# Shortening the Path to Productive Investment: Evidence from Input Fairs and Cash Transfers in Malawi*

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

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**Abstract**

While cash transfers consistently show large effects on immediate outcomes like consumption, limited access to markets may mute their impact on productive investment. In an experiment in Malawi, we cross-cut cash transfers with an "input fair", designed to reduce transport costs to access agricultural inputs. Cash alone increases investment by 25%, while the input fair doubles this effect. Input fairs alone were ineffective. A mistimed public subsidy program undoes the incremental effect of the joint intervention (though not of cash alone) by causing input fair purchases to crowd out subsidized inputs, such that the program had no effect on quantities.

*JEL Codes:* O13, Q12
*Keywords:* cash transfers, market access, productive investment, FISP, input fairs

*Aggarwal: Indian School of Business and J-PAL (shilpa_aggarwal@isb.edu); Jeong: World Bank (dahyeonjeong@worldbank.org); Kumar: World Bank (nkumar18@worldbank.org); Park: KDI School of Public Policy and Management (park@kdis.ac.kr); Robinson: UCSC, BREAD, CEGA, J-PAL and NBER (jmrtwo@ucsc.edu); Spearot: UCSC (aspearot@ucsc.edu). We thank USAID for funding and GiveDirectly for their collaboration. The research protocol for this study was approved by the IRBs of UCSC and the Malawi NCRSH. This trial is registered as AEARCTR-0004869 (including a pre-analysis plan). There are many individuals that contributed to this project, but space prevents us from thanking them all. We thank Patrick Baxter, Emanuele Clemente, Calvin Mhango, Monica Shandal, Patrick Simbewe, and Asman Suleiman at IPA Malawi. We thank Kris Cox and Pace Phillips at IPA and Temina Madon of CEGA for help in early stages of the project; Geneviève Barrons, Michael Cooke, and Shaunak Ganguly at GiveDirectly; and Joe Amick, Joe-Hoover Gbadyu, Daniel Handel, Scott Jackson, and Stephen Scott at USAID. We thank James Murotho and Agora for organizing the input fairs. We are extremely grateful to all the enumerators who collected this data. The findings, interpretations, and conclusions expressed in this paper are entirely those of the authors, and do not necessarily represent the views of USAID, the World Bank or its affiliated organization, its Executive Directors or the governments they represent.

# 1 Introduction

Cash transfers have become an increasingly popular policy tool, and a vast literature has convincingly demonstrated the beneficial effects of one-time, unconditional cash transfers on an array of outcomes. These studies generally show large effects on immediate consumption, but more ambiguous effects on productive investment or income generation.[1] Understanding why investment responses may be muted is particularly important for increasing the longer-term efficacy of cash transfer programs.

An important reason why productive investment may be limited in some contexts is the existence of other constraints, such as investment indivisibilities (Balboni et al. 2021; Kaboski et al. 2022), limits in entrepreneurial ability (Banerjee et al. 2021; Beaman et al. 2020; Maitra et al. 2017), or missing markets for risk mitigation (Karlan et al. 2014). In this paper, we focus specifically on poor market access for farmers, which prior work has shown to be an important barrier to investment in productive inputs (Aggarwal et al. 2022b; Cedrez et al. 2020; Minten et al. 2013; Kumar et al. 2022).

We conducted an experiment in 300 villages in rural Malawi. All households in half the villages received one-time cash transfers from the NGO GiveDirectly, delivered shortly before planting. The transfers averaged $500, a large sum in this context. In a companion paper (Aggarwal et al. 2022a), we examine the impact of cash on a host of outcomes; in this paper, we focus specifically on the effect of cash on productive farm investment. To examine the impact of reducing travel costs, we cross-cut an individual-level market access treatment, organized jointly with Agora, a large local retailer with a network of input retail locations in the country, located mostly in market centers (they have about 20 locations in the study area, spread over two districts in Southern Malawi). We coordinated with Agora to offer inputs for sale on predesignated days at locations near farmers' homes (mostly schools), and marketed these as "input fairs". We subsidized the cost for farmers to attend these events.[2]

[1]For example, see Aggarwal et al. (2022a), Blattman et al. (2013), Egger et al. (2022), Haushofer and Shapiro (2016), Haushofer and Shapiro (2018), McIntosh and Zeitlin (2021) and McIntosh and Zeitlin (2022).

[2]The market access event is very unlikely to affect outcomes beyond input investment, and thus is included

In addition to a control group, our final sample is comprised of three main treatment groups: a group that received market access only, a group that received cash only, and a group that received both together. The pure control group received neither intervention.

Among those offered only market access, take-up was modest: only 3% of farmers purchased inputs at the event, and average spending was only about $0.50. However, take-up was significantly higher for those also receiving cash: the percentage purchasing inputs increased by 9 percentage points (a 450% increase over the input fair-only treatment), and the value of purchase increased by $3.45. Conditional on take-up, the value of the purchase was about $33, slightly more than the value of a 50 kg bag of fertilizer which costs on average about $28. Most of the spending at the input fair event was focused on chemical fertilizer - about 95% of the value of purchases was spent on fertilizer. This is reflective of the overall patterns of input usage in this context: in a baseline survey that we did in these villages in 2019, farmers spent an average of $17.2 on agricultural inputs, and 93% of this amount ($16) was spent on fertilizer. Given this strong skew towards fertilizer, we mainly focus on fertilizer in this paper (even though the event was designed to provide access to all inputs).

Because this is a context with high fertilizer adoption, take-up at the event may be inframarginal (i.e. it may be the case that those who attended the event were going to purchase anyway); thus, our most important result is the effect on overall fertilizer usage (from all the sources) on the farm. We find that cash transfers alone increased spending on chemical fertilizer by about $4.91 (equivalent to 25% of fertilizer usage in the control group), implying that cash alone can increase agricultural investment.[3] Providing the input fair in addition to cash increased investment by another $4.05 on average, thus almost doubling the effect of cash alone. As would be expected given the take-up results, the input fair alone had no effect.

Our results, therefore, show that in this context, simultaneously addressing market access

as a control but is not discussed extensively in our companion paper (Aggarwal et al. 2022a).

[3]This result is in contrast to Karlan et al. (2014), which finds no statistically significant effect of cash alone on investment in Ghana.

substantially increases the effect of cash on agricultural investment. Our results are closely related to the literature about large unconditional cash transfers, which includes many studies by now. While these studies show consistently large effects on immediate outcomes, they show mixed results on productive investment. One study that shows large effects on physical capital is Blattman et al. (2013), which is set in the context of a business grant program, where beneficiary selection was tied to business proposals, which may have acted as a nudge to the respondents to invest their grants productively. We show that market access may be a limiting factor that may mute the effects of cash on investments.

Our paper is also closely related to research which documents and quantifies market access in rural areas. Transport costs impede farmers from adopting modern technologies such as fertilizer by increasing the prices of inputs and reducing those of outputs, directly impacting the profitability of these technologies (Aggarwal et al. 2022b; Gebresilasse 2023; Minten et al. 2013). Moreover, as shown by Aggarwal et al. (2022b), when input retailers are located at a distance from farmers, the adoption decision is complicated further by other non-pecuniary factors, such as uncertainty about availability and prices. In such a context of poor access, our intervention works on two levels - one, the input fair itself resolves the uncertainty about availability, and the transport subsidy to attend the fair reduces the cost to access these inputs.

Finally, our input fair intervention is very similar to several other recent or ongoing studies, including Dillon (ongoing) in Mali and Udry (2019) in Northern Ghana. Our research also has some similarities with recent work on similar concepts, such as centralized job fairs (Abebe et al. 2020; Abebe et al. 2022; Bassi and Nansamba 2021; Beam 2016), although such kinds of job fairs may resolve multiple constraints at once, such as provide credible information on the returns to various jobs and improve match quality between employers and employees. Our intervention, on the other hand, is more narrowly focused to largely be about transport costs.

Our results show that timing complementary interventions parallel to cash transfers can change how those cash transfers can be spent. The welfare impact of such a program, therefore, depends on the item that is being offered: while we focused on easing access to agricultural inputs, the program could easily have been organized around another expenditure category, such as immediate consumption. Welfare also depends on the prices that are being offered. Our study was designed to reduce the (travel cost-adjusted) price that people would pay. However, our final result, context-specific to Malawi, highlights how nudging people to purchase productive inputs at a specific event can have unintended consequences. In particular, the effect of our program was complicated by the timing of Malawi's Farm Input Subsidy Program (FISP), which was unexpectedly delayed in the year of study. During the period of study, FISP was implemented via the disbursement of paper coupons to selected farmers; farmers then can redeem these coupons at local input retailers. According to data we collected in early 2019, coupon distribution was implemented in October (before planting in November) in the two years preceding the intervention; for this reason, we scheduled the input fairs in October 2019. However, in that year, beneficiaries had not even been identified by October. As a result, the input fairs took place *before* FISP coupons had been handed out. Thus farmers came to our events not knowing if they would receive FISP or not, which likely dampened demand. Additionally, people who purchased inputs did not know their FISP status, and they may have been less likely to receive FISP coupons,[4] or as non-beneficiaries of FISP, may have received less from others, or as beneficiaries, may have shared more with others.[5] Indeed, we find that while those in the cash and input fair cross-cut treatment spent twice as much as the cash alone farmers on fertilizer, the quantity of fertilizer (in kgs) that they bought is no higher. Thus, we find that the competing influence of FISP mitigated eventual impacts, and show how such an intervention can (partially) go awry. Despite this

[4]This may happen if local leaders could manipulate FISP allocation to favor those who did not get the subsidy to visit the input fairs. This is unlikely on paper as FISP is allocated randomly within the village, but is not implausible on the ground as village chiefs have considerable power and there is past research that documents the chief's intervention in this matter (Basurto et al. 2020). We test for this and find no evidence of such manipulation in the data, suggesting that our intervention did not undo the FISP randomization.

[5]There is an extensive literature documenting the widespread prevalence of sharing of FISP coupons. See, for example, Basurto et al. (2020) and Kumar et al. (2022), among others.

contextual wrinkle, however, our results on take-up and expenditure at the input fairs show that enabling market access within the design of cash transfer programs (and indeed, other programs that ease liquidity constraints, such as credit) may be an important path through which productive investments could be unlocked.

The rest of this paper is organized as follows. Section 2 describes the context, experiment and data; Section 3 describes our results; and Section 4 concludes.

# 2 Context, Experimental Design, and Data

## 2.1 Agriculture in Malawi

Malawi has a unimodal rainfall pattern with a single agricultural season. Planting begins around November, and the harvest season begins around April or May. As such, inputs are typically purchased shortly before planting.

A very important institutional detail is the existence of Malawi's Farm Input Subsidy Program (FISP). The scale and targeting of FISP has changed dramatically over time, and earlier iterations of the program have been studied extensively in prior work.[6] Under FISP, respondents receive coupons for inputs, which are redeemable at a subsidized price at local input retailers. During the time period of study, there are three key facts that are relevant for this paper. First, as in prior years, the subsidy was substantial: each beneficiary was provided coupons for approximately a 75% discount on inputs that are worth about $50 at market prices. Consequently, redemption is close to universal. Second, for various reasons including fairness and concerns about nepotism, the subsidy was provided to randomly selected beneficiaries in the year of our evaluation.

A final important detail is that, in our study year, FISP coupons were given out *after* our evaluation. We scheduled our intervention to occur in October 2019, close to planting in

[6] A partial list of papers includes Chirwa and Dorward (2013), Dorward et al. (2008) and Basurto et al. (2020), among others.

November, which we assumed would be after coupons were given out. However, by October, beneficiaries had not even been identified in our area, and coupons were only given out later. Consequently, treatment farmers did not know if they were to receive FISP or not, and then later may have redeemed FISP. We return to this issue in much more detail below.

## 2.2 Setting and Cash Transfer Experiment

The NGO GiveDirectly (henceforth, GD) implemented the cash transfers in two districts of Malawi in 2019-2020 - Chiradzulu and Machinga. Villages within each district were eligible if their number of households, as measured in the 2018 population census, was less than 100. In total, 300 villages were included in the study, and 150 of these villages received cash transfers. All households in treatment villages received cash. In each village (treatment a well as control), we enrolled 10 households in the study, i.e., we did baseline and endline surveys with them.

The average cash transfer amount was $500, a substantial amount in this context (where average household monthly expenditures was roughly $34 at baseline). The amount of the transfer was randomized between $250, $500 and $750. To ensure liquidity, transfers were paid out in increments of $250, paid out once per month; therefore, households that received $500 received the money over 2 months and those receiving $750 received it over 3 months. Cash transfers were disbursed via mobile money; households who did not have prior access to mobile money were provided with access to a mobile-money-enabled SIM during enrollment. We coordinated with GD to ensure that all treatment households received at least their first transfer by the time of planting in 2019 (November); however, households receiving the larger amounts were still receiving cash post-planting (see Figure A1 for more detail).

## 2.3 Input Fair Treatment

In order to encourage cash transfer households to invest into productivity-improving inputs, we organized input fairs shortly before planting, in October 2019.[7]

Of the 300 villages in the main cash transfer study (Aggarwal et al. 2022a), we selected 100 to be in the input fair treatment, split equally between cash treatment and cash control - thus, we had 100 pure control villages, 100 cash-only, 50 input fair-only, and 50 both cash and input fair (input fair was implemented in less than half the sample due to partner concerns about powering the basic cash versus control comparison). Treatment was stratified by "traditional authority," the administrative unit below districts in Malawi.

The input fair treatment entailed 2 elements: (a) an input fair organized at a location (mostly schools) near the village, and (b) a transport subsidy to individual respondents to visit the fair. We explain each of these elements below, but we want to first note sample enrollment into these elements. In each of the 100 villages that got the input fair, we invited every member of the village to the input fair. The transport subsidy, however, was given only to a subset of the villagers: each of the 10 households in the study sample received the subsidy, as did 20 other randomly selected households in the village. As such, while subsidy receipt essentially varies at the village level for individuals in our data-collection sample (because either all 10 households received the subsidy or none of them received it), on the ground, this was an individual-level randomization - some households in these villages did not receive vouchers.

The input fairs were organized in collaboration with Agora Ltd., a major agricultural retailer in southern Malawi. Agora is a major participant in the FISP program. In consultation with Agora, we selected convenient location for the inputs fair, and ultimately planned event at 14 locations (13 schools, and 1 Agora shop). Each of the 100 input fair villages was

[7] We had planned to implement a similar intervention in Liberia in 2020, the sister site for the cash transfer study (see Aggarwal et al. 2022a). However, the input fair interventon in Liberia was disrupted by COVID-19. There is a pre-analysis plan for this study on the AEA registry (AEARCTR-0004869) which includes both countries together, but we are forced to restrict attention to Malawi alone.

assigned to the closest location (so that each event included a number of villages at a single time). The average distance between village and the input fair location is about 3km.

A team of enumerators visited the input fair villages in the days preceding the event to advertise it. To reduce the cost of the event as much as possible, we reimbursed respondents for travel costs. Using public transportation, we estimated that the cost of traveling (with a bag of fertilizer) was roughly $0.14 per km on rural unpaved roads (which are the type used to travel to the input fair locations), or about 150 MWK (see Kumar et al. 2022 for more details). We decided to randomize the amount of the discount, between 4 amounts: (1) a flat rate of $0.14 (or 100 MWK); (2) $0.27 per km (of distance between the village and the input fair); (3) $0.55 per km; and (4) $0.82 per km. We provided cash for the one-way trip at the household visit, and provided the return amount at the event itself; purchasing inputs was not a requirement. Thus, potentially, people may have attended the event simply to access the transport voucher, although this should not impact our take-up numbers as take-up is defined in terms of a purchase, not a visit.

During our visits to villages, we attempted to reach every household sampled for our study, but could not reach about 5% of the sample, who therefore did not receive our invitation or transport voucher. These households could have still attended the event but their travel costs were not subsidized. Also, one of the 14 events was ultimately cancelled, because of transportation problems encountered by the input provider (Agora).

## 2.4 Data

In each study village, we conducted baseline surveys in April-July 2019 and endline surveys exactly 2 years after, in April-July 2021. Since we only enrolled 10 households per village in the data-collection, the total sample size for the study is 2,944. Surveys were targeted at female heads of households (because one of the key outcomes of the main evaluation is intimate partner violence – see Park et al. (2022) for details).[8]

[8] In addition, 2 households from each village were randomly selected to take part in bi-monthly phone surveys. This data is used extensively in the main evaluation (Aggarwal et al. 2022a) and in a study evaluating the

In this analysis, our main outcome data comes from two primary surveys. First, we calculate take-up using data collected administratively at the input fair itself (for the treatment group only). Second, we conducted an endline survey 2 years after the baseline, in April-July 2021. Ninety-five percent of baseline respondents completely the endline, and attrition was balanced across treatment groups (see Table A1). For the purpose of this paper, the key outcome of interest is agricultural input usage; however, though not powered, for completeness we also show results for further downstream outcomes such as output, crop choice, and agricultural labor.

## 2.5 Summary Statistics and Randomization Check

Table 1 presents summary statistics and a check of randomization balance for a selected set of indicators. For each variable, we show the control mean in Column 1, and the difference between each treatment group and the control group in Columns 2-4. The average respondent is 40 years old, has 4.9 years of education, and the average household has 4.7 members. Ninety-three percent of the sample is female, because we targeted female heads of household for the main evaluation. Average household expenditures are $35 per month, and the average household has total assets worth about $1,505 (including land and housing, durable goods, livestock, business assets, and financial assets). Ninety-one percent of households own farm land; of those, the average land size is 1.3 acres. Eighty-two percent of households used fertilizer in the year prior to the project, and the average expenditure was about $18. Our sample is largely balanced - of the 30 regression coefficients in the table, only 1 is significant (years of education, among the input fair respondents). Another important coefficient is that for total assets, in the cash + input fair treatment. While the coefficient is not significant, it is large ($114, equivalent to about 9% of the control mean). For this reason, we control for both of these covariates in all regressions, although our results are qualitatively no different even when we do not control for these covariates.

effect of COVID-19 lockdowns in Liberia and Malawi (Aggarwal et al. 2022c), but is not a focus here.

Table 1: Summary Statistics and Randomization Check

| | (1) Pure Control | (2) Coefficient on Difference (Treatment - Control) | (3) | (4) |
|---|---|---|---|---|
| | Mean (std. dev.) | Cash only | Input fairs only | Cash + Input fairs |
| Age | 40.46 | -0.55 | -0.46 | -0.83 |
| | (15.02) | (0.67) | (0.92) | (0.82) |
| =1 if female | 0.93 | 0.01 | 0.01 | -0.00 |
| | | (0.01) | (0.01) | (0.01) |
| Years of education | 4.89 | -0.06 | -0.42** | 0.06 |
| | (3.36) | (0.19) | (0.21) | (0.25) |
| Number of household members | 4.73 | 0.11 | 0.06 | 0.02 |
| | (2.07) | (0.10) | (0.12) | (0.12) |
| Total expenditure (last month, USD) | 35.34 | -1.27 | -2.21 | 0.36 |
| | (33.88) | (1.84) | (1.97) | (2.35) |
| Value of total assets[a] (USD) | 1505.48 | -42.02 | 20.71 | 114.16 |
| | (1988.49) | (99.97) | (130.10) | (131.97) |
| =1 if own farm land | 0.91 | 0.01 | -0.01 | 0.01 |
| | | (0.01) | (0.02) | (0.02) |
| if yes: Farm land size (acres) | 1.26 | -0.02 | 0.04 | -0.07 |
| | (1.04) | (0.05) | (0.07) | (0.07) |
| =1 if used fertilizer | 0.82 | 0.00 | -0.01 | 0.02 |
| | | (0.02) | (0.02) | (0.02) |
| Fertilizer expenditure (USD) | 17.71 | 0.10 | 0.48 | 2.01 |
| | (26.36) | (1.27) | (1.72) | (1.77) |
| Observations | 975 | 987 | 488 | 494 |

Notes: Sample is restricted to households who did not receive FISP coupons. Monetary values are winsorized at the 99th percentile. Dependent variable in rows, each row shows coefficient from a separate regression on respective dependent variable. Standard errors clustered at village level and all regressions include strata fixed effects. $*p < 0.1, **p < 0.05, ***p < 0.01$

[a] Assets include land and housing, durable goods, livestock, business assets, and financial assets.

# 3 Results

## 3.1 Take-up

Table A2 shows summary statistics from the input fair, separately for the treatment and control groups. The first row shows take-up of any input, while the remaining rows break this down by specific input (fertilizer, seeds, or pesticides). We find only modest take-up of the input fair. Only 7% of the sample bought any inputs at all at the fair, spending just over $2. We note meagre take-up by the input fair-only treatment, where only 3% of the respondents bought something and the average spending was only 50 cents. Take-up by the cross-cut treatment is much higher - with 12% of the respondents from that group making a purchase and spending $4 on average. Though we did not explicitly encourage a focus on fertilizer,[9] we see that this is clearly what happened: 95% of purchases by value in the event were fertilizer. As discussed in the introduction, this is not atypical for Malawi, where in our baseline data 93% of the expenditure on inputs was spent on fertilizer. For this reason, we will focus on fertilizer adoption as our main indicator of impact in the remainder of the paper.

In Table 2, we estimate the effect of cash on take-up in a regression (focusing on fertilizer since it was the main input purchased). We regress take-up on input fair with a sparse set of controls as follows:

$$Y_{iv} = \beta Cash_v + \sum_{j=2}^{4} \alpha_j V_j + \lambda_s + \varepsilon_{iv} \tag{1}$$

In this specification, $Cash_v$ is a treatment indicator which takes value 1 if individual $i$ is in a cash transfer treated village and 0 otherwise; $V_j$ are fixed effects for voucher amounts (200, 400, or 600 MWK per km, against the flat 100 MWK payment as baseline); and $\lambda_s$ are strata fixed effects. The coefficient of interest is $\beta$, which represents the effects of cash on input purchases at the events.

[9]Promotional material for the event mentioned all inputs, not fertilizer specifically.

Table 2: Take-up at the Input Fair

| | (1) =1 if purchased fertilizer | (2) Expenditure (USD) | (3) Amount (kg) |
|---|---|---|---|
| Cash ($\beta$) | 0.09*** | 3.26*** | 6.09*** |
| | (0.02) | (0.81) | (1.57) |
| Control mean | 0.02 | 0.42 | 0.77 |
| Observations | 982 | 982 | 982 |

Notes: The sample is restricted to who were offered the market access intervention. Regressions include fixed effects for voucher amounts, as well as for randomization strata. Standard errors clustered at village level. Exchange rate: 1 USD = 730 MWK. $*p < 0.1, **p < 0.05, ***p < 0.01$

Table 2 shows that those in the cash group were 9 percentage points more likely to buy fertilizer, and spent \$3.3 more on fertilizer. Conditional on purchase, the control group therefore spent about \$27 while the treatment group spent about \$31 (a 50 kg bag of NPK or Urea fertilizer cost about \$28 during this time period). Table A3 shows effects by voucher amount (the omitted group received a flat 100 MWK show-up fee). We see some weak evidence that higher voucher amounts may have increased take-up: all coefficients are positive. However, they are non-monotonic and we cannot reject equality of the coefficients.[10]

## 3.2 Input Adoption

While take-up suggests a measurable effect of the intervention, this does not necessarily imply an ultimate effect on input adoption, since the input fair could potentially crowd out purchases that would happen anyway, either through FISP or from the market. To investigate this, we examine the impact of the interventions on *total* input usage reported by the respondents in the endline survey, as compared to the control group. We run the

[10]Take-up by FISP beneficiary status, is shown in Table A4. We see similar take-up figures among beneficiaries and non-beneficiaries, which is expected since beneficiary status was only announced after the input fair treatment.

following regression:

$$Y_{iv} = \beta Cash_v + \gamma InputFair_v + \delta Cash_v \times InputFair_v + \eta Y_{iv0} + \lambda_s + \theta X_i + \varepsilon_{iv} \qquad (2)$$

where $Cash_v$ is a treatment indicator which takes value 1 if individual $i$ is in a cash transfer treated village and 0 otherwise. $InputFair_v$ takes value 1 if individual $i$ belongs to a village which received an input fair, and 0 otherwise. $Y_{iv0}$ is the baseline value of the outcome variable and $\lambda_s$ are strata fixed effects. $X_i$ is a vector of baseline controls for total assets and education, since treatment groups differ in those variables at baseline. Standard errors are clustered at the village level.

Results are presented in Table 3. Column 1 shows the extensive margin and Column 2 the (unconditional) total expenditure on fertilizer (in USD).

Table 3: Input Adoption

| | (1) =1 if used fertilizer | (2) Expenditure (USD) |
|---|---|---|
| Cash ($\beta$) | 0.05*** | 4.91*** |
| | (0.02) | (1.13) |
| Input fair ($\gamma$) | 0.01 | -0.98 |
| | (0.02) | (1.19) |
| Cash $\times$ Input fair ($\delta$) | -0.03 | 4.05** |
| | (0.03) | (1.87) |
| | | |
| $p$-value: | | |
| $\gamma + \delta = 0$ | 0.320 | 0.036 |
| Pure control mean | 0.85 | 18.76 |
| Observations | 2,784 | 2,784 |

Notes: Expenditure on chemical fertilizer is the total expenditures on fertilizer used on own farm, excluding the expenditure on fertilizer shared with others. Regressions include baseline measurements of outcome, strata fixed effects, and baseline controls for education level and asset value. Standard errors clustered at village level. Exchange rate: 1 USD = 730 MWK. $*p < 0.1, **p < 0.05, ***p < 0.01$

We begin by noting that despite the high levels of input usage at baseline (85% and

$19 in the pure control group), cash transfers meaningfully and significantly improve input adoption (by 5 percentage points and $4.9). This result is noteworthy even in isolation, since it shows a channel between one-time cash transfers and agricultural productivity.

By contrast, we find no effect of the input fair alone. Although a prior literature has shown important effects of market access on input usage, reducing transport costs alone appears to have been ineffective in this setting. While a null result is inconsistent with our priors, we conjecture that perhaps details of the Malawian agricultural environment may have dampened effects, particularly the fact that the FISP program exists but distribution had not yet occurred – perhaps respondents were waiting to see if they were a beneficiary before purchasing inputs at the full retail price.

The main result, however, is that of the combined effect of cash and market access. While we find no effect on the extensive margin (in fact, the point estimate is the wrong sign, though not significant), we find a large effect on the intensive margins of $4.1, significant at 5%, which is virtually identical to the effect of cash alone. This implies that the combined effect of cash and market access doubles the effect of cash alone: the combined effect ($10) is 48% of baseline fertilizer expenditure.[11] Table A5 shows effects on input adoption by FISP beneficiary status. We see that the cash * input fair effect is driven by non-beneficiaries, a result which we turn to below.

## 3.3 Unintended Consequences

Our results thus far show that the input fair combined with cash increased expenditure on fertilizer, above and beyond the effect of cash alone. While our focus was on agricultural investment, a similar design could be used for other types of investment; for example, cash transfers could have been coupled with incentives to invest for other purposes, such as education.

[11] Table A6 shows effects by the specific amount of cash. As expected, we find a monotonic relationship between the amount of cash and take-up. The results for the cash*input fair treatment are noisier on the extensive margin, but follow the same pattern for quantities.

As such, the ultimate impact of this sort of bundling will depend on contextual details, for example prices or access outside of the intervention. In our experiment, there is one particular policy program that affected our intervention: Malawi's Farm Input Subsidy Program (FISP). During our study period, the subsidy was for about 75% of the cost of inputs, and was distributed via a paper coupon handed out to beneficiaries, redeemable at a local shop. As discussed earlier, we designed the intervention to occur after FISP coupons had been allocated, but this did not happen in practice; instead, people came to the input fairs not knowing if they would qualify for FISP or not. During this time period, the actual distribution of FISP coupons was supposed to be random: to address corruption/nepotism concerns, the government decided to allocate subsidies randomly (see Kumar et al. 2022 for an analysis of the program, which shows that the randomization appeared to have been effective). It is conceivable that local leaders could undo this FISP randomization as a result of our intervention, prioritizing other farmers for coupons. This is very unlikely for the cash transfer experiment, since during this time period local village-level chiefs were meant to only be involved in the distribution. In addition, the cash transfers were universal; however,the benefits of the input fair were not universal (the event itself was available to everybody, but only selected respondents were invited and received vouchers). We find no evidence that the interventions affected targeting: see Table A7. There is no decrease in the likelihood of FISP receipt in the cash or cash * input fair groups.

So while there is no effect on coupon receipt itself, after FISP is disbursed, the inputs are widely shared, often at the direction of the chief (i.e. Basurto et al. 2020), and as such people who purchased early at the input fair may have lost out on sharing later on, and so may have effectively paid higher prices.

We examine this in Table 4. First, in Column 1, we show the overall quantity of fertilizer. We then look at market purchases (Columns 2-3) and purchases via FISP, either via direct purchase for beneficiaries or sharing for non-beneficiaries (Columns 4-5). For the cash treatment, we find a statistically significant effect of about 11.9 kg (about 18% on a base of

67 kg), occurring mostly via market purchases. We see a small, positive, insignificant effect on FISP fertilizer.

Table 4: Fertilizer bought at market and shared via FISP

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | | Market | | FISP | |
| | Total amount (kg) | Expenditure (USD) | Amount (kg) | Expenditure (USD) | Amount (kg) |
| Cash ($\beta$) | 11.94*** | 4.48*** | 9.23*** | 0.41 | 2.63 |
| | (2.42) | (1.14) | (1.95) | (0.46) | (2.32) |
| Input fair ($\gamma$) | 2.25 | -1.01 | -0.37 | 0.03 | 2.55 |
| | (3.06) | (1.19) | (2.14) | (0.53) | (2.83) |
| Cash $\times$ Input fair ($\delta$) | -0.91 | 4.03** | 3.58 | 0.07 | -4.30 |
| | (4.16) | (1.85) | (3.16) | (0.76) | (3.65) |
| *p*-value: | | | | | |
| $\gamma + \delta = 0$ | 0.639 | 0.035 | 0.170 | 0.858 | 0.451 |
| Pure control mean | 67.28 | 10.58 | 20.83 | 8.19 | 46.45 |
| Observations | 2,784 | 2,784 | 2,784 | 2,784 | 2,784 |

Notes: Expenditure on chemical fertilizer is the total expenditures on fertilizer used on own farm, excluding the expenditure on fertilizer shared with others. Regressions include baseline measurements of outcome, strata fixed effects, and baseline controls for education level and asset value. Standard errors clustered at village level. Exchange rate: 1 USD = 730 MWK. $*p < 0.1, **p < 0.05, ***p < 0.01$

However, for the cash * input fair treatment, we observe no incremental effect whatsoever on the overall quantity used, despite the sizeable effect on expenditures that we observed in Table 3. From the remaining columns, we see what happened: while market purchases increased (though insignificantly by amount), FISP purchases decreased by a similar amount, cancelling out any effect of the intervention.[12]

# 4 Discussion and Conclusion

This paper aims to understand the effect of simultaneously relieving liquidity and market access constraints on agricultural investment. We find that relaxing liquidity constraints

[12] Table A8 shows this decomposition by FISP beneficiary status. Though results are imprecise, we see a similar crowd-out effect among beneficiaries and non-beneficiaries. Finally, for completeness, we show effects on downstream outcomes including crop choice and output (Table A9) and agricultural labor (Table A10). We are not well-powered to see such effects, since the effect on inputs is modest; as expected, we don't see any statistically significant treatment effects.

alone via cash transfers can expand input usage significantly. This is an important finding as the role of liquidity constraints in impeding input adoption has been long suspected, but is not well established (see papers such as Croppenstedt et al. 2003 and Karlan et al. 2014).

By contrast, we find that reducing transport costs via our input fair treatment alone does not lead to any uptick in input usage. An emerging literature, specifically, work by Aggarwal et al. (2022b) and Cedrez et al. (2020), shows that farmers located in remote villages have poor physical access to input retailers. It is an open policy question if merely removing some of these access constraints, for example, by subsidizing retailer entry into remote locations, is likely to lead to increased technology adoption. Though our prior was that such an intervention would be effective, we do not find evidence to support this here. We conjecture that contextual details in Malawi are a primary explanation, specifically the existence of FISP, but we hope future research will shed more light on this question.

Even despite the context, however, we find that the combination of the input fair and cash transfer treatments had a large effect, doubling the effect of cash alone. This result highlights that relieving multiple constraints concurrently can boost the effect of a single intervention. While this study was designed to understand how to improve the efficacy of cash transfers for boosting productive investments, this finding has implications for a broad range of input adoption policies. For example, a subsidy program that is accompanied by a strengthening of the input retail network will likely be more effective than a stand-alone subsidy program.

An open question for future work is whether our results are driven in part by a nudge effect. In our experiment, the input fair may have made agricultural inputs particularly salient, and nudged people to invest in them specifically (similar to prior work such as Duflo et al. 2011). However, in a real-world analogue of an expanded retail network, this channel would not be present. Our experiment was not designed to disentangle this effect from that of pure access, and we leave this exploration to future work.

In a similar vein, our research clearly shows that making inputs more accessible can spur adoption when farmers already have cash on hand. It is very possible, however, that making *any* product accessible would have increased spending on it, and thus any beneficial effect of such an intervention depend on what is being offered. In our case, it turned out that Malawi's large-scale FISP input program was disbursed only *after* our intervention, and beneficiaries of the input fair + cash intervention received less of these benefits (due to reduced sharing). Thus, while the total amount of fertilizer used in these villages increased, this is not true of the beneficiaries themselves. While this detail was unexpected and specific to the Malawi context, it does point to the importance of carefully designing complementary interventions to programs such as cash transfers.

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# 5 Appendix

Figure A1: Timeline of Survey Activities

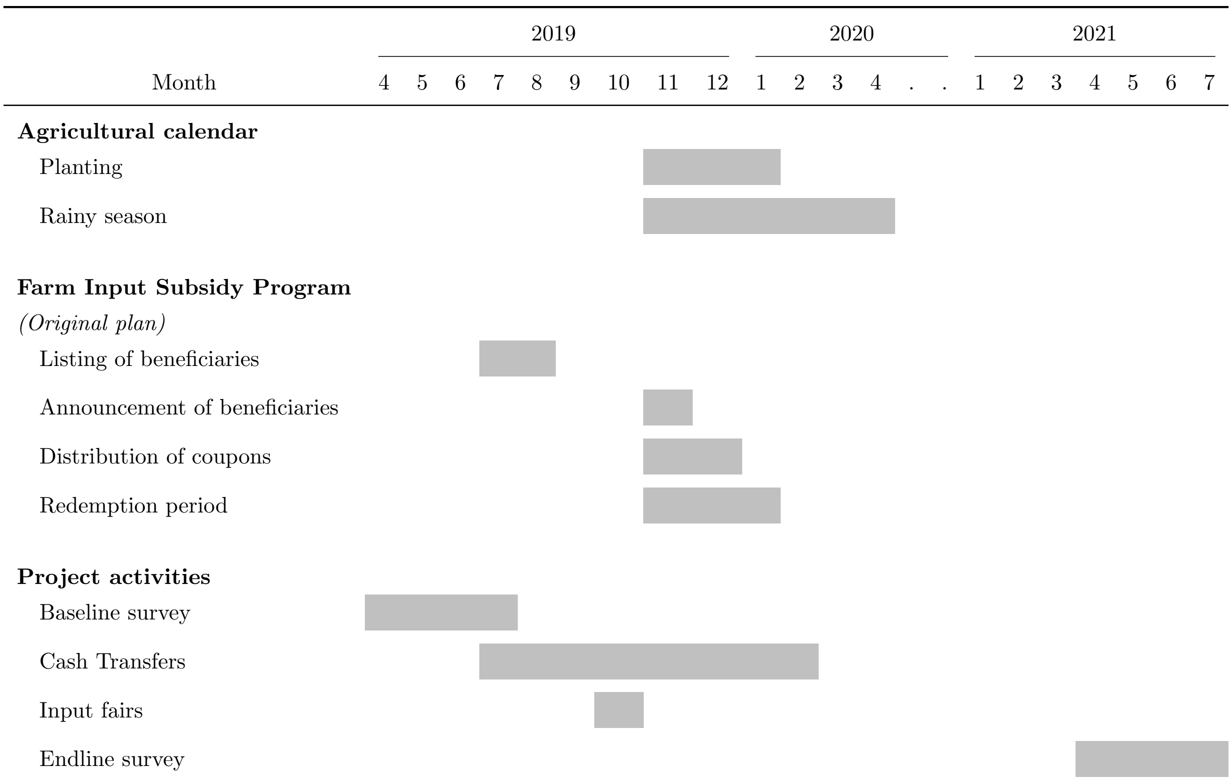


Table A1: Endline Survey Attrition

| | (1)<br>=1 if completed<br>endline survey |
|---|---|
| Cash | 0.01 |
| | (0.01) |
| Input fair | -0.00 |
| | (0.01) |
| Cash × Input fair | 0.00 |
| | (0.02) |
| | |
| Pure control mean | 0.94 |
| Overall mean | 0.95 |
| Observations | 2,944 |

Notes: Regression include strata fixed effects. Standard errors clustered at village level and are in parentheses.. $*p < 0.1, **p < 0.05, ***p < 0.01$

Table A2: Take-up by Type of Input

| | (1) Total input fair sample | (2) Input fair treatment only | (3) Cash + input fair |
|---|---|---|---|
| **Any input** | | | |
| =1 if bought any | 0.07 | 0.03 | 0.12 |
| Amount spent (USD) | 2.23 | 0.50 | 3.95 |
| | (9.23) | (3.49) | (12.31) |
| **Chemical fertilizer** | | | |
| =1 if bought any | 0.06 | 0.01 | 0.11 |
| Amount spent (USD) | 2.12 | 0.39 | 3.82 |
| | (9.10) | (3.25) | (12.18) |
| **Improved seeds** | | | |
| =1 if bought any | 0.01 | 0.01 | 0.01 |
| Amount spent (USD) | 0.08 | 0.07 | 0.10 |
| | (0.91) | (0.78) | (1.02) |
| **Pesticides** | | | |
| =1 if bought any | 0.01 | 0.01 | 0.00 |
| Amount spent (USD) | 0.04 | 0.04 | 0.03 |
| | (0.71) | (0.72) | (0.71) |
| Observations | 982 | 494 | 488 |

Notes: Sample restricted to study households in market access intervention arm. Monetary values winsorized at 99th percentile. Exchange rate: 1 USD = 730 MWK.

Table A3: Take-up by Travel Voucher Amount

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | =1 if purchased fertilizer | | Expenditure (USD) | |
| Voucher MKW200 per km | 0.02 | 0.02 | 0.79 | 0.81 |
| | (0.02) | (0.02) | (0.66) | (0.64) |
| Voucher MKW400 per km | 0.05** | 0.05** | 2.09*** | 2.00** |
| | (0.02) | (0.02) | (0.76) | (0.77) |
| Voucher MKW600 per km | 0.02 | 0.02 | 1.23 | 1.24 |
| | (0.02) | (0.02) | (0.86) | (0.85) |
| Cash treatment | Pooled | Disaggregated | Pooled | Disaggregated |
| $p$-values: | | | | |
| joint equality | 0.463 | 0.553 | 0.433 | 0.498 |
| joint significance | 0.094 | 0.104 | 0.026 | 0.030 |
| Voucher MKW100 per km: mean | 0.04 | 0.04 | 1.04 | 1.04 |
| Observations | 982 | 982 | 982 | 982 |

Notes: The sample is restricted to respondent who who were offered the market access intervention. Regressions include fixed effects for voucher amounts. The omitted group received a flat 100 MWK fee. Standard errors clustered at village level. Exchange rate: 1 USD = 730 MWK. $*p < 0.1, **p < 0.05, ***p < 0.01$

Table A4: Input Fair Take-up, by FISP beneficiary status

| | (1) | (2) | (3) |
|---|---|---|---|
| | =1 if purchased fertilizer | Expenditure (USD) | Amount (kg) |
| **Panel A. Non-FISP beneficiaries** | | | |
| Cash ($\beta$) | 0.09*** | 3.22*** | 6.12*** |
| | (0.02) | (0.83) | (1.66) |
| Control mean | 0.02 | 0.42 | 0.77 |
| Observations | 774 | 774 | 774 |
| **Panel B. FISP beneficiaries** | | | |
| Cash ($\beta$) | 0.08*** | 3.12** | 5.53** |
| | (0.03) | (1.19) | (2.14) |
| Control mean | 0.01 | 0.28 | 0.52 |
| Observations | 208 | 208 | 208 |

Notes: The sample is restricted to who were offered the market access intervention. Panel A is for the subsample who had not received FISP before our intervention, and Panel B is for those who had. Regressions include fixed effects for voucher amounts, as well as for randomization strata. Standard errors clustered at village level. Exchange rate: 1 USD = 730 MWK. $*p < 0.1, **p < 0.05, ***p < 0.01$

Table A5: Input Adoption, by FISP beneficiary status

| | (1)<br>=1 if used<br>fertilizer | (2)<br>Expenditure<br>(USD) |
|---|---|---|
| **Panel A. Non-FISP beneficiaries** | | |
| Cash ($\beta$) | 0.06*** | 4.80*** |
| | (0.02) | (1.31) |
| Input fair ($\gamma$) | 0.00 | -1.26 |
| | (0.03) | (1.36) |
| Cash $\times$ Input fair ($\delta$) | -0.04 | 5.07** |
| | (0.03) | (2.14) |
| | | |
| $p$-value: | | |
| $\gamma + \delta = 0$ | 0.081 | 0.024 |
| Pure control mean | 0.84 | 18.86 |
| Observations | 2,239 | 2,239 |
| | | |
| **Panel B. FISP beneficiaries** | | |
| Cash ($\beta$) | 0.01 | 6.61*** |
| | (0.03) | (2.17) |
| Input fair ($\gamma$) | 0.03 | 1.50 |
| | (0.03) | (2.41) |
| Cash $\times$ Input fair ($\delta$) | 0.03 | -2.39 |
| | (0.05) | (3.68) |
| | | |
| $p$-value: | | |
| $\gamma + \delta = 0$ | 0.085 | 0.750 |
| Pure control mean | 0.89 | 18.40 |
| Observations | 545 | 545 |

Notes: Expenditure on chemical fertilizer is the total expenditures on fertilizer used on own farm, excluding the expenditure on fertilizer shared with others. Panel A is for the subsample who had not received FISP before our intervention, and Panel B is for those who had. Regressions include baseline measurements of outcome, strata fixed effects, and baseline controls for education level and asset value. Standard errors clustered at village level. Exchange rate: 1 USD = 730 MWK. $*p < 0.1, **p < 0.05, ***p < 0.01$

Table A6: Input Adoption, by cash transfer amount

| | (1) =1 if used fertilizer | (2) Expenditure (USD) |
|---|---|---|
| Cash \$250 ($\beta_1$) | 0.03 | 2.29* |
| | (0.02) | (1.30) |
| Cash \$500 ($\beta_2$) | 0.06** | 6.11*** |
| | (0.02) | (1.78) |
| Cash \$750 ($\beta_3$) | 0.06** | 6.34*** |
| | (0.03) | (1.59) |
| Input fair ($\gamma$) | 0.01 | -0.98 |
| | (0.02) | (1.19) |
| Cash \$250 × Input fair ($\delta_1$) | -0.01 | 2.00 |
| | (0.03) | (2.14) |
| Cash \$500 × Input fair ($\delta_2$) | -0.07* | 4.72 |
| | (0.04) | (2.99) |
| Cash \$750 × Input fair ($\delta_3$) | 0.00 | 5.74** |
| | (0.05) | (2.75) |
| *p*-values: | | |
| $\gamma + \delta_1 = 0$ | 0.982 | 0.570 |
| $\gamma + \delta_2 = 0$ | 0.042 | 0.173 |
| $\gamma + \delta_3 = 0$ | 0.737 | 0.056 |
| $\delta_1 = \delta_2 = \delta_3$ | 0.184 | 0.424 |
| Pure control mean | 0.85 | 18.76 |
| Observations | 2,784 | 2,784 |

Notes: Expenditure on chemical fertilizer is the total expenditures on fertilizer used on own farm (FISP and non-FISP), excluding the expenditure on fertilizer shared with others. Regressions include baseline measurements of outcome, strata fixed effects, and baseline controls for education level and asset value. Standard errors clustered at village level. Exchange rate: 1 USD = 730 MWK. $*p < 0.1, **p < 0.05, ***p < 0.01$

Table A7: FISP Beneficiary Status in Year of Program

| | (1)<br>=1 if received FISP voucher<br>after intervention (2019-2020) |
|---|---|
| Cash ($\beta$) | -0.02 |
| | (0.02) |
| Input fair ($\gamma$) | 0.04 |
| | (0.03) |
| Cash $\times$ Input fair ($\delta$) | -0.01 |
| | (0.04) |
| | |
| $p$-value: | |
| $\gamma + \delta = 0$ | 0.218 |
| Pure control mean | 0.39 |
| Observations | 2,784 |

Notes: Regressions include baseline measurements of outcome (FISP beneficiary status before our intervention) and strata fixed effects. Standard errors clustered at village level. $*p < 0.1, **p < 0.05, ***p < 0.01$

Table A8: Fertilizer bought at market and obtained via FISP, by FISP beneficiary status

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | | Market | | FISP | |
| | Total amount (kg) | Expenditure (USD) | Amount (kg) | Expenditure (USD) | Amount (kg) |
| **Panel A. Non-FISP beneficiaries** | | | | | |
| Cash ($\beta$) | 11.75*** | 4.34*** | 9.23*** | 0.42 | 2.49 |
| | (2.63) | (1.31) | (2.19) | (0.50) | (2.46) |
| Input fair ($\gamma$) | 0.59 | -1.22 | -1.17 | -0.06 | 1.71 |
| | (3.23) | (1.33) | (2.34) | (0.58) | (3.06) |
| Cash × Input fair ($\delta$) | 1.40 | 4.97** | 5.73 | 0.18 | -4.23 |
| | (4.52) | (2.10) | (3.56) | (0.83) | (3.95) |
| $p$-value: | | | | | |
| $\gamma + \delta = 0$ | 0.534 | 0.022 | 0.090 | 0.850 | 0.313 |
| Pure control mean | 65.58 | 11.07 | 21.27 | 7.79 | 44.31 |
| Observations | 2,239 | 2,239 | 2,239 | 2,239 | 2,239 |
| **Panel B. FISP beneficiaries** | | | | | |
| Cash ($\beta$) | 14.04*** | 6.01*** | 10.50** | 0.61 | 4.02 |
| | (5.43) | (2.22) | (4.24) | (1.02) | (5.15) |
| Input fair ($\gamma$) | 11.45* | 0.80 | 4.65 | 0.72 | 7.33 |
| | (6.75) | (2.49) | (5.85) | (1.09) | (5.68) |
| Cash × Input fair ($\delta$) | -13.96 | -1.63 | -8.30 | -0.81 | -7.27 |
| | (9.33) | (3.74) | (7.54) | (1.57) | (8.11) |
| $p$-value: | | | | | |
| $\gamma + \delta = 0$ | 0.704 | 0.764 | 0.452 | 0.936 | 0.992 |
| Pure control mean | 73.94 | 8.65 | 19.10 | 9.75 | 54.84 |
| Observations | 545 | 545 | 545 | 545 | 545 |

Notes: Expenditure on chemical fertilizer is the total expenditures on fertilizer used on own farm, excluding the expenditure on fertilizer shared with others. Panel A is for the subsample who had not received FISP before our intervention, and Panel B is for those who had. Regressions include baseline measurements of outcome, strata fixed effects, and baseline controls for education level and asset value. Standard errors clustered at village level. Exchange rate: 1 USD = 730 MWK. $*p < 0.1, **p < 0.05, ***p < 0.01$

## Table A9: Crop Choice and Agricultural Productivity

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| | Maize (staple) | | | Number of crops planted/harvested: | | | |
| | =1 if planted any | Amount harvested (kg) | Value of harvest (USD) | Non-staple cereals[a] | Legumes[b] | All crops pooled | Total value of non-staples harvested (USD) |
| Cash ($\beta$) | 0.02** | 45.27** | 12.94** | 0.03 | 0.07* | 0.15*** | 9.26 |
| | (0.01) | (17.88) | (5.11) | (0.03) | (0.04) | (0.05) | (7.95) |
| $\text{MA}_{t}reat$ | 0.00 | 18.59 | 5.32 | 0.05 | -0.03 | 0.02 | -5.00 |
| | (0.01) | (23.32) | (6.67) | (0.03) | (0.05) | (0.06) | (8.21) |
| Cash × Input fair ($\delta$) | -0.01 | -10.94 | -3.13 | -0.05 | -0.03 | -0.12 | 11.09 |
| | (0.01) | (33.41) | (9.55) | (0.05) | (0.07) | (0.09) | (13.23) |
| $p$-value: | | | | | | | |
| $\gamma + \delta = 0$ | 0.786 | 0.746 | 0.746 | 0.819 | 0.274 | 0.113 | 0.551 |
| Pure control mean | 0.96 | 213.48 | 61.03 | 0.36 | 0.88 | 2.23 | 49.72 |
| Observations | 2,784 | 2,784 | 2,784 | 2,784 | 2,784 | 2,784 | 2,784 |

Notes: Regressions include baseline measurements of outcome, strata fixed effects, and baseline controls for education level and asset value. Standard errors clustered at village level. Exchange rate: 1 USD = 730 MWK. $*p < 0.1, **p < 0.05, ***p < 0.01$

## Table A10: Agricultural Land and Labor Supply and Demand

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | | Labor supply (past month, hours) | | Labor demand (past month, hours) |
| | Land size for farming (acre) | Own farm | Casual labor (off farm) | Own farm |
| Cash ($\beta$) | -0.68 | 1.18 | 0.39 | 0.17 |
| | (0.46) | (0.92) | (2.37) | (0.45) |
| $\text{MA}_{t}reat$ | -1.01** | 0.94 | 4.43 | 0.12 |
| | (0.42) | (1.03) | (2.90) | (0.60) |
| Cash × Input fair ($\delta$) | 1.54** | -1.01 | -5.51 | -0.76 |
| | (0.65) | (1.56) | (3.89) | (0.70) |
| $p$-value: | | | | |
| $\gamma + \delta = 0$ | 0.276 | 0.952 | 0.675 | 0.081 |
| Pure control mean | 2.29 | 9.67 | 20.68 | 1.03 |
| Observations | 2,784 | 2,784 | 2,784 | 2,784 |

Notes: Regressions include baseline measurements of outcome, strata fixed effects, and baseline controls for education level and asset value. Standard errors clustered at village level. Exchange rate: 1 USD = 730 MWK. $*p < 0.1, **p < 0.05, ***p < 0.01$