
Number of 50kg bags of NPK sold	858.41	1,561.76
Number of 50kg bags of Urea sold	692.81	1,256.80
Number of 50kg bags of CAN sold	101.07	248.17
Number of 50kg bags of DAP sold	0.00	0.00
Total revenue from selling fertilizer last year (USD)	32,728.74	71,519.06
Observations	466	

Note: Sample restricted to shops that sell any NPK or Urea. Variables winsorized at 95th percentile.

Table A2: Correlation between Remoteness and Farmer Characteristics

	Mean (SD)	Remoteness
	(1)	(2)
Panel A: Farmer Characteristics		
=1 if female headed household	0.40	-0.04
	(0.49)	(0.03)
Household head age (in 10 years)	4.38	-0.10
	(1.52)	(0.26)
Related to Chief	0.48	0.07**
	(0.50)	(0.03)
Household size	4.89	0.36
	(2.03)	(0.30)
Respondent years of education	4.72	-0.75***
	(3.37)	(0.29)
FISP coupon received last year	0.16	-0.03**
	(0.36)	(0.01)
Land size in acres	1.70	0.11
	(1.40)	(0.11)
Panel B: Production Capacity (in kg/acre)[1]		
FAO-GAEZ production capacity for low input level	2.37	0.45***
	(0.93)	(0.15)
FAO-GAEZ production capacity for high input level	8.48	1.42***
	(2.71)	(0.54)
Observations	2367	2367

Data is restricted to households that grew maize in 2017-18. Standard Errors clustered at village level are in parentheses. Estimates include regression of dependent variables in column 1 on remoteness measures. FAO variables have been rescaled by dividing by 1000.

[1]Regressions for production capacity are at village level.

Table A3: Comparing FISP Beneficiaries and Non-Beneficiaries

	Mean/(SD)	Bivariate	Multivariate
	(1)	(2)	(3)
Household head age (in 10 years)	4.37	0.011***	0.010***
	(1.51)	(0.003)	(0.004)
Related to chief	0.48	0.019*	0.018*
	(0.50)	(0.011)	(0.010)
=1 if female headed household	0.40	-0.005	-0.002
	(0.49)	(0.010)	(0.010)
Household size	4.89	0.004*	0.003
	(2.04)	(0.002)	(0.002)
Education level of respondent	4.67	-0.002	0.001
	(3.36)	(0.001)	(0.002)
FISP coupon received last year	0.14	0.047***	0.044***
	(0.35)	(0.016)	(0.016)
Land size in acres	1.71	0.010***	0.007**
	(1.40)	(0.004)	(0.004)
p-value for F-test on joint significance			0.00
Observations	6,827	6,827	6,827
Households		2,564	2,564

Notes: Data is from three agricultural season, 2017-18, 2018-19 and 2019-20. Data is restricted to households that grew maize. The dependent variable takes value as 1 if the household received FISP. Dependent variable mean is 13%. Column 1 shows the control mean and standard deviation, column 2 shows coefficients of a bivariate regression of FISP status on household characteristics and includes village fixed effects and column 3 shows coeffificents of a multivariate regression of FISP on household characteristics. Standard errors are clustered at the village level and are in parentheses. ***, **, and * represent significance at 1%, 5%, and 10%, respectively.

Table A4: Remoteness and Sharing / Selling of FISP Benefits

	Quantity received by non-coupon holders (kg)		Quantity shared or sold by coupon holders (kg)	
	(1)	(2)	(3)	(4)
Remoteness (β)	-9.13***	-8.12***	-7.65***	-7.82***
	(0.78)	(0.91)	(1.08)	(1.25)
Household controls	N	Y	N	Y
Mean	27.67		27.17	
Observations	6153	6153	923	923

Notes: Regressions are restricted to non-coupon holders in columns 1 and 2, and FISP coupon holders in columns 3 and 4. All coefficients are from separate regressions of the respective dependent variable on remoteness measure. The dependent variable in Columns 1-2 is the quantity of FISP fertilizer bought by non-beneficiaries. On average, 46 kgs were sold by farmers for a total of $0.05. Data is from one agricultural seasons, 2017-18. Remoteness is a standardized measure at the village level. Standard errors clustered by village are in parentheses. ***, **, and * represent significance at 1%, 5%, and 10%, respectively.

Table A5: Randomization Inference p-values

	=1 if used fertilizer		KGs of fertilizer used	
	(1)	(2)	(3)	(4)
FISP	0.13***	0.09***	14.78***	9.56***
	(0.00)	(0.00)	(0.00)	(0.00)
Remoteness (β)	-0.10***		-7.49***	
	(0.00)		(0.00)	
FISP x Remoteness (γ)	0.11***	0.07***	12.14***	9.98***
	(0.00)	(0.00)	(0.00)	(0.00)
Dependent variable mean	0.83	0.83	53.71	53.71
Observations	6827	6827	6827	6827

Notes: All regressions include household controls. Household controls include variables in Table 3. Columns (2) and (4) also include household fixed effects. Data is restricted to households that grew maize. Remoteness is a standardized measure at the village level. Randomization inference p-values are in parentheses. ***, **, and * represent significance at 1%, 5%, and 10%, respectively.

Table A6: FISP, Remoteness and Harvest Quantity

	(1)	(2)
FISP	23.78*	22.13
	(13.26)	(16.53)
Remoteness (β)	-46.13***	
	(7.84)	
FISP $\times$ Remoteness (γ)	31.78**	17.05
	(13.42)	(16.84)
p-value: $\beta + \gamma$	0.28	
Dependent variable mean	411.13	411.13
Observations	6328	6328
Households	2520	2520
Household FE	N	Y

Notes: Column (1) includes household controls. Household controls include variables in Table 3. Columns (2) also includes village fixed effects. Data is restricted to households that grew maize. Data is from three agricultural seasons, 2017-18, 2018-19 and 2019-20. The dependent variable winsorized at 95th percentile. Remoteness is a standardized measure at the village level. Standard errors clustered by village are in parentheses. ***, **, and * represent significance at 1%, 5%, and 10%, respectively.

Table A7: FISP and the Input Adoption-Remoteness Gradient, for 2017-18 and 2018-19 only

	=1 if used fertilizer		KGs of fertilizer used	
	(1)	(2)	(3)	(4)
FISP	0.16***	0.06***	16.16***	6.77***
	(0.01)	(0.01)	(2.10)	(2.14)
Remoteness (β)	-0.12***		-9.94***	
	(0.01)		(1.43)	
FISP $\times$ Remoteness (γ)	0.13***	0.03**	13.16***	12.29***
	(0.01)	(0.02)	(2.27)	(2.33)
p-value: $\beta + \gamma$	0.55		0.18	
Dependent variable mean	0.80	0.80	43.01	43.01
Observations	4455	4455	4455	4455
Households	2530	2530	2530	2530
Village FE	N	Y	N	Y
Household FE	N	Y	N	Y

NNotes: Columns 1 and 3 include all the variables in Table 3, while Columns 2 and 4 include household fixed effects. Data is restricted to households that grew maize. Table includes data from two agricultural seasons, 2017-18 and 2018-19. Remoteness is a standardized measure at the village level. All regressions include year fixed effects. Standard errors clustered by village are in parentheses. ***, **, and * represent significance at 1%, 5%, and 10%, respectively.

Appendix B: FAO-GAEZ Productivity Measures

In our main regressions, we include a measure of land productivity based on GIS data from the FAO-GAEZ website. The FAO-GAEZ measures primarily use nine datasets, including observed climate data, future climate predictions, soil resources data, which includes information on soil characteristics, elevation and terrain-slope, land cover, observed crop calendars, grid-level population data, grid-level livestock data and agricultural area and production data. The process to generate the productivity measure involves several modules that model the above-mentioned raw datasets to calculate a measure of land productivity, that is, the ratio of actual and potential yield for two varying levels of inputs - high and low. The two input levels do not depend on observed input choices; instead, varying levels of agricultural practices and resources are considered. Low-input farming assumes subsistence-based agriculture, characterized by traditional practices, labor-intensive techniques, and no chemical inputs or modern technology. High-input farming assumes advanced, market-oriented agriculture, utilizing high-yielding crop varieties and chemical inputs like fertilizers and pesticides.

Table B1: Correlation between FAO-GAEZ productivity measures and Harvest quantity

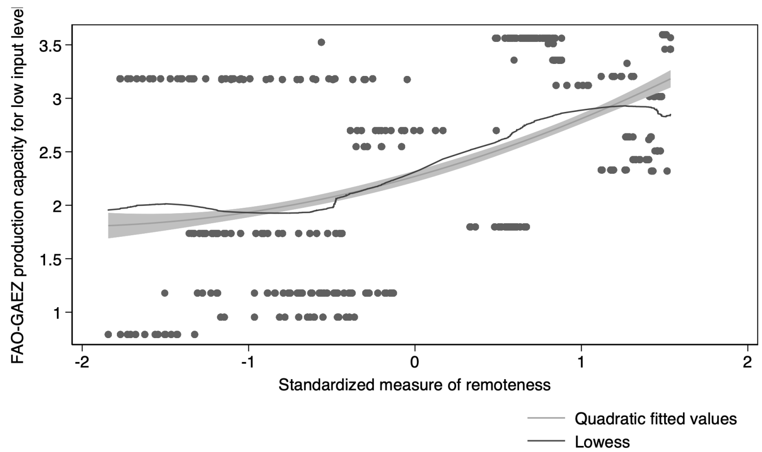
	Maize Harvest		
	(1)	(2)	(3)
FAO-GAEZ production capacity for low input level	132.14***		71.29
	(6.83)		(61.29)
FAO-GAEZ production capacity for high input level		37.82***	17.56
		(1.91)	(17.07)
Dependent variable mean			
Observations	2339	2339	2339

Notes: FAO variables have been rescaled by dividing by 1000. Standard errors clustered by village are in parentheses. ***, **, and * represent significance at 1%, 5%, and 10%, respectively.

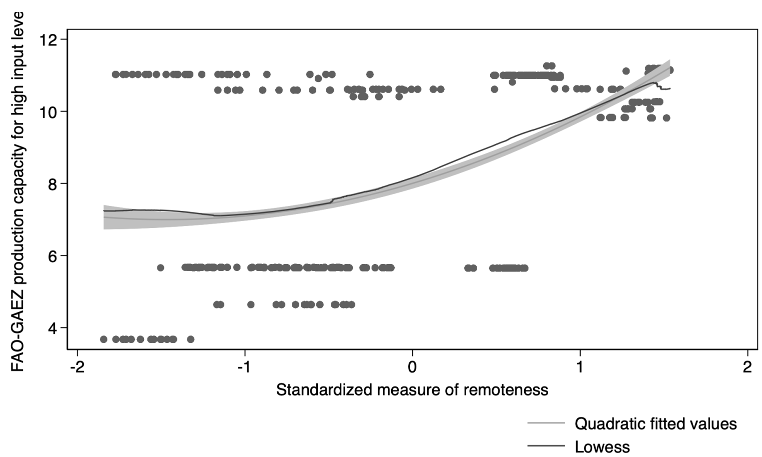
We present two results in this section. First, Table B1 shows the correlation between these measures and our measured harvest productivity. The two measures strongly correlate with maize harvest in the sample. Figure B1 plots the distribution of FAO-GAEZ production capacity against the standardized measure of remoteness. All three panels show that production capacity is higher for varied input levels in remote areas. Panel B shows that at high input levels, the production capacity of remote villages is much higher than that of less remote areas. Finally, in Panel C, we plot the difference in production capacities across low and high input levels against standardized remoteness measures. The figure suggests that the production-capacity gap is much more significant in remote villages. This figure also indicates that there is a non-linear relationship between remoteness and FAO-GAEZ productivity measures. As a result, we include quadratic terms of the land productivity measures in all our regressions.

Figure B1: FAO-GAEZ Productivity and Remoteness

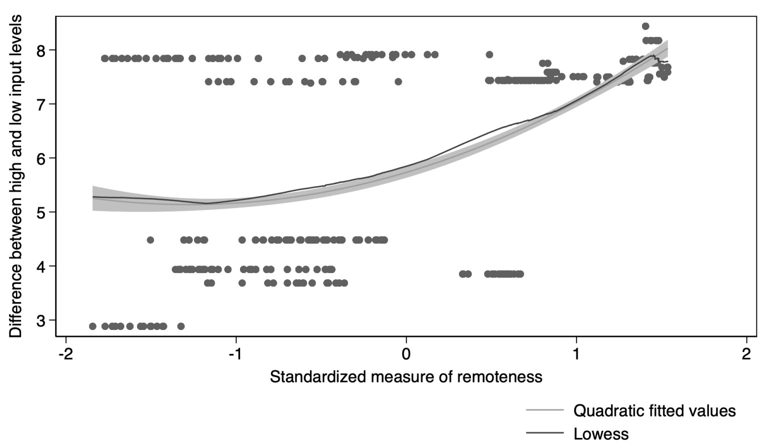
(a) FAO-GAEZ Production Capacity Measure for Low Input Level



(b) FAO-GAEZ Production Capacity Measure for High Input Level



(c) Difference between High and Low Input FAO-GAEZ Production Capacity Measures



Appendix C: Robustness

Robustness to excluding households that received FISP in 2016-17

Table C1: Determinants of FISP Beneficiary Status, excluding households who received FISP in 2016-17

	Mean (SD)	Coefficients on variables: FISP	FISP × Remoteness
	(1)	(2)	(3)
Household head age (in 10 years)	4.33	0.20***	0.01
	(1.52)	(0.06)	(0.06)
Related to chief	0.48	0.04**	0.00
	(0.50)	(0.02)	(0.02)
=1 if female headed household	0.40	-0.02	-0.03
	(0.49)	(0.02)	(0.02)
Number of household members	4.88	0.20**	0.07
	(2.04)	(0.08)	(0.08)
Education level of respondent	4.68	-0.11	0.12
	(3.38)	(0.13)	(0.14)
Land size (acres)	1.70	0.13**	0.01
	(1.39)	(0.06)	(0.05)
Observations	5,856		
Households	2,515		

Notes: Data is from three agricultural seasons, 2017-18, 2018-19 and 2019-20. Mean of dependent variable is 13%. Data is restricted to households that grew maize. Each row represents a separate regression of the dependent variable on on FISP status and FISP × Remoteness. Column 1 shows the mean and standard deviation of household characteristics, while Columns 2-3 show regression coefficients. All regressions include village fixed effects. Standard errors are clustered at the village level and are in parentheses. ***, **, and * represent significance at 1%, 5%, and 10%, respectively.

Table C2: FISP and the Input Adoption-Remoteness Gradient, excluding households who received FISP in 2016-17

	=1 if used fertilizer		KGs of fertilizer used	
	(1)	(2)	(3)	(4)
FISP	0.13***	0.08***	14.06***	11.68***
	(0.01)	(0.02)	(1.99)	(2.26)
Remoteness (β)	-0.11***		-7.11***	
	(0.01)		(1.24)	
FISP $\times$ Remoteness (γ)	0.11***	0.07***	11.07***	9.28***
	(0.01)	(0.02)	(2.05)	(2.44)
p-value: $\beta + \gamma$	0.78		0.06	
Dependent variable mean	0.82	0.82	52.26	52.26
Observations	5856	5856	5856	5856
Households	2515	2515	2515	2515
Household FE	N	Y	N	Y

Notes: Columns 1 and 3 include all the variables in Table 3, while Columns 2 and 4 include household fixed effects. Data is restricted to households that grew maize. Table includes data from three agricultural seasons, 2017-18, 2018-19 and 2019-20. Remoteness is a standardized measure at the village level. All regressions include year fixed effects. Standard errors clustered by village are in parentheses. ***, **, and * represent significance at 1%, 5%, and 10%, respectively.

Table C3: Placebo Check, excluding households who received FISP in 2016-17

	=1 if used fertilizer		KGs of fertilizer used	
	(1)	(2)	(3)	(4)
Received FISP next year	-0.05*	-0.05*	2.61	2.41
	(0.03)	(0.03)	(3.39)	(3.34)
Received FISP next year × Remoteness		-0.02		-3.83
		(0.02)		(2.38)
Dependent variable mean	0.79	0.79	43.13	42.54
Observations	3867	3867	3817	3867
Households	2410	2410	2391	2410

Notes: This table regresses FISP allocation status in the next (t+1) year on outcomes in the current (t) year. All regressions include household controls. Household controls include variables in Table 3. All regressions include year fixed effects. Data is restricted to households that grew maize. Standard errors clustered by village are in parentheses. ***, **, and * represent significance at 1%, 5%, and 10%, respectively.

Robustness to excluding households that are related to the chief

Table C4: Determinants of FISP Beneficiary Status, excluding household related to chief

		Coefficients on variables:	
	Mean (SD)	FISP	FISP × Remoteness
	(1)	(2)	(3)
Household head age (in 10 years)	4.36	0.16*	-0.07
	(1.50)	(0.08)	(0.10)
=1 if female headed household	0.38	-0.04*	-0.02
	(0.485)	(0.02)	(0.03)
Number of household members	4.88	0.21*	0.31**
	(2.034)	(0.12)	(0.13)
Education level of respondent	4.93	0.21	0.24
	(3.444)	(0.18)	(0.18)
FISP coupon received last year	0.14	0.01	-0.06**
	(0.343)	(0.02)	(0.03)
Land size (acres)	1.68	0.12	0.01
	(1.41)	(0.08)	(0.07)
Observations	3,533		
Households	1,337		

Notes: Data is from three agricultural seasons, 2017-18, 2018-19 and 2019-20. Mean of dependent variable is 13%. Data is restricted to households that grew maize. Each row represents a separate regression of the dependent variable on on FISP status and FISP × Remoteness. Column 1 shows the mean and standard deviation of household characteristics, while Columns 2-3 show regression coefficients. All regressions include village fixed effects. Standard errors are clustered at the village level and are in parentheses. ***, **, and * represent significance at 1%, 5%, and 10%, respectively.

Table C5: FISP and the Input Adoption-Remoteness Gradient, excluding household related to chief

	=1 if used fertilizer		KGs of fertilizer used	
	(1)	(2)	(3)	(4)
FISP	0.14***	0.10***	16.63***	10.75***
	(0.01)	(0.02)	(2.83)	(3.08)
Remoteness (β)	-0.11***		-6.80***	
	(0.02)		(1.52)	
FISP $\times$ Remoteness (γ)	0.11***	0.08***	14.85***	10.88***
	(0.02)	(0.02)	(3.53)	(2.99)
p-value: $\beta + \gamma$	0.88		0.03	
Dependent variable mean	0.84	0.84	54.82	54.82
Observations	3533	3533	3533	3533
Households	1337	1337	1337	1337
Household FE	N	Y	N	Y

Notes: Columns 1 and 3 include all the variables in Table 3, while Columns 2 and 4 include household fixed effects. Data is restricted to households that grew maize. Table includes data from three agricultural seasons, 2017-18, 2018-19 and 2019-20. Remoteness is a standardized measure at the village level. All regressions include year fixed effects. Standard errors clustered by village are in parentheses. ***, **, and * represent significance at 1%, 5%, and 10%, respectively.

Table C6: Placebo Check, excluding household related to chief

	=1 if used fertilizer		KGs of fertilizer used	
	(1)	(2)	(3)	(4)
Received FISP next year	-0.01	-0.01	-0.36	-1.00
	(0.02)	(0.02)	(3.79)	(3.83)
Received FISP next year × Remoteness		-0.01		-2.25
		(0.03)		(2.96)
Dependent variable mean	0.81	0.81	45.18	44.35
Observations	2301	2301	2259	2301
Households	1314	1314	1300	1314

Notes: This table regresses FISP allocation status in the next (t+1) year on outcomes in the current (t) year. All regressions include household controls. Household controls include variables in Table 3. All regressions include year fixed effects. Data is restricted to households that grew maize. Standard errors clustered by village are in parentheses. ***, **, and * represent significance at 1%, 5%, and 10%, respectively.

Appendix D: Technical Appendix

In section 3.2, we estimate trade costs using a multinomial logit motivated by a spatial model to recover the ad-valorem trade costs implied by farmer decision-making. An outline of the model, estimation and results is below.

Precisely, suppose that a farmer f from village i chooses from a set of agro-retailers $j \in J$ that are located in a set of villages $v \in V$. The retail price charged at an agroretailer j in v is r_{vj}. Buying from each agroretailer involves receiving a productivity shock with a mean that is specific to the agroretailer, and distributed Frechet. Following **aggarwal2022market**, the farmer, on each trip in our dataset, chooses amongst available agro-dealer locations, incuring an ad-valorem trade cost τ_{iv} in traveling from their village i to agro-dealer-location v. Using this modeling structure, it is straightforward to derive that the probability a farmer f from village i chooses an agro-dealer from village v on trip t is the following.

$$\Pr\left(v \ chosen\right) = \frac{\exp\left(\delta_v - \varepsilon \log(\tau_{iv})\right)}{\sum_{v'} \sum_{j'} \exp\left(\delta_{v'} - \varepsilon \log(\tau_{iv'})\right)} \tag{6}$$

Here, δ_v captures the quality adjusted retail prices of retailers in location v, which farmers weight against the trade costs in traveling from their village i to the agro-dealer location v. Given the structure of our data, we adopt a simple specification for trade costs, where the elasticity adjusted ad-valorem cost is a linear function of distance on main roads, $Main_{iv}$, and the distance on rural roads, $Rural_{iv}$. Thus, the specification we take to the data can be written as:

$$\Pr\left(j \ in \ v \ chosen\right) = \frac{\exp\left(\delta_v + \alpha Main_{iv} + \beta Rural_{iv}\right)}{\sum_{v'} \sum_{j'} \exp\left(\delta_{v'} + \alpha Main_{iv} + \beta Rural_{iv}\right)\right)} \tag{7}$$

Equation (7) can be estimated by McFadden's alternative-specific conditional logit. The results from doing so are presented in Appendix Table D1. Clearly, distance on main roads has a smaller effect on agro-dealer-location-choice than distance on rural roads, which implies higher trade costs for rural roads.

To quantify these estimates, we appeal to **aggarwal2022market** and assume $\varepsilon_a \approx 8$. In doing this, we can calculate the ad-valorem equivalent trade cost for any length of trip with any main-rural composition of travel. For example, the average trip in our data includes 2.55 km on main roads and 6.62 on rural roads. Using these distances, and $\varepsilon_a = 8$, we calculate that the ad-valorem equivalent trade cost for the average trip is 24%. At the mean retail price of $30, this trade costs is approximately $7.48 - a substantial sum for farmers in this region.

Table D1: Estimates from Multinomial Logit Model

	(1)
KM on Main Roads	-0.163***
	(0.0081)
KM on Rural Roads	-0.201***
	(0.0086)
Observations	370,760

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1