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The Role of (Mis)Perceptions in Product Choice: Evidence from a Randomized Trial In Zambia

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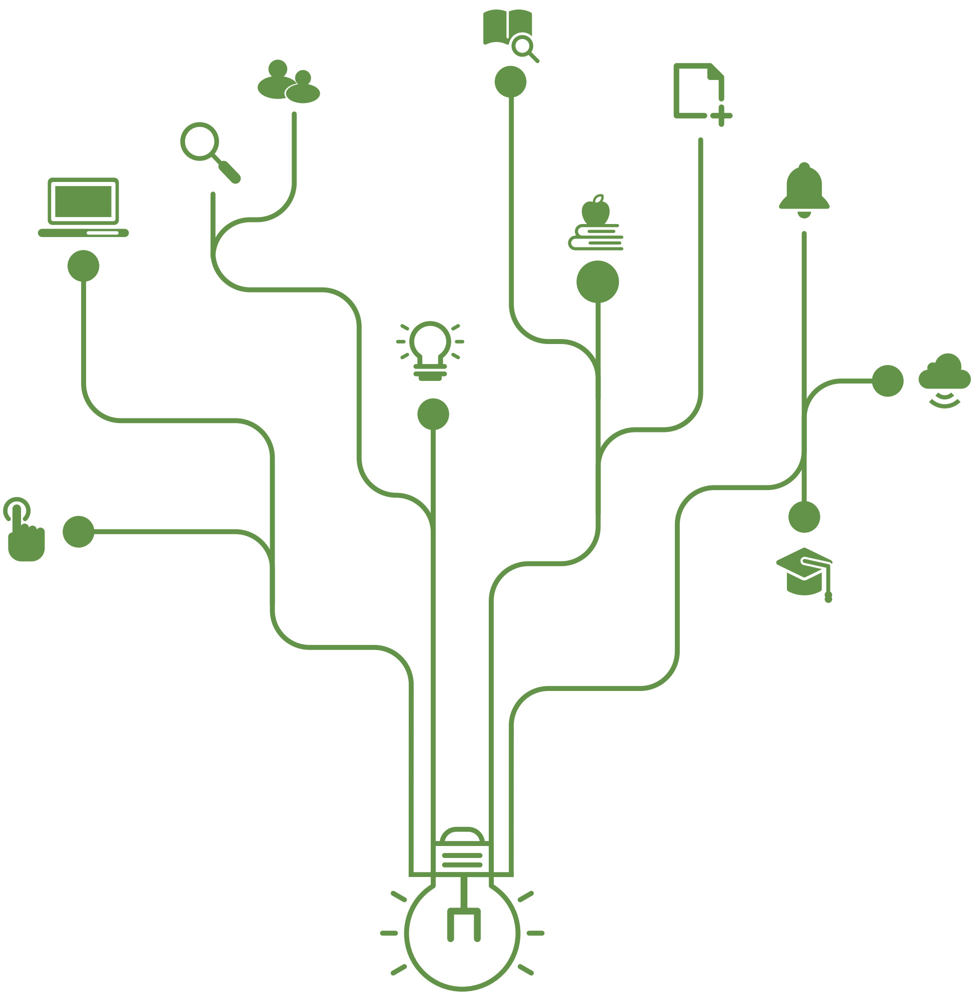

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The Role of (Mis)Perceptions in Product Choice: Evidence from a Randomized Trial In Zambia*

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Abstract

We study whether small retailers are deterred from stocking products by misperceptions about their profitability. We design a simple algorithm to identify products that are not stocked by many retailers despite being stocked and profitably sold by similar shops. We find that retailers not stocking the product generally expect the products to be unprofitable. We subsidize a randomly selected subset to stock the product and find that they earn comparable profits to those who endogenously stock the product at baseline. Treated retailers are significantly more likely to stock the product one month after incentives end, suggesting a change in perceptions.

JEL Classifications: D22, D83, L26, M21, O12, O33
Keywords: management, firm behavior, profitability perception, retail markets, small firms

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1 Introduction

Management is a clear driver of differences in firm-level productivity (Bloom et al., 2013). What is less clear, however, is what it means to be an effective manager. Although certain managerial practices are correlated with higher profits (and thus, perhaps, better management), precisely how these practices improve decision-making is less clear. One potential mechanism is that good managers have more accurate beliefs about the current or future state of the world (Bloom et al., 2020). This hypothesis is theoretically elegant in that it fits neatly within a standard model of profit-maximization under imperfect information. Yet there is relatively little evidence directly linking the inaccurate or noisy beliefs of managers to suboptimal business decisions. Aside from proving a principle, direct evidence would help policymakers better understand the boundaries of the firm's information set. Although multinational corporations that invest billions in market research may have rich information sets, the typical entrepreneur in a developing country–a market vendor or a corner shop–may lack even basic information. Generating and providing these insights through cheap and effective policy interventions could immediately raise the incomes of millions of small entrepreneurs.

This paper studies the role of inaccurate beliefs in driving a decision that is both prosaic and universal: which products the firm should stock. Though relevant for any firm, it is especially important for our sample: small and micro retailers in Lusaka, Zambia. We combine detailed data from the field with statistical learning algorithms and a randomized controlled trial to test whether misperceptions about the profitability of certain products may prevent small shops from stocking them.

We first collect data on the inventories of roughly 2,000 shops in 25 retail markets. Teams of enumerators took pictures of the full inventory. These were hand-coded into machine-readable data and run through a simple algorithm to cluster shops that stock similar products. Within each cluster we identified one product stocked by some but not all shops. These "target" products—whether they are stocked, what retail prices are charged and order prices paid if so, and beliefs about those prices if not—were the subject of a subsequent survey taken with a subset of the shops. We find that shops stocking the target product generally earn higher per-unit profits on it than on their “main goods” (the three that account for the largest share of revenue). But shops that do not stock the product believe that if they stocked the product, they would earn similar profits.

This result is consistent with the idea that shops may have misperceptions about the profitability of the products. But it is also consistent with other explanations. It could be that shops face different patterns of demand, and the ones that would find the products most profitable are precisely the ones that stock it. It could also be that the entry of shops that do not stock the good would, through competition, drive down the profits of everyone.

In either of these alternative scenarios, a shop that were randomly induced to start stocking the product would earn lower profits than those who endogenously stock the product at baseline. We test this hypothesis by running a randomized controlled trial on a sample of roughly 300 shops. Treated shops were recommended to stock the products, and given generous reimbursements for either one or two weeks. Control shops received neither recommendation nor reimbursement. Treated shops were told that after the reimbursement ended they were welcome to continue stocking the products or stop, and should make whatever decision was best for their business.

We used both follow-up phone surveys and visits by unidentified “mystery shoppers” to identify whether shops stocked the products during and then after the reimbursements ended. Initial take-up was 10 to 45 percent depending on the treatment and the measure. But by both measures, shops given two weeks of reimbursement were still stocking the product at the end of the study period two months later. By the more conservative measure (mystery shoppers), shops in the two-week group were 37 percent more likely than the control group to still be stocking the product in the final weeks of the study. By contrast, those given only one week of reimbursement were no longer stocking at rates significantly different from the control group. That may suggest that, given two weeks to experiment with the target product, shops decided they were consistently profitable.

The most common reason given by shops to not be stocking the product at baseline is that they perceive it is not profitable. But we find that shops in the treatment group that did and did not perceive the product to be profitable earned similar per-unit profits when they began stocking the product. We also find that shops in the treated groups earn similar per-unit profits to shops that were already stocking the product before the intervention. We find some suggestive evidence that the benefits of stocking the products went beyond mere positive per-unit profits. Based on two measurements from a separate survey (by a different study team) we find that shops in the two-week reimbursement group had more customers than control shops. It is possible that widening the range of products attracted more customers. Consistent with this result, we do not find any evidence that treated shops were spending or selling any less of their main products despite selling more of the target products. Taken together our results suggest that shops had inaccurate perceptions of the profitability of these products, and shops given two weeks to experiment with the product may have changed their perceptions (based on their decision to keep stocking the products).

Our results may help reconcile a puzzling result in the literature on management. While many randomized trials suggest that direct and individualized interventions like management consulting and mentorship can have large impacts (Bloom et al., 2013; Brooks et al., 2018; Bruhn et al., 2018), interventions aimed at training general skills have been less transformative. Though positive in aggregate, individual studies cannot always detect significant impacts because the effects are relatively small (McKenzie and Woodruff, 2006). Our results suggest that concrete, actionable information can have impacts. One interpretation is that managers struggle to apply general skills to generate better information, or to translate that information into profitable actions. That interpretation would be consistent with studies showing that rules of thumb (Drexler et al., 2014) or booklets of locally-sourced advice (Dalton et al., 2021), have significant and positive impacts. Brooks et al. (2018) show suggestive evidence that management mentors helped their mentees mainly by telling them where to find inexpensive suppliers for their products.

2 Experimental Design and Data

2.1 Study Setting

Our study sample is drawn from the sample of small retailers in Lusaka, Zambia, from our sister project (Samaniego de la Parra and Shenoy, 2024). With the photographs of the inventories of each shop censused in September 2022, we constructed a database of the shops' stocked products and clustered them based on the similarity of product types. We then identified a "target" product in each cluster that is stocked by some shops but not all. We conducted our own baseline survey in January-March 2023, asking the shops whether they currently stock the target product, and we included into our study sample only those who reported not stocking the product then.

2.2 Intervention

The interventions of this study are providing small retail shops with 1-week or 2-week subsidies for stocking new products that we recommend. We undertook a series of steps to identify the products to be recommended. First, we digitized the inventories of all censused shops, creating a comprehensive database of the products they stocked. Using this data, we conducted a clustering analysis to group the shops based on the types of products they carried, resulting in eight distinct clusters. Within each cluster, we focused on identifying "target" products—items that were only stocked by a subset of shops within the cluster. We describe each step in more detail as follows.

Digitizing the inventories

In September 2022, our sister project (Samaniego de la Parra and Shenoy, 2024) conducted a comprehensive census of around 3,000 small retailers across 25 markets in Lusaka, during which we gathered photographs of the current inventories of the shops. Enumerators manually identified the products visible in each photograph using a standardized set of harmonized product codes. To further minimize the risk of measurement errors, each image was independently reviewed by two enumerators, and in instances where discrepancies arose between their identifications, a third enumerator was brought in to adjudicate and provide the final determination.

Clustering shops and identifying target products

Given that the range of products is large in comparison to the number of shops in our sample, we utilized an algorithm to reduce the dimensionality of the data by extracting the first five principal components, each shop then being represented as a point within this five-dimensional space. To identify clusters of similar shops, we applied a k-cluster analysis, a method that searches for k centroids within the vector space. The goal of this analysis is to minimize the average or median distance between each point (representing a shop) and its nearest centroid. This approach, which is a fundamental form of unsupervised machine learning, automates the otherwise labor-intensive task of categorizing shops into distinct groups. While some shops did not fit neatly into any cluster, the majority were successfully assigned to clusters where at least 50% of the shops stocked the most common product within that group.

We identified 8 clusters of shops, and within each cluster, we singled out one "target" product, which was stocked by approximately 30% of the shops (Table A1).

2.3 Experimental Design

With approximately 1,000 shops for which we identified a shop cluster, we conducted a baseline in January-March 2023, and identified 271 shops who did not stock the target product. They were randomly assigned to 3 groups: (1) 1-week subsidy for stocking the target product we recommended, (2) 2-week subsidies, and (3) pure control (who received no subsidies).

2.4 Data Collection

The main data used for analysis in this paper comes from three sets of surveys.

2.4.1 Pre-Survey

We identified 271 shops who did not stock the target product at baseline, and conducted a pre-treatment survey with them in July-August 2023. We measured whether they stocked the target product and if any, how much they paid for procurement and how much they are charging their customers, along with similar information for their 3 main good that account for the largest share of shop revenue. At the end of the survey, we enrolled the treatment groups.

2.4.2 Bi-weekly Phone Survey

We conducted a phone survey every two weeks for each shop for a total of 6 rounds during June-October 2023. In the bi-weekly phone survey, we not only asked retailers whether specific products were currently in stock but also included a range of questions similar to those in our in-person survey. These questions focused on key business metrics such as sales volumes, profitability, and overall business performance. Additionally, we gathered data on retailers' beliefs and expectations regarding future sales and profitability of each product.

2.4.3 Weekly Mystery Shoppers

To ensure the accuracy of self-reported data, particularly regarding product availability, we employed a team of "mystery shoppers" to independently verify shop claims. Since retailers may overstate product availability—perhaps to appear cooperative or agreeable to enumerators—this additional layer of assessment was crucial. The mystery shoppers visited each shop on a weekly basis, for a total of 12 weeks for each shop (during June-October 2023), to check and report whether the products were actually being stocked. By cross-referencing these independent observations with the survey responses, we aimed to reduce potential bias and improve the reliability of the inventory data collected.

2.5 Summary Statistics and Randomization Check

Table 1 reports the demographics and shop characteristics of the study sample at baseline and experimental balance across treatment arms.

In Panel A, about 69% of shop owners or managers are female, with the average age being around 41 years. Slightly more than half of the shop owners are married, and they have an average of 2.35 children living in their households. In terms of education, 29% of the control group have completed secondary school, while 61% are literate in English.

Additionally, almost all shop owners (94%) have access to a mobile phone, with 62% owning a smartphone. A small share (16%) operates another business alongside their shop.

In terms of shop-level characteristics (Panel B), shops have been in operation for an average of 14 years and are open for about 75 hours per week. The typical display area of the shop measures 9.2 square meters, and 33% of the shops have dedicated storage space. Around 38% of the shops are connected to electricity, and the average value of business assets are at $393. On average, shops employ 0.88 workers and serve around 50 customers per day, and weekly revenue is $189. While about 20% of shops report stockouts in the past week, around 31% of shops have regular suppliers. While none of these shops stocked the target product at baseline (by construction of the study sample), 12% reported they have previously stocked it.

Overall, when comparing the one-week and two-week subsidy groups to the control group, they look reasonably balanced, only two coefficients out of 42 being statistically significant.

Table 1: Baseline Summary Statistics and Experimental Balance

	(1) Control Mean	(2) 1-Week Subsidy - Control	(3) 2-Week Subsidy - Control
Panel A. Demographics of shop owner/manager			
=1 if female	0.69	-0.06	-0.05
		(0.07)	(0.07)
Age	40.93	-3.09*	-1.70
	[12.56]	(1.75)	(1.89)
=1 if married	0.55	-0.00	-0.06
		(0.07)	(0.08)
Number of children living together	2.35	0.28	-0.00
	[1.77]	(0.26)	(0.29)
=1 if completed secondary school	0.29	-0.01	0.04
		(0.07)	(0.07)
=1 if literate in English	0.61	-0.02	0.02
		(0.07)	(0.07)
=1 if has mobile phone	0.94	0.02	-0.00
		(0.03)	(0.04)
=1 if has smartphone	0.62	0.10	0.01
		(0.07)	(0.08)
=1 if operates another business	0.16	0.09	0.06
		(0.06)	(0.06)
Panel B. Shop characteristics			
Years of operation	13.74	-1.84	-2.11
	[10.92]	(1.51)	(1.63)
Number of hours a week the shop is open	74.53	1.94	0.18
	[16.60]	(2.28)	(2.41)
Display area (squared meters)	9.18	0.40	36.49
	[11.02]	(1.81)	(35.64)
=1 if has storage space	0.33	-0.05	-0.07
		(0.07)	(0.07)
=1 if connected to electricity	0.38	-0.02	0.04
		(0.07)	(0.07)
Value of business assets (USD)	393.44	144.20	61.76
	[949.77]	(168.97)	(162.69)
Number of employees	0.88	-0.09	-0.14
	[1.09]	(0.17)	(0.15)
Number of customers a day	50.73	8.26	9.17
	[42.65]	(6.67)	(6.56)
Revenue (past week, USD)	189.40	-7.05	41.00
	[269.09]	(41.43)	(44.85)
=1 if any stockout in past week	0.20	0.05	0.02
		(0.06)	(0.06)
=1 if has regular supplier	0.31	0.01	0.11
		(0.07)	(0.07)
=1 if has stocked the niche product earlier	0.12	0.02	0.12**
		(0.05)	(0.06)
Observations	90	97	84

Note: Columns 1 presents the means and standard deviations for the control group; Columns 2 and 3 report the differences with treatment groups and standard errors in parentheses. ***, **, and * represent significance at 1%, 5%, and 10%, respectively.

3 Results

3.1 First Stage on Stocking Target Product

We first measure shops' adoption of the target product in response to our intervention subsidies. We estimate the dynamic treatment effects using our multiple post-treatment data as follows:

$$Y_{i(c)t} = \sum_t \beta_t Treat\_1week_{i(c)} D_t + \sum_t \gamma_t Treat\_2week_{i(c)} D_t + \delta Y_{i(c)0} + \phi_c + \varepsilon_{i(c)t} \quad (1)$$

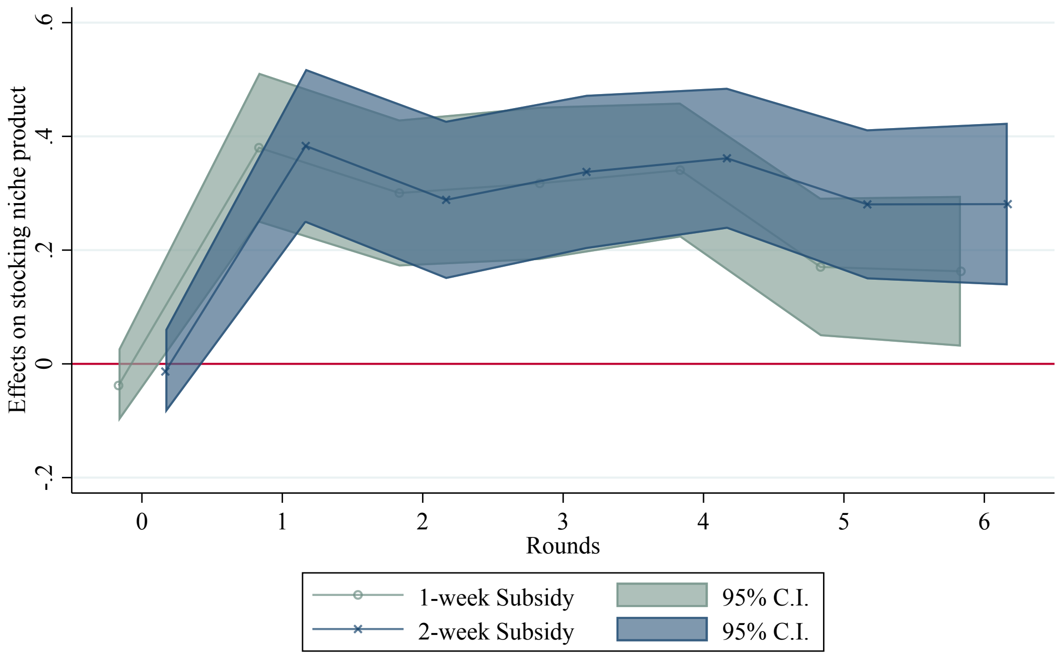
where $Y_{i(c)t}$ is an outcome for shop i in shop cluster c at survey round t. $Treat\_1week_{i(c)}$ and $Treat\_2week_{i(c)}$ are binary variables equal to 1 for shops assigned to 1-week subsidy and 2-week subsidies, respectively, and 0 otherwise; D_t is a binary variable equal to 1 for the data point collected at t-th round of data collection; $Y_{i(c)0}$ is the value of the pre-treatment outcome variable (measured at the Pre-survey); and ϕ_c are shop cluster (at which the randomization was stratified) fixed effects.

Figure 1 plots the coefficients and the 95% confidence intervals for β and γ from Equation 1. Each round is two weeks, where Round 1 is the first two-week window since the start of our interventions. Panel (a) shows the results from the bi-weekly phone surveys. We find both groups show consistently positive effects until the end of our data collection, 12 weeks post-treatment. While the 1-week subsidy group shows a drop after 8 weeks to below 20 percentage points, for those who received the subsidies for two weeks the effects persist at around 30 percentage points even after 12 weeks post treatment.

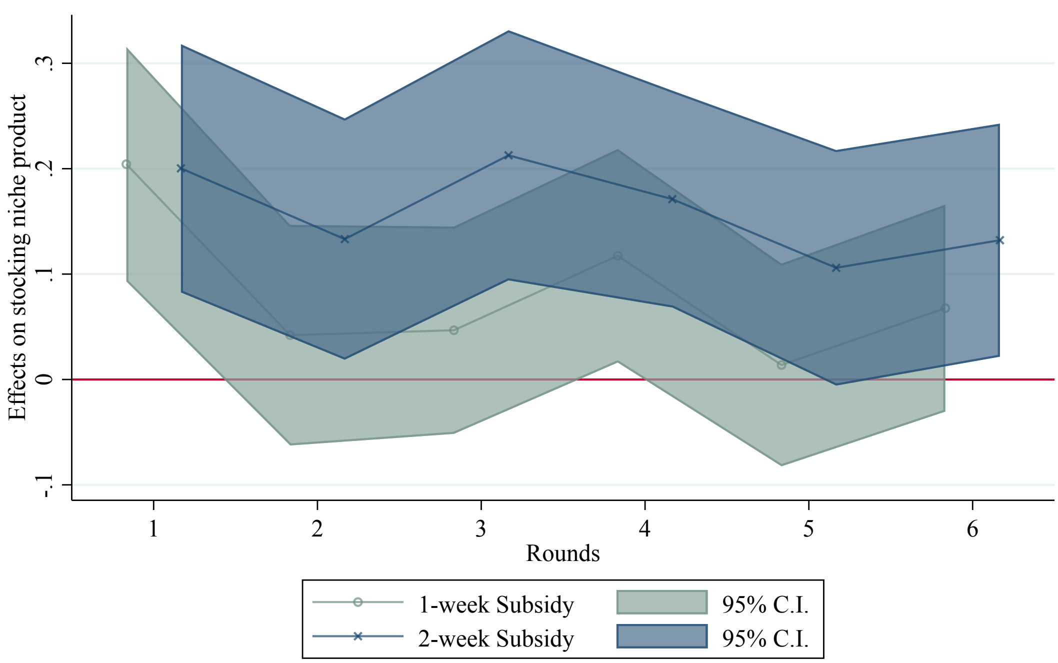
In Panel (b), we use a more conservative measure, data from the the mystery shoppers, which were conducted every week but aggregated into two-week period for comparability with the phone survey results. For the 1-week subsidy group, the effects are significant at around 20% point only in Round 1, but quickly die out from Round 2. For the 2-week subsidy group, the effects look much more persistent until the final round, while there is a gradual decline from around 20 to 13 percentage points.

Figure 1: Dynamic Effects on Stocking Target Product

(a) Data from phone surveys



(b) Data from mystery shoppers



Note: Regressions include baseline measurement and product×round fixed effects. Standard errors are clustered at store level.

We now show pooled regression results, and run the following:

$$Y_{i(c)t} = \beta Treat\_1week_{i(c)} + \gamma Treat\_2week_{i(c)} + \delta Y_{i(c)0} + \lambda_i + \phi_{ct} + \varepsilon_{i(c)t} \tag{2}$$

where λ_i and ϕ_{ct} are shop and shop cluster×survey round fixed effects, respectively.

Table 2 show results using the phone survey data. For the full period of 12 weeks (6 rounds), we find 27-29 percentage points increases in stocking the target product for the 1-week subsidy group and a slightly higher 30-31% points for the 2-week subsidy group, while the difference between treatment groups are not significant. Consistent with the dynamics shown in Figure 1, the effects are larger in earlier rounds (Rounds 1 and 2), where the gradient is steeper for the those who received the subsidies for only 1 week.

Using data from the mystery shoppers, Table 3 show smaller effects overall. The pooled effects across all survey rounds are 6-8 percentage points for 1-week subsidy group, and 16 points for 2-week group, the difference being statistically significant. In columns 3-6, we find the target product stocking effects are more persistent for those who received the subsidies for two weeks. For the 1-week group, the effects are significant only in the earlier rounds, and become insignificant in the later rounds.

Table 2: Effects on Stocking Target Product (Phone Survey, bi-weekly)

	(1)	(2)	(3)	(4)	(5)	(6)
	Full period		Rounds 1 and 2		Rounds 5 and 6	
1-Week Subsidy	0.29***	0.27***	0.36***	0.34***	0.20***	0.15***
	(0.06)	(0.05)	(0.06)	(0.06)	(0.07)	(0.06)
2-Week Subsidy	0.30***	0.31***	0.34***	0.33***	0.27***	0.27***
	(0.07)	(0.05)	(0.07)	(0.06)	(0.08)	(0.06)
Stock at Baseline		0.36***		0.40***		0.33***
		(0.04)		(0.05)		(0.06)
Constant	0.33***	0.26***	0.33***	0.29***	0.30***	0.25***
	(0.03)	(0.04)	(0.02)	(0.04)	(0.03)	(0.04)
Product × Round FEs	X	X	X	X	X	X
Store FEs	X		X		X	
Test of equality	0.928	0.410	0.856	0.933	0.364	0.076
Control mean	0.35	0.36	0.35	0.40	0.33	0.35
Number of unique shops	270	270	258	259	256	258
Observations	1,736	1,472	730	474	747	496

Note: Robust standard errors in parentheses. ***, **, and * represent significance at 1%, 5%, and 10%, respectively.

Table 3: Effects on Stocking Target Product (Mystery Shopper, weekly)

	(1)	(2)	(3)	(4)	(5)	(6)
	Full period		Rounds 1 and 2		Rounds 5 and 6	
1-Week Subsidy	0.06	0.08**	0.10**	0.12***	0.02	0.04
	(0.04)	(0.03)	(0.05)	(0.05)	(0.04)	(0.04)
2-Week Subsidy	0.16***	0.16***	0.16***	0.17***	0.12**	0.12***
	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)
Stock at Baseline		0.31***		0.31***		0.27***
		(0.04)		(0.05)		(0.05)
Constant	0.32***	0.23***	0.34***	0.25***	0.32***	0.25***
	(0.03)	(0.02)	(0.04)	(0.03)	(0.03)	(0.03)
Product × Round FEs	X	X	X	X	X	X
Test of equality	0.026	0.063	0.276	0.393	0.034	0.073
Control mean	0.31	0.31	0.34	0.34	0.32	0.32
Number of unique shops	275	275	268	268	268	268
Observations	2,909	2,909	894	894	1,019	1,019

Note: Robust standard errors in parentheses. ***, **, and * represent significance at 1%, 5%, and 10%, respectively.

3.2 Effects on Firm Performance

We use data collected from a sister project (CITE) to measure treatment effects on shop's overall performance. This data were collected at the time matching Rounds 1 and 6 of our phone surveys and mystery shopper data collection—i.e. two weeks and 12 weeks post treatment, respectively. We run the same regression specification as Equation 2.

Table 4 show the results. From the pooled regressions (Panel A), we find that the 2-week treatment group had 20 percent more customers compared to the control group. From Panels B and C, we find that while the effect on the number of customers gets smaller from Round 1 to 6, it's still 18 percent and significant after 12 weeks since our subsidies. The effects on profit are 10-12 percent but insignificant.

For the 1-week group, we find smaller and insignificant effects on the number of customers, but a significant increase in profit by 13 percent. Yet the profit effects become insignificant after 12 weeks. For either group, we don't find any meaningful effects on the aggregate mark-up of all products the shop is selling.

Table 4: Effects on Store-level Outcomes

	(1) log(no. of customers)	(2) log(profit)	(3) Aggregate mark-up
Panel A. Rounds 1 and 6			
1-Week Subsidy	0.07	0.13*	0.02
	(0.06)	(0.07)	(0.02)
2-Week Subsidy	0.20***	0.11	0.00
	(0.08)	(0.09)	(0.02)
Test of equality	0.072	0.797	0.258
Control mean	3.16	4.57	1.45
Number of unique shops	245	244	244
Observations	454	452	452
Panel B. Round 1 only			
1-Week Subsidy	0.07	0.16*	0.03
	(0.06)	(0.09)	(0.03)
2-Week Subsidy	0.23***	0.10	0.00
	(0.08)	(0.12)	(0.03)
Test of equality	0.052	0.639	0.209
Control mean	3.15	4.57	1.44
Observations	230	230	230
Panel C. Round 6 only			
1-Week Subsidy	0.06	0.11	0.01
	(0.07)	(0.10)	(0.03)
2-Week Subsidy	0.18*	0.12	-0.00
	(0.09)	(0.10)	(0.03)
Test of equality	0.201	0.950	0.576
Control mean	3.18	4.56	1.46
Observations	224	222	222

Note: Regressions include baseline measurement of outcome and product x round fixed effects. Robust standard errors in parentheses. ***, **, and * represent significance at 1%, 5%, and 10%, respectively.

In later rounds of our phone surveys (Rounds 5 and 6), we measured more detailed questions on procuring and selling each product. We asked these questions not only the for target product but also for each shop's three main goods. We run the same specification as

Equation 2.

The results are shown in Table 5. While the treatment shops reported significantly higher expenses and sales of the target product and higher mark-up too (insignificant for the 1-week group), we find no significant effects on the shop's main products.

Table 5: Effects on Expenses and Sales by Product Group

	(1)	(2)	(3)	(4)	(5)	(6)
	Target product			Main products		
	expenses	sales	mark-up	expenses	sales	mark-up
1-Week Subsidy	6.11***	14.27**	0.65	67.13	73.52	-0.04
	(1.84)	(5.63)	(0.58)	(48.94)	(46.29)	(0.70)
2-Week Subsidy	3.56***	6.44***	0.83*	127.82	-0.94	-0.77
	(1.33)	(1.96)	(0.47)	(103.62)	(24.82)	(0.56)
Test of equality	0.178	0.184	0.804	0.583	0.105	0.203
Control mean	4.20	4.27	0.80	92.58	108.01	2.42
Number of unique shops	253	253	253	253	253	253
Observations	488	488	488	488	488	488

Note: Monetary values in USD. Regressions include baseline measurement of outcome and product x round fixed effects. Robust standard errors in parentheses. ***, **, and * represent significance at 1%, 5%, and 10%, respectively.

At our pre-survey, shops most frequently cited a lack of perceived profitability as the reason for not stocking the target product (Table A8). However, in Table 6, we find that among the treatment group, shops that initially viewed the product as unprofitable earned comparable sales and per-unit profits to those that did not share this perception once they began carrying the product. Additionally, the shops that did not stock but decided to stock the target product after our subsidies earned mark-ups similar to those already stocking the product before our intervention.

Table 6: Target Product Profitability at Rounds 1-2, by non-stocking reasons at pre-survey

	(1)	(2)	(3)	(4)	(5)
	At presurvey, for target product:				
	Not stocked		Stocked	p-value (mean difference)	
	because unprofitable	other reasons		Unprofitable - Others	Not stocked - Stocked
Niche product: sales (2 weeks)	324.53	331.13	305.60	0.942	0.750
Niche product: profits (2 weeks)	22.95	53.07	41.75	0.690	0.911
Niche product: markup	1.95	1.72	2.15	0.421	0.208
Observations	85	61	87		

Note: Treatment groups only.

4 Conclusion

This paper gives evidence that the stated perceptions of managers may diverge from reality, driving them to make suboptimal decisions. The evidence that would change their perceptions—seeing that a product can be sold at a profitable price—is unavailable unless they begin stocking the product that they believe is unprofitable. In such cases the misperception is self-sustaining.

This idea may help explain why poor management practices persist among small retailers. Larger firms can experiment without accepting too much risk, and necessarily observe a wide range of sales outcomes. Their scale may help them improve their management—a converse to the more widely accepted idea that good management is necessary for scale (Lucas Jr, 1978). Small firms lack this informational advantage and, one may speculate, thus remain small. Future research may fruitfully test whether this idea is true, and whether sustainable interventions to help small firms share and pool information can overcome the handicap.

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Appendix A

Table A1: Shop Clusters and Target Products

Cluster No.	Niche Product	% stocked at Census
1	Deodorants	31%
2	Onions	40%
3	Flour and milled products	30%
4	Exercise books	31%
5	Creams/lotions for common skin ailments	30%
6	Powdered drink mix	30%
7	Sport or energy drink	30%
8	Ginger root	31%

Note: ***, **, and * represent significance at 1%, 5%, and 10%, respectively.

Table A2: Attrition Balance

	(1)	(2)	(3)
	Proportion of observations in:		
	Phone Survey (bi-weekly)	Mystery Shopper (weekly)	
	round level	week level	aggregated round level
1-Week Subsidy Group	-0.01	-0.00	-0.01
	(0.03)	(0.03)	(0.03)
2-Week Subsidy Group	-0.03	-0.00	-0.00
	(0.04)	(0.03)	(0.03)
Control mean	0.90	0.88	0.95
Control SD	0.21	0.18	0.16
Observations	271	271	271

Note: ***, **, and * represent significance at 1%, 5%, and 10%, respectively.

Table A3: Effects on Store-level Outcomes - Phone Survey Rounds 1 and 6

	(1)	(2)	(3)	(4)	(5)	(6)
	log(number of customers)		log(profit)		Aggregate mark-up	
1-Week Subsidy	0.04	0.07	0.05	0.13*	0.04	0.02
	(0.06)	(0.06)	(0.08)	(0.07)	(0.03)	(0.02)
2-Week Subsidy	0.18**	0.20***	0.07	0.11	0.02	0.00
	(0.09)	(0.08)	(0.12)	(0.09)	(0.03)	(0.02)
Measurement at Baseline		0.81***		0.77***		0.56***
		(0.04)		(0.06)		(0.07)
Constant	3.26***	0.53***	4.69***	0.98***	1.42***	0.62***
	(0.03)	(0.14)	(0.04)	(0.27)	(0.01)	(0.09)
Product $\times$ Round FEs	X	X	X	X	X	X
Store FEs	X		X		X	
Test of equality	0.102	0.072	0.852	0.797	0.605	0.258
Control mean	3.18	3.16	4.57	4.57	1.46	1.45
Number of unique shops	235	245	234	244	234	244
Observations	666	454	663	452	663	452

Note: Robust standard errors in parentheses. ***, **, and * represent significance at 1%, 5%, and 10%, respectively.

Table A4: Effects on Store-level Outcomes - Phone Survey Round 1 only

	(1)	(2)	(3)	(4)	(5)	(6)
	log(number of customers)		log(profit)		Aggregate mark-up	
1-Week Subsidy	0.04	0.07	0.06	0.16*	0.04	0.03
	(0.07)	(0.06)	(0.09)	(0.09)	(0.03)	(0.03)
2-Week Subsidy	0.21**	0.23***	0.03	0.10	0.02	0.00
	(0.09)	(0.08)	(0.14)	(0.12)	(0.03)	(0.03)
Measurement at Baseline		0.84***		0.82***		0.58***
		(0.04)		(0.07)		(0.07)
Constant	3.27***	0.41***	4.70***	0.78**	1.42***	0.58***
	(0.02)	(0.14)	(0.03)	(0.35)	(0.01)	(0.10)
Product × Round FEs	X	X	X	X	X	X
Store FEs	X		X		X	
Test of equality	0.056	0.052	0.823	0.639	0.525	0.209
Control mean						
Number of unique shops	211	230	211	230	211	230
Observations	422	230	422	230	422	230

Note: Robust standard errors in parentheses. ***, **, and * represent significance at 1%, 5%, and 10%, respectively.

Table A5: Effects on Store-level Outcomes - Phone Survey Round 6 only

	(1)	(2)	(3)	(4)	(5)	(6)
	log(number of customers)		log(profit)		Aggregate mark-up	
1-Week Subsidy	0.03	0.06	0.06	0.11	0.03	0.01
	(0.07)	(0.07)	(0.11)	(0.10)	(0.03)	(0.03)
2-Week Subsidy	0.17	0.18*	0.12	0.12	0.02	-0.00
	(0.10)	(0.09)	(0.12)	(0.10)	(0.03)	(0.03)
Measurement at Baseline		0.78***		0.73***		0.54***
		(0.05)		(0.05)		(0.08)
Constant	3.28***	0.64***	4.70***	1.19***	1.43***	0.66***
	(0.02)	(0.18)	(0.03)	(0.24)	(0.01)	(0.11)
Product × Round FEs	X	X	X	X	X	X
Store FEs	X		X		X	
Test of equality	0.178	0.201	0.620	0.950	0.971	0.576
Control mean						
Number of unique shops	207	224	205	222	205	222
Observations	414	224	410	222	410	222

Note: Robust standard errors in parentheses. ***, **, and * represent significance at 1%, 5%, and 10%, respectively.

Table A6: Stocking Effects on Store-level Outcomes (2SLS, phone survey) - Combined

	(1) log(no. of customers)	(2) log(profit)	(3) Aggregate mark-up
Panel A. Rounds 1 and 6			
Stocked Niche Product	0.49**	0.41	-0.00
	(0.22)	(0.27)	(0.06)
Control mean	3.17	4.57	1.46
Number of unique shops	232	231	231
Observations	402	400	400
Panel B. Round 1 only			
Stocked Niche Product	0.28	0.44	0.03
	(0.19)	(0.27)	(0.07)
Control mean	3.16	4.54	1.46
Observations	189	189	189
Panel C. Round 6 only			
Stocked Niche Product	0.69*	0.35	-0.02
	(0.37)	(0.35)	(0.09)
Control mean	3.18	4.59	1.46
Observations	213	211	211

Note: Regressions include baseline measurement of outcome and product x round fixed effects. Robust standard errors in parentheses. ***, **, and * represent significance at 1%, 5%, and 10%, respectively.

Table A7: Stocking Effects on Store-level Outcomes (2SLS, mystery shopper) - Combined

	(1) log(no. of customers)	(2) log(profit)	(3) Aggregate mark-up
Panel A. Rounds 1 and 6			
Stocked Niche Product	0.74*	0.63	0.00
	(0.38)	(0.43)	(0.10)
Control mean	3.17	4.57	1.46
Number of unique shops	232	231	231
Observations	402	400	400
Panel B. Round 1 only			
Stocked Niche Product	0.38	0.63	0.04
	(0.28)	(0.40)	(0.10)
Control mean	3.16	4.54	1.46
Observations	189	189	189
Panel C. Round 6 only			
Stocked Niche Product	1.10	0.56	-0.03
	(0.80)	(0.63)	(0.16)
Control mean	3.18	4.59	1.46
Observations	213	211	211

Note: Regressions include baseline measurement of outcome and product x round fixed effects. Robust standard errors in parentheses. ***, **, and * represent significance at 1%, 5%, and 10%, respectively.

Table A8: Reasons for not stocking target product (at Pre-survey)

	(1)	(2)
	Mean (=1 if yes)	
	Whether later decided to stock niche product (in rounds 5-6)	
	Yes	No
Existing competitor makes it unprofitable to stock	0.26	0.22
Unprofitable / net cost exceeded net revenue	0.12	0.24
Too little demand / too many unsold	0.18	0.09
Only stock seasonally (not the right season now)	0.12	0.12
Outside of the store's main product	0.12	0.09
No refrigerator/freezer	0.06	0.11
Supplier not available	0.06	0.09
Liquidity constrained	0.10	0.07
Market rules	0.06	0.05
No experience/knowledge	0.02	0.07
Other	0.07	0.10
Observations	50	85

Table A9: Reasons for starting to stock target product

	(1)	(2)	(3)
	Mean (=1 if yes)		
	Control group	1-week subsidy group	2-week subsidy group
I decided to experiment	0.78	0.63	0.60
Request from customer	0.19	0.17	0.26
Heard from supplier that this good was selling well	0.05	0.10	0.10
Heard from other shops that this good was selling well	0.00	0.02	0.02
Heard from friends/family that this good was in demand	0.00	0.00	0.01
Heard from radio / newspaper / TV / other mass media	0.00	0.01	0.00
Heard from professional organization	0.00	0.00	0.00
Because of the reimbursement offer	0.20	0.46	0.54
A friend received a reimbursement offer and suggested I start stocking it	0.00	0.00	0.00
I observed other shops in the area stocking it these past few weeks	0.00	0.01	0.00
Observations	109	145	130