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Shortening the path to productive investment: Evidence from input fairs and cash transfers in Malawi[☆]

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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 27%, while the joint provision of cash and the input fair increases investment by about 40%; thus, the incremental effect of the input fair is equivalent to about a 50% increase compared to the effect of cash alone. Input fairs alone were ineffective.

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] Yet an increase in productive investment is necessary if effects of cash are to persist in the longer-term, and so 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; Bassi et al., 2022; Kaboski et al., 2022), limits in entrepreneurial ability (Banerjee et al., 2021; Beaman et al., 2023; Maitra et al., 2017), or missing markets for risk mitigation (Cole et al., 2017; Emerick et al., 2016; Ghosh and Vats, 2023; Karlan et al., 2014). This study is set in rural Malawi,

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

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where the vast majority of households farm, and therefore, the most natural form of productive investment would be in agricultural inputs such as chemical fertilizer and improved seeds. In this context, one particularly important constraint is market access, and prior work has shown it to be an important barrier to investment in productive farm inputs (Aggarwal et al., 2022b; Cedrez et al., 2020; Minten et al., 2013). Specifically, since agro-retailers tend to be sparsely located, the travel cost-adjusted prices of inputs are high; in some cases, even rendering these inputs unprofitable (Suri, 2011). In addition, farmers may face other non-pecuniary costs of travel, such as uncertainty about input availability (Aggarwal et al., 2022b). In related work in Malawi, the same context as this study, we document that the average farmer faces an effective ad-valorem travel cost of about 20% on a bag of fertilizer purchased at market prices (i.e., $5.4 on a retail price of about $29 for a 50 kg bag). Moreover, these costs go up by nearly $2 for every standard deviation increase in our measure of the remoteness of the farmer's village (Kumar et al., 2022).

To what extent do these access constraints prevent productive investment of cash transfers by beneficiary farmers? We conducted an experiment in 300 villages in two districts in Southern Malawi to study this question. All households in half the villages received one-time cash transfers from the NGO GiveDirectly, delivered shortly before planting. The transfers averaged $500 in nominal terms, a large sum in this context, equivalent to about 125% of average estimated annual household expenditure.[2] To examine if the impacts of these transfers are muted by market access constraints, we cross-cut an individual-level experiment. The add-on experiment was 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). We coordinated with Agora to offer inputs for sale on predesignated days at locations near farmers' homes (usually in schools), and marketed these as "input fairs". We subsidized the cost for farmers to attend these events, to get at the "best-case" scenario where transport costs are fully eliminated.[3]

Thus, our experimental design generates four treatment groups: a group that received market access only, one that received cash only, one that received both together, and a control group that received neither intervention. Because the vast majority of purchases (about 95% by value) at the input fair were of fertilizer, we focus our analysis on take-up for this input.[4] However, we did offer other inputs – hybrid seeds and pesticides – at the fairs.

An important contextual detail in the interpretation of our study is the presence of the Farm Input Subsidy Program (FISP). FISP is a long-standing program that has formed the backbone of Malawi's food security strategy over the past 2 decades. In the year of our study, FISP provided a subsidy worth approximately 75% on inputs that are worth about $50 at market prices. The subsidies were distributed as paper coupons, redeemable at local retailers who were selected to participate in the program. Subsidies are not universal; during our study year, 19% of our sample received the subsidy. However, as documented in a large prior literature, FISP benefits are often shared with non-beneficiaries, many times at the direction of a local chief (i.e. Basurto et al. (2020)). Sharing typically involves a beneficiary splitting a bag with a non-beneficiary, with each paying their share of the total subsidized cost (rather than giving away fertilizer for free). Because planting occurs around November, beneficiaries are meant to be selected and coupons distributed in September/October.

However, there are several important details about the implementation of the program in the year of our study. First, due to concerns about nepotism and poor targeting, FISP was moved to a purportedly random distribution, beginning in 2017–18 and continuing into 2019–20. We study this policy in a companion paper (Kumar et al., 2022), but find evidence that the allocation in 2019–20 may have been flawed (in particular, many people report receiving only partial coupons, which is not in accord with program rules). Thus, while we perform heterogeneity in this paper by FISP beneficiary status, we do not argue that the allocation is random. Second, the input fairs took place in mid October 2019; we scheduled the fairs for this time so that it would occur *after* the FISP coupons had been distributed. Unfortunately, FISP was delayed that year, and so people did not know their official FISP status or whether someone was going to share inputs with them at the time of the input fair.

With these details in mind, we turn to our main results. We find limited effect of the input fair alone: only 2% of farmers purchased inputs at the event. Those that did purchase at the event bought on average about 1 50 kg bag of fertilizer, but because so few bought anything at the event, unconditional average spending was just under 50 cents. However, take-up was substantially higher among those who also received cash: the proportion buying inputs increased by 9 percentage points (to 5.5 times that of the non-cash group), and purchases increased by even more on a proportional basis.

Because purchases at the event may be inframarginal (i.e., those who purchased at the event might have otherwise purchased inputs anyway), the more important result is the effect of the intervention on overall fertilizer usage from all sources (i.e., including, but not limited to the input fair). We find that cash transfers alone increased spending on chemical fertilizer by about $5.4 (equivalent to 27% of fertilizer usage in the control group), implying that cash alone can increase agricultural investment. Providing the input fair in addition to cash increased investment to about $8.1, thus increasing the effect of cash by about $2.5, or 50%. The difference between cash only and cash + input fair has a p-value of 0.07. Expectedly, effects are driven entirely by market purchases of fertilizer, and not by FISP: the additional $2.5 spending on fertilizer is concentrated on market purchases only, with a p-value of 0.0523 for the cash intervention compared to cash + input fair. We observe little evidence of crowd-out effects: the incremental effect of the input fair on adoption ($2.5) is close to that shown in the take-up ($3.3). As would be expected given the low take-up of the input fair without cash, the input fair alone had no effect, suggesting that resolving access constraints in isolation may have limited efficacy in encouraging adoption.

Our results show that in this context, simultaneously addressing market access 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. These papers show consistently large effects on immediate outcomes, but more mixed evidence for productive investment. A few recent studies show large effects on physical capital (Blattman et al., 2013; McIntosh and Zeitlin, 2022), although these are differentiated by their context of being set around a business grant program in the former case and explicitly targeting unemployed youth in the latter case. Therefore, these may not have captured the investment ability and propensity of a more general population. In the recent (Banerjee et al., 2023) evaluation of universal basic income in Kenya, the authors show that neither a guaranteed income stream nor a lump-sum cash transfer ($500 nominal, equivalent to that in our study) was successful in increasing agricultural input usage or agricultural income (point estimates are positive but insignificant), although the authors do find large effects on non-agricultural income and investments. Thus, the question of how to improve the investment impact of cash transfers in a largely agrarian setting remains open.

[2] 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.

[3] The market access event is very unlikely to affect outcomes beyond input investment, and thus is included as a control but is not discussed extensively in our companion paper (Aggarwal et al., 2022a).

[4] 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 $18.11 on agricultural inputs, and 90% of this amount ($16.34) was spent on fertilizer.

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 this study, we show that improving access can spur investment — but only if credit/liquidity constraints are simultaneously addressed.

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, 2022; Bassi and Nansamba, 2021; Beam, 2016), although such kinds of job fairs may resolve multiple constraints at once, whereas our intervention is narrowly focused around transport costs.

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

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

2.1. Farm Input Subsidy Program (FISP)

Malawi has a large-scale Farm Input Subsidy Program (FISP).[5] Under FISP, respondents receive coupons for inputs, which are redeemable at a subsidized price at local input retailers. The subsidy was substantial during the period we studied: each beneficiary received coupons for a 75% discount on inputs that are worth about $50 at market prices. Consequently, redemption is nearly universal (Kumar et al., 2022). However, the subsidy is only given out to a small minority of households: in our data, only about 15% of households (chosen randomly in the district office through a computer-based lottery) receive FISP in any given year.[6] Thus, many farmers must buy fertilizer from retail shops at the market price, making market access an important constraint.[7] An important detail about the FISP program is that benefits are commonly shared among villagers, often at the direction of village chiefs.[8]

A final important detail is that, in our study year, FISP coupons were given out *after* our intervention. We scheduled our intervention to occur in October 2019, just before planting in November. FISP coupons are supposed to be distributed well before this time, but in some years it does happen that the program implementation is delayed. Unfortunately, this is what happened in 2019: by October, beneficiaries had not even been announced, and coupons were only given out later. Consequently, treatment farmers did not know if their FISP status. We return to this issue in greater detail below.

2.2. Setting and cash transfer experiment

The NGO GiveDirectly (henceforth, GD) implemented cash transfers in two districts of Malawi in 2019–2020 — Chiradzulu and Machinga. Villages with less than 100 households (as measured in the 2018 population census) were eligible. 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 as well as control), we attempted to enroll 10 households in the study, i.e., we did baseline and endline surveys with them. However, we were not able to enroll 10 households in every village.

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 October 2019 to ensure that they had sufficient liquidity at the time of planting, which occurs in the month of November in these areas.[9] See Figure A1 for more detail on when the different project activities were implemented.

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, between October 14 and 18, 2019.[10]

Of the 300 villages in the main cash transfer study (Aggarwal et al., 2022a), we selected 100 for 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 voucher 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 voucher, however, was given only to a subset of the villagers: each of the 10 households in the study sample received the

[5] The scale and targeting of FISP has changed dramatically over time, and earlier iterations of the program have been studied extensively in prior work. A partial list of papers includes Chirwa and Dorward (2013), Dorward et al. (2008) and Basurto et al. (2020), among others.

[6] Random allocation was adopted as a practice in 2016–17 due to allegations of nepotism under the previous allocation mechanism which was run via village chiefs.

[7] FISP vouchers must also be redeemed at the same retail shops as where market fertilizer is sold, and so transport costs must be incurred by FISP beneficiaries as well. However, our intervention was not designed to address this issue as FISP coupons can only be redeemed at select locations, and the input fair was not one of these.

[8] We also note that our data suggests that during the 2019–20 season (i.e., the season of interest), marked by a contentious presidential election, there may have been departures from the FISP allocation rules: nearly 3X the usual number of people reported receiving FISP, and many received partial coupons, for which there is no policy provision. Thus, while we perform heterogeneity in this paper by FISP beneficiary status, we do not argue that the allocation is random.

[9] Every treatment household received their first tranche of cash transfer payments by October 9, 2019.

[10] 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 intervention 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 in this paper, we are forced to restrict attention to Malawi alone.

transport voucher, as did 20 other randomly selected households in the village.

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 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 3 km. We created a schedule of events in collaboration with Agora. On those dates, Agora sent a truck with inputs to the location. The inputs included a few items that would be useful to farmers, specifically several varieties of fertilizer, seeds and pesticides.

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 randomized the discount, between 4 amounts: (1) a flat rate of $0.14 (or 100 MWK); (2) 200 MWK or $0.27 per km (of distance between the village and the input fair); (3) 400 MWK or $0.55 per km; and (4) 600 MWK or $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.[11]

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 canceled, 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. Outcomes were collected at the household level, but the 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. (2024) for details).[12] In this paper, we do not focus on gendered outcomes, but instead focus on household-level agricultural outcomes.

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.[13] Ninety-five percent of baseline respondents completely the endline (2784 households), 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. Measuring input usage is slightly complicated in this context, due to the presence of FISP and the widespread sharing of FISP benefits. To measure usage, we asked respondents to separately report their purchases of fertilizer at market (unsubsidized) prices (whether paid for in cash or on credit), and the fertilizer that they obtained via FISP. As mentioned above, sharing of FISP packages is common; typically, a beneficiary will split her package with another person, and that person will pay for their share of the inputs; it is rare to give away inputs for free. To measure FISP fertilizer, we asked beneficiaries to record how much of the coupon they redeemed, and then separately about how much they shared with others. We consider the residual amount, kept by the respondent for her own farm, as her FISP purchases. For non-beneficiaries, we asked about fertilizer shared by beneficiaries. Thus, because fertilizer is not typically saved, our measure of purchases (expenditure and quantity) should be considered the value and quantity of input actually used on the farm.[14]

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 the 2784 households that completed an endline). 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 40.5 years old, has 4.9 years of education, and the average household has 4.9 members. Ninety-four percent of the sample is female, because we targeted female heads of household for the main evaluation. Average household expenditures are $34 per month, and the average household has total assets worth about $1525 (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-three percent of households used fertilizer in the year prior to the project, and the average expenditure on fertilizer was about $16. 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 ($102, equivalent to about 6% of the control mean). For this reason, we control for both of these covariates in all regressions.

3. Results

3.1. Take-up

Table A3 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: only 7% bought an input at the fair, spending just over $2 on average. We note, however, that take-up is higher among those who also received cash: 12% vs. 3% on the extensive margin, and $4 vs. $0.5 in expenditures. Comparing rows, we also observe that the vast majority of purchases (95% by value) were for fertilizer. As discussed in the introduction, this is not atypical for Malawi, where in our baseline data 93% of 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.

[11] In Table A2, we regress attendance on the travel voucher amount and find that attendance increases with the voucher amount, especially relative to the base case. However, we will show later in the paper that there is no relationship between voucher amount and purchases at the event, despite these differences in attendance.

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

[13] We note that some of our outcomes may be noisily measured as the endline data was collected about 1.5 years after the market access intervention and about a year after the relevant harvest. It is possible, therefore, that some of the effects are attenuated.

[14] In measuring fertilizer use, we found several data errors, such as people reporting market expenditure quantities but no dollar amount. For these, we impute expenditures using the average price of fertilizer.

Table 1
Summary statistics and randomization check.

	(1) Mean (std. dev.)	(2) Coefficient on difference (Treatment - Control)	(3)	(4)
	Control	Cash only	Input fairs only	Cash + Input fairs
Age	40.54	−0.51	−0.07	−0.94
	(14.94)	(0.69)	(0.93)	(0.82)
=1 if female	0.94	0.01	0.01	−0.00
		(0.01)	(0.01)	(0.01)
Years of education	4.87	−0.13	−0.48**	0.04
	(3.35)	(0.19)	(0.22)	(0.25)
Number of household members	4.77	0.11	0.04	0.01
	(2.08)	(0.11)	(0.12)	(0.12)
Total expenditure (last month, USD)	33.84	−1.53	−2.02	0.04
	(28.49)	(1.54)	(1.68)	(1.99)
Value of total assets[a] (USD)	1524.51	−51.27	61.04	102.46
	(2017.90)	(103.00)	(135.97)	(135.77)
=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)	1.27	−0.02	0.02	−0.08
	(1.05)	(0.06)	(0.08)	(0.07)
=1 if used fertilizer	0.83	0.00	−0.01	0.02
		(0.02)	(0.02)	(0.02)
Fertilizer expenditure (USD)	16.37	−0.25	0.21	0.07
	(18.82)	(0.95)	(1.19)	(1.23)

Notes: N = 2784. Dependent variable in rows, each row shows coefficient from a separate regression on respective dependent variable. Standard errors clustered at village level. $* \, p < 0.1, ** \, p < 0.05, *** \, p < 0.01$.
[a] Assets include land and housing, durable goods, livestock, business assets, and financial assets. This variable is winsorized at the 95th percentile.

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 as follows:

$$Y_{iv} = \beta Cash_v + \sum_{j=2}^{4} \alpha_j V_{ij} + \mu X_{iv} + \lambda_s + \varepsilon_{iv} \quad (1)$$

$Cash_v$ is a treatment indicator which takes value 1 if individual i lives in a village v that received cash; V_j are fixed effects for voucher amounts (200, 400, or 600 MWK per km, against the flat 100 MWK payment as baseline); X_{iv} are individual-level controls for education and assets as described below Table 1; and λ_s are strata fixed effects. Finally, because of the importance of FISP, we also present results both pooled (Panel A), and separately by FISP beneficiary status (Panel B). The coefficient of interest is β, which represents the effects of cash on input purchases at the events.

Table 2 shows that those in the cash group were 9 percentage points more likely to buy fertilizer, spent \$3.3 more on fertilizer, and purchased about 6 more kg (all on a very low base in the control group). Conditional on purchase, the average respondent bought a 50 kg bag of NPK or Urea fertilizer (which cost about \$28 during this period).[15]

Panel B shows results by FISP beneficiary status. We see a similar effect of cash in the FISP and non-FISP groups, and observe no effect of FISP on take-up in and of itself. This is consistent with our prior that FISP status was not known at the time of the input fair.[16] It is possible, however, that the upcoming announcement of FISP discouraged purchase for the entire sample since people may have been waiting to purchase via FISP.

[15] Table A4 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.

[16] In Table A5, we regress eventual FISP receipt on the various treatment status indicators and find that those in the input fair only treatment were more likely to have reported receiving FISP. This is likely by chance as there is no reason why FISP allocation should be endogenously impacted by our treatment.

Table 2
Fertilizer purchases at input fair.

	(1) =1 if purchased fertilizer	(2) Expenditure (USD)	(3) Amount (kg)
Panel A. Pooled regression			
Cash (β)	0.09***	3.29***	6.14***
	(0.02)	(0.82)	(1.60)
Control mean	0.02	0.42	0.76
Observations	929	929	929
Panel B. Interaction with FISP beneficiary status			
Cash (β)	0.09***	3.27***	6.26***
	(0.03)	(1.06)	(2.12)
FISP (γ)	−0.01	−0.29	−0.54
	(0.01)	(0.37)	(0.70)
Cash × FISP (δ)	0.01	0.05	−0.32
	(0.03)	(1.25)	(2.39)
p-value:			
$\beta + \delta = 0$	0.014	0.012	0.015
Control mean	0.02	0.50	0.93
Observations	929	929	929

Notes: The sample is restricted to who were offered the input fair 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. $* \, p < 0.1, ** \, p < 0.05, *** \, p < 0.01$.

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. To examine this, we estimate effects in the pooled sample, using the following ANCOVA regression:

$$Y_{iv} = \beta CashOnly_v + \gamma InputFairOnly_v + \delta Cash + InputFair_v + \eta Y_{iv0} + \lambda_s + \theta X_{iv} + \varepsilon_{iv} \quad (2)$$

where $CashOnly_v$ is a treatment indicator which takes value 1 if individual i is in a village which received cash only and 0 otherwise. $InputFairOnly_v$ takes value 1 if individual i belongs to a village which received an input fair only, and 0 otherwise. $Cash + InputFair_v$ takes value 1 if individual i belongs to a village which received both the cash and input fair interventions. Y_{iv0} is the baseline value of the outcome

Table 3
Fertilizer adoption.

	(1)	(2)	(3)	(4)	(5)	(6)
	Total		Market		FISP	
	Expenditure (USD)	Amount (kg)	Expenditure (USD)	Amount (kg)	Expenditure (USD)	Amount (kg)
Cash only (β)	5.42***	11.08***	4.72***	8.29***	0.48	2.71
	(1.15)	(2.68)	(1.16)	(2.28)	(0.46)	(2.38)
Input fair only (γ)	−0.52	0.98	−0.65	−2.17	0.09	3.05
	(1.19)	(3.26)	(1.23)	(2.40)	(0.55)	(2.98)
Cash + Input fair (δ)	8.07***	12.37***	7.43***	11.81***	0.27	0.57
	(1.45)	(3.18)	(1.38)	(2.55)	(0.57)	(2.59)
p-value:						
$\beta = \delta$	0.070	0.679	0.053	0.160	0.704	0.364
Control mean	19.89	69.12	11.22	22.28	8.68	46.84
Control SD	24.57	58.88	24.93	48.82	9.68	47.28
Observations	2784	2784	2784	2784	2784	2784

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. $*\ p < 0.1, **\ p < 0.05, ***\ p < 0.01$.

variable and λ_s are strata fixed effects. $X_i v$ 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. We provide a test for whether $\beta = \delta$ at the bottom of the table.

Results are presented in Table 3. Odd-numbered columns show expenditures while even-numbered columns show quantities. Columns 1–2 show the total usage of fertilizer, summed up over market and FISP fertilizer. Columns 3–6 show results for each type of purchase separately. Note here again that both — beneficiaries (directly) and non-beneficiaries (via sharing) could benefit from FISP.

In Columns 1–2, we see that cash alone increases input usage by about \$5.4 (about 27% on a control mean of \$19.9), and has a similar effect on quantities. Though not our main focus, this result shows a channel between one-time cash transfers and agricultural productivity. By contrast, we find no effect of the input fair alone. Although the prior literature has shown important effects of market access on input usage, including our concurrent work with this same data (Kumar et al., 2022), the intervention to reduce 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.

However, when we examine the combined cash and input fair intervention, we find a sizeable effect of \$8, about \$2.5 larger than cash alone (and about 40% of baseline fertilizer expenditure). The difference between the effect of cash alone, and the combined effect of cash and the input fair, is significant at 10% (with a *p*-value of 0.07). This implies that the combined effect of cash and market access is about 48% larger than the effect of cash alone.

Next, we break down total purchases by market and FISP fertilizer (Columns 3–6). We see that effects (as expected) are driven by market fertilizer. The difference between cash and cash + input fair has a *p*-value of 0.053. The effect on quantities is 3.5 kilos higher in the combined treatment group and is borderline significant with a *p*-value of 0.16. We see no effect of any intervention on FISP purchases.

Despite the effects on expenditures, differences in the measured quantities of fertilizer are statistically no higher in the combined treatment group than for cash-only. There are two reasons for this. First, in Columns 3–4, we see that the ratio of the quantity of fertilizer compared to the expenditure, i.e. the average market price, is slightly higher in the combined group (\$0.63) than in the cash only group (\$0.57). We do not have an explanation for why this would be, and believe the most likely explanation is measurement error, but this causes the difference in quantities to be proportionally less than that in expenditures. Second, in Column 6, the cash only group is more likely to buy FISP fertilizer than the combined group (by 2 kg). While this difference is insignificant, it attenuates the effect on quantities. Ultimately, the most likely explanation for the lack of an effect on quantities is measurement error; nevertheless, we acknowledge that results are weaker for quantities than for expenditures.

Table 4 decomposes the effects shown in Table 3 by FISP beneficiary status. For both beneficiaries and non-beneficiaries, we see statistically significant effects of both cash and cash + input fair, compared to control. The incremental effect of the input fair is positive for both groups, but is only significant at 10% (*p*-value of 0.056 for FISP beneficiaries) and has a *p*-value of 0.151 for non-beneficiaries. As expected, results are driven by market expenditures.

Table 5 shows effects by the specific amount of cash. As expected, we find a monotonic relationship between the amount of cash and take-up. We also see a similar pattern for the incremental effect of the input fair. The difference between cash only and the combined treatment is \$1.4, \$3.1, and \$3.3 for the \$250, \$500, and the \$750 transfers respectively. This result is suggestive that effects of the input fair are larger when there is more cash on hand.

Finally, we turn to studying the impact of increased input use on harvest and production outcomes. We show these results in Table A6. We find large effects of the cash only intervention on the maize harvest (harvest amount and value both go up by 21% relative to the control). We also find that the cash group diversified their crop portfolio, planting 0.1 more crops, a 5% effect. As expected, given the results on input usage, we find that those in the combined treatment did similarly, i.e., there is no incremental effect of the market access treatment.

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 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

Table 4
Fertilizer Adoption, by FISP beneficiary status.

	(1)	(2)	(3)	(4)	(5)	(6)
	Total		Market		FISP	
	Expenditure (USD)	Amount (kg)	Expenditure (USD)	Amount (kg)	Expenditure (USD)	Amount (kg)
Panel A. Non-FISP beneficiaries						
Cash only (β)	6.12***	10.28***	5.64***	9.61***	0.36	0.32
	(1.58)	(3.37)	(1.53)	(2.94)	(0.51)	(2.47)
Input fair only (γ)	−0.46	−0.80	−0.70	−1.56	0.22	0.43
	(1.77)	(4.38)	(1.78)	(3.40)	(0.67)	(3.29)
Cash + Input fair (δ)	8.60***	11.71***	8.60***	14.12***	−0.47	−2.30
	(2.08)	(4.33)	(2.04)	(3.79)	(0.58)	(2.86)
p-value:						
$\beta = \delta$	0.239	0.735	0.151	0.217	0.133	0.323
Control mean	20.65	59.48	15.05	29.62	5.61	29.86
Control SD	27.82	62.35	27.95	54.82	8.72	42.48
Observations	1765	1765	1765	1765	1765	1765
Panel B. FISP beneficiaries						
Cash only (β)	3.89***	12.56***	2.63**	4.95*	0.94	7.53**
	(1.45)	(3.91)	(1.30)	(2.65)	(0.70)	(3.25)
Input fair only (γ)	0.22	1.64	1.06	1.06	−0.90	0.82
	(1.46)	(4.53)	(1.37)	(2.64)	(0.85)	(4.38)
Cash + Input fair (δ)	7.23***	11.39**	6.14***	9.70***	0.88	1.71
	(1.78)	(4.62)	(1.74)	(3.13)	(1.05)	(4.28)
p-value:						
$\beta = \delta$	0.080	0.810	0.056	0.165	0.952	0.155
Control mean	18.48	87.04	4.09	8.64	14.39	78.40
Control SD	16.90	46.90	15.74	30.75	8.74	38.82
Observations	1019	1019	1019	1019	1019	1019

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. $* \ p < 0.1, ** \ p < 0.05, *** \ p < 0.01$.

Table 5
Heterogeneity by size of cash transfer.

	(1) Expenditure (USD)	(2) Amount (kg)
Cash \$250 only (β_1)	2.51*	5.40
	(1.31)	(3.47)
Cash \$500 only (β_2)	6.60***	13.55***
	(1.75)	(3.60)
Cash \$250 only (β_3)	7.15***	14.28***
	(1.70)	(3.92)
Input fair only (γ)	−0.51	0.98
	(1.19)	(3.26)
Cash \$250 + input fair (δ_1)	3.61**	3.26
	(1.66)	(3.59)
Cash \$500 + input fair (δ_2)	9.63***	13.46***
	(2.40)	(5.04)
Cash \$750 + input fair (δ_3)	11.31***	21.39***
	(2.22)	(4.54)
p-values:		
$\beta_1 = \delta_1$	0.537	0.610
$\beta_2 = \delta_2$	0.271	0.987
$\beta_3 = \delta_3$	0.103	0.183
Control mean	19.89	69.12
Control SD	24.57	58.88
Observations	2784	2784

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. $* \ p < 0.1, ** \ p < 0.05, *** \ p < 0.01$.

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, we find that combining 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.

Finally, we wish to draw attention to the fact that fertilizer investments formed only about 4% of household expenditure at baseline, and according to our results in this paper, the marginal propensity to invest in fertilizer out of the total cash transfer amount was only about 1%, which goes up to 1.6% in the combined intervention. Though this increase is small in absolute terms, we argue that providing these (and other similar) investment avenues, however limited, is likely the key to creating durable benefits from cash transfers as at the end of 2 years, less than 1% of the total cash transfer amount was held by households in cash savings (Aggarwal et al., 2022a). A full analysis of all forms of expenditure and investment can be found in that paper.

CRediT authorship contribution statement

Shilpa Aggarwal: Conceptualization, Writing – original draft, Writing – review & editing, Methodology. **Dahyeon Jeong:** Conceptualization, Methodology, Project administration. **Naresh Kumar:** Project administration. **David Sungho Park:** Data curation, Formal analysis, Investigation, Methodology, Project administration. **Jonathan Robinson:** Conceptualization, Funding acquisition, Methodology, Writing – original draft, Writing – review & editing. **Alan Spearot:** Conceptualization, Methodology.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

Data will be made available on request.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jdeveco.2024.103288.

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