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# WORKING PAPER NO. 778

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# *Traders Beyond Borders Incentives to Trade and Microenterprise Performance*

**Paolo Falco[*], Mattea Regina Stein[†], and Eleanor Wiseman[‡]**

**Abstract**

We experimentally investigate the impact of an incentive for crossborder trade on micro-enterprise performance at the Kenya-Uganda border. Through a randomized controlled trial (RCT), we assess the effects of a scalable monetary incentive combined with an information treatment. The monetary incentive has strong positive effects on trade volume, revenue, profit, and savings, which persist beyond the incentive period. We explore the underlying mechanisms driving these results, particularly the role of profit reinvestment, and changes in trading practices. This study contributes critical insights into the potential of targeted interventions to enhance cross-border trade and improve the economic outcomes of micro-entrepreneurs.

**JEL Classification:** C93, D73, F14, H26, L26, Q17.

**Keywords:** cross-border trade, micro-enterprises, subsidy, RCT.

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# 1 Introduction

National borders impose significant frictions on economic activity, and their importance as barriers has grown amid a recent resurgence of protectionist trade policies (Fajgelbaum et al., 2020). Even between neighboring countries with deep economic integration and minimal formal trade barriers, a border can produce sizable price differentials that persist despite opportunities for arbitrage (Gorodnichenko and Tesar, 2009; Gopinath et al., 2011; Wiseman, 2023), suggesting unrealized gains from trade and underlying frictions continuing to limit market integration.

Coupled with the well-documented benefits of international trade for firm development and growth (Bernard et al., 2007; Amiti and Konings, 2007; Atkin et al., 2017), the persistence of such border frictions suggests considerable scope for policy interventions to incentivize cross-border trade.

Policymakers and researchers have placed the spotlight on a range of specific obstacles to cross-border trade, including red tape, information, and capital, but attention has been mostly devoted to studying larger formal firms. Little is known, on the other hand, about the impact of fostering international trade on the smaller businesses that constitute the majority of economic activity in many developing countries.

We address this knowledge gap by conducting a randomized controlled trial (RCT) to study the impact of an incentive-to-trade scheme implemented at the border between Kenya and Uganda. The area is an ideal setting for our intervention: Trade agreements minimize trading costs, yet, price differentials persist between the two sides of the border and are not eroded by trade (Wiseman, 2023). Existing evidence also shows that traders in this area overestimate the risks associated with cross-border trade (Falco and Wiseman, 2024). These observations point to suboptimal trading levels and motivate our experiment.

Our main intervention is a monetary incentive. Traders receive a subsidy up to 4 times over a period of two months, conditional on crossing the border for their trading activities. we additionally test whether layering an information component on top produces incremental effects: a subset of traders also receives information treatment to reduce misperception of the risks of crossing.[1] Our main intervention is scalable: the monetary incentive

[1]Specifically, it shows traders that the main risks associated with crossing the official border (bribes, harassment, goods confiscation, and arrest) are lower than commonly believed.

amounts to a modest sum, and the payments could be feasibly distributed through existing border infrastructure.

We find that the intervention has sizable impacts. The monetary incentive generates an increase of approximately 24% in trade volume detected six months after the end of the treatment, which translates into a 24-26% increase in revenues and profits. The information treatment does not significantly change the impact. These gains translate into meaningful household-level impacts: treated traders save more, and accumulate more household assets, though we do not detect significant impacts on consumption.

Turning to mechanisms, the findings are consistent with a model of business expansion through profit reinvestment, whereby the intervention kick-starts the growth process and allows firms to accumulate working capital, which can sustain higher profits. We also find evidence of broader business upgrading: treated traders become more stable businesses, shift from domestic to cross-border trade, shift to official border crossings, diversify the goods they trade and their supplier base, intensify their activities on a fixed schedule aligned with market days, and update their beliefs.

This paper makes three important contributions. First, we contribute to the literature on interventions to boost international trade by showing that a small, scalable incentive-to-trade can induce sustained trade growth among micro-entrepreneurs. Previous experimental studies have focused on different policy instruments and, typically, on larger formal firms: connecting firms to international markets (Atkin et al., 2017), providing export training (Volpe Martincus and Carballo, 2008), and offering grants to facilitate access to international markets (Ali et al., 2025). The closest precedent to our work with small-scale, often informal traders is Croke et al. (2023), who study a training intervention providing small-scale traders at the DRC-Rwanda border with information on procedures, tariffs, and rights, and find reductions in bribe payments and gender-based violence – though mainly via traders shifting to unofficial early-morning crossings. We differ in two key respects: our main instrument is a direct monetary incentive rather than information, and it induces traders to cross more and to do so through official border posts, generating sustained gains in trade volumes, revenues, and profits.

Second, we make an important contribution to the literature on fostering micro-enterprise growth, a fundamental yet often elusive challenge in the developing world and beyond. Work in this area has focused on a wide

array of constraints to enterprise growth, including lack of capital (de Mel et al., 2008; Fafchamps et al., 2014), limited skills (McKenzie, 2017), and behavioral biases (Caria and Falco, 2024). We show that a modest financial incentive coupled with an easy-to-enforce condition has a strong and sustained impact on firm performance. Though we are unable to disentangle the role of the conditionality from the sheer impact of the cash transfer, existing evidence suggests that a modest unconditional transfer would have been unlikely to generate the sustained impacts we observe (Brudevold-Newman et al., 2024; Crosta et al., 2024). The conditionality is especially salient in our setting where the majority of traders are women: resources reaching female microentrepreneurs may be redirected toward spouses' enterprises or wider household claims rather than reinvested in the woman's own business (Fafchamps et al., 2014; Bernhardt et al., 2019; Riley, 2024; Gazeaud et al., 2023). By tying the transfer to a specific productive use, the conditionality plausibly weakens these intra-household reallocation pressures.

Third, since the businesses in our sample are typically unregistered micro-enterprises and our intervention incentivizes them to cross the border through an official border crossing point – the only way to practically implement the monetary incentive – our findings speak to the literature on the impacts of formalization policies (de Mel et al., 2013; de Andrade et al., 2016; Bruhn, 2011; Benhassine et al., 2018; Campos et al., 2023). We show that our incentive increases the amount of trade through the official border but has little impact on trade through unofficial border crossings. This points to segmentation in the spectrum of trading activities conducted through the two types of borders. On the other hand, we have suggestive evidence that combining the monetary incentive with debiasing information on the risks of trading through the official border may be more effective in reducing informal trade.

# 2 Context and Sampling

Our sample of cross-border traders was selected from 20 open-air marketplaces situated along the Kenya–Uganda border. The sample was concentrated around two main border crossing points: Port Victoria, which serves as a lake-border post on Lake Victoria, and Lwakhakha, which is a land crossing. All selected markets are located on the Kenyan side of the border, specifically within Busia, Bungoma, and Siaya counties. A full list of these

sites is provided in Table A1 in the appendix.

Border communities are strongly dependent on cross-border exchange, and markets in close proximity to crossing points tend to draw both domestic and international traders. These markets sell a wide variety of products, with agricultural produce and fish among the most common. The scale of activity varies widely: some traders operate large trucks supplying distant markets, while others engage in small-scale trade across nearby border markets. Many of these small traders are women, who cross once or several times per week on foot, bicycle, motorbike, or boat. Regional trade agreements in East Africa reduce the costs of trading across the border, and customs duties for small traders are typically minimal. Despite this simplified regime, inefficiency, corrupt procedures, and uncertainty over fees drive a general perception that trading across the border remains risky. Falco and Wiseman (2024) document that the average trader in our sample overestimates these risks, and they tend to especially overestimate the risks of trading through the formal border relative to the risks of trading through the informal border. This motivates the design of our information treatment below.

The sample for this study consists of 464 traders randomly selected among those operating in the aforementioned markets. We defined traders as someone who buys goods in markets/locations that are different from where they sell the goods. We limited the sample to active cross-border traders, i.e., traders who had engaged in cross-border trade in the past 4 weeks and were planning on engaging in cross-border trade in the next 4 weeks.[2] Starting with an initial census of all traders in the surveyed markets, we randomly selected subjects for the intervention in two phases, November 2023 and August 2024, using the same criteria and method: the first recruitment phase included 357 traders, and the second recruitment phase increased the sample by 107 traders. Sampling was carried out on weekdays when the market was busiest. The randomization is described below.

Table 1 provides a description of our sample of traders. Women make up 81% of the sample. This female majority is a common pattern

[2]We decided to focus on traders who were already engaging in cross-border trade for conceptual and practical reasons. From a conceptual perspective, we intended to cleanly isolate the effects of our intervention on the intensive margins of trade behavior (intensity, choice of products), as opposed to confounding both the intensive and the extensive margin (whether to trade internationally in the first place). From a practical perspective, prior experience in the area indicated that our intervention may have been underpowered to detect effects on the extensive margin.

among traders in East Africa and makes our setting well-suited to study female micro-entrepreneurs, a population of special interest to academic research and policymakers. The average trader is relatively senior (the mean age is close to 43), and the vast majority of traders are Kenyan nationals. 70% of traders import goods from Uganda to Kenya, 17% are exporters, and 13% are both importers and exporters. 81% of the traders in our sample engage in some domestic trade in addition to cross-border trade.

Virtually all traders in our sample own their trading business and consider trading as the main source of income. 87% of traders are also farmers, and 63% of our sample traders sell the products they grow themselves. The businesses are small, with the average number of employees below 1. The majority of traders transport the goods themselves across the border. This is consistent with our priors about this sector being largely populated by small businesses that do not rely on hired transport services. Traders average hyperbolic-sine-transformed revenue is 10.8, which corresponds to approximately 25,500 Kshs (around 200 USD) and average transformed revenue is 9.4, or about 6,000 Kshs (46 USD) in profits per week. The last two columns of Table 1 shows that baseline characteristics are balanced between the two treatment groups and the control group.

# 3 Experimental Design

## 3.1 Intervention

Our main treatment was a monetary incentive: We subsidized up to four trading trips over two months with a fixed lump-sum transfer of 2,000 KSh per trip (approx. USD 15), amounting to 33% of average transformed weekly profits for the traders involved. The condition for receiving the transfer was crossing the border for a trading trip. Program officers verified that the condition was met by meeting traders at the border, and disbursed the incentive on the spot.[3] The intervention is, therefore, akin to a subsidy that could be disbursed by public officials working at the border, and it offers direct insights into an easily implementable and scalable design.

[3]We implemented this intervention at two *official* border crossings. Disbursing incentives at *unofficial* crossings would have been impractical (traders in the markets we surveyed use only these two official crossings, but many unofficial ones), and incentivizing, de facto, informal trade would have posed legal issues.

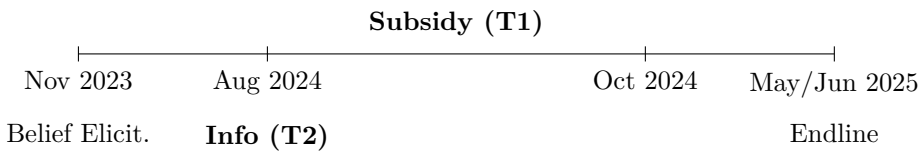
Table 1: Baseline summary statistics & balance

| | Mean | SD | Min | Max | C - T1 | C - T2 |
|---|---|---|---|---|---|---|
| ***Individual characteristics*** | | | | | | |
| Age | 43.389 | 10.991 | 19 | 73 | -1.544 | -0.210 |
| Male | 0.191 | 0.393 | 0 | 1 | 0.001 | 0.048 |
| HH size | 6.858 | 3.205 | 1 | 20 | -0.322 | 0.244 |
| ***Trade Type*** | | | | | | |
| Importer | 0.704 | 0.457 | 0 | 1 | 0.052 | -0.023 |
| Exporter | 0.166 | 0.373 | 0 | 1 | 0.006 | 0.005 |
| Both importer and exporter | 0.130 | 0.336 | 0 | 1 | -0.059 | 0.018 |
| Some domestic trade in past 12 months | 0.809 | 0.393 | 0 | 1 | 0.018 | -0.079 |
| ***Trader Characteristics*** | | | | | | |
| Owns trading business | 0.995 | 0.070 | 0 | 1 | -0.000 | -0.007 |
| Trading is main source of income | 0.983 | 0.130 | 0 | 1 | -0.007 | -0.026 |
| Is a farmer | 0.870 | 0.336 | 0 | 1 | 0.052 | -0.008 |
| ***Trading business outcomes*** | | | | | | |
| N workers in past 30 days | 0.528 | 1.820 | 0 | 30 | 0.266 | 0.314 |
| Monthly revenue (past year) [IHS] | 10.838 | 1.202 | 7 | 14 | 0.023 | 0.160 |
| Monthly profit (past year) [IHS] | 9.393 | 1.241 | 2 | 12 | 0.126 | 0.019 |

Monetary values in Kshs and transformed (IHS). Data collected at Baseline. The last two columns show the difference in means between control and the two treatments and t-test results. ***, **, and * indicate significance at the 1, 5, and 10 percent critical value of t-tests.

One treatment group (T1) received only the monetary incentive, while another (T2) combined the monetary incentive with an additional information intervention. Based on prior work by Falco and Wiseman (2024), we had evidence that traders overestimate the risks associated with crossing the border. Specifically, we measured key perceived risks of incurring bribes, harassment, confiscation of goods, and arrest, and we found that traders tend to overestimate the likelihood of such events occurring. These estimates were validated through a range of methods, including sending decoy traders through the border in order to obtain an unbiased assessment of actual risks. Moreover, the results showed that traders tend to overestimate the likelihood that such risks occur when crossing the official border *relative to* using unofficial crossings. Since our intervention was designed to be conducted at the official border, we hypothesized that such biases may limit

Figure 1: Timeline




the impact of the treatment, and we chose to test this explicitly through an additional debiasing intervention aimed at correcting misperceptions. By means of easy-to-understand info cards, our surveyor informed subjects in this treatment group about the gap between people's perceived risks and actual risks. Figure A1 shows the info card on the risk of paying bribes. Similar cards were used for the risks of harassment, confiscation, and arrest. This additional treatment occurred at the start of the subsidy period. Subjects in T2, therefore, received the information treatment prior to receiving the first monetary transfer. Subjects in the control group received no treatment. The surveyed markets are sufficiently large relative to the size of our samples to suggest that spillover effects are unlikely. Setting up the study to be able to test for such effects formally was beyond our available resources.

Figure 1 shows the full timeline of the project. The belief elicitation used to produce the information treatment took place in November 2023 and is documented in Falco and Wiseman (2024). The information treatment was administered in August 2024, at the beginning of the subsidy period, which ended in late October 2024. The endline took place in June 2025, six months after the subsidy had ended.

## 3.2 Randomization, Baseline, and Endline Surveys

Participants were randomly selected through a random walk strategy in the sampled markets conducted in November 2023. We assigned 179 traders to the control group, 178 to T1, and 107 to T2. We stratified the random assignment by border location (Lake Victoria and Lwakhakha) and trader gender.

We conducted the baseline survey in two phases. Phase I was conducted in November 2023 and covered the control group and T1. Phase II was conducted in August 2024 and covered T2. The lag between the two phases was due to a delay in funding availability for T2. It is important to

note, however, that no treatment took place before the second-phase baseline (for T2).

The endline survey took place at the same time for *all* subjects between May and June 2025.[4]

### 3.3 High-Frequency Data

From August 2024 (start of the treatments) to the June 2024 (endline), we conducted several rounds of short phone interviews in addition to the in-person baseline and endline surveys. We conducted five rounds of high-frequency surveys: a first round just before the start of the subsidy period in August 2024, two rounds during the subsidy period (September and October 2024), and two rounds between the end of the subsidy period and the endline survey (December 2024 - February 2025). This rich data allows us to assess the trajectory of impacts over time.

## 4 Results

### 4.1 Estimation Strategy

When baseline values of the dependent variable are available, we estimate a standard ANCOVA model:

$$y_{i1} = \alpha + \beta T_i + \gamma y_{i0} + \varepsilon_i, \tag{1}$$

where $y_{i1}$ is the dependent variable for trader $i$ at endline, $T_i$ is a treatment dummy and $y_{i0}$ is the baseline value of the dependent variable.[5]. The treatment dummy captures assignment to treatment, and we estimate intention-to-treat (ITT) effects. When baseline values of the dependent variable are not available, we estimate the equivalent model omitting $y_{i0}$. We transform

[4]The data collection was administered by IPA Kenya using SurveyCTO. The data collected was subjected to regular high-frequency quality checks, and back-checks were conducted for 10% of all completed surveys. When quality checks flagged missing variables or inconsistencies, callbacks were made and corrections implemented promptly.

[5]Our main specifications pool T1 and T2 in order to maximize statistical power, as the monetary transfer – which both received – was the main intervention and we find no statistically significant additional effect of the information treatment received by T2. Appendix tables show disaggregated results. Results are robust to including strata fixed effects (see Appendix).

monetary amounts using a standard inverse hyperbolic sine transformation and use robust standard errors.

## 4.2 Treatment Impacts on Business Outcomes

We estimate our main treatment impacts on two sets of outcomes. First, we focus on changes in trade volume as a result of the treatment (number of cross-border trading trips and average volume per trip). Second, we estimate impacts on key business outcomes: revenue and profit.

We find that the monetary incentive significantly increases cross-border trade volume (Table 2). Treated subjects conduct 1.21 more trading trips over the previous four weeks, relative to a control group average of approximately 4.96 trips. This is an increase of 24%.[6] The value of goods traded per trip is unchanged, indicating that the intervention primarily affects the extensive margin of conducting additional trips. We also find that increased trade volumes translate into large and statistically significant effects on revenue and profit, which increased by 26% and 24% respectively.

Overall, these results point to the conclusion that our treatment enabled traders to expand their business: they undertook more cross-border trading trips, generating higher revenue and profit. Their businesses scaled, in fact, roughly proportionally, given similar percentage increases in trips,

[6]When we disaggregate the two treatments, both point estimates are statistically significant and that for T2 is somewhat larger than for T1, but the difference is not statistically significant (see Appendix Table A2).

revenues, and profits.

Table 2: Treatment Effects on Trade Volume and Business Outcomes

| | (1) N Trips | (2) Trip Value | (3) Revenues (IHS) | (4) Profits (IHS) |
|---|---|---|---|---|
| Treatment | 1.207** | 0.071 | 0.264*** | 0.235** |
| | (0.499) | (0.112) | (0.094) | (0.111) |
| | [0.016] | [0.524] | [0.005] | [0.036] |
| Dep Var | | | | |
| Mean (C) | 4.961 | 9.368 | 9.569 | 8.024 |
| Baseline Control | | X | X | X |
| Observations | 409 | 374 | 373 | 371 |

Note: Standard errors robust (reported in parentheses, p-values reported in brackets). * p<0.1, ** p<0.05, *** p<0.01.

## 4.3 Treatment Impacts on Asset Accumulation and Household Outcomes

We next investigate whether our treatments impacted working capital accumulation and key household outcomes. Since the intervention allowed businesses to expand and become more profitable, one may expect this to translate into greater working capital accumulation and greater consumption, asset accumulation, or savings for the households of treated traders. We test these hypotheses in Table 3. In line with our hypotheses, we find that treated traders are able to save more money (Panel A, Columns 2 and 3) and detect suggestive evidence of an impact on the value of their stock (Panel A, Colum 1, p-value 0.104) – which is their key working capital. Impacts also extend to the household, as traders accumulate substantially more household assets (Panel B, Columns 4 and 5).[7] We do not detect, on the other hand, statistically significant impacts on household consumption (Panel C).

[7]We detect no impacts on agricultural assets (Panel B, Columns 6 and 7), in line with our finding below in Section 5.3 of no impact on traders' agricultural activities.

Table 3: Impacts on Savings and Consumption

***Panel A: Working capital and savings***

| | (1) Stock value (ihs) | (2) Saved money | (3) Savings amount (ihs) |
|---|---|---|---|
| Treatment | 0.602 | 0.088* | 0.833* |
| | (0.370) | (0.051) | (0.478) |
| | [0.104] | [0.085] | [0.082] |
| Dep Var Mean (C) | 7.761 | 0.510 | 4.657 |
| Observations | 409 | 409 | 409 |

***Panel B: Assets***

| | (4) Bought HH assets | (5) HH assets amount | (6) Bought AG assets | (7) AG assets amount |
|---|---|---|---|---|
| Treatment | 0.094* | 0.794* | -0.010 | -0.091 |
| | (0.049) | (0.449) | (0.040) | (0.378) |
| | [0.058] | [0.078] | [0.811] | [0.811] |
| Dep Var Mean (C) | 0.340 | 3.054 | 0.810 | 7.265 |
| Observations | 409 | 409 | 409 | 409 |

***Panel C: Consumption***

| | (8) HH expenditures (ihs) | (9) Food expenditure (ihs) | (10) Special food exp. (ihs) | (11) Days eaten meat |
|---|---|---|---|---|
| Treatment | 0.126 | -0.066 | 0.391 | -0.106 |
| | (0.102) | (0.115) | (0.396) | (0.376) |
| | [0.217] | [0.566] | [0.323] | [0.777] |
| Dep Var Mean (C) | 10.52 | 9.642 | 3.617 | 2.556 |
| Observations | 409 | 409 | 409 | 409 |

Note: Standard errors robust (reported in parentheses, p-values reported in brackets). * p<0.1, ** p<0.05, *** p<0.01.

# 5 Mechanisms

Having documented the main treatment impacts, in this section, we explore the mechanisms that underlie the treatment effects. First, we focus on a classic mechanism based on capital constraints, which we investigate through the lens of a simple theoretical model of business growth through profit reinvestment. Using our high-frequency data, we show that the impact trajectories are consistent with this framework. Second, we investigate whether our treatment fostered changes in the way traders conduct their business. Evidence of such changes (*business upgrading*) would support the idea that our treatment induced deeper transformation that may help sustain the gains in profitability we detected.

## 5.1 Profit Reinvestment

We begin by exploring a classic channel based on capital constraints. We do so by first presenting a simple model to guide our thinking. The model formalizes how traders who lack access to savings and credit must finance their operations out of current cash flow, and how temporary incentives like the one we provided may affect the evolution of trade volume over time.

### 5.1.1 Setup and Assumptions

Each period $t$, a trader chooses a trade volume $q_t$. Trade is financed entirely out of profits plus external transfers—there is no credit or outside savings. This is a good approximation for the context of our experiment, as suggested by Figure A2 and A3, which show that lack of capital is a major constraint to business expansion. Per-unit revenue is $r$, per-unit cost is $c$, and treated traders receive a lump-sum subsidy $S$ in each period when our intervention is active.[8] Profit in period $t$ is then

$$\pi_t = (r - c)\, q_t + S. \tag{2}$$

[8]Though the incentive we provide is proportional to the number of trading trips undertaken (a fixed lump-sum per trip), we cap it to a maximum of one subsidized trip every two weeks and to four subsidized trips in total. It is therefore reasonable to model it as a fixed sum per period, where each period may approximately correspond to two weeks over which a single transfer is received.

A share $\lambda_t \in (0,1)$ of profits is reinvested into the business, increasing the next period's trade volume (the remaining share $(1-\lambda)$ is consumed):

$$q_{t+1} = q_t + \lambda_t \pi_t \tag{3}$$

$$= q_t + \lambda_t \Big[ (r-c)\, q_t + S \Big]. \tag{4}$$

This setup captures the idea that many informal traders roll over profits to finance the next round of trading. The subsidy $S$ enters as a liquidity infusion.

Importantly, the reinvestment rate $\lambda_t$ is allowed to vary between periods. This captures the possibility that our treatment may induce traders to temporarily reinvest a larger share of available resources into the business. The reason is that the conditionality of our subsidy on conducting cross-border trade may have nudged respondents to allocate the subsidy to business purposes. We find support for this idea: participants overwhelmingly report investing the subsidy in their business (Figure A4) and they are significantly more likely to use the subsidy for business purposes than to reinvest general windfall profits (Figure A5).

### 5.1.2 Predicted Dynamics

Starting from equation (4),

$$q_{t+1} = q_t + \lambda_t \Big[ (r-c)\, q_t + S \Big] \tag{5}$$

$$= \Big[ 1 + \lambda_t (r-c) \Big] q_t + \lambda_t S. \tag{6}$$

Let

$$a_t = 1 + \lambda_t (r-c)$$

be the *growth factor*. Solving recursively for a given $a_t = a$ yields

$$q_t = a^t q_0 + \lambda_t S \sum_{j=0}^{t-1} a^j, \qquad t \geq 1. \tag{7}$$

Summing the geometric series gives the closed form

$$q_t = a^t q_0 + \lambda_t S \, \frac{a^t - 1}{a - 1}.^{9} \tag{9}$$

[9] If $a = 1$ (i.e. $\lambda_t (r-c) = 0$), the path is linear:

$$q_t = q_0 + t\, \lambda S. \tag{9}$$

Since $\lambda_t$ varies over time, so does $a_t$, and the model allows for different growth regimes. When the share of available resources that gets reinvested is relatively high, trade grows fast. When the reinvestment rate decreases, trade growth slows down. If our experiment induces higher reinvestment rates during the initial phase when the conditional incentive is available, we should observe a period of rapid initial expansion. As reinvestment rates fall once the incentive is phased out, the growth rate of treated businesses may slow down. The implication is that treatment effects should be largest in the first months of the study period. As the incentive is phased out, firms continue to reinvest their profits, but they may do so at a lower rate, and treatment impacts may become lower. Using high-frequency data collected over the course of the study period, we can test whether the impact trajectory we observe is consistent with these predictions.

## 5.2 Estimated Impact Trajectories

Our high-frequency data allows us to study the trajectories of impacts over the course of the study period. Subjects responded to a brief monthly survey conducted over the phone. The first phone call (Round 1) took place at the start of the subsidy period (August 2024), before subjects started to receive the monetary incentive, but after those in T2 had received the information treatment (which was administered in person at the end of the baseline interview). This implies that a comparison between control and T1 in Round 1 should show no impact, while a comparison of T2 and control in Round 1 may, in principle, show the immediate impact of the information treatment on its own. The time-horizon is, however, too short between the baseline (early August) and the first round phone call (late August) to expect a significant effect. Round 2 (September 2024) and Round 3 (October 2024) were conducted during the subsidy period. Round 4 (November 2024) and Round (January/February 2025) were conducted after the subsidy period ended and before the in-person endline (May/June 2025).

Table 4 shows that the monetary incentive had immediate impacts on the volume of trade (number of trading trips over the past four weeks). Treatment effects appear in Round 2 as soon as the subsidy becomes available, and they persist until the endline. Consistent with the prediction of

the model we see a slight (insignificant) decline in the effect.[10]

Table 5 shows a similar pattern for revenues, which is also in line with the model. The effect of the treatment is strongest when the subsidy is available and the rate of profit reinvestment is highest. Once the subsidy is removed, the impact decreases, but remains significant at endline.[11] Table A6 shows a similar pattern for profits.

Table 4: Treatment Effects on Trade Volume, High Frequency Data

| | (1) Round 1 | (2) Round 2-3 | (3) Round 4-5 | (4) Endline |
|---|---|---|---|---|
| Treatment | -0.377 | 1.708*** | 1.561*** | 1.207** |
| | (0.478) | (0.451) | (0.472) | (0.499) |
| | [0.431] | [0.000] | [0.001] | [0.016] |
| Dep Var Mean (Control) | 5.291 | 4.385 | 5.041 | 4.961 |
| Round FE | | X | X | |
| R-Squared | .001 | .03 | .023 | .014 |
| Observations | 409 | 778 | 764 | 409 |

Note: Standard errors robust (reported in parentheses, clustered at trader level for rounds 2-5; p-values reported in brackets). * p<0.1, ** p<0.05, *** p<0.01.

[10] When we disaggregate the treatments, no such decline appears for T2, where the additional information treatment may have further bolstered the effect and compensated for the drop due to the incentive being phased out, see Appendix Table A4.

[11] When we disaggregate the treatments, we observe again that the impact of T2 remains more stable, see Appendix Table A5.

Table 5: Treatment Effects on Revenues, High Frequency Data

| | (1) Round 1 | (2) Round 2-3 | (3) Round 4-5 | (4) Endline |
|---|---|---|---|---|
| Treatment | 0.078 | 0.354*** | 0.414*** | 0.264*** |
| | (0.096) | (0.081) | (0.094) | (0.094) |
| | [0.416] | [0.000] | [0.000] | [0.005] |
| Dep Var Mean (Control) | 9.683 | 9.483 | 9.489 | 9.569 |
| Round FE | | X | X | |
| Baseline Control | X | X | X | X |
| R-Squared | .259 | .281 | .256 | .217 |
| Observations | 389 | 713 | 678 | 373 |

Note: Standard errors robust (reported in parentheses, clustered at trader level for rounds 2-5; p-values reported in brackets). * p<0.1, ** p<0.05, *** p<0.01.

## 5.3 Business Upgrading: Changes in Trading Practices

Our intervention may have affected businesses not only by relaxing capital constraints and increasing trade volumes, but also by inducing broader business transformation. Greater engagement in cross-border trade may generate learning, experimentation, and changes in business practices. In what follows, we explore these additional channels of impact. Because most of these outcomes were not pre-registered, we interpret the evidence as exploratory.

We begin by examining broader changes in business activity, including business stability, the balance between domestic and cross-border trade, participation in official versus unofficial trade, and labor allocation (Table 6). We then turn to more specific changes in cross-border trading practices, focusing on sourcing, border-crossing strategies, and selling behavior (Table 7). Finally, we study whether traders update their beliefs about the profitability of cross-border trade (Table 8).

The evidence points to substantial business upgrading among treated firms. Table 6, Panel A, shows that treated businesses become more stable and increasingly specialize in cross-border trade. First, treated traders are 5.4 percentage points more likely to report having actively traded during the previous four weeks over the study period (Column 1). In a sector characterized by strong seasonality, this increase in business continuity suggests

greater business stability.

Second, treated traders substitute away from domestic trade and toward cross-border trade. Traders in our sample commonly engage in both activities, yet treated firms are 11.8 percentage points less likely to have conducted domestic trade in the previous four weeks and 7.8 percentage points more likely to have conducted cross-border trade (Columns 2 and 3). This pattern suggests that once capital constraints on cross-border trade are relaxed, traders reallocate activity toward the more profitable cross-border margin. Consistent with this interpretation, the probability of operating as both an importer and an exporter – rather than trading in only one direction – almost doubles, increasing by 4.1 percentage points (Column 4).

We also examine whether the intervention affects the use of official versus unofficial border crossings. Conducting trade through official channels may itself represent a form of business upgrading in our setting. Table 6, Panel B, provides suggestive evidence that the treatment reduces reliance on unofficial crossings: the share of trips conducted unofficially falls by 5.2 percentage points relative to a control-group mean of 13.3% (Column 8; p-value 0.144).[12] Overall, however, the intervention mainly increases official trade while leaving unofficial trade largely unchanged (Columns 6 and 7). One possible explanation is that unofficial trade was already relatively uncommon in our sample, leaving limited scope for further reductions.

The increased focus on cross-border trade is also reflected in labor allocation. Table 6, Panel C, shows a decline in hours worked in activities other than trading and agriculture (Column 11).[13] By contrast, we find no effect on total hours worked in trading overall (Column 9), consistent with traders reallocating effort within trading activities from domestic to cross-border trade rather than increasing total labor supply.[14] We also find no evidence of a shift away from agriculture (Column 10).

[12]When separating the treatments, this effect is driven entirely by T2, consistent with the fact that the additional information treatment explicitly emphasized the official border, see Appendix Table A7.

[13]Given the small number of hours involved, these are likely low-profitability activities undertaken to "make ends meet".

[14]It was not possible to separately measure hours devoted to domestic versus cross-border trade because selling activities for domestically sourced and imported goods typically occur simultaneously.

Table 6: Changes to the business overall

| ***Panel A: Business overall*** | | | | |
|---|---|---|---|---|
| | (1) Business stability | (2) Domestic trade | (3) CB trade | (4) Both imp and exp |
| Treatment | 0.054** | -0.118*** | 0.078* | 0.041* |
| | (0.021) | (0.042) | (0.040) | (0.025) |
| | [0.011] | [0.005] | [0.051] | [0.091] |
| Dep Var Mean (C) | 0.895 | 0.755 | 0.806 | 0.0458 |
| Baseline Control | | X | X | X |
| Observations | 409 | 381 | 380 | 407 |
| ***Panel B: Official vs. unofficial trade*** | | | | |
| | (5) N trips | (6) N off. trips | (7) N unoff. trips | (8) Share unoff. |
| Treatment | 1.207** | 1.294** | -0.088 | -0.052 |
| | (0.499) | (0.520) | (0.190) | (0.036) |
| | [0.016] | [0.013] | [0.645] | [0.144] |
| Dep Var Mean (C) | 4.961 | 4.527 | 0.541 | 0.133 |
| Baseline Control | | X | X | X |
| Observations | 409 | 392 | 389 | 316 |
| ***Panel C: Labor allocation – hours worked*** | | | | |
| | (9) Trading | (10) Agriculture | (11) Other | (12) Total |
| Treatment | 3.252 | -5.289 | -8.254** | -10.291 |
| | (10.904) | (5.753) | (3.615) | (11.365) |
| | [0.766] | [0.359] | [0.023] | [0.366] |
| Dep Var Mean (C) | 164.2 | 60.58 | 17.66 | 242.5 |
| Observations | 409 | 409 | 409 | 409 |

Note: Business stability is the share of survey rounds (2-5, endline) during which the trader indicated the business being active (past 4 weeks). Domestic trade and CB trade are dummies indicating whether the trader conducted any domestic and cross-border trade in the past 4 weeks. Both imp and exp is a dummy indicating that the trader is both an importer and an exporter (as opposed to one-way trading). Standard errors robust (reported in parentheses, p-values reported in brackets). * p<0.1, ** p<0.05, *** p<0.01.

We next examine how practices specific to cross-border trading change (Table 7)[15]. The intervention appears to induce greater experimentation and diversification in the sourcing of goods (the buying side). Panel A shows that treated traders increase the number of goods traded cross-border as well as the number of suppliers they source from within their buying markets (Columns 1–4). Thus, treated traders not only trade more, but also diversify both products and sourcing networks.

Turning to how traders cross the border, Panel B provides suggestive evidence that treated traders experiment with new border crossings (Column 5; p-value 0.118). By contrast, we find no evidence of experimentation with new transport modes. The likelihood of hiring a transporter is unchanged, while the number of transporters hired in the past four weeks increases by 17.3%, closely matching the increase in the number of cross-border trips in the same timeframe. Combined with the absence of effects on trade volumes per trip (Table 2), this suggests that traders expand trade primarily by making more trips using existing transport technologies, while potentially varying the crossings they use.

On the selling side, the evidence suggests intensification and stabilization rather than geographic expansion. Panel C of Table 7 and Appendix Table A8 show that treated traders increase the frequency and regularity of trade within their main market, rather than entering new markets or actively searching for new customers. Markets in our setting typically operate two days per week, and treated traders appear to adapt their trading schedule accordingly. The increase in total trips is driven primarily by a rise in trading exactly twice per week (Appendix Table A8).[16] Consistent with this pattern, treated traders are substantially more likely to report following a fixed crossing schedule rather than varying crossing days week to week (Column 10). At the same time, we find no increase in the number of markets served (Column 8), no increase in selling days in the main market (Column 9), and no effect on efforts to attract new customers (Column 11). Business upgrading therefore appears to take the form of greater intensity, regularity, and coordination within established trading relationships rather than market expansion.

[15]Where possible, we pool high-frequency survey rounds 2-5 and endline data to increase power and better capture experimentation. Endline-only specifications are used when outcomes were not measured in earlier rounds.

[16]Appendix Figure A2 further supports this interpretation: 38% of traders cite market schedules as a major reason for not undertaking more trips.

Finally, Table 8 provides evidence of learning and belief updating. Treated traders report expecting to undertake one additional profitable trading trip per week relative to the control group (Column 1). This closely mirrors the estimated treatment effect on actual trade volume, suggesting that the intervention changes not only behavior but also traders' beliefs about the returns to cross-border trade.

Table 7: Cross-border trade: Changes to buying, crossing, selling practices

| ***Panel A: Buying*** | | | | |
|---|---|---|---|---|
| | (1) N new CB goods | (2) N CB goods | (3) New buy market | (4) N suppl (log) |
| Treatment | 0.283* | 0.587*** | -0.024 | 0.159** |
| | (0.170) | (0.215) | (0.029) | (0.067) |
| | [0.096] | [0.007] | [0.406] | [0.018] |
| Dep Var Mean (C) | 1.132 | 2.238 | 0.921 | 1.159 |
| Observations | 1952 | 1952 | 404 | 409 |

| ***Panel B: Crossing*** | | | |
|---|---|---|---|
| | (5) N new crossings | (6) Hired transp. | (7) N transp. |
| Treatment | 0.039 | 0.002 | 0.173* |
| | (0.025) | (0.051) | (0.088) |
| | [0.118] | [0.971] | [0.051] |
| Dep Var Mean (C) | 0.108 | 0.529 | 0.695 |
| Observations | 1952 | 409 | 409 |

| ***Panel C: Selling*** | | | | |
|---|---|---|---|---|
| | (8) New sell market | (9) N sell days | (10) Fixed CBT days | (11) New customers |
| Treatment | 0.017 | -0.131 | 0.143*** | 0.007 |
| | (0.051) | (0.219) | (0.052) | (0.028) |
| | [0.740] | [0.550] | [0.006] | [0.808] |
| Dep Var Mean (C) | 0.412 | 3.953 | 0.381 | 0.915 |
| Observations | 405 | 402 | 381 | 409 |

Note: Outcomes are pooled across survey rounds (2-5, endline) where possible (Columns 1, 2, 5; round fixed effects included). N CB goods is the number of goods traded cross-border. N suppl is the number of suppliers. Transp. stands for transporters. N sell days is the number of selling days in the main market. Fixed CBT days is a dummy indicating that cross-border trade is conducted on a fixed schedule as opposed to changing week-to-week. New customers is a dummy. Standard errors robust, clustered at the trader level for pooled outcomes (Columns 1, 2, 4, 7) (reported in parentheses, p-values reported in brackets). * p<0.1, ** p<0.05, *** p<0.01.

Table 8: Beliefs updating

| | (1) Expected no. of profitable trips | (2) Plans to do more trips | (3) Official crossing more costly | (4) Official more risky | (5) Min profit expected per trip | (6) Max profit expected per trip |
|---|---|---|---|---|---|---|
| Treatment | 0.992* | 0.007 | -0.087** | -0.063 | 0.070 | 0.033 |
| | (0.526) | (0.068) | (0.039) | (0.039) | (0.123) | (0.115) |
| | [0.061] | [0.919] | [0.028] | [0.102] | [0.566] | [0.773] |
| Dep Var | | | | | | |
| Mean (C) | 5.118 | 0.459 | 0.216 | 0.196 | 7.406 | 8.311 |
| Observations | 231 | 231 | 409 | 409 | 409 | 409 |

Note: Standard errors robust (reported in parentheses, p-values reported in brackets). * p<0.1, ** p<0.05, *** p<0.01.

# 6 Welfare Analysis

This section evaluates the social desirability of the incentive-to-trade program using a simple cost–benefit framework. We show that both our interventions are highly cost-effective.

We focus on three components of net welfare:

$$\Delta W = \underbrace{\Delta\Pi}_{\text{trader surplus}} + \underbrace{\Delta R}_{\text{fiscal effects}} - \underbrace{(1+\phi)\,C}_{\text{program resource cost}}, \tag{10}$$

where $\Delta\Pi$ is the change in traders' profits (producer surplus), $\Delta R$ captures fiscal externalities (e.g., customs/VAT from a shift toward official trade), $C$ is the program's direct implementation cost, and $\phi \geq 0$ is the shadow cost of public funds (i.e., welfare loss that occurs due to the economic distortions caused by raising tax revenue). Throughout, we report conservative estimates by (i) treating transfers as a resource cost to the public sector (rather than neutral within a consolidated budget) and (ii) setting $\Delta R = 0$ to conservatively assume that the additional trade generated by the treatment does not result in additional custom liabilities, which are typically minimal for small traders.[17]

[17] In reality, this is likely to be positive, albeit low, and constitutes a potential upside, as discussed below.

## 6.1 Costs

T1 provides a lump-sum subsidy of 2,000 KSh per verified trading trip for up to four trips over ≈2.5 months; thus

$$C_{\text{transfers}} = 8{,}000 \text{ KSh per treated trader.}$$

Let $C_{\text{admin}}$ denote per-trader administrative costs (verification at border and disbursement). Our conservative program cost is

$$C \;=\; C_{\text{transfers}} + C_{\text{admin}} \;=\; 8{,}000 + C_{\text{admin}} \quad \text{(KSh)}.$$

We report benefit–cost ratios (BCRs) for a range of $C_{\text{admin}} \in [0, 2{,}000]$ KSh and $\phi \in \{0, 0.2\}$.

## 6.2 Benefits to Traders

Endline estimates (Table 2) show sizable profit gains: profits increase by 24% relative to control. Control group weekly profits average 2,475 KSh. Therefore,

$$\text{weekly profit gain } g_{\text{T}} \;=\; 0.24 \times 2{,}475 \;=\; 594 \text{ (KSh.)}$$

We aggregate gains over two horizons: during-treatment (10 weeks) and post-treatment through endline (26 weeks). Because the high-frequency trajectory suggests persistence with some attenuation, we report a sensitivity parameter $\theta \in [0, 1]$ describing the average fraction of the endline gain that persists each week in the post period. The total per-trader profit gain is

$$\Delta\Pi_{\text{T}}(\theta) \;=\; \underbrace{10\, g_{\text{T}}}_{\text{treatment window}} \;+\; \underbrace{26\, \theta\, g_{\text{T}}}_{\text{post window}} \;=\; 5{,}940 \;+\; 15{,}444\, \theta \quad \text{(KSh)}. \tag{11}$$

We emphasize that these are *intention-to-treat* gains and already net of any average business-as-usual evolution.

## 6.3 Benefit–Cost Ratios

Define the benefit–cost ratio as

$$\text{BCR} \;=\; \frac{\Delta\Pi + \Delta R}{(1+\phi)C}.$$

Table 9 reports BCRs under alternative persistence ($\theta$), administrative costs ($C_{\text{admin}}$), and shadow costs ($\phi$), setting $\Delta R = 0$.

Table 9: Benefit–Cost Ratios (BCR) Under Alternative Assumptions

| Assumptions | T (profit +24%) | |
|---|---|---|
| | $\phi = 0$ | $\phi = 0.2$ |
| $\theta = 1$, $C_{\text{admin}} = 0$ KSh | 2.67 | 2.23 |
| $\theta = 0.5$, $C_{\text{admin}} = 0$ KSh | 1.71 | 1.42 |
| $\theta = 0.33$, $C_{\text{admin}} = 0$ KSh | 1.38 | 1.15 |
| $\theta = 1$, $C_{\text{admin}} = 2{,}000$ KSh | 2.14 | 1.78 |
| $\theta = 0.5$, $C_{\text{admin}} = 2{,}000$ KSh | 1.37 | 1.14 |
| $\theta = 0.33$, $C_{\text{admin}} = 2{,}000$ KSh | 1.10 | 0.92 |

Notes: BCR $= (\Delta\Pi)/((1+\phi)C)$ with $\Delta R = 0$. Costs $C = 8{,}000 + C_{\text{admin}}$ KSh. Benefits $\Delta\Pi$ from (11). $\theta$ is the average share of the endline profit gain that persists each post-treatment week over 26 weeks. Profits are weekly; control group mean is 2,475 KSh.

**Interpretation.** Even if only a third of the endline profit gain persists on average during the six months post-treatment ($\theta = 0.33$), the program remains welfare-improving (BCR $> 1$) across all configurations except the most pessimistic case, which combines substantial administrative costs with a 20% shadow cost of public funds (BCR $= 0.92$, marginally below unity). With stronger persistence ($\theta \in [0.5, 1]$), BCRs are comfortably above unity, ranging from 1.14 to 2.67.

## 6.4 Cost per Outcome

It is also informative to scale costs by physical outputs. Endline estimates imply +1.207 trips per 4 weeks. Over 10 treatment weeks plus 26 post weeks, the implied additional trips per trader are approximately

$$\Delta\text{Trips} \;\approx\; 1.207 \times \frac{10 + 26}{4} \;=\; 10.9 \text{ trips.}$$

Because the subsidy pays for at most 4 trips, most additional trips occur without direct transfers. A back-of-the-envelope cost per induced trip is

therefore

$$\frac{C}{\Delta\text{Trips}} \approx \frac{8{,}000}{10.9} \approx 734 \text{ KSh per extra trip}$$

(ignoring admin and shadow costs). Combined with the unchanged average value per trip (Table 2, col. 2), this indicates that the program primarily shifts the extensive margin (more trips), which is consistent with the profit-reinvestment mechanism.

### 6.5 Potential Fiscal Upside and Externalities

Two additional channels may enhance welfare beyond the conservative baseline:

1. *Fiscal revenue.* Table 6, Panel B, shows that treatment effects are driven by additional official crossings. If even small levies/VAT apply at official posts, $\Delta R > 0$, raising $\Delta W$ in (10).

2. *Rule-of-law and safety externalities.* A shift toward official crossings may reduce exposure to harassment and bribes along informal routes and improve compliance. We lack precise measures to monetize these effects, so we set them to zero in baseline figures.

### 6.6 Summary

Under conservative assumptions, both interventions deliver trader profit gains that substantially exceed program costs over a 9–month horizon (2.5 months on-treatment + 6 months post). T2 exhibits larger private benefits and thus higher BCRs. Given the modest transfer size, the ease of verification at border posts, and evidence of persistence, the program appears highly cost-effective. Periodic re-offers (e.g., annual quotas of subsidized trips) would likely sustain gains at comparatively low cost, further improving welfare.

## 7 Conclusions and Policy Implications

This paper experimentally investigates whether incentivizing cross-border trade can improve the performance of micro-enterprises operating at the Kenya–Uganda border. Through a randomized controlled trial, we test a simple and scalable intervention consisting of a modest monetary incentive

conditional on conducting cross-border trade, with a subset of traders additionally receiving information designed to correct misperceptions about the risks associated with official border crossings.

We find large and persistent treatment effects. Six months after the end of the intervention, treated traders conduct significantly more cross-border trading trips and experience substantial increases in revenues and profits. The evidence is consistent with a mechanism based on profit reinvestment: temporary increases in liquidity enable traders to expand business activity by reinvesting profits into future trade. We also find evidence of broader business upgrading. Treated traders become more stable businesses, reallocate activity toward cross-border trade, diversify products and suppliers, adopt more regular trading schedules, and update their beliefs about profitable trading opportunities.

The welfare implications of the intervention are encouraging. A simple cost–benefit analysis shows that both treatments are highly cost-effective under conservative assumptions. Our research also bears clear and actionable policy implications. The interventions we test can be delivered at existing border crossings with limited additional infrastructure and resources, conditional on the possibility of monitoring the transfers carefully, ideally through a digital payment system to prevent fraud. The fact that the intervention operates through existing border infrastructure substantially enhances its scalability and practical feasibility.

Our results on the trajectories of impacts further suggest that the optimal policy design would offer financial incentives periodically (e.g., a fixed number of subsidized trips per trader every year) to boost reinvestment into the business and support sustained growth. Rather than functioning as a permanent subsidy, the intervention may therefore be most effective as a recurring liquidity injection that helps traders overcome temporary capital constraints and sustain business expansion over time.

We acknowledge that our design is not equipped to examine the potential general-equilibrium effects that would result from scaling up the program to the entire population of traders. However, in a population that faces severe credit constraints and where untapped growth potential is likely to be substantial, the initial impacts of scaling such interventions may be large. At the same time, careful monitoring would be necessary to reassess effectiveness over time as market conditions adjust.

Overall, our findings suggest that targeted incentives to trade can constitute an effective and scalable policy tool to foster business growth

among micro-entrepreneurs operating in border regions. More broadly, the results indicate that relatively modest interventions that relax short-run liquidity constraints and encourage participation in international markets can generate meaningful and persistent improvements in economic performance among small firms.

Future research should investigate the long-run equilibrium effects of scaling similar interventions and further disentangle the role of conditionality and earmarking relative to the pure liquidity effect of the transfer itself.

# Appendix

Figure A1: Info Card for Treatment 2


Figure A2: For those not planning to do more trips next month, why not?

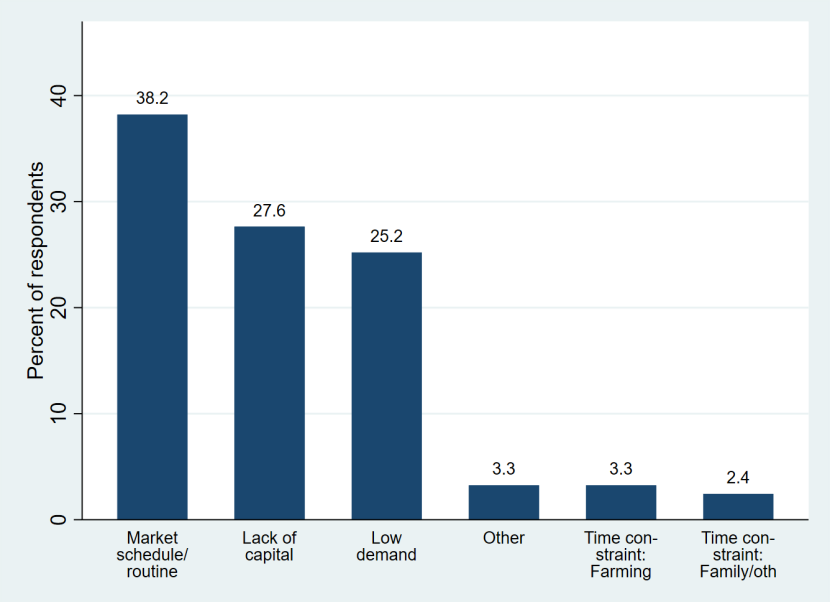


Figure A3: Factors that determine the decision to go on the next trading trip

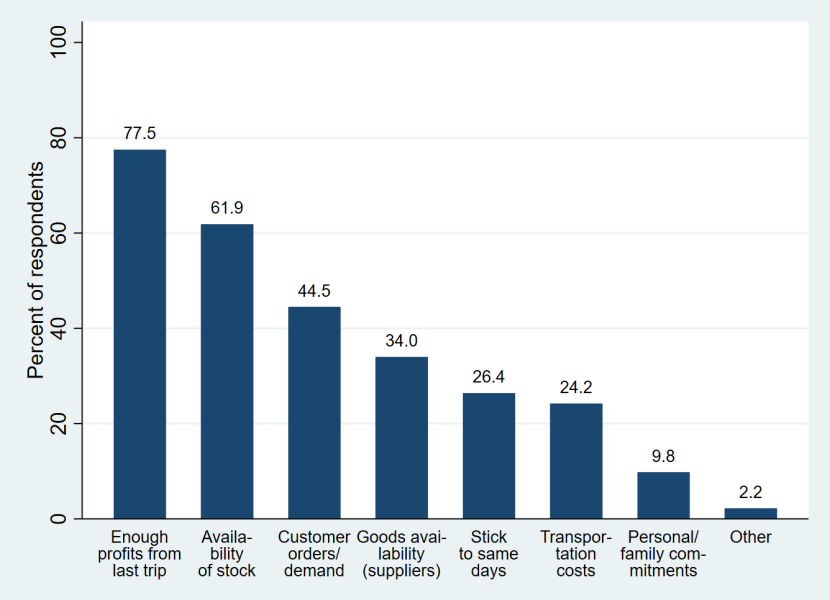



Figure A4: Use of the subsidy (treatment groups only)

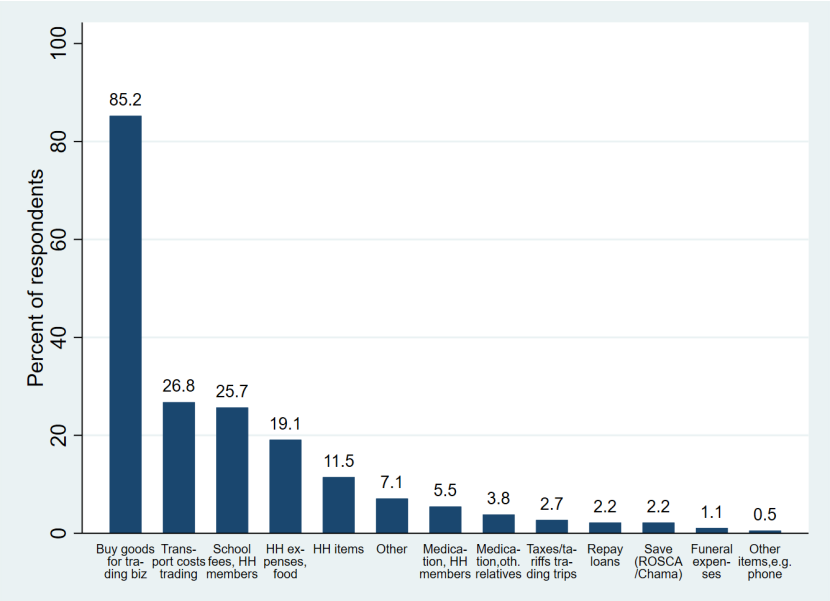


Figure A5: Use of the subsidy vs. use of windfall profits

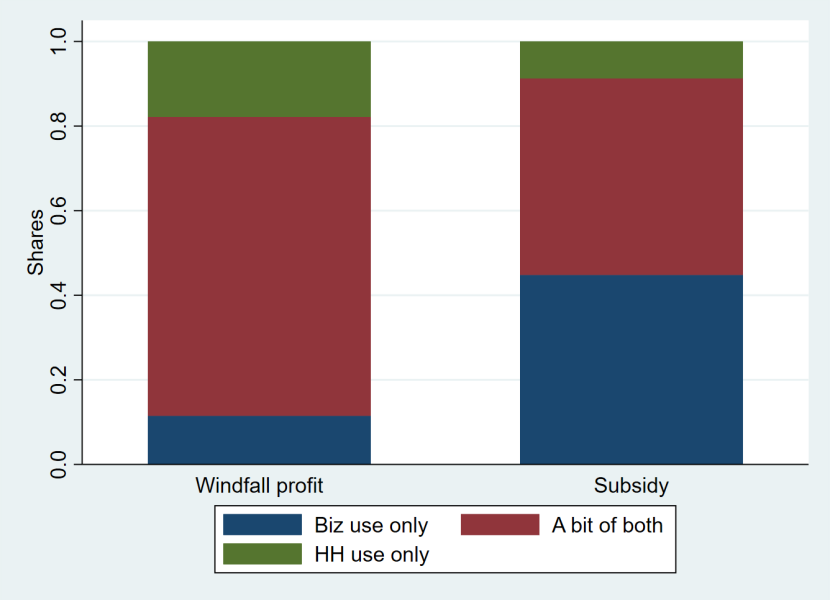


Table A1: Surveyed Markets

| **Closest Official Border Crossing** | **Market Name** |
|---|---|
| Lwakhakha | Malakisi |
| Lwakhakha | Lwakhakha main |
| Lwakhakha | Akiriamet |
| Lwakhakha | Changara |
| Lwakhakha | Ang'urai |
| Lwakhakha | Sirisia |
| Lwakhakha | Cheptais |
| Port Victoria | Sinyenye |
| Port Victoria | Port Victoria main |
| Port Victoria | Bumbe |
| Port Victoria | Mubwayo |
| Port Victoria | Budalangi |
| Port Victoria | Busembe |
| Port Victoria | Bulemia |
| Port Victoria | Nyadorera |
| Port Victoria | Sega |
| Port Victoria | Ugunja |
| Port Victoria | Sidindi |
| Port Victoria | Sigomre |
| Port Victoria | Siaya Modern |

Table A2: Treatment Effects on Trade Volume and Business Outcomes (by Treatment Group)

| | (1) N Trips | (2) Trip Value | (3) Revenues | (4) Profits |
|---|---|---|---|---|
| T1 (Subsidy) | 1.027* | 0.132 | 0.224** | 0.246** |
| | (0.563) | (0.121) | (0.103) | (0.121) |
| | [0.069] | [0.276] | [0.030] | [0.042] |
| T2 (Subsidy+Info) | 1.503** | -0.023 | 0.326** | 0.217 |
| | (0.616) | (0.139) | (0.134) | (0.139) |
| | [0.015] | [0.870] | [0.015] | [0.119] |
| Dep Var | | | | |
| Mean (C) | 4.961 | 9.368 | 9.569 | 8.024 |
| Baseline Control | | X | X | X |
| Observations | 409 | 374 | 373 | 371 |

Note: Standard errors robust (reported in parentheses, p-values reported in brackets). * p<0.1, ** p<0.05, *** p<0.01.

Table A3: Treatment Effects on Trade Volume and Business Outcomes (with Strata FE)

| | (1) N Trips | (2) Trip Value | (3) Revenues | (4) Profits |
|---|---|---|---|---|
| Treatment | 1.195** | 0.065 | 0.269*** | 0.239** |
| | (0.500) | (0.107) | (0.095) | (0.112) |
| | [0.017] | [0.544] | [0.005] | [0.033] |
| Dep Var | | | | |
| Mean (C) | 4.961 | 5.511 | 5.459 | 5.493 |
| Baseline Control | | X | X | X |
| Strata FE | X | X | X | X |
| Observations | 409 | 374 | 373 | 371 |

Note: Standard errors robust (reported in parentheses, p-values reported in brackets). * p<0.1, ** p<0.05, *** p<0.01.

Table A4: Treatment Effects on Trade Volume, High Frequency Data (by Treatment Group)

| | (1) Round 1 | (2) Round 2-3 | (3) Round 4-5 | (4) Endline |
|---|---|---|---|---|
| T1 (Subsidy) | -0.541 | 1.610*** | 1.649*** | 1.027* |
| | (0.551) | (0.512) | (0.549) | (0.563) |
| | [0.327] | [0.002] | [0.003] | [0.069] |
| T2 (Subsidy+Info) | -0.108 | 1.863*** | 1.423** | 1.503** |
| | (0.581) | (0.575) | (0.558) | (0.616) |
| | [0.853] | [0.001] | [0.011] | [0.015] |
| Dep Var Mean (Control) | 5.291 | 4.385 | 5.041 | 4.961 |
| Round FE | | X | X | |
| R-Squared | .003 | .031 | .024 | .015 |
| Observations | 409 | 778 | 764 | 409 |

Note: Standard errors robust (reported in parentheses, clustered at trader level for rounds 2-5; p-values reported in brackets). * p<0.1, ** p<0.05, *** p<0.01.

Table A5: Treatment Effects on Revenues, High Frequency Data (by Treatment Group)

| | (1) Round 1 | (2) Round 2-3 | (3) Round 4-5 | (4) Endline |
|---|---|---|---|---|
| T1 (Subsidy) | 0.114 | 0.373*** | 0.488*** | 0.224** |
| | (0.107) | (0.088) | (0.104) | (0.103) |
| | [0.287] | [0.000] | [0.000] | [0.030] |
| T2 (Subsidy+Info) | 0.022 | 0.325*** | 0.299** | 0.326** |
| | (0.126) | (0.106) | (0.119) | (0.134) |
| | [0.863] | [0.002] | [0.012] | [0.015] |
| Dep Var Mean (Control) | 9.683 | 9.483 | 9.489 | 9.569 |
| Round FE | | X | X | |
| Baseline Control | X | X | X | X |
| R-Squared | .26 | .281 | .261 | .219 |
| Observations | 389 | 713 | 678 | 373 |

Note: Standard errors robust (reported in parentheses, clustered at trader level for rounds 2-5; p-values reported in brackets). * p<0.1, ** p<0.05, *** p<0.01.

Table A6: Treatment Effects on Profits, High Frequency Data

| | (1) Round 1 | (2) Round 2-3 | (3) Round 4-5 | (4) Endline |
|---|---|---|---|---|
| Treatment | 0.080 | 0.325*** | 0.386*** | 0.235** |
| | (0.092) | (0.081) | (0.100) | (0.111) |
| | [0.388] | [0.000] | [0.000] | [0.036] |
| Dep Var Mean (Control) | 8.286 | 8.120 | 8.067 | 8.024 |
| Round FE | | X | X | |
| Baseline Control | X | X | X | X |
| R-Squared | .167 | .156 | .14 | .114 |
| Observations | 388 | 711 | 675 | 371 |

Note: Standard errors robust (reported in parentheses, clustered at trader level for rounds 2-5; p-values reported in brackets). * p<0.1, ** p<0.05, *** p<0.01.

Table A7: Changes to the business overall (by Treatment Group)

| | (1) N Trips | (2) N Off. | (3) N Unoff. | (4) Share Unoff. |
|---|---|---|---|---|
| T1 (Subsidy) | 1.027* | 1.193** | -0.064 | -0.035 |
| | (0.563) | (0.589) | (0.214) | (0.040) |
| | [0.069] | [0.043] | [0.766] | [0.382] |
| T2 (Subsidy+Info) | 1.503** | 1.463** | -0.126 | -0.079** |
| | (0.616) | (0.643) | (0.227) | (0.038) |
| | [0.015] | [0.023] | [0.580] | [0.036] |
| Dep Var Mean (C) | 4.961 | 4.527 | 0.541 | 0.133 |
| Baseline Control | | X | X | X |
| Observations | 409 | 392 | 389 | 316 |

Note: Standard errors robust (reported in parentheses, p-values reported in brackets). * p<0.1, ** p<0.05, *** p<0.01.

Table A8: Number of trips per week: at least...

| | (1) One trip | (2) Two trips | (3) Three trips | (4) Four trips |
|---|---|---|---|---|
| Treatment | 0.136*** | 0.112** | 0.033 | 0.019 |
| | (0.050) | (0.051) | (0.031) | (0.022) |
| | [0.006] | [0.029] | [0.284] | [0.369] |
| Dep Var Mean (C) | 0.556 | 0.431 | 0.0915 | 0.0392 |
| Observations | 409 | 409 | 409 | 409 |

Note: Standard errors robust (reported in parentheses, p-values reported in brackets). * p<0.1, ** p<0.05, *** p<0.01.

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